Comments on De Vroey "Lucas on the Relationship between Theory and Ideology"

In this paper De Vroey reviews Lucas' methodology. First, he distinguishes it in various ways from what methodologists generally believe. Secondly he examines the extent to which Lucas and the New Classical agenda was influenced by ideology; he thinks extensively. Thirdly, he tries to locate Lucas in the Walrasian (theory for its own sake) or Marshallian (theory that fits the facts) approach to economics; he thinks Lucas is a confusing mixture. This is an interesting and well-argued essay, written from the viewpoint of a methodologist and a historian of ideas. However, Lucas is a practising economist; like many such, his methodology acts more as a rationalisation of what he does than a template for his actions as an economist. I shall comment on De Vroey's work from the same angle- that of a practising economist.

Lucas' methodology au pied de la lettre

The methodology described by De Vroey is the standard one Lucas inherited from his Chicago mentor, Milton Friedman. Friedman (1953) stated that the role of theory was to set out a model of reality that was built on principles of economic maximization- homo economicus. Such a theory would typically by highly simplified and in literal terms no doubt even untrue; its merits would be established by testing the extent to which it could explain reality 'as if' it was true. Lucas' main practical contribution to this standard 'as if' approach in economics was not methodological but practical: he pressed microeconomic optimizing behaviour to its limits in his theories, retaining the parameters of preferences and technology in his models, where Friedman and his immediate followers at the time used models of aggregate supply and demand. Lucas' hard-to-refute argument for moving on was that for the design of policy one must use models with parameters that will not shift with policy change.

De Vroey spends some space discussing whether a theory is more than a model; Lucas said no, other methodologists apparently say yes. But while this is no doubt a matter of great importance to methodologists, it is of little practical importance in economics once one includes the necessity of testing against the facts in the method. For there is no way of testing a theory against the facts if it is not expressed as a model. Thus Lucas, like Friedman, was merely making a practical point about testability. Whether this 'excludes' such economists as Keynes from the category of 'theorists' is surely not of much interest since noone would deny that Keynes' ideas could be turned into testable models.

If one takes Lucas' statement of his methods as accurate and complete, then De Vroey's second and third sections would simply fall to the ground. According to this statement, it is quite irrelevant whether ideology is the source of a model since that model will stand or fall by its compatibility with the facts. Thus the facts are judge and jury in the Friedman method. Similarly the policy conclusions of a model that fits the facts can perfectly properly be communicated to politicians, regardless of whether their premises contained an ideology that would be therefore included in the communication.

Thus section two on Lucas' crypto-free-market ideology fails to note this critical discipline on ideology in the system. As for section three, again it is clear from the methodology that Lucas cannot be a Walrasian but must be a Marshallian in his stress on testability.

Lucas' methodology in practice

However matters are not so simple. The testing of the models of Lucas, Prescott and their Chicago-Minnesota disciples has run into huge problems which have made these economists less than willing to be subject to the orthodox methods of econometrics. A quotation from an interview with Thomas Sargent illustrates this point:

`...my recollection is that Bob Lucas and Ed Prescott were initially very enthusiastic about rational expectations econometrics. After all, it simply involved imposing on ourselves the same high standards we had criticized the Keynesians for failing to live up to. But after about five years of doing likelihood ratio tests on rational expectations models, I recall Bob Lucas and Ed Prescott both telling me that those tests were rejecting too many good models.' Tom Sargent, interviewed by Evans and Honkapohja (2005, p.6)

De Vroey mentions calibration as a new method of making models testable, which is true enough. The problem has been that the tests employed do not obey the normal rules of statistical inference. Thus it has been accepted as part of the methodology for evaluating these models (see Canova, 1994) that they should be regarded as 'false', in the sense that they cannot be the data generating mechanism. By implication normal tests cannot be applied; also the models cannot be tested at all- since if a model is false, then plainly it cannot be tested to see if it is false. However, what is then sought for is some sort of measure of closeness to the data. As Canova (1994, p. S124) puts it we want a measure that tells us 'Given that the model is false, how true is it?' What we have seen in the Chicago-Minnesota literature for the last two decades or so has been the informal comparison of 'stylised facts'- descriptions of key features of the data such as variances and cross-correlations- with simulated equivalents from their models. The implicit suggestion is that these comparisons test whether these unrealistic reproduce particular features of the real world relevant to a particular purpose; if they do, then a user of them for that purpose could consider them to be accurate enough to be used. This seems in practice to be a return to testing in the Friedman manner since the whole point of this was to test frankly unrealistic models 'as if' they were true. However, the comparisons are too informal to be regarded as proper statistical inference; when formal methods are applied it seems that different conclusions may well be reached - see Le et al (2010) for an example. Thus the testing of these models is in a sort of limbo; something apparently like testing is being done, but it is not rigorous.

This new direction charted by Lucas and Prescott has resulted in some confusion in macroeconomics, provoking James Heckman, the Nobel-prizewinning microeconometrician, to remark to a London newspaper in the past few weeks that

'…Macroeconomics is not a science, it's a patchwork of theorems and bad data. There is little serious work on the subject, which seems dominated by beliefs' (Heckman, 2010).

Work in macroeconomics has broken up into schools between which communication is limited. On the one hand there is the Chicago-Minnesota school we have just examined. On the other there is the school initiated by Sims (1980), a severe critic of the 'unreality' of the new models; this favours the fitting of time-series relationships to data with a minimum of theory, as in Vector Auto Regressions. Then there is the school of Bayesian modellers who impose strong prior assumptions on their mostly New Keynesian models and whose tests of variations from these assumptions are severely qualified by their starting point. There are yet other approaches. But it is all a far cry from the simple methodology of Classical statistics set out in De Vroey's account of Lucas. According to that view, economists should set out their models, however 'unrealistic' (or 'false' in literal terms) and test them against the facts 'as if true'. Instead we have from the three main schools respectively models that are not tested meaningfully against the facts; VAR models that are not theories at all but simply mimic the facts; and Bayesian models that are rigged not to be tested against the facts except in marginal ways.

Concluding remarks

Returning to De Vroey's paper, I therefore feel that he has missed a key element in the methodological situation in macroeconomics. Lucas' supposed methodology is more honoured in the breach than the observance. The implication of this is that when it comes to sections two and three of his paper De Vroey has a much stronger case than he could possibly have if the methodology were being seriously adhered to.

What the Chicago-Minnesota school is doing, at least up to now, is, in the words of the methodologists, 'immunising' its theories against rejection. This of course is where the connection to ideology then comes in, as well as that to the Walrasian approach. If the facts are no longer judge and jury in a way that would be widely recognised across the economics profession, then a theory inspired by ideology can be smuggled into political advice without sanction. One can see then why Lucas is concerned about 'non-intervention' and 'non-exploitation' of theory for ideological ends; he is only too aware of how little real testing is going on for this class of models. Also this does imply a strong Walrasian element in this modelling activity, to the extent that the testing it is subjected to is rather limited. Of course pure theory has an important role in economics, certainly in initiating ways of thinking that may lead to testing and so to scientific advance. Lucas' vision of what theory should do in macroeconomics has certainly borne great fruit. But we need to appreciate that, with testing still not operative, the full aims of Lucas' methodology- laudable as they are- are not, at least not yet, being implemented in practice.

References

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