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► **To cite this version:**

Aurélien Goutsmedt, Erich Pinzón-Fuchs, Matthieu Renault, Francesco Sergi. Reacting to the Lucas Critique: The Keynesians' Pragmatic Replies. 2017. <halshs-01625169>

HAL Id: halshs-01625169

<https://shs.hal.science/halshs-01625169v1>

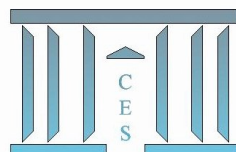
Submitted on 27 Oct 2017

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2017.42



Reacting to the Lucas Critique: The Keynesians' Pragmatic Replies

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Abstract

We illustrate how the Lucas Critique was called into question by Keynesian macroeconomists during the 1970s and 1980s. Our claim is that Keynesians' reactions were carried out from a pragmatic approach, which addressed the empirical and practical relevance of the Critique. Keynesians rejected the Critique as a general principle with no relevance for concrete macroeconomic practice; their rejection relied on econometric investigations and contextual analysis of the U.S. 1970s stagflation and its aftermath. Keynesians argued that the parameters of their models remained stable across this period, and that simpler ways to account for stagflation (such as the introduction of supply shocks into their models) provided better alternatives to improve policy evaluation.

Keywords: History of macroeconomics; Lucas Critique; Keynesian macroeconomics; Stagflation

JEL codes: B22; B41; E60; E12

¹ Corresponding author: Aurelien.Goutsmedt@univ-paris1.fr. We would like to thank Marcel Boumans and the participants of the conference on the "History of macroeconomic modelling" at Utrecht University for their comments on the paper. We would also like to thank Mickaël Assous, Antoine d'Autumne, Michel De Vroey, Pedro Garcia Duarte, and Kevin D. Hoover for their valuable help on preliminary versions of this paper. The usual caveat applies.

[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.

Stanley Fischer (1996, 21)

Introduction

In his “Econometric Policy Evaluation: A Critique” Robert Lucas (1976) explicitly criticized Keynesian mainstream macroeconomic models for their inability to correctly predict the effects of alternative economic policies (Lucas, 1976, 20).² Lucas summarizes his argument against these models into a “single syllogism”:

given that the 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 (*ibid.*, 41).

According to this view, Lucas’s criticism would be just a matter of logic in which (A) the structure of an econometric model is based on relations that describe the behavior of economic agents; (B) this behavior changes along with changes in the decisions of policy-makers; and (C) the structure of the model also changes along with changes in economic policy.³ Following the logical construction of his argument, Lucas (1976) drew a prescriptive methodological principle: in order to build models that are reliable for quantitative policy evaluation, modelers should formulate behavioral equations that take into account individuals’

² Lucas (1976, 19, fn.2; 21) explicitly referred to Klein and Goldberger (1955) and Tinbergen (1952), even if his Critique targeted more generally the various models stemming from these works. Hereafter, we will characterize this line of work as “Keynesian macroeconometrics,” as is common in the historiographical literature. Yet, it is important to recall that “Keynesian macroeconometrics” results from diverse theoretical influences (see Pinzón-Fuchs, 2014). For a more comprehensive view on the methodology of the Keynesian macroeconometrics, see Renault (2016), Goutsmedt (2017) and Pinzón-Fuchs (2017).

responses to changes in policies. In addition, this logically grounded prescriptive principle also set the bases for the criticism of the Keynesian models, which, according to Lucas, did not abide by this basic principle, and therefore conducted to misleading policy evaluations.⁴

Today, most contemporary macroeconomists and some historians of economics interpret the Lucas Critique as a “syllogism,” arguing that it was (and still is) logically unquestionable, and that it led forcefully to an immediate disqualification of the Keynesian macroeconometric approach. This interpretation constitutes the “standard narrative” of the history of the Lucas Critique (Hall, 1996; Mankiw, 1990; Snowdon and Vane, 2005; Woodford, 2003), in which Lucas (1976) is commonly regarded as a path-breaking innovation that dismissed old-fashion, flawed modeling practices.⁵

We consider that this interpretation is highly incomplete, not to say biased, for two main reasons. First, because Keynesians’ point of view is missing in this history, even if this group of macroeconomists were the targets of the Critique. Yet, throughout the 1970s and 1980s, there was a fierce reaction from the older and younger generations of Keynesians. Second, we consider the standard narrative to be incomplete, because the Lucas Critique was far more complex than just a syllogism that relied exclusively on logical grounds. Indeed, Lucas (1976) presents, at least, a twofold dimension. A methodological dimension, which highlighted a limitation in the Keynesian practice by calling into question the stability of the structural parameters of large-scale macroeconometric models. And a positive dimension, which suggested that the 1970s stagflation had been produced by a change in the behavior of economic agents provoked, in turn, by a change in economic policy. Emphasizing this multi-

³ See the Appendix for a more comprehensive overview of Lucas’s argument.

⁴ Lucas conceded, indeed, that macroeconometric models were “well designed” for addressing questions “unrelated to quantitative policy evaluation” such as “short-term forecasting” (*ibid.*).

⁵ The “standard narrative” is the common depiction of the recent evolution of a discipline produced by its practitioners and adopted by some historians. For a more general description of the standard narrative of the history of macroeconomics, see Duarte and Lima (2012) and Sergi (2017).

dimensional character of the Lucas Critique is important to understand the Keynesians' reaction, which disputed the relevance of the Critique along these two different dimensions.⁶

In section 1, we show that the standard narrative is historically misleading. After illustrating how the interpretation of the Lucas Critique as a syllogism is widespread, we address the old Keynesians reactions to it. Old Keynesians, such as Franco Modigliani, James Tobin, Robert Solow, and Edmond Malinvaud basically considered the Lucas Critique a postulate whose empirical significance and scope were still to be demonstrated. In doing so, the old Keynesians raised a fundamental objection against the Lucas Critique by simply asking when (if at all) this principle was relevant to economic policy evaluation. Even if their answer to this question was *a priori* negative, they were rather willing to consider that the burden of proof of the relevance of the Critique rested on the new Classical macroeconomists.

In section 2, we focus on the reactions of the younger generation of Keynesian macroeconomists—mainly those of Alan Blinder, Stanley Fischer, Olivier Blanchard, Robert Gordon, and Stephen R. King. Conversely to the old Keynesians, the younger generation was willing to test directly the empirical relevance of the Critique. Concerned with the macroeconomic context of the 1970s and the stability of the Phillips Curve, this younger generation studied parameter instability in their models through various econometric tests. These tests led them to conclude that the kind of instability assumed by Lucas (1976) could not be observed, and so, its Critique was to be refuted as empirically irrelevant.

In section 3, we consider the way in which Keynesians questioned the positive dimension of the Lucas Critique. In particular, we study the reactions of Lawrence R. Klein, Stanley Fischer, Robert Gordon, Otto Eckstein, and Robert Solow, who recognized that their models had not performed at their best during the stagflation period, and who, taking Lucas's

⁶ The Appendix suggests some elements for a further analysis of the Lucas Critique as a multidimensional argument, involving theoretical and empirical insights. In other words, we argue that the idea of the Lucas Critique as a syllogism is neither accurate nor faithful to Lucas's own understanding of this principle. For additional insight on this matter, see an earlier version of this paper (Goutsmedt *et al.*, 2015).

argument seriously into account, asked whether it provided a viable alternative to understand the economic context of the 1970s. Once again Keynesians considered that the Critique was not relevant for this purