

Some Comments on “Lucas on the Relationship between Theory and Ideology”

In this assessment of the paper “Lucas on the Relationship between Theory and Ideology”, I will take advantage of the fact that I am not a referee and hence I am not expected to produce a report. Instead, the following lines must be read as being some reflections about the paper, and especially about the debate that has taken place between the referee 1 and the author.

I think that a correct assessment of this paper requires that it should be evaluated from two different points of view. From a general perspective, we must take into account that, underlying the writings of the author, there is the general goal of making us (economists) stop thinking and discussing the more general problems that lie beneath our models – this is hinted by the author at the beginning of the second page in his answer to the report of referee 1. To me this is so a healthy intellectual exercise that I cannot but endorse and subscribe it.

Regarding this general goal I would say that the paper has completely succeeded in generating the desired effect. A glance at the very interesting and well articulated report that the paper has given rise to, as well as the subsequent answer by De Vroey will serve to verify this extreme. I am sure that this is simply a foretaste of the kind of reflections and debates that the paper will promote if it is published.

Turning the attention to more particular aspects of the paper, and keeping in mind the general goal above mentioned, I think that the choice of the theme is very well suited for the purposes of the author since, (i) Lucas has been one of the most influential macroeconomists of the last decades and; (ii) as De Vroey correctly points out, he is one of the economists who has been most interested in questions of methodology. Thus, this author is a living example of the kind of exercise we are invited to do by De Vroey.

Second, though I am not an expert on the subject, I think that De Vroey has correctly synthesized Lucas’s methodological position, as well as his particular views regarding the nature and place of models (which he equates to theories) in economics. In addition, the paper is clear in bringing to the light other peculiarities of his thinking, especially his position regarding what De Vroey labels the “non-exploitation” precept.

Before proceeding to comment more extensively on the debate between the author and the referee 1, I think that is fair to remember my initial warning: the following should not be taken as a formal assessment of both the paper and the report of referee 1, but only as a set of reflections that have come to my mind after reading both documents and that I now present here in a (somewhat) ordered form.

I think that the key element in Lucas’s methodology is his drastic distinction between ideology, on the one hand, and theory (i.e. mathematical models), on the other. Notice that this distinction implies that any argument that does not take the form of a mathematical model must be placed under the heading of “ideology”. This basic idea – I think – is germane for the following observations.

1. I agree with the first referee that Lucas’s methodology seems to be a rationalisation of what he does rather than a template for his actions as an economist. However, I do not think that the simple inclusion of testing against facts in Lucas’s method makes his

methodological position accurate and complete, as he/she seems to suggest. In this sense, referee 1 main criticism to De Vroey's paper seems to be that Lucas's acceptance of empirical testing exercises a discipline on ideology. According to this view it would be irrelevant whether ideology is the source of a model since it would stand or fall by its compatibility with facts. From the reading of De Vroey's paper, it seems to me that things are not so simple – as, on the other hand, referee 1 himself recognizes in his report.

A direct conclusion that can be obtained from Lucas's "ideology-model" dichotomy is that theories are not able to settle down ideological disputes – and it seems that this is the conclusion reached by Lucas himself, as it is attested by the last paragraph and quotations of section two of De Vroey's paper. It follows from the fact that models are not intended to shed light on ideological issues, but they are designed to be like programs, accepting economic policy rules as inputs and generating statistics which describe the operating characteristics of time series as outputs. To me, this position does not only imply that ideology is immune from empirical testing (since it is the models which are put under the test) and, consequently, that the economist's ideological underpinnings are not threatened by the verification/refutation of some model; but it also implies that our models are handicapped since they cannot be used to discuss these ideological underpinnings.

However, the former reasoning would have only a minor effect on referee's statement had Lucas stuck with his view that mathematical models are entities completely separated from ideology. If this were the case, models would retain a place in economic thinking coherent with referee's explanations: though they could not be used to solve ideological disputes, they would be able to generate policy recommendations – the function for which they are designed, according to Lucas. In this sense we, as economists, could still use them in practical matters, putting our ideological views aside while the empirical confrontation of the outcomes of our models with the available data reveals which model performs better than the others, and can be used for giving political advice.

