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► To cite this version:

Aurélien Goutsmedt, Erich Pinzon-Fuchs, Matthieu Renault, Francesco Sergi. Criticizing the Lucas Critique: Macroeconometricians' Response to Robert Lucas. 2016. halshs-01364814

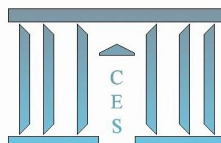
HAL Id: halshs-01364814

<https://shs.hal.science/halshs-01364814>

Preprint submitted on 12 Sep 2016

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Macroeconometricians' Response to Robert Lucas**

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2015.59



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5^{ème} Congrès de l'AFEP
Lyon, 1-3 Juillet 2015

Abstract

The standard history of macroeconomics considers Lucas (1976)–“the Lucas Critique”–as a path-breaking innovation for the discipline. According to this view Lucas’s article dismissed the traditional macroeconomic practice calling for new ways of conceiving the quantitative evaluation of economic policies. The Lucas Critique is considered, nowadays, as a fundamental principle of macroeconomic modeling (Woodford, 2003). The interpretation and the application of the Critique, however, represent still unsolved issues in economics (Chari et al., 2008). Even if the influence of Lucas’s contribution cannot be neglected, something seems to be missing in the narrative: the reactions of the economists that were directly targeted by the Critique. Modeling practices of economic policy evaluation were not overthrown immediately after Lucas (1976), creating a divide between theoretical and applied macroeconomics (Brayton et al., 1997).

The purpose of this paper is to study the reactions of the macroeconometricians criticized by Lucas. We focus especially on those macroeconometricians who worked on policy evaluation and who held an expertise position in governmental institutions. We categorize the

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different reactions to the Critique, in order to enrich the understanding of the evolution of modeling and expertise practices through the analysis of the debates—which have not yet been completely solved.

In the first section we propose a careful account of Lucas’s argument and of some of the previous works anticipating the substantial outline of the Critique (like Frisch’s notion of autonomy). Second, we bring our own interpretation of Lucas (1976). We think that we find two points of view in Lucas paper : a prescriptive one that tell you how to build a good macroeconometric model (it is the standard interpretation of the article); a positive one that relies on the fact that the Lucas critique could be seen as an attempt to explain a real-world phenomenon, the stagflation. Third, we classify the reactions of the Keynesian macroeconometricians following this line of interpretation. On the prescriptive side, the Keynesians protested against the New Classical solution to the Lucas critique (the use of the rational expectation hypothesis among other things). Klein, for instance, proposed an alternative microfoundational programme to study more empirically the formation of expectations. On the positive side, the Keynesians put into question the relevance of the Lucas Critique to explain the rise of both unemployment and inflation in the 1970s. They tried to test the impact of policy regime changes and of shifts in agents behaviour. According to us, in general, the explanation of the stagflation was elsewhere.

Keywords: History of macroeconomics, Keynesian economics, Lucas Critique, Macroeconometrics, Rational Expectations.

JEL Code: B22, B41, E60.

[Lucas 1976] had an extraordinary effect. Practising econometricians routinely make a bow in the direction of the “Lucas Critique” claiming either that it does not apply to their work or that they have taken care of the difficulties raised by Lucas. (Fischer, 1996, p.21)

Introduction

"Econometric Policy Evaluation: A Critique" (Lucas, 1976) represents without any doubt one of the most famous papers in macroeconomics. This paper is largely acknowledged as a turning point in the history of macroeconomic modeling and as a symbol of the disrupted period that the 1970s were for macroeconomics. This disruptive character of the 1970s did not only consist on the theoretical and methodological upheavals occurring inside the discipline. Disruptions would also stem from the convoluted macroeconomic context itself. During the 1970s, the inflation rate reached one of its highest levels in the United States, remaining always above 6 per cent (except for 1971) and reaching two peaks (12% in December 1974 and more than 14% in March 1980). At the same time, the unemployment rate steadily increased until it reached a peak following closely the inflation top (9% in January 1975).¹ Consequently, the Phillips Curve which had fitted so well until the end of the 1960s, displaying a *negative* relation between inflation and unemployment, became the major weakness of the Keynesian macroeconomic theory. In the data, the Phillips Curve seemed to have completely disappeared.

In his 1976 paper, Lucas claimed that the traditional Keynesian macroeconomic approach - Lucas targeted the Klein and Goldberger (1955) model - ignored that when economic policy changes, the structure of the relationships between economic variables shifts due to changes in agents' behavior (notably due to changes in their expectations). This would invalidate econometric inferences between past data and the forecasted effects of a new policy that is central to evaluate the impact of an economic policy.

Lucas's paper is supposed to have had a terrific impact on the Keynesian framework. For Preston Miller “the Lucas Critique was fatal and [so] new

¹ Source: economic data of the Federal Reserve Bank of Saint-Louis.

approaches had to be developed” (Miller, 1994, p.xv). Robert Hall underlined the revolutionary nature of the argument when it came to the moment of lauding Lucas’s work just after he was awarded the Nobel Prize:

The Lucas critique [...] has revolutionized the evaluation of policy, down to the most practical level in central banks and finance ministries. Policy evaluation procedures now routinely respect the dependence of private decision rules on the government’s policy rule. [...] Work on the Phillips Curve has been virtually abandoned, devastated by the theoretical and empirical force of the critique. Builders of large-scale models for the U.S. Federal Reserve and the IMF strive to address the Lucas critique.

(Hall, 1996, p.38)

Contemporary macroeconomics considers Lucas (1976) as a cornerstone of the discipline, as a fundamental problem one has to take into account in order to construct a consistent model.² Michael Woodford, in the introduction of his book - which represents today the emblem of the “New Neoclassical Synthesis” (Goodfriend and King, 1997) - describes the Lucas Critique as one of the two basic justifications for building microfounded macroeconomic models:

The development of a model of the monetary transmission mechanism with clear foundations in individual optimization is important, in our view, for two reasons. One is that it allows us to evaluate alternative monetary policies in a way that avoids the flaw in policy evaluation exercises using traditional Keynesian macroeconometric models stressed by Lucas (1976).

(Woodford, 2003, p.13)

The history of macroeconomics, told by the macroeconomists themselves, considers the Lucas Critique as a path-breaking innovation that immediately dismissed the traditional macroeconometric practice by force of the argument. Such an account of history is fundamentally linear, and considers macroeconomics as if it was only driven by scientific progress, as if it kept moving towards a better understanding of economic phenomena, and as if it was interspersed by breaking points. In this account of history, some articles or books are erected as major historical events and are supposed to have suddenly changed the way of doing macroeconomics.

² Even if the routine building of macroeconomic models with explicit microfoundations made the younger generation less aware of this legacy.

In the case of the Lucas Critique, the first flaw of this historiographical approach is that one could be led to think that, thanks to Lucas, the problem of ignoring the changes in structure had already been solved, and that models subject to the Lucas Critique have disappeared. Actually, strong debates around the application of the Lucas Critique remain. Standard features of contemporary dynamic stochastic general equilibrium (DSGE) models, as long-term wage contracting (of the type suggested by Fischer, 1977) and the Calvo index (Calvo, 1983) are constantly attacked in terms of their invariance to policy changes.³ Moreover, the formulation of the Critique and the proposition of a potential solution (the use of rational expectations) were not enough to build models which were robust with the Lucas Critique. Far from being an obvious issue, the realization and introduction of the Critique into the practices of macroeconometricians was a long and complicated process - as the building of the Liverpool model illustrates (Minford et al., 1984). Despite Hall's statement quoted above, some macroeconomic models that are not consistent with the Lucas Critique are still used today by institutions that provide economic policy recommendations.⁴

The second flaw of this historiographical approach is that the standard history of macroeconomics does not mention the reaction of the "victims" of Lucas's attack. And yet, these reactions definitively existed, and they were stormy! The formulation of the Lucas Critique is then a controversy of the classical form, characterized by an attack, and by some replies to this attack. We think that the study of scientific controversies is fundamental to make the history of the discipline and to understand current issues in macroeconomics. And so, we believe that, in order to build a more accurate history of macroeconomics, it is necessary to study the reactions of the Keynesian macroeconometricians facing Lucas (1976).

The purpose of our article is to build a typology of the reactions of the Keynesian macroeconometricians to the Lucas Critique in the years following its publication. This typology relies on our interpretation of "Econometric Policy Evaluation" both as a *positive* and as a *prescriptive* statement. We think that this duality in Lucas's paper, and then in the reactions to it,

³ The advanced textbook written by Minford and Peel (2002, chap. 6) gives a substantial place to the former question (following Barro 1977a), while Duarte Duarte (2011, p. 396, fn. 7) discusses the latter.

⁴ We have not led yet a systematic inquiry of the considerations for the Critique in the building of institutional macroeconomic models. But we know that the situation for France is particularly interesting, since the French Treasury uses two types of models. The MESANGE model which is rather a structural model with no explicit microfoundations (and so subject to the Lucas Critique) and the EGEE model, which is a standard DSGE model.

enables to explain the success of Lucas's 1976 paper. The common path of interpretation is, of course, the *prescriptive* one: Lucas's article represents a methodological norm, a rule for modeling, a prescription for preventing macroeconometricians from ignoring the reaction of agents facing economic policies. Lucas (1976) can be interpreted as paving the way to avoid this problem. We will show, however, that Lucas's paper could also be interpreted in a *positive* way, as an effort to understanding the stagflation situation in the U.S.⁵ In this case, changes in agents' behavior would then provide an explanation about the cause of a real world phenomenon. We think that the reactions to Lucas (1976) can be better understood by enlightening the Lucas Critique *imbroglio*, i.e. the ambiguity of that positive and prescriptive statement. This interpretation allows us to adopt a broader point of view on the reception of the Lucas Critique and a more structured overview of the Keynesian replies.

