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# Robert Lucas and the Twist of Modeling Methodology. On some Econometric Methods and Problems in New Classical Macroeconomics.

Francesco Sergi*

**Abstract**

The purpose of this contribution to the epistemology and history of recent macroeconomics is to construct a clear understanding of econometric methods and problems in New Classical macroeconomics. Most historical work have focused so far on theoretical or policy implication aspects of this research program set in motion by Robert Lucas in the early seventies. On the contrary, the empirical and econometric works of New Classical macroeconomics have received little attention.

I focus especially on the contributions gathered in *Rational Expectations and Econometric Practice*, edited in 1981 by Lucas and Thomas Sargent. The main claim of this article is that the publication of this book must be regarded as a turn in macroeconomics, that would bring macroeconometric modeling methodology closer to Lucas's conception of models.

The analysis of the New Classical macroeconometrics through the Lucas methodology allow us to propose an original historical account of the methods presented in *Rational Expectations and Econometric Practice*, but also of the problems that flawed this approach.

**Keywords:** history of macroeconomics, Lucas (Robert), Sargent (Thomas), macroeconometrics, modeling methodology

**JEL Classification:** B22, B41

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**Résumé**

L'objectif de cette contribution à l'épistémologie et à l'histoire de la pensée économique est de proposer une compréhension claire des méthodes et des problèmes économétriques de la nouvelle macroéconomie classique. La plupart des travaux historiques à ce sujet se sont focalisés sur les aspects théoriques ou sur les implications de politique économique de ce programme de recherche, lancé par Robert Lucas au début des années soixante-dix. En revanche, le travail empirique et économétrique de la nouvelle macroéconomie classique a reçu peu d'attention par les historiens.

L'article s'intéresse plus particulièrement aux contributions rassemblées dans *Rational Expectations and Econometric Practice*, ouvrage collectif de 1981 dirigé par Lucas et Thomas Sargent. La principale thèse de l'article est que la publication de ce livre entérine un tournant dans la modélisation macroéconométrique, en étroite résonance avec la conception méthodologique de Lucas sur les modèles.

L'analyse de la macroéconométrie des nouveaux classiques par le prisme de la méthodologie lucasienne nous permet de proposer une vision historique originale des méthodes présentées dans *Rational Expectations and Econometric Practice*, tout comme des problèmes qui ont entravé le développement de cette approche.

**Mots-clés :** histoire de la macroéconomie, Lucas (Robert), Sargent (Thomas), macroéconométrie, méthodologie et modélisation

**Classification JEL :** B22, B41

# 1 A turn in macroeconometric modeling

*Rational Expectations and Econometric Practice* (Lucas and Sargent, 1981c) collects 34 published and unpublished articles by 17 different authors, all dealing "with various aspects of the general problem of drawing inferences about behavior from observed economic time series" (Lucas and Sargent 1981b, p. xi). The volume aims at summing up the empirical techniques of New Classical macroeconomics[1], their applications and the subsequent findings. As the editors put it in their introduction, the general

1. I preferred here to use the contemporary label "New Classical macroeconomics" rather than the original denominations "monetarism mark II" or "rational expectations macroeconomics". The substantial content is equivalent, and it designates the macroeconomic research program initiated essentially by Lucas (1972b). For a comprehensive discussion about labels for this approach, see Hoover (1988, Chap. I).

perspective of the volume is to "offer something to the economist who wishes to apply the idea of rational expectations to problems of estimation, testing, policy evaluation or control" (Lucas and Sargent 1981b, p. xxxviii).

In a similar vein of a "synthesis" of New Classical macroeconomics, Robert Lucas edited, in the same year, *Studies in Business Cycle Theory* (Lucas, 1981c), gathering his most influential works. Two years before, Thomas Sargent had published an advanced textbook, *Macroeconomic Theory* (Sargent, 1979), illustrating the main technical issues in New Classical macroeconomics. These three volumes should be regarded as an attempt to unify, to clarify, to popularize and to provide theoretical and methodological consistence to the New Classical research program in macroeconomics of the 1970s.[2] Actually, *Rational Expectations and Econometric Practice* is the more puzzling publication in this trilogy. Even if Sargent's contribution is quantitatively very important, *Rational Expectations and Econometric Practice* represents a *genuine* synthesis, i.e. a collective effort rather than an individual personal sum-up provided by the leading figures of the New Classical macroeconomics.[3] Indeed, at a first glance, the author-year-publishing structure of the articles collected in the book (see Appendix 1) clearly embodies this collective dimension. A further analysis allows to draw the frontiers of the research network involved in the publication of *Rational Expectations and Econometric Practice*. The volume gathers very different authors, even if the two editors are the two main contributors; authors have also heterogeneous academic affiliations (such as MIT, Chicago, or Columbia), even if Carnegie-Mellon and the University of Minnesota are the dominant institutions. The period covered is about 21 years, even if the contributions are mainly concentrated in the period between 1976 and 1981. Both manuscripts and articles published in a wide range of academic journals are presented (from the *Journal of Economic Dynamics and Control* to the *Brookings Papers on Economic Activity*), even if most of the articles had been published in the *Journal of Political Economy* and in *Econometrica*.

2. Such a claim seems, retrospectively, not very controversial (as well because it is implicit in other contributions, such as Snowdon and Vane, 2005). Nevertheless, an editorial history of those publications, founded on archival and interview materials, should be developed to provide sound historiographical evidences of this intention of the authors.

3. Of course, not all the papers—especially the earliest ones, as Muth (1961)—have been conceived in the first place as a contribution to the specific program of New Classical macroeconomics. Nevertheless, we can argue that the different authors adhere to some extent to this program, since they all accepted to re-print their articles in this volume.

## 1.1 Methods in New Classical Macroeconometrics

The first purpose of this article is to analyze this heterogeneous volume and to reconstruct an ordered comprehension of the econometric methods of New Classical macroeconomics. I will refer to this specific strain of literature as "New Classical macroeconometrics" (NCME hereafter). My claim is that the input of Lucas, as the "methodological spokesperson" (De Vroey, 2015) of the New Classical macroeconomics, provides the essential foundations for the NCME approach. This is the strong claim of our paper: the connection between Lucas's methodology and the NCME is needed to build an historical perspective on our subject. This link also constitute the originality of the present article (and what distinguishes it from a plain "literature review").[4]

Lucas's methodology must be understood as a specific way of conceiving the interaction between theory and the empirical world through models. In this article, I will characterize the Lucasian conception of models as the "L-twist", merging apriorism and instrumentalism: models must adopt some specific assumptions, which are non-testable (optimization in a general equilibrium framework, rational expectations); nevertheless, a model has the function of a forecasting device. A model must indeed produce testable predictions, corroborating the underlying assumptions.[5] Following those two requirements, formulated by Lucas, the NCME developed two objectives.

The first objective can be seen as a "positive" research program. Indeed, NCME attempts to work out empirical evidences against the so-called "Keynesian" macroeconometrics and in favor of the New Classical results.[6] The main issue here is the econometric test of the "Keynesian" Phillips curve (showing a trade-off between inflation and unemployment) against the "natural rate hypothesis" (postulating monetary neutrality and policy ineffectiveness).[7] Basically, the positive side of NCME consists in supporting the

4. I must also note that the explicit methodology presented in Lucas's writings lead me to a specific focus on his figure, clearly the most influential on this perspective. However, Sargent played also a crucial role for the NCME; this role will be downplayed here. About Sargent, see Sent (1998).

5. Hoover (1988), in his concluding chapter about New Classical macroeconomics and the Austrian school, already provided the intuition for this reading of Lucas's methodology.

6. For the sake of simplicity, I use, as New Classical macroeconomists did, the word "Keynesian" to designate the dominant paradigm in macroeconomics and macroeconometrics during the 1960s and 1970s—a part of what is presently labeled as the "New Classical Synthesis". Therefore, I leave aside the question of knowing to which extent this approach actually followed the ideas of Keynes and to which extent it did not. For an extensive discussion about this matter, see De Vroey (2009). For a discussion about the influence of the Cowles Commission macroeconometric agenda, and especially of Lawrence Klein in this line of research, see Pinzón-Fuchs (2014).

7. Again, I will not discuss here the consistence of the (widespread) interpretation of

claim for money neutrality both in the short and in the long run, except in presence of monetary surprises. This result is consistent with the theoretical framework formulated first in Lucas (1972b).

