Models as Laboratories. Robert E. Lucas on Expertise.

Francesco Sergi*

ESHET Annual Conference – Young Scholar Program
Antwerp, 21-22/05/2017

Abstract

This article addresses Robert Lucas's methodological view about the relation among theories, models and the real world. My central claim is that Lucas's methodology is ambivalent. On the one hand, in his published works, Lucas is advocating for a clear-cut distinction between a-realistic models' assumptions and realistic models' implications. In this first perspective, models' pertinence with respect to the real world relies only on realism of models' implications. On the other hand, especially in unpublished writings, Lucas argues that models' assumptions are "analogies" of casual mechanisms. In this second perspective, models' pertinence relies also on the consistency between models' assumptions and the real world. As these two perspectives are mutually exclusive we can highlight how Lucas's methodology is ambivalent. Furthermore, I suggest that this ambivalence results from his vision of models as "laboratories" for expertise.

*Université Paris 1 - Centre d'Économie de la Sorbonne (CES). Francesco.Sergi@univ-paris1.fr.

Introduction

This contribution to the history of macroeconomics addresses Robert E. Lucas's methodological views on the relation among theories, models and the real world.[1] This perspective is inspired by Morgan and Morrison (1999)'s account of models. It relies on two claims: first, models are an autonomous scientific practice, distinct from theory and observation; secondly, models play the role of "mediators" between theories and the real world.[2]

In order to analyze Lucas's view on the relation among theories, models and real world, I provide a particular epistemological framework, involving the following four criteria. (1) Modeling is about defining "assumptions" (formalisms and concepts providing the syntax and the semantic of models)[3] and deriving "implications" (the logical consequences inferred from models' assumptions). (2) Models need to be assessed with respect to an internal validity condition—i.e. a set of criteria defining the relationship between theories and models. (3) Models need to meet an external validity condition—i.e. a set of criteria defining the relation between models and the real world. (4) Modelers need to decide if they rank internal over external validity condition (or the opposite), to be able to discriminate between competing models. This particular framework allows me to link Lucas's methodology to the epistemological question of the link between casual explanation and expertise.

The central claim of this article is that Lucas's methodology is ambivalent toward the external validity condition. On the one hand, in his published works, Lucas advocates for a clear-cut distinction between the epistemological status of models' assumptions and the status of models' implications:

[1] Within economics, let's take the following definitions. By "real world", I refer to the set of phenomena "observed" by economists: observation is the process of establishing some characteristics of interest of the real world, either on a qualitative or quantitative manner. I mean by "models" logical-mathematical systems, built on "assumptions" and "implications" (*cf. infra*). Finally, "theories" are logically consistent sets of formal (mathematical) and informal (literal) concepts, relations, laws: such a system helps in understanding or expressing the real world. Note that these definitions leave aside the controversial issues surrounding the epistemological and ontological status of the real world—namely, if such a world actually exists independently from theory and observation—to the extent that such a question is not pertinent for my topic.

[2] Furthermore, such a framework allows a broader view of the history of macroeconomics (Sergi, 2017). For a deeper discussion of the literature in philosophy of science about models, see Sergi (2014). Note that my work has no ambition of providing a contribution to that literature; indeed, I simply used its ideas for building my specific historiographical viewpoint.

[3] For instance, in a model about consumption behavior, "utility" is a concept and $U(.)$ they way of formalizing it. The former provides the semantic of the mathematical system, the latter its syntax; together, they represent an assumption of this model.

the former are not propositions about the real world (they are a-realistic), while the latter should be assessed with respect to their consistency with observations. In this perspective, the external validity condition relies on "realism" of models' implications (section 1). On the other hand, especially in unpublished writings, Lucas argues that models' assumptions are propositions about the real world, providing "analogies" of *casual mechanisms* at work in the real world. In this view, the external validity condition involves also an assessment of the consistency between models' assumptions and the real world. As these two definitions of external validity are mutually exclusive, and to the extent that Lucas is supporting both, we can highlight how his methodology is ambivalent. Furthermore, my claim is that this ambivalence results from Lucas's idea that models should serve as "laboratories" for expertise in macroeconomic policy rules. Hence, in order to fulfill this task, models should provide casual explanation of real world phenomena (section 2). This requirement corresponds to Mäki (2005)'s definition of models as experiments, i.e. substitute (analogous) systems involving isolation (control) of casual mechanism.

My article builds on Michel De Vroey (2015)'s work. However, I try to elaborate a different understanding of the ambivalence in Lucas's methodology. Instead of using a general opposition between "Walrasian" and "Marshallian" theoretical framework, I suggest to focus on a particular matter, namely the relation between theories, models and real world. Reshaping the question in such way sheds a new light on Lucas's contribution to macroeconomics, by highlighting his emphasis on expertise and his vision of models as laboratories. Hence, my contribution suggests a new question about the central contribution made by Lucas to the methodology of macroeconomics, and an original framework in order to address such a question.[4]

My reconstruction of Lucas's methodology relies on heterogeneous materials—mainly because Lucas never had written any explicit and complete methodological contribution. This involves: articles, notes and books published by Lucas, alone or with others, for the period going from Lucas and Rapping (1969) to Lucas (1987);[5] articles, notes and comments published

[4] Note that, even if it is central, Lucas's work and especially Lucas's archives have been, until now, surprisingly neglected by historians (valuable exceptions are Hoover, 1988; Boumans, 2004; De Vroey, 2015). However, it is also important to precise that Lucas's work should not be seen as the only important contributions to the methodological turn in macroeconomics. Closest Lucas's co-authors had also developed strong methodological views—see for instance Thomas Sargent's methodology (Sent, 2006; Boumans and Sent, 2013; Goutsmedt, 2016).

[5] After this date, Lucas has focused on growth theory (Lucas, 2002). As I am addressing the role played by Lucas in the changes occurred in macroeconomics since the 1970s, I supposed that it is more pertinent to analyze only his work about business cycles.

later by Lucas and expressing his retrospective view on his own work during the 1970s (Lucas, 1996, 2001, 2004, 2007); interviews with Lucas (Klamer, 1984; Snowdon et al., 1994; Snowdon and Vane, 1998, 2005); unpublished materials and correspondence, collected at the David M. Rubenstein Rare Book and Manuscript Library (Duke University).[6]

1 Theory, models and real world in Lucas's published writings

1.1 The internal validity condition: models' assumptions and neo-Walrasian theory

In Lucas' view, a model is said to be internally valid if its assumptions rely on concepts and formalisms pertaining to "general equilibrium theory". Lucas insists on the narrow equivalence between this specific theory and models' assumptions, especially by emphasizing that the two terms are barely synonyms: "I mean theory in the sense of models" (Lucas, 1987, 2), or "I prefer to use the term 'theory' in a very narrow sense, to refer to an explicit dynamic system" (Lucas, 1988, 5).[7] By "general equilibrium theory", Lucas means more specifically neo-Walrasian theory developed by Kenneth Arrow, Gerard Debreu and Lionel McKenzie (Debreu, 1959).

This equivalence between model's assumptions and neo-Walrasian theory corresponds to an even narrower definition. For Lucas, the relevant "theory" to be incorporated into models actually corresponds to four concepts:

- individual behavior is characterized as a dynamic optimization program. Each individual optimize an intertemporal objective function, under resource constraints;
- optimal individual plans are mutually compatible. Full flexibility of prices allows this coordination;[8]
- all markets clear simultaneously at each point of time. The general equilibrium is stable and unique;

[6] I want to thank the Bruce Caldwell, director of the *Center for History of Political Economy*, for his kind welcoming and invaluable help during my visiting at Duke in the Summer 2016, which allowed me to consult Lucas's archives.

