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Methodological Problems in Macroeconomics: Curriculum and Computers[1]

John Smithin, York University[2]

Introduction

The financial crisis of 2008, and the subsequent worldwide economic depression and continuing dislocation, have made little or no impression on the way macroeconomics is taught at the university level, from Economics 101 through graduate school. It has been "business as usual", which (it seems to me) means an almost studious avoidance of any attempt to acquire knowledge of how monetary economies actually work.

Robert Skidelsky (2009) was moved by this situation to suggest removing the responsibility for graduate instruction in macroeconomics from economics departments altogether, and locating them in some other academic unit such as history, philosophy, sociology, political science, or elsewhere. However, I don't think that this solution would work, because if there are indeed serious issues with the way in which economics is taught in academia, the same is true of all the other disciplines. They all have their own biases and hang-ups, driven as much by academic politics as scholarly inquiry.

I would prefer to see a dedicated 18-month progamme such as a *Master of Monetary Macroeconomics* or *Master of Macroeconomic Policy*, on the model of current *Masters of Business Administration* (MBA) programmes. In an MBA programme "business administration", itself, is supposed to be the main object of the exercise, and the other courses the students take, such as accounting, finance, marketing and so on, are definitely cast in the mold of "service courses".[3] In a similar way, the curriculum of the proposed MMM or MMP should be driven by

bona fide macroeconomists, who almost by definition, if they really are *bona fide*, will be philosophically, historically and politically informed, but whose main interest is in the field itself. Additional courses in economic sociology, history or philosophy would be prominent in the curriculum, but would recognizably be service courses, on the model of the current MBA and other professional graduate degrees.

The new MMM or MMP would be more focused than current programmes in *Public Adminstration* or *Public Policy,* which have to deal with the whole spectrum of public policy. It would have a completely different subject matter to the graduate courses in "economic policy" currently offered by economics departments, which, as I think everyone knows (except perhaps the unsuspecting students), are simply "mathematical economics lite".

So, why is that mainstream macroeconomics (as we shall call it) is in such a state that serious writers (and serious speakers at this conference) suggest it should be done away with altogether? I will identify a number of methodological problems that beset the subject as currently practiced, and suggest ways in which these problems might be resolved. I will argue that the same solutions would benefit equally teaching and research, and ultimately public policy itself.

Methodological Problems in Mainstream Macroeconomics

There are four major methodological problems which need to be identified and addressed before any remedies may be suggested.

The first of these is simply the basic premise of neoclassical *micro*economic theory, that beneath the "veil of money" economic activity is fundamentally a question of barter exchange.

This is “the barter illusion” in the unforgetable words of Dillard (1988). The focus is inevitably on what Keynes called the *barter economy*, and money and credit take a back seat.

A second and similar problem is the virtual identification of the term “economy theory” with the use of differential and stochastic calculus to solve the optimization problems of the “representative agent”, or agents. This can be called the *Microfoundations Delusion*, following King (2012). This is what I wrote for the jacket copy of John King’s book:

> The “illusion” has been with us for a very long time whereas the “delusion” is of relatively more recent vintage. Together they have blocked mainstream macroeconomics from achieving a basic understanding of monetary and macroeconomic phenomena at a time when this is most urgently needed.

Calculus is the mathematics of infinitesimal change. So, perhaps the underlying idea for this (if not its full brain-stultifying application in contemporary graduate schools) goes back at least to Alfred Marshall’s motto *“natura non facit saltum”* (nature does not make leaps) from the front-piece of the *Principles of Economics* (1890). But Marshall’s motto is deeply unsuitable for the social sciences on all counts. In the first place, it is clear that in the real world *natura facit saltum* all the time. Secondly, we are *not* dealing primarily with *natura* or the so-called “brute facts” - although it is true that “physics envy” (Mirowski 1989) is certainly an important factor in the sociology of the economics profession - but primarily with the social world. This has an entirely different ontology in which literal “revolutions” in the political sense of the term are simply par for the course (Smithin 2009).

