Economics
The Open-Access, Open-Assessment E-Journal

Vol. 5, 2011-4 | February 23, 2011 | http://dx.doi.org/10.5018/economics-ejournal.ja.2011-4

Lucas on the Relationship between Theory and Ideology

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Abstract This paper concerns a neglected aspect of Lucas's work: his methodological writings, published and unpublished. Particular attention is paid to his views on the relationship between theory and ideology. I start by setting out Lucas's non-standard conception of theory: to him, a theory and a model are the same thing. I also explore the different facets and implications of this conception. In the next two sections, I debate whether Lucas adheres to two methodological principles that I dub the 'non-interference' precept (the proposition that ideological viewpoints should not influence theory), and the 'non-exploitation' precept (that the models' conclusions should not be transposed into policy recommendations, in so far as these conclusions are built into the models' premises). The last part of the paper contains my assessment of Lucas's ideas. First, I bring out the extent to which Lucas departs from the view held by most specialized methodologists. Second, I wonder whether the new classical revolution resulted from a political agenda. Third and finally, I claim that the tensions characterizing Lucas's conception of theory follow from his having one foot in the neo-Walrasian and the other in the Marshallian-Friedmanian universe.

JEL B 22, B 31, B 41, E 30
Keywords Lucas; new classical macroeconomics; methodology

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Citation Michel De Vroey (2011). Lucas on the Relationship between Theory and Ideology. *Economics: The Open-Access, Open-Assessment E-Journal*, Vol. 5, 2011-4. doi:10.5018/economics-ejournal.ja.2011-4. http://dx.doi.org/10.5018/economics-ejournal.ja.2011-4

1 Introduction

The relationship between ideology and economic theory is not a topic that one would expect Robert Lucas to have addressed. Well, this is wrong. Among the economists who changed the course of macroeconomics in the 1970s–1980s, Lucas was actually the one who devoted the most attention to discussing the methodological dimension of their enterprise — to an extent which led Prescott to hail him as “the master of methodology, as well as defining problems” (Prescott’s interview with Snowdon and Vane 2005, p. 351). My aim in this paper is to reconstruct Lucas’s standpoint on the relationship between ideology and theory, and to locate it within his wider methodological vision.

Such a project faces several difficulties. One of these is that there is no single article in which Lucas explores his methodological standpoint in a systematic way. It has to be reconstructed from the many scattered remarks on the subject to be found in both his published and unpublished works. Lucas’s unpublished works have been collected in the Lucas Archives at Duke University’s Special Archives division (Lucas, various). They are mainly drafts and correspondence, and they are invaluable for my inquiry for several reasons: they may complement the published works, they may shed light on the genesis of Lucas’s mature vision, and they may reveal Lucas’s deep-seated views, what he really believed but decided not to reveal in public. On the other hand, unpublished pieces found in archives also raise difficulties: they must be interpreted cautiously because the reason they have remained unpublished may well be that the author has abandoned the views expressed in them. Be that as it may, the greater the congruency of drafts with published pieces, the higher the likelihood that they are more than tentative ideas committed to paper. The test of such congruency is that any idea expressed in a draft should have a corresponding passage in a published piece. This is often true for Lucas, a typical situation being one in which ideas that are expressed succinctly, if not inadvertently, in a published article are based upon a fuller treatment in earlier notes which remained unpublished.[1]

The paper is organized as follows. In its first part, I discuss Lucas's view of 'good' macroeconomic theory and ideology. I show that his definition of theory is narrower than the common understanding, and continue with a few other clarifications of his vision. In Part 2, I discuss Lucas's position with respect to what I call the 'non-interference' precept, the proposition that ideology should not interfere into theory. In the third part, I do the same with the 'non-exploitation' precept, the proposition that a model's policy conclusions should not be translated into policy recommendations to public authorities whenever these conclusions are built into the model's premises. Part 4 is my assessment of Lucas's standpoint. First, I bring out the extent to which Lucas departs from the view held by most specialized methodologists. Second, I wonder whether the new classical revolution resulted from a political agenda. Third, I claim that Lucas's methodological standpoint shows signs of tension, which I explain by arguing that Lucas has one foot in the Walrasian and the other in the Marshallian universe. That is, while proclaiming himself to be a neo-Walrasian economist, Lucas has no qualms working along Friedmanian methodological principles. To me, this is troublesome since I view these two approaches as incompatible.

[1] The Lucas archives also contain draft versions of published papers. A comparison of the final draft with the published version sometimes reveals that Lucas decided to delete whole paragraphs of the draft paper. These are passages which Lucas wrote but eventually chose not to publish, in my opinion often for diplomatic reasons. Nonetheless, the commentator will find such passages especially enlightening and may well wish to quote them. This is fair game, I think, once an author has agreed to open his archives. However the reader should be warned about the particular status of these passages. In the present article, the situation occurs only once, apropos of Lucas's review article of Tobin's *Asset Accumulation and Economic Activity* book.

2 Lucas's Conception of Macroeconomic Theory and Ideology[2]

2.1 Economic Theory

Lucas holds a narrow view of what macroeconomic theory ought to be. This can be summarized as:

(a) There should be no split between the principles underpinning microeconomics and those underpinning macroeconomics (Lucas 1987, p. 107–108). That is, macroeconomics without (choice-theoretic) micro-foundations is unacceptable.
(b) Macroeconomics is part of general equilibrium analysis. Its concern is the working of an entire economy, and it ought to account for the interactions between the component parts of the economy.
(c) A macroeconomic theory and a mathematical model are one and the same thing. This conception, which can be traced back to Walras, runs counter to another, more widespread, understanding of the relationship between theory and model, according to which a theory and a model are two distinct entities: a theory is a set of propositions about reality while a model, be it mathematical or in prose, is an attempt at rigorously setting out the implications of some part of the theory. [3]
(d) A theory is concerned with imaginary constructions; it is avowedly non-realistic.

> On this general view of the nature of economic theory then a 'theory' is not a collection of assertions about the behavior of the actual economy but rather an explicit set of instructions for building a parallel or analogue system—a mechanical, imitation economy. A 'good' model,

[2] Friedman wrote his famous "Methodology of Positive Economics" essay (Friedman 1953) almost from scratch, with little reference to the methodology literature of the time. This is also true of Lucas's writing. For a wider analysis of Lucas's epistemology, see Vercelli (1991).

[3] Thus, in the traditional view, the model is subservient to the theory. The following quote from Leijonhufvud is a fine depiction of this viewpoint: "I propose to conceive of economic 'theories' as a set of beliefs about the economy and how it functions. They refer to the 'real world'.…'Models' are formal but partial representations of theories. A model never encompasses the entire theory to which it refers" (1997, p. 193).

> from this point of view, will not be exactly more ‘real’ than a poor one, but will provide better imitations (Lucas [1980a] 1981a, pp. 271–272).
>
> Progress in economic thinking means getting better and better abstract, analogue economic models, not better verbal observations about the world (Lucas [1980a] 1981a, p. 276).

Central assumptions of macroeconomic models, such as rational expectations, ought to be viewed as modeling devices, technical model-building principles rather than propositions about reality.[4]

(e) Macroeconomic models are of no interest if they fail to reach policy conclusions. “The central question that macroeconomists need to resolve: Which necessarily abstract models can help us to answer which practical questions of economic policy?” (Lucas (various) Box 23, Tobin folder). According to Lucas, old style macro econometric models were praiseworthy because they resulted in policy conclusions, a virtue that, he thinks, has been lost in new Keynesian models, such as efficiency wages models.[5]

(f) Theory ought to be tested against facts. Its aim is to construct “a fully articulate artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies” (Lucas [1977] 1981a, p. 219).