To illustrate our argument, we have tried to build a large corpus, allowing for the identification of the reactions in the ten years following the publication of "Econometric Policy Evaluation". Our point of departure was the systematic inquiry of Ericsson and Irons (1995). Their aim was to look at the papers that tried to empirically test the validity of the Lucas Critique. In order to do so, they built a database with all the articles citing Lucas (1976). They used different categories to classify the papers (is it a theoretical, an empirical or a mixed contribution? Is the reference to Lucas substantial, tangential or does the paper just postulate the validity of the Lucas Critique?). With the typology of Ericsson and Irons, we have tried to select the most promising papers that would allow us to find substantial discussions about Lucas. We also searched for other sources of evidence in books (the research of Ericsson and Irons only focused on journal articles) and also in interviews. We looked at the proceedings of some macroeconomic conferences (for instance, the "Rational expectations and economic policy" conference held at the NBER in October 1978 or the conference held at the Federal Reserve Bank of Boston, in June 1978, called "After the Phillips Curve: Persistence of High Inflation and High Unemployment"). There is no doubt that we have missed some important elements and that our bibliography is far from being exhaustive. We think that further research on proceedings could be very fruitful, especially as it allows to study controversies in their most direct and informal forms (notably through the "discussion" and "reply" sessions of the conferences). Nevertheless, we consider that our typology is exhaustive, and

⁵ This is what Snowdon (2007, pp.547-548) called the "ideas hypothesis", which states that an inappropriate theoretical framework has led to bad economic policies that have increased inflation.

so, it is flexible enough to easily taking into account any new contribution.

The article is structured as follows. Section 1 proposes first a brief recall about the formal content of Lucas (1976), and a discussion of this article as a mainly prescriptive argument. We then place the Lucas Critique in relation to the history of econometrics, especially to Ragnar Frisch's notion of autonomy. We show here that the prescriptive side of the Lucas Critique is nothing but a well-known problem in macroeconomic modeling. Section 2 reconstructs the origins of the substantial argument of Lucas (1976) in Lucas's earlier writings. We find that the argument was already present in earlier works, but that it was embedded within a mainly positive program on the form of the Phillips Curve. We conclude that the originality (and the success) of "Econometric Policy Evaluation" can be explained by an *imbroglio*, an entanglement between a prescriptive and a positive side of the argument. Section 3 synthesizes the reactions against the prescriptive side of the Lucas Critique. We try to take into account the heterogeneous efforts for handling the problem emphasized by Lucas in an "innovative" way, both by New Classical macroeconomics and by other authors, especially by Lawrence R. Klein. Section 4 presents the Lucas Critique as a contextual, positive argument about the understanding of the 1970s stagflation. The critical responses of macroeconomists (like Alan Blinder, Lawrence R. Klein or Otto Eckstein) about this positive side of the Critique are discussed. Relying on our previous analysis, the conclusion provides some methodological insights on the New Classical revolution in macroeconomics and proposes some further lines of research.

1 The Lucas Critique: no new prescription under the sun of macroeconomic modeling

In this section we first propose a comprehensive description of "Econometric Policy Evaluation"; here, we emphasize the prescriptive dimension of Lucas's arguments. We intend to recall that the substantial prescriptive contents of Lucas (1976) can be traced back to older debates among econometricians, at the dawn of the discipline. Thus, from a historical point of view, "Econometric Policy Evaluation", in its mainly prescriptive dimension, is not as path-breaking and not as original as claimed by the standard account in the history of macroeconomics. In other words, we show that the problem tackled by Lucas (1976) had been previously emphasized by other

authors, especially by Frisch in his own “critique” of Tinbergen’s work.⁶

1.1 What is the Lucas Critique?

“Econometric Policy Evaluation: A Critique” explicitly aims, first of all, at criticizing, in a clearly “destructive” (Lucas, 1976, p. 41) perspective, the mainstream econometric approach in macroeconomics (“the theory of economic policy”), claiming that this approach “is in need of major revision.” (*ibid.*, p. 20).⁷ Lucas argued that he would possess the main arguments that provide the bases for such a revision.

The argument was directed against the possibility for traditional macroeconomic models to correctly state (or to quantitatively predict) the effects of alternative economic policies.⁸ Lucas introduced the problem in the following way:

These contentions [in “in the theory of economic policy”] will be based not on deviations between estimated and “true” structure prior to a policy change but on the deviation between the prior “true” structure and the “true” structure prevailing afterwards.

(Lucas, 1976, p. 20)

Model parameters estimated on past data, which are determined by a previous economic policy, are no longer correct if the economic policy changes: in one word, a correctly specified model cannot include decision rules that are *invariant* of the economic policies. The mechanism underlying the variation of parameters is the individual behavior (the rules governing individual decisions), which take into account economic policies and so change along with the policy regimes. Indeed, Lucas (1976) essentially consists on a prescriptive statement about the way of modeling that would produce a sound quantitative evaluation of the distinct effects of alternative economic policies: it clearly states what modelers must avoid (considering the parameters invariant with respect to changes in policies) and the alternative way to go (taking into account the parameters drifts in response to changes in policies).

⁶ Aldrich (1989) has already made this point.

⁷ Lucas explicitly criticized Klein and Goldberger (1955) (Lucas, 1976, p. 19, fn. 2) and Tinbergen (1952) (*ibid.*, p. 21) and he did not address any direct critique to any model that would be more recent than that.

⁸ Nevertheless, Lucas conceded that these models can generally forecast well in the short run: “[...] I shall argue that the features which lead to success in short-term forecasting are unrelated to quantitative policy evaluation, that the major econometric models are (well) designed to perform the former task only [...]” (*ibid.*, p. 20, Lucas’s emphasis).

Lucas Critique is traditionally formalized as follows.⁹ 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:

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

where z_t is a vector of exogenous and arbitrary (non-stochastic) variables, representing the “environment” of the economic agents, ϵ_t a vector of random shocks (independent and identically distributed). The main interesting feature in the environment z_t are obviously economic policy decisions. The traditional econometric approach estimates f by pre-specifying a distribution function F and by estimating a vector of fixed behavioral parameters θ :

$$s_{t+1} = F[\theta, z_t, s_t, \mu_t] \quad (2)$$

Once equation (2) has been estimated, econometricians can simulate the model for different i paths of policies ($\{z_i\}_t$) and they can quantitatively compare the different situations ($s_{t+1} | \{z_i\}_t$).

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

$$\lambda = G[s_t, z_t] \quad (3)$$

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

$$s_{t+1} = F[\theta(\lambda), z_t, s_t, \mu_t] \quad (4)$$

and the econometric problem is to formalize and to estimate the function $\theta(\lambda)$, which is then the “major revision” for which Lucas is calling for here.

According to Lucas, the specification of such a relationship must deal with two questions (to which “Econometric Policy Evaluation” only alludes):

⁹ Lucas (1976) presents both a general discussion of this idea (sections 2 to 4, section 6), which is detailed here, and three precise examples (sub-sections 5.1 to 5.3). The first example discusses Friedman (1957) permanent income hypothesis, in a similar vein of the discussion given by Muth (1960). The second example is borrowed from Lucas and Prescott (1971). The third example, i.e. the specific application of the Lucas Critique to the determination of the Phillips curve, is reported here in the Appendix.

the description of the optimizing behavior of the economic agents and the description of the way these agents form their expectations about the future. According to Lucas, the former question is not problematic since economic theory, and especially general equilibrium theory, would know how to deal with it. On the contrary, the question about the formation of expectations can be solved (i.e., $\theta(\lambda)$ can be specified and estimated), only if changes in policies consist in changes in rules. In this case, one must use expectations that are formed rationally in the sense of Muth (1961).¹⁰ Lucas precises that:

[this principle] does not attribute to agents unnatural powers of instantly divining the true structure of policies affecting them. More modestly, it asserts that agents' responses become predictable to outside observers only when there can be some confidence that agents and observers share a common view [...]

(Lucas, 1976, p. 41)

The force of the perspective argument of “Econometric Policy Evaluation” resides both in the criticism itself and in the explicit formulation of a new principle for macroeconometric modeling activity.¹¹

1.2 The Lucas Critique before Lucas

Lucas himself claimed that “there is little in this essay which is not implicit (and perhaps to more discerning readers, explicit) in Friedman (1957), Muth (1961) and, still earlier, in Knight (1921)” (Lucas 1976, p. 258) and suggests to the reader to “see in particular Marschak’s discussion in Marschak (1953) [...] and Tinbergen’s in Tinbergen (1956), especially his discussion of qualitative policy” (*ibid.*, fn. 3). The work of Friedman (1957) provided a specific example for Lucas (1976, sect. 5.1.); Muth (1961) and Knight (1921) provide theoretical inspiration for the solution of the Critique, especially in terms of the specification of expectations (*ibid.*, p. 41).

¹⁰ For recall, expectations are rational if the subjective probability distribution (the expectation of economic agents) equals the objective probability distribution. The expectation at time $t - 1$ of the value of a variable x at time t equals then the expected value of x , conditionally to the set of available past information Ω_{t-1} :

$$x_t^e(t-1) = \mathbb{E}_{t-1}(x_t | \Omega_{t-1}) \quad (5)$$

A weaker form of rational expectations (which made explicit the stochastic character of x) allows for an error term, so that expectations write $x_t^e(t-1) = \mathbb{E}_{t-1}(x_t | \Omega_{t-1}) + \mu_t$. μ_t is uncorrelated with Ω_{t-1} , so that there is neither perfect foresight nor systematic bias in the information process.

¹¹ The latter will be discussed in subsection 3.1.

Marschak (1953) and Tinbergen (1956) constitute more interesting references¹², as they emphasized the problem from the perspective of econometric practice and, more specifically, from the perspective of policy advising. Although, even if econometricians were aware of the fundamental problem raised later by Lucas, both Marschak and Tinbergen rejected the idea that this problem would be relevant for macroeconomic modeling, i.e. they rejected the problem from a prescriptive point of view.

