Econometric Methods and Problems in New Classical Macroeconomics

Francesco Sergi*

Séminaire du Laboratoire d'Economie d'Orléans (LEO)

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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Rational Expectations and Econometric Practice (Lucas and Sargent, 1981c) collects 34 published and unpublished articles by 17 different authors, all dealing "with various aspects of the general problem of drawing inferences about behavior from observed economic time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 1981c, p. xi). The purpose of the volume is to sum up the empirical techniques, the application of the latter and the subsequent findings of New Classical macroeconomics[1]. As the editors put it in their introduction, the general 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, on his own, an advanced textbook, *Macroeconomic Theory* (Sargent, 1979), illustrating the main technical issues in New Classical

*Université Paris 1 - Centre d'Économie de la Sorbonne (CES). Francesco.Sergi@univ-paris1.fr. The article was presented at the 19th Annual Conference of the European Society for the History of Economic Thought (ESHET). The author thanks Paolo Paesani, Pedro Garcia Duarte and Roger Backhouse for their valuable comments.

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

macroeconomics. These three volumes should be regarded as an attempt to unify, to clarify, to popularize and to provide theoretical and methodological consistence to this research program developed during the 1970s.[2] Actually, *Rational Expectations and Econometric Practice* is the more puzzling among those three publications. First of all, even if Sargent's contribution is quantitatively very important, the volume represents an genuine synthesis, in the sense that it represent a "collective" effort rather than an individual sum-up provided by the leading figures of the New Classical macroeconomics.[3] *Rational Expectations and Econometric Practice* attracted our attention because it provides a deliberate attempt of a synthesis of a particular research program within New Classical macroeconomics, all with preserving a collective dimension and its heterogeneity. Indeed, at 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 drawn 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, 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 1976-1981 period. Both manuscripts and articles, published in a wide range of academic journals, are presented, even if most of the articles had been published in the *Journal of Political Economy* and in *Econometrica*.

The first purpose of our article is therefore to analyze this heterogeneous volume and to reconstruct an ordered comprehension of the econometric methods of New Classical macroeconomics. We will refer to this literature as "New Classical macroeconometrics" (NCME hereafter). We claim here 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. We must precisely insist that one of the strong claims of our paper is about the needed connection between Lucas methodology and NCME developments. This link make the originality of the present article

[2] This claim seems, retrospectively, not much 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 been 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)—were conceived in the first place as a contribution to the specific program of New Classical macroeconomics. Nevertheless, we can argue that the different authors have some degree of adhesion to the latter, to the extent that they accepted to re-print their articles in this volume.

(and what distinguished it from a plain "literature review").[4]

Lucas methodology must be understood as a specific way of conceiving the interaction between theory and empirical world through models. In this article, we 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 a function of forecasting device, it must indeed produce testable predictions, corroborating the underlying assumptions.[5]

Following those requirements, formulated by Lucas, the NCME developed two objectives.

The first purpose can be seen as a "positive" research program. Indeed, NCME tries to work out empirical evidences against the so-called "Keynesian" macroeconomics[6] and in favor of the New Classical results. 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). Basically, the positive side of NCME consists in supporting the 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 purpose of the NCME program is a "prescriptive" one. The general goal is to provide operational guidelines for macroeconometric modeling. Those rules are new and alternative to the traditional macroeconometric approach of the "Keynesian" *à la* Klein and Goldberger (1955), criticized in the first, positive line of research. More precisely, the NCME aims to define an explicit set of rules for building and estimating a complete model of the

[4] We must also notice that the explicit methodology presented in Lucas's writings lead us to a specific focus on his figure, clearly more influential on this perspective. However, Sargent played a more crucial role in the NCME, theoretically but also technically, which will be in some way downplayed here, for the same reason. About Sargent, see Sent (1998).

[5] Hoover (1988), in his concluding chapter about New Classical macroeconomics and the Austrian school, still provided the intuition for the arguments developed here.

