

This paper was never published. The reason is that when Colander, Holt and Rosser read my response, found below, to their 3800-word comment on the symposium on post-Keynesian and heterodox economics that Fred Lee and I had organized and had published in the Review of Political Economy in 2012, they decided to withdraw their comment, which had been accepted in January 2013 and scheduled to be published in the July 2013 issue. By so doing my response, which had been accepted for publication two months later for the same July 2013 issue, became pointless. What is found below is exactly the text that had been edited and accepted in March 2013, with no changes.

How Can Critics so Bright as Colander, Holt & Rosser Be so Misunderstood by Heterodox Economists?*

MARC LAVOIE

University of Ottawa, Canada

ABSTRACT *In their comment on this journal's 2012 symposium on the future of Post-Keynesian and heterodox economics, Barkley Rosser, David Colander & Ric Holt (RCH) offer nothing new beyond their well-rehearsed previous arguments. While there is a grain of truth in what they affirm, they repeat their questionable claim that heterodox economists have shut themselves out of mainstream discourse, and they continue to understate the antipathy towards scholars, ideas and models that do not fit the orthodox hard core. This rejoinder argues that RCH missed the point of the symposium, and that some of their other writings actually contradict their present claims.*

1. Introduction

Here we go again. David Colander, Ric Holt and Barkley Rosser (CHR) in their various permutations have devoted hundreds of pages to the issue of heterodox versus orthodox economics, but they feel that what they have already said is not yet enough. In their short comment on our 2012 symposium on the future of Post-Keynesian and heterodox

* *Correspondence Address:* Marc Lavoie, Department of Economics, 120 University Street, Ottawa, Ontario, Canada, K1N 6N5. Email: mlavoie@uottawa.ca

economics, they manage to provide no less than twelve self-citations out of 16 cited works (Rosser, Colander & Holt, 2013; henceforth RCH).

2. To Be or Not to Be DSGE

While Colander has been for a long time an advocate of the complexity or post-Walrasian approach, in a paper presumably written before the Lehman Brothers default he was telling us that the dynamic stochastic general equilibrium (DSGE) model ‘is well founded, scientific, and potentially more progressive’; he was highlighting the ‘intellectual coherence and mathematical elegance of DSGE models’; and he was concluding that ‘thanks to DSGE we know more than we used to about the economy’, despite having noted a page before that the DSGE standard ‘involves idealizing to a world we *can* fully understand in the hope that understanding this simplified world will help us better understand our own’ (Colander & Rothschild, 2009, p. 126–127).

A year later, Colander (2010, p. 420) offers a rather different assessment of DSGE models, saying that the primary, and ‘highly problematic’, reason for its success was ‘that it was appropriately micro-founded’. The new Colander (2010, p. 424) adds that before the financial crisis economists ‘were well aware of the model’s serious limitations in describing the real-world macro economy’. Well, not all of them were. In 2010, some well-known neoclassical economists were still claiming that ‘any interesting model must be a dynamic stochastic general equilibrium model. From this perspective, there is no other game in town.... If you have an interesting and coherent story to tell, you can tell it in a DSGE model. If you cannot, your story is incoherent’ (V. V. Chari, as quoted by Garcia Duarte, 2012, p. 220).

Sceptical readers, interested in an account of the failings of orthodox macroeconomists and of their response to the global financial crisis, ought to read instead the entertaining and enlightening report of Philip Mirowski (2011).

3. On the Quaintness of Top-Tier Journals

But why is the DSGE model, with its serious limitations, the workhorse of modern orthodox economists? Why aren't a great variety of macroeconomic models being published in our top-tier journals? According to the new Colander (2010), this is because of the ruthless approach to gate-keeping adopted by mainstream economists heading journals. This he says, not once, but on four occasions in his more recent article:

- 'The problem was that the DSGE model ... was the only model being used, and that too many researchers felt that they had to use it' (p. 421).
- 'Too many macroeconomists felt that if they did not toe the DSGE line, they were unlikely to be published in journals that would lead to their advancement' (p. 420).
- DSGE models 'became dominant not because they shed the most light on macroeconomic policy issues, but because they were tractable, and working on them was most likely to lead to publications that would advance an economist's career' (p. 424).
- Models that could incorporate the possibility of crises 'were difficult to publish in top journals because those journal were committed to a certain type of so-called micro-grounded DSGE models as the only legitimate approach' (p. 420).