Marschak, in his contribution to the 14th *Cowles Commission Monograph*, addressed the problem of the useful knowledge that had to be produced by economists/econometricians to help policy-makers “to make the best decisions”. In this article, Marschak clearly stated the same kind of problems emphasized later by Lucas:

In predicting the effect of its decisions (policies) the government thus has to take account of exogenous variables, whether controlled by it (the decisions themselves, if they are exogenous variables) or uncontrolled (e.g. weather), and of structural changes¹³, whether controlled by it (the decisions themselves, if they change the structure) or uncontrolled (e.g. sudden changes in people’s attitude).

(Marschak, 1953, p. 8)

Following Marschak’s understanding, changes in the structure are not systematic. In particular, Marschak (Marschak, 1953, p. 25) suggested that very few changes in policy-making could change the structure. In this case, econometricians “just” need to derive the new structure after observation of new data, the knowledge of the old structure and the knowledge of past changes in the old structure being an essential informational source.

In his 1956 book *Economic Policy: Principles and Design* Tinbergen also addressed the problem of producing valuable expertise for policy-makers through econometric modeling. In Chapter 5 “Qualitative policy: changing the structure within given foundation” (explicitly quoted in Lucas, 1976), Tinbergen discussed the situation “in which the structure of the economy is changed” (Tinbergen 1956, p. 149) as, for instance, through a change in the “pricing scheme” (*ibid.*, p. 161), which is exactly the same kind of example used by Lucas in his “Econometric Policy Evaluation” (see the Appendix). According to Tinbergen, shifts in the economic structure are “less frequent” and “to be seen as long-term policies” (*ibid.*, p. 149): these changes, however, when effective, call for a “methodological change” in the way of evaluating alternative economic policies:

¹² Haavelmo (1944, p. 27) also provides a similar point of view.

¹³ Here, changes in the form of economic relationships between variables.

In principle this investigation [about qualitative policy] will mean that each time a comparison is made between two states of the economy: the original state and the situation created by the structural change considered. [...] Such an investigation will only be possible if we know how the economy behaves in the new situation [...]. The characteristics of quantitative policy, just discussed, make it somewhat premature to deal with the problems of such policy in the way chosen for the treatment of quantitative policy.

(Tinbergen, 1956, p. 151-153)

These prescriptive conclusions of Tinbergen about macroeconomic modeling for policy evaluation had already been subject to harsh criticisms long before Lucas, since the 1930s. In 1938, Ragnar Frisch wrote a review of Tinbergen's 1939 book *Statistical Testing of Business-Cycle Theories*, where Frisch accused Tinbergen of not having discovered *autonomous relations*, but *coflux relations*.¹⁴ Frisch provided indeed the first systematic exposition of the econometric problem of structural invariance, which was emphasized by Marschak, Tinbergen and, finally, Lucas. Moreover, one can claim that the prescriptive side of the Lucas Critique is already explicit in "Frisch's Critique" of Tinbergen.

In his seminal contributions to the econometric research program (Frisch, 1933, 1934, 1938), Frisch clearly stated that econometrics would be concerned with two alternative approaches. On the one hand, econometrics would be concerned with the analysis of *autonomous* economic relationships that would be discovered by means of structural estimation methods. This first approach would provide the way of discovering the more "fundamental" equations that would constitute the "essence of theory". On the other hand, econometrics would be concerned with the analysis of non-autonomous relationships, which would consist on the *confluence analysis* method. This method of confluence analysis would be concerned with the analysis of regressions where more than one linear relation connects the variables in questions (Bjerkholt, 2005), i.e., when the degree of autonomy of the relations is very low. The distinction between autonomous and non-autonomous economic relationships in Frisch's understanding has to be understood as a matter of degree. Some economic relations can be more or less autonomous than other, and it is only those with a higher degree of autonomy which would be invariant to economic

¹⁴ As Morgan (1990) explains, Tinbergen's first volume was circulating in mimeographed form in 1938 before its official publication, and it was "evaluated at a special conference in Cambridge (England) in July in that year". Frisch could not attend to the conference, but he sent his memorandum, which arrived after the event (*ibid.*). This memorandum was published for the first time in Hendry and Morgan (1995).

policy changes. Yet, these relations with higher degrees of autonomy are more difficult to be observed by the econometrician or economist.¹⁵

According to Frisch (1938), a system of equations (representing the economic relationships) is an autonomous system if one equation (or its parameters) can change without causing any changes in the other equations of the system (and in their parameters).¹⁶ Then, the main property implied by Frisch’s definition of autonomy is that an autonomous system of equations can be used for policy-simulation purposes. Autonomous systems would be very difficult to discover and to estimate according to Frisch. In the case of Tinbergen’s macroeconomic modeling program, and in particular in the case of Tinbergen (1939) League of Nations work, Frisch (1938) remained pessimistic about the fact that Tinbergen was not dealing with autonomous relations but with coflux relations. In a similar vein of Keynes (1939) criticism to Tinbergen, Frisch (1938) clearly stated that “it is only coflux relations that are determined by Tinbergen, and the lack of agreement between these equations and those of pure theory cannot be taken as a refutation of the latter”.¹⁷ These coflux equations would be irreducible to the most fundamental equations, since these would contain a minor degree of autonomy.

This brief account of the prescriptive side of the Lucas Critique shows that the problem emphasized by Lucas was well known by most of the “founding fathers” of the econometric program, and was explicitly discussed and formulated. What is interesting, indeed, is that this fundamental problem was put aside at some point, and forgotten especially by some econometric practitioners in the subsequent decades.¹⁸ Then, the interesting question for the reception of the “Econometric policy evaluation” is not so much about the “path-breaking” or the “original” character of Lucas’s work; rather, it is about the reasons that made possible to re-open the discussion about the invariance of the structure of the macroeconomic models.

2 The Lucas Critique *imbroglio*

In the previous section we argued that the success of the “Lucas Critique” cannot be rationalized as a prescriptive methodological advancement. Conversely, we consider here that this success within macroeconomics owes much

¹⁵ See Boumans (2010) for a detailed discussion of this problem.

¹⁶ See Aldrich (1989) for a formal account of the notion of autonomy.

¹⁷ Keynes (1939) main criticism was about the impossibility of testing economic theories by means of econometrics.

¹⁸ For a comprehensive discussion of the disappearing of autonomy, see Qin (2014).

to the ambiguity of Lucas's paper. Indeed, Lucas's paper is not restricted to display the Critique in its narrow sense, but it appears as embedded within two other proposals: the Rational Expectations Hypothesis, now REH (section 2.1), and a positive explanation of the economic crisis of the 1970s (section 2.2). This ambiguity is what we call the Lucas Critique *imbroglio*.

2.1 The Lucas Critique and its relation to REH

Strictly considered, the Lucas Critique implies that standard macro-econometric models cannot potentially compare effects of alternative policy rules since the parameters are not deeply structural (i.e. policy invariant). In essence, it is a negative result because it instructs how not to do macro-econometric modeling. Thus, from a methodological point of view, the purpose of the Lucas Critique is basically prescriptive. It raises an internal criticism towards standard macro-econometric models, which can be reduced to a "single syllogism" as Lucas pointed out at the very end of his article:

This essay has been devoted to an exposition and elaboration of a single syllogism: given that structure of an econometric model consists of optimal decision rules of economic agents, and that optimal decision rules vary systematically with changes in the structure of series relevant to the decision maker, it follows that any change in policy will systematically alter the structure of econometric models.

(Lucas 1976, p. 41)

Now, once the issue raised (or, recalled), the following step was to determine to what extent standard macro-econometric models were able to handle with it. On that point, Lucas's paper is still very moderate, and it just suggests that the issue can be easily grasped. But, even if Lucas finally doubted that his arguments would be grasped so easily, he indicated, in the end, that it was just a matter of subjective appraisal.

These points are, I think, important, but their implications for the future are unclear. After all, the major econometric models are still in their first, highly successful, decade. No one, surely, expected the initial parameterizations of these models to stand forever, even under the most optimistic view of the stability of the unknown, underlying structure. Perhaps the adaptive character of this early stage of macro-economic forecasting is merely the initial groping for the true structure which, however ignored in statistical theory, all practitioners knew to be necessary. If so, the arguments of this paper are transitory debating points, obsolete soon after they are written down. *Personally, I would not be sorry if this were the case, but I do not believe it is.*

(Lucas 1976, p. 24, our emphasis)

And the way Lucas justified his own skepticism is, indeed, highly subjective. Basically, Lucas relied on his conviction that agents' expectations are nothing but rational, i.e. that they are neither static (Lucas 1976, p. 25) nor adaptive in the long run (*ibid.*, p. 26). Lucas firstly, and surreptitiously, introduced Muth's concept when he discussed the case of the consumption function (*ibid.*, p. 27). But he still referred a second time to this concept in the section 6 ("Policy considerations") where he claimed that a sluggish change of θ is both false and misleading (*ibid.*, p. 39) and that a stable $\theta(\lambda)$ can emerge only if "policy changes occur as fully discussed and understood changes in *rules*", which may be if rationally expected (*ibid.*, p. 41). Thus, despite the fact that the Lucas Critique does not inevitably imply the REH (since it is a potential solution and not a necessary implication of the Critique), the association between the Critique and the REH is highly suggested in this paper.

Though, the reason for such an association is not mysterious at all. It simply appeared natural to Lucas, who actually never disentangled his Critique from his own research program based on REH. Indeed, from an analytical as well as an historical point of view, we even claim that it is not possible to disentangle the content of this article from Lucas's previous works. In order to capture this idea, we need to recall the genealogy of this article that became to be quoted as "the Lucas Critique".