The second objective of the NCME program is a "prescriptive" one. The general goal is to provide operational guidelines for macroeconometric modeling. These rules are not only new, but they also constitute an alternative to the traditional macroeconometric approach *à la* Klein and Goldberger (1955). More precisely, the NCME aimed at defining an explicit set of rules for building and estimating a complete model of the business cycle, following the a priori hypotheses proposed by Lucas: optimizing behavior in a dynamic general equilibrium framework and rational expectations [8]. Such models must compute and estimate a set of dynamic optimal decision rules for the economic agents and an estimation of the so-called "deep" parameters (preferences, technology, forms of stochastic processes). The subsequent result shall be a model capable of reproducing the main statistical characteristics of the actual business cycle, and to forecast the effects of alternative economic policies in a way consistent with the Lucas Critique (Lucas, 1976).

The core of the NCME is grounded then, on the one hand, on a positive approach testing the neutrality of money, and, on the other hand, on a prescriptive approach attempting at the identification and estimation of the deep parameters governing the optimal dynamic decision rules of economic agents. From the perspective of the econometric techniques, we also distinguish two types of contributions from the NCME. On the one hand, a bunch of innovative contributions tries to bring new econometric methods to the field of applied macroeconomics, especially the cross-equation restrictions approach and the time series analysis in the wake of Granger's definition of causality (Granger, 1969). On the other hand, we find a large amount of "applied" works in the NCME, bringing those new methods to the analysis of different samples, application and examples.

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the "Keynesian" Phillips curve, which is actually a major simplification. On this topic, see Hoover (2015) and Forder (2014).

8. For recall, expectations are rational if the subjective probability distribution (the expectation of economic agents) equals to the objective probability distribution. The expectation at time $t-1$ of the value of a variable $x$ at time $t$ equals the expected value of $x$, conditionally to the past available information $\Omega_{t-1}$:

$$x_t^e(t-1) = \mathbb{E}_{t-1}(x_t|\Omega_{t-1})$$

This formulation relies on the idea that all the available information is processed, in a pertinent and homogenous way by all the economic agents. The idea of rational expectation was first introduced by Muth (1961). As the concept is central for the NCME approach, this article is reprinted as the opening chapter in the Lucas-Sargent volume.

## 1.2 Problems in New Classical Macroeconometrics

The NCME aims at providing a reliable alternative to "Keynesian" macroeconometric models. But it is also worthy to note that, at the very same period around the publishing of *Rational Expectations and Econometric Practice*, two competitive approaches—calibration and vector autoregressive (VAR) models—challenged the methods presented in Lucas and Sargent (1981c). Even if Lucas and Sargent's publication is not the full stop of the NCME program [9], considering its historical context, it should be regarded as a turning point in econometric practices applied to macroeconomics. This is an additional motivation for focusing on this particular volume.

The NCME is usually considered, from the perspective of the history of applied econometrics, as a main break with the Cowles Commission's legacy in macroeconomics (see for instance Gilbert and Qin 2005). This understanding puts (correctly) its emphasis on the substantial implications of the "Lucas Critique", casting doubt upon the usefulness of the "Keynesian" macroeconometric models *à la* Klein and Goldberger (1955). [10] The NCME, however, did not completely shut down the macroeconometric modeling approach developed (especially) following Jan Tinbergen and Lawrence Klein's approches. Instead, New Classical macroeconomists clearly recognizes the fundamental relevance of carrying on econometric estimation of structural parameters in macroeconomic models. In their understanding, the combination of theoretical models with econometrics provided a more "sound" or "scientific" discipline, which would be more reliable in advising policy-makers. [11] Indeed, when we retrospectively compare the NCME approach to two other major approach to macroeconometrics rising at this time (calibration and VAR), NCME finally look more like an attempt to (radically) amend "Keynesian" macroeconometric models, than like an attempt to completely destroy them—even if, at the time, this was of course not the dominant impression.

We definitively cannot say so for the calibration method introduced by the real business cycle (RBC) approach, initiated by "Time to Build and Fluctuate Aggregation" (Kydland and Prescott, 1982). Despite the common methodological ground and purpose with the NCME (to identify deep param-

9. Ingram (1995) provides a useful synthesis of the further developments of the NCME.

10. For a more comprehensive discussion about the (not so) "path-breaking" value of the Lucas Critique, see Goutsmedt et al. (2015).

11. As Lucas puts it in his famous Critique: "Surely, the increasing sophistication of the "naive" alternatives to the major forecasting models is the highest of tributes to the remarkable success of the latter. I hope I can succeed in dissociating the criticism which follows from any denial of the very important advances in forecasting ability recorded by the econometric models, and of the promise they offer for advancement of comparable importance in the future" (Lucas, 1976, p. 20).

eters in order to quantitatively reproduce the business cycle), RBC models give up econometric estimation for setting the parameter values with prior information *outside* the model. Even if Kydland and Prescott advocated that calibration was still a form of econometric approach, in a wide and even more "truthful-to-the-origins" sense (Kydland and Prescott, 1996), the rejection by econometricians of the RBC assessment methodology was rough. Even Lars P. Hansen, a leading figure of the NCME, pointed out to what extent the calibration method is far from providing a reliable alternative to traditional macroeconometric modeling (Hansen and Heckman, 1996). The net divide between RBC and NCME forces the historian to explore the two approches separately, despite their common origins. [12]

In a quite different perspective but with a similar result, Christoper Sims's "Macroeconomics and Reality" (Sims, 1980) puts in question the theory-driven approach to econometrics led by "Keynesians" and NCME (see especially Sims, 1982). In short, Sims proposed to simply reject any identification restriction in macroeconometric modeling, and then to focus on reduced-forms models, specified as vector autoregressive processes (VAR), instead of structural models. This very fruitful data-driven (or "with not too much a priori theory") approach provided a second competitive research program to the NCME.

This clarification about the relation between NCME, "Keynesians", RBC and VAR modeling strategies leads to the second purpose of this article. Indeed, the NCME clearly expressed is ambition to become the alternative to traditional "Keynesians" macroeconometric modeling: since, at the same time, competing approaches arisen, one shall conclude that "something was missing" in the NCME methods presented in Lucas and Sargent (1981c), avoiding it to realize its ambition. Our purpose is then to analyze the methodological and technical difficulties flawing the NCME. Furthermore, from the positive side, the empirical results about money neutrality were very controversial, not only according to the opponents to the NCME, but also for their main promoters. The prescriptive side is even more problematic. At the time of *Rational Expectations and Econometric Practice*, the NCME could not offer a valuable alternative methodology to the traditional macroeconometric models. Their models were not only disappointing from the perspective of quantitative reproduction of business cycle features: in addition to this failure, they were highly complex to solve and estimate, suffering of similar specification and identification problems as their "Keynesian" opponents. These difficulties will be analyzed from the perspective

12. The consistence between Lucas's conception of modeling and the RBC approach is discussed in Sergi (2014b).

of Lucas's methodology: we will emphasize how the problems of the NCME were grounded on the paradoxical implications (the "L-trap") arising from the L-twist conception of modeling.

## 1.3 What is left aside

The main claim of the present article is that *Rational Expectations and Econometric Practice* must be regarded as a turn in macroeconometric modeling. This turn is closely related to Lucas's conception of models. Indeed, our purpose is to construct, as an historian, a clear understanding of the methods *and* problems of the NCME. I will conclude this (long) introductory section by clarifying some general historical issues raised by the study of the NCME. Even if such general aspects will not be addressed by this article, it is worthy recalling how our study of *Rational Expectations and Econometric Practice* provides a contribution to a wider research program.

The historians of (macro)economic thought, with the exception of Hoover (1988, Chap. VIII) and Kim (1988, Chap. 5), have payed little attention to the NCME. My contribution aims indeed at completing, with this specific aspect, the narrative about the transformation of macroeconomics starting from the 1970s. Some recent contributions (Duarte and Lima 2012; Backhouse and Boianovski 2013; De Vroey 2009, 2015), have been re-exploring, in a critical perspective, what is often refereed as the "New Classical revolution". Historical research in this vein has been rediscovering the tortuous paths of the transformation of the field, shining a light on blind alleys, emphasizing crossroads and bifurcations in those decades of debates and trial-and-error attempts by competing research programs. [13] The emphasis I put here on the problems of NCME aims at contributing to this interpretation of the recent history of macroeconomics as a long-standing process: the "New Classical revolution" was not made in a day, or in one paper. Quite the opposite, the NCME program seems to have reached an aporia in 1981, resulting in the popularization of competitive empirical approaches, like the RBC and VAR models. My claim is that this emergence of alternative approaches is linked to the weaknesses of the Lucasian methodology (the L-trap).

