

Criticizing the Lucas Critique: Macroeconometricians' Response to Robert Lucas

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[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 one of the most famous papers in macroeconomics. This paper is 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, but also on the rather convoluted macroeconomic context of that period. During the 1970s, the annual inflation rate reached its highest level in the United States (in the

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postwar era), remaining always above 5% from April 1973 until September 1982 and reaching two peaks (12.2% in December 1974 and 14.7% in March 1980). At the same time, the unemployment rate steadily increased until it reached a peak following closely the inflation top (9% in May 1975).¹

There appears to be a conventional view in macroeconomics sustaining the idea that both this convoluted macroeconomic context and Lucas's (1976) paper would have caused a devastating effect on one of the strongholds of Keynesian theory: the Phillips curve. This idea, however, is a very general and widespread perception in macroeconomics, which misses many important points of the history of the Phillips curve. It is not our aim to discuss this history here, but some clarifying points are, at least, worth mentioning.² In its more simple version, this conventional view regards the Phillips curve as a popular tool for policy analysis, displaying a *negative* relation between inflation and unemployment. This relation had fitted well empirical data until the end of the 1960s, but seems to have disappeared in the 1970s. And so, after many macroeconomists—including Lucas (1976)—, the Phillips curve would have become the major weakness of Keynesian macroeconomic theory. This view, however, does not take into account the variety of the existing Phillip curves in the 1970s, nor does it take into account the fact that the only curve that actually “collapsed” in the 1970s was Phillips (1958)'s original version, which had been estimated without taking into account price expectations. And yet, the 1958 original curve was never that important for economic policy (Forder, 2010 and Hoover, 2015, p.15). It was only after Phelps (1967)'s and Friedman (1968)'s expectations-augmented versions of the Phillips curve that it actually became a popular tool for policy use (*ibid.*).

In his 1976 paper, Lucas singled out the Klein and Goldberger (1955) model as the “earliest version” of this line of works, embodying the Phillips curve trade-off between inflation and output as the crucial element for policy analysis. Lucas made clear, however, that his target was more generic, and that he was aiming at criticizing the traditional aggregate Keynesian macroeconomic approach as a whole. Lucas's claim was that aggregate models will be useless for evaluating policy changes, if their parameters do not correspond to the underlying structure of the economy. This would particularly be the case when the parameters of aggregate models do not represent the behavior of private agents as optimizing, forward-looking decision rules. As such decision rules vary along with policy changes, then these shifts in the model would invalidate econometric inferences between past data and the

¹ Source: US Office of Labor Statistics.

² For a more detailed discussion of the history of the Phillips curve see Forder (2014) and Hoover (2015).

forecast effects of a new policy. Finally, such aggregate models would fail to account for the influence of a particular economic policy.

Lucas (1976) is supposed to have had a terrific impact on the Keynesian framework. For Preston Miller “the Lucas Critique was fatal and [so] new 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 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 for consistent modeling. Michael Woodford, in the introduction of his book—the emblem of the “New Neoclassical Synthesis”—states that the first basic principle for building macroeconomic models is “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.

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 subjected 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 the rational expectation hypothesis) 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—illustrated, for example, by the building of the Liverpool model (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. 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 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 this article is to provide a better explanation of the reactions of the Keynesian macroeconometricians to the Lucas Critique in the years following its publication and, finally, to provide a better explanation of the success of the Lucas Critique. Our explanation will be based on an interpretation of “Econometric Policy Evaluation” both as a positive and as a prescriptive statement. We think that this duality is present as well in Lucas’s paper and in the reactions to it. This allows us to better understand why Keynesians did not provide a global, pertinent, convincing response to Lucas, which weakened their position inside the profession.

The common path of interpretation is, nowadays, 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

³ The advanced textbook written by Minford and Peel (2002, chap. 6) gives a substantial place to the question of long-term age contracting (following Barro 1977), while Duarte (2011, p. 396, fn. 7) discusses questions related to the Calvo index.

⁴ The situation for France is particularly interesting, since the French Treasury uses two types of models: MESANGE, a structural model with no explicit microfoundations (and so subject to the Lucas Critique) and EGEE, a standard DSGE model.

⁵ This is what Snowdon (2007, pp.547-548), following Romer (2005) called the “ideas hypothesis”, where an inappropriate theoretical framework has led to bad economic policies that have increased inflation.

would provide an explanation of 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 between the positive and the prescriptive statements. 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 relied on a large corpus, allowing for the identification of the reactions within ten years following the publication of “Econometric Policy Evaluation”. Our point of departure was the systematic inquiry of Ericsson and Irons (1995). Ericsson and Irons’s 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), using different categories to classify the papers. To construct these categories, the authors asked themselves: (1) whether the paper under scrutiny consisted on a theoretical, an empirical or a mixed contribution. (2) Whether the reference to Lucas was substantial or tangential. And (3) whether the paper just postulated the validity of the Lucas Critique. With the typology of Ericsson and Irons’s, we selected 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’s focused only on journal articles) and also in interviews. We looked at the proceedings of some macroeconomics conferences (for instance 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 so, it is flexible enough to easily taking into account any new contribution.

