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Review of Jesper Jespersen's *Macroeconomic methodology: a post Keynesian perspective.* Cheltenham (MA): Edward Elgar, 2009, 272 pp., and of Luigi Pasinetti's *Keynes and the Cambridge Keynesians: a revolution to be accomplished.* Cambridge and New York: Cambridge University Press, 2009 [2007], 412 pp.

ROGER E. BACKHOUSE
University of Birmingham
EIPE, Erasmus University Rotterdam

Since its rise to prominence in the late 1970s and early 1980s, many economists have been discontented with 'representative-agent' macroeconomics. Neither its policy implications nor the assumptions on which it is based seem credible in the light of the stagflation of the 1970s; persistent high unemployment—especially in Europe—in the 1980s; the repeated financial crises in many parts of the world during the 1990s; or, of course, the financial crisis of 2007-2008. By constructing models in which co-ordination failures were impossible, a necessary consequence of the combination of far-sighted representative agents (quite apart from the absence of any real role for something as basic as money) the creators of such models seem to rule out the possibility that they will ever explain what drives capitalist economies from one crisis to another. The door would seem to be wide open to new methodologies for doing macroeconomics, such as these two books claim to provide.

Reading either of these books, the macroeconomist is confronted with a clear choice: either to reject virtually all the macroeconomics taught in leading graduate schools today, or to be branded as a 'neoclassical' economist, guilty of all sorts of sins. The 'new Keynesian economics', whether emphasising institutionally based labour market rigidities or imperfections of competition, does not offer a way out, for it is seen by both authors as being essentially neoclassical. This is an uncomfortable position for anyone who is unhappy with recent developments in the discipline to be in. Fortunately, as I will suggest, it is a choice that does not have to be made and should not be made.

Following a number of recent post Keynesian writers, Jespersen finds the unity of post Keynesian economics to lie at the methodological level, in the well-known methodology of critical realism advocated by Tony Lawson. This postulates a stratified reality, in which the deepest layer—of causal mechanisms, power structures, and institutional relations—is covered by two other layers, of events and data respectively. It is interesting to note the contrast with the methodological pluralism that used to be taken as characterising post Keynesian economics (such as Geoffrey Harcourt's 'horses for courses'—see, for example, Harcourt and Hamouda 1988). Jespersen's account is also of interest for the way this methodology is linked to the three worlds of Karl Popper, correctly seen as an opponent rather than a supporter of 'positivism'. I will not question the claim that Lawson's critical realism can fit post Keynesianism well. Indeed, my view remains that it is so elastic that a good case can be made for it fitting virtually any approach to economics, even neoclassical economics.

This orientation leads Jespersen to start his account of macroeconomic methodology by laying out the ontology of post Keynesian economics, for the first stage in a critical realist methodology is 'initial ontological reflection', mapping the 'macroeconomic landscape' (Jespersen 2009, 95). This leads into a discussion of uncertainty and the need to model the economy as a whole, and to an emphasis on path-dependence rather than equilibrium. At the risk of oversimplifying the arguments, I suggest that there are important parallels (that I will discuss later) with Lionel Robbins's well known essay (1932): whereas Robbins claimed to deduce all the facts of economics from the assumption of scarcity, Jespersen manages to deduce the Keynesian notion of effective demand from the 'ontological fact' of uncertainty. Effective demand depicts a causal relationship and hence is part of the deep reality of capitalist economies. The world is inherently Keynesian at a deep level. This is, of course, reminiscent of Marx's claim to be laying bare the realities of capitalist society.