Even if "Econometric Policy Evaluation: A Critique" was published in 1976, Lucas actually had already completed this work three years before, in early April 1973.¹⁹ The first draft of the paper was presented on April 20, 1973 at the University of Rochester, during one of the first *Carnegie-Rochester Conference on Public Policy*. The main subject of that session dealt with the Phillips Curve; Karl Brunner, as the organizer of the conference, had asked Lucas for "a survey of the empirical evidence on the Phillips curve" (King 2003, p. 249). After some modifications on May 1973²⁰, Lucas published this version of the paper as a Carnegie Mellon working paper

¹⁹ According to Sargent's testimony: "On a Friday early in April 1973, I organized a small conference on rational expectations at Ford Hall at the University of Minnesota. On Saturday morning, I received a phone call from Rita Lucas relaying a request from Bob, who was playing baseball, that I return to Ford Hall to search for an important folder Bob had misplaced. I found a file containing a handwritten draft of 'Econometric Policy Evaluation' and mailed it to Bob". (Sargent, 1996, p. 539, footnote 3, our emphasis).

²⁰ In the acknowledgments for this second version (May 1973), Lucas thanks Edward Prescott for his commentary during the Carnegie-Rochester conference, which brought Lucas to substantially modifying his April 1973 first draft. For further analysis on the *variorum* of "Econometric Policy Evaluation: A Critique", see Young (2014).

(Lucas, 1973a). The 1973 working paper is essentially the same version as the better known version of 1976, published in the *Carnegie-Rochester Conference on Public Policy* proceedings (Lucas, 1976). Hence, the substantial content of Lucas's Critique was circulating since 1973, and it was even quoted by some authors (Cooley and Prescott, 1976; Sargent, 1976). Thus, it is not surprising that the intellectual genesis of the Lucas Critique can be easily identified in Lucas's previous works, especially in Lucas (1972a) and in Lucas (1972b). These two contributions were written (very likely) just before the first draft of Lucas (1976).

"Expectations and the Neutrality of Money" (Lucas, 1972b) is commonly designated as the cornerstone of the New Classical macroeconomics and as one of the most commented contributions by Lucas. Even Lucas himself considered it as his most influential work (Snowdon and Vane, 2005, p. 200).²¹ The main result of this model is that random changes in agent's environment (i.e. random changes in monetary policy, or "monetary surprise") impact agent's behavior, and so the aggregates (i.e. the level of output), if and only if these changes are not correctly anticipated by the agents. Here, despite the fact that agents are maximizing their utility function and that they are forming their expectations rationally (in Muth's sense), changes in the environment are not correctly anticipated because the information is incomplete. Indeed, agents living in different "island-markets" confuse variations in nominal prices with variations in relative prices (the so-called "signal-extraction problem"). Under this unique condition, the traditional Phillips curve is said to hold; otherwise, only the "natural rate hypothesis" holds. There is no doubt that the core of this model anticipates the substantial content of Lucas (1976), in which a special emphasis is also made on the "Phillips curve" (see the Appendix).

Furthermore, Lucas (1972a) article about "Econometric Testing of the Natural Rate Hypothesis" suggests a new econometric research path for testing these two alternative versions of the inflation-output relationship (the standard Phillips curve and the "natural rate hypothesis").²² Hence, Lucas (1972a) is a logical step forward in respect of Lucas (1972b)²³, since the

²¹ Although, in a bibliometric sense at least, "Econometric Policy Evaluation", as well as Lucas (1978) are far more quoted and commented (Andrada, 2014).

²² Lucas (1973b) is also concerned with the testing of the natural rate hypothesis across a sample of countries.

²³ As Lucas wrote himself, "my plan was to translate what I had learned from writing "Expectations and the Neutrality of Money" into linear examples that would make it clear to a much wider readership why the standard tests for "long run" effects [...] were not informative about the Friedman-Phelps hypothesis. This paper became "Econometric Testing of the Natural Rate Hypothesis"." (Lucas, 2001, p. 21)

econometric question is now brought to the fore. In this article, Lucas anticipates more clearly the content of the Lucas Critique:

A once-and-for-all move to a new, fixed demand level implies a change in the policy *parameters* [...]. This policy cannot be evaluated by simply summing parameters implied by some previous, now irrelevant policy.

(Lucas, 1972a, p. 99, Lucas's emphasis)

So, it would be wrong to consider the Lucas Critique as being autonomous from Lucas's previous works, especially autonomous from Lucas (1972a) and Lucas (1972b); or, even, from his wider effort to build an alternative research program in macroeconomics, based on the REH.²⁴ Here is the first ambiguity of Lucas (1976), namely to suggest (or to allow to assume) that the Lucas Critique implies the REH, either as the cause or as the (unique) solution for that issue. We saw above that such an association is present in Lucas (1976) but still, knowing Lucas's previous works, can legitimately stand for a valuable interpretation of the Lucas Critique. From that perspective, it is no surprise that most of commentators interpreted the REH as a consubstantial part of the Lucas Critique. Even inside the New Classical macroeconomics approach, the confusion is often made (see a.o. Turnovsky, 1984, Rossiter, 1985, Jung, 1986 and even Sargent, 1996).²⁵ Now, let us turn to the second ambiguity of Lucas (1976).

2.2 The Lucas Critique and its positive scope

In its narrow sense, the Lucas Critique implies that standard macro-econometric modeling failed to compare the effects of alternative policy rules since it did not take into account the changes in the agents' behavior in

²⁴ The agenda of the New Classical Macroeconomics's research program will be formulated later, in its most clearly way by Lucas and Sargent (1979). Following other historians of economic thought (Duarte and Lima, 2012; De Vroey, 2009; Vercelli, 1991; Hoover, 1988), we analyze the Lucasian research program (then resumed by the label "New Classical macroeconomics") as an alternative scientific paradigm to "Keynesian macroeconomics" in virtue of its several shifts: a shift in the object of macroeconomics (from unemployment to business cycles); a shift in the way of conceiving expectations (from backward-looking expectations to RE); a shift in the theoretical framework (from a Marshallian to a Walrasian basis) and in the scope of the equilibrium concept (from a static to an intertemporal equilibrium); then, a shift in the results on the efficiency of economic policies. For a more extensive comparison, see De Vroey (2015).

²⁵ Symptomatically, this latter stated: "the rational expectations critique of simulations is attributed to Lucas (1976)" (Rossiter, 1985, p. 324).

response to a change in economic policy. Again, “Econometric Policy Evaluation” is primarily concerned with a prescriptive motive about how to provide sound econometric policy evaluation, or how to improve it. It does not go further than that. Lucas made the effort of well defining the prescriptive scope of his Critique of standard macro-econometric models - notably, the Critique does not apply to short-term forecasting:

For the question of the short-term forecasting, [...] we have seen that this conclusion is of only occasional significance. For issues involving policy evaluation, in contrast, it is fundamental; for it implies that comparisons of the effects of alternative policy rules using current macroeconomic models are invalid regardless of the performance of these models over the sample period or in ex ante short-term forecasting. The argument is, in part, destructive: the ability to forecast the consequences of "arbitrary", unannounced sequences of policy decisions, currently claimed (at least implicitly) by the theory of economic policy, appears to be beyond the capability not only of the current-generation models, but of conceivable future as well.

(Lucas, 1976, p. 41-42)

Now, once the major (or unique?) purpose of Lucas (1976) is recalled, it appears weird to wonder about the positive scope of the Lucas Critique. Though, regarding the economic context in the first years of the 1970s, there is no doubt that Lucas was concerned about something more. In a nutshell, while inflation and unemployment were constantly rising, the stabilization policies turned out to be ineffective to counter a phenomenon (the so-called “stagflation”) that seemed to refute the received Keynesian theoretical view at the time - namely, the hypothesis of a trade-off between inflation and unemployment based on the Phillips curve.

Though, and rather surprisingly, Lucas’s paper remains very distant from current economic policy considerations. After some words in introduction on stabilization policies and on the possibility to maintain permanently a high rate of inflation to keep a low rate of unemployment (Lucas, 1976, p. 19), no more mention is made of the economic context. No matter the economic crisis in the 1970s, the stagflation and its plausible causes (like the rising price of oil), Lucas’s paper is only concerned with the methodology of macroeconomic modeling. The basic purpose is, though, to show that this methodology is flawed. From here, it just takes one step to interpret the Lucas Critique as aiming at making the Keynesian policy-advisers responsible for stagflation (especially G. Ackley and A. Okun, presidents of the Council of Economic Advisers in the late 1960s).²⁶

²⁶ Muchlinski (1999) still analyzed this issue.

Actually, this step will be reinforced a few years later by Lucas, in a paper co-written with Thomas Sargent (Lucas and Sargent, 1979) still famous for its provocative words against the Keynesian paradigm. More substantially, this paper provided the opportunity for Lucas to extend the scope of his Critique in a more positive direction. Now, and combined with the claim that Keynesian macro-econometric models lacked some clear and consistent specification of the dynamic individual behavior, the Lucas Critique led to accuse Keynesian models for having underestimated the role of optimizing behavior and expectations when new economic policies were implemented. From that perspective, according to Lucas and Sargent the early 1970s have represented a key test for the relevance of Keynesian models:

A key element in all Keynesian models is a “tradeoff” between inflation and real output: the higher is the inflation rate, the higher is output (or equivalently, the lower is the rate of unemployment). For example, the models of the late 1960s predicted a sustained unemployment rate in the United States of 4 percent as consistent with a 4 percent annual rate of inflation. Many economists at that time urged a deliberate policy of inflation on the basis of this prediction. Certainly the erratic “fits and starts” character of actual U.S. policy in the 1970s cannot be attributed to recommendations based on Keynesian models, but the inflationary bias on average of monetary and fiscal policy in this period should, according to all of these models, have produced the lowest average unemployment rates for any decade since the 1940s. In fact, as we know, they produced the highest unemployment since the 1930s. This was econometric failure on a grand scale.