We first propose a brief recall of the formal content of Lucas (1976), and a discussion of this article as a mainly prescriptive argument. Then, we place the Lucas Critique in relation to the history of econometrics, especially to Ragnar Frisch’s notion of autonomy, showing that the prescriptive side of the Critique is nothing but a well-known problem in macroeconometric modeling. Furthermore, we reconstruct the origins of the substantial argument of Lucas (1976) in Lucas’s earlier writings and find that the argument was already present in earlier works, but that this substantial argument was embedded within a mainly positive program on the Phillips Curve. And so, the origi-

nality (and the success) of “Econometric Policy Evaluation” can be explained by an *imbroglio*, an entanglement between a prescriptive and a positive argument. We then synthesize the reactions against the prescriptive side of the Lucas Critique, taking into account the heterogeneous efforts for handling the problem emphasized by Lucas in an “innovative” way, especially through the works of Lawrence R. Klein. Finally, we present the reactions to the Lucas Critique as a contextual and positive argument about the understanding of the 1970s stagflation.

1 The Lucas Critique: a well-known prescription in macroeconomic modeling

In this section we first propose a description of “Econometric Policy Evaluation”, emphasizing the prescriptive dimension of Lucas’s arguments. We recall that the substantial prescriptive contents of Lucas (1976) can be traced back to older debates among econometricians especially by Frisch in his own “critique” of Tinbergen’s work.⁶ Thus, from a historical point of view, “Econometric Policy Evaluation”, in its mainly prescriptive dimension, is not as path-breaking as claimed by the standard account in the history of macroeconomics.

1.1 What is the Lucas Critique?

In a clearly “destructive” perspective (Lucas, 1976, p. 41), “Econometric Policy Evaluation: A Critique” explicitly aims at criticizing mainstream 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 providing the bases for this major revision.

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

⁶ Aldrich (1989) already suggested this interpretation.

⁷ Lucas explicitly targeted Klein and Goldberger (1955) (*ibid.*, p.19, fn.2) and Tinbergen (1952) (*ibid.*, p.21), but his target, again, was generic. He was targeting all the aggregate Keynesian macroeconomic models that descent particularly from Klein and Goldberger (1955).

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).

These contentions [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 identified 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 “right” way of modeling that would produce a sound quantitative evaluation of the distinct effects of alternative policies: it states what modelers must avoid (to consider the parameters invariant with respect to changes in policies) and it also states the alternative way to go (to take into account the parameters drifts in response to changes in policies).

Lucas Critique is formalized as follows.⁹ The evolution of the relevant variables of the economic system s_t can be described by the function f :

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

where z_t is a vector of exogenous variables, representing the “environment” of the economic agents (including policy decisions), ϵ_t a vector of random shocks (i.i.d.). Traditional econometrics identifies f (not directly known) by means of a distribution function F and through the estimation of 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 points out that the behavioral parameters in θ should not be fixed (so, they are not invariant for all $\{z_i\}_t$): these parameters should be a *function of the individuals optimizing decisions rules* (λ), which reacts to changes in z_t . This relation between government decisions and individuals’ decisions

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 is borrowed from Lucas and Prescott (1971). The third example deals with the determination of the Phillips curve.

can be written as $\lambda = G[s_t, z_t]$, with G a known function. Then, the motion of the economy is actually described by the relation:

$$s_{t+1} = F[\theta(\lambda), z_t, s_t, \mu_t] \quad (3)$$

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

According to Lucas, the specification of $\theta(\lambda)$ must deal with two questions (to which “Econometric Policy Evaluation” only alludes): (1) the description of the optimizing behavior of the economic agents and (2) the description of the way these agents form their expectations about the future *in a forward-looking* way. According to Lucas, the first question is not problematic since economic theory, and especially general equilibrium theory, would know how to deal with the description of agents’ optimizing behavior. On the contrary, the second question about the formation of expectations can only be solved (i.e., $\theta(\lambda)$ can be specified and estimated), 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 prescriptive argument of “Econometric Policy Evaluation” resides both in the criticism itself and in the explicit formulation of a new principle for macroeconomic modeling. This new principle consisted on (1) the explicit specification of the individuals’ dynamic optimization rules, and (2) on the estimation of only the so-called “deep” (invariant) parameters, like preferences and technology. This perspective was the main concern of those

¹⁰ Expectations are rational if the subjective probability distribution (agents’ expectations) equals the objective probability distribution. At time $t - 1$, the expected value of a variable x at time t equals then the mathematical expected value of x , conditionally to the set of available past information Ω_{t-1} :