In a nutshell, Lucas's contributions in the 1970s challenged many traditional views in macroeconomics, but the main disruptive topic was the

13. This perspective has put in question the "standard" or "naïve" representation of the history of macroeconomics as a "steady accumulation of knowledge" (Blanchard, 2000, p. 1375), a linear evolution through "better" theory, "better" models and "better" policy-making. Dynamic Stochastic General Equilibrium (DSGE) models are supposed to embody the accomplishment of this progress. For a comprehensive discussion about the "naïve" history of recent macroeconomics, see Sergi (2015).

conception of models. Lucas was advocating for specifical, strong theoretical a priori constraints in modeling (rational expectations, optimization in a general equilibrium framework); he also claimed that such hypotheses are non-refutable from empirical evidence. From an instrumentalist perspective, Lucas was also supporting the idea that these constraints would not threaten the empirical performance of models' *results*—which are refutable.[14] This "L-twist" raises a more general question, which will not be extensively discussed in this paper: the epistemological and methodological transformations of the role of modeling in macroeconomics. About this issue, I will simply note here that such a transformation had been only partially achieved by the NCME program. The positive side of the NCME applied some new econometric techniques, but followed a quite traditional perspective on methodology: a macroeconometric model was conceived as a useful "mediator" between theory and empirical world (Morgan and Morrison, 1999), especially in the perspective of discriminate between competing theories. To the opposite, the prescriptive side of the NCME, following the L-twist, deeply modifies this idea of models as mediators. The details of this transformation are only briefly reported here: a comprehensive discussion about the changes in the epistemological status of models in contemporary macroeconomics should be developed in further works.

Finally, the study of the NCME raises two other wider issues, which will not be discussed here. They are connected to the closer relation (emphasized by Hoover, 1995) between history of macroeconomics and history of econometrics. The first problem concerns the application of econometric methods in macroeconomics, especially the recurrent character of some fundamental controversies about identification or the notion of structural parameters. A clear example of this permanent debate is the closeness between the arguments of the Lucas Critique and the Frisch notion of autonomous relations (and the subsequent critique addressed to Tinbergen) (Goutsmedt et al., 2015). The second issue is the dissemination in macroeconomics of new econometric techniques, starting from Lucas (1976). This perspective should be addressed by appealing to the recent works about history of econometrics (especially Qin, 2013).

The article is structured as follows: section 2 analyzes the Lucas conception of models (the L-twist) and its limits (the L-trap); section 3 reviews the consistence between the Lucas "modeling agenda" and the positive and prescriptive contributions collected in *Rational Expectations and Econometric*

14. An extensive discussion about the Lucas conception of modeling can be found for instance in Vercelli (1991); De Vroey (2011) and in Sergi (2014a).

*Practice*; section 4 presents the technical and substantial problems encountered by the NCME.

# 2 The L-twist and the L-trap

Methodology is still "a neglected aspect of Lucas's work" (De Vroey, 2011). This is possibly so because few explicit methodological contributions can be found in Lucas's writings, both published and unpublished. Nevertheless, one can collect many relevant methodological statements in his work between the 1970s and the 1980s (especially Lucas 1977, 1980, 1981c, 1987; Lucas and Sargent 1981a,c) and also in more recent writings (Lucas 1988, 1996, 2001, 2004). [15]

Modeling practices are one of the main aspects of Lucas's methodological contribution. [16] For the purpose of this paper, I am interested in the new conception of modeling in macroeconomics developed by Lucas in his works during the 1970s and, more explicitly, in the 1980s. [17] Lucas's definition of models can be resumed by the following three principles:

1. A model is an artificial economy, providing a laboratory for experiments about policy-making.
2. The model results must fit the real world data (*both* historical and future), in order to provide quantitative conditional forecasts for discussing the effects of alternative economic policies.
3. A model is built following two assumptions: dynamic optimization in a general equilibrium framework and rational expectations. The realism of such assumptions is not a matter of discussion; those assumptions cannot be tested (nor refuted), neither theoretically nor econometrically. Although, their implications (e.g. the neutrality of money) can be tested.

15. Sergi (2014a) develops a more comprehensive description of the ideas on the Lucas methodology synthesized here.

16. For Lucas, methodological matters are entirely modeling matters. To investigate his methodology means to investigate a particular conception of models. For example, in Lucas's understanding, "model" is basically the synonym for "theory": "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." (Lucas, 1980, p. 697). Or, again: "I mean theory in the sense of models, that one can write down and do something with, not in the sense of 'opinion' or 'belief'." (Lucas, 1987, p. 2).

17. Therefore, I will leave aside what the exactly the "old" ("Keynesian") conception of modeling was. About this point, see for instance Vercelli (1991) and De Vroey (2015).

In this section, I will develop in more detail the Lucasian conception of models, or the "L-twist": an original methodological conception merging apriorism with instrumentalism. I will then discuss the challenges raised by this conception ("the L-trap"). The understanding of Lucas's vision on modeling will provide the fundamental basis for the interpretation of the NCME agenda.

## 2.1 Macroeconomic modeling and the L-twist

In Lucas's perspective, a model is, first of all, a mathematical formalism, more specifically a system of dynamic stochastic equations. For Lucas, this system must "provide a fully articulated, artificial economic system, that can serve as [a] laboratory in which (...) policies can be tested" (Lucas, 1980, p. 696). Indeed, Lucas postulates that modeling provides the only suitable method for expertise in the field of policy-making:

> 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, p. 6, Lucas's emphasis)

A model is indeed, for Lucas, a mathematical description of an artificial economy, designed to perform policy-making experiments. To perform correctly this function, a model must fulfill the following requirements:

> We want a model that fits historical data and that can be simulated to give reliable estimates of the effects of various policies on future behavior.
>
> (*ibid.*, p. 7)

What does Lucas mean by "fit the historical data"? The expression rises two problems: what kind of historical data is relevant, and what are the criteria to appreciate the fit of model's results to those data?

The first problem consists indeed in providing a sharp delimitation of the "historical data", namely, the kind of real world phenomena that the model takes into account. Lucas focuses on what he calls "business cycle phenomena", namely the fact that "in capitalist economies, aggregate variables undergo repeated fluctuations about trend, all of essentially the same character" (Lucas, 1977, p. 7). These fluctuations of aggregate variables (output, consumption, investment, prices, and so on) "do not exhibit uniformity of either period or amplitude, which is to say, they do not resemble the deterministic wave motions (...)" (*ibid.*, p. 9). Nevertheless, for Lucas,

"with respect to the qualitative behavior of co-movements among [economic time] series, business cycles are all alike" (*ibid.*) and their characteristics are "common to all decentralized market economies" (*ibid.*). Indeed, the goal of macroeconomic modeling is to build an artificial world which imitates such regularities, i.e. which imitates the dynamics and the joint-dynamics (the co-movements) of the main aggregate macroeconomic time series.

The second problem is to define a standard procedure to quantitatively evaluate the consistence between artificial worlds (models) and real world phenomena (business cycles). According to Lucas, such a procedure consists in comparing the statistical characteristics of the two time series. In other words, one must observe the same "co-movements" in aggregate variables for the time series generated by the model and for the past time series of the real world.[18] As relevant statistical characteristic, Lucas had in mind two features: (1) the same counter-/pro-cyclical properties (same-sign covariances) and (2) the same amplitude in volatility. Lucas explicitly refers here to the Adelman and Adelman (1959) computer simulation of the Klein and Goldberger (1955) model for U.S. economy as a benchmark for the "fit" of historical data:

> This achievement [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. The Keynesian macroeconomic models were the first to attain this level of explicitness and empirical accuracy.
>
> (Lucas 1977, p. 11)

Although, according to Lucas, the historical data fit is a necessary but not a sufficient condition for a model to be a valuable laboratory for policy experiments. Lucas (1976)'s celebrated article (the Lucas critique) makes clear how to draw this distinction between artificial worlds that just "fit the historical data" (like "Keynesian" macroeconometric models) and artificial world serving as laboratory for "experimenting" on alternative policies. Models are effective in evaluating alternative policies, i.e. they can provide reliable predictions, if they integrate individuals' dynamic responses to changes in economic policy. Those reactions cannot be invariant as in "Keynesian" models.[19] Indeed, Lucas regarded forecasting as "conditional forecasting",

18. Lucas was strongly influenced by Herbert Simon's work on "artificial world" (Simon, 1969). Indeed, the way Lucas conceives the test of a model is a sort of Turing test. See Boumans (1997) for a discussion on this topic.