[4] “One can ask, for example, whether expectations are rational in the Klein–Goldberger model of the United States economy; one cannot ask whether people in the United States have rational expectations” (Lucas (various) Box 23, Barro Folder).

[5] “Keynesian models in the 1960s, and this is what excited people like Sargent and me, were operational in the sense that you could quantify the effects of various policy changes by simulating these models. You could find out what would happen if you balanced the budget every year, or if you increased the money supply, or changed fiscal policy. That was what was exciting. They were operational, quantitative models that addressed important policy questions. Now in that sense new Keynesian models are not quantitative, are not fitted to data, there are no realistic dynamics in them. They are not used to address any policy conclusions. What are the principal policy conclusions of ‘new Keynesian economics’? Ask Greg Mankiw that question the next time you interview him. *(laughter)* I don’t even ask that they prove interesting policy conclusions, just that they attempt some” (Lucas’s interview with Snowdon and Vane 1998, p. 131).

> Our task as I see it is to write a FORTRAN program that will accept specific economic policy rules as 'inputs' and will generate as 'output' statistics describing the operating characteristics of times series we care about, which are predicted to result from these policies (Lucas [1980a] 1981a, p. 288).

> The empirical testing of the theory is critical precisely because we know that the axioms are abstractions, necessarily 'false', so we need to know whether and under what range of circumstances these abstractions are adequate (Lucas 1986, p. S 408).

A theory/model ought to be assessed by its ability to make correct predictions. The better its ability to reproduce past events, the more trustworthy the model is for assessing new policy measures.[6]

While Lucas never wrote a systematic exposition of his methodological standpoint—his "Methods and Problems in Business Cycle Theory" ([1980a] 1981a) comes closest to such an enterprise—it should be noted that about half the essays in his book, *Studies in Business Cycle Theory*, are of a methodological nature. The above account is drawn mainly from them. Note also that, with a few exceptions (e.g. Lucas and Rapping ([1969] 1981a)), Lucas's main contributions were qualitative or critical. Item (f), confrontation of theory with facts (as encapsulated in the FORTRAN quotation given above), was at the time more a program for the future than an assessment of what had been achieved. Its implementation came a few years later in Kydland and Prescott's (1982) distinctive contribution. Here is how Woodford describes it:

> The real business cycle literature offered a new methodology, both for theoretical analysis and for empirical testing. … It showed how [Lucas-type] models could be made *quantitative*, emphasizing the assignment of realistic numerical parameter values and the computation of numerical solutions to the equations of the model, rather than being content with merely qualitative conclusions derived from more general

[6] "*Would you agree that the appropriate criterion for establishing the fruitfulness of a theory is the degree of empirical corroboration attained by its predictions?* Something like that. Yes" (Lucas's interview with Snowdon and Vane 1998, p. 131).

> assumptions. The "equilibrium business cycle models" of Lucas had really only been parables; they could not be regarded as literal descriptions of an economy, even allowing for the sort of idealization that all models of reality have…Real business cycle models are instead quantitative models that are intended to be taken seriously as literal depictions of the economy, even if many details are abstracted from. The literature emphasizes the numerical predictions of the models, when parameter values are assigned on the basis of measurement of the relevant aspects of an actual economy (Woodford 1999, pp. 25–26).

Lucas has fully endorsed this development which he considers as a prime example of the way in which macroeconomics should evolve.

> How well did [the Kydland–Prescott model] do? People are still arguing about this, but what is interesting is not so much the outcome of this argument, which is certainly still in doubt, as the language in which it is being conducted. When people criticize real business cycle models they criticize their assumptions about demography, preferences, technology, and market structure, in terms that are recognizable to any economist. The private jargon that macroeconomists developed during the period where it was cut off from mainstream economic theory—multipliers and gaps and Phillips curves and so on—has largely passed out of use. Macroeconomists today are expected to be able to discuss their ideas in the language of Arrow, Debreu and McKenzie. This is progress (Lucas 2007, p. 7–8).[7]

2.2 Ideology

Lucas readily admits that, like all other citizens, economists hold ideological views. Here, the term 'ideology' does not carry any pejorative

[7] The quotation comes from an unpublished note written for the 1993 CORE Conference on general equilibrium held in Louvain-la-Neuve. The note was later inserted in an editorial written for a special issue of *Economic Theory* in honor of Edward Prescott. In the comments added to the initial note, Lucas lifts the reservation expressed at the beginning of the passage, saying "In 1993, I was still reluctant to acknowledge the substantive success of real business cycles models. Ten years later, this was no longer the case" (Lucas 2007, p.9).

meaning, or refer to arguments imbued with bad faith or based on the defense of particular interests. Rather, it designates a vision of an ideal organization of society along several dimensions, one of which is the economic one (which is the only that will be considered in this article). The following two quotations, the first drawn from an interview with *The Region*, the second from a draft of his review of Tobin's Yrjö Jahnson lectures (Tobin 1982), summarize Lucas's standpoint:

> In economic policy, the frontier never changes. The issue is always mercantilism and government intervention vs. laissez faire and free market (*The Region* 1993, p. 3).

> There are, it seems to me, two schools of macroeconomic (and perhaps all) social policies: one which keeps the power of government to injure in the front of its mind, and stresses policies which take the form of institutional constraints on government action, and another which focuses on the power of government to improve welfare, and seeks methods by which this power may be exercised more effectively (Lucas various, Box 23, Tobin folder).

The split is between those who want to give competition its full rein, the defenders of the self-regulating characteristics of markets (or 'free marketers') and those, the Keynesians, who think that the market economy, although the best economic system, can buttress failures, in particular an insufficiency in aggregate demand, which it is the state's role to remedy. Lucas makes no secret of his preference for the free market viewpoint.[8]

[8] However, this does not make him a libertarian as the following extract from his correspondence makes clear: "When I walk through Lincoln Park, I'm glad that some ancient Chicago government did not say: 'It is a difficult problem for thousands of individuals to coordinate on providing themselves with parks, but there is no reason to believe governments have any particular aptitude for solving such problems. Let's leave it to the free market'. So far as I know, governments are the *only* means by which the parks problems has been solved for large cities" (Letter to K. Matsuyama, March 29, 1995, Lucas (various) Box 15, Correspondence 1995 folder).

2.3 The Narrowness of Lucas's Conception of a Theory

The hallmark of Lucas' methodological standpoint is his narrow conception of a theory, which treats a theory and a mathematical model as identical. To him, everything else is meta-theoretical. This is definitely a minority viewpoint. The counterpart of this narrowness is an extended conception of ideology. To Lucas, any discussion that fails to take the form of a mathematical model belongs to the sphere of ideology. As a result, he is led to define as ideological, studies that are usually viewed as theory. This is the case for the work of economists such as Coase and Hayek. In spite of his sympathy with these authors, Lucas finds that they are engaged in ideological rather than theoretical activities. Their attitude, he writes, is

> Not so much a defense of free markets as it is an unwillingness to discuss the issue. … What I want from economics is a set of principles I can use to evaluate proposed government interventions, case by case, on their individual merits. I agree that explicit modeling can give a spurious sense of omniscience that one has to guard against…*But if we give up explicit modeling, what have we got left except ideology?* I don't think either Hayek or Coase have faced up to this question (letter to K. Matsuyama, March 29, 1995, Lucas (various) Box 15, folder 1995, my emphasis).[9]

A further testimony to Lucas's touchiness about the boundary between theory and ideology can be found in an exchange of letters with Modigliani. Invited to participate in a study of disagreement among economists by submitting a list of the five living economic theorists he appreciated the most, Lucas reacted by asking Modigliani to clarify the following point: "Do you mean 'theory' in the recent sense of specialist in mathematical economics, or just someone who uses economic reasoning rigorously in their work? In the first sense, Arrow is in and Friedman is out; in the second, I would include both" (Lucas, various, Box 25, Correspondence

[9] As far as Coase's theory is concerned, Lucas's claim is supported by a recent article by Steve Medema surveying the different ways in which the Coase theorem is represented in textbooks. It shows that this so-called theorem is interpreted in diametrically opposite ways according to the ideological views of the textbooks' authors (Medema 2010).