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

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 (which can be two alternative ways for describing forward-looking expectations).

who recognized the prescriptive relevance of the Critique and accepted its implications. A full econometric research program, starting from the middle of the 1970s, tried to build a new class of models which would properly handle the econometric problem raised in Lucas (1976).¹¹

This approach also extended Lucas (1976) attack towards a more institutional level. Miller and Rolnick (1980), for instance, criticized the models used by the Congressional Budget Office (CBO):

The CBO's model, like all existing macroeconomic 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, pp.171-172)

Throughout the 1980s, the effect of Lucas's (1976) prescriptive claim remained more influential in academic macroeconomics than in professional macroeconomics. In fact, both theoretical and technical reasons explain the limited dissemination of this approach (Sergi, 2015). Nevertheless, some disciples attempted to spread this ambition of Lucas's prescriptive claim across other institutions, as providing an alternative to Keynesian macroeconomic models.

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). Tinbergen (1956) constitutes the most interesting reference, as he emphasized the problem from the perspective of econometric practice for policy advising.

In his 1956 book *Economic Policy: Principles and Design* Tinbergen 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

¹¹ Lucas and Sargent (1981) provide first synthesis of this line of research. For a comprehensive discussion of Lucas and Sargent (1981) and of the New classical macroeconomics, see Sergi (2015).

is changed” (Tinbergen 1956, p.149) as, for instance, through a change in the “pricing scheme” (*ibid.*, p.161).¹² 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:

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. [...] 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, pp.151-153)

Although he was aware of the fundamental problem of non-invariance raised later by Lucas, from the econometric practitioner’s perspective, Tinbergen thought that this problem would be relevant for econometric policy evaluation only in particular cases, and so he rejected the problem from a prescriptive point of view.¹³ Tinbergen’s conclusions about the use of macroeconomic modeling for policy evaluation had already been subject to harsh criticisms since the 1930s, long before Lucas (1976). 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, emphasized by Marschak, Tinbergen himself and, finally, Lucas. Thus, 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 through structural estimation methods. This first

¹² Which is the same kind of example used in Lucas (1976, sect. 5.3).

¹³ The same remark applies to Marschak, in his contribution to the 14th *Cowles Commission Monograph*, and to Haavelmo’s manifesto: see Marschak (1953, p. 8, p. 25) and Haavelmo (1944, p. 27).

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).

approach would provide the way of discovering the more “fundamental” equations, the “essence of theory”. On the other hand, econometrics would be concerned with the analysis of *non-autonomous* relationships, which would consist on the method of *confluence analysis*. This method would investigate 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 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 a change in the form of an equation does not change the form of any other equation of the system.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”. 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.¹⁷ Then, the interesting question for the reception of Lucas (1976) is not so much about the “path-breaking” or the

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

¹⁶ See Aldrich (1989) for a formal account of the notion of autonomy. See also Simon (1953) for a clarification of the meaning of structural or autonomous equations in econometrics.

¹⁷ For a comprehensive discussion of the disappearing of autonomy, see Qin (2014) and Aldrich (1989).

“original” character of Lucas’s work; it is rather about the reasons that made possible to re-open the discussion about the invariance of the structure of the macroeconomic models. And we think that this reopening owes much to the particular context of the 1970s and to the *imbroglio* behind Lucas’s article.

2 The Lucas Critique *imbroglio*

In the previous section we argued that the success of Lucas (1976) cannot be rationalized as a prescriptive methodological advancement. Conversely, we consider here that its success within macroeconomics owes much to its ambiguity. Indeed, Lucas’s paper is not restricted to display the Critique in its narrow sense, but it appears as embedded within two other proposals, a prescriptive one and a positive one: (1) the use of rational expectations in modeling, and (2) the explanation of the economic crisis of the 1970s, through the rejection of the output-inflation trade-off. This ambiguity is what we call the Lucas Critique *imbroglio*.

2.1 The Lucas Critique and its relation to rational expectations

As discussed above, the Lucas Critique is, in essence, a negative result, instructing macroeconomists about how not to practice macroeconomic modeling. Thus, the purpose of the Critique is basically prescriptive. It raises an internal criticism towards standard macroeconomic models that can be reduced to a “single syllogism” as Lucas pointed out at the end of his article:

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 of structural change was recalled, the following step was to determine to what extent standard macroeconomic models were able to deal with it. On that point, Lucas’s paper is still very moderate, and it suggests only that the issue might even be grasped within the traditional macroeconomic framework:

Perhaps the adaptive character of this early stage of macroeconomic 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)