19. For a recall about the interpretations of the Lucas Critique, seeGoutsmedt et al. (2015, section 1).

i.e. as answering "questions of the form: how would behavior have differed had certain policies been different in specified ways?" (Lucas, 1977, p. 12). Only this feature, jointly with historical data fit, can insure that the model provides a trustful laboratory for economic policy evaluation.[20] Indeed, as a fundamental criterion for discriminating between competing models, Lucas is advocates for a double test: a preliminary test about the performance of models in reproducing past data (retrodiction) and a crucial test about the their conditional prediction of alternative economic policies.

The problem is then, how should these models be built? What kind of assumptions shall we use for construct our artificial economy? An explicit statement about the assumptions to be used for modeling can be found in Lucas (1980). It is worth quoting Lucas *in extenso* about this issue:

> To serve this [experimental] function well, it is essential that the artificial "model" economy be distinguished as sharply as possible in discussion from actual economies. Insofar as there is confusion between statements of opinion as to the way we believe actual economies would react to particular policies and statements of verifiable fact as to how the model will react, the theory is not being effectively used to help us to see which opinions about the behavior of actual economies are accurate and which are not. This is the sense in which insistence on the "realism" of an economic model subverts its potential usefulness in thinking about reality. 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". (...) A "good" model, from this point of view, will not be exactly more "real" than a poor one, but will provide better imitations.
>
> (Lucas, 1980, pp. 696-697)

Lucas suggests here that realism of assumptions is not a necessary condition in the kind of modeling practice he's advocating for: to fulfill its experimental function, a model must be able to provide *results*, and not mechanisms (hypotheses) that imitate real world dynamics. Lucas suggests clearly that the focus on constructing "realistic" hypotheses for models is misleading: looking for a close relation between the real world and the model assumptions is a distraction to the modeler, which must only focus about the predictions of the model being realistic. Performing such realistic forecasts is the only criterion to discriminate between different assumptions, no matter of

20. "Without knowledge as to which structural parameters remain invariant as policy changes, and which change (and how), an econometric model is of no value in assessing alternative policies. It should be clear that this is true regardless of how well [models] fit historical data, or how well they perform in unconditional forecasting." Lucas and Sargent 1981a, p. 52).

their "realisticness". Basically, we can relate this position to a strong instrumentalist view, accounting for scientific theories (here, models) as merely forecasting devices, based on unrealistic assumptions and tested only on the basis of their predictions.[21] This implies that the hypotheses are accepted to be valid instruments if their conclusions meet the empirical facts (the "as if" principle).

But, can the modeler admit any kind of assumptions as long as the model predicts well? Not for Lucas, whom, at the same time, advocates that only some very specific assumptions must be adopted in macroeconomic models. In his understanding, only a precise set of hypotheses allows models to fulfill their function. Those specific assumptions are the optimizing behavior of economic agents, in a dynamic general equilibrium framework, jointly with the rational expectations hypothesis. The use of the general equilibrium approach is advocated by Lucas as a necessary reunification of economics as a whole (or, the "microfoundation of macroeconomics"). The use of this framework is then an a priori constraint on the way of modeling:

> I think it is fairly clear that there is nothing in the behavior of observed economic time series which precludes ordering them in equilibrium terms, and enough theoretical examples exist to lend confidence to the hope that this can be done in an explicit and rigorous way.
>
> (Lucas, 1977, p. 25)

Rational expectations are also justified as an a priori hypothesis, not as a positive (or realistic) statement about the way economic agents actually form their expectations about the future:

> 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, p. 13)

According to Lucas, rational expectations are the second essential characteristic for ensure that a model produces reliable conditional forecasts. Indeed, general equilibrium (optimizing behavior) and rational expectations are universal and general laws, which cannot result from observation of reality or

21. Contrary to the "popular legacy" (Mäki, 2009) of Friedman (1953), Lucas focuses more on the idea of *conditional* prediction. For a deeper discussion about Lucasian and "Friedmanian" instrumentalism, see Sergi (2014a, section 2).

experience and, as a consequence, cannot be empirically refuted or tested: they are a priori necessary valid instruments.

We discussed above how Lucas's understanding of models can be characterized as a specific twist between an apriorist and an instrumentalist methodology. We turn now to Lucas's empirical agenda, in order to explore how such a modeling practice is supposed to consistently work.

## 2.2 Lucas's empirical agenda and the L-trap

Lucas describes retrospectively his own work in the 1970s as essentially theoretical. He considers this choice as a part of an implicit division of labor within New Classical macroeconomists:

> [Klamers's question:] *You seem to have abandoned econometrics.*
>
> [Lucas's answers] I'm not a very good econometrician any more. (...) It's not a stuff I'm very familiar with. Part of it is that I started with some empirical work a few years ago in which I was interested and to which I was going to put a fair amount of time. Then, I learned that Sargent and Sims were starting on the same line. That was very discouraging. First of all, I don't like races. Second, those guys know a lot more time series than I do. Somehow the idea that they were working on the same thing, and probably doing it better up in Minneapolis, just completely dampened my enthusiasm for my own work. And insofar I had any ideas, I just tried to tell them, tried to influence them and not carry on some parallel investigation.
>
> (Lucas in Klamer, 1984, p. 46)

Nevertheless, Lucas's bibliography at this time accounts for two empirical papers (Lucas, 1972a, 1973). These empirical contributions are not included in Lucas and Sargent (1981c) as they were published in Lucas (1981c); nevertheless, they strongly influenced the further development of the NCME, through their methodological insight. This is the first reason for reviewing them in this subsection; an additional motivation is that these articles illustrate Lucas's empirical agenda and they reflect his conception of modeling put into practice.

"Econometric Testing of the Natural Rate Hypothesis" (Lucas, 1972a) represents, according to Lucas himself (Lucas 2001, p. 21), a suggestion for an econometric test of the main conclusions drawn in "Expectations and the Neutrality of Money" (Lucas, 1972b). The model presented by Lucas focuses on one specific co-movement of the actual business cycle, namely the relationship between output and inflation (the "Phillips curve"). Starting from

rational expectations on prices, the model predicts the existence of a "natural output rate", which is insensitive to systematic changes in monetary policies [22] but sensitive to "monetary surprises" [23]. The article aims at proposing a standard econometric procedure to test this empirical result about monetary neutrality and ineffectiveness of monetary policy, derived from rational expectations: "How (if at all) can models of this class be tested?" (Lucas, 1972a, p. 98). The idea is to process an estimation by a cross-equation restriction on the parameters of a complete simultaneous equation model (see Appendix 2). Actually, Lucas (1972a) is merely "a suggestion" of that procedure: Lucas does not undertake any actual econometric test of his model.

"Some International Evidence on Output-Inflation Tradeoffs" (Lucas, 1973) provides an actual econometric test of the Phillips curve for a sample of countries. Here, Lucas is testing again the neutrality implication of Lucas (1972b), but from a different perspective: he tries to show that, the higher the inflation volatility (i.e. the higher the unsystematic, random part of monetary policy), the higher the effects on output of a monetary policy. The idea is then to estimate a Phillips curve for countries with different levels of inflation (high, intermediate, low) and to show that the slope of the curve narrows one for the weaker level of inflation (for the countries where the "signal extraction problem" is weaker). The results of estimation confirm this conclusion, and lead then to corroborate the findings about the natural rate of output, monetary neutrality and policy ineffectiveness. Unfortunately, as Lucas himself acknowledged later, the estimation procedure is incorrect (even if the results had been confirmed by further contributions). [24]

These first attempts to build a model fulfilling the L-twist methodology described in the previous subsection seem indeed very unsatisfactory. This is what we can call the "'L-trap": the a priori hypotheses do not lead to

22. "The solution of the model implies a natural output rate that cannot be bettered on average" (Lucas, 1972a, p.98). The notion of a "natural rate of output" is obviously a reference to Friedman (1968).