[6] For sake of simplicity, we use, as New Classical macroeconomists did, the word "Keynesian" to designate the dominant paradigm in macroeconomics during the 1960s and 1970s (presently labeled also as the "New Classical Synthesis"), leaving aside the question of knowing to which extent this approach followed the idea of Keynes and to which extent it does not. For an extensive discussion about this matter, see for instance De Vroey (2009). For a discussion about the influence of Cowles Commission macroeconometric agenda, and especially of Lawrence Klein, in this line of research, see Pinzón-Fuchs (2014).

business cycle, following the a priori hypotheses proposed by Lucas: optimizing behavior in a dynamic general equilibrium framework and rational expectations[7]. 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, form 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 ground then, on the one hand, in a positive approach testing the neutrality of money, and, on the other hand, in a prescriptive approach trying to identify, by econometric estimation, the deep parameters governing economic agent's optimal dynamic decision rules. From the perspective of the econometric techniques, we also distinguish two approaches of the NCME for answering both those two questions. On the one hand, a bunch of innovative contributions tries to bring new econometric methods to the field of applied macroeconomics. We can easily identify two types of innovations: the approach by cross-equation restrictions and the time series analysis, especially in the wake of Granger's definition of causality (Granger, 1969). On the other hand, we can found a larger amount of "applied" works, taking those new methods to different samples, application and examples.

The NCME aims to provide 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 this publication is not the full stop to the NCME program[8], it should

[7] 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 Ω_{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.

[8] Ingram (1995) provides an useful synthesis of the further developments of the NCME.

be regarded, considering its historical context, 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) his 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).[9] Although, the NCME did not (and would not) completely shut down the macroeconometric modeling approach developed (especially) by the Cowles Commission, Jan Tinbergen and Lawrence Klein. Instead, New Classical macroeconomists clearly recognize 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, reliable in advising policy-makers.[10] Indeed, the NCME should be regarded as an attempt at (radically) amending "Keynesians" macroeconometric models, and not as an attempt to completely destroy them.

We 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 (using deep parameters for quantitatively reproducing business cycles), RBC models give up econometric estimation for setting the parameter values with prior information *outside* the model. Even if Kydland and Prescott advocated further 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 a leading figure of the NCME, as Lars P. Hansen, pointed out how the calibration method is far from providing a reliable alternative to traditional macroeconometric modeling (Hansen and Heckman, 1996). The net divide between RBC calibration and NCME estimation force the historian to explore the

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

[10] As Lucas, in his famous Critique, put it: "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).

two separately, despite their common origins.[11]

In a quite different perspective but with a similar result, Christoper Sims's "Macroeconomics and Reality" (Sims, 1980) put in question the theory-driven approach to econometrics leaded by both NCME and RBC. This very fruitful, data-driven (or "with not too much a priori theory") approach, using vector autoregressive models (VAR), provided a second competitive research program to the NCME.

This clarification about the relation between NCME, "Keynesians", RBC and VAR modeling strategies leads us to the second purpose of our article. Indeed, the NCME expressed clearly is ambition to become the alternative to traditional "Keynesians" macroeconometric modeling: since, at the same time, competing approaches had arose, 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 as well for their main contributors. The prescriptive side is even more problematic. At the time of *Rational Expectations and Econometric Practice*, the NCME could not offer a valuable alternative to the traditional macroeconometric models. Their model were highly complex, disappointing from the perspective of quantitative reproduction of business cycle features and suffering of similar specification and identification problems as their "Keynesian" opponents.[12] Again, those difficulties will be analyzed from the perspective of Lucas methodology: we will emphasize the paradoxical implications arising from the L-twist conception of modeling, which we call the "L-trap".

The main claim of our article is that *Rational Expectations and Econometric Practice* must be regarded as a turn in macroeconometric modeling, closely related to Lucas's conception of models. Indeed, our purpose is to construct, as an historian, a clear understanding about methods *and* problems of the NCME.

We will conclude this (long) introductory section by clarifying some general historical issues raised by our specific object. Even if they will not be addressed by this article, it is worthy to recall how our study of *Rational Expectations and Econometric Practice* must be regarded as a contribution

[11] The consistence between Lucas conception of modeling and the RBC approach is discussed in Sergi (2014b).

[12] This claim, as we will show further in our article (section 4), came not only from the opponents to the NCME, but also from their leading supporters.

to a more wider research program.