The way Lucas justified his own skepticism was, indeed, highly subjective. Basically, Lucas relied on his conviction that agents' expectations are nothing but rational.¹⁸ Thus, even if the association between the Critique and the rational expectations is highly suggested in "Econometric Policy Evaluation", the Lucas Critique does not inevitably imply the rational expectations: the core assumption is that private agents are *forward-looking*.¹⁹ Therefore, rational expectations are a potential solution and not a necessary implication of the Critique. This distinction remains unclear nowadays, but even at the time of the Critique, very few macroeconometricians understood it this way. Sims (1982) emphasized the distinction, claiming that the Lucas Critique (what he confusingly calls "the rational expectations critique") and the "rational expectations hypothesis" should be analyzed separately: "The rational expectations assumption (...) is stronger than what is needed to justify the rational expectations critique" (Sims, 1982, pp.111-112).²⁰ Though, the reason for such an association between the Lucas Critique and the rational expectations is not mysterious at all. It simply appeared natural to Lucas, who actually never disentangled his Critique from his own research program based on rational expectations. Indeed, from an analytical as well as an historical point of view, we claim that it is not possible to disentangle the content of this article from Lucas's previous works. In order to capture

¹⁸ Lucas (1976) first, and implicitly, 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 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 they are rationally expected (*ibid.*, p.41).

¹⁹ The distinction between the Lucas Critique and rational expectations as a solution to it, can be found in Hoover (1988): "The important point [of the Lucas Critique] is not that agents have rational expectations, but that they take *some* account of the policy rules" Hoover (1988, p.190).

²⁰ And also: "fortunately, the rational expectations assumption is not the only logically consistent way to proceed with econometric policy analysis in the face of the rational expectations critique." (Sims, 1982, p.114). Sims is arguing here that rational expectations are a valid solution for the Lucas Critique only in the specific (and, actually, very rare) case of changes in policy *rules*.

this idea, we need to recall the genealogy of Lucas (1976).

Even if “Econometric Policy Evaluation: A Critique” was published in 1976, Lucas actually had already completed this work three years before, in early April 1973.²¹ 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*.²² After some modifications on May 1973, Lucas published this version of the paper as a Carnegie Mellon working paper (Lucas, 1973a)²³. The 1973 version is essentially the same 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 already quoted by some authors (Cooley and Prescott, 1976; Sargent, 1976). Thus, 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).

In “Expectations and the Neutrality of Money” (Lucas, 1972b) Lucas anticipates the Phillips curve example used in Lucas (1976), while in “Econometric Testing of the Natural Rate Hypothesis” Lucas (1972a) he anticipates more clearly and substantially the content of the Lucas Critique. In this article, Lucas wrote:

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)

Moreover, in “Econometric Testing of the Natural Rate Hypothesis” Lucas suggests a new econometric research path for testing two alternative versions of the inflation-output relationship (the standard Phillips curve and the “natural rate hypothesis”).²⁴ The econometric question of the invariant-parameters is brought to the fore: Lucas recommended a cross-equation test for assessing the consistence of macroeconometric models.

It would be wrong to consider the Lucas Critique as being autonomous from Lucas’s previous works, and especially from Lucas (1972a) and from Lucas (1972b); also, it would be wrong to consider it autonomous from Lucas’s

²¹ According to Sargent’s testimony (Sargent, 1996, p.539, fn.3).

²² 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).

²³ On the *variorum* of “Econometric Policy Evaluation”, see Young (2014).

²⁴ Lucas (1973b) is also concerned with the testing of the natural rate hypothesis.

wider effort to build an alternative research program in macroeconomics, based on the rational expectations hypothesis.²⁵

Here is the first ambiguity of Lucas (1976): the suggestion (or the possibility to assume) that the Lucas Critique necessary implies the rational expectations, either as the cause or as the (unique) solution for that issue. Again, such an association is present in Lucas (1976) and what is more, Lucas's works previous to the Critique can provide an interpretation of the Lucas Critique that goes in the direction of this ambiguity. From that perspective, it is not surprising that most of the commentators interpreted the rational expectations 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). We think, however, that there is a second ambiguity behind Lucas (1976): the existence of a positive scope in the argument.

2.2 The Lucas Critique and its positive scope

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

Now, once this major purpose of Lucas (1976) is recalled, it is important to wonder about the positive scope of the Lucas Critique. Regarding the economic context in the first years of the 1970s, there is no doubt that Lucas was concerned not only about the prescriptive issue, but about something more. In a nutshell, while inflation and unemployment were constantly rising, the stabilization policies turned out to be ineffective to counter stagflation, a phenomenon that seemed to refute the received Keynesian theoretical view at the time—namely, the hypothesis, based on the Phillips curve, of an existing trade-off between inflation and unemployment.