1993 folder). I surmise that Lucas has a strong preference for the 'recent sense' to the effect that, for all his admiration for Friedman, he would exclude him from the circle of real theorists!

2.4 Model Economies as Analogous Systems

Lucas's 'Tobin and Monetarism' article is a stern criticism of Tobin's Jahnson lectures (Tobin 1982). In it, Lucas comments that:

> It does not seem to me a critical or an economic insight to observe that that one can detect differences between the world described in this paper ["Expectations and the Neutrality of Money"] and the United States, or that it utilizes 'questionable *ad hoc* assumptions', or that it leaves facts unexplained…Insofar as theoretical models of this type have an influence that is worth trying to counteract (as Tobin wishes to do), it must be because people perceive useful analogies between the patently artificial world of the model and the world we live in, and not because they are unable to distinguish between these two different worlds. If so, the successful criticism must go beyond an enumeration of the ways in which the model and reality differ to offer some perception of the nature of the analogies that are being drawn and some argument to the effect that these analogies are misleading (Lucas, 1981b, pp. 562–3).

In this passage, Lucas suggests that the usefulness of a model depends on whether it is a good analogy of the reality it is supposed to illuminate. The reader may ask what Lucas had in mind when he talked about an analogy, but no clue is provided in the published article. However, when searching Lucas's archives, I found a series of drafts—some handwritten, some typed—in which he expanded at length on the notion of an analogy, on the role of models, and on the relationship between modeling and economic policy.[10] The following discussion is based on one such fragment.

[10] My guess is that these drafts were written at the end of the 1970s, a period in which Lucas gave many seminars on rational expectations and business cycle theory. Several of the insights in them have found their way into published material—for example, I counted seven occurrences of the term 'analogy' in Lucas's 'Methods and Problems' article. Unfortunately the drafts are collected in a

Lucas's argumentation starts from two premises, both of which may come as a surprise. The first is that a model is an observable reality:

> We speak of modeling phenomena or models *of* phenomena, suggesting that observed phenomena are one kind of thing and models of them another thing, but I want to define a model to be itself a phenomenon: something the behavior of which can be observed. Then what is the relationship between a set of phenomena and the second set that we call a model of the first set? I will call this relationship *analogy* (Lucas, various, Box 17).

This still does not clarify what Lucas meant by the term 'analogy'. An answer is provided later in the same draft, Lucas writing that he takes an analogy to "mean a symmetric relationship between two things". These things may either be a thing in the usual sense and a theory, which to Lucas is just another thing, or two distinct procedures for generating observations. In another set of notes, Lucas adds that "we must make liberal use of analogies: judgments that one situation is similar enough to another to call for the same reaction".

The second premise follows from a bold comparison between economics and anthropology:

> Economic theory, like anthropology, 'works' by studying societies which are in some relevant sense simpler or more primitive than our own, in the hope either that relations that are important but hidden in our society will be laid bare in simpler ones, or that concrete evidence can be discovered for possibilities which are open to us which are without precedent in our own history. Unlike anthropologists, however, economists simply invent the primitive societies we study, a practice which frees us from limiting ourselves to societies which can be physically visited as sparing us the discomforts of long stays among savages. This method of society-invention is the source of the *utopian*

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disordered way in the archives: a series of fragments, often four or five pages long, all related to the same themes, but without any indication as to how they might be combined. To the best of my knowledge they never appeared in a finalized form in a published piece. In view of their interest and the lack of easy access, I shall quote from them extensively.

> character of economics; and of the mix of distrust and envy with which we are viewed by our fellow social scientists. The point of studying wholly fictional, rather than actual societies, is that it is relatively inexpensive to subject them to external forces of various types and observe the way they react. If, subjected to forces similar to those acting on actual societies, the artificial society reacts in a similar way, we gain confidence that there are useable connections between the invented society and the one we really care about (Lucas, various, Box 23, Lucas's emphasis).

Possibly, anthropologists will fail to recognize themselves in Lucas's account, but leaving this aside, the comparison succeeds in bringing out Lucas's message: models are fictional economies from the manipulation of which we can learn about the functioning of real economies. Lucas's reason for resorting to use of analogies follows from his view that macroeconomics should be used to assess the impact of policy measures. Normally, the impact of such policies cannot be assessed experimentally.[11] So second-best ways need to be found. One of these is to look for analogous cases—has there been another, not too different, country where the policy under consideration has been tried? If yes, this experience may constitute a valuable reference. But then, Lucas's argument goes on, real-world analogies are scarce. Again, a way out exists. The reference for such comparisons does not need to be a real-world experience; a model economy can do the job. Actually, it can do it better, the advantage of the model economy over the real-world analogy being that it can be controlled to improve on the similarities it offers. This is Lucas's second justification for model building.

> I like to think of theories—economic and psychological, both—as simulatable systems, *analogues* to the actual system we are trying to study. From this point of view, the Wharton model, say, bears the same kind of logical relationship to the United States economy as France, say, does: it is just a different economy, or system, but one that is similar

[11] According to Lucas (see e.g. 1981b, p. 559), the stagflation episode of the 1970s was a near real-world experiment and hence an exception to the above remark.

> enough to the U.S. economy that we might hope to learn about the properties of one through the study of the other. If our objective is to learn what the consequences of introducing a value added tax in the U.S. might be, we might study its consequences in France or simulate the Wharton system under such a tax or, better still, do both (Lucas, various, Box 17, Lucas's emphasis).

Lucas returned to the analogy theme in an address given to Chicago students in December 1988 and entitled "What Economists Do":

> We are story tellers, operating much of the time in worlds of make believe. We do not find that the realm of imagination and ideas is alternative to, or retreat from, practical reality. On the contrary, it is the only way we have found to think seriously about reality (Lucas 1988, p. 5).

Assume, Lucas writes, that the issue to be discussed is the connection between changes in the money supply and economic depressions. Here is then a possible story. Imagine an old-fashioned amusement park with roller coasters, fun houses, etc. Such a park can be viewed as a separate economy functioning with its proper money, the tickets used by the patrons to ride the facilities and obtained by exchanging dollars at the entry of park. What would happen, Lucas wonders, if the rate of exchange between dollars and the fictive tickets money was suddenly changed so that the purchasing power of a dollar would decrease to only 80 percent of its earlier parity? Different consequences can arise, but certainly nobody is going to buy more tickets than before. The ensuing decrease in the total number of tickets in circulation will trigger a reduction in real output and employment, the occurrence that we call a depression. True, this is just a story, but it is less gratuitous than it may look at first since its very purpose is to pave the way for the claim that real-world depressions are caused by monetary contraction. This, Lucas argues, is what economists do, "to argue by analogy from what we know about one situation [the model economy] to what we would like to know about another quite different situation [the real-world]" (Lucas, 1988, p.5).