[7] The reader will note that, hereafter in this article, I will note point out again when Lucas uses the word "theory" in the sense of "models".

[8] The coordination mechanism among agents relies implicitly on the Walrasian auctioneer.

- expectations are rational, i.e. expectations made by economic agents in the model correspond to the future expected values of the model itself.

In Lucas's perspective, only models incorporating these four concepts as assumptions fulfil the internal validity condition.

I am inclined to endorse De Vroey (2015)'s view that Lucas's conception of models has a firm underpinning in neo-Walrasian general equilibrium approach. However, at a closer look, I think that we should apply at least two caveats. First, the four concepts listed above are only a partial correspondence with the broader set of concepts of neo-Walrasian theory. Some crucial features (such as coordination mechanisms, completeness markets or uniqueness and stability of equilibrium) are missing from Lucas's definition of the internal validity condition. This is certainly the main reason for the skeptical attitude of neo-Walrasians toward Lucas's work.[9] A second caveat is Lucas's definition of the theoretical approaches that he identifies as being "general equilibrium theory". Indeed, his definition assimilates Arrow-Debreu,[10] Friedrich Hayek,[11] Léon Walras (Lucas, 1986, S413), Don Patinkin,[12] and David Hume (Lucas, 1986, S405-S407 ; Lucas, 1996, 661-669). From the perspective of the history of economic thought, such a broad definition of "general equilibrium theory" look at best confusing and, at worst, nonsense.[13]

[9] See for instance the criticism coming from Arrow (1978) or from Frank Hahn (De Vroey, 2015, 194). Other criticisms, made by Jean-Michel Grandmont and Karl Shell about Lucas's definition of uniqueness (Fischer, 1996, 18; Lucas, Archives, Box 4, Folder: 1980 2/3) had given birth to a full neo-Walrasian research program about dynamic general equilibrium models with sunspots equilibria (Cherrier and Saïdi, 2015). This program is clearly a rival approach with respect to Lucas's vision of "neo-walrasian" approach to macroeconomics.

[10] Lucas (1980a, 706-708) ; Lucas and Sargent (1979 1981, 58) ; Lucas (2007, 7). See in his correspondence: "I'm afraid I am a hopeless neo-Walrasian" (Lucas, Archives, Box 3, Folder: 1977 2/2).

[11] Lucas (1977, 7, 23). Out of the record, he expresses some doubts about his own claim (Lucas, Archives, Box 3, Folder: 1977 1/2 ; Lucas, Archives, Box 5, Folder: 1982 1/2). He will indeed abandon this idea few years later: "I once thought of myself as a kind of Austrian, but Kevin Hoover's book [Hoover (1988)] persuaded me that this was just a result of my misreading of Hayek and others." (Snowdon and Vane, 1998, 121)

[12] Lucas (1980a, 696) ; Lucas (2004, 16). Apparently, this was quite a surprise for Patinkin himself: "it's gratifying to know that my seminar served as a stimulus—though I am somewhat curious about the path which this stimulus took." (Patinkin to Lucas, 23/06/1980, Lucas, Archives, Box 4, Folder: 1980 1/3) Lucas will "clarify" this stimulus some years later: "Patinkin and I are both Walrasians, *whatever that means.*" (Lucas, 2004, 16, my emphasis)...

[13] De Vroey (2015) picked up the "at best" option, and suggested a possible way of clarifying the common ground among all these different traditions. However, his reconstruction of the history of macroeconomics as a Marshallian-Walrasian divide is still controversial (see for instance Duarte, 2016).

However, Lucas's view actually makes sense if we consider his understanding of the history of economics, which relies on the idea that individual optimizing behavior, market clearing, dynamic general equilibrium and rational expectations are the stable conceptual core of economic thought across the last two centuries.

Lucas's internal validity condition relies indeed on one main justification, namely the ambition of a "reunification" of economics.[14] According to Lucas, economics has evolved, during the last two centuries, within a common theoretical framework, defined by Classical economists. Indeed, economic thought has *always* been based on the same concepts, namely individual optimizing behavior, market clearing, general equilibrium and rational expectations:

> I think the basic view of economics that Hume and Smith and Ricardo introduced, taking people as basically alike, pursuing simple goals in a pretty direct way, given their preferences [...] that this is it for economics. *We got that view from Smith and Ricardo, and there have never been any new paradigms or paradigm changes or shifts.* [...] So you've got this kind of basic line of economic theory[15].
>
> (Lucas, 2004, 21-22, my emphasis)

Hence, changes in economic theory are actually not about concepts, but about mathematical formalisms available for studying and developing these concepts—as well as in quantitative methods for discussing their empirical relevance:

> And then I see the progressive [...] element in economics as entirely technical: better mathematics, better mathematical formulation, better data, better data-processing methods, better statistical methods, better computational methods. I think of all progress in economic thinking, in the kind of basic core of economic theory, as developing entirely as learning how to do what Hume and Smith and Ricardo wanted to do, only better.
>
> (Lucas, 2004, 22)

[14] An additional justification, already emphasized by De Vroey (2011), is preserving economics from ideological bias, thanks to the peculiar rigor of new-Walrasian mathematical reasoning.

[15] Note that I will not enter into a discussion about Lucas's vision of history of economics. It is evidently simplistic, with respect to the whole literature in history of economic thought. Nevertheless, such a vision provided the first powerful background for today standard narrative of recent evolution of macroeconomics, that I had discussed elsewhere (Sergi, 2015a).

According to Lucas, the Keynesian approach to macroeconomics represents the main deviation from this path:[16] "Keynes founded that sub-discipline, called macroeconomics, because he thought that it was impossible to explain the characteristics of business cycles within the discipline imposed by classical economic theory." (Lucas and Sargent, 1979 1981, 58)

Therefore, Lucas's internal validity condition—a narrow equivalence between models' assumptions and neo-Walrasian theory—is justified as an attempt to bringing the discipline back on his original and distinctive conceptual core. This argument is closely related to well-known Lucas's agenda of "microfoundations" of macroeconomics.[17] Lucas presents his microfoundational program as a proposition to reorient economics on his historical path of progress:

> research during the 1970s forced me and many others progressively further away from [the Keynesian] view and toward a general-equilibrium point of view that seemed to me essentially the same in substance, however different in method, as the view taken by many pre-Keynesian theorists.[18]
>
> (Lucas, 1981, 2)

Microfoundations will reconcile the study of aggregate phenomena and the study of individual behavior—in short, macroeconomics and "microeconomics" (to be intended then in a narrow sense, as neo-Walrasian general equilibrium theory). Lucas sees his own work exactly as an effort to "reincorporate aggregative problems such as inflation and the business cycle within the general framework of 'microeconomic theory'." (Lucas, 1987, 107) However, as it was already emphasized by Hoover (1988, 1995, 2010), Lucas's idea of microfondations is a reductionist idea: the aggregate dimension of

[16] I will not discuss the pertinence of the generic label "Keynesian". In Lucas's writings, "Keynesian macroeconomics" refers as well to John M. Keynes, to further developments by others (especially the Hicksian IS-LM framework) and to the macroeconometric program developed by Klein and Goldberger (1955).