The NCME has received little attention by the historians of (macro)economic thought, except for Hoover (1988, Chap. VIII) and Kim (1988, Chap. 5). Our contribution aims indeed to complete, on this particular aspect, the narrative about the transformation of macroeconomics strating from the 1970s. Some recent contributions (let quote, among others, Duarte and Lima 2012; Backhouse and Boianovski 2013; De Vroey 2009, 2015), has tried to re-explore, in a critical perspective, what is often refereed as the "New Classical revolution". Historical research in this vein has rediscovered the tortuous paths of the transformation of the field, shone a light on blind alleys, emphasized crossroads and bifurcations in those decades of debates and trial-and-error attempts by competing research programs.[13] The emphasis we put here on the problems of NCME tries to contribute 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 to the opposite, the NCME program seems, in 1981, to have reached an aporia, contributing to the emergence of competitive empirical approaches, as the RBC and VAR models. Our claim is that this linked to the weaknesses of the L-twist (the L-trap).

In a nutshell, Lucas's program in the 1970s, strongly anchored in a methodological perspective, would challenged mainly the conception of models. Lucas was advocating for more theoretical a priori constraints in modeling (rational expectations, optimization in a general equilibrium framework), and claiming that such hypotheses are non refutable from empirical evidence. Although, in an instrumentalist perspective, Lucas claimed also that these constraints would not endanger 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. The positive side of the NCME applied some new econometrics techniques but in a quite traditional perspective: a macroeconometric model is useful as a "mediator" between theory and empirical world (Morgan and Morrison, 1999), especially in the perspective of discriminate between competing

[13] This perspective 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 those progresses. For a comprehensive discussion about the "naïve" history of recent macroeconomics, see Sergi (2015).

[14] An extensive discussion about the Lucas conception of modeling can be found in De Vroey (2011) and in Sergi (2014a).

theories. To the opposite, the prescriptive side of the NCME, following 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 researches.

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).[15] 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 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), possibly because no explicit methodological contribution can be found in Lucas's writings, both published and unpublished. Nevertheless, one can collect many relevant methodological statements in his work in the 1970-1980 period (especially Lucas 1977, 1980, 1981c, 1987; Lucas and Sargent 1981a,c) and moreover in some recent writings (Lucas 1988, 1996, 2001, 2004).[16]

Modeling practices are a main aspect of Lucas methodological contribution.[17] In his work in the 1970s and, more explicitly, in his 1980s papers, Lucas proposed a new conception of modeling for macroeconomics, which we

[15] This issue is addressed in Goutsmedt et al. (2015).

[16] In Sergi (2014a) we developed a more comprehensive description of the ideas on Lucas's methodology synthesized in here.

[17] We can even argue that, for Lucas, methodological matters are entirely modeling matters. To investigate his methodology means to investigate a particular conception of

are interested for here.[18] Lucas 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 neither econometrically. Although, theirs implications (e.g. the neutrality of money) can be tested.

In this section, we will develop in more details the Lucasian conception of models, or the "L-twist": an original methodological conception merging apriorism with instrumentalism. We will then discuss the challenges raised by this conception (the L-trap). The understanding of Lucas 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 laboratory in which (...) policies can be tested" (Lucas, 1980, p. 696). Indeed, Lucas postulates that modeling provides the only suitable method for expertise about policy-making:

models. For example, in Lucas 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).

[18] Then, we must leave aside what the exactly the "old" ("Keynesian") conception of modelling was. About this point, see for instance Vercelli (1991) and De Vroey (2015).

> (...) 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 perfom this fonction, 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 are 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, investments, 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 theirs characteristics are "common to all decentralized market economies" (*ibid.*). Indeed, the goal of macroeconomic modeling is to build an artificial world which imitates, by its dynamic, such regularities, i.e. which imitates the dynamics and the joint-dynamics (the co-movements) of the main aggregates macroeconomic time series.

The second problem is to define a standard procedure to evaluate quantitatively the consistence between artificial worlds (models) and real world phenomena (business cycles). According to Lucas, such a standard is that the two time series show the same statistical characteristics. In other words, one must observe the same co-movements in aggregate variables for the time

series generated by the model and for the real world past time series.[19] As relevant statistical characteristic, Lucas had in mind two features: the same counter-/pro-cyclical properties (same-sign co-variances) and the same amplitude in volatility. Lucas explicitly refers here to 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 sufficient condition for a model being a valuable laboratory for policy experiments. Lucas (1976)'s celebrated article (the Lucas critique) mades clear how drawn this distinction between artificial worlds that just "fit the historical data" (like Keynesians macroeconometric models) and artificial world serving as laboratory for experiment alternative policies. Models are effective in evaluating alternative policies, i.e. they can provide reliable predictions, if they integrate individual's dynamic responses to changes in economic policy. Those reactions cannot be invariant as in Keynesian models.[20] Lucas regarded indeed forecasting as "conditional forecastings", i.e. 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 experiment:

Indeed, the fundamental criteria for discriminating between competing models is, according to Lucas, the test about two aspects: a preliminary empirical test about the performance of models in reproducing past data (retrodiction) and a crucial test about the empirical consistency of their conditional forecasts of alternative economic policies.

