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Book Review

[Book Review of] Modern business cycle theory,
Barro, Robert J. (ed.), Oxford, Blackwell, 1989

Weltwirtschaftliches Archiv

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Suggested citation: Scheide, Joachim (1992) : [Book Review of] Modern business cycle theory, Barro, Robert J. (ed.), Oxford, Blackwell, 1989, Weltwirtschaftliches Archiv, ISSN 0043-2636, Vol. 128, Iss. 1, pp. 168-171, <http://hdl.handle.net/10419/2172>

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LITERATUR – LITERATURE

REZENSIONEN – BOOK REVIEWS

Barro, Robert J. (Ed.), *Modern Business Cycle Theory*. Oxford, Cambridge, Mass., 1989. Basil Blackwell, Harvard University Press. 337 pp.

The cover of this book shows a lot of bank notes. This is peculiar because the role of money is not considered to be very important anymore for business cycles, as Robert Barro also states in the introduction: “. . . the accounting for major short-run nonneutralities of money was a misplaced priority for the new classical approach” (p. 4). Indeed, new classicals do have problems in explaining a major impact of monetary policy on output, thus giving rise to the theory of real business cycles. This shift in priorities could not really be expected in the 1970s when the new classical approach, stressing unanticipated changes in money, emerged as the alternative to the Keynesian explanation of the Phillips-curve. Apart from that, the title is somewhat misleading since the book also contains articles on aspects of macroeconomics other than business cycles. After all, recent developments have shown that the distinction between growth and cycles is, at least to some extent, artificial, and this collection of eight essays is more of an account of new classical economics in general. It presents an overview of the “second generation” of new classical theory with contributions by leading economists who are either strong proponents or at least sympathetic to this approach.

This book is testimony to the fact that progress has been made in a large number of areas. The articles take the reader through the experience of model building based on “first principles” and also to the frontiers of that approach in various areas of research. Much has changed in the way macroeconomics is pursued today, and this is also due to the contributions of authors using equilibrium models with rational expectations. Whether this approach is the most promising one is certainly disputed by many economists. Some would altogether deny the necessity of building on first principles (maximizing agents, cleared markets, etc.); others would at least add some other features (such as stickiness of prices and wages) to the models should this approach fail to explain relevant phenomena in reality. Against the background of the present controversy among macroeconomists, it is an advantage of the book that the authors openly discuss the limits of the particular models at hand which may or may not be resolved by future work.

In Chapter 1, *Bennett McCallum* analyzes “Real Business Cycle Models”. A basic model is developed which is then used to describe time paths of macroeconomic variables such as consumption and investment. The behavior of the time series depends on shocks to technology and on the nature of these shocks. McCallum then discusses the empirical work on real business cycles, also by comparing results of various models in the literature. He demonstrates the strengths of those models – for example the fact that the overall fit with actual data is surprisingly good – and also the weaknesses in terms of the poor fit of particular variables. Since the early phase of this approach, several modifications have been made to overcome these problems. McCallum concludes that the weaknesses he discusses may be due to the simplicity of existing models rather than to any basic flaw in the strategy of real business cycle theory. Surely, this theory is highly controversial among macroeconomists, also because of the policy implication which simply says that fluctuations in economic activity are Pareto optimal

and do not require any efforts of stabilization policy. In this sense, they go even further than earlier new classical models in which there was at least some message saying that shocks from monetary or fiscal policy should be avoided.

The new growth theory is analyzed by *Paul Romer* in Chapter 2 (“Capital Accumulation in the Theory of Long-Run Growth”). Several stylized facts – such as the relationship between output growth and export growth or output growth and the share of investment – are described which growth theory should be able to explain. Romer systematically goes through the most important models that have been developed in recent years and which may be used to solve the puzzles that are left when one relies on the traditional neoclassical growth model of the Solow-type. Most of the new equilibrium models stress externalities and thus do not solely rely on factor inputs when explaining growth paths. To be sure, none of these models can give answers to all the questions raised by Romer – questions which are also of immense interest to non-academic economists. However, the research agenda of equilibrium theorizing can now be formulated. And in Romer’s words: “. . . the tools have evolved to the point where growth theory is on the verge of having something interesting to say about growth” (p. 52).