Despite all this, RCH (2013, fn. 9) insist that there is little ideological gate-keeping in orthodox journals and, indeed, that as regards ideological bias, the editors of heterodox journals are worse than those of mainstream journals! In support of this latter claim, they offer nothing more than a vague allusion to their own experience.

Katarina Juselius, who works with theory-agnostic cointegrated vector autoregressive (CVAR) models and was one of the co-authors of Colander et al. (2009), points out that ‘it is hard to convince an editor of a US top journal to publish empirical results that have been found by a careful CVAR analysis. The immediate reaction to such results is mostly that CVAR results are not consistent with the results of the DSGE theory models, hence they must either be wrong or irrelevant’ (Juselius, 2009, p. 11). If the work of well-known econometricians like Juselius is routinely dismissed because it gives results that conflict with the conventional paradigm, what leads RCH to think that heterodox ideas repackaged in mainstream guise can make a dent?

4. On the Lack of Practical Advice for Research

RCH (2013) suggest that heterodox economists should pursue a Trojan Horse approach, taking up a suggestion of Earl & Peng (2012). But Earl & Peng advocate this strategy mainly for teaching purposes. Aside from CHR’s positive pieces of advice that I discussed in my symposium paper, what does the Trojan Horse strategy mean *concretely* for *research*? Barkley Rosser (2000, p. x) would perhaps advise us to make use of non-linear dynamics, so as to be more successful on our attempts ‘to shake and stir the economics profession’. Rosser (2000, p. ix) however himself admits that his book manuscript on non-linear dynamics was turned down by thirteen publishers before it

finally got accepted, a fact which does not bode well for heterodox non-linear dynamics. Ric Holt (2007, p. 101) contends that heterodox economists ought to engage with orthodox economists, since there are some elements of a common language; the example he gives is the general agreement that saving and investment tend to equalize. But how far will that get us? Paul Krugman himself has a hard time having a conversation with fresh-water economists, and his solution to present woes is to embrace a framework—the IS-LM model—that most heterodox economists reject.

David Colander has in fact provided some concrete—but not very promising—advice for Post-Keynesian economists. Identifying himself as a ‘Post Keynesian fellow traveler’, Colander (2001, p. 375) was more than a decade ago already claiming that Post-Keynesians were doing a poor job at marketing their ideas. His ‘blunt’ advice at the time was that ‘in today’s environment you can’t market the term “effective demand”—you *can* market the term “effective supply”’. What does that mean? According to Colander (2001, p. 380), ‘expectations of demand become self-fulfilling’, so the supply decisions of firms are the key. What kind of expectations would that be? Evidently, he had rational expectations in mind: ‘model consistent expectations are reasonable’ (Colander, 1996, p. 8). His other suggestion (*ibid.*, p. 9) is to add an extra component to the standard production function—a coordination component—which will affect expectations.

Imagine what Post-Keynesian economics would look like if Colander’s advice had been followed. The recession of 2001 and the Great Recession would both be explained in terms of self-fulfilling expectations, coordination failure and effective supply!

5. Tango Dancers Have always Been in Scarce Supply

RCH (2013) wonder why ‘something so right as heterodox economics [has had] so little influence’. But one could respond with similar questions. Why has an idea as bright as the tax-based incomes policy (TIP) that Colander defended as a young scholar had no influence at all on policy? Why did Colander’s battle to change the name of the aggregate demand curve in textbooks come to nothing. Why did Colander’s efforts turn out to be in vain when he sought to get economists to address the sterility of US graduate programs in economics? The answer, if I may speak for the contributors to our symposium, is that it takes two to tango.