Though, and rather surprisingly, Lucas's paper remained very distant from current economic policy considerations. After some words on stabilization policies and on the possibility to permanently maintain a high rate of inflation to keep a low rate of unemployment (Lucas, 1976, p. 19), no other

²⁵ The agenda of the New Classical Macroeconomics's research program will be formulated later, in its most clear way by Lucas and Sargent (1979). For a synthetic account, see for instance De Vroey (2009, 2015).

mention is made of the economic context. Independently of the economic crisis of the 1970s, the stagflation or its plausible causes, Lucas's paper seems only concerned with the methodology of macroeconomic modeling, showing that this methodology is flawed. Actually, as we showed above, there is a strong connection between Lucas (1976) and his previous works about the "natural rate hypothesis" (Lucas, 1972b,a) . Such a connection should be seen not only as a connection in terms of the hypotheses (rational expectations) but also in terms of *results*: indeed, Lucas's work pointed out to the ineffectiveness of active, discretionary stabilization policy inspired by the Keynesian theoretical framework.²⁶ From here, it just takes one step to interpret the Lucas Critique as aiming at making the Keynesian policy-advisers responsible for stagflation.²⁷

Actually, this step would be reinforced a few years later by Lucas, in a paper co-authored with Thomas Sargent, called "After Keynesian Macroeconomics" (Lucas and Sargent, 1979) This paper provided the opportunity for Lucas to extend the scope of his Critique in a more positive direction. 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 the Keynesian models, especially from the point of view of the predicted trade-off between inflation and unemployment:

Many economists [in the late 1960s] urged a deliberate policy of inflation on the basis of this prediction. [...] 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 *economic failure* on a grand scale. (Lucas and Sargent, 1979, p.6, our emphasis)

Even if Lucas and Sargent definitely extended the Lucas Critique in a positive way, they also confined themselves to what we call the "weak thesis". Indeed,

²⁶ This result is, again, in the line with Friedman (1968). It was even made more radical by Kydland and Prescott (1977): "[we found that] stabilization efforts have the perverse effect of contributing to economic instability.[...] active stabilization may very well be dangerous and it is best that it not be attempted" (Kydland and Prescott, 1977, p.487).

²⁷ This part of responsibility was not a fancy of New Classics: Keynesian macroeconomists were actually well introduced in the field of policymaking. For instance, G. Ackley and A. Okun served as Chairmen of the Council of Economic Advisers in the late 1960s. Muchlinski (1999) already analyzed this issue.

they refused to attribute the failure of 1970s economic policies to Keynesian macroeconometric models: “Certainly the erratic ‘fits and starts’ character of actual U.S. policy in the 1970s cannot be attributed to recommendations based on Keynesian models” (*ibid.*). Their point was surely to stress the failure of U.S. expansive fiscal and monetary policies and of the inconsistencies of Keynesian models. Lucas and Sargent do not argue, however, that these inconsistencies (in particular the absence in the models of the 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.²⁸

Here again, beyond the circumlocutions of Lucas and Sargent, it is not surprising that a “strong thesis” had also been developed for giving an extended positive scope to the Lucas Critique. This is even less surprising if one considers, 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, 1972a,b). The stronger version of the Lucas Critique thesis implies going a step further and assuming that defective Keynesian models amplified the economic troubles of the 1970s (in particular the “great inflation”). This interpretation has been sufficiently widespread in macroeconomics since the 1970s and a clear expression of it can be found in Snowdon (2007)²⁹:

[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). (Snowdon, 2007, p.547)

At the end of this section, Lucas’s paper appear very much as an *imbroglia*, since the Lucas Critique about non-invariant parameters (such as we

²⁸ This ambiguity can also be found on the *policy-ineffectiveness proposition*. Despite the fact that New Classical 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 illegitimacy of such a link, see Hoover 1988, chapter 9).

²⁹ In addition to Tobin (1980b,a, 1981), see for instance Martin Baily: “Some observers suggest that a growing awareness of stabilization policy has caused the apparent inflationary bias that has characterized the post-war economy” Baily (1978, p.46). Even Sims takes up this conclusion, claiming that traditional macroeconometric policy analysis “is [nowadays] widely believed to be unjustifiable or even the primary source of recent problems of combined high inflation and low economic activity” (Sims, 1982, p.107).

synthesized it in section 1), is not disentangled from two other elements (that we have developed in this section): (1) the prescription for rational expectations hypothesis, and (2) the positive explanation of the economic crisis of the 1970s (which is close to the *policy ineffectiveness proposition* according to the “strong thesis”). From that perspective, the diffusion of the Lucas Critique must be understood as processed under this form of *imbroglio* (*i.e.* as a package of these three elements) so that the interpretation of Lucas (1976) by Keynesian macroeconometricians has been misleading during the 1970s. We will show how Keynesians were confused by this *imbroglio*, and provided divergent and partial responses to the Lucas Critique.