The problem is that Lucas seems to have accepted that models are not immunized from ideology. We can find a couple of passages that corroborate this conclusion. First, there is his statement that by seeking an equilibrium account of business cycles one accepts in advance rather severe restrictions – see page 13 of De Vroey's paper. Second, in his interview with Snowdon and Vane (1998: 127) Lucas also affirms that equilibrium is a property of how we see things, not a property of the world. Consequently, our ideological views are translated to the mathematical models we formulate, embedded in some technical devices or notions such as the idea of equilibrium. Then, the conclusions we get from our models are not completely freed from our ideological views – which, recall cannot be tested. Thereby, though we still can confront the models against the data, we now must refrain from exploiting their results for policy advice. This further limits the usefulness of our models, for now they can neither be used (at least directly) to accomplish one of the goals of macroeconomics – more precisely, goal (e) in page 3. What use can we make of mathematical models, then? At first sight, it seems that models have a very limited use in Lucas's thinking: (i) they facilitate progress in economic thinking by enabling economists speak the same jargon, [see the quotations in pages 3 and 4] so that (ii) they make it possible to settle policy issues in a way that makes controversy productive, which to Lucas is means “ [get]... economists talking

and thinking about issues that our equipment may let us make some progress on”, [see pages 11-12 of De Vroey’s paper and the quotations that appear]. Clearly, then, models are in some sense protected against empirical testing, for they are ultimately judged in terms of their ability to make controversy productive. This may help to explain the hesitations expressed by Lucas to Sargent about the outcomes of the empirical testing on rational expectations models – see the quotation in page 2 of the report of referee 1.

Therefore, it seems to me that not only the statement that “facts are judge and jury” of models does not hold in the practice of Lucas’s methodological prescriptions, but neither in their theoretical foundations. As a corollary of this conclusion, let me add that the compartmentalization that macroeconomics has recently experienced – and that referee 1 so nicely summarizes in his report – should not be surprising. In some sense it honours Lucas’s methodological position: each researcher begins the theory from the ideological background he has received at its school, and tries to translate his/her views into a theoretical model. The validity of these models should not be judged in terms of whether they are accepted or rejected by empirical tests, but in their ability to set the different points of view about reality out in terms that facilitate communication between economists – well, according to referee 1, we must admit that macroeconomic theory has not performed very well in this respect.

Admittedly, this reading of Lucas may be plagued with difficulties. The most striking one that comes to my mind is how Lucas’s account of the 1970 stagflation episode can be accommodated within it. It also suggests that Lucas has reneged on his initial compromise to make macroeconomic a practical discipline, oriented toward facts. A possible way out is that Lucas considers that reality still provides us with general guidelines to produce our models. Obviously, these guidelines are vague and hence, if we limit to observe it, we will obtain anything else than an array of verbal observations. But sometimes we are lucky and reality clearly points out which kind of ideological points of view perform better in explaining what is happening, as it was the case of the 1970 stagflation episode.

2. Lucas’s drastic “ideology-models” dichotomy directly implies that all his views on methodology either must be classified as being ideological or, at best, as belonging to the limbo of metatheoretical propositions. So, his (methodological) rationalization of his theoretical work is, in fact, ideology (or metatheory). This an important point when evaluating his change from Keynesianism to Friedmanism: according to his drastic distinction, we must conclude that this change was driven out by ideological reasons. Obviously, this is not to say that the new classical revolution stemmed from a political agenda.

I would like to add a final (and minor) comment to this debate. De Vroey seems to be astonished by the summary that referee 1 makes of his paper; particularly with the point in which the latter states that the paper deals, among other things, with the extent to which New Classical agenda was influenced by ideology. I think that this confusion may have been (indirectly) induced by the wording the author has chosen in the abstract of the paper, where he states “(...) I wonder whether the new classical revolution resulted from a political agenda”. Maybe rewording this phrase would avoid this confusion.

To conclude: I have found the reading of De Vroey's paper not only very interesting, but also very stimulating. As a consequence, I cannot but recommend its reading – and if I were a referee I would also recommend its publication.

References

Snowdon, Brian and Vane, Howard R.(1998): 'Transforming macroeconomics: an interview with Robert E. Lucas Jr.', *Journal of Economic Methodology*, 5: 1, 115 -146