In the interests of brevity, I will not debate the details of Jespersen's argument, which covers much that I omit here. Instead, I propose to discuss the premises on which it rests. The first of these is the claim that one should begin with ontology—with 'the fundamental nature of being and reality' (Jespersen 2009, 130, 2n.). Jespersen offers many arguments relating to uncertainty, but they seem to rest either on a belief that this is the way the world is, or on the claim that the world must be characterised by uncertainty (which is of course sharply distinguished from risk). This is reminiscent of the grounds on which Robbins claimed that the economic world was characterised by choice under conditions of scarcity (which is why I drew the analogy with Robbins earlier on). Whether one considers such insights to be 'intuitions' or deductions from what we observe, how do we distinguish between the Robbinsian and post Keynesian views of reality? Intuitions need to be tested, for there are senses in which both Robbins and the post Keynesians are right even though their intuitions seem to lead in very different directions. Perhaps the problem is starting with ontology: maybe these 'deep' objects that apparently populate the economic world should be seen as constructions arising from our theorising and ontology is the worst place to start.

This leads to the second assumption underlying Jespersen's critical realist methodology. He argues that there is a divide between two traditions:

> 1. Methodological individualism and closed system reasoning [...], theoretically rooted in deductivism and logical positivism.
>
> 2. Socially embedded macroeconomic theory based on open system reasoning with a deliberate affinity to reality (the economy as a whole) (p. 96).

Neoclassical economics falls squarely in the first tradition and post Keynesian economics in the second. This raises two questions. The first is whether the divide is achieved through ignoring work that might challenge it. The obvious recent example is George Akerlof's theorising about individuals as social agents. Akerlof's individuals are socially embedded as are the agents that he and Robert Shiller explore through behavioural methods. Akerlof is absent from Jespersen's index, but I feel safe in conjecturing that he would place him on the neoclassical side of the divide, for many of his theories of social interaction rest on assumptions about individual behaviour. That is the result of the methodology Jespersen employs, according to which ontology is fundamental rather than a construction placed upon an economic theory.

In many ways, Pasinetti's (2009 [2007]) book is very different, both in structure and in aim—Pasinetti is more comfortable than Jespersen in claiming the mantle of Keynesianism rather than qualifying it as 'post'

Keynesianism. Yet Pasinetti shares Jespersen's belief that there is an insuperable divide between neoclassical and Keynesian economics. Even more clearly than Jespersen, his book is about going 'Beyond neoclassical economics', the title of chapter eight, the first chapter in the section where he lays out his own production-oriented approach. The argument is buttressed by accounts of past ideas that argue for a methodological break, first between 'mercantilism' and 'classical economics', represented above all by David Ricardo, and second between 'classical' and 'neoclassical economics'. The classical approach focuses on production, in contrast to the focus on exchange found in both mercantilism and neoclassical economics. What is needed, Pasinetti claims, is to return to the classical approach with its stress on production.

A further parallel is that, like Jespersen, Pasinetti sees a layering of theory, if not of reality, that could easily be expressed in critical realist terminology. I would contend that the new classical macroeconomics and real business cycle theory, the clearest case of representative-agent modelling, can also be defended using critical realist methodology. Like Jespersen, both groups consider technology to be part of what Jespersen calls the deep reality, though of course they part company in that Robert Lucas would add tastes as the source of invariant parameters. Pasinetti, influenced as much by Piero Sraffa and the literature on linear production theory, talks about this layering in terms of a 'separation theorem', stating that we must disengage

> those investigations that concern the foundational bases of economic relations—to be detected at a strictly essential level of basic economic analysis—from those investigations that must be carried out at the level of the actual economic institutions, which at any time any economic system is landed with, or has chosen to adopt, or is trying to achieve (p. 275).

The foundational bases are to be found in Sraffa's *Production of commodities by means of commodities* (1960), albeit modified to allow for technical progress increasing the productivity of labour. It is represented in the classical concern for 'natural' prices, which interestingly are seen by Pasinetti to have a normative dimension.