[17] The term "microfoundations" should be understood broadly as the analysis of the relationship between aggregate phenomena and individual behavior. As already stressed by Hoover (2012), many microfoundational programs were developed in history of macroeconomics. Lucas's microfoundational agenda—based on optimizing individual behavior, market clearing and rational expectations—is one among others. However, as it is the only agenda of interest for this paper, I will, hereafter, simply speak about "microfoundations" instead of "Lucasian microfoundations".

[18] This is consistent with Lucas's retrospective vision about his own role in the history of the discipline. He described himself as a "Romanov of the Keynesian Revolution" (Lucas, 1987, 2) and argues that his work was rather a "counter-revolution" than a "revolution" (Snowdon and Vane, 2005, 280).

economic phenomena (macroeconomics) should be collapsed into individual phenomena (microeconomics). This idea relies on methodological individualism, to the extent that aggregate phenomena lose any peculiar and autonomous characteristic (ontological, epistemological) with respect to the individual level. As a consequence, any distinction among sub-fields of economics, their objects and methods, should disappear:

> the term 'macroeconomics' will simply disappear from use [...] We will simply speak of economic theory.
>
> (Lucas, 1987, 107-108)

1.2 The external validity condition: the distinction between assumptions and implications

Lucas's external validity condition relies on a distinction between models' assumptions and models' implications—hereafter, a distinction between "a-realistic" assumptions and "realistic" implications. The former are abstract propositions, pertaining exclusively to the fictive world of models; the latter are propositions about the real world. Hence, a model is said to be externally valid if its *implications* are consistent with observation. More precisely, this consistency takes the form of an "imitation" of the statistical characteristic of the business cycle.

A-realistic assumptions

According to Lucas's published writings, model's assumptions do not seek to describe (even in terms of analogy, approximation or caricature) any of the real world observed characteristics. Assumptions are simply propositions about the fictitious world of the model, and have no direct relation with the real world—although, they serve as instruments for deriving propositions about it (*cf. infra*). Therefore, assumptions cannot be seen as "unrealistic", i.e. "false" proposition about the real world. My claim is that Lucas rather suggests that assumptions are "a-realistic"—i.e. propositions with no relation with the real world. Indeed, for Lucas, assumptions cannot be assessed in any way with respect to their consistence with the real world:

> Any model that is well enough articulated to give clear answers to the questions we put to it will necessarily be artificial, abstract, patently "unreal" [...] insistence on the "realism" of an economic model subverts its potential usefulness in thinking about reality.
>
> (Lucas, 1980a, 696)

Moreover, assumptions cannot be inferred from observation. This is consistent with the internal validity condition, and results from Lucas's endorsement of neo-Walrasian tradition, which has strong underpinning in hypothetico-deductive method.

Rational expectations, for instance, are characterized by Lucas as "axioms", which make sense exclusively in the context of modeling:

> The term "rational expectations", as Muth used it, refers to a consistency axiom for economic models, so it can be given precise meaning only in the context of specific models. I think this is why attempts to define rational expectations in a model-free way tend to come out either vacuous ("People do the best they can with the information they have") or silly ("People know the true structure of the world they live in").
>
> (Lucas, 1987, 13)

Hence, rational expectations should not be seen as an attempt of describing the way economic agents *of the real world* build their expectations. Such an assumption simply aims at characterizing the way economic agents *of the model* formulate expectations:

> 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, quoted in De Vroey, 2015, 177)

As a consequence, rational expectations as models' assumptions cannot be evaluated (qualitatively or quantitatively) with respect to their accuracy or plausibility. They cannot be "tested" or associated with any real world observation: "no brain map is ever going to label a clump of cells the rational expectations center" (Lucas, Archives, Box 13, Folder: Directions of macroeconomics 1979).

The realism of implications

Conversely, models' implications are propositions about the real world. In order to assess such propositions, one need to look at their consistency with quantitative and/or qualitatively observation of the real world. "Qualitative observation" involves no statistical or econometric apparatus. In what follows, Lucas is mostly referring to quantitative observation, involving statistical methods. More precisely, the assessment of implications relies on their ability to *imitate* the real world: "A 'good' model, from this point of view,

will not be exactly more 'real' than a poor one, but will provide better imitations" (Lucas, 1980a, 697).[19] Hence, the relation between models and the real world entirely relies on the imitation provided by model's implications.

The assessment of the external validity condition should be carried out along the dimensions targeted by the model—in his writing, the "questions" that we asked to the model:

> Of course, what one means by a "better imitation" will depend on the particular questions to which one wishes answers
>
> (Lucas, 1980a, 697)

According to Lucas, the "particular" question asked to a *macroeconomic* model is about the "business cycle". By this expression, he means a set of recurrent co-movements of aggregate time series about a trend (the fact that "aggregate variables undergo repeated fluctuations about trend" (Lucas, 1977, 7)).[20] Co-movements in aggregate variables can be statistically measured by covariance between those variables, paying attention to their signs, magnitudes—especially relative magnitudes—and lags.[21] The business cycle is, in Lucas's understanding, a homogeneous phenomenon, involving empirically observed regularities across all market economies: "in capitalist economies, aggregate variables undergo repeated fluctuations about trend, all of essentially the same character." (Lucas, 1977, 7)[22] Hence, business cycles are qualitative invariant over time and over countries—for instance,

[19] Lucas "clarifies" in a letter to Ray Fair (2/10/1981) what he means here by "better imitations": " 'Better ' means something like 'fitting more facts' or 'fittings facts better' or 'passing more tests' or something like this." (Lucas, Archives, Box 4, Folder: 1981 3/3). This definition is still very imprecise ("something like this") and mixing two different dimensions of imitation: mimic "more facts" is quite another matter than mimic facts "better", as well as passing "more tests" is not defining the form and the power of such tests.

[20] Lucas's definition is admittedly inspired by Frisch (1933) and Slutzky (1937): cycles are oscillations, triggered by exogenous shocks and propagated by endogenous economic mechanisms (Louçã, 2004). For Lucas, the exogenous shocks generating the cycles are stochastic changes in the quantity of money, and propagation mechanism is individual behavior of economic agents. According to Lucas, trend results from demographic and technological factors, related to long run growth: this definition (which was missing a statistical corollary) was later criticized by Hodrick and Prescott (1978); Kydland and Prescott (1982).

[21] Lucas (1977, 9) enumerate five characteristics facts about the cycles to be imitated by models, referring to the findings of Burns and Mitchell (1946); Mitchell (1951) and Friedman and Schwartz (1963). For an historical perspective about these "stylized facts" see Duarte (2015).

[22] Except for the Great U.S. depression, which should be analyzed separately (De Vroey, 2015, 198).

we can observe that, say, prices have a stronger procyclical behavior in the U.S. than in France, but we cannot observe countercyclical prices.[23] Hence, business cycles are "all alike" (Lucas, 1980a, 697) and a "universal" model of the cycle is needed (*ibid.*).[24]

To sum-up, the external validity condition consist in a match between the covariances of aggregate time series simulated by the model (the model implications) and the observed covariances. According to Lucas, the benchmark for a successful imitation is the simulation of Klein and Goldberger (1955)'s model carried out by Adelman and Adelman (1959):[25]

> [Adelman and Adelman 1959] signaled a new standard for what it means to understand business cycles. One exhibits understanding of business cycles by constructing a model in the most literal sense: a fully articulated artificial economy which behaves through time so as to imitate closely the time series behavior of actual economics.
>
> (Lucas 1977, 11)

As suggested by Boumans (1997, 2004), such an idea of models as imitations implies that Lucas's external validity condition could be seen as the ability of a model to pass a "Turing test" (Turing, 1950). Note that this condition is fully consistent, for Lucas with a-realistic models' assumptions:

> I think it is exactly this superficiality that gives economics much of the power that it has: its ability to predict human behavior without knowing very much about the makeup and the lives of the people whose behavior we are trying to understand.[26]
>
> (Lucas, 1986, S425)

[23] "These movements do not exhibit uniformity of either period or amplitude, which is to say, they do not resemble the deterministic wave motions which sometimes arise in the natural sciences." (Lucas, 1977, 9).