3 How to handle the Lucas Critique without rational expectations?

In this section, we try to synthesize the main ways to take into account the prescriptive side of the Lucas Critique. We discuss the Keynesian reactions to the New Classical solution, that is to say the use of the rational expectations. Lawrence R. Klein’s work in the 1970s and the 1980s constitutes not only a “paradigmatic” example of dealing with Lucas (1976), but also an alternative to the New Classical solution.

While most Keynesians acknowledged the non-invariance problem raised by the Lucas Critique, they also refused to take for granted the interpretation that rational expectations would constitute the unique valuable solution. Besides, as rational expectations were first seen as the key feature leading to the policy ineffectiveness proposition, it soon concentrated the major part of “angry” reactions.³⁰ The Keynesian opposition to the New Classical solution relied mostly on a realism basis. Quite simply, rational expectations are not a good description (or even a consistent approximation) of the *actual* way economic agents form their understanding of the future of the economy. Rather illustrative is the reaction of Franco Modigliani, who claimed (in his interview with Klammer) to reject the rational expectations hypothesis 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

³⁰ From that perspective, it is not a mere coincidence that the New Classical revolution was first christened the “Rational Expectation revolution”. It took 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.

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. (Klamer, 1984, pp.125-126)

Such trial on unrealism was similarly undertaken by most Keynesians, like Solow (1978) or Tobin (1981). Interestingly, the Keynesians started to react to the rational expectations only after Lucas (1976) whereas this hypothesis was less extensively developed in this paper than in Lucas (1972b). Hence, this focus on the rational expectations 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, the alternative solutions proposed by Keynesians were not necessarily homogeneous.

While some Keynesians kept assuming adaptive expectations (like several monetarists did), other acknowledged the need to rely on a more forward-looking concept of expectations. This is the line of research of Keynesians like George Katona and Lawrence R. Klein.

Klein's reaction was one of the most vigorous from the Keynesian faction, doing important efforts to develop alternative solutions to the Lucas Critique. Considering first rational expectations potentially as a sound way of treating the problem of expectations in macroeconomic models, Klein ended up rejecting this hypothesis because it was asking too much from the data, namely it "[was asking] 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” (*ibid.*). Hence, the use of the rational expectations in macroeconomic models do not allow to lay the foundations for a secure method in face of the Lucas Critique, simply because it is not a rigorous assumption about expectations formation, both intuitively (as in the Modigliani understanding) and in respect of the econometric practice. As an alternative, Klein (following the line developed for years by Katona, see Katona, 1980) advocated for a more realistic approach in the modeling of expectations, in order to rigorously derive a description about how people really react to changes in economic policy . This way of treating expectations, however, is inseparable from his wider research program.

Indeed, Klein carried a true alternative to the New Classical approach, developing a microfoundational program that (Hoover, 2012) labeled the “aggregation program”. In this program, “each of the Keynesian functions was analyzed at a microeconomic level and its implications for a feasible macroeconomic model [were] considered” (Hoover, 2012, p.41). Klein aimed at “disaggregating” the macroeconomic model “as far as the data [would] permit” (*ibid.*, p.51). Klein’s way of treating the problem of expectations typically follows this way of thinking. First of all, macroeconometricians did not wait for the Lucas Critique to integrate expectations 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 expectations 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 (see section 4), the problem raised by Lucas had to be taken into consideration.³¹ Following the fact that “the rational expectations school [had] raised some important questions about the dynamics of the macroeconomy” (Bodkin et al., 1991, p.553), Klein brought in some propositions to deal with the formation of expectations and with the agents’ reaction in the face of 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 the formation of expectations. 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 of inflation, the statistics on orders, the statistics on housing starts, and all the things we call anticipations variables to be very im-

³¹ Klein acknowledged, indeed, 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 modeling the supply side in macroeconomic models. Then, macroeconomists had to tackle, in priority, the modeling issues of the energy and raw materials sectors.

portant. 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 both to understand how agents conceive of macroeconomic variables and to determine what are the expectations for these particular variables. With this amount of data, the economist 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. Is there 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 this question, confronting Klein’s approach directly. Perhaps this is why Klein’s proposal did not seem to have found its way into the academic world. Yet, other than Klein’s, the alternative propositions to New Classical macroeconomic program concerning the Lucas Critique, seem to be rather scarce during the 1970s. In fact, the debates focused on the empirical question hidden behind Lucas (1976): do expectations and bad economic policies explain the disturbances of the 1970s?

4 The “positive” responses to Lucas

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. Thus, Lucas’s article, as a worthy heir of Friedman (1968), follows the “ideas hypothesis” trend of explanations of the 1970s, christened in this way by Christina Romer (2005). This stance defends the view that “economic ideas were the key source of the Great Inflation, and indeed most of the policy failures and successes of the postwar era” (Romer, 2005, p.177). Here, the bad economic idea would be the belief in a long run trade-off between inflation and unemployment³².