RCH (2013) say that ‘heterodox economists have not been doing well institutionally over the last decades’. But they never did well, essentially for ideological reasons. Veblen, Means, Tarshis, Sweezy, the Marxists, all of us, never had it easy (Lee, 2009). It seemed for a time, during the Cambridge capital controversies, that cutting-edge orthodox researchers in top schools paid attention to what was being done by some heterodox economists—Joan Robinson and the Sraffians. But this was a fluke: it just turned out that, at the same moment, both groups of economists were interested in multi-sector linear models. Getting a position in an economics department forty years ago was easier for a non-conventional economist because at the time the mainstream was receptive not only to Keynesian modes of thinking but also to the idea that market mechanisms can produce dysfunctional outcomes. This changed abruptly with the monetarist counter-revolution and the rise of new classical economics. When Margaret Thatcher and Ronald Reagan came to power, the climate for alternative views hit the ice age. In addition, while many economics departments have shrunk over the years,

presumably because undergraduate students have been discouraged by the discipline's lack of relevance, the number of PhDs in economics has increased drastically, with a large number of graduates, heterodox and orthodox, being unable to land tenure-track academic jobs.

The dice became loaded for good when quality-weighted journal article metrics became the fashionable way to measure the 'productivity' of an academic economist. Frederic Lee quickly identified the dangers of such measurements for pluralism and eclecticism, criticizing the British research quality assessment exercise, which became a template for many other countries, and predicting that it would bring to an end the hiring of academics holding alternative views (Lee & Harley, 1998). At the time, Lee was accused of not playing ball, of lacking diplomacy and of endangering communication between orthodox and heterodox economists. But Lee's assessment of the evolution of academic economics as a consequence of these biased ranking exercises, in Britain and elsewhere, turned out to be fully prescient, as Colander (2010, p. 425) recognizes. This did not happen for lack of engagement, as CHR sometimes imply: in all countries—in the UK, in France, in Italy—heterodox economists did participate in these journal or department ranking exercises, trying to tame the tide.

6. Definitions in Disarray

I end my reply with two remarks regarding the comment by RCH. It is a bit disconcerting to see that RCH attach so much importance to defining who is a heterodox economist and who is not, and also to determining the relative importance of the heterodox schools of thought, all the while insisting that the priority should be to defend heterodox ideas rather

than affirm one's identity as a heterodox economist. RCH (2013) have some qualms about Lee's definition of heterodox economics because it would exclude neo-Austrian economists. Whether neo-Austrians should be considered heterodox economists is an open question. In earlier work I have identified what I consider to be the common presuppositions of heterodox schools of thought in economics (Lavoie, 1992, 2011). The neo-Austrians would fulfill at most two of the five heterodox presuppositions so identified, so I think it is fair to exclude them for historical and methodological reasons, as a number of historians of thought do (Wrenn, 2007).

Secondly, RCH (2013) make a complete mess of my careful attempt to establish a taxonomy that would provide a common understanding of the distinctions made by CHR, Lee, Tony Lawson, John Davis and others. I drew a sharp distinction between heterodox economists on the one hand and all non-heterodox economists on the other; the latter group were by definition orthodox economists. But, following Roger Backhouse, I also recognized a distinction between dissenters and mainstream economists: all heterodox economists are dissenters, as are some eclectic and cutting-edge orthodox economists; the rest of orthodox economists—those who defend the conventional views that are imbedded in textbooks—belong to the mainstream. The diagram I presented was meant simply to present this conceptual breakdown (Lavoie, 2012, p. 322).