[24] Therefore, conversely to what Lucas claims (*cf. infra*, n.24), his approach is deeply divergent from Burns and Mitchell's perspective, which aimed at a contextual explanation of each singular cycle.

[25] Lucas considers Adelmans' simulation as a technical development of Slutzky (1937)'s "new conception of modeling", namely that imitation of business cycle is the external validity condition for macroeconomic models (Lucas, Archives, Box 13, Folder: Barro, Robert, 1974, 2000, undated).

[26] As emphasized by Boumans (1997, 2004), Lucas was inspired by Herbert Simon's *The Sciences of the Artificial* (Simon, 1969). However, Simon is very skeptical about the way Lucas applied these ideas: "You seem to think that empirical issues can be settled by sophisticated statistical methodologies [...] and *without close detailed inquiry into actual human behavior at the grass roots.* The failure of the profession in reaching any consensus about [statistical methodologies] is a fairly loud and negative verdict on that strategy. The only alternative I know is to look directly at behavior in the small." (Lucas, Archives, Simon to Lucas, 10/01/1984, Box 5, Folder: 1984 2/2, my emphasis)

1.3 Ranking internal over external validity: the Lucasian apriorism

How to discriminate between two models, providing equivalent imitations of the business cycle?[27] Lucas argues that, in this case, one should always select the model fulfilling the internal validity condition. In one word, Lucas ranks internal over external validity. This is explicit in "Econometric Policy Evaluation: A Critique" (Lucas, 1976), where Lucas emphasizes that external validity alone is not a "sufficient condition" for modeling:

> Although empirical corroborations alone are not a sufficient condition for a valid model: the unquestioned success of the forecasters should not be construed as evidence for the reliability of the structure proposed in that theory.
>
> (Lucas, 1976, 24)

Hence, Lucas ranks the consistency between model's assumptions and neo-Walrasian theory over the consistency between model's implications and real world. One important justification is that the latter can be artificially improved by introducing free parameters:

> It is a commonplace that competing theoretical views ought to be judged on their ability to fit the facts. There is a sense in which this is true, but also an important sense in which it is false. Any econometrician who enters a horserace with someone using twice as many free parameters as he is deserves whatever he gets, which will in general be the last place[28].
>
> (Lucas, Archives, Box 13, Folder: Directions for macroeconomics, 1979)

Lucas's idea about ranking internal over external validity results from the distinction between a-realistic assumptions and realistic implications. Indeed, as models' assumptions cannot be assessed empirically, neither in a direct nor in an indirect way (i.e., through an assessment of their implication), it follows that inconsistency between implications and observation cannot cast doubt on the pertinence of assumptions. According to Lucas, if a model fulfilling the internal validity condition fails with respect to external

[27] Note that the external validity condition still allows to discriminate between two models equally fulfilling the internal validity condition: "not all well-articulated models will be equally useful. [...] we need to test [such models] as useful imitations of reality" (Lucas, 1980a, 697).

[28] Here, Lucas is implicitly targeting Keynesian macroeconometrics.

validity condition, hence the problem is to be found in inadequacy of empirical testing procedures, or in data sets available. As a consequence, those procedure should be rejected, but the model should not.[29]

This argument is consistent with Lucas's idea of history of economics as a linear process driven by technical progress. Indeed, new and better empirical testing procedure will help to fulfil the external validity condition:

> To date, however, no equilibrium model has been developed which meets these standards and which, at the same time, could pass the test posed by Adelman and Adelman (1959). My own guess would be that success in this sense is five, but not twenty-five years off.
>
> (Lucas, 1977, 25)

Or, again:

> Our preference would be to regard the best currently existing equilibrium models as prototypes of better, future models which will, we hope, prove of practical use in the formulation of policy.
>
> (Lucas and Sargent, 1979 1981, 62)

This position will have an important impact in the development of new classical macroeconometrics (on this matter, see Sergi, 2015b).

Lucas's methodology described above escapes traditional epistemological classification, instrumentalism and apriorism—moreover, that is an additional reason for having adopted, in this paper, a specific epistemological framework. Lucas's distinction between a-realism of assumptions and realism of implications might suggest a similarity between Lucas's methodology and instrumentalism, which, in economics, has been often associated with the "popular legacy" (Mäki, 2009b) of Milton Friedman (1953)'s "Methodology of Positive Economics". Indeed, Lucas's supporters (as for instance Sheffrin, 1983, 9-10), his opponents (see for instance Tobin in Klamer, 1984, 146) as well as few historians (Kim, 1988, 9) claim that Lucas's methodology

[29] In his "Professional Memoir" (Lucas, 2001), Lucas illustrates his commitment to this methodological principle using an anecdote about the time he was simply a high school student. He narrates that, in a report about a biology assignment, he pointed out how the inconsistency between the theory and the facts would be explained more likely by measurement errors than by rejecting the theory. This leads him to advocate that "some evidences" should be "let asides": "I don't think there is anyone who knows me or my work as a mature scientist who would not recognize me in this story. The construction of theoretical models is our way to bring order to the way we think about the world, but the process necessarily involves *ignoring some evidence* or alternative theories—setting them aside." (Lucas, 2001, 5, my emphasis)

is a particular form of Friedmanian "as if" methodology. I think that this interpretation is fundamentally wrong, at least for two reasons. The first is simply about the controversial nature of Friedman (1953): as already emphasized by Mäki (2009a) (and, earlier, by Mongin, 1987, 1988) Friedman's position should be read rather as a realistic stance than an instrumentalist one. Indeed, according to Friedman, in order to correctly perform their role of instrument of prediction assumptions should characterize to some extent a purposeful feature of reality (by approximation or idealization at least). From this perspective, Lucas's position about a-realism of assumption is far from Friedman's idea.[30] The second reason for rejecting the view that Lucas's methodology is instrumentalist is about the theoretical constraint upon assumptions. Indeed, has I emphasized it in the first section, Lucas argues that model's assumptions should be formulated in the first place by fulfilling an equivalence with concepts and formalisms of neo-Walrasian theory. Hence, assumptions are not picked up only because they provide some pertinent instrument for predictions. In addition, as we discussed it in this subsection, assumptions cannot be evaluated, even indirectly, following their ability to provide consistent prediction—conversely to instrumentalist tradition.[31] Hence, if Lucas's methodology was in any way instrumentalist, model's assumptions should be rejected (falsified) if their results do not match real world observations (in instrumentalist terms, if assumptions are not good instrument for prediction). Lucas's view is the opposite: the internal validity condition is the only condition that model's assumptions should fulfil, and, hence, assumption cannot be rejected, neither directly or indirectly (by rejecting their implication). Therefore, Lucas's idea of ranking internal over external consistency is closer to a kind of aprioristic methodology, as already emphasized by Buiter (1980, 38) and Hoover (1988, 238). However, in aprioristic methodology, theoretical proposition are proposition about the real world, and are supposed as true a priori. Lucasian apriorism is peculiar to the extent that theoretical propositions (here, model's assumptions) are

[30] Lucas seems aware of this distinction. Indeed, he never quoted Friedman (1953) in his writings (both published and unpublished). The only mention to Friedman (1953) can be found in a late interview (Snowdon and Vane, 1998, 132), where Lucas seems to support the Friedmanian vision ("I am certainly a Friedmanite."), but introducing many caveat ("there are some crucial things that a theory should account for").