Admittedly, Lucas's reasoning goes on, the analogy that one person finds persuasive will look ridiculous to another. Is there an escape? Here is his answer:

> Well, that is why honest people can disagree. I don't know what one can do about it, except keep trying telling better and better stories, to provide the raw material for better and more instructive analogies. How else can we free ourselves from the limits of historical experiences so as to discover ways in which our society can operate better than it has in the past? (Lucas 1988, p. 5).

Thus, for all its possibly obfuscated language, a mathematical model is at bottom a story. At the beginning, it will be just as vague a story as the above. Progress consists in 'making it better' in terms both of the introduced concepts and of the logical steps involved in the argumentation.

I started my presentation of Lucas's view of a theory by arguing that, to him, a theoretical proposition is a statement about a fictive economy rather than an actual economy. As a result, he claims, the right question to ask about such propositions is not whether they are true or false (because the answer is always 'false').[12] The proper question to ask is: "All we can say about an analogy is that it is good or bad, useful or useless, and such subjective terms only raise further questions: Good for what? Useful for what purpose"? (Lucas, various, Box 23, Barro folder).

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[12] "Do we value this theory (if one can discuss valuing the theory of value!) because we agree that it implies a set of verbal propositions about observations that can be refuted by keeping our eyes open for black swans [because seeing a black swan would indicate that the proposition that all swans are white is refuted (MDV)]? If so, what are these sentences that express—more accurately than the theorems or formulas themselves—what this theory really means, really implies? Shall we test the theory by checking sentences like: "in all economies, production possibility sets have nonempty interiors?" or "People tend to act in their own self-interest"? against what we see, the way we are supposed to check swan colors? Shall we dismiss Arrow and Debreu's theory as vacuous, and Kydland and Prescott's application of it as wrong? (Yes, I think, except for 'dismiss'). An alternative point of view toward things and theory is this: we observe things and events, and we perceive analogies among them" (Lucas, various, Box 23).

2.5 Lucas's Skepticism about Expertise

The sequel to the fragment quoted above about economics and anthropology is also interesting. It introduces two additional themes. The first, which again can be traced back to Walras, is that economic theory is utopian in nature.[13] The second is that economists agree too easily to act as experts, a role which society invites them to play, but about which they should be wary:

> This utopian character of economic theory is the only reason anyone cares about economics; it is also the source of the deep envy and mistrust with which we are viewed by our fellow social scientists. In an age in which utopian thinking is for entirely understandable reasons, unfashionable, most economists take pain to disguise what it is that we actually do. We are accepted as hardheaded, practical men-of-affairs, we enjoy the influence (or at least the appearance of influence) to which this image affords us access, and not a few of our number are at least as good as our legal colleagues in exercising this influence. In this exercise, it proves expedient to attempt to fit the popular image of a scientist as one who knows 'the facts', whose views are dictated by the 'evidence'. In this attempt, it is invaluable to be able to appear as a representative of a professional consensus, as one whose opinions may receive the empirical confirmation by a journalist with the resources to check the view of six others (five to agree, and one to prove the rule) (Lucas, various, Box 23, Barro folder).

Lucas's questioning of the role of expert has not been limited to his unpublished writings. He broached the subject in his article "Rules, Discretion and the Role of Economic Advisor" (Lucas [1980b] 1981a). To him, that role is in large part a by-product of the US events that led to the rise of Keynesian macroeconomics. It all began, he claims, with the Employment Act of 1946. This Act, which embodied the lessons drawn from the Great Depression (that high unemployment was costly, and that

[13] "Pareto believes that the aim of science is to come closer to reality through successive approximations. I, for one, believe that the eventual aim of science is to bring reality close to a certain ideal. This is why I formulate this ideal" (Walras 2000, p. 567, my translation).

government policy had the ability to affect unemployment), exerted a strong effect on the practice of monetary economics in the postwar period:

> Renamed macroeconomics, this sub-discipline [monetary economics] defined itself to be that body of expertise the existence of which was presupposed in the Employment Act, and the practitioners devoted themselves to the development and refinement of forecasting and policy evaluation methods which promised to be of use in the annual diagnosis-prescription exercise called for by the act (Lucas [1980b] 1981a, p. 250).

According to Lucas, the vision underlying the Employment Act (and supported by Keynesian theory) was that the government had the ability to manipulate the economy in a costless way for the well-being of all. But taking this line had a flip side: it led to any in-depth reflection about the institutional reforms necessary to avoid further great depressions being swept under the rug.

> Instead, within the existing institutional framework, the role of the economic expert as a day-to-day manager expanded rapidly, and the role of the academic macroeconomist became that of equipping these experts with ideas, principles, formulas which gave, or appeared to give, operational guidance on the task with which these economic managers happened to be faced (Lucas 1980b] 1981a, p. 251).

For his part, Lucas strongly objected to such solicitations from civil society because they were based on the premise that some manipulations of the economy could be successful, while he felt that the theoretical framework justifying such manipulations was deeply flawed.[14] To him, the 1970s

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[14] This point is well developed in an aside to Lucas's "Understanding Business Cycles" article: "Accompanying the redirection of scientific interest occasioned by the Keynesian Revolution was a sharp change in the nature of the contribution to policy which economists hoped to offer and which the public has come largely to accept. The effort to 'explain business cycles' had been directed at identifying institutional sources of instability, with the hope that, once understood, these sources could be removed or their influence mitigated by appropriate institutional changes. The process envisaged was the painfully slow one of public discussion and legislative reform; on the other side, there was the hope of long-term or 'permanent' institutional improvement. The abandonment of the

stagflation episode was the day of reckoning, the symptom of much deeper problems, namely the end of the vision associated with the Employment Act and Keynesian macroeconomics.

All in all, Lucas has kept faith with the principles he espoused in the “Rules, Discretion…” paper. Compared to other economists of his standing (i.e. Nobel-prize winners), his presence in the media and his ‘expert’ offerings of advice have been scarce.

3 Lucas on the ‘Non-Interference’ Precept

There is a widespread view that ideology should not interfere with theory. I propose to call this the ‘non-interference’ precept. At first sight, there is little doubt that Lucas fully adheres to this precept. His criticism of Coase and Hayek mentioned above seems to be that asserting the identity of theory and model is the only way to chase ideology away. However, signs of a different standpoint can be found in his draft writings and correspondence. In these, he seems to admit that ideology underpins theoretical development. Take the following passage belonging to the fragment on which I drew heavily above:

> A purely sociological consequence of the advent of rational expectations has been a turning of this rhetorical table: now Keynesians are on the defensive about their *models* (or lack of them). There is no doubt that this advantage will prove transient. Keynesians are already learning to ‘wing it’ on policy questions, without use of the IS-LM apparatus, or

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effort to explain business cycles accompanied a belief that policy could effect immediate, or very short-term, movement of the economy from an undesirable current state, *however arrived at*, to a better state. The belief that the latter objective is attainable, and the attempt to come closer to achieving it is the only legitimate task of research in aggregate economics, is so widespread that argument to the contrary is viewed as ‘destructive’, a willful attempt to make life more difficult for one’s colleagues who are only trying to improve the lot of mankind. Yet the situation is symmetric. If the business cycle theorists were correct, the short-term manipulation on which much of aggregative economics is now focused only diverts attention from discussion of stabilization policies which might actually be effective; such postponement is, moreover, accompanied by the steady and entirely understandable erosion in the belief on the part of non-economists that aggregative economics has anything useful to say” (Lucas [1977] 1981a, pp. 216–217, Lucas’s emphasis).

> with any other apparatus to replace it (Lucas, various, Box 23, Barro folder).

