optimal investment policy under restrictions on the installation of investment goods including both irreversibility and the use of internal resources. Chapter 6 in the first volume is a somewhat methodological paper arguing that empirical and theoretical work are best not carried out in isolation. Finally, in light of the recent developments in the theory of investment, his review article (chapter 13) in Volume I – published in the *Journal of Economic Literature* in 1971 – is interesting. There, he compares twelve alternative investment functions on the determinants of desired capital, the time structure of the investment process, and replacement investment. Conclusions are that real output is the most important single determinant of investment expenditures, and internal liquidity is not an important determinant of investment. In this article, Jorgenson concludes that an important open question in the study of investment is the integration of uncertainty into the theory and econometrics of investment. Obviously, and in line with Jorgenson's own arguments, there is a demand for further specialisation of the neoclassical theory of the firm stressing irreversibility of investment decisions and uncertainty of the environment in which such decisions are made. But unlike Jorgenson, I would argue that alternative theories may provide useful alternatives to the neoclassical theory. For instance the role of financing constraints may help explain investment behaviour of firms.

GERARD H. KUPER

University of Groningen

Beyond Microfoundations: Post Walrasian Macroeconomics. Edited by COLANDER (DAVID). (Cambridge and New York: Cambridge University Press, 1996. Pp. xiii+266. £35.00 hardback, US $44.95 hardback. ISBN 0 521 55237 0.)

This is an interesting collection of essays, which purport to define or exhibit a particular school of thought in macroeconomics that Colander calls 'post-Walrasian'. When I received the book it was the first time I had heard the term being used. I was curious to see what it might mean, but also wary because the taxonomy of 'schools of thought' can be a barren exercise (as Otto Klemperer once said of musicologists, they know a lot about the 'ology', but not much about the music). My feelings having read the book are mixed: the material covered is certainly interesting, although whether it deserves putting under one 'post-Walrasian' umbrella is another thing.

First, what does Colander mean by the term 'post-Walrasian'? He says that the distinguishing characteristics of the post-Walrasian (PW) approach to macroeconomics are: (1) multiple equilibria and complexity, (2) bounded rationality, (3) institutions and non-price coordinating mechanisms. The best way to see this is in terms of what he means by 'Walrasian' macroeconomics: the belief in an economy with a unique equilibrium which has simple linear dynamics, in which conventional maximising agents have rational expectations and markets are efficient. One can dispute with Colander here. For example, there is nothing inherently 'Walrasian' about linear dynamics. Linearizing a system is simply a method of solution. Indeed, much of the initial work on endogenous fluctuations and chaos has been made in a Walrasian framework. Likewise, the multiplicity of equilibria is in no way ruled out by assuming perfect competition. However, whilst we might dispute the tightness of Colander's definition, I think we know what he means: orthodox/mainstream macro tends to be Walrasian with a unique equilibrium and agents have rational expectations.

The book contains a collection of essays: some are methodological/history-of-thought; others surveys of recent research areas. Barkley Rosser gives a very useful survey of the literature on chaotic dynamics; John Bryant writes two chapters, one on co-ordination of team production and the other on investment as a co-ordination problem. Clower and Howitt argue that the structure of markets matters (institutions need to be taken seriously): Leijonhufvud argues for not-too-rational agents. Hans van Ees and Hary Garretson look at endogenising the Natural rate and multiple equilibria; Robert Martel considers the issue of heterogeneity and aggregation. Colander develops all of this in a general framework and historical perspective, looking at the policy implications.

There is much that is useful and valuable in this book. However, I want to look in detail at a couple of issues. First, the Ees/Garretson chapter on 'endogenising the Natural Rate'. They take as the point of departure the Phelps structural framework, although the standard Nickell/Layard NAIRU model would have done as well. Their main critique of the structuralist approach is the uniqueness of equilibrium: 'if the existence of multiple equilibria is acknowledged and taken seriously, then a rather different theory of economic behaviour and policy making is required' (page 203). I agree in spirit, but find it disconcerting that the authors are unaware of the work of Alan Manning on multiple equilibria and increasing returns in the UK labour market (*Economic Journal* 1990), along with the theoretical work of several people on a 'Natural range' of equilibria (including Ian MacDonald, V. Bhaskar, and myself in different models). Multiplicity of equilibria is an interesting and under-researched topic: however, I can see no reason for calling it post-Warasian.

For the historian of economic thought, Colander's chapter 'The macro-foundations of micro' is very revealing. He describes the new Keynesian response to the new classical in terms of being '... the equivalent of convincing people that classical birds were really Keynesian antelopes, even though they looked, felt, and chirped like birds'. As I read him, Colander believes that there is a Walrasian approach to modelling which includes the 'traditional microeconomic choice theoretic foundations': new Keynesian approaches that keep this aspect are (implicitly) doomed to fail. I disagree with Colander on this. First, most of the substantive post-Walrasian models in the book stick to the traditional framework: that is certainly true of the work on chaos and sunspots. Whilst there may be multiple equilibria and other weird and wonderful features, all of the models I have seen have optimising agents with perfect foresight or rational expectations. In a sense, it is interesting to see how little you need to depart from the canonical Walrasian model to get these sort of unorthodox results. I think exactly the same goes for models of coordination failures: the presence of strategic complementarity and spillovers detracts not one little bit from the 'traditional' nature of the microfoundations. Thirdly, the issue of bounded rationality is indeed important, and rapidly becoming a central issue in several areas of economics (most importantly in game theory). However, old arguments (Alchian's notion of natural selection etc.) indicate that the Walrasian approach is not incompatible with bounded rationality *per se*.

To me, there is not a 'Walrasian' bundle consisting of inseparable elements. Walrasian simply means the assumption that markets are perfectly competitive: nothing more and certainly nothing less. The Walrasian equilibrium has certain very special properties that arise from its assumption that agents are price takers: most obviously, it is Pareto efficient which implies that in some sense welfare is maximised if there is a representative agent. This is the key assumption that above all others conditions the sort of macroeconomics you get. To me the main objective of macroeconomic research is not to look at non-linear models or bounded rationality *per se*, but first to make the break with the fundamental assumption of perfect competition. It is only then that the world can become really interesting. Oddly enough, the topic of imperfect competition is hardly mentioned by Colander or the other contributors. However, Perry Mehrling does argue that '... the evolution of macroeconomics has ... been driven by the fateful decision long ago to adopt Walrasian equilibrium as the long-run model' (page 80). I think that the decision was largely inevitable given that there was little alternative: it is precisely the task today to build the alternative that is as rich and tractable as the Walrasian.

Whilst I do have this fundamental problem with the approach advocated by this book, that does not mean that much of it is not excellently written, informative, and stimulating: whether or not one believes in the 'post Walrasian' label, the contents are individually well worth looking at.

HUW DIXON

University of York

Fiscal Policy: Lessons from Economic Research. Edited by AUERBACH (ALAN J.). (Cambridge, Mass. and London: MIT Press, 1997. Pp. xii+475. £42.50 hardback. ISBN 0 262 01160 3.)

The eight papers and accompanying comments presented in this book were originally presented at a conference at the University of California held in February 1996. The authors have been encouraged to offer views and to focus on important areas of important and topical issues rather than present neutral and comprehensive surveys. There are two discussants for each paper and this reflects the potentially controversial areas under review. The material pre-