[31] Note a third distinction between Lucas's methodology and instrumentalism, namely the difference between providing predictions and imitations. Instrumentalism is about prediction of not-yet-occurred events; Lucas's methodology, at the opposite, is about reproducing past events. We could eventually consider that Lucas is however thinking about reproducing past events as a kind of prediction (retrodiction), to the extent the target phenomenon (business cycle) is invariant over time—hence, reproducing the past cycle is equivalent to predicting the new one.

not propositions about the world, and cannot be considered as true a priori (but rather as logically neutral a priori). The common ground is that, still, theoretical propositions are not falsified by observation.

2 Models as laboratories and the ambivalence of Lucas's methodology

According to Lucas, the central purpose of macroeconomic modeling is to provide expertise of economic policy. However, this purpose is contradictory with the external validity condition as described above. Indeed, in order to provide expertise, a model should produce an explanation of the causes (or mechanisms) at work in the real world.[32] As imitation is not enough for providing policy analysis, therefore the previous distinction between a-realistic assumptions and realistic implications is not pertinent anymore. If one wants models to serve as "laboratories" for expertise, providing casual explanation, model's assumptions should be propositions about the real world mechanisms. Lucas develops this point, especially in his unpublished writings: assumptions should be "analogies" with the real world. From this new definition of the relation between assumptions and the real world arises the ambivalence of Lucas methodology, as he is supporting two rival external validity conditions: a-realism of model's assumptions and analogical status of the same.

2.1 Models, experiments and expertise

The fundamental role of expertise

Lucas sees models as tools for formulating policy recommendations. In this sense, models are "laboratories" for "testing out" different outcomes of policy rules: "one of the functions of theoretical economics is to provide fully articulated, artificial economic systems that can serve as laboratories in which policies [...] can be tested out" (Lucas, 1980a, 696). In his conclusion to "Methods and Problems in Business Cycle Theory", Lucas argues very clearly that this objective is the fundamental purpose of macroeconomics:

[32] If the word "cause" in itself is disturbing for Lucas, he nevertheless admits that it represents *the* question to be addressed by macroeconomic models: "What are the causes of the business cycle? This is not a very well-posed question (because "cause" is such a bad word) but it is question [sic] that anyone trying to model economic time series has to ask, in one form or another" (Lucas, Archives, Box 5, Folder: Barro, Robert, 1974, 2000, undated).

> Our task [as macroeconomists] as I see it [...] is to write a FORTRAN program that will accept specific economic policy rules as "input" and will generate as "output" statistics describing the operating characteristics of time series we care about.[33]
>
> (Lucas, 1980a, 714)

For Lucas, economic expertise provided by models is a necessary condition for an enlightened, well-informed political debate ("useful policy discussions"):

> useful policy discussions are ultimately based on *models* [...] in the sense that participants in the discussion must have, explicitly or implicitly, some way of making a quantitative connection between policies and their consequences.
>
> (Lucas 1987, 6, Lucas's emphasis).

Knowledge about the consequences of alternative policies allows to discriminate among them. For this reason, models need to provide the kind of "input-output" expertise described above. This way of "orienting public debate" is a peculiar characteristic of a "democratic society":

> Why do we need a theory? The general answers, I think, is that in a democratic society it is not enough to believe oneself to be right; one must be able to explain *why* one is right[34]
>
> (Lucas 1977, 26, Lucas's emphasis).

What kind of expertise should be provided? For Lucas, expertise should absolutely not be about, as in Keynesian approach, "day-by-day management" (Lucas, 1980b, 202) or "fine tuning" of the business cycle (Lucas, Archives, Box 3, Folder: 1978 1/4, Letter to Senator Philip Crane). In Lucas's view, modeling should be used to provide "permanent rules" for economic policy (Lucas, 1977, 8), which should lead to fixing an "economic constitution" (Lucas, 1987, 104). Such a constitution should settle, for instance, the maximum amount of public spending and the growth rate of the quantity of money—as earlier suggested by Friedman (Lucas, 1980b).[35]

[33] Note that, in this quote, the model-laboratory is a "black-box": alternative policies are "inputs" of the expertise process, model's implications are "outputs", inferences bringing from inputs to outputs seems not to be relevant. This conception of expertise is close to the Turing test definition of external validity condition, to the extent expertise relies exclusively on the imitation ability of the model (1.2).

[34] What does Lucas mean here by "explain" ("*why* one is right")? In the context of this quote, the sense is ambiguous: it might be understood as "providing some justification" of opinions or as "providing casual explanation" of the mechanisms at work.

[35] See, on this matter, correspondence between Lucas and Friedman and between Lucas and James Buchanan (Lucas, Archives, Box 4, Folder: 1980 1/3). Indeed, the very

Models as laboratories

The ultimate goal of modeling is hence to serve as a "laboratory" for "experimenting" alternative economic policies. Referring to models as laboratories is not at all a rhetorical trick in Lucas's writings, but a substantial idea. He clarified his vision in the introduction of *Models of Business Cycles*:

> In a general way, the problem of macroeconomics—really, of *all* applied economics—is to go from non-experimental observations on the past behavior of economy to inferences about the future behavior of the economy under alternative assumptions about the way policy is conducted.
>
> (Lucas, 1987, 7, Lucas's emphasis)

"Adaptive Behavior and Economic Theory " (Lucas, 1986) reinforces this remarks by discussing the similarities (and some differences) between modeling and experimental activities in economics—with a special emphasis on Vernon Smith's works.[36]. However, for Lucas, models are the only possible framework for conducting experiments about macroeconomic policy. On the one hand, material (or "actual") experiments in the framework of closed laboratories (as in experimental economics) are impossible in macroeconomics, for ontological reasons (Lucas, 1986, S424). On the other hand, natural experiments are unacceptable because of their social cost:

> It must be taken for granted, it seems clear, that simply attempting various policies that may be proposed on actual economies and watching the outcome must not be taken as a serious solution method: social experiments on the grand scale may be instructive and admirable, but they are best admired at a distance.[37]

same expression of "economic constitution" has been explicitly inspired by Buchanan and Wagner (1977).

[36] It is interesting to note that the contemporary approach of "experimental macroeconomics" (Ochs, 1995; Duffy, 1998; Ricciuti, 2004; Duffy, 2008) see in this Lucas's article the very seminal contribution of their hybrid research program, mixing macroeconomics and experimental and behavioral economics. See for instance Duffy (2016, 1-2): "I will place the origins [of macroeconomic experiments] with Lucas's 1986 invitation to macroeconomists to conduct laboratory experiments to resolve macro-coordination problems that were unresolved by theory".