How to build such a model? What kind of assumptions shall we use for construct our artificial economy? An explicit statement about the assump-

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

[20] See Appendix 2 for a recall about the Lucas Critique.

tions to be used for modeling can be found in Lucas (1980). It is worth quoting Lucas *in extenso* about this point:

> 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, p. 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" foundations for models is misleading: looking for a close relation between real world and model's mechanisms 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 for discriminate between different assumptions, no matter of the "realisticness" of the latter. Basically, we can resume this position as strongly instrumentalist view, accounting for scientific theories (or models) as merely forecasting devices, based on unrealistic assumptions and tested only on the basis of their predictions.[21] Note that this imply that the hypotheses are accepted to be valid instruments if their conclusions meet the empirical facts (the "as if" principle).

But are all assumptions admitted then? Not for Lucas, which, in the same time, advocates that only some very specific assumptions must adopted in macroeconomic models. In his understanding, only a precise set of hypotheses allows models to fulfill their function. Those specific assumption are the

[21] To the difference of 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).

optimizing behavior of economic agents, in a dynamic general equilibrium framework, joint with the hypothesis of rational expectation. The use of general equilibrium approach is advocated by Lucas as a necessary reunification of economics as a whole. 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 expectation are the second essential characteristic for insure 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 experience and, as a consequence, cannot be empirically refuted or tested: they are a priori necessary valid instruments.

We discussed above how Lucas 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 practices is supposed to consistently work.

2.2 Lucas empirical agenda and the L-trap

Lucas himself retrospectively describes 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 those articles illustrate Lucas's empirical agenda for putting to work his conception of modeling.

Econometric Testing of the Natural Rate Hypothesis (Lucas, 1972a) represents, according to Lucas itself (Lucas 2001, p. 21), a suggestion for an econometric test of the main conclusions drawn in Lucas (1972b). The model presented by Lucas focus 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 aim of the article is to propose 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 the complete simultaneous equation model, as presented in Appendix 3. Actually, Lucas (1972a)

[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.

is merely "a suggestion" of that procedure: Lucas does not carried up any actual econometric test of his model.

Some International Evidence on Output-Inflation Tradeoffs (Lucas, 1973) is an actual econometric testing of the Phillips curve on a sample of countries. Here, Lucas is testing again the 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 Phillips curve for countries with different levels of inflation (high, intermediate, low) and to show that the slope of the curve narrow one for the weaker level of inflation (the countries were the "monetary surprise" is rarer). 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's itself acknowledged later, the estimations procedure is incorrect (even if the results had been shown to be correct by further contributions).[24]

Those first attempts for constructing a model fulfilling the L-twist methodology described above seem indeed very unsatisfying. This what we can call the "'L-trap": the a priori hypotheses do not lead to 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 theirs predictions, is a failure. In the middle of the 1970s, Lucas himself must admit that no practical example illustrates the models he's 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)

[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).

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. To the opposite, the empirical strategies consist in keep trying, hoping that a day shall come, where the results of the a priori hypotheses will lead to successful empirical prediction.

Lucas expressed indeed a "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 settle in 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 to construct an ordered historical interpretation of the state of the NCME program at the publishing of Lucas and Sargent (1981c). It not provides neither an exhaustive, a detailed or a technical literature review of the NCME at this period: our purpose here is 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 we recalled in the introduction, the NCME had two purposes. The first was a positive goal: to bring empirical evidences to support the main theoretical result of Lucas (1972b), i.e. the neutrality of money and policy ineffectiveness (the "natural rate hypothesis"). The second was a prescriptive aim: 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

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

[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 classed the contributions in six "sections" following a very different logic, which is not worth discussing here (Lucas and Sargent, 1981b, pp. xvi-xxvi).

standard of Lucas, by providing some empirical tests of the *conclusions* of the models, by verifying if the latter can reproduce historical data and provide conditional forecasting. The non-refutation of the results corroborate the use of the a priori hypotheses: they are then valid instruments for forecasting (the "as if" principle). In a reverse perspective, the prescriptive side of the NCME tries to specify the implification 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 rigorous mathematical and theoretical development, but mainly as constructing operational models, which can be 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.[27]

3.1 Econometric tests of New Classical macroeconomic models

The NCME approach focus on the test of the main results in 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 in case of a "monetary surprise"). More concretely, the NCME tries to estimate the slope of the Phillips curve, to bring empirical evidence the curve is actually a vertical line (the "natural rate hypothesis").