23. Monetary surprises are defined as unsystematic misunderstanding, by economic agents, of changes in the nominal level of prices, which they interpret as changes in relative prices.

24. "By this tile, Rapping had become interested in other issues, so I proceeded on my own. This was unfortunate, since Rapping would have caught the more glaring of the econometric errors that mar this paper [Lucas, 1973]. Despite these mistakes, however, the paper's main conclusions have stood well up. In an unpublished paper, Jose Alberro replicated these results with correct econometric methods and with a much larger sample of countries. Since then, of course, Sargent, Robert Barro, and others have devised time-series tests of the natural-rate hypothesis suited to data for a single country, so that the empirical burden on my cross-country tests has been considerably lightened" (Lucas 1981c, p.12-13).

accurate retrodictions and conditional predictions (neutrality of money and ineffectiveness of monetary policy). Then, the instrumentalist position, consisting in justifying the a priori hypotheses as valid instruments, based on the non-refutation of their predictions is a failure. In the middle of the 1970s, Lucas himself had to admit that no practical example illustrates the models he is advocating for:

> 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 the Adelmans (1959). My own guess would be that success in this sense is five, but not twenty-five years off.
>
> (Lucas, 1977, p. 25)

Actually, in the conclusion of this paragraph, one can better guess the solution to solve the L-trap. Of course, it does not consist in rejecting or modifying the a priori hypotheses of models, which are, by definition, not affected by the empirical refutation of the model results. On the contrary, the empirical strategies consist in keep trying, hoping that a day shall come, when the results of the a priori hypotheses will lead to successful empirical predictions.

Lucas expressed indeed his "faith" about being "very close" to a suitable macroeconometric modeling practice, meeting the methodological criteria of the L-twist (apriorism and instrumentalism). But his "faith" is in fact more worldly than a religious belief. Actually, Lucas's claim has strong foundations in an old-fashion positivist way of conceiving the scientific activity as a steady accumulation of knowledge, driven essentially by technological progress. [25] Actually, Lucas is (more or less implicitly) calling for more sophisticated and systematic econometric attempts to meet the retrodiction and prediction standards that he had settled for his modeling methodology. This is the challenge taken up by the NCME program and the contributors to *Rational Expectations and Econometric Practice*.

# 3 The NCME econometric practice

This section aims at constructing an ordered historical interpretation of the state of the NCME program at the publishing of Lucas and Sargent (1981c). It does not provide an exhaustive, nor a detailed or a technical

25. The conception of scientific progress in Lucas is discussed in Sergi (2015).

literature review of the NCME at this period: my purpose here is to analyze the "collective" effort of the works gathered in the 1981 volume, in the perspective of the methodological ambition of the L-twist presented above. [26]

As I recalled in the introduction, the NCME had two goals. The first was a positive goal: to bring empirical evidence to support the main theoretical result of Lucas (1972b), i.e. the neutrality of money and policy ineffectiveness. The second was a prescriptive goal: to set new rules for macroeconometric modeling practices. We can now easily understand those two goals as the two sides of the Lucas conception of modeling. The positive goal of the NCME try to meet the instrumentalist standard of Lucas, by providing some empirical tests of the *conclusions* of the models—by verifying if the optimizing-rational expectations models can reproduce historical data and provide conditional forecasting. The non-refutation of the results corroborate the use of the a priori hypotheses (the "as if" principle): they are then valid instruments for forecasting. In a reverse perspective, the prescriptive side of the NCME attempts to specify the implications of the Lucasian apriorism, i.e. to precise the "proper way" for building models with some predefined characteristics—the a priori hypotheses: optimization and rational expectations). Moreover, the "proper way" must be regarded not only as a purely mathematical and theoretical development, but mainly as an attempt to construct operational models, which can be actually estimated for testing their conformity with past and future data.

This section addresses, first, the methods and results of the NCME positive side (test of the neutrality of money and policy ineffectiveness); second, it explores the methods and techniques for formulating and estimating macroeconomic models following the a priori restrictions suggested by Lucas.

## 3.1 Econometric tests of New Classical macroeconomic models

The NCME approach focuses on testing the main results of Lucas (1972b): assuming optimizing agents with rational expectations in a general equilibrium framework, there is no systematic trade-off between inflation and unemployment (or output), which means that money is neutral and monetary policy ineffective (unless the situation is one of a "monetary surprise", with a "signal extraction problem"). More concretely, the NCME tries to estimate

26. As a consequence, our classification of the articles collected in the volume does not correspond at all with the one used by the editors, which had classified the contributions in six "sections" following a very different logic, which is not worth discussing here (about this issue, see Lucas and Sargent, 1981b, pp. xvi-xxvi).

the slope of the Phillips curve, and to bring evidence about the fact that the curve is actually a vertical line (the "natural rate hypothesis").

The NCME insists on three errors made by "Keynesians", which lead them to systematically underestimate the slope of the Phillips curve : first, they assume that inflation is a highly serial correlated process; second, "Keynesians" use inflation lags as a proxy for expectations; third, they favor single equation tests. The first error is mainly discussed in Sargent (1981c).[27] In that paper, Sargent shows a weak serial-correlation for inflation. The second error must be corrected by using rational expectations, which lead also to the correction of the third error: the Phillips Curve must be tested by a cross-equation restriction estimation, of the type suggested (but not carried out) by Lucas (1972a). Here, the fundamental idea is that individually testing the significance of various parameters and of the reduced forms (as in "Keynesian" models) is not a satisfying procedure: the crucial test must be about the *relationship* between parameters (see Appendix 2). Sargent (1981e) sums-up this double criticism and carries out the "proper tests": the result is the non-refutation of the "natural rate hypothesis", then, the validity of rational expectation and optimization as sound instruments.

The main implication of the "natural rate hypothesis" is the neutrality of money and, consequently, the ineffectiveness of monetary policy: monetary policy cannot systematically influence output by increasing the money supply. Testing this proposition brings up a second econometric innovation: the use of the Granger-causality test in the wake of Granger (1969) and Sims (1981).[28] Barro (1981b) and Barro (1981c) try to apply directly the Granger causality to the question of "'monetary surprise". Indeed, in Lucas (1972b), only unexpected changes in money supply can affect the levels of

27. All the articles collected in Lucas and Sargent (1981c) were not refereed here in respect of their first date of publishing, but in respect of their position in the volume. For each, a note in the bibliography specifies the complete reference about the first publishing.

28. For recall, Granger (1969) suggests that the causality relation between two stationary stochastic processes $Y, X$ is defined as: $\sigma^2(X|U) < \sigma^2(X|U-Y)$ ($U$ denoting all available information at time $t$, including past values of $X$ and $Y$, and $\sigma^2(X|\overline{U})$ the variance of the predictive error of the optimum, unbiased, linear least square predictor of $X$). Sims (1981) uses the Granger definition for testing unidirectional causality between two macroeconomic time series $X, Y$. He estimates:

$$\begin{aligned} Y_t &= \alpha_1 Y_{t-1} + \alpha_2 X_{t-1} + u_t \\ X_t &= \beta_1 X_{t-1} + \beta_2 Y_{t-1} + v_t \end{aligned}$$

One can conclude that $X$ cause $Y$ if $\widehat{\alpha_2} \neq 0$ (or, if one can better predict the current value of $X$ by using all available information *including* $Y$ than by excluding $Y$ of the information set). The causality is unidirectional (no feedback of $Y$ on $X$) if the $\widehat{\beta_2} = 0$. In this case, one can say that $X$ is an "exogenous" variable.

unemployment and output. The test in Barro (1981b) leads exactly to this empirical result, providing evidences that expected money supply growth does not cause (in Granger's sense) changes in the level of unemployment, but unexpected changes in monetary supplies does. Barro (1981c) give a more detailed account of this first finding, explicitly linking money supply with price formation and unemployment with output. In this vein, Sargent and Wallace (1981a) and Sargent (1981f) studied hyperinflation periods and showed that, in this specific case, monetary creation Granger-causes inflation, with no feedback. Similar contributions in the volume, like Barro (1981a), discussed some specific conditions for ineffectiveness of monetary policies to hold (namely, that monetary authority has no informational advantage).