[37] He also insists, in further writings: "I want to understand the connection between changes in the money supply and economic depressions. One way to demonstrate that I understand this connection—I think the only really convincing way—would be for me to engineer a depression in the United States by manipulating the U.S. money supply. I think I know how to do this, though I'm not absolutely sure, but 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, 1)

(Lucas, 1980a, 710)

Natural experiences has to be forbidden because of their social consequences.[38] Hence, models are the only framework left that can be used as laboratory for experiences: "artificial economic systems can serve as laboratories in which policies *that would be prohibitively expensive to experiment with in actual economies* can be tested out at much lower cost" Lucas, 1980a, 696, my emphasis).

Model's implications are then to be seen under a different light than simple imitations of real world: they are "possible words", scenarios, counterfactual. Lucas speaks more precisely about "conditional forecast"—an answer to the question: "what would happen if this policy will be implemented?" Lucas emphasizes that the ability of a model in answering this kind of question is substantially different from his ability to imitate the business cycle ("unconditional forecast"). Hence, a model providing a good imitation is not forcefully able to serve as laboratory for expertise:

> Yet the ability of a model to imitate actual behavior in the way tested by the Adelmans has almost nothing to do with its ability to make accurate conditional forecasts, to answer questions of the form: how would behavior have differed had certain policies been different in specified ways.
>
> (Lucas, 1977, 12)

From this simple remark results an important shift in Lucas's view about the external validity condition. Indeed, he argues that, in order to work as a laboratory (to provide conditional forecast), a model should rely on assumptions describing the actual casual mechanisms of the real world.

2.2 Analogies and casual explanation

Does a model possibly provide an economic expertise by working as a "black box"? I.e., does a model provide expertise by producing an input-output-like analysis (the policy/its consequences), without anyexplanation of the casual mechanisms at work? This is the question that James Tobin addressed (indirectly) to Lucas:

[38] This limitation does not implies that, when realized, some natural experiences can be exploited for inferring policy conclusions. In Lucas's understanding, this is for instance the case of U.S. stagflation in the 1970s: this was a natural experience that provided clear evidence about the ineffectiveness of Keynesian policies (Lucas and Sargent, 1979 1981, 49).

> I don't think that models so far from realistic description should be taken seriously as guides to policy. Evidently, Lucas thinks otherwise.
>
> (Tobin in (Klamer, 1984, 111))

This criticism targets the Lucasian distinction between a-realistic assumptions and realistic implications.

As emphasized by De Vroey (De Vroey, 2011, 9-11 et De Vroey, 2015, Chap. 11), the unpublished writings of Lucas show that he was very well aware of the well-founded of the kind of criticisms made by Tobin. To face them, Lucas suggests that model's assumptions should be related to the real world by some kind of "analogy"—to be understood in a very broad sense, as a "symmetric relation":

> we observe things and events, and we perceive analogies among them. [...] I like to think of theories [...] as simulatable systems, analogues to the actual system we are trying to study
>
> (Lucas, Archives, Box 27, Folder: Adaptive behavior)

Lucas also forcefully endorses the vision about economic models "as metaphors", suggested in McCloskey (1990):

> McCloskey's main thesis, as I understand it, is that economic models or theories are metaphors, and that we use them by appealing to analogies between the fictional world of the theory and the reality we want to understand. I think this view is right.
>
> (Lucas, Archives, Box 7, 1989 1/3)

Furthermore, Lucas suggests that the analogy between assumptions and real world aims at describing casuality. In this sense, a model is set of propositions isolating from reality crucial casual mechanisms. In the published works, this idea is not explicitly advocated, but one can however identify some very ambiguous formulations, suggesting this interpretation. In the following quote for instance, Lucas argues that a model should account ("imagine", "understanding") for "the way agents' decisions" are taken:

> The problem of quantitatively assessing hypothetical countercyclical policies (say, a monetary growth rule or a fiscal stabilizer) involves imagining how agents will behave in a situation which has never been observed. To do this successfully, one must have some understanding of the way agents' decisions have been made in the past and some method of determining how these decisions would be altered by the hypothetical change in policy.

(Lucas, 1975, 1114)

Further in the same article, Lucas talks about "explanation", (without mentioning what he means by this term):

> The introduction of separate, informationally distinct markets is not a step toward "realism" or (obviously) "elegance" but, rather, an analytical departure which appears essential (in some form) to an *explanation of the way in which business cycles can arise and persist in a competitive economy.*
>
> (Lucas, 1975, 1132, my emphasis)

Unpublished writings are more explicit on this question. In his notes, Lucas emphasizes the importance of investigating causality in macroeconomics(Lucas, Archives, Box 5, Folder: Barro, Robert, 1974, 2000, undated; see footnote 35). However, the casual elements working in the model are still not the same that those at work in the real world—they are only analogues causalities:

> An apparent difficulty in this idea of causation is that casual relationships are properties of a particular model, not of "reality" itself.
>
> (Lucas, Archives, Box 5, Folder: Barro, Robert, 1974, 2000, undated)

Only a model isolating, by analogy, real world casual mechanisms can be used as a laboratory for expertise:

> 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, Archives, Box 13, Folder: Directions of macroeconomics 1979)

Here Lucas is refering to casual mechanisms of the models as "forces similar to those acting on actual society". This implies that the model is not a "black box" for expertise (neither a fully "fictional society"); conversely, the model relies on "useable connections" with the real world.

The above quotes suggest that Lucas is advocating for a different external validity condition with respect to the one discussed in the first section of this paper. This is what I call the "ambivalence" of Lucas's methodology. This ambivalence can actually be highlighted in most of his published work. For

instance, in Lucas (1980a), we can find seven occurrence of the word "analogy" for qualifying model's assumptions; however, at the same time, Lucas argues in the same text that "a 'theory' is not a collection of assertions about the behavior of the actual economy" (Lucas, 1980a, 697). Elsewhere, he insists on this point, qualifying his assumptions as "particular", "abstract" or "simple" (Lucas, 1972, 103,104,105,107,117,122) but to be intended "metaphorically" (Lucas, 1975, 1139).

Introducing the notions of analogy and causality bring to a redefinition of external validity condition; however the internal validity condition remains the same: a model should always be built on the concept and formalism of the neo-Walrasian theory. These assumptions are simply supported by a new justification, which is, in the terms of their consistence with the real world.[39] Hence, concepts as optimization, market clearing and rational expectations are no more just "axioms" and propositions pertaining exclusively to some "artificial world". They are now substantial analogies with the real world, and, moreover, analogies with casual mechanisms at work in the real world. In this perspective, rational expectations are a "reasonable" analogy with the actual formation of expectation for economic agents facing a stable environment (the business cycle):

> Insofar as business cycles can be viewed as repeated instances of essentially similar events, it will be reasonable to treat agents as reacting to cyclical changes as "risk" or to assume their expectations are rational that they have fairly stable arrangements for collecting and processing information, and that they utilize this information in forecasting the future in a stable way, free of systematic and easily correctable biases.
>
> (Lucas, 1977, 15)[40].

In a similar way, individual optimization (here, more specifically, intertemporal tradeoff) is described as a form of "understanding" and of "explanation" of the way individual choices

> The time pattern of hours than an individual supplies to the market is something that, in a very clear sense, he chooses. Understanding

[39] Goutsmedt (2016) emphasized this same argument as a very explicit position in Sargent's work.