(Lucas and Sargent, 1979, p.6)

As we can see above, even if Lucas and Sargent extended definitely the Lucas Critique in a positive way, they also confined themselves to what we call the “weak thesis”. Indeed, they refused to attribute the failure of 1970s economic policies to Keynesian macroeconometric models. Their point is surely to stress the failure of U.S. expansive fiscal and monetary policies and of the inconsistencies of Keynesian models, but they do not let suppose that these inconsistencies (in particular the mistaken reactions of the agents to changes in economic policy) are (even, partly) responsible for the bad results of the policies implemented in the early 1970s.²⁷

²⁷ Such an ambiguity can also be found about the policy-ineffectiveness proposition. Despite the fact that New-Classicals never tried to draw a clear cut between their theoretical models and their normative conclusions (see, for instance, the claim of Sargent in Klammer 1984, p.70), it is not a coincidence that some economists (for example, Tobin (1981)) regarded the New-Classical Revolution as the second wave of a more general Monetarist Revolution (on the legitimacy of such a link, see Hoover (1988), chapter 9).

But here again, beyond the circumlocutions of Lucas and Sargent, it is not surprising that a “strong thesis” has also been developed for giving an extended positive scope to the Lucas Critique. This is even less surprising if one consider, as we did in the first part of this section, the close relation between Lucas (1976) and the immediately preceding works of Lucas about the neutrality of money (Lucas, 1972b,a). The stronger version of the Lucas Critique thesis implies doing a step further in supposing that defective Keynesian models have amplified the economic troubles of the 1970s (in particular the “great inflation”). Such an interpretation must have been sufficiently wide-spread in macroeconomics to remain so intact after decades, so that its clearest expression can be found in Snowdon (2007):

[T]here are several plausible explanations of the emergence of the “Great Inflation”. One persuasive explanation is the “Idea Hypothesis”. This hypothesis emphasizes a number of policy errors that had their origins in a mainstream acceptance of a defective Keynesian theoretical framework that encouraged monetary policy to become “unusually prone to creating volatility during the late 1960s and the 1970s” (citing Bernanke 2004). This framework downplayed the importance of inflation relative to unemployment, neglected the influence of endogenously determined inflationary expectations, displayed undue optimism about the ability of policymakers to use both fiscal and monetary policy to “fine-tune” the economy, gave too much weight to non-monetary forces (cost-push shocks) as contributing factors to rising inflation while neglecting the influence of monetary factors, and accepted unrealistically low estimates of the sustainable (natural) rate of unemployment (citing DeLong 1997, Orphanides 2003, Nelson 2005).

(Snowdon, 2007, p. 547)

At the end of this section, Lucas’s paper appears well as an *imbroglio* since the Lucas Critique *stricto sensu* appears as not disentangled from two other elements: on the one hand the REH and, on the other hand, a positive explanation of the economic crisis of the 1970s (which is closed to the policy ineffectiveness proposition according to the “strong thesis”). From that perspective, the diffusion of the “Lucas Critique” must be understood as having been processed under this form of *imbroglio* (*i.e.* a whole package) so that the interpretation of Lucas (1976) has long been misleading in macroeconomics, especially during the 1970s. At a first sight, Keynesians themselves looked to be confused by it, by focusing their criticisms on the REH or the policy-ineffectiveness proposition but never tackling the Lucas Critique *stricto sensu*. Yet, such an impression is misleading. Indeed, most of Keynesians acknowledged the problem raised by Lucas as it really was,

namely a long-standing issue in econometrics. Then, they simply rejected the lucasian package in its prescriptive and positive form. In other words, they were criticizing the REH as a solution for the prescriptive side of the Lucas Critique (section 3) as well as its positive scope for explaining the economic crisis of the 1970s (section 4). So reconsidered, Keynesian reactions to Lucas (1976) must be then understood as an answer to the Lucas Critique *imbroglio*.

3 Can we handle the Lucas Critique?

In this section, we try to synthesize the main ways to taking into account the prescriptive side of the Lucas Critique. We explore first the New Classical attempts to construct, following the Lucas Critique implications, a modeling strategy that could compete with traditional macroeconometric models. Second, we discuss the Keynesian reactions to the New Classical solution, that is to say the use of the REH. Third, we discuss Lawrence Klein work in the 1970s and the 1980s as a “paradigmatic” example of dealing with Lucas (1976) and proposing an alternative to the New Classical solution.

3.1 The New Classical way

In 1981, Robert Lucas and Thomas Sargent edited *Rational Expectations and Econometric Practice*. This volume collected 34 published and unpublished articles by 17 different authors, and, as the editors put it in their introduction, the general purpose was 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 1981a, p. xxxviii). The aim was then to sum up the empirical techniques as well as their application and the subsequent findings of what we will call the New Classical macro-econometrics (NCME).²⁸ In our perspective, this volume is an interesting synthesis of the econometrical and modeling effort made by those who “swore allegiance” to the Lucas Critique: in more plain terms, those who recognized the prescriptive consistence and relevance of the Critique and accepted its implications, e.g. the rational expectations and the general equilibrium approach.

The first purpose of the NCME can be seen as an “empirical” research program. Indeed, NCME tried to work out empirical evidences against the so-called “Keynesian” macroeconomics and in favor of the rational expectation hypothesis. The main issue was the econometric test of the Keynesian

²⁸ For a comprehensive discussion about the NCME, see Sergi (2015).

Phillips curve against the “natural rate hypothesis”. Basically, the empirical side of NCME consisted in supporting the claim for money neutrality on the short and long run (except in presence of monetary surprises). This result consistent with the theoretical foundation of the rational expectation hypothesis, as formulated first in Lucas (1972b). There, the Lucas Critique was “packed” with a more general claim, especially about its implications (as we already recalled in section 2.1), which were empirically tested.

The second purpose in the NCME program, in which we are interested here, was “normative”. The aim was to construct an explicit set of rules for building an estimated and complete model of the business cycle. Obviously, such a model must follow the methodological and theoretical principles of the New Classical macroeconomics (rational expectations, optimizing behavior, dynamic general equilibrium); in the same time, it shall be able to reproduce the main features of the actual business cycles and to forecast the effects of alternative economic policies. Consequently, those (conditional) forecasts are supposed to be safe from the Lucas Critique point of view. In one word, this second line of research of the NCME try to build a valuable alternative to the Keynesian macroeconomic models, which handled properly the econometric problem raised in Lucas (1976).

The normative side of the NCME mainly relied on the idea of building estimated macroeconomic model providing an estimation of the optimal dynamic decision rules and they, so-called, “deep” parameters (preferences, technology) describing agents’ behavior face to particular economic environments and their changes. This way of modeling proceed in three levels: first, by formulating, computing and estimating (with maximum likelihood methods) the optimal dynamic decision rules for economic agents; second, by specifying and estimating the form for stochastic processes in the economy; third, by testing cross-equation restrictions on the parameters of those estimated equations.²⁹ The cross-equation restrictions are supposed to insure the consistence and the relationship between parameters, so that the model will be robust to the Lucas Critique.³⁰ The cross-equation restriction approach was combined with time series analysis, especially in the wake of the Granger’s definition of causality (Granger, 1969): this analysis helps the modeler in establishing the exogeneity of parameters.³¹ Hansen and Sargent

²⁹ This line of empirical research strictly follows Lucas (1972a)’s prescription.

³⁰ Following the notation used in subsection 1.1, the general idea is to test the consistence between estimated form of G and F).

³¹ The NCME made as well an intensive use of both of these techniques for testing the neutrality of money. The test in terms of Granger’s causality is more frequent in this empirical side of the NCME: well known contributions in this vein are Sargent and Wallace (1973) and Barro (1977b).

(1981a) and his multi-variate version Hansen and Sargent (1981b) is the more “achieved” example of such a model.

The dissemination in the field of macroeconomic expertise of the NCME was quite limited. When we look, for instance, at the non-academic affiliations of the authors contributing to Lucas and Sargent (1981b), we can distinguish basically two types of institutions that are represented: the Federal Reserve Bank of Minneapolis and the NBER. All the leading figures of the NCME worked as consultants for the Minneapolis Fed (Sargent, Prescott, Wallace, Sims); although, among the macroeconomists which had contributed to the NCME more occasionally, we can quote Robert Hall, who was at the time the Director of the “research program on economic fluctuations and growth” of the NBER.

The reasons for such a modest success of NCME are, first of all, the technical (computational) obstacles to the estimation of such models (required by the high number of restrictions to undertake tests simultaneously) as well as identification and specification problems linked to highly non-linear forms of decision rules, stochastic processes and cross-equation restrictions. We can also suppose that another important constraint in the spreading of NCME models relies on the complexity of the NCME models, in respect of the general training level of macroeconomists at the time (in terms of mathematical techniques on dynamic optimization).

Despite its weak institutional dissemination, the NCME approach had also a very aggressive attitude in respect of the modeling practices of institutions. Three interesting examples are the article of three less known Minnesotans, Miller (1978), Anderson (1979) and Miller and Rolnick (1980). In his contribution to the Symposium of *The Journal of Business* about econometric forecasting, Miller exposed his optimistic view about the development of NCME:

[...] [econometric methods] can [provide more accurate forecasts] if the econometric restrictions in the models are derived from theories of individual optimizing behavior in an explicitly defined dynamic stochastic setting. This is no longer a hope for the future. It is a current reality. Sargent (1977b) has employed this methodology to a study of the labor market. I believe this to be the first successful attempt to derive restrictions across stochastic processes based on a model of optimizing behavior. I believe Sargent’s approach is the right direction for future modeling efforts because it promises to produce restrictions which will hold up over time and under policy interventions.