This mention of social ontology brings up the third serious methodological problem to be noted, which is the reliance on statistical probability theory as the main empirical method in economics, under the label of econometrics (*aka* “economy tricks”). Strictly speaking, the theorems of statistical probability theory do not apply in the social world, and this was a point

that Keynes had already made quite clearly as early as the *Treatise on Probability* (1921). Later, when Keynes (1937) was asked to sum up the contribution of his *General Theory* in the famous *Quarterly Journal of Economics* article, great stress was laid on the presence of fundamental uncertainty in the socio-economic and politico-economic environments, rather than probabalistic risk.[4] For further discussion about the different ontologies of the social world and natural world, the reader can consult Searle (2010), for example, on *Making the Social World*, and Smithin (2013a) on "the requirements of a philosophy of money and finance" in Harcourt and Pixley, eds., (2013).

Fourthly, and finally (perhaps also in the sense of decisively), there is the incoherence of any and all attempts that have been made at deriving "capital theory". This is true, in particular, of the efforts of the Austrian school and the mainstream school, but there is really no satisfactory treatment of the concept of capital by any economic school. On this topic, we could do no better than consult Geoff Harcourt's (1969) summary of the issues in his classic *Journal of Economic Literature* article. According to Harcourt's mentor Joan Robinson (writing earlier in the 1950s) and as quoted by Cohen and Harcourt (2003, 201):

> . . the production function has been a powerful instrument of mis-education. The student of economic theory is taught to write $Q = f(L, K)$ where L is a quantity of labour, K a quantity of capital and Q a rate of output of commodities. He is instructed to assume all workers alike, and to measure L in man-hours of labour; he is told something about the index-number problem in choosing a unit of output; and then he is hurried on to the next question, in the hope that he will forget to ask in what units K is measured. Before he ever does ask, he has become a professor, and so sloppy habits of thought are handed on from one generation to the next.

The sloppy habits of thought have persisted, and continue to vitiate any type of economic theory derived on the basis of capital-theoretic arguments. In the real world, "every banker and every commercial man knows that there is only one kind of capital, and that is money". This is the

comment that Mitchell Innes (1914, 355) made on this very point, exactly one hundred years ago.

Is Anything Worth Salvaging from the Wreckage?

We need to pause at this point to ask just what is left of the programme of the mainstream school, and I suppose that an honest intellectual answer would be "not very much". But this can be a tough sell, in particular to people who have already invested the time and energy in learning the various techniques (including myself). I think that the above wording makes enough caveats to cover these potential objections. The criticism is of econometrics as the *main* empirical method, the *virtual* identification of economic theory with differential calculus, and so on. These things are hardly likely to disappear entirely. For example, if one goes on to suggest a greater use of numerical methods as one option (which I will do), a possible source of trial values or starting values for the parameters would be econometric exercises. (The emphasis would then be on the words "trial" and "exercise"). Similarly, in a detailed process analysis, it is always possible to work out "long run multipliers" by calculus, but what is really important is the process itself.

Having made these concessions, it is also reasonable then to point to the opposite danger, that of making so many caveats as to return to the *status quo*.

What is the Correct Methodology for Monetary Macroeonomics?

As a counterpoint to the seemingly insuperable problems facing the economics profession, in its less than half-hearted attempt to study monetary phenomena, we can also list four possible alternative methodological principles which might help us to do better. There is not necessarily a strict one-to-one correspondence between the problems that have been identified and the solutions for them, so each of the solutions is simply listed by the numbers (i) to (iv).

The first correct principle is (i) *to use explicitly macroeconomic methods*. Simply put, the nature of money both necessitates and justifies a macroeconomic approach.

Secondly (ii) *restrict attention to relatively small models of both closed and open economies*, the former being useful primarily for theory and the latter for practical policy analysis. Relatively small models are required to ensure that the logic of each approach is fully comprehensible, both to those who are constructing the models and those who are using them. There should be no "black boxes".

Thirdly, and very importantly, we should (iii) *take seriously the notions of endogenous money and bank credit creation.* Although there are currently various competing claims being made about scholarly priority on this issue, my own view is that this is one of the main *collective* contributions of the various heterodox schools of monetary economics, such as Post Keynesian theory – in both its horizontalist and structuralist versions - circuit theory, modern money theory (MMT) and others.

