

Michel De Vroey. Reactions to referee No 1

Thank you for our report, which I enjoyed reading. It is beautifully written, which is unusual for a referee report, and full of interesting insights. As you may guess, I have several remarks to make.

First of all, I want to underline that my paper is not a charge against Lucas. Usually, people have a passionate attitude towards him, either admiring him blindly or fully rejecting him. I have tried to make a balanced assessment bringing out both the subtlety of his methodological standpoint and its ambiguities, especially his being torn between Walras and Marshall. Next, I do not recognize myself in what you write in your summary of the paper: the author “examines the extent to which Lucas and the New Classical agenda was influenced by ideology; he thinks extensively”. My paper is not on Lucas *and* the New Classical agenda. Its only concern is Lucas, and there are no indication that other new classical or real business cycle macroeconomists would endorse his views. Moreover, the point I am trying to make is more subtle than what you write. On the one hand, Lucas believes that economic theory is underpinned by ideology (without giving a pejorative meaning to the notion of ideology). On the other hand, his overarching methodological preoccupations is to immunize theory from ideology. Whether or not he succeeds in this endeavor, it remains in my eyes that, be it just because of his epistemological awareness, Lucas is less in the thrall of ideology than many of his colleagues. Therefore, I cannot endorse your statement that the central claim of my paper is that Lucas’s theory is ‘extensively influenced by ideology’.

I also question another of your statements, namely that “Lucas is a practising economist”. Take his *Studies in Business Cycle Theory* book. Half of its chapters are methodological. When studying the Lucas archives, I found a series of drafts which were of a methodological nature. Not many practising economists have as much reflected on method as him. Likewise, I view the Lucas critique as a methodological contribution. Small wonder then that I also disagree with you when you write that, “Lucas’ main practical contribution to this standard ‘as if’ approach in economics was not methodological but practical: he pressed microeconomic optimizing behavior 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”. Reading this, I wonder what exactly you mean by ‘methodological’ and ‘practical’. To me, the Lucas critique is emblematically methodological while, of course, having practical consequences.

Your main criticism of my paper follows from your viewpoint that, if Lucas’s methodological stance should be summarized in one proposition, it would be that it is selfsame to the Friedmanian conception, “factual assessment is judge and jury as far as the validity of theories is concerned”. This statement looks too rudimentary to me. As my paper documents, the

matter is more complicated. Actually, its very contribution is to bring this out. I understand that such an inquiry may hardly be the practising economists' cup of tea, but I hope that you will concur that nonetheless it may be of some interest to the macroeconomists' community. In other words, in these times of high specialization and high technicality, some meta-theoretical contributions can be useful (this is why I submitted this paper to a general journal rather than to a history of economics or methodological one).

You end this section of your report by making two statements. The first is that I fail to notice the impact of the factual testing discipline on ideology. This is not fully true as I wrote the following: "Lucas recurrently expressed the view that the stagflation of the 1970s was a quasi-laboratory experiment allowing economists to discriminate between the Keynesian Phillips-curve and the natural-rate-of-unemployment models, and the result of which was a hands down victory for Friedman's theory. Because of this predictive success, Lucas's qualms about the political exploitation of the model's conclusion vanish! In spite of their ideological content, theoretical policy conclusions can be used in support of policy recommendations on the basis of the predictive success of empirical work. In other words, the prediction criterion has overridden the non-exploitation principle." The second statement is that "again it is clear from the methodology that Lucas cannot be a Walrasian but must be a Marshallian in his stress on testability". But what do you do of Lucas's recurrent claims that he is a neo-Walrasian? Should they be thrown away as non-sense or smokescreen? I prefer to address the problem straight on; my split-theoretical personality conclusion ensues.

I now turn to the section entitled "Lucas's methodology in practice". Here, I am in full agreement with you. Your review of the troops in presence is remarkable. I would liked to have written it myself. My only qualm is about your saying that your remarks are a far cry from the methodology of classical statistics set out in the paper. This is inexact for the simple reason that I didn't set out anything. Actually, I did not enter into the discussion of the validity of the calibration method endorsed by Lucas as I felt that it was a different subject matter. I now realize that I was mistaken. Lucas's immunization strategy hinges on the success of empirical work. Therefore, I need to discuss it.

Your concluding remarks are stimulating. You are right in noticing that the mitigated success of the empirical testing of RBC models impinges on my argumentation. I agree that the paper should be revised along this line. I have to think more about how to do this, but for the moment the following three points come to my mind. 1) Lucas would most probably keep defending empirical testing; he could admit its present-day limits but would probably express optimistic views about future improvements. 2) For the time being, the result that empirical testing is inconclusive should lead him to adhere more strictly to the 'non-exploitation' principle. However, this would put him in the uncomfortable position of being a 'methodological saint' in a world of 'methodological sinners' since most other economists do

no bother about the 'non-exploitation principle'. 3) If Lucas were to admit your conclusion, he should be led to proclaim a full instead of a half adherence to the neo-Walrasian perspective. This would make his methodological vision more consistent but also more ethereal.