(Miller 1978, pp. 583-584)

This confidence in the NCME approach made it possible to provide a harsh attack on the modeling practices in other institutions. Anderson (1979) provides a new version, of two main macroeconometric models used at the Federal Reserve Bank of St. Louis and at the Board of Governors ³² that included rational expectations and cross-equation restrictions. In a similar vein, Miller and Rolnick (1980) criticizes the models used by the Congressional Budget Office (CBO):

[...] the CBO's model, like all existing macroeconometric models, is useless for policy analysis: it allows neither reliable prediction of the economic effects of alternative policies nor proper evaluation of alternative economic outcomes. We argue that the CBO should adopt a rational expectations, equilibrium approach in order to overcome these difficulties.

(Miller and Rolnick 1980, p. 171-172)

The general claim made by Lucas and Sargent (1979) on the failure of Keynesian macroeconometric models in providing sound expertise, as well as the program of the NCME derived by from the Lucas Critique, stand as quite academic through all the 1980s. Nevertheless, as the three examples above have showed, there was an attempt of trying to spread the ambition of providing an alternative to Keynesian macroeconometric models across other institutions, where a new generation of macroeconometricians was relieving the older generation. The Minneapolis Fed was the epicenter of such an attempt. However, it does not mean that this attempt went without obstacles and objections from the Keynesian faction.

3.2 Criticizing the rational expectations solution

While most Keynesians acknowledged the problem raised by the Lucas Critique, they also refused to take for granted the REH as the unique valuable solution. Besides, as this latter was firstly seen as the key feature leading to the policy ineffectiveness proposition ³³, it soon concentrated the major part of angry reactions. The Keynesian opposition to the New Classical solution

³² The model used in the Fed was called the Fed-MIT-Penn model. He was construct by the Federal Reserve Board jointly with Albert Ando and Franco Modigliani. See Bodkin et al. (1991, p.110).

³³ From that perspective, it is not a mere coincidence if the New Classical revolution was first christened the "Rational Expectation revolution". It took then some years for the majority of Keynesians to realize that it was possible to build models with rational expectations where stabilization policies are useful and welfare improving.

relied mostly on a realisticness basis. Rather illustrative is the reaction of Franco Modigliani, who claimed (in his interview with Klammer) to reject the REH because of its lack of realism:

See, what bothers me about rational expectations is that these people are really pushing specific implications. If it is just a matter of saying you have to take reactions to policies into account, I would agree. Yes, policy measures can change the structure of the economy. Modeling this will be very hard, but there is no objection of principle. [...] [M]y objection is not one of principles, but of applications. [...] I find particularly objectionable the postulate that all rational agents believe the quantity theory of money holds instantly, because there is no reason in the world that that should be true. I tend to believe very few people know what money means and what it does. For example, I could imagine people are much more responsive to the announcement of a tax reduction than to an announcement that tells them at what rate money is growing. It may be that that has an effect on unemployment, but I do not think it means anything to people. So the fundamental way to proceed in modeling, I think, is to take into account the role of expectations in whatever way seems best and most productive. I can think of situations where assuming that expectations are consistent with the model is a convenient way to start. But we cannot base all our conclusions on that assumption.

(Klammer, 1984, p.125-126)

Such trial on unrealism was similarly undertaken by most Keynesians, like a.o. Solow (1978) or Tobin (1981). Interestingly, those latter started to react to the REH only after Lucas (1976) whereas this hypothesis was less extensively developed in this paper than in Lucas (1972b). Hence, this focus on the REH must be interpreted as a way to react to the Lucas Critique in the following sense: if the problem raised by Lucas holds, the solution he suggests makes no sense. Now, we need to note that the alternative solutions proposed by Keynesians were not necessarily the same ones. In a nutshell, while some of them were keeping to assume adaptive expectations (like several monetarists), others were acknowledging the need to rely on a more forward-looking concept of expectations. This is the line of research of Keynesians like Katona (1980) but also Lawrence Klein. Besides, this latter is the one who most reacted to the Lucas Critique and made efforts to develop alternative solutions to it. Considering in a first time that rational expectations can be a sound way of treating the problem of expectations in macroeconomic models, he finally rejected because it was asking too much from the data, namely it "asks both to generate the expectations and [to]

provide the model estimates with simulation" (Klein and Mariano, 1987, p.442). Thus, according to him, the rational expectations approach would entail an identification problem:

I think that for expectations—unless we get fresh information - we have an identification problem. From an econometric point of view we used to characterize the problem of using the same data to estimate first the variance - covariance matrix of observation error and then coefficients based on these as eating one's tail - to make the sample try to do both things. I think that the people who want to use the model to generate expectations and then estimate the model are also eating their own tails. They are assuming that their methodology is correct without validating that assumption. Many people seem to like the procedure, but I think it faces a fundamental problem.

(Klein and Mariano, 1987, p.442)

The fundamental problem Klein was talking about would be a major epistemological problem, since "[t]here is little attention paid to whether [macroeconomists using this approach] are right or not" and the single thing they would pay attention to is "only [...] the fact that it is a procedure that makes expectations endogenous" (Klein and Mariano, 1987, p.442). But Klein would go even further and would say that "[he] deplore[s] the willingness [of these macroeconomists] to make very strong assumptions about the way expectations are formed, simply for the sake of getting the rational expectations approach as just a technical device to get elegant results, but which presented serious methodological problems". Hence, the use of the REH in macro-econometric models simply do not enable to lay the foundations for a secure method in front of the Lucas Critique. Reversely, Klein advocated for a more realistic approach in the modeling of expectations, in order to understand how people really react to changes in economic policy (following the line developped for years by Katona). However, his way of treating expectations is inseparable from his wider research program that we need, now, to explain at length.

Lawrence R. Klein carried a true alternative to the new classical approach, developing a microfoundational program that Kevin Hoover labeled "aggregation program" (Hoover, 2012). In this program, "each of the Keynesian functions was analyzed at a microeconomic level and its implications for a feasible macroeconometric model considered" (Hoover, 2012, p.41). Klein aimed at "disaggregating" the macroeconomic model "as far as the data [would] permit" (Hoover, 2012, p.51). And Klein's way of treating the problem of expectations is typical of this way of thinking. First of all, macroeconometricians did not wait for the Lucas Critique to integrate expectation equations in

the model. While the idea had emerged in Lucas's work, Adams and Duggal (1974) built an "Anticipation version" of the Wharton model - a model built in part by Klein, following his former "PostWar quarterly model" (Klein, 1964). Adams and Duggal wanted to study the effect of expectations on the multiplier and used for that purpose three types of expectation variables: the Michigan index of consumer sentiment, the BEA (Bureau of Economic Analysis) Investment Anticipations and data on housing starts. These variables were included in the model, "both as explanatory variables and with enough additional equations to explain their formation" (Bodkin et al., 1991, p.127).

Even if the issue of expectations was not central for Klein in the 1970s (as section 4 testifies)³⁴, the problem raised by Lucas had to be taken into consideration. Following the fact that "the rational expectations school has raised some important questions about the dynamics of the macroeconomy" (Bodkin et al., 1991, p.553), Klein brought some propositions to deal with the formation of expectations and with the agents' reaction face to changes in economic policy. As underlined above, the opposition with the New Classical school was rather methodological. Klein rejected the kind of instrumentalist philosophy conveyed by Lucas and his followers. It seemed more useful for Klein "to estimate explicitly, rather than implicitly", the equation of expectations formation. Klein was more interested in discovering the "actual" process behind the way agents would form their expectations. The key element in this research area was the use of new microeconomic information:

In my opinion, the best way is to go to the source of expectations and find out what people actually expect or anticipate and to endogenize that within the frame-work of models. That means that we should integrate sampling investigations on subjective expectations together with market and accounting data for the economy and treat that as one big system with the subjective expressions of expectations as endogenous variables. I think that is a very straightforward procedure, and one that will prove to be the best. This approach will have true informational content because we will be trying to model people's stated expectations in a realistic way. We must take account of the life of these expectations. In fact, it is rather short, and that means we have to have repeated subjective observations. I find the European business test surveys, the surveys of consumers, the various surveys

³⁴ Klein acknowledged the importance of expectations in macroeconomic modeling: "I think expectations are very important and I think that the model builders have recognized it from day one" (Klein and Mariano, 1987, p.419). But he considered that the disturbances of the 1970s raised the issue of the modeling of the supply side in macroeconomic models. Macroeconomists had to tackle the modeling of the energy and raw materials sectors.

of inflation, the statistics on orders, the statistics on housing starts, and all the things we call anticipations variables to be very important. They need to be integrated directly into the models.

(Klein and Mariano, 1987, pp.419-420)

It is clear that Klein advocated for a "bottom-up" line of research. Microeconomic surveys enable to see how agents look at macroeconomic variables and what their expectations for these variables are. With this amount of data, you can try to find some fundamental equations that describe the behavior of agents, and then understand how they react to economic policy. This approach clearly represents an alternative to the New Classical way of modeling expectations. However, as Hoover (2012) puts it, the major problem with this approach relies on the aggregation issue. Does it exist a reliable method to aggregate all these subjective data on agents' expectations? The "representative agent microfoundational program" of the New Classical School is a way to avoid the question directly confronting Klein's approach. Perhaps this is why Klein's proposal did not seem to have found its way in the academic world. The alternative propositions to the NCME, concerning the Lucas Critique, seem to be rather scarce at that time. In fact, the debates were focusing on the empirical question hidden behind Lucas (1976): do expectations and bad economic policies explain the disturbances of the 1970s?