Here, Lucas's point is that Keynesian economists will react to their loss of supremacy by inventing models that will vindicate their ideological standpoint, a prediction that has been fulfilled. However, Lucas is wrong when he said that the Keynesians' strategy of 'winging it' was a purely sociological occurrence. They may well have acted on ideological grounds, but the end result is that new models were created. Hence their attempt was as much theoretical as ideological! What is true for Keynesians is of course also true for anti-Keynesians. Friedman is a fine example in this respect. His Presidential Address (Friedman 1968), which blazed the way for the new classical revolution, is a good example of 'winging it'!

Thus ideology underpins economists' work. However, in Lucas's opinion, theoretical discussions cannot settle ideological disputes. The following two quotations, the first drawn from the same fragment as above, the second from Lucas's correspondence, make the point:

> The classical issues of the proper role of government in a democratic society, of 'laws versus men' or 'rules versus authority', are not going to be settled by technical advances in economics. It follows that no one's position on such basic questions needs to be threatened by such new technologies as may come to be at our disposal [Lucas has in mind here rational expectations MDV] (Lucas, various, Box 23).

> Really, there is never going to be such a thing as an uncontroversial way to settle disputes over economic policy, nor do I see why one would hope for such a state of affairs. It seems that our job is to try to make controversy *useful*, by focusing on discussable, analyzable issues. In Taylor's first paper, for example, contract length was selected arbitrarily (hence 'controversially') and was central to the operating characteristics of the model. But labor contracts are something we can find independent evidence on (as John did) or theorize about (as many people are now doing). Work like this is productive not because it settles policy issues in a way that honest people can't disagree over but because it channels controversy onto potentially productive tracks, because it gets us talking

> and thinking about issues that our equipment may let us make some progress on. (Lucas, various, Box 29, letter to Sims, dated July 15, 1982).

In this passage, Lucas seems to admit that the position taken by macroeconomists is influenced by their ideological standpoint. This being granted, economists should "channel controversy onto productive tracks". But 'channeling' ideology does not mean excluding it, while 'productive tracks' does mean using mathematical models (i.e. making the conversation mathematical). The authors who invented staggering-contracts models wanted to justify the efficiency of the monetary-policy claim against Friedman and Lucas, a project that Lucas found legitimate and conducive to progress. But this opinion amounted to accepting that theory is ideologically driven. In this way, Lucas, to all intents and purposes, moves away from the non-interference principle.

If my interpretation is accepted, what counts for Lucas is that ideology, the presence of which cannot be avoided, is at least tamed. For him, this happens when theory takes a mathematical form.[15]

4 Lucas on the 'Non-Exploitation' Precept

The 'non-interference' precept, discussed above, is easy to understand. Next, I want to discuss a more mysterious principle, namely the idea that the policy conclusions of models should *not* be translated into policy recommendations for governments.

Lucas wants macroeconomic models to result in policy conclusions. This will raise no eyebrows. More intriguing, however, is his cautious attitude to the political exploitation of these conclusions. The following passage from the concluding section of his "Understanding Business Cycles" article is instructive in this respect:

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[15] A by-product of this form is that simplifies interpretation. Any theory written in prose runs the risk of generating endless sterile hermeneutic discussions, as witnessed by the fate of Keynes's *General Theory* (1936). The use of mathematical models eliminates such ambiguities.

> By seeking an equilibrium account of business cycles, one accepts *in advance* rather severe limitations on the scope of governmental countercyclical policy which might be rationalized by the theory (Lucas [1977] 1981a, p. 234, Lucas's emphasis).

The important element of this quote is the term 'in advance', italicized by Lucas. By stressing this he admits that the limitation on countercyclical policy follows from the premises of his model. If this is the case, in all honesty, the model-builder economist has no grounds for recommending the government not to engage in countercyclical policy. Thus, Lucas's position is balanced on a razor's edge: models need to reach policy conclusions, but economists should refrain from exploiting these conclusions politically.

Again, while this point is only hinted at in Lucas's published papers, much more is to be found in the archives:

> One now reads of rational expectations not in *Econometrica* but in *Time* and *Business Week*, where it appears as a 'school' or 'theory' with apparently sweeping implications for important issues of economic policy. These implications seem primarily of a "conservative" cast, favoring a reducing role for government, balanced budget fiscal policy, and tight and "unaccommodating" monetary policy. Now the idea of limited government, budget balance and tight money are not unimportant to me; they are high on the list of values I carry into the voting booth every year, and for reasons I am willing to defend in some detail. These developments are not, then, one which I find unwelcome or displeasing, nor do I find the journalistic treatment of rational expectations any less accurate than similar treatments of other developments in economics. Yet there is something wrong, and necessarily transient, with this easy translation of a technical contribution to economic theory into a platform for economic policy. … There can be no simple connection between what appears on the scratch pads of professional economists, however original, and important

conclusions about the way our society ought to operate (Lucas, various, Box 23, Barro folder).[16]

Interpreted in a minimal way, this passage states that economists should be cautious in extending the policy conclusions of their models into direct advice to governments. Taken maximally, it means that economists should totally refrain from politically exploiting the results of their models. This is the 'non-exploitation' precept: since policy conclusions are embedded in the models' premises, intellectual honesty forbids transforming them into policy recommendations.

What justifies such a precept? There would be no problem if an ideological view and a policy conclusion were separate notions. But this is not so. Take Lucas's "Expectations and the Neutrality of Money" article ([1977] 1981a,). It is a confirmation of Friedman's theoretical conclusion about the inefficiency of monetary policy (Friedman 1968). An associated conclusion is that central bankers should be constrained by rules instead of enjoying discretion. Look now at the ideological sphere. The free-market ideology states that the market economy works better without state interference. This general principle can take many concrete forms. When it comes to monetary matters, the prevalence of rules over discretion is one of these. But since this prevalence is grounded on the inefficiency of monetary policy, the latter should also be considered a tenet of the free-market ideology. So, the inefficiency of monetary policy is both a theoretical conclusion of the models used by Friedman and Lucas and a tenet of the free-market ideology. But then it must be recognized that Lucas's theory

[16] That there no simple connection does not mean that there is no connection. The text continues: "There is also, I believe, something very right about these connections, something very permanent. If this belief is correct, then it may be of value to attempt to distinguish exactly what it is that is wrong about currently popular views about rational expectations and economic policy from that which is right. Doing this successfully would involve discussing the relationship between theory and policy in terms which differ rather markedly, but which are not really more difficult or technical, from those in current use in policy oriented discussions" (Lucas, various, Box 23, Barro folder). Unfortunately, these passages come from loose sheets and, while Lucas seems to announce a new discussion of the relationship between theory and policy, nothing of the kind is to be found in the archives.

has an ideological dimension.[17] The non-exploitation principle permits its presence to be neutralized.

The excerpts from Lucas's writings I have explored up to now suggest that Lucas abides by the 'non-exploitation' precept. Such an attitude of 'methodological purism' requires a high degree of stoicism, since it amounts for the model-builder to forgoing the opportunity to promote her preferred ideology. However, as was the case for the 'non-interference' principle, other passages suggest that escapes from the non-exploitation principle are possible, what of course would be good news because it is odd to require that macroeconomic models led to policy conclusions while refusing to take stock of them.

Although this has only rarely been pointed out (see however, Hahn (1982), Modigliani (1989) and De Vroey (2004)), Friedman's policy conclusion that monetary expansion has no durable effect on unemployment is premised on the assumption that the natural rate of unemployment has already been attained when this expansion takes place. This is a case where Keynesians would admit that there is no rationale for monetary activism. In other words, the inefficiency conclusion is as much built into the premises of Friedman's model as it is in Lucas's. Hence the non-exploitation precept should apply. However, this is not the line taken by Lucas. The reason he took another line is that an additional element was brought into the picture, the possibility of obtaining an empirical validation of the theory's prediction, a possibility that was absent from his own models. 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, the result of which was a hands down victory for Friedman's theory. After an experiment of that kind, any qualms about the political exploitation of the policy conclusions of the model in point can be put aside.