The NCME insist on three errors made by Keynesians, which lead systematically to underestimate Phillips curve's slope: first, they assume that inflation is a highly serial correlated process; second, Keynesians use inflation lags as a proxy for expectations; third, they proceed only to single equation tests. The first error is mainly discussed in Sargent (1981c), pointing empirical evidences for inflation showing weak serial-correlation. The second error must of course 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). Inhere, the fundamental idea is that testing the sig-

[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.

nificance of various parameters (as in "Keynesian" models) is not a sufficient test: the crucial test must be about the *relationship* between parameters (see Appendix 3). 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 valid instruments.

The second implication of the neutrality is policy ineffectiveness: monetary policy cannot systematically influence output by increasing the money supply. Testing this proposition bring up a second econometric innovation: the use of the Granger-causality test in the wake of Granger (1969) and Sims (1981) (for a technical sum-up of this concept, see Appendix 4). 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 unemployment and output. The test in Barro (1981b) leads exactly to this empirical result, providing evidences that expected money supply growth does not Granger-cause 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, as Barro (1981a), discussed some specific conditions (namely, monetary authority has no informational advantage) for ineffectiveness of monetary policies to hold.

The same framework is applied to a different question by Hall (1981), following Muth (1981b). In his paper, Robert Hall test the life cycle-permanent income hypothesis of Friedman to conclude that "only unexpected changes in policy affect consumption" (Hall, 1981, p. 503). In a general way, those 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 we will not address here.

The positive side of the NCME program brings then some empirical evidences supporting the results of the a priori hypotheses of the Lucas way of modeling. The test of the conclusion, in this instrumentalist approach, is assumed to provide strong corroboration for New Classical models builded with optimizing agents and rational expectations. Sargent and Wallace formulate very precisely their understanding of the Lucas modeling methodology as the main guideline or 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: to edit 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 usefully formalized the problem as follows. The action u_t chosen by an economic agent (e.g. 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.[28] 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 and Sargent 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 reacts to changes in their environment, to the extent that $h_2 = f(z_{t-1}, \epsilon_t)$ or, simpler, 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.[29] NCME models, which

[28] 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.

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

"properly" specifies h as a function of f through the rational expectations hypothesis, are no more subject to the Lucas Critique, and, as a consequence, they are capable of producing consistent conditional forecasting of the effects of alternative economic policies.[30] Note that there is 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 summarized it, the NCME strategy for modeling "involves estimating agent's decision rules 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). The Granger-causality test is also a fundamental tool for determining the exogenous character of a variable, i.e. if it belongs to the set of stochastic processes. As in the previous section, we must distinguish here between different contributions to this problem.

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 innovation which are necessary to the NCME. Namely, a manuscript of Muth (Muth, 1981a), published here for the first time, 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 (but, this time, in a more "conscious" way) of the NCME approach 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. demand schedule shifts randomly). In the same vein, Sargent (1981d) tries to formulate and estimate an optimal decision rule for labor demand. Similar efforts are made by Sargent and Wallace (1981c) and Sargent and Wallace (1981c), in computing optimal monetary policy rules (in the perspective of debating the consistence of Friedmanian k-percent rules). To the opposite, Sargent (1981b) tries to define the stochastic processes faced by the agents (namely, the structure of the interest rates).[31] This computational effort for solving 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). Those two contribution quickly became the hallmark for formulating and estimating New Classical macroeconomic

[30] "After Keynesian Macroeconomics" (Lucas and Sargent, 1981a) extensively discuss this point.

[31] We must remark here that, on this prescriptive line of research, the work of Sargent represent a dominant influence on the NCME, both quantitatively and qualitatively.

models. They are very rich articles "technically" speaking, which means that Hansen and Sargent provides here a full, detailed range of the solution and estimation methods.