According to this perspective, Lucas (1976) was not a mere prescriptive paper questioning the methods of macroeconometricians. Lucas’s paper

³² We have already mentioned how dubious the existence of such a belief can be (see Introduction).

implicitly aimed at criticizing the economic policies of the 1960s as responsible of the stagflation of the U.S. economy. The question addressed to some Keynesians was to establish whether the simultaneous increase of inflation and unemployment was the result of changes in the policy regime and in the agents' expectations. In this way, Lucas's article led to an empirical dispute to assess the fundamental causes of the unfavourable economic situation of the U.S. during the 1970s. In general, while they acknowledged the importance of expectations in macroeconomics, Keynesians considered that stagflation forced them to amend and to enrich the "Consensus Macroeconomic Framework" of the 1970s (Tobin, 1980b). Instead of just throwing out the baby with the bathwater, macroeconometricians had to define a new research agenda that would allow them to take into account the events of the 1970s.

4.1 Testing the empirical validity of the Lucas Critique

Alan Blinder is emblematic of the empirical way to address the problem.³³ Blinder cast doubt on the legitimacy of the New Classical Revolution (Blinder, 1987, 1988).³⁴ While, during the 1980s, most of the young economists would 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

³³ In the early 1970s, Blinder was Professor at Princeton and Research Associate of the NBER. In 1975, he was Deputy Assistant Director of the Congressional Budget Office (CBO), criticized by Miller and Rolnick (1980) (see section 1.1 above).

³⁴ Even if in a very crude version, the empirical refutation of Blinder was already present in his book on stagflation (Blinder, 1979, p. 92).

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. Economists “put two and two together and jumped like lemmings to the wrong conclusion”(Blinder, 1988, p.283). Economists who saw the rising inflation and the changes in the correlation between inflation and unemployment, considered that the Lucas Critique would explain all these movements: for them “the government had adopted a more inflationary policy” (*ibid.*).

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, claiming that there was no proof whatsoever that the disappearance of the traditional Phillips curve was the consequence of a change in the agents’ economic behavior in response to a change in policy or in the economic environment:

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. (Blinder, 1988, p. 278)³⁵

Blinder acknowledged that the Critique might be true and that the modeler had to take some precautions in interpreting their results. But he also argued that if one wants to work in a scientific way, one has to check whether 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:

³⁵ This anti-empirical nature of the Lucas Critique was already put forward in 1984 by Blinder in an interview with Arjo Klamer: “All you have to do in this country (...) 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).

It is indeed remarkable that the Lucas policy evaluation critique has triumphed without any detailed empirical support beyond Lucas's accusation that macroeconomic 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)

Some Keynesians applied themselves to assessing the empirical relevance of the Lucas Critique. Blinder (1988) run F-tests on several autoregressions to observe if the rise in inflation in the early 1970s was the result of a change in expectations. A Phillips equation consistent with Lucas' views could be:

EQUATION

However, in order to test this equation, we consider that $A(L)p_{t-1}$ represents a good proxy of the expectation variable $E_{t-1}(p_t)$. If the equation (??) with $A(L)p_{t-1}$ as a proxy continues to fit the data, it means that there was no fundamental change in the way agents formed their expectations. Blinder estimated several simple autoregressions for the period 1955:2 to 1987:4, searching for statistically significant breaks. He concluded that: "None of these F statistics is remotely close to conventional significance levels. Thus, there is no evidence for a shift in the lag coefficients A(L)" (Blinder, 1988, p.283).

Blanchard (1984) enroled in the same way, taking as point of departure the Volcker deflation which constituted a true change in the policy regime (the Fed had changed the interest rate targeting to money stock. He tried to assess the effect of the change in monetary policy on the Phillips equation structure and on the term structure of interest rates. He used the Phillips curve of the Data Resources Incorporated (DRI) model, as it was designed and estimated in 1978. He then studied how the coefficients had changed each time he had added a new year in the sample. He concluded for the stability of the Phillips curve relation, arguing that:

This in no way implies that the above relation is a correctly specified, structural relation, only that the movement of wage inflation, given unemployment, has not been strongly affected by the policy change. This may be due either to unchanged ways of forming expectations, or to expectations playing little role in the determination of wage inflation (Blanchard, 1984, pp.213-214)³⁶

³⁶ Englander and Los (1983) found the same type of results.

Concerning interest rates, however, Blanchard acknowledged that “expectations appear to have changed and the term structure is very much subject to the Lucas Critique” (Blanchard, 1984, p.214).

Otto Eckstein (1978, 1983), one of the co-founders of the DRI model in 1969, tried to assess the relevant determinants of the crisis and the validity of the rational expectations hypothesis, using a different method. He computed simulations on the DRI model Eckstein (1978) to study the impacts of six major shocks on the rate of unemployment and inflation. He then looked if the results of the simulations mimicked the actual data. He showed that the major causes of 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 in both inflation and unemployment.