The same framework is applied to a different question by Hall (1981), following Muth (1981b). In his paper, Robert Hall tests the Friedman life cycle-permanent income hypothesis to conclude that "only unexpected changes in policy affect consumption" (Hall, 1981, p. 503). In a general way, these empirical studies about ineffectiveness of economic policies feed the more theoretical debates presented in the last part of *Rational Expectations and Econometric Practice*, like Kydland and Prescott (1981), which I will not address here.

Therefore, the positive side of the NCME program brings empirical evidence supporting the results of neutrality and ineffectiveness, implied by the a priori hypotheses of the Lucasian models. The test of these conclusions, in from an instrumentalist perspective, is assumed to provide strong corroboration for New Classical models built with optimizing agents and rational expectations. Sargent and Wallace formulate very precisely their understanding of the Lucas modeling methodology as the main guideline of their own econometric practice:

> Ordinarily, we impose two requirements on an economic model: first, that it be consistent with the theoretical core of economic-optimizing behavior within a coherent general equilibrium framework; and second, that is not refuted by observations.
>
> (Sargent and Wallace 1981b p. 208)

We turn now to the prescriptive side of the NCME research program, aiming at editing the operational rules and techniques for constructing a complete model of the business cycle.

## 3.2 How to build a New Classical macroeconometric model?

The core of the prescriptive side of the NCME can be seen as a very simple rule about model-building practices. The purpose is to construct models which analytically derive and estimate an optimal dynamic decision rule for the economic agents. In their introduction to *Rational Expectations and Econometric Practice*, Lucas and Sargent formalized the problem as follows. The action $u_t$ chosen by an economic agent (e.g. the amount of consumption, investment, etc.) maximize an objective function $V$ (a preference or profit function), which depends on two separate factors: the evolution of the environment $z_t$ (including economic policy) and the evolution of the control variables $x_t$.[29] The decision rule $h$ can then be decomposed as

$$h(z_t, x_t) = h_1(h_2(z_t), x_t)$$

Indeed, in Lucas's and Sargent's understanding, the decision rule consist of an "optimization part" ($h_1$) and a "forecasting part" ($h_2$). Note that the decision rule is dynamic, i.e. agents react to changes in their environment, to the extent that $h_2 = f(z_{t-1}, \epsilon_t)$ or, simply, that $h = T(f)$. From the NCME perspective, the error of structural "Keynesian" models was to specify decision rules which were not derived from an optimizing behavior and, as a consequence, invariant in respect of changes in $f$.[30] This error was pointed out by the Lucas Critique Lucas (1976). The NCME models, which "properly" specifies $h$ as a function of $f$ through the rational expectations hypothesis, are no more subject to such an error, and, as a consequence, they are capable of producing consistent conditional forecasting of the effects of alternative economic policies.[31] Notice that there are anyway some "deep" structural parameters (the objective function, the preferences and the technology) that are invariant in respect of changes in the environment.

As Hansen and Sargent (1981a) summarized it, the NCME strategy to build macroeconometric models "involves estimating agent's decision rules

29. Then, an agent is supposed to choose the $u_t$ which maximizes:

$$\mathbb{E}_0 \left\{ \sum_{t=0}^{\infty} V(z_t, x_t, u_t) \right\}$$

under the constraints that $x_{t+1} = g(z_t, x_t, u_t)$, $g$ being the "technology" and $z_{t+1} = f(z_t, \epsilon_t)$, $f$ being the "stochastic process" governing the evolution of exogenous variables.

30. Typically, the expectations $h_2$ were specified as lagged variables, producing only backward-looking ("adaptive") expectations.

31. "After Keynesian Macroeconomics" (Lucas and Sargent, 1981a) extensively discusses this point.

jointly with models for stochastic processes they face, subject to cross-equation restrictions implied by the hypothesis of rational expectations" (Hansen and Sargent 1981a, pp. 7-8). In addition to this general principle, we must notice that Granger-causality tests also play a fundamental role to determine the exogenous character of a variable—this means, whether the variable belongs to the set of stochastic processes. This prescriptive ambition of the NCME is embodied (and enriched) by the contributions to *Rational Expectations and Econometric Practice* presented in the following lines.

Four contributions (Muth, 1961, 1981a; Granger, 1969; Sims, 1981) were included in the volume because they introduced, independently from the NCME program itself, the main technical innovations which are necessary to the NCME. Namely, a manuscript of Muth (Muth, 1981a), published for the first time in that volume, is presented by the editors as a contribution showing "the way to literally all of the estimation techniques for rational expectations models that subsequent researchers have used or proposed" (Lucas and Sargent, 1981b, p. xx).

The main forerunners of the NCME approach (but, this time, "consciously" contributing to this research program) are Lucas (1981a,b) and Lucas and Prescott (1981): in this (early) articles, Lucas and Prescott try to derive an optimal decision rule for investment, in which firms face a stochastic environment (e.g. the demand schedule shifts randomly). For the development of this prescriptive line of research, the work of Sargent represents a dominant influence on the NCME, both quantitatively and qualitatively. In Sargent (1981d), we can find a first work in the same vein than Lucas and Prescott (1981)—a model attempting to formulate and estimate an optimal decision rule for labor demand. Similar efforts are made by Sargent and Wallace (1981a) and Sargent and Wallace (1981c), in computing optimal monetary policy rules (in the perspective of debating the consistence of Friedmanian k-percent rules). On the contrary, Sargent (1981b) tries to define the stochastic processes faced by the agents (namely, the structure of the interest rates). This computational effort for solving a maximization problem in a general equilibrium framework with rational expectations reaches is acme with the two papers of Hansen and Sargent (Hansen and Sargent, 1981a,b). These two contributions quickly became the hallmark for the NCME, providing the full, detailed range of the solution and estimation methods for New Classical macroeconomic models.

The issue of the estimation of the optimal decision rules is addressed in all the papers with a relative homogenous technical apparatus. The Granger test for causality is widely used for "disentangling the parameters governing the stochastic processes that agents face from the parameters of their objective function" (Hansen and Sargent, 1981a, p. 91). In reference to the previous

notation, Granger-causality allows to distinguish between endogenous and exogenous parameters, so between the problem of identifying $f$ and $V$ (and subsequently, $h_2$ and $h_1$). Computational methods of dynamic optimization are used for "deriving a convenient (tractable) expression for decision rules" (*ibid.*, p. 92)[32]; time-series analysis provides specification for the form of stochastic processes and properties of error terms in the model. Finally, maximum likelihood methods are used to estimate the decision rules. These form of estimation is very much discussed in the volume, especially by some purely technical contributions like Wallis (1981) and Chow (1981).[33]

The purpose of this section was to briefly illustrate the concrete form of the NCME methods and the results presented in *Rational Expectations and Econometric Practice*. Our purpose was to strictly relate the L-twist with the "collective" effort for providing positive corroboration of the results of money neutrality and the prescriptive attempt to define rules for building, solving and estimating New Classical models. We turn now to the weaknesses of the NCME program and their relation with the L-trap.

# 4 Problems in NCME

*Rational Expectations and Econometric Practice* was conceived, by admission of its editors themselves, as a "bandwagon" (Lucas and Sargent, 1981c, p. xxxviii) for the NCME, collecting almost exclusively the work of "those who find the hypotheses [of the NCME] promising and attractive" (*ibid.*).

This section aims at clarifying the difficulties of the NCME program, as they were admitted by its promoters (and, to be clear, not as they were pointed out by their opponents). Again, we will not provide here a detailed, technical sum-up of the problems for solving and estimating such macroeconomic models.[34] Our purpose is to enlighten the obstacles of the NCME—both in the empirical support of its theoretical results and in the provision of operational versions of its models. This perspective raises a similarly dilemma to the one already discussed for the L-trap.

32. At the time, such methods had been extensively discussed in Sargent's textbook (Sargent, 1979).

33. It must be noticed here that the Generalized Method of Moments (Hansen, 1982) will be implemented only one year after the publication of the Lucas-Sargent volume.