4 The "positive way" to criticize the Lucas Critique

As we claimed in section 2, the common contemporary interpretation of Lucas (1976) is a "prescriptive" one: the Lucas Critique is presented as a principle that must be taken into account in order to avoid the inconsistency of macroeconomic models designed for policy evaluation. But it also represents an attempt to explain the stagflation period by the failure of Keynesian econometric policy evaluation (the "ideas hypothesis", as Snowden, 2007 labels it). In this section, we will see how the Lucas Critique received a positive interpretation, as a contextual, positive attempt to explain why bad economic policies had thrown the United States economy into stagflation. Indeed we analysis how Lucas (1976) has led to an empirical dispute. We consider that the debate around the interpretation of stagflation provides a broader frame for understanding the reactions of Keynesian macroeconomicians face to the Lucas Critique. The empirical refutations of the Lucas

Critique belongs to a larger issue: finding how to explain what was happening during the 1970s. The Keynesians general perspective was also to defend the performance of the standard macroeconometric models in the 1970s, and to set up their research agenda for the years to come.

4.1 Refuting the Lucas Critique: The Phillips Curve is still very much alive

Alan Blinder is emblematic of this way to address the problem.³⁵ In his reply to the Lucas Critique (Blinder, 1987, 1988)³⁶, Blinder adopted a much wider point of view: he casted doubt on the legitimacy of the New Classical Revolution. While, in the 1980s, the vast majority of the young economists did not declare themselves "Keynesians" anymore, Blinder was wondering if this scientific change had occurred for good epistemological reasons. According to him, this was obviously not the case, since the New Classical Revolution was a theoretical revolution without any empirical grounds:

A scientist from another discipline might naturally surmise that the data of the 1970s had delivered a stunning and unequivocal rejection of the Keynesian paradigm. He would look for some decisive observation or experiment that did to Keynes what the orbit of Mercury did to Newton. But he would look in vain. [...] [T]here was no anomaly, (...) the ascendancy of new classicism in academia was instead a triumph of a priori theorizing over empiricism, of intellectual aesthetics over observation and, in some measure, of conservative ideology over liberalism.

(Blinder, 1988, p. 278)

According to Blinder, New Classical macroeconomics proposed no relevant explanation of stagflation. Actually, New Classical macroeconomists just took advantage of the temporary empirical invalidation encountered by the Keynesian theory. Blinder thought that the success of the Lucas Critique was the result of a bad inference, following the assessment of this invalidation:

³⁵ At that time, Blinder was at Princeton and Research Associate of the NBER. In 1975, he was Deputy Assistant Director of the Congressional Budget Office (CBO). No doubt that he knew well the CBO's model criticized by Miller and Rolnick (1980), as explained above.

³⁶ The empirical refutation of Blinder was already there—but in a very crude way—in his book on stagflation (Blinder, 1979, p. 92).

Academic readers of Lucas put two and two together and jumped like lemmings to the wrong conclusion. The facts were (a) that inflation rose and (b) that the correlation between inflation and unemployment changed. The (untested) assertion was that the Lucas critique explained why (b) followed from (a): the government had adopted a more inflationary policy. (Blinder, 1988, p.283)

(Blinder, 1988, p. 283)

After the first oil shock in October 1973, the empirical relation between inflation and unemployment disappeared, and New Classical insights gained in popularity in the academic sphere. In its criticizing of the Phillips relation, the Lucas critique seemed to be indirectly validated as it offered a theoretical justification for the disappearance of the Phillips curve.

Alan Blinder strongly rose up against that use of the Lucas Critique. Acknowledging the failure of the Phillips curve to correctly describing the new 1970s situation, Blinder stated that Keynesian economics reacted very promptly in adding new features (like oil prices) that allowed for a better fit of the Phillips curve with data (Blinder, 1987, p. 133).³⁷ At the same time, Blinder claimed that there was no proof whatsoever that the Phillips curve disappearance was the consequence of a change in the agents' economic behaviour, and he tried to prove econometrically the opposite case:

It was remarkable how uncritically the Lucas critique was accepted. Had governments really decided to 'ride up' the Phillips curve toward higher inflation, as Lucas claimed, or had they simply encountered bad luck from the supply side? The former was assumed even though the latter seems clearly to have been the dominant factor quantitatively. Did the more inflationary environment shift the distributed lag $a(L)$? Rather than seek evidence on this point, partisans of the Lucas critique became econometric nihilists. Theory, not data, was supposed to answer such questions; and theory allegedly said yes.

[...] There is no evidence for a shift in the lag coefficients $A(L)$. And that, in turn, suggests that the breakdown of the old-fashioned Phillips curve cannot be attributed to the reason emphasized by Lucas.

(Blinder, 1988, p. 278)

The New Classicals' behavior was anti-empirical and the Lucas Critique soon became a kind of leitmotiv to attack Keynesian models.³⁸ Blinder acknowledged that the Critique may be true and that the modeler has to be

³⁷ Blanchard (1984) proposed an econometric test in the same vein, backing the claim that the traditional Phillips curve is still empirically relevant.

³⁸ This anti-empirical nature of the Lucas Critique was already put forward in 1984 by Blinder in an interview with Arjo Klammer: "All you have to do in this country (...)

careful. But, if one wants to work in a scientific way, one has to check if changes in agents' economic behavior have a true and substantial effect on the relation one is studying. Stanley Fischer defended the same point when he claimed:

It is indeed remarkable that the Lucas policy evaluation critique has triumphed without any detailed empirical support beyond Lucas's accusation that macroeconometric models in the 1960s all predicted too little inflation for the 1970s. The general [theoretical] point made by the critique is correct and was known before it was so eloquently and forcefully propounded by Lucas. That the point has been important empirically, however, is something that should have been demonstrated rather than asserted.

(Fischer, 1983, p. 271)

Without such an empirical verification - that has to be done for every studied relation - the popularity of the Lucas argument is not justified. According to Blinder, casting doubt on the Lucas Critique is important especially as it leads to a "back to basics" movement (Blinder, 1988, p.285) which promoted strong rationality as an *a priori* condition to model agents' behavior.

4.2 Keynesian Models do actually explain Stagflation

Alan Blinder was not the only econometrician reacting against the Lucas Critique in this way. A part of Klein's response relies on the defense of the prediction power of Keynesian macroeconomic models during the 1970s and on the refusal to considering expectations as the major determinant of the unexpected inflation. In a paper written in 1976, Klein "showed that the Wharton model, if given a conventional fiscal shock, would generate the usual trade-off relationship, but if given a food or fuel price shock would generate a situation of rising unemployment and rising inflation" (Klein, 1985, p. 293).³⁹

right now is scream mindlessly, "Lucas critique!" and the conversation ends. That is a terrible attitude. The Lucas critique may be correct, but I have seen no persuasive evidence in any sphere to indicate that it is empirically important. The empirical case is yet to be made" (Klamer, 1984, p. 166).

³⁹ We did not succeed in grabbing the 1976 paper cited by Klein. In Klein (1985), he quoted it in this terms: "Klein, Lawrence R. "The Longevity of Economic Theory", Quantitative Wirtschaftsforschung, H. Albach *et al.* Tübingen: J.C.B. Mohr, 1977, 411- 19. This paper was read to E. Malinvaud's seminar in Paris, November, 1976".

This assertion is at odds with Lucas's argument on the impossibility of evaluating economic policies by means of econometric models. Large-scale macroeconometric models *à la* Klein, many of which had "Phillips curves as structural equations of the labor market" (Klein, 1985), were capable of realistically simulating the effects of an economic policy, as well as the unexpected effects of price shocks. This point brings the debate to the question on the *originating source* of the 1970s inflation to the question whether economic policy caused inflation (Lucas's position) or whether inflation was rather the result of price shocks (Klein's position).

According to Klein, "inflation was not policy induced", or at least, it was "certainly not [induced] from the policies that were coming from the neoclassical-Keynesian model". Inflation "was purely exaggerated by the food and oil shocks" (Klein, 1985, p.291). Taking price shocks as a valid (though partial) explanation of the inflationary surge of the 1970s, large-scale macroeconometric models would stand the test of their simulation capacities. They would prove useful in simulating not only the effects of a particular economic policy, but also the effects of an external shock to the system.

Consequently, modeling the energy and raw materials sectors, instead of introducing expectations to the models, was at the top of the research agenda according to Klein. He explained that until the 1970s "[m]any people failed to realize how important energy or oil, in particular, was for the economy because it represented only a tiny share of total GNP" (Klein, 1985, p. 290). This neglecting of the energy (and food) sector constituted the source of the underestimation of the inflation rate in the forecasts of the Wharton model. Once the macroeconometric models introduced the energy and the agricultural sectors, by the mid-1970s, econometricians "were able to overcome a lack of information from [these] area[s] of economic activity" (Klein, 1985, p. 292). This new available information allowed econometricians to build "an amplified model that was able to handle the inflation problem more realistically by mid-1975, when inflation was still strong", which yielded a moderate forecast error (Klein, 1985, p. 292).

To strengthen his claim, Klein used the work of Otto Eckstein (1978, 1983), one of the co-founders of the DRI model in 1969, who tried to assess the relevant determinants of the crisis and the validity of the rational expectations hypothesis. Using the DRI model, Eckstein (1978) had studied the impacts of six major shocks on the rate of unemployment and inflation. He showed that the major causes of the stagflation were, in order, "the Energy Crisis (the major contributor, according to Eckstein's analysis, to both observed inflation and unemployment and hence highly stagflationary), the Agricultural Price Explosion, monetary policies [...], the devaluation of the dollar, price decontrol (for inflation) or price controls (for unemployment),

and fiscal policies, 1969-74 (the least important for inflation and, in a sense, for unemployment)” (Bodkin et al., 1991, p. 126). Few years later, Eckstein led some new tests and drew a clear general conclusion : “[C]hanges in policy regime seem to have been among the minor sources of structural change of the economy and of forecasting error in the actual historical record” (Eckstein, 1983, pp.xi-xii). The empirical work of Eckstein was clearly aiming at discrediting the use of the Lucas Critique argument to explain the rise both in inflation and unemployment.