[40] Rational expectations as realistic analogy with actual expectation formation seems to have become later a more clear-cut and widespread position than in the 1970s: "rational expectations are forward-looking and imply a much more sophisticated, and more realistic, way of forming expectations; agents learn from their mistakes, use their intellectual capacity to understand the way the economy works and exploit available information in an efficient way" (Svensson, 1996, 2)

> employment fluctuations must involve, at some point, understanding how this choice is made or what combination of preferences characteristic and changing opportunities gives rise to the patterns we observe [...] Ignoring this simple point seems to me simply bad social science: an attempt to explain important aspects of human behavior without reference either to what people like or what they are capable of doing.
>
> (Lucas, 1981, 4)

Even market clearing is discussed as a realistic feature. This is particularly explicit in Lucas's discussion of sunspots equilibria, were he argues that this concept is to be rejected as unrealistic:

> I just don't see how a society would ever hit on the sunspot-varying equilibria you exhibit [...] I can't think of any historically observed situation [...] I don't see [sunspot equilibria] as models of human behavior.
>
> (Lucas, Archives, Box 4, Folder: 1981 1/2, Correspondence with Karl Shell, 30/9/1981)[41]

The realistic status of assumptions plays an important role in "Econometric Policy Evaluation: A Critique" (Lucas, 1976). In this paper, Lucas formalizes his well-known critique of Keynesian macroeconometric models: they are "useless" for expertise, because they are unable of making correct conditional forecast. This weakness his, in Lucas's understanding, a consequence of the loose way of explaining individual behavior face to a shift in policy rules. The econometric analysis illustrates this phenomenon by emphasizing the "deviation between the prior 'true' structure [of an economy] and the 'true' structure prevailing afterwards" (Lucas, 1976, 258). Following the Lucas critique then, policy evaluation should be conducted with an econometric model, which includes a theoretical structure that contains pertinent analogies with the real world. Moreover, the set of relations constituting the model should also be statistically tested, with the methods of structural econometrics. This also the only explicit definition of "causality" that one can find in Lucas writings (in his unpublished papers):

> A casual relation in the sense I am using the term is a property of a structural economic model
>
> (Lucas, Archives, Box 4, Folder: 1981 1/2, Correspondence with Karl Shell, 30/9/1981)

[41] See also: "No attemps is made to argue that any of the equilbria found resemble observed economic behavior in any respect" (Lucas, Archives, Box 6, Folder: 1986 2/2).

Conclusion

This article addressed Lucas's methodology from the perspective of the relation between theory, model and the real world. It emphasized Lucas's ambivalence toward the external validity condition. On the one hand, Lucas advocates for a distinction about a-realist assumptions and realistic results. On the other hand he argues for realistic assumptions, providing analogies with casual mechanisms at work in the real world. The latter position is motivated by the idea of models working as laboratories for policy experience and, therefore, isolating causality.

My contribution provides a new perspective on Lucas's methodological contribution to macroeconomics, which is, I think, crucial for understanding further developments of modeling practices by new classical macroeconomics, real business cycles theory and, more recently, by the new neoclassical synthesis.

References

Adelman, I. and Adelman, F. L. (1959). The Dynamic Properties of the Klein-Goldberger Model. *Econometrica*, 27(4):596–625.

Arrow, K. J. (1978). The future and the present in economic life. *Economic Inquiry*, 16(2):157–169.

Boumans, M. (1997). Lucas and Artificial Worlds. *History of Polical Economics*, 29(4):63–88.

Boumans, M. (2004). *How Economists Model the World into Numbers*. Routledge, London.

Boumans, M. and Sent, E.-M. (2013). A Nobel Prize for Empirical Macroeconomics: Assessing the Contributions of Thomas Sargent and Christopher Sims. *Review of Political Economy*, 25(1):39–56.

Buchanan, J. M. and Wagner, R. E. (1977). *Democracy in Deficit. The Political Legacy of Lord Keynes*. Academic Press, New York (NY).

Buiter, W. H. (1980). The Macroeconomics of Dr. Pangloss. A Critical Survey of the New Classical Macroeconomics. *The Economic Journal*, 90(357):34–50.

Burns, A. F. and Mitchell, W. C. (1946). *Measuring Business Cycles*. National Bureau of Economic Research, New York (US).

Cherrier, B. and Saïdi, A. (2015). The Indeterminate Fate of Sunspots in Economics. Available at SSRN 2684756.

De Vroey, M. (2011). Lucas on the relationship between theory and ideology. *Economics: The Open-Access, Open-Assessment E-Journal*, 5(2011-4):1–39.

De Vroey, M. (2015). *A History of Modern Macroeconomics from Keynes to Lucas and Beyond*. Cambridge University Press, Cambridge (UK).

Debreu, G. (1959). *Theory of Value. An Axiomatic Analysis of Economic Equilibrium*. Yale University Press, New Haven (NJ).

Duarte, P. G. (2015). From real business cycle and new Keynesian to DSGE Macroeconomics: Facts and models in the emergence of a consensus. Technical Report 2015-05, Departement of Economics, University of São Paulo (FEA-USP).

Duarte, P. G. (2016). Macroeconomists as Revolutionary Schoolmates. Comments on Michel De Vroey's A History of Macroeconomics from Keynes to Lucas and Beyond. *Œconomia. History, Methodology, Philosophy*, 6(1):129–137.

Duffy, J. (1998). Monetary Theory in the Laboratory. *Federal Reserve Bank of St. Louis Economic Review*, 1998(80):9–26.

Duffy, J. (2008). Experimental Macroeconomics. In Durlauf, S. N. and Blume, L. E., editors, *The New Palgrave Dictionary of Economics*. Palgrave Macmillan, Basingstoke (UK).

Duffy, J. (2016). Coordination Problems. In Kagel, J. H. and Roth, A. E., editors, *Handbook of Experimental Economics*, volume 2, pages 0–0. Princeton University Press, Princeton. A paraître. `http://www2.econ.iastate.edu/tesfatsi/LaboratoryMacroeconomics.JDuffy2008.pdf` (consulté le 12/07/2016).

Fischer, S. (1996). Robert Lucas's Nobel Memorial Prize. *The Scandinavian Journal of Economics*, 98(1):11–31.

Friedman, M. (1953). The Methodology of Positive Economics. In Friedman, M., editor, *Essays in Positive Economics*, pages 3–43. University of Chicago Press, Chicago (IL).

Friedman, M. and Schwartz, A. J. (1963). *A Monetary History of the United States, 1867-1960*. Princeton University Press, Princeton.

Frisch, R. (1933). Propagation Problems and Impulse Problems in Dynamic Economics. In Frisch, R. and Koch, K., editors, *Essays in Honour of Gustav Cassel*, pages 171–147. George Allen and Unwin Ltd., Londres.

Goutsmedt, A. (2016). Thomas Sargent face à Robert Lucas : une autre ambition pour la Nouvelle Economie Classique. Communication à 16$^{\text{ème}}$ Conférence de l'Association Charles Gide pour l'histoire de la pensée économique, Strasbourg, 14-16 avril 2016.

Hodrick, R. J. and Prescott, E. C. (1978). Postwar U.S. Business Cycles: An Descriptive Empirical Investigation. Technical Report 4-78-79, Carnegie-Mellon Working Paper. Publié en 1997 dans *Journal of Money, Credit and Banking, 29*(1) : 1-16.

Hoover, K. D. (1988). *The New Classical Macroeconomics: A Sceptical Inquiry*. Basil Blackwell, Oxford.

Hoover, K. D. (1995). Is Macroeconomics for Real? *The Monist*, 78(3):235–257.