The issue of 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 specifying 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 errors 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]

This section aimed 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 to put emphasis on the difficulties of the NCME program, as they were admitted by its protagonists (and, to be clear, not as they were pointed out by their opponents). Again, we will not provide here

[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 (GMM) will be implemented only one year after the publication of the Lucas-Sargent volume Hansen (1982).

a detailed, technical sum-up of the problems for solving and estimating such macroeconomic models.[34] Our purpose is to enlighten the obstacle of the NCME, both in providing empirical support for their results and operational versions of their models. The perspective, similarly to the one adopted in 2.2, aims to emphasis the dilemma raised by the L-trap.

4.1 Do NCME provide sound empirical evidences?

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

Nevertheless, in this very same volume, much contributors stay very prudent, or even skeptical, on the current development of the program in respect of its positive side. 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)

Here, we use two specific examples presented in the Lucas-Sargent volume to nuance our previous statement.

The result of policy ineffectiveness, i.e. the fact that monetary policy cannot systematically increase output, except for the "monetary surprise" case, is not a so clear-cut result 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 put in debate the generality of the monetary neutrality result. The puzzling nature of those articles is that they follow the fundamental principles of New Classical modeling (optimization, rational expectations). Despite this commitment to the Lucasian

[34] Ingram (1995) still provided a synthetic account of those problems.

conception of models, they come to contradict the general result of policy ineffectiveness. Those refutations of policy neutrality result shall lead, in an instrumentalist understanding, to a refutation of the hypotheses (optimization, rational expectations) as valid instruments for modeling.

A more general and even more disturbing result is the "observational equivalence" emphasized by Sargent (1981g). In his article, Sargent shows that both "Keynesians" and New Classical rules for monetary policy can produce the very same kind of empirical evidences. 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 indeed the question of the importance of the apriorist methodology for macroeconometric modeling, because the refutation of the result by empirical evidence is not a sufficient test for rejecting the a priori hypotheses:

> 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 is here very well illustrating the bifurcation of the research paths caused by the L-trap. Instead of leading to a rejection of the a priori hypotheses, the unclear nature of empirical evidences lead to strengthen the theoretical a priori in modeling. Since optimization and expectations are the "right" hypotheses, if their implications are not corroborated by empirical tests, the latter must be rejected as not well-posed or not well-executed. As a consequence, macroeconometric modeling must move to more sophisticated procedures for testing, in order to corroborate 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 very 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 *in extenso*, 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)

Sargent is here 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 assumed, a priori, that the "true" result is the ineffectiveness of policy.

Our point here was to show how the controversial nature of the empirical evidences, brought by the positive side of the NCME, opens the way for a more prominent role of the prescriptive side. But can the latter 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 methodology for macroeconomic modeling claims that, to build a macroeconometric model, one must adopt two crucial assumptions about optimization and about expectations. In the previous section, we provided a brief synthesis of the methods implemented by the NCME (essentially under Sargent's lead). Here, we discuss some problems on those solutions and estimation strategies.

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. With a more critical perspective, one can understood the sense of these problems as the burden of the constraints imposed by Lucasian apriorism, 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; at the same time, the L-trap make impossible to ignore the defective empirical results obtained with these assumptions.

Identification is the first main issue flawing NCME approach. The main argument against "Keynesian" macroeconometric modelling is that it imposes a priori identification of the decisions rules (h_1 and h_2 in previous notation), especially by constraining expectations formation. As we suggested in the previous section, NCME claims to not impose such a restriction: instead, an NCME model must provide a fully derivation of those rules 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 objective functions "deep parameters" (Hansen and Sargent, 1981b, p. 150-151).[35]

Specification is the second problem of the NCME modeling practice. Again, the NCME attacked Keynesian macroeconometric model by pointing the non-sense of their a priori restriction over the form of the decisions 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 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 terms) is also a main requirement for bearing maximum likelihood methods: it leads also to impose some technical 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 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 will quite unsustainable for a complete, fully articulated model of the business cycle. The constraint is here 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 Ex-*

[35] Ingram (1995, p.21-22) provides a technical description of the problem.

pectations 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, 149).[36]

In this section we try to point out the main problems in the NCME program. We emphasized that, again, those difficulties arise from the L-trap. The vulnerability of the empirical evidences open the way for an internal debate about the further line of research. In the option of a more sophisticated empirical strategy, the NCME must deal with considerable problems of identification, specification and tractability. Those problems derives from the technical constraints imposed by the use optimization and rational expectations as the a priori hypotheses.