34. Ingram (1995) already provided a synthetic account of these problems.

## 4.1 Do NCME provide sound empirical evidence?

The instrumentalist view implicit in Lucas's understanding of modeling puts the emphasis on the ability of models to reproduce historical data and to provide conditional forecasts of the effects of alternative economic policies. In section 3.1, we suggested that the positive line of research of the NCME embodies this methodological principle. We briefly presented the heterogeneous contributions collected in *Rational Expectations and Econometric Practice* which tried to provide evidence for supporting the results of money neutrality and policy ineffectiveness. As we reported, some evidence, produced with the specific methods of the NCME (cross-equation restriction, Granger-causality test), seem to corroborate these two predictions.

Nevertheless, in this very same volume, many contributors were very cautious, or even skeptical, on the current development of the program in respect to its positive ambition. McCallum writes for instance:

> For the most part, the formal econometric evidence developed to date is not consistent with the neutrality proposition. But the power of the existing tests is probably not high and, in any event, the evidence not entirely clear-cut.
>
> (McCallum, 1981b, p. 291)

McCallum's statement implicitly make reference to two kind of contribution, presented in the Lucas-Sargent volume. These results bring us to nuance our previous statement (3.1) about the positive achievement of the NCME.

The result of policy ineffectiveness, i.e. the fact that monetary policy cannot systematically increase output (except for the "monetary surprise" case), is not so much a clear-cut finding of the NCME. Strange enough for a collection of articles conceived as a "bandwagon", the contributions of Fischer (1981); Sargent and Wallace (1981a); Sargent (1981f) and McCallum (1981a) sound actually very cacophonous. They all essentially bring into the debate the generality of the result of monetary neutrality. The puzzling nature of those articles is that they follow the fundamental principles of New Classical modeling (optimization and rational expectations). Despite this commitment to the Lucasian conception of models, they come to contradict its main implication. These refutations of the result of policy ineffectiveness shall lead, in an instrumentalist understanding, to a refutation of the hypotheses (optimization, rational expectations) as valid instruments for modeling. But this is not the case.

A more general and even more disturbing result is the "observational equivalence" emphasized by Sargent (1981g). In his article, Sargent shows that both "Keynesian" and New Classical rules for monetary policy can produce the very same kind of empirical evidence. More precisely, the estimated

reduced forms of those rules give similar estimation results; then, we cannot reject one rule or the other by simply relying on the estimation of the reduced form. The "observational equivalence" raises the question of the importance of the apriorist methodology for macroeconometric modeling[35]. Indeed, the refutation of the result by empirical findings is no more a sufficient test for discriminate between alternative hypotheses. Sargent concludes:

> To rule on the policy issue thus requires bringing to bear theoretical considerations or doing empirical work of a kind considerably more subtle than that directed solely at estimating reduced forms.
>
> (Sargent, 1981g, p. 554)

The conclusion of Sargent illustrates very well the bifurcation of the research paths caused by the L-trap. Instead of leading to a rejection of the hypotheses, the unclear nature of empirical findings lead to strengthen the theoretical a priori in modeling. Since optimization and rational expectations are the "right" hypotheses, if their implications are not corroborated by empirical tests, the tests are then considered to be wrong. They must be rejected as if they were not well-posed or not well-executed. As a consequence, macroeconometric modeling must move to more sophisticated procedures for testing, in order to corroborate at the end the a priori hypotheses.

In a very similar vein, Sargent (1981a)'s test of a complete New Classical model for the United States reaches the conclusion that one must reject the New Classical policy ineffectiveness result. Although, as the empirical result is (again) not clear-cut, Sargent argues that this confirm the "presumption of guilty" of traditional "Keynesian" macroeconometric models. It is worthy to quote here Sargent's conclusion at length, as it confirms, again, the influence of the aprioristic bias in interpreting empirical results:

> Some evidence for rejecting the model has been turned up [in this article], but it is far from being overwhelming and decisive. (...) the test had turned up little evidence requiring us to reject the key hypothesis of the model that government monetary policy and fiscal policy variables do not cause unemployment or the interest rate. The fact that such evidences has been hard to turn up ought to be disconcerting to users of the existing macroeconometric models, since as usually manipulated those models all imply that monetary and fiscal policy *do* help cause unemployment and the interest rate.
>
> (Sargent, 1981a, p. 550)

35. Note that Sims (1980) take exactly this result to bring a radical different conclusion, consisting in rejecting any kind of identification restriction derived from the theory as unnecessary.

Here Sargent is reversing the burden of proof on "Keynesian" models: since one can hardly proof the effectiveness of economic policy, it follows that this result is suspect, especially when we assume, a priori, that the "true" result is the ineffectiveness of policy...

Following the L-trap, my point here was to show how the controversial nature of the evidences, brought by the positive side of the NCME, opens the way for a more prominent role of the prescriptive side, i.e. for more a priori theoretical restrictions for macroeconometric modeling. But can the prescriptive side *actually* provide the "proper way" for producing the "right" results?

## 4.2 Problems in solving and estimating NCME models

The apriorist view implicit in Lucas's methodology for macroeconomic modeling claims that, in order to build a macroeconometric model, one must adopt two crucial assumptions: optimization and rational expectations. In subsection 3.2, we provided a brief synthesis of the methods implemented by the NCME (essentially under Sargent's lead). Here, we discuss some problems of those methods.

Despite an optimistic foresight, even Lucas and Sargent admitted that one must consider "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, 1981a, p. 309). In the NCME understanding, the difficulties of the modeling program are essentially a "technical" rub. In a more critical perspective, one can analyze the sense of these problems as the burden of the constraints imposed by the Lucasian aprioriism, i.e. the exigences in the derivation of optimal dynamic decision rules, in a general equilibrium framework with rational expectation. Again, this must be analyzed jointly with the instrumentalist requirement, which is that such strongly constrained models must provide empirically testable results. Those requirements, implied by the L-twist, make impossible to modify the model assumptions for meeting a better data fit or a greater tractability (as did the "Keynesian" tradition); at the same time, the L-trap makes impossible to ignore the defective empirical results obtained with these assumptions.

Identification is the first main issue flawing the NCME approach. The main argument against "Keynesian" macroeconometric modeling is that it imposes a priori identification of the decisions rules ($h_1$ and $h_2$ in previous notation), especially by constraining expectations formation. This lead to incorrectly identified parameters, to the extent that they are including policy-regime contingencies. As we suggested in the previous section, the NCME claims not to impose such misleading restrictions: instead, an NCME model

must provide a fully derivation of these rules from optimizing principles, in order to reach a complete identification of the parameters. Although, even in the NCME, an a priori constraint on at least one parameter is necessary, especially for the "deep parameters" in the objective functions (Hansen and Sargent, 1981b, p. 150-151).[36]

Specification is the second problem of the NCME modeling practice. Again, the NCME attacked "Keynesian" macroeconometric modeling by pointing the non-sense of their a priori restriction over the form of the decision rules (for instance, "Keynesian" consumption fonctions, which are not derived from an optimization rule). The ambition of the NCME is to derive the decision rules in a way consistent with the optimization in a general equilibrium framework. Actually, in its own practices, NCME macroeconometricians are constrained to specify, for instance, quadratic objective functions in order to obtain linear decision rule: this constraint is essential to carrying out the estimation, but it is not theoretically grounded. The cross-equation restrictions on the relations between parameters are as well highly non-linear, adding then a supplementary effort for the modelers. The specification of stochastic processes (for instance, specifying the structure of the error term) is also a main requirement for bearing maximum likelihood estimation: it leads also to impose some *ad hoc* constraints to the specification of those forms (or, alternatively, to use limited-information methods).

