like Meltzer does, as urging a set of policy rules to make subjective expectations consistent with employment-maximising behaviour by private agents. Bateman's argument that Keynes changed his mind on epistemology seems to rest on a confusion between what people believe, and what it is rational for them to believe.

O'Donnell and Carabelli agree with each other that Keynes's epistemological position did not change, but interpret it differently. For O'Donnell, Keynes's probability judgements are true in the sense that formal logic is true, and are anchored in the 'real' world through knowledge of the empirical data, *h*. Carabelli interprets Keynes's probability theory as a 'logic of opinion', or, roughly, a form of social knowledge, relative to 'cognitive circumstances'. The argument between them might seem to turn, in part, on whether the data, *h*, is thought to include belief (or language) systems; Carabelli offering an idealist (Wittgensteinian?) interpretation of Keynes's epistemology, while O'Donnell's is a rationalist–empiricist one. In line with her view of Keynes's epistemology, Carabelli interprets Keynes's critique of 'classical' economics in methodological terms, as based on an assertion of the 'organic interdependence' of social processes; while O'Donnell emphasises the importance of the 'weight of argument' in Keynes's theory of agent behaviour in the *General Theory*. Both contributions are outstanding.

The two books, taken together, are part of the ongoing story of the attempt to reconstruct, not Keynesian economics, but economics, on the basis, on the one hand, of Keynes's social philosophy, and, on the other, of his theory of behaviour. These efforts have so far yielded discordant shafts of light; it is still too early to say whether they will yield 'fruit'.

ROBERT SKIDELSKY

Warwick University

New Keynesian Economics Volume 1: Imperfect Competition and Sticky Prices. Edited by MANKIW (N. GREGORY) and ROMER (DAVID). (Cambridge, Mass., and London: MIT Press, 1991. Pp. x+430. £14.95 paperback. ISBN 0 262 63133 4.)

New Keynesian Economics Volume 2: Coordination Failures and Real Rigidities. Edited by MANKIW (N. GREGORY) and ROMER (DAVID). (Cambridge, Mass., and London: MIT Press, 1991. Pp. x+430. £14.95 paperback. ISBN 0 262 63134 2.)

The publication of a collection of already published papers rarely has a major impact on the economics profession. Occasionally, however, such a collection can meet a need to define and bring to prominence the identity of a body of work as constituting a school of thought. As is apparent from their very title, these two volumes aim to define a corpus of work as being 'New Keynesian'. Mankiw and Romer briefly discuss the meaning of this term in the introduction, and Mankiw has recently written a paper (not included in these volumes)

entitled 'The Reincarnation of Keynesian economics', which is particularly concerned with distinguishing the new from the old. The story Mankiw and Romer tell is that the Keynesian consensus of the 1960s was shaken by the emergence of new classical economics in the 1970s. The twin cornerstones of the new classical approach were the derivation of macroeconomics from firm microeconomic foundations, and an adherence to competitive equilibrium (markets 'clear'). For Mankiw, the ascendancy of the new classical approach seemed complete in 1980. He quotes in his paper from Robert Lucas's article 'The death of Keynesian economics' (*Issues and Ideas*, winter 1980):

> One cannot find good, under-forty economists who identify themselves or their work as 'Keynesian'. Indeed, people even take offense if referred to as 'Keynesian'. At research seminars, people don't take Keynesian theorizing seriously any more; the audience starts to whisper and giggle at one another.

Well, at Chicago perhaps. New Keynesian economics, Mankiw and Romer argue, was the attempt in the 1980s to reformulate the central elements of Keynesian economics (such as nominal rigidities) on firm microeconomic foundations. They pin-point two defining characteristics of New Keynesian economics. Firstly, the view that the classical dichotomy is violated, so that nominal variables such as the money supply can cause fluctuations in real variables. Secondly, that market imperfections are crucial to understanding economic fluctuations, 'Thus the interaction of nominal and real imperfections is a distinguishing feature of new Keynesian economics' (vol. 1, page 2). Mankiw's own viewpoint seems to be that whilst the long-run is entirely classical, the short-run is new Keynesian. For example, he points out that most economists accept the natural-rate hypothesis, and the view that savings generate investment (as evidenced by the resurgence of neoclassical growth-theory).

In my view, Mankiw and Romer's view of new Keynesian economics is far too narrow. It is far too narrow geographically, in that it takes a narrowly American (Bostonian) view of macroeconomics. It is far too narrow historically, in that it fails to recognise work done before 1980. Of course, these two narrownesses are linked: in the 1970s most of the serious work on imperfect competition and macroeconomics was being done in Europe and Japan. I find it incredible that there is not even a reference to Jean-Pascal Benassy's or Takashi Negishi's papers from the late 1970s. Sadly, very few of the many European contributions from the 1980s are cited, and certainly none included in this parochial collection. However, the most important narrowness of their vision is their failure to realise that the significance of imperfect competition goes far beyond a 'Keynesian' short-run. When you replace the auctioneer with wage- and price-setting agents the whole nature and significance of equilibrium changes. Not only will the equilibrium tend to be inefficient, but there is the possibility of multiple equilibria that can be ranked in welfare terms. Imperfect competition opens up the possibility and desirability of effective macroeconomic intervention. The reason for this narrowness of vision stems, I suspect, from a fixation with monetary policy, again symptomatic of

an American perspective. Whilst it has been shown under very general conditions that money will be neutral in imperfectly competitive models (see Benassy, 'Imperfect competition, unemployment and policy', *European Economic Review*, vol. 31 (1987), 417–26, neither referenced nor included), the scope for fiscal policy to have beneficial effects is greatly enhanced. I do not see how a Keynesian short-run can coexist with a classical long-run when you have replaced the idea of competitive market equilibrium with imperfect competition.