In Chapter 3, *Sanford Grossman* touches on an issue which does not seem to be so new compared to the developments described in other parts of the book (“Rational Expectations and the Informational Role of Prices”). However, the results he derives are striking and very close to Friedrich von Hayek’s analysis on the workings of a market economy. In the rational expectations equilibrium – an extension of the Walrasian system – market participants, even if they come to the market with different information, leave the market with complete information which is the best available. This is due to the informational role of prices which von Hayek described as a mechanism for communicating information, the invisible hand. Therefore, in the framework of Grossman’s model, “. . . the allocations brought about by competitive prices are as if each trader had all the information” (p. 143). The conclusion is that the social planner having complete information cannot do a better job of allocation, i.e., the situation cannot be improved in the Pareto sense.

Several of the chapters could begin with the notion that the particular field under discussion has been tremendously influenced by the new classical theory. Just as growth theory, the theory of consumption which *Robert Hall* surveys in Chapter 4 (“Consumption”) was largely dormant for a long time. Hall’s starting point is one implication of the permanent income hypothesis with rational expectations which implies that consumption follows a random walk. Since this is largely, but not fully supported by the data, there is a puzzle because in some instances changes in transitory income appear to have an effect on consumption. This conclusion can be explained in models with a liquidity constraint on households. Other models are discussed as well, e.g., one that explicitly includes durable goods. The conclusion is – as is with other areas discussed in the book – that a consensus model may still be far away but we have an idea what it might look like.

Robert Barro himself surveys the work on fiscal policy in Chapter 5 (“The Neoclassical Approach to Fiscal Policy”) which is a field he has influenced tremendously since the mid-1970s. A central topic is, of course, the Ricardian equivalence theorem. This theory is described in a formal model and then discussed in the final part of the text which is, by the way, partly similar or even identical to other publications by the author. Barro takes up many criticisms and concludes that the objections regarding the assumptions of households that do not live forever, of imperfect loan markets, of uncertainty about future taxes and the problem of the timing of taxes (i.e., in connection with income taxes instead of lump-sum taxes on which the theorem is based) may be valid. However, they do not necessarily contradict the Ricardian view because their effect on,

say, interest rates or income is often ambiguous. The empirical implications of the model are "... thus far not sharply at odds with the limited evidence that is available ... " (p. 224), and Barro concludes that it would be useful to have more quantitative information about the consequences of fiscal policy. This pertains not only to the purely transitory effects but also to the influence on economic growth, an area to which Barro has also contributed in recent years.

The problem of time consistency and the related discussion on rules versus discretion are the topics of the following Chapters 6 and 7 ("Reputation, Coordination, and Monetary Policy" by *Kenneth Rogoff*, and "Time Consistency and Policy" by *V.V. Chari, Patrick Kehoe and Edward Prescott*). Rogoff analyzes the question to what extent reputation can substitute for legal constraints on macroeconomic policy, an issue which has been discussed in the game-theoretic approach to monetary policy since the seminal work by Kydland and Prescott. Reputation comes into play when the private sector is unsure of the central bank's objectives which may include the preferences regarding policy targets or the costs associated with breaking a commitment. The author describes a model with a number of variations in strategies and discusses other publications to demonstrate the importance of reputational considerations. But despite the progress, "... a number of important questions remain unanswered". In particular, there is a difficulty in defining precisely the equilibrium; in fact, we may often have a multiplicity of equilibria which in turn may imply different prescriptions for monetary policy. Since the aim of this research – and certainly one of its attractive features – is to treat government behavior not as exogenous but to look into the black box, this conclusion is enough incentive for more research.

Chari, Kehoe and Prescott (CKP) discuss the time-consistency problem that emerges if Ramsey equilibria differ from the time-consistent equilibria. They present an overview of various assumptions concerning economic agents and the government and then analyze two of the most common examples of the time-consistency problem, namely capital taxation and government debt. In both cases, the government has an incentive to break previous commitments, i.e., to levy a tax on existing capital and to default on accumulated debt. While it may be optimal to do so – in fact, it usually is optimal because it avoids distorting taxation which is welfare-reducing – it has negative long-term consequences for economic growth by reducing capital accumulation and, possibly, the provision of public goods. Therefore, a society would be better off if the government followed a rule that precludes such policies. However, in the interpretation of CKP, "... society cannot choose between commitment and no commitment" (p. 303). Bad policies can be avoided only, one may conclude, by constitutional changes as suggested by, e.g., James Buchanan and von Hayek.