I never imagined that anyone would use a ruler to measure the size of each of my rectangles so as to estimate my presumed assessment of the relative importance of these various groups, as RCH do (2013, Table 1). This is a preposterous exercise. Just as ludicrous are RCH's own guesstimates of the relative significance of the various categories of economists, which they admit have no basis in empirical data! Moreover,

RCH seem to mix up their own taxonomy with mine so that their numbers do not add up. In my nomenclature orthodoxy and heterodoxy, by definition, add up to 100%; in RCH's table, the sum in the first column is 95%. The dissenters and mainstream categories must also add up to 100% by definition; RCH add these up to 90%. For all the care RCH took to measure my rectangles, they did not bother to make sense of my definitions.¹

7. Conclusion

Twenty years ago, like CHR, I wrote that 'neoclassical economics appears to be fragmenting' (Lavoie, 1992, p. 422). I also claimed that heterodox economists are 'developing common concerns', 'empirical work in post-Keynesian economics is now more prevalent than ever', and 'neo-Marxians and post-Keynesians alike now indulge in mathematical formalization'. I added that 'the development of non-linear mathematics and non-linear models ... has made many post-Keynesian concepts and ideas easier to justify and more amenable to formalization' (ibid., pp. 422–423). All this however has not been enough for Post-Keynesian or heterodox economics to make progress in academia.

CHR's explanation, in a nutshell, is that orthodox economists have not shown enough tolerance, while heterodox economists have done a bad marketing job and 'are shutting themselves out' (Colander, Holt & Rosser, 2010, p. 404). But as pointed out by Gary Mongiovi (2012, p. 207), the retrenchment towards heterodox journals and heterodox conferences only occurred after it became nearly impossible to have a

¹ It is worth noting that Colander (2010, p. 425) previously got it right when he spoke of 'heterodox economists and other dissenting economists'.

conversation with orthodox economists. The conversation disappeared for various reasons: ideological reasons, the trend towards hyper-formalization, and the hyper-specialization of fields within economics. What the paper by Earl & Peng (2012) that RCH liked so much argues is that a successful marketing strategy requires taking on board the essence of neoclassical theory—in Lakatosian terms, accepting its core elements while fudging around in the protective belt. Behavioral economics is a good example. The original behavioral economics *à la* Herbert Simon was discarded because it rejected optimization. New behavioral economics became fashionable because its adherents were careful to retain optimization in some form, either as the main behavioral rule or as the normative benchmark by which to assess the results of the heuristics and bias program (this is nicely discussed by Sent, 2004, and by Berg & Gigerenzer, 2010).

CHR probably have a point when they say that heterodox economists have made tactical and strategic mistakes.² Who has not? But this is like the mote in the eye. The increasing over-specialization of economists and the trend towards hyper-formalization that make communication difficult between various fields and various schools of thought, along with the widespread adoption of biased bibliometrics in assessing research, are much more important causes of the difficulties encountered by heterodox economics. But the exclusionary practices of too many orthodox economists are no doubt the most crucial factor.

RCH (2013) fail to get the main message of our 2012 symposium when they

² Indeed, CHR, in their various writings, never say who they have in mind when they criticize heterodox economists and their strategies; are they targeting all heterodox economists other than themselves? (Holt and Rosser, notwithstanding their discomfort with the heterodox label, have occasionally identified themselves as such.)

exhort heterodox economists ‘to go beyond attacking the orthodoxy’. Our main message was precisely that heterodox economics has already moved well beyond that stage. Heterodox economics stands on its own, has its own theories and fields of investigation, and does not need orthodox theory as a benchmark.