The last principle is (iv) to "*make use of only two fundamental units of quantity, namely, quantities of [real] money-value and quantities of employment*" which is a quote from Keynes's *General Theory* (1936, 41). This avoids the quagmire of capital theory. The interpolation in the quote is necessary because Keynes actually talked only about *money* values rather than *real* money values, which left hostages to fortune in the shape of later accusations about "money illusion", and so forth. The change simply recognizes the standard modern concepts of the national income and expenditure accounts, and aggregate price indices.

Is There a Better Way?

A few years ago, in my *Money, Enterprise, and Income Distribution* (2009, 56) I explicitly argued that one way forward is simply to take a step back, and return to practice of monetary macroeconomics in the style of writers such as Keynes, the *later* Hicks, and the Post Keynesians.

But, having said this, does it mean that there is *nothing* that the methods and technology of the twenty-first century can add? Of course not, and this is where the idea of "computers" comes in. I once attended a conference in the mid-1980s (in fact organized by Omar Hamouda, Bernie Wolf, and myself) in which one of the speakers put forward the attractive notion of a modern-day Keynes "surrounded by a high-tech 'circus'[5] with .... personal computers .... (and) ... current problems being attacked by (his) intuition .... aided by current technology" (Bodkin, Klein and Marwah 1988, 9-10). The fact of the matter is, however, that in the 1980s the relevant technology was *not* yet available. I had already made an attempt in this direction myself in 1982 (Smithin 1982), and the computers of those days could hardly be called "high-tech".

Now, in the twenty-first century, the situation is quite different, as can be seen, for example, by studying the path-breaking work by Wynne Godley and Marc Lavoie (2007) on *Monetary Economics*. Therefore, these days, there need be no hesitation in suggesting that ***non-stochastic computer simulation methods in discrete time*** can provide (a), a theoretical method that can handle fundamental uncertainty and (b), an empirical method based on the principle of *abduction*, rather than induction. The concept of "reflexivity", discussed in depth at yesterday's breakfast panel, is also highly relevant to the development of a new empirical method. In effect, the argument is that numerical methods are able to satisfy all of the methodological *desiderata* put forward in the previous section.

Tony Lawson (1997) has discussed the topics of both abduction and fundamental uncertainty, from a philosophical point of view, in his influential book *Economics and Reality*.

Smithin (2013b) and Smithin and Zhou (2014) have shown how the suggested methods might be put into operation in both the research and teaching contexts.

Conclusion

It might be best to end with a quote from Keynes[6] himself, in a letter to George Bernard Shaw, of all people, written on January 01, 1935:

> … to understand my state of mind … you have to know that I believe myself to be writing a book on economic theory which will largely revolutionise - not, I suppose, at once but in the course of the next ten years - the way the world thinks about economic problems. When my new theory has been duly assimilated and mixed with politics and feelings and passions, I can't predict what the final upshot will be in its effects on action and affairs. But there will be a great change, and, in particular, the Ricardian foundations of Marxism will be knocked away … I can't expect you, or anyone else, to believe this at the present stage. But for myself I don't merely hope what I say - in my own mind I'm quite sure.

In the end Keynes did not really, and certainly not permanently, change the way the world thinks about economic problems. Otherwise we would hardly be in the mess we are today. Nor did he knock away the Ricardian foundations of Marxism. These remain potential tasks for the current generation of young scholars. However, the tasks are unlikely to be fulfilled if these scholars continue to have to learn precisely the *wrong* set of principles and attitudes in the "hothouse" atmosphere of the graduate schools of contemporary academia.

Notes

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2. John Smithin is Professor of Economics in the Department of Economics and the Schulich School of Business, York University, Toronto, ON, Canada, M3J 1P3; tel: +1 (416) 736 2100, ext. 33623; e-mail: jsmithin@ yorku.ca.

3. It is ironic that the self-described "business educators" invariably treat macroeconomics *itself* as just such a service course. This is another very strong reason for the more important discipline to strike out on its own.

4. These are issues that Paul Davidson (*e.g.* 2009) has been stressing for many years.

5. The "circus" was the name given to the small group of Keynes's closest followers in Cambridge in the early 1930s.

6. This appeared for many years on the back cover of a paperback edition of the *General Theory,* put out by Harcourt Brace in 1964, and widely-used by students at the time.

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