In the final chapter, *Neil Wallace* describes monetary models ("Some Alternative Monetary Models and Their Implications for the Role of Open-Market Policy"). The existence of money is in all models explained by the non-pecuniary services that money offers. This conclusion is based on the observation that the rate of return on similar assets (such as government bonds) is higher than that on money. By studying the connection between monetary and fiscal policy he shows that open-market operations are not neutral because they affect this yield differential. What this means precisely for modeling business cycles is not explicitly spelled out; however, this finding raises doubt concerning the often made assumption of complete neutrality of open-market operations.

Looking at the articles of this volume, one may ask whether the whole spectrum of new classical macroeconomics is covered. Unfortunately, topics related to international economics are entirely missing. If other theories of business cycles, economic growth, policymaking and so on are questioned in this book, there is definitely also a need to include, e.g., an alternative to the still standard Mundell-Fleming model; issues such as

the equilibrium theory of the exchange rate or the analysis of international policymaking could also have been included in this volume. In this sense, the book offers interesting insights for many but not all of the important policy issues of today. Also, one may miss, of course, contributions by other leading exponents of the new classical approach.

The articles in this book vary in style and substance: While some are more like a survey or an exposition of a theory, others are focusing on very special – often highly technical – problems of particular models. The reader is often confronted with difficulties, e.g., with the idea of multiple equilibria. This is, to be sure, one advantage of the book because the current limits of the new classical approach or of economic theory in general are openly discussed. On the other hand, however, because of this focus on problems, some articles may fail to convince many readers of the attractiveness of the approach and the successes that have been achieved so far. In the introduction, Barro tries to work exactly in that direction and stresses the relevance of the various approaches, which makes it a good guide for reading the book.

Needless to say, this book is certainly a must at universities since it is one of the most relevant collections – possibly the best, anyway – of important and up-to-date essays in the area of new classical theory by various leading economists. Non-academics, however, will find it difficult to understand the advantages of this approach. Certainly, the book was not designed for that purpose, but nevertheless it would be worthwhile thinking of ways to bring the message to politicians as well since new classicals have a lot to say about desirable economic policy.

Joachim Scheide

Don, Henk, Theo van de Klundert, Jarig van Sinderen (Eds.), Applied General Equilibrium Modelling. Dordrecht, Boston, London 1991. Kluwer Academic Publishers. X, 188 S.

In der mathematischen theoretischen Nationalökonomie, die in der Begriffsbildung der Mathematik „angewandte“, da zielgerichtete Mathematik darstellt, wurde formal bewiesen (Arrow-Debreu), daß ein Walrasianisches (neoklassisches) allgemeines Gleichgewichtsmodell unter bestimmten Voraussetzungen (Konkavität der Nutzen-, Konvexität der Produktionsfunktionen) eine Lösung besitzt. Daher ist jedes konkrete Modell dieser Art, bei dessen mathematischer Formulierung diese Voraussetzungen eingehalten werden, zweifellos lösbar. Werden allen ursprünglich unbestimmten Parametern des Modells bestimmte numerische Werte zugeordnet, ergibt sich eine dementsprechende numerische Lösung des Modells. Die notwendigen Lösungsalgorithmen stellt die „praktische“ Mathematik zur Verfügung. Derart numerisch gelöste Modelle werden heute in der Nationalökonomie als „angewandte“ allgemeine Gleichgewichtsmodelle bezeichnet. Dabei ist das Adjektiv „angewandt“ in der Praxis sehr irreführend, weil es dem Sprachgebrauch der Mathematik entnommen ist, dieser Sprachgebrauch aber wohl nicht dem allgemeinen Sprachverständnis entspricht. Im allgemeinen Sprachverständnis dürfte das Wort „angewandt“ weitgehend mit „praktisch“/„realitätsbezogen“ gleichgesetzt werden. Bei „angewandten“ allgemeinen Gleichgewichtsmodellen bezieht es sich jedoch allenfalls auf die Tatsache, daß allen im Modell auftretenden Parametern bestimmte numerische Werte verliehen wurden und das Modell dann numerisch gelöst wurde.¹ Es will jedoch nichts über den Realitätsgehalt des Modells aussagen. Diese Auffassung wird offenbar von allen Autoren des Sammelbandes geteilt.

¹ Weil auch der Beitrag von Keyzer in den vorliegenden Band aufgenommen wurde, werden offenbar auch nicht numerisierte rein theoretische Modelle zu den „angewandten“ gerechnet, was darauf hindeutet, daß es im Grunde nur – wie in der Mathematik – darauf ankommt, daß das Modell auf ein Ziel ausgerichtet ist.