Admittedly, such real-world experiments are rare. While they might be engineered, it is fortunate that this does not happen—"a real virtue of the democratic system is that we do not look kindly on people who want to use our lives as a laboratory" (Lucas 1988, p. 1). Substitutes exist, however.

[17] The same is true, of course, for Keynesian or neo-Keynesian theory.

Above, I have dwelled on two features of the analogy building enterprise, that it is a story-telling activity and that, to all intents and purposes, the story told ought to be encapsulated in a mathematical model. Now an additional element needs to be introduced: the analogous fictive economy must be put to the test of a confrontation with reality. The more accurate the model's predictions in past experiments, the more trustworthy its new usages will be.

Again, it must be concluded that Lucas takes a razor edge position. On the one hand, he reckons that intellectual honesty forbids to recommend a model's policy conclusion to the government in as far as this conclusion is embedded in the premises of the model. This is the non-exploitation principle. On the other hand, this principle can be overtaken in as far as a way of empirically assessing the model's validity exists.

5 Assessing Lucas's Standpoint

5.1 Lucas on Methodology, Compared to Methodologists on Methodology

My analysis indicates that Lucas has devoted a lot of attention to methodology. However, this is methodology as conceived by an economist, not methodologists' methodology. Treading in Friedman's footsteps when he wrote "The Methodology of Positive Economics" (Friedman 1953), Lucas is a self-taught methodologist who does not bother to relate his thoughts on methodology to the methodological literature. Had he done so, he would have realized that his identification of a theory and a model is very much a minority viewpoint. Most methodologists take the opposite stance.[18] While assessing the pros and cons of the two standpoints is beyond my competence, I would nonetheless criticize Lucas for hammering away at his claim that a theory and a model are identical (the result of which is that only mathematical economics is admissible economics) without justifying it in a systematic way. He contents himself to declare,

[18] See e.g. Hands (2001), Hausman (1992), Mäki (2002), Morgan and Morrison (1999).

firstly, that no cumulative progress can occur without this identity and, secondly, that it prevents theory from being contaminated with ideology. Avoiding these pitfalls is an undeniable advantage of the mathematical method. However, pointing them out may not suffice to seal the matter.

The narrowness of Lucas's conception of theory also surfaces when considering where he locates the boundary between ideology and theory. In my view, Lucas's definition of ideology is too broad. As we have seen, to him, everything that is not mathematically modeled is ideological. I am of the opinion that there are better ways to make the division between ideology and theory. First, draw a distinction between ideology, defined as a normative viewpoint about the respective roles of the state and the market in organizing the economy, and theory, defined as an articulated conceptual apparatus. Then distinguish two types of theory: those abiding by the Walras–Lucas standpoint, where a theory is a mathematical model, and those adhering to the Marshall–Leijonhufvud view (also that of most methodologists) that a theory and a model operate at two different levels. In this taxonomy, Coase's and Hayek's work and many other studies, including Keynes's *General Theory*, are included in the 'theory' category. Lucas's dismissive judgment about such contributions could still be held but would then just relate to their being an unproductive way of doing theory.

The analogy idea is another of Lucas's idiosyncrasies. As far as I am aware, this idea is absent from methodological writings. Not surprisingly, the only place I encountered a similar view was in an article written by another economic theorist venturing into the field of methodology, Robert Sugden. In his, "Credible Worlds: The Status of Theoretical Models in Economic" (Sugden 2002), Sugden reflects on model-building in economics by examining the methodological status of two well-know papers, Akerlof's market-for-lemons article (Akerlof 1970) and Schellings's "Sorting and Mixing: Race and Sex" paper introducing the checkerboard city model (Schelling 1978). Comparing these models to prevailing methodological conceptions, Sugden concludes that none of the prevailing approaches can completely account for the distinct contributions of these two papers. He ends up proposing his own take on the subject. Here is how he summarizes it:

We gain confidence in such inductive inferences, I suggest, by being able to see the relevant models as instances of some category, some of whose instances actually exist in the real world. Thus, we see Schelling's checkerboard cities *as possible cities*, alongside real cities like New York and Philadelphia. We see Akerlof's used-car market *as a possible market*, alongside real markets such as the real market for used cars in a particular city, or the market for a particular type of insurance…On this view, the model is not so much an abstraction from reality as a *parallel reality*. The model world is not constructed by starting with the real world and stripping out complicating factors: although the model is *simpler* than the real world, the one is not a *simplification* of the other. The model is realistic in the same sense that a novel can be called realistic. In a realistic novel, the characters and locations are imaginary, but the author has to convince us that they are *credible*—that there could be people and places like those in the novel" (Sugden 2002, p. 131; his emphasis).

Sugden's view of models as credible worlds comes close to Lucas's definition of models as pertaining to analogous economies. However, as will be seen below, there is an important difference between the two views since, unlike Lucas, 'credible world' economists stop short of extolling the virtues of empirical work.

5.2 A Political Agenda?

Once it is recognized that Lucas is aware of the ideological underpinning of theoretical work, can the further step be made of declaring that—horror of horrors—the new classical revolution resulted from a political agenda? Or, to put it the other way round, is it conceivable that the transformation from a paradigm supporting a given ideology to one supporting the rival ideology could arise without a political agenda or ideological motivation? Beyond doubt, Luca's answer to this last question will be 'yes', and he would cite his own work as a case in point. This he made clear in his "My Keynesian Education" (Lucas 2004) lecture. In this piece, he developed the view that there was a time when he considered himself a Keynesian—not a fundamentalist Keynesian, taking the *General Theory* as a holy text, but somebody ready to work on developing Keynesian empirical models. But, the story goes on, in experimenting with these models, he stumbled over methodological problems which, upon further scrutiny, proved to be insuperable. So, when Lucas became a strong opponent of Keynesian theory, he claimed it was on purely scientific grounds.[19]

However, this might not be the full story. We do not know when Lucas became a defender of free-market ideology, but we do know from his “Professional Memoir” (Lucas 2001) that this approach was not one that he imbibed with his mother’s milk. So, at some time, he must have changed his mind. It can be presumed that this happened during his graduate education in Chicago. If he started to work within the Keynesian paradigm after becoming a conservative, there must have been, at this time, a tension between his own ideology and the policy conclusion towards which the approach he was working in was geared. At this juncture, two events occurred more or less concomitantly: Lucas’s discovery of the methodological flaws in Keynesian theory, and Friedman’s introduction of the idea of a natural rate of unemployment. For my part, I believe that Lucas’s criticisms of Keynesian theory were basically correct. But had he been Keynesian in an ideological sense, he might have decided to work on reconstructing Keynesian theory so as to preserve the congruency between his ideology and the theory—to become a ‘non-Walrasian’ macroeconomist à la Barro–Grossman, Drèze, or Benassy, to create new Keynesian models

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[19] The following observations that Sargent made in interviews can, I think, be safely extended to Lucas: “*But isn’t it the case that what you define as a problem depends on what your starting position is?* Absolutely. That’s exactly why rational expectations stuff was developed by people within the Keynesian tradition. There were people trying to knock off and destroy the Keynesian tradition from the outside, who weren’t sympathetic enough to it to learn it. And I mean it was the monetarist tradition or something like that. And the paradox is that, I would say what’s ended up being perceived as the most destructive in Keynesian tradition is from its own children. You know? Because, if you look at what, say Lucas, Prescott, Wallace, and Barro were working on, those were all pieces of a Keynesian model, a high-tech Keynesian style” (Sargent interviewed by Sent (1998, pp. 165–166). “[Klamer]: *Are the political aspects an important question?* [Sargent]: I am not really interested in politics. The rational expectations stuff is clearly not politically motivated. People from all sorts of different political perspectives contribute to it. It’s more a technical revolution .… No, it’s certainly not politically motivated” (Sargent interviewed by Klamer 1984 p. 80).

