Comment on "Axel in Wonderland: DSGE"

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"Axel in Wonderland" is Professor Leijonhufvud's effort to make sense of the research that is currently undertaken in Europe's (and elsewhere) leadings central banks as they cope with the worse financial crisis since the great depression. He finds their continued focus on Dynamic Stochastic General Equilibrium (DSGE) models at least perplexing. In order to understand why this is the case we need to consider these developments in relation to his own background, and he himself makes a remark on this when discussing the history of the discipline. He writes:

"It is often useful to think of the History of Economic Thought as a growing decision tree. We have arrived at the present state of the subject through decisions made by prior contributors who have persuaded the entire economics profession, or some especially influential segment of it, to take a particular sequence of forks in preference to what seemed the alternatives at the time. DSGE is a branch of the tree that prospered abundantly up until the crisis. Here we are posed with a choice between two of its outermost twigs. (I find myself perched on another branch altogether.)" (page 1-2)

The parenthesis reveals the author's characteristic modesty, as he is not simply perched on another branch, but his work altogether forms another branch of this almost 250 year old oak. What is more, where one is 'perched' informs his/her perspective on the developments in DSGE modelling that consume so much energy and talent draining resources from the rest of the tree.

The two twigs that Professor Leijonhufvud considers in some detail are modifications within the DSGE world that aim to make these models deal better with issues in the labour market, and especially deal with the problem of unemployment. These modifications are: 1) Labour market frictions. These frictions can be due to a host of issues that include labour unionisation, or information asymmetry, or problems with incentives, etc. 2) "Preference shocks that shift the marginal disutility of labor" (page 2). While I concede that (1) has been a broader and more widely studied avenue of research among the scholars working in this field, in the rest of this comment I will concentrate on (2) because I think it has interesting, and often ignored, ramifications for this broader research project.

Let me start by providing some context. DSGE modelling is part of a broader program within economics that has tried, since the 1970s, to introduce robust microtheoretical foundations in macroeconomic models. Starting with the work of Robert Lucas the two pillars of this program[2] have

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[2] I am here taking a very broad view, as there are important and substantial technical, ideological and other differences between the Rational Expectations models of Lucas, the Real Business Cycles models following Kydland and Prescott and the later literature on New Keynesian modelling. But these two pillars are shared by all these programs and their substantial progeny.

been that any macroeconomic behaviour of the economy must be directly linked to individual actions, and that individual behaviour follows the rationality axioms that are the bedrock of neoclassical microeconomic theory. Because of these two pillars the theoretical, even scientific, superiority of this research program was proclaimed over other programs that reputedly introduced *ad hock* assumptions that lacked 'true' theoretical basis when their models faced empirical difficulties. In this research programme the most widely used models assume that you have a typical individual, or a typical household, and that the world is a multiple of this household. This creates a direct link between individual (or household) action and macroeconomic behaviour. And since these individuals are rational and operate in a general equilibrium world, they always respond in a way that maximises their welfare, or, as economists call it, their utility. The problem that this individual continuously faces is *unforeseen* shocks, which make him/her alter their behaviour to cope with these changes that unavoidably alter their current and future plans. Thus, the world around these agents continually changes, and they have to change their rational decisions across a number of variables (consumption/saving, work/leisure, portfolio choices etc.) to take account of the new environment. These unforeseen events where, in the first generation Real Business Cycle models in the 1980s, technology shocks, but they later came to include other types of shocks, even government induced ones. Over the last 10 or so years, preference shocks have surfaced occasionally as possible modifications in this field's efforts to overcome discrepancies between what the models predict and real data.

Thus it is not surprising that preference shocks are again considered, this time in relation with labour market issues and unemployment. What I do find surprising is that researchers working in the DSGE mould find this avenue a possible way forward. That is they find this to be a modification upon which further growth on this field can take place. For the sake of argument let us assume that a DSGE model with a type of preference shock that relates to the labour-leisure decision of this neoclassical representative individual, produces simulated results for this economy that mimic, very closely, the real data that we have. Would this be a success for DSGE modelling? Or to put it in less loaded terms, what would this mean for the DSGE branch of economics?

I think we are faced with an interesting conundrum. On one side we have come up with a model that does very well by the metrics of success that this research program uses, which is the comparison of simulated to real data. On the other side this apparent success comes with utilising a type of shock that at least appears to contradict one of the pillars of the broader research program. This is because preference shocks suggest that this representative individual is irrational, at least with respect to his labour decision, in that he takes unforeseen actions (unforeseen to *himself*) that violate his rationality and have no clear cognitive or other reason. He then uses all his –substantial- rational machinery to compensate for this unforeseen change in his environment, brought about entirely because he changed his mind on how much labor to provide. If indeed this is a good description of how individuals behave in reality, I wonder what the general message from this is. To claim that this is a step towards some type of behavioural economics, or bounded rationality, seems a bit of a stretch as we have no complex (or simple) clearly articulated explanation why this individual displays this (or any) type of irrationality. And in any case broader questions are unavoidable. For example, why is it that he is irrational in only this way, and only in his behaviour towards this particular choice?

Taking a step back, and looking at these developments on this branch of economic modelling from the foliage of another branch, one may be able to frame even larger questions. One of them is: when has a research project run its course, reached an impasse that shows that this program needs to be abandoned for something else and perhaps radically different? A conventional answer to this question emanating from theories of knowledge or the literature on scientific progress, is that programmes fail either because they lose their contact with reality and the phenomena they try to explain, forecast or predict,[3] or they reach a point of crisis as they find empirical or analytical results that contradict foundational aspects of the research project.

It is this second criterion that is at the centre of our current puzzle. I, looking from outside, see this latest trend as a negation of the whole program, and if such modelling modifications become established we appear to have returned to the start of this research endeavour. Thus, to me at least, models with all kinds of preference shocks appear to be lacking microfoundations as much as earlier models, that, by the way, had other interesting characteristics- and Professor Leijonhufvud gives an indication of these in his article. Have we simply traded one black box for another? And then why bother with the sophisticated mathematical machinery of rational agents, if the most important feature that drives the phenomena we are interested in (e.g. unemployment) are preference shocks we know close to nothing about?

What is even more remarkable, however, is that these modifications, these preference shocks, are not seen by those perched on the DSGE branch as a possible indictment of this research field. On the contrary, they appear to be seen as new research avenues that add to the power of these models to explain better or more of our economic environment. This is a disturbing development, because it indicates that their perspective on what is happening in this research field is drastically different from that of others sitting on other branches, and sharing, presumably, the same trunk of this tree. But does this common field, this trunk that unites us still be something we can all appeal to? Can we as a discipline find epistemic criteria that would allow us to not only discard twigs but also develop different branches of economic knowledge? Professor Leijonhufvud's paper is arguing in favour of such a systemic change and offers pointers on how to proceed. My fear is that we may be reaching a point of no return, a complete collapse of common ground for solving scientific disagreements between the different proponents in this field of knowledge. This collapse means that we cannot, as a discipline, discuss the merit of whole branches of research, and even agree to discard one branch and start investigating others. In fact, the very concept of the discipline as a tree with branches then comes into question. And then Professor Leijonhufvud's 'wonderland', may continue to create dystopian realities for some time to come.

[3] And Professor Leijonhufvud makes a number of observations of how DSGE models fare in this regard.