Hoover, K. D. (2010). Idealizing reduction: The microfoundations of macroeconomics. *Erkenntnis*, 73(3):329–347.

Hoover, K. D. (2012). Microfoundational programs. In Duarte, P. G. and Lima, G. T., editors, *Microfoundations Reconsidered*, pages 19–61. Edward Elgar, Cheltenham (UK).

Kim, K. (1988). *Equilibrium Business Cycle Theory in Historical Perspective*. Cambridge University Press, Cambridge (UK).

Klamer, A. (1984). *The New Classical Macroeconomics. Conversations with the New Classical Economists and Opponents*. Wheatsheaf Books, Brighton (UK).

Klein, L. R. and Goldberger, A. S. (1955). *An Econometric Model of the United States, 1929-1952*. North Holland, Amsterdam.

Kydland, F. E. and Prescott, E. C. (1982). Time to Build and Aggregate Fluctuations. *Econometrica*, 50(6):1345–1370.

Louçã, F. (2004). Swinging all the way: the education of Doctor Lucas and foes. *History of Political Economy*, 36(4):689–735.

Lucas, R. E. (1972). Expectations and the Neutrality of Money. *Journal of Economic Theory*, 4(2):103–124.

Lucas, R. E. (1975). An Equilibrium Model of the Business Cycle. *The Journal of Political Economy*, 83(6):1113–1144.

Lucas, R. E. (1976). Econometric Policy Evaluation: A Critique. In *Carnegie-Rochester Conference Series on Public Policy*, volume 1, pages 19–46. Elsevier.

Lucas, R. E. (1977). Understanding Business Cycles. *Carnegie-Rochester Conference Series on Public Policy*, 5:7–29.

Lucas, R. E. (1980a). Methods and Problems in Business Cycle Theory. *Journal of Money, Credit and banking*, 12(4):696–715.

Lucas, R. E. (1980b). Rules, Discretion, and the Role of the Economic Advisor. In Fischer, S., editor, *Rational Expectations and Economic Policy*, pages 199–210. University of Chicago Press, Chicago (IL).

Lucas, R. E. (1981). *Studies in Business-Cycle Theory*. MIT Press, Cambridge (MA).

Lucas, R. E. (1986). Adaptive behavior and economic theory. *Journal of Business*, 59(4, Part 2):S401–S426.

Lucas, R. E. (1987). *Models of Business Cycles*. Basil Blackwell, Oxford.

Lucas, R. E. (1988). What Economists Do. Mimeo, quoted in De Vroey (2011).

Lucas, R. E. (1996). Nobel Lecture: Monetary Neutrality. *Journal of Political Economy*, 104(4):661–682.

Lucas, R. E. (2001). Professional Memories. Nobel Economists Lecture Series at Trinity University, San Antonio, Texas.

Lucas, R. E. (2002). *Lectures on Economic Growth*. Harvard University Press, Harvard.

Lucas, R. E. (2004). Keynote Address to the 2003 HOPE conference: my Keynesian education. *History of Political Economy*, 36(5):12–24.

Lucas, R. E. (2007). Remarks on the Influence of Edward Prescott. *Economic Theory*, 32(1):7–11.

Lucas, R. E. (Archives). Robert E. Lucas Papers, 1960-2011 and undated. David M. Rubenstein Rare Book & Manuscript Library, Duke University.

Lucas, R. E. and Rapping, L. A. (1969). Real Wages, Employment, and Inflation. *Journal of Political Economy*, 77(5):721–754.

Lucas, R. E. and Sargent, T. ([1979] 1981). After Keynesian Macroeconomics. In Lucas, R. E. and Sargent, T. J., editors, *Rational Expectations and Econometric Practice*, pages 295–320. George Allen and Unwin Ltd., London. First published in 1979, *Federal Reserve of Minneapolis Quarterly Review*, 3:1-6.

Mäki, U. (2005). Models are experiments, experiments are models. *Journal of Economic Methodology*, 12(2):303–315.

Mäki, U., editor (2009a). *The Methodology of Positive Economics. Reflections on the Milton Friedman Legacy*. Cambridge University Press, Cambridge (UK).

Mäki, U. (2009b). Reading *the* methodological essay in XX-century economics. A map of multiple perspectives. In Mäki, U., editor, *The Methodology of Positive Economics. Reflections on the Milton Friedman Legacy*, pages 47–67. Cambridge University Press, Cambridge (UK).

McCloskey, D. N. (1990). *If You're So Smart. The Narrative of Economic Expertise*. University of Chicago Press, Chicago (IL).

Mitchell, W. C. (1951). *What Happens During Business Cycles*. National Bureau of Economic Research, New York (US).

Mongin, P. (1987). L'instrumentalisme dans l'essai de M. Friedman. *Economies et sociétés*, 21(10):73–106.

Mongin, P. (1988). Le réalisme des hypothèses et la partial interpretation view. *Philosophy of the Social Sciences*, 18(3):281–325.

Morgan, M. S. and Morrison, M., editors (1999). *Models as Mediators: Perspectives on Natural and Social Science*. Cambridge University Press, Cambridge (UK).

Ochs, J. (1995). Coordination Problems. In Kagel, J. H. and Roth, A. E., editors, *Handbook of Experimental Economics*, pages 195–252. Princeton University Press, Princeton.

Ricciuti, R. (2004). Bringing Macroeconomics into the Lab. Labsi – Experimental Economics Laboratory University of Siena Working Paper Series 004, University of Siena.

Sent, E.-M. (2006). *The Evolving Rationality of Rational Expectations: An Assessment of Thomas Sargent's Achievements*. Cambridge University Press, Cambridge (UK).

Sergi, F. (2014). Quelle méthodologie pour une étude des modèles DSGE ? suggestions à partir d'un état des lieux des recherches sur la modélisation. Documents de travail du CES 2014.67, Centre d'Economie de la Sorbonne (CES).

Sergi, F. (2015a). L'approche (faussement ?) naïve à l'histoire des modèles DSGE. Documents de travail du CES 2015.66, Centre d'Economie de la Sorbonne (CES).

Sergi, F. (2015b). Robert Lucas and the Twist of Modeling Methodology. On some Econometric Methods and Problems in New Classical Macroeconomics. Documents de travail du CES 2015.88, Centre d'Economie de la Sorbonne (CES).

Sergi, F. (2017). *De la révolution lucasienne aux modèles DSGE. Réflexions sur les développements recents de la modélisation macroéconomique*. PhD thesis, Université Paris 1 Panthéon-Sorbonne, Paris.

Sheffrin, S. M. (1983). *Rational Expectations*. Cambridge University Press, Cambridge.

Simon, H. (1969). *The Science of Artificial*. MIT Press, Cambridge, MA.

Slutzky, E. ([1937). The Summation of Random Causes as the Source of Cyclic Processes. *Econometrica*, 5(2):105–146.

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

Snowdon, B. and Vane, H. R. (2005). *Modern Macroeconomics: its Origins, Development and Current State*. Edward Elgar, Cheltenham (UK).

Snowdon, B., Vane, H. R., and Wynarczyk, P. (1994). *A Modern Guide to Macroeconomics. An Introduction to Competiting Schools of Thought*. Edward Elgar, Cheltenham (UK).

Svensson, L. E. O. (1996). The Scientific Contributions of Robert E. Lucas, Jr. *The Scandinavian Journal of Economics*, 98(1):1–10.

Turing, A. M. (1950). Computing machinery and intelligence. *Mind*, 59(236):433–460.