In a more general perspective, the NCME a priori hypotheses lead to the problem of tractability of the models, which requires great effort in terms of computations and estimation. This "technical" or "computational" burden became heavier and heavier for more and more extensive models: it is very high for the case of investment with one control variable (Hansen and Sargent, 1981a), no need to say that it would be quite unsustainable for a complete, fully articulated model of the business cycle. The constraint here is more technical and technological, meaning that computational powers and mathematical training of macroeconomists bounded the possibility for developing and estimating the macroeconometric models of the NCME. Those limitations are of course well understood by the contributors of *Rational Expectations and Econometric Practice*, since they put the emphasis on looking for some parsimony in the use of computation and estimation techniques (Hansen and Sargent, 1981a, p. 149).[37]

36. Ingram (1995, pp. 21-22) provides a technical description of the problem.

37. A more clear example of this willingness to simplify the NCME approach is "A Rational Expectations Approach to Macroeconometrics" (Mishkin, 1983). Here, Mishkin claims that "Estimation is simple to execute with the techniques of this book and readily available computer packages (...): this is less true of techniques such as Hansen and Sargent (1981a)'s" (Mishkin, 1983, p. 2). This is true, but Mishkin uses more identification restric-

In this section we pointed out the main problems in the NCME program. We emphasized that these problems arise from the L-trap. The vulnerability of the empirical evidences open the way for an internal debate about the further line of research. Aiming at a more sophisticated empirical strategy, the NCME must deal with considerable problems of identification, specification and tractability. These problems derive from the technical constraints imposed by the use of optimization and of rational expectations as the a priori hypotheses.

# Concluding remarks

This article discussed how the methods and problems of the NCME, presented in *Rational Expectations and Econometric Practice*, can be understood in the light of the notions of the L-twist and the L-trap. The positive and prescriptive side of the NCME can be analyzed as a modeling practice obeying to the apriorist/instrumentalist principle proposed by Lucas. Indeed, the difficulties of this modeling practice shall be interpreted as the subsequent dilemma brought by this methodological approach. *Rational Expectations and Econometric Practice* embodies both the ambition of the Lucasian modeling methodology and its weaknesses.

The Lucasian modeling methodology made of this volume a turning point and must be regarded with more attention in the perspective of constructing a less linear and more "Frostian" perspective on the history of macroeconomics.[38] Indeed, the difficulties of the NCME open the way for three divergent research paths, providing different solutions to the following question: what shall we do if the results of the a priori hypotheses do not fit the data (both historical and future) and/or do not allow to build tractable models? One solution consists in keep going in the same direction, trying to get "sounder" evidence and "more operational" models. This path is that of the NCME after Lucas and Sargent (1981c). Such a way is paved with more and more sophisticated and *ad hoc* computational and econometric techniques, reproducing, in some aspects, the "bricolage techniques" of "Keynesian" macroeconometric modeling that NCME aimed to criticize. This way essentially represents the further development of the NCME after the period under consideration. The alternative way to escape the L-trap is to reject

tions than the standard NCME approach, allowing then for a less extensive computational and estimation effort.

38. "Two roads diverged in a wood, and I– \ I took the one less traveled by, \ And that has made all the difference." (Robert Frost, "The Road not Taken", in *Mountain Interval* (1916). The metaphor is suggested by Hartley (2014).

the a priori assumptions, as advocated by the VAR models. An even more radical path can be found in calibration methods of the RBC, giving up estimation methods of the NCME and embracing more deeply the aprioristic constraints.

As I emphasized in the introduction, my analysis in this article focuses on an historical comprehension of the methodological concerns that constitute the core of *Rational Expectations and Econometric Practice*. Indeed, other and interesting researches can be undertaken taking as starting points the issues presented in this paper, especially in the direction of a deeper epistemological discussion about the status of macroeconometric models after the "New Classical revolution".

# Appendix

## Appendix 1

Table 1: *Rational expectations and Econometric Practice*: year

| First publication or draft | Number of articles and manuscripts |
|---|---|
| before 1972 | 8 |
| 1972-1975 | 6 |
| 1976-1981 | 19 |
| s.d. | 1 |
| Total | 34 |

Table 2: *Rational Expectations and Econometric Practice*: Journals

| Publication in | Number of contributions |
|---|---|
| *Journal of Political Economy* | 9 |
| *Econometrica* | 6 |
| *Journal of Economic Dynamics and Control* | 3 |
| Other Journals | 12 |
| Manuscripts | 4 |
| Total | 34 |

Table 3: *Rational Expectations and Econometric Practice*: authors

| **Author** | **Number of contributions** | **Academic affiliation (1981)** |
|---|---|---|
| Thomas J. Sargent | 13 | University of Minnesota |
| Robert E. Lucas | 4 | University of Chicago |
| John Muth | 3 | Carnegie-Mellon |
| Lars P. Hansen | 3 | Carnegie-Mellon |
| Robert J. Barro | 3 | University of Rochester |
| Neil Wallace | 2 | University of Minnesota |
| Edward C. Prescott | 2 | Carnegie-Mellon |
| Gregory C. Chow | 2 | Princeton University |
| Bennet T. McCallum | 2 | University of Virginia |
| Guillermo Calvo | 1 | Columbia University |
| Stanley Fischer | 1 | MIT |
| John Taylor | 1 | Princeton University |
| Kenneth Wallis | 1 | University of California |
| Christopher Sims | 1 | University of Minnesota |
| C. W. Granger | 1 | University of Nottingham |
| Robert Hall | 1 | Stanford University |
| Finn Kydland | 1 | Carnegie-Mellon |

Table 4: *Rational Expectations and Econometric Practice*: Methods / 1

| **Type of contribution** | | |
|---|---|---|
| **Theoretical** | **Econometrical** | **Other** |
| Lucas Chap. 4 | Muth Chap. 1 | McCallum Chap. 15 |
| Lucas Chap. 5 | Muth Chap. 2 | Lucas and Sargent Chap. 16 |
| Lucas and Prescott Chap. 6 | Sargent Chap. 3 | |
| Barro Chap. 12 | Hansen and Sargent Chap. 7 | |
| Fisher Chap. 13 | Hansen and Sargent Chap. 8 | |
| McCallum Chap. 14 | Sargent Chap. 9 | |
| Kydland and Prescott Chap. 31 | Sargent and Wallace Chap. 10 | |
| Calvo Chap. 32 | Sargent and Wallace Chap. 11 | |
| | Muth Chap. 17 | |
| | Wallis Chap. 18 | |
| | Chow Chap. 19 | |
| | Granger Chap. 20 | |
| | Sims Chap. 21 | |
| | Sargent and Wallace Chap. 22 | |
| | Sargent Chap. 23 | |
| | Sargent Chap. 24 | |
| | Sargent Chap. 25 | |
| | Hall Chap. 26 | |
| | Sargent Chap. 27 | |
| | Sargent Chap. 28 | |
| | Barro Chap. 29 | |
| | Barro Chap. 30 | |
| | Taylor Chap. 33 | |
| | Chow Chap. 34 | |

## Appendix 2

**The cross-equations restriction approach**

Lucas (1972a) proposed a model of the following kind:

$$\widehat{y}_t = y_t - \overline{y}_t + \alpha(p_t - p_t^e) + \epsilon_t$$

$$p_t = m_t - y_t + u_t$$

$$m_t = \lambda + m_{t-1} + e_t$$

Equation 1 represents an aggregate supply function in the understanding of Lucas and Rapping (1969), equation 2 an aggregate demand function, and equation 3 a monetary policy rule fixing the evolution of the quantity of money. The expectations are supposed to be rational, i.e. $p_t^e = \mathbb{E}(p_t|I_{t-1})$.

Rearranging the above equations, we can described the model with two equations, relating price level expectations $p_t^e$, the actual growth of money $m_t - m_{t-1}$, the rule for monetary policy $\lambda$, the global output $\widehat{y}_t$ and the random component of output $\overline{y}_t$:

$$\begin{aligned} p_t^e &= \lambda + m_{t-1} - \overline{y}_t \\ \widehat{y}_t &= -\frac{\lambda\alpha}{1+\alpha} + \frac{\alpha}{1+\alpha}(m_t - m_{t-1}) + \epsilon_t + \frac{u_t}{1+\alpha} \end{aligned}$$

The reduced forms to be estimated are then:

$$\begin{aligned} m_t &= a_0 + a_1 m_{t-1} \\ \widehat{y}_t &= b_0 + b_1 m_t + b_2 m_{t-1} \end{aligned}$$

If the model is correct, then the estimation results must logically lead to:

$$\begin{aligned} a_0 &= \lambda & a_1 &= 1 \\ b_0 &= -\frac{\lambda\alpha}{1+\alpha} & b_1 &= \frac{\alpha}{1+\alpha} \quad b_2 = \frac{-\alpha}{1+\alpha} \end{aligned}$$

Then, an indirect way for econometrically testing the model is to test the following cross-equation restrictions:

$$\frac{b_0}{a_0} = -b_1 \qquad b_1 = -b_2$$

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