In his conclusion to the conference held at the Federal Bank of Boston, "After the Phillips Curve: Persistence of High Inflation and High Unemployment,"⁴⁰ Solow seemed to sum up well the Keynesian empirical position. Even if he acknowledged that “there is a very valuable and important point which is in very large part due to Lucas and Sargent, and one must give them credit for it, that what often looks casually like a change in structure is really the economic system reacting to its own past” (Solow, 1978, p. 205). According to Solow, the true challenge of stagflation was elsewhere. Instead of throwing out the baby with the bath water, macroeconometricians should improve the models by emphasizing the supply side of the economy, more precisely by emphasizing “the side of food, oil, nonfuel minerals, and the depreciation of the dollar” (Solow, 1978, p.205).

This section suggested some examples of macroeconometricians reactions face to the positive side of the Lucas Critique, i.e. its interpretation of stagflation in the 1970s. The claim of this section was to clarify how those reactions focused, on the one hand, in providing empirical evidences against Lucas Critique implications and, on the other hand, in constructing a reliable explanation of the stagflation within the traditional macroeconomic modeling approach.

Concluding remarks

Our paper suggested an exhaustive typology for building an ordered comprehension of the heterogeneous reactions of Keynesian macroeconometricians against Lucas (1976), providing some examples to illustrate the usefulness of this classification. Our claim is that a deep understanding of the topic of the response to the Lucas Critique must be based on the appraisal of the Lucas *imbroglio*, which means that readers of the Lucas Critique should be aware of the fact that “Econometric Policy Evaluation” made a (con)fusion

⁴⁰ It is the conference where Lucas and Sargent has presented their warlike paper, “After Keynesian Macroeconomics” (Lucas and Sargent, 1979).

between two registers: the prescriptive interpretation and the positive account of the Lucas Critique. The prescriptive interpretation implies a general principle for building consistent macroeconometric models, providing sound economic policy analysis. We insisted on the fact that this level of the argument was not new for econometricians, and that it is nothing more than a revival of Frisch's critique of Tinbergen's work for the League of Nations, following Frisch's concept of autonomy. The positive account of the Lucas Critique is strongly related to the discussion of the U.S. stagflation in the 1970s and to the issue of the invalidation of the Phillips Curve. In the same register, the rational expectations approach and the policy ineffectiveness of the macroeconometric models implied by the Critique were also a part of the prescriptive/positive "package" leading to the Lucas *imbroglio*. We claimed that this *imbroglio* can be regarded as a main reason of the disruptive impact of Lucas (1976). Attracted by the stagflation issue and the policy ineffectiveness proposition, Keynesian macroeconometricians rarely answered directly to the Lucas Critique *stricto sensu* and seemed to dodge the problem.

Our opinion is that further researches in the recent history of macroeconomics can bring two important developments about the issues discussed in this paper. The first one is, obviously, a more deeper and detailed account of some specific points (e.g. the alternative program of Klein, or the debates about the stagflation), which implies to explore more exhaustively the existent literature.

Another line of research should take a closer look to the inner reasons of the Lucas *imbroglio*. Indeed, the disruptive character of Lucas's contribution must be closely interpreted as a methodological and as an epistemological turn in the history of the discipline, especially in its conception of the role of models and in their relation (and "mediation") with theory and with the empirical world. Actually, following the insights of Lucas (their "methodological spokesperson"), New Classical macroeconomists support what we could call an aprioristic and instrumentalist view of modeling. Their models must be built including some *a priori* assumptions (optimization in a general equilibrium framework, rational expectations) especially to escape from the Lucas Critique.⁴¹ These two hypotheses are aprioristic since they are non-realistic and supposed to be non-testable (in an instrumentalist perspective, only their predictions should be tested), and so they are non-refutable. On the other hand, these same models must provide some empirical (econometric) corrob-

⁴¹ Note that the *a priori* assumptions are also justified by their consistence with the general methods of economics (Lucas, 1977): indeed, they should be regarded as the only sound "microfoundations" for macroeconomics. This is, however, far to be so, as we recalled in the case of Klein (section 3) and as is discussed in Hoover (2012).

oration of their results (by reproducing past data or by producing forecasts), to prove that the *a priori* hypotheses are valid instruments for prediction (or retrodiction). This general methodological view can be easily interpreted as the very underlying principle of the *imbroglio* between prescriptive and positive statements. The weakness of this conception is that a paradox arises when the model results are not empirically verified, while the hypotheses underlying the results cannot be rejected.⁴²

The aprioristic bias of the New Classical methodology is, of course, barely understandable from a “Keynesian” macroeconometric perspective. In the case of the Keynesian models, *a priori* hypotheses were also necessary and even very important as sources of information that would enrich the models (Klein, 1960). The nature of these *a priori* hypotheses, however, completely differed from that of New Classical modelers, since the latter would not emphasize at all on the importance of the “realisticness” of these hypotheses, while in the case of the Keynesian macroeconometricians, like Klein, the “realisticness” of the *a priori* hypotheses was just fundamental.⁴³ Moreover, the work of Klein testified of the wish to build more empirical microfoundations to macroeconomic models. The later does not have to rely on *a priori* microeconomic propositions and must be disaggregated in function of the data you own.

New Classical macroeconomics deeply challenge the modeling practices in this perspective, which, we suggest, is largely responsible for the controversial reception of the Lucas Critique. Moreover, the understanding of this topic can also enlighten the slow dissemination of New Classical macroeconomic models in policy-making institutions. To put it clearly, the radical change in the way of providing econometric policy evaluation, advocated by Lucas (1976), did not take place until the very recent years, with the rise of the DSGE models as a spread practice within central banks, even if, as we recalled in the introduction, the DSGE models are far from being well-established.

For all these reasons, we can definitely affirm that the responses to the Lucas Critique are an underestimated part of the history of macroeconomics. And hence, a more careful study of these responses and reactions might actually bring about a deeper understanding of the evolution and current state of the discipline of macroeconomics.

⁴² For a more comprehensive discussion about this topic, see Sergi (2015).

⁴³ For a more comprehensive discussion of this topic, especially in Klein’s early work on econometrics, see Pinzón-Fuchs (2014).

Appendix

The traditional Phillips curve (Samuelson and Solow, 1960) suggests an explicit relationship between the wage-inflation rate $\frac{\Delta w}{w}$ and the level of unemployment U , of the kind⁴⁴:

$$\frac{\Delta w}{w} = \alpha U^z + \beta \quad (1)$$

with α, β and z fixed parameters. In this framework, monetary policies have a real effect in setting the rate of unemployment through the manipulation of the inflation rate. Econometric estimations of this relationship aim at inferring the numerical value of the parameters from observed paths of U_t and w_t .

Lucas (1976) suggests that the actual relation between output and inflation must be described by specifying the optimization behavior of the individuals and their expectations.⁴⁵ Lucas wants to demonstrate that the parameter α is not fixed or stable, and that it will change following changes in the environment, especially following changes in monetary policy.

The economy is characterized by N markets. The production in each market i at time t , $\forall i = 1 \dots N$, is determined by a permanent and a transitory component:

$$y_{i,t} = \bar{y}_{i,t} + \tilde{y}_{i,t} \quad (2)$$

The transitory component $\tilde{y}_{i,t}$ is supposed to be a linear function of the deviation between actual and perceived relative prices on the market i :

$$\tilde{y}_{i,t} = \beta(p_{i,t} - p_{i,t}^e) \quad (3)$$

The price level on the market i , which is actually observed by agents, is determined by:

$$p_{i,t} = p_t + z_{i,t} \quad (4)$$

with p_t a variable, common to all markets and $z_{i,t}$ a random disturbance (normally distributed, with mean 0 and variance τ^2 , independent from the distribution of p_t), representing the relative price variations across time and markets. The distribution of p_t is regarded by agents, on the basis of past information, as following a mean \bar{p}_t and a variance σ^2). The two components of $p_{i,t}$ cannot be observed separately by the agents. The real general level

⁴⁴ For a history of different specifications of the Phillips Curve, see Qin (2011).

⁴⁵ The example presented here is inspired by Lucas (1976)'s section 5.3, but is obviously the core of the well-known model in (Lucas, 1972b).

of prices is simply described as the average across the N markets of the i particular prices:

$$1/N \sum_{i=1}^N p_{i,t} = p_t + 1/N \sum_{i=1}^N z_{i,t} \quad (5)$$

For an N that is large enough, in order for it to converge to its mean, we can assume that the general price level in the economy is the average of the particular prices of each market.

Agents in each market are supposed to determine their supply following their expectations p_i^e, t , which are rational in the sense of Muth (1961):

$$p_t^e = \mathbb{E}(p_t | p_{i,t}, I_{t-1}) = (1 - \theta)p_{i,t} + \theta \bar{p}_t \quad (6)$$

with $\theta = \frac{\tau^2}{\sigma^2 + \tau^2}$.

Then, the cyclical component of the aggregate production is written as $\tilde{y}_{i,t} = \theta\beta(p_{i,t} - \bar{p}_t)$ and global production is:

$$y_{i,t} = \bar{y}_{i,t} + \theta\beta(p_{i,t} - \bar{p}_t) \quad (7)$$

Following the above equation, the cyclical variation of the level of output directly depends on the spread between the actual price level in a specific market i and the anticipated global price level \bar{p}_t . As the expectations are rational, we can easily conclude that any variation in the distribution of general price level modifies the output. According to Lucas, if we suppose that the general price level follows a random walk:

$$p_t = p_{t-1} + \epsilon_t \quad (8)$$

with $\epsilon_t \sim (\mu, \sigma^2)$. Then, the output can be written as

$$y_{i,t} = \bar{y}_{i,t} + \theta\beta(p_t - p_{t-1}) + \theta\beta\mu \quad (9)$$

Under the condition of a stability in parameters θ, β , an observable (econometrically estimable) stability of this relationship is guaranteed. But, if the values of these parameters change, for instance due to a change in monetary policy (or otherwise, a change in the distribution of p_t), then the relationship is not valuable for simulation of alternative policies anymore.

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