(e.g. staggering contracts models) or to take a line similar to Gordon's. But, as I have already suggested, Lucas was experiencing a lack of congruency between his ideology and Keynesian theory. Friedman's theory offered him the possibility of changing this state of affairs. Two models stood side by side, the Keynesian Phillips curve and the natural rate of unemployment models, underpinned by opposing ideological visions. By writing his "Expectations and the Neutrality of Money" article, Lucas chose Friedman's camp. Here, the historian of economic theory faces a signal extraction problem: there is one observation, the move towards a model supporting conservative policy conclusions, and two possible causes, ideology and theory. It may be surmised that both have played a part. If this surmise is accepted, we must conclude that the ideological dimension played a role in Lucas's work, and there is no shame in this.

Moreover, even if it is accepted that Lucas's transformation of macroeconomics comprises an ideological dimension, it would still be an exaggeration to say that it followed from a political agenda. Actually, Lucas, Sargent and their associates did not need to act upon a political agenda because that the job of gearing theory towards a political agenda other than the Keynesian one had already been carried out by Milton Friedman. Hence they could concentrate on working, as pure technicians, on the conceptual and technical modifications that would provide firmer ground for Friedman's policy conclusions. In other words, it seems that a division of labor occurred implicitly, with Friedman doing the political job, and the next generation undertaking the theoretical developments needed to underpin the political agenda initiated by Friedman.[20]

5.3 The Consistency of Lucas's Conception of Theory

I have already pointed out that Lucas wavers between two positions on the relation between ideology and theory, as well as on the political use of

[20] An additional caveat is that a radical turn in policy conclusions does not need a scientific revolution. To wit, Friedman's expectations-augmented Phillips curve model was hardly a full break away from standard Keynesian macroeconomics. The same is true for New Keynesian Phillips curve models with respect to the real business cycle models.

conclusions drawn from the models. I now want to link his hesitations to his conception of a theory. Is this conception consistent? I have my doubts. At issue is whether the following set of propositions: (a) theory and model are the same thing; (b) theoretical propositions pertain to the model economy rather than to reality; (c) models should be assessed in terms of their usefulness rather than their truth value; (d) macroeconomics has a utopian dimension; is compatible with the proposition (e) that the validity of a model is to be ascertained on the ground of its predictive ability.

The rationale for raising this question lies in my past work on the history of neoclassical theory, which led me to claim that the Walrasian and the Marshallian approaches should be viewed as two alternative rather than complementary research programs.[21] Lausanne and Chicago are two different universes. Authors such as Arrow, Debreu, and Hahn, of the Lausanne School, have repeatedly hammered home the point that Walrasian general equilibrium theory is an abstract construction, the strength of which lies in its ability to posit issues in a rigorous way. Its main interest with respect to reality, they argue, is to provide a negative benchmark. The Chicago approach is totally different. According to Friedman, whom I take as the emblematic Marshallian economist, the aim of economic theory is to answer specific questions, not at the level of principles but in order to reveal concrete truths. The theorist ought to elicit propositions about reality that have a predictive value, and the decision about the merits of rival theories should be based on a comparison of their predictions.

I am of the opinion the two schools each have numerous merits, but they should not be mixed. My criticism of Lucas's methodological vision is then that he has one foot in Lausanne and the other in Chicago. Symptomatic of this ambiguity is the fact that, while he himself claims a Walrasian

[21] For a succinct account of this research, see De Vroey (2009a, 2009b). It can be inferred from what Lucas said in interviews that he might concur with my claim: "Question [to Lucas]: You acknowledge that Friedman has had a great influence on you, yet his methodological approach is completely different to your own approach to macroeconomics. Why did his methodological approach not appeal to you? Answer: I like mathematics and general equilibrium theory. Friedman didn't. ... Question: His methodological approach seems more in keeping with Keynes and Marshall. Answer: He describes himself as Marshallian, although I don't know quite what it means. Whatever it is, it's not what I think of myself" (Lucas's interview by Snowdon and Vane 1998, p.132).

affiliation—"I am a hopeless 'neo-Walrasian'" (Lucas, various, Box 30, Correspondence 1977–79 folder, in a letter to Driscoll, dated November 23, 1977)—most neo-Walrasians are reluctant to acknowledge him as one of them.

> Bob [Lucas] was in the Chicago tradition and was very concerned about empirical testing—whatever the hell that means—something that I have little sympathy for and very little interest in, to be perfectly honest. So there was quite a difference in viewpoints about why you did theory and what the relevance of theory is (Cass's interview with Spear and Wright 1988, p. 546).

Likewise, I doubt that Friedman would have been pleased with Lucas's claim of allegiance to the neo-Walrasian paradigm. To him, the view that the validity of any economic theory rests on its predictive power and the rejection of general equilibrium à la Walras were part and parcel.[22]

To further develop my point, I will proceed step by step. Let me first consider Lucas's conception of theory under the assumption that it comprises only propositions (a) to (d). This amounts to just considering Lucas's "Expectations and the Neutrality of Money" and "An Equilibrium Model of the Business Cycle" papers and forgetting about his endorsement of real business cycle models. I pose two questions: first, which affiliation would best fit this redefinition of Lucas's conception of theory, the Walrasian or the Marshallian? Second, can this conception of a theory be considered robust?

My answer to the first question is that Lucas's approach, as redefined above, is purely Walrasian. Neo-Walrasian economists could easily hail these papers as excellent contributions to their field. Marshallians, such as Friedman, would agree with this diagnosis, and for this reason they would find it uninteresting. As to the second question, my answer is 'Yes' with an important qualification. Take Lucas's "Expectations and the Neutrality of Money" model (Lucas [1972],1981). While its logical consistence is not to be questioned, its usefulness is more problematic. Remember that Lucas

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[22] Friedman's claim was that Walrasian theory was too cumbersome an apparatus to be of any use for doing applied work. On this, see De Vroey 2009b).

states that the right question to ask about a given model economy is whether it is useful, not whether it is true. Neo-Walrasians would agree with this statement, for it would be surprising if any economist declared that his or her theory was totally useless. But, this being said, neo-Walrasians are rather vague when it comes to describing what their theory's usefulness consists of.[23] If Lucas's conception of theory excluded empirical testing, he would have to fall back on the negative benchmark argument of traditional neo-Walrasians instead of his more precise meaning of usefulness as the ability of analogous models to be empirically assessed.

Let me now drop the fable of a Lucasian conception of theory without the empirical comparison feature, and return to his full-fledged conception (that is, including (e)). The same two questions can be asked. As to the first one (Marshall or Walras?), it can no longer be concluded that this conception is purely Walrasian. Standard neo-Walrasians would be reluctant to embrace it because it fails, they would argue, to recognize the limits of the Walrasian (or neo-Walrasian) research program.

> The [Walrasian] 'equilibrium' story is one in which empirical work, ideas of fact and falsifications, played no role at all (Weintraub 1983, p. 37).