The classical approach might seem poles apart from the Keynesian: in the early years of post Keynesian economics this was the view of many of those who were adopting the label. However the methodological parallel between Pasinetti's separation theorem and critical realism is clear. Paradoxically, given the absence of both uncertainty and dynamic analysis from Sraffa's *Production of commodities*, in Pasinetti's hands the production approach, which might at first sight seem at odds with Jespersen's focus on uncertainty, leads to similar requirements for good economics: theories must be dynamic and recognise the fact of uncertainty. Keynesian economics can be married to the classical approach.

What concerns Pasinetti for most of his book, however, is not developing this paradigm, but explaining why it was not taken up more widely within the economics profession as a whole. The answer, Pasinetti argues, lies in Cambridge (UK) where there was to be found an array of talented individuals who should have been able to create and propagate the new paradigm. His starting point is, naturally, Keynes. His first two chapters argue that Keynes wanted to break decisively with orthodoxy, but that after Keynes an accommodation with orthodoxy took place. The explanation of why the Keynesian revolution was aborted lies, for Pasinetti, in the Cambridge school itself. The generation comprising Keynes's pupils failed to achieve its potential, either in developing the new paradigm or in training a generation that would take over from them. He develops this theme, with some repetition for the pieces were written for different occasions, in highly readable chapters on Joan Robinson, Richard Kahn, Nicholas Kaldor, Richard Goodwin, and Piero Sraffa (on whom there are essentially three essays, albeit numbered as sections of a single chapter).

After the *General theory*, Pasinetti argues, there was a divide between those followers of Keynes who wanted to break with orthodoxy (Kahn, Kaldor, Robinson, and Sraffa) and those who compromised with it to different degrees (such as Roy Harrod and John Hicks). (Harrod and Hicks, though not Cambridge economists, could have formed a powerful force, Pasinetti argues, had they combined with the Cambridge Keynesians.) Not only were they divided, failing to work together to develop the production paradigm as a basis for Keynesian economics (as for example Goodwin and his Cambridge contemporary and close friend, Richard Stone, failed to work together) but they failed to produce a further generation.

This account is fascinating as an insider's view of Cambridge. Pasinetti is surely right to argue that the sociology of the economics profession is important to an understanding of which ideas prospered and which did not. However, there are puzzling features of his account. It makes Cambridge the centre of the world—indeed, at times Cambridge seems to comprise most of the known world—and ignores the profound transformations that had taken place in economics during and since the Keynesian heyday. Surely, after 1945 it was developments in the United States, in places like Harvard, Princeton, MIT, and Chicago, that determined the path the profession would follow. Cambridge was not without influence (many Americans visited regularly and Cambridge had strong connections with MIT), but it cannot be considered in isolation (or even along with Oxford).

Is it right to argue that Keynes's pupils failed to train a third generation to take over, and that Cambridge was simply given up to the neoclassicals? Before concluding that this is the right perspective, I would want to know more about the generation comprising Robert Rowthorn, Robert Neild, John Eatwell, and those who set up the *Cambridge Journal of Economics*. There is also the paradox that one of the key 'neoclassical' economists at Cambridge was Frank Hahn, who was supervised by Nicholas Kaldor, with a thesis on income distribution in the Kaldor-Robinson 'Keynesian' mould. Hahn was, moreover, a harsh critic of the uses of general equilibrium theory that are rightly criticised here; indeed, he was a staunch defender of Keynesian ideas. The example of Stone, whom Pasinetti discusses, and who had clear connections with Keynes, would seem to call for greater questioning of the divide between neoclassicals and Keynesians, as does Goodwin's ability to engage with neoclassical economists. James Meade, a very significant figure at Cambridge in the 1960s, also needs closer examination, for though a self-confessed neoclassical economist, he was also a long-standing associate of Keynes, having been involved in the development of the multiplier and also having worked closely with Keynes during the Second World War.