References

- Berg, N. & Gigerenzer, G. (2010) As-if behavioral economics: neoclassical economics in disguise?, *History of Economic Ideas*, 18, pp. 133–166.
- Colander, D. (1996) Overview, in: D. Colander (Ed.) *Beyond Microfoundations: Post Walrasian Macroeconomics* (Cambridge: Cambridge University Press).
- Colander, D. (2001) Effective supply and effective demand, *Journal of Post Keynesian Economics*, 23, pp. 375–382.
- Colander, D. (2010) The economics profession, the financial crisis, and method, *Journal of Economic Methodology*, 17, pp. 419–427.
- Colander, D. (2011) Is the fundamental science of macroeconomics sound?, *Review of Radical Political Economics*, 43, pp. 302–309.
- Colander, D., Holt, R. P. F. & Rosser, J. B. (2010) How to win friends and (possibly) influence mainstream economics, *Journal of Post Keynesian Economics*, 32, pp. 397-408.
- Colander, D. & Rothschild, C. (2009) Complexity and macro pedagogy: the complexity vision as a bridge between graduate and undergraduate macro, in: G. Fontana and M. Setterfield (Eds) *Macroeconomic Theory and Macroeconomic Pedagogy* (Basingstoke; Palgrave Macmillan).

- Colander, D. et al. (2009) The financial crisis and the systemic failure of academic economics, Kiel Institute for the World Economy, working paper No 1489.
- Earl, P. E. & Peng, T. C. (2012) Brands of economics and the Trojan horse on pluralism, *Review of Political Economy*, 24, pp. 451–468.
- Garcia Duarte, P. (2012) Not going away? Microfoundations in the making of a new consensus in macroeconomics, in: P. Garcia Duarte & G. Tadeu Lima (Eds) *Microfoundations Reconsidered: The Relationship of Micro and Macroeconomics in Historical Perspectives* (Cheltenham: Edward Elgar).
- Holt, R. P. F. (2007) What is Post Keynesian economics, in: M. Forstater, G. Mongiovi & S. Pressman (Eds) *Post Keynesian Macroeconomics: Essays in Honour of Ingrid Rima* (London: Routledge).
- Juselius, K. (2009) Special issue on using econometrics for assessing economic models – an introduction, *Economics, The Open-Access, Open-Assessment E-Journal*, 3, pp. 1–20.
- Lavoie, M. (1992) *Foundations of Post-Keynesian Economic Analysis* (Aldershot: Edward Elgar).
- Lavoie, M. (2011) History and methods of post-Keynesian economics, in: E. Hein & E. Stockhammer (Eds) *A Modern Guide to Keynesian Economics and Economic Policies* (Cheltenham: Edward Elgar).
- Lavoie, M. (2012) Perspectives for Post-Keynesian economics, *Review of Political Economy*, 24, pp. 321–335.
- Lee, F. S. & Harley, S. (1998) Peer review, the Research Assessment Exercise and the demise on non-mainstream economics, *Capital and Class*, 66, pp. 23–51.

- Lee, F. S. (2009) *A History of Heterodox Economics: Challenging the Mainstream in the Twentieth Century* (London: Routledge).
- Mirowski, P. (2011) 'The spontaneous methodology of orthodoxy, and other economists' affliction in the Great Recession', in: J. B. Davis & D. W. Hands (Eds), *The Elgar Companion to Recent Economic Methodology* (Cheltenham: Edward Elgar).
- Mongioli, G. (2012) 'Orthodoxy, heterodoxy, and Post-Keynesian economics: notes on taxonomy', in: F.S. Lee & M. Lavoie (Eds) *In Defense of Post-Keynesian and Heterodox Economics: Responses to their Critics* (London: Routledge).
- Rosser, J. B. (2000) *From Catastrophe to Chaos: A General Theory of Economic Discontinuities*, 2nd edn (Boston: Kluwer Academic Publishers).
- Rosser, J. B., Colander, D. & Holt, R. P. F. (2013) 'How can something so right as heterodox economics have so little influence?', *Review of Political Economics*, this issue.
- Sent, E.-M. (2004) 'Behavioral economics: how psychology made its (limited) way back into economics', *History of Political Economy*, 36, 735–760.
- Wrenn, M. V. (2007) 'What is heterodox economics? Conversations with historians of economic thought', *Forum of Social Economics*, 36, pp. 97–108.