> It is for all these reasons that I have always held the view that the Walrasian theory in all of its manifestations is an important theoretical benchmark but that a vast and unruly terrain had to be traversed before one understood (let alone predicted) the behavior of an actual economy. No economist and certainly no theorist should be ignorant of the Walrasian theory, and no economist and certainly no theorist should

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[23] For example, in the sequel to the passage of Cass's interview quoted above, Cass says that "I am still of the opinion that theory is more a way of organizing your thoughts, how you would think about the world. And it's strongest in providing counterexamples when people confidently clam that something is true in general" (Cass 1988, p. 546). In the same vein, Hahn writes that "The importance of this intellectual achievement [The Arrow-Debreu model] is that it provides a benchmark. By this I mean that it serves a function similar to that which a perfectly healthy body might serve a clinical diagnostician when he looks at an actual body" (Hahn [1982] 1984, p. 308).

pronounce on actual economies and policies on its basis alone (Hahn 1983, p. 224). [24]

By contrast, Marshallians would full-heartedly endorse Lucas's last proposition (that the validity of a model is to be ascertained on the ground of its predictive ability). They would thereby rejoice that Lucas remains a Chicago economist, despite what they would see as his flirtation with the Lausanne School on other scores.

What about the consistency of the complete Lucasian methodological standpoint? Here it less Lucas's own research that ought to be considered than that of his followers, in particular Kydland and Prescott, whose methodology he endorsed. Abandoning the traditional path taken by econometricians, i.e. statistical inference, Kydland and Prescott opted for an alternative strategy, calibration, which they borrowed from computational general equilibrium analysis.[25] Calibration consists of assigning values to the model's parameters by drawing from panels, national accountings or other data banks. Whenever such data are unavailable, the vacuum must be filled by giving values that are in accordance with what economic theory teaches.[26] Calibration completed, Kydland and Prescott's method implies a series of additional steps ending up with comparing stylized facts of reality such as variances and cross-relations with their simulated equivalents from the model. As soon as it turns out that the model mimics the stylized fact,

[24] Walras himself could be invoked. On page 17 of his copy of Cournot's *Principes de la théorie de la richesse*, held at the Centre Walras-Pareto of the University of Lausanne, he made the following annotation: "la théorie pure n'attend aucune confirmation de la réalité" [pure theory requires no confirmation from reality] (Baranzini and Bridel 2005, p. 360, note 3).

[25] While there are many testimonies of Lucas's endorsing the Kydland and Prescott research line (see e.g. the quotation given p. 4), he has been more evasive as far as calibration *per se* is considered (see Lucas 2001, p. 28 and Lucas 2007). However since Kydland and Prescott is in large part applied and since calibration is the cornerstone of computation, Lucas's general endorsement cannot but encompass calibration.

[26] Proper statistical inference is thus put aside as unfit for the theoretical purpose. "We emphasize that quantitative theoretical research and empirical research are fundamentally different activities and fundamentally different tools are needed. If the objective of the research is to derive the quantitative implications of the neoclassical growth theory for business cycle fluctuations, the use of statistical tools to select the parameters that best fit the business cycle observation is not sound scientific practice" (Prescott and Candler 2008, p.1-2).

success is proclaimed. Thereby, Friedman's methodology seems to be honored: an admittedly non-realistic model can be hailed as solid theory because its predictions have been verified.

Above, I have brought out Lucas's scruples about exploiting the policy conclusions of models whenever these conclusions happen to be embedded in the premises of the model. I have also shown that bringing empirical verification into the picture allows them to be lifted. Assume verification is successful. Thereby, when it comes to assessing the effects of possible policy measures, the analogous model becomes almost as reliable as real-world episodes for the purpose. The policy conclusions drawn from the models may still square with the model-builder's ideological preferences but this would be nothing more than a coincidence. Had the model-builder's personal ideology been to the opposite, he or she should nonetheless be compelled to recommend the conclusion of the model since 'facts have spoken'. Ideology it thus tamed. Facts are judge and jury.

Unfortunately, things are not as simple. In effect, the calibration method has hardly made the unanimity amongst empirical macroeconomists.[27] Such an absence of methodological consensus has a direct impact on my discussion as it forbids eradicating the non-exploitation principle. Not that calibration is a useless exercise. The model's 'success' may increase the confidence in the value of the analogous model. The limitations and flaws that are being brought out are also useful since they pave the way, to use Lucas expression in 1988 address, for "better and more instructive analogies" (Lucas 1988, p. 5). But all in all, facts come short of playing the role that Friedman ascribed to them. What is provided is just a measure of closeness to the data. Thus, the non-exploitation principle should keep ruling and Lucas to remain bound by it (on the condition, of course, that my account of his views is correct).

A series of further implications follow on. First, Lucas's endorsement of real business cycle models should be toned down. Second, he should

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[27] To get a flavor of the existing debate, the reader is invited to consult the symposium on Macroeconomics and Methodology in Volume 10 of *The Journal of Economic Perspective* (1996). It comprises a bullish extolling of calibration by Kydland and Prescott (Kydland and Prescott 1996) followed by a scathing attack by Hansen and Heckman (Hansen and Heckman 1996) and a less aggressive but nonetheless stern criticism by Sims (Sims 1996).

criticize his followers when they apply to reality propositions that are valid only for the model economy or when they forget that their conclusions are embedded in their model's premise.[28] Third and finally, sticking to the non-exploitation principle may be fine as far as intellectual honesty is concerned but what if one is alone in abiding by it since this amounts to wanting to be saint in a world of sinners?

6 Concluding Remarks

The aim of this paper was to assess Lucas's methodological ideas, in particular the relationship he saw between ideology and theory, and *not* his contribution to the development of macroeconomics. Two conclusions can be drawn. First, Lucas must be praised for having engaged in a detailed reflection on methodological issues. Few, if any, contemporary theorists have dealt with methodology in such a subtle and original way as he did. My second conclusion is that the consistency of Lucas's overall conception can be questioned. I view him as walking on a tightrope. I have shown that, while there are good grounds for concluding that Lucas adheres to the 'non-interference' and the 'non-exploitation' precepts, other passages in his writing tilt towards the opposite viewpoint. But the main ambiguity lies in his endorsement of Kydland and Prescott's grafting of a Friedmanian empirical perspective onto a neo-Walrasian model.

The lesson I draw from this is that, somewhat oddly, being consistent in the meta-theoretical field can be more difficult than being consistent in the theoretical field. The existence of these tensions may also explain why Lucas never wrote a synthesis of his methodological ideas, contenting himself with acting as a Zen master providing profound, if elliptic, remarks

[28] For example, in their reply to Temin's criticism of RBC studies of the Great Depression (2008), Kehoe and Prescott write that the tentative finding of the book they edited is that "bad government policies can turn ordinary economic downturns into great depressions. These findings are especially relevant now, in the late 2008" (Kehoe and Prescott 2008, p. 21). It happens that the models defended by Kehoe and Prescott are constructed in such a way that the government is the only possible suspect of any malfunctioning. Hence it can be stated that their conclusion is embedded in the premises of these models.

but failing to provide any clues about how the pieces of the jigsaw fit together.

Acknowledgement A first version of this paper was presented at seminars given at the University of Toronto and the University of British Columbia. I am grateful to the participants at these seminars for their remarks. I also want to acknowledge my gratitude to Robert Lucas for having authorized him to quote from the Lucas Archives held at Duke University, as well as for his comments on the paper. Kevin Hoover's and David Laidler vivid comments on an earlier version were also stimulating. Finally, I thank Alessandro Vercelli, the two anonymous referees and the five invited readers of *Economics, The Open-Access, Open-Assessment E-Journal* for their interesting comments.

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