Post Keynesian economics is, as many post Keynesians acknowledge, a programme that is in need of considerable development. That makes studies of post Keynesian methodology potentially important. But, despite my doubts about representative-agent macroeconomics, neither of these books persuades me that post Keynesians have yet developed a workable methodology. If what are believed to be insights into economic reality are to be of any use, they need to be operationalised, and this seems not to have happened with post Keynesian economics. Hence, I find myself wanting to know more about what post Keynesians do in practice, rather than their fundamental beliefs about what they believe should be done. There is some of this in both books, though primarily as a statement about how post Keynesians should construct macroeconomic theory. Thus, even though methodological stances have moved on, these two books suggest to me that there is still as great an emphasis as ever on creating an identity through defining post Keynesian economics in opposition to a stylized neoclassical economics as there was in the 1980s when the survey by Harcourt and Hamouda (1988) was published.

To me, the fault seems to lie in the belief that ontology is fundamental and that theorising should begin with analysis 'at the essential level', to use Pasinetti's phrase. It is this that leads naturally to the postulation of a fundamental divide between neoclassical and other approaches. One thing that is interesting about Keynes is that, though he clearly did believe he was fomenting a revolution in economic theory (a belief that seems amply justified by events, even if there is room for disputing the details) he was able to work with both those who became seen as post Keynesians and those who are seen by Pasinetti as having compromised: Meade, Harrod, Stone.

If we do need a new paradigm (and I leave open the question of whether this is the right way to think about the changes that are needed in macroeconomics) perhaps it is something that will be recognised only after the event. That would suggest that it would be more fruitful to start, like the new Keynesians who are dismissed in these books, with looking for new ways to solve problems, postponing discussion of ontology to a much later stage (if it is needed at all). I have no doubt, for example, that Shiller's behavioural approach to Keynesian problems has limitations, yet it is surely worth exploring and does not merit dismissing as simply 'neoclassical'. Similarly, Stiglitz (2010) criticises the post Keynesian focus on uncertainty as opposed to risk, not because he fails to understand the distinction, but because he does not see that it plays any role in explaining the events that led up to the recent financial crisis. (It is interesting to note that, as Harcourt recently pointed out, Stiglitz has claimed that he learned much from Kaldor as a student at Cambridge in the 1960s.) It may be that uncertainty is a feature of the economic world and that any system economists are likely to consider is open—sensible 'neoclassical' economists would not dispute either of these points—but the question is how one analyses such an economy.

The analogy with Robbins made earlier, is relevant because his *Essay* perhaps offers a cautionary tale for post Keynesians. Robbins's approach to economics started with ontology—the belief that the world was characterised by individual agents making choices under conditions of scarcity. This led him to a belief in the primacy of economic theory that was inconsistent with his own belief that empirical work was important. In drawing a sharp distinction, of which I was reminded by Jespersen's discussions of layered reality and by Pasinetti's 'separation theorem', between propositions of permanent significance and ephemeral relationships, he was led to neglect the 'middle ground' of relationships that may not be permanent but last long enough to be important in practice: the territory explored by modern econometrics. Critical realists see as much with their talk of 'demi-regularities', but fail—I suggest—to see its full significance.

If my argument is right, the attempt to find methodological unity in post Keynesian economics may, paradoxically, be a step backwards from Harcourt's potentially more pragmatic 'horses for courses' approach, in which the diversity of methods was celebrated, or the advocacy of pluralism of which Sheila Dow is a representative. These, at least, have the potential to challenge the insuperable methodological divide, postulated by both Jespersen and Pasinetti, between neoclassical and post Keynesian economics. Pluralism might even allow an accommodation with 'neoclassical' economists such as Hahn and Stiglitz who share the post Keynesians' admiration for Keynes.

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Roger E. Backhouse is professor of the history and philosophy of economics at the University of Birmingham (UK) and also at EIPE, Erasmus University Rotterdam (The Netherlands). His most recent book is *The puzzle of modern economics: science or ideology* (Cambridge University Press, 2010).

Contact e-mail: <reb@bhouse.org.uk>

Website: <http://www.socscistaff.bham.ac.uk/backhouse/homepage>