These results have probably encouraged Keynesians to dismiss the “expectational” tale (CITER TOBIN) for stagflation. Even if they recognized the role played, for example, by expectations in economic mechanisms (section 3), they considered that the research priority was somewhere else.

4.2 What should economists do after the 1970S?

A part of the Keynesian 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.

First, Keynesians considered that the Phillips curve was not death in the 1970s. Very quickly, the curve was adjusted by adding an expectational term in the equation, following the works of Friedman (1968) and Phelps (1967). Besides, if the traditional Phillips curve (or the “augmented” Phillips curve) had statistically disappeared in the 1970s, this was so because of supply shocks that were not taken into account:

But that was a failure of theory, which was repaired in the late

sixties, following work by Friedman and Phelps, by adding the expected inflation rate to the Phillips curve. The 1973 supply shock also led to an underprediction of inflation in the major models, but that has nothing to do with the Lucas critique—unless the Lucas critique is reduced to the statement that models are inevitably misspecified. (Fischer, 1988, p.302)

According to Blinder (1987, p.133), once we have added some expectational variables and supply shocks in the Phillips curve equation, we could fit the data pretty accurately for the 1970s. Hence, the Keynesian framework was still valid and Lucas (and Sargent) had gotten too far.

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).³⁷ This assertion is at odds with Lucas’s argument on the impossibility of evaluating economic policies by means of econometric models. Large-scale macroeconomic models *à la* Klein and Goldberger (1955), 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 macroeconomic 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, according to Klein, modeling the energy and raw materials sectors, instead of introducing expectations to the models, was at the top of the research agenda. Klein 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,

³⁷ We did not succeed in obtaining 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”.

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).

This reflects the the alternative way that Klein had already promoted in his presidential address to the *AEA* (Klein, 1978, TO BE DEVELOPED). The research project and effort was already in progress, as Eckstein (1976) illustrates. Eckstein considered at that time that one of “the most exciting econometric research today is in the fields of energy, capacity planning, food supply” (Eckstein, 1976, p.19). Indeed, once macroeconomists had acknowledged that “the models failed because they did not properly allow for [understanding] the full impact of the food and energy situations”, it became a priority to rebuild the models (including the DRI model in particular) “in order to cope better with the new sources of instability” (Eckstein, 1976, p.17).

In his conclusion to the conference held at the Federal Bank of Boston, “After the Phillips Curve: Persistence of High Inflation and High Unemployment”, Solow seemed to sum up well the Keynesian empirical position.³⁸ 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 [even if] 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. 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 in the face of the positive side of the Lucas Critique, i.e. Lucas’s interpretation of stagflation in the 1970s. The aim of this section was to clarify what was the focus of these reactions, on the one hand, in providing empirical evidences against the implications of the Lucas Critique and, on the other hand, in constructing a reliable explanation of the stagflation within the traditional macroeconometric modeling approach.

³⁸ This is also the conference where Lucas and Sargent presented their polemical paper, “After Keynesian Macroeconomics” (Lucas and Sargent, 1979).

Concluding Remarks

Our paper suggested an interpretation of the Lucas Critique that allow to build an ordered comprehension of the heterogeneous reactions of Keynesian macroeconometricians against Lucas (1976). We provided some examples to illustrate the usefulness of this interpretation. Our general claim is that a deep understanding of these responses must be based on the appraisal of the Lucas Critique *imbroglio*, i.e. of the (con)fusion between two registers: the prescriptive and the positive interpretation of the Lucas Critique. The prescriptive interpretation implies a general principle for building consistent macroeconomic models, providing sound policy analysis: one must avoid policy-invariant parameters and, in order to do that, one must use rational expectations. We insisted on the fact that the first level of the argument (policy-invariance of parameters) was nothing more than a revival of Frisch's critique of Tinbergen, following the concept of autonomy; rational expectations are the actual innovation bring by Lucas to the question, but they should be regarded as a part of the general part of the New Classical research program, rather than as the fundamental argument of Lucas (1976). 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 macroeconomic 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 the main reason of the disruptive effect of Lucas (1976). Attracted by the stagflation issue and the policy ineffectiveness proposition, Keynesian macroeconometricians rarely answered directly to the Lucas Critique about policy-invariant parameters and seemed to dodge the problem.

Our opinion is that further research 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) . The second 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 turn in the history of the discipline, especially in its conception of the role of models and in their relation with theory and with the empirical world. New Classical macroeconomics deeply challenge the modeling practices in the Keynesian tradition, 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.

For all these reasons, we can 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 bring about a deeper understanding of the evolution and current state of the discipline of macroeconomics.

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