The papers collected in these volumes are divided into seven groups: costly price-adjustment; staggered wages and prices; imperfect competition; co-ordination failures; credit rationing; efficiency wages and hysteresis; 'the goods market'. Many of these papers are classics (for example, Akerlof and Yellen's 'Near-rational model of the business cycle', Stanley Fischer's 'Long-term contracts', MacDonald and Solow's 'Wage-bargaining and employment'). I shall restrict myself to a few general comments. The importance of menu-costs has always been exaggerated, in terms of providing an account of macro-economic price-rigidity. At most, menu-costs can explain why individual firms do not adjust their prices in response to small changes in exogenous parameters (nominal demand, wages). We live, however, in an inflationary environment where aggregate price-flexibility is combined with infrequent price changes by individual firms in some markets. How individual price-stickiness can be combined with aggregate price flexibility was ably demonstrated by Caplin and Spulber ('Menu costs and the neutrality of money', included). However, the issue of *wage* flexibility seems to me to be at least as important, if not more so. Ball, Mankiw, and Romer's excellent paper, 'New Keynesian economics and the output-inflation trade-off' (included) finds that when mean inflation is higher, real output tends to respond less to changes in nominal income, which they take as indicating less price-stickiness: as inflation rises, firms change their prices more often. However, I suspect that the emphasis on *price* flexibility in this interpretation is misplaced. Although they dismiss wage-rigidity as 'old Keynesian', their evidence is just as consistent, with nominal wages becoming more flexible as mean inflation rises (e.g. due to indexation). Certainly, in the industrial organisation literature, 'menu costs' have never featured as an important explanation of rigid prices.

Along with 'menu-costs', the other quintessentially 'new Keynesian' idea is that of the coordination problem, as set out by Cooper and John (included). 'Externalities' or 'spillovers' (these terms are used interchangeably in many of the papers) across markets lead to the possibility of multiple equilibria when agents' actions are strategic complements. With imperfect competition, these equilibria can be Pareto-ranked. This develops the old idea that agents might get stuck in a low-level equilibrium, where if they all expanded (output or expenditure) together, they could reach a higher equilibrium where they were all better off. As yet, this is just a theoretical possibility, and one that needs empirical investigation. Alan Manning has tested and found support for a multiple equilibrium model of UK employment (CEPR discussion paper 540, not included), which is promising.

For a long time, the classical (whether 'neo' or 'new') viewpoint has dominated much of our thought. It conceives of markets as competitive, with the economy possessing a unique equilibrium – the natural rate. This viewpoint is based on the combination of a price-taking non-strategic agent operating in competitive markets. This world-view is being overtaken, however, by one in which agents are strategic, and where equilibrium is defined in terms of optimal strategies rather than hypothetical demand and supply curves. In addition to the 'new' industrial organisation and international economics, we can see emerging a new macroeconomics. The appellation 'Keynesian' is perhaps a little too narrow, but it is certainly the case that this new vision will take us far away from the classical viewpoint, both in terms of normative welfare analysis and positive policy analysis. It will also enable us to make more sense of the world we live in. These two volumes bring together some of the most important papers in this new approach. Their publication will, I believe, mark a historic landmark in the emergence of this new approach, in the sense of recognising an accumulation of research, defining its 'image', and providing a reference point for subsequent research.

HUW DIXON

University of York

Methodological Foundations of Macroeconomics. By VERCELLI (ALESSANDRO). (Cambridge and New York: Cambridge University Press, 1991. Pp. £30.00 hardback, US $54.50 hardback. ISBN 0 521 39294 2.)

Alessandro Vercelli's book is a translation from the Italian, with substantial additional material, of his *Keynes dopo Lucas. I fondamenti della macroeconomica* (1987). Vercelli identifies methodology with the 'heuristic model', that piece of the economist's mental machinery that stands on the ridge-line between Schumpter's categories of economic vision and economic analysis, and in methodology between epistemology and practice. Unfortunately, Vercelli cannot quite keep the delicate balance needed to negotiate such a path, and his book slips into three largely independent parts.

Part I is devoted to methodology, largely out of the context of particular applications. Vercelli provides often provocative, and sometimes enlightening, discussions of the meaning of equilibrium, stability, causality, risk, and uncertainty. While some of Vercelli's analysis is first-rate (his criticism of the rational-expectations hypothesis on the basis of its implications for stability and its restrictive notion of rationality should undermine the confidence of any *dogmatic* advocate), too much space is devoted to drawing distinctions and making classifications at a rate that quickly outruns the concrete applications.

Vercelli draws on a broad background of knowledge of mathematics, natural sciences, and philosophy. But this proves to be a weakness as well as a strength. Vercelli appears unable to make up his mind about the reader's background knowledge. Many of the arguments are elliptical or touch only lightly on the