Conclusion

The article discussed how the methods and problems of the NCME, presented in *Rational Expectations and Econometric Practice*, can be understood at the light 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 latter 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.[37] 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 consist in keep going in the same direction, trying to get "sounder" evidences

[36] A more clear example of this willingness to simplify the NCME approach is "A Rational Expectations Approach to Macroeconometrics" (Mishkin, 1983). Inhere, 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 than the standard NCME approach, allowing then a less extensive computational and estimation effort.

[37] "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).

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 computational and econometric techniques, reproducing, in some aspects, the "bricolage techniques" of Keynesian macroeconometric modeling that NCME aimed to criticize. This way represent essentially the further development of the NCME after the period under our consideration. The alternative way to escape the L-trap is to reject the a priori assumptions, as advocated by the VAR models. A middle path can be found in calibration methods of the RBC, giving up estimation methods of the NCME and embracing more deeply the aprioristic constraint.

As we still emphasized in the introduction, our 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 carried up starting from the issues presented above, especially in the direction of an inner 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 Lucas Critique (Lucas, 1976)

Let s_t be a state vector of all the relevant variables describing an economy at time t (consumption, capital stock, etc.). The evolution of the system can be described by the function f, which is fixed and not directly known:

$$s_{t+1} = f[z_t, s_t, \epsilon_t]$$

where z_t is exogenous and non-stochastic vector (representing mainly the economic policies), ϵ_t a vector of random shocks (independent and identically distributed). The traditional econometric approach estimates f by pre-specifying a distribution function F and estimating a vector of fixed behavioral parameters θ :

$$s_{t+1} = F[\theta, z_t, s_t, \mu_t]$$

Once this equation has been estimated, econometricians can simulate the model for different i paths of policies ($\{x_i\}_t$) and quantitatively compare the different issues ($s_{t+1}|\{x_i\}_t$).

Lucas criticizes this approach by pointing the fact that the behavioral parameters in the vector θ are not fixed (so invariant for all $\{x_i\}_t$), but *a function of the individuals optimizing decisions rules* λ, which reacts to changes in z_t. This relation between government decisions and individual's decisions can be written as:

$$\lambda_t = G[s_t, z_t]$$

With G a known function . Then, the motion of the economy is actually described by the relation

$$s_{t+1} = F[\theta(\lambda), z_t, s_t, \mu_t]$$

and the econometric problem is to estimate the function $\theta(\lambda)$, which represent the dynamic optimal decision rule of economic agents.

Appendix 3

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 representing an aggregate supply function in the understanding of Lucas and Rapping (1969), equation 2 an aggregate demand function, 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 λ, 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 estimate writes 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:

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

Appendix 4

The Granger-causality approach

Granger (1969) proposed the following operational (testable) definition of a causality relation between two stationary time series.

Let $P_t(A|\overline{A})$ being the optimum, unbiased, linear least square predictor of the value at time t of a stationary stochastic process A using all past values of this process (noted $\overline{A}$). Let denote as $\epsilon_t(A|\overline{A})$ the predictive error of $P_t(A|\overline{A})$, and $\sigma^2(A|\overline{A})$ its variance.

For two stationary stochastic processes Y, X and for all available information U at time t, then one can say that Y causes the variable X if:

$$\sigma^2(X|\overline{U}) < \sigma^2(X|\overline{U-Y})$$

This definition suggests that one can test causality by observing 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.

One can spot a feedback relationship between X and Y (Y is causing X *and* X is causing Y) if :

$$\begin{aligned} \sigma^2(X|\overline{U}) &< \sigma^2(X|\overline{U-Y}) \\ \sigma^2(Y|\overline{U}) &< \sigma^2(Y|\overline{U-X}) \end{aligned}$$

Sims (1981) suggest to use the Granger definition of causality for testing unidirectional causality between two macroeconomic series X, Y. The test formulate two relationships, representing the best linear predictor of the current value of each variable, including exclusively lagged variables. A simple version with one lag can be written as:

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

By estimating those equations, one can conclude that X cause Y if $\widehat{\alpha_2} \neq 0$. The causality is unidirectional (no feedback of Y on X) if the $\widehat{\beta_2} = 0$. In this latter case, one can say that X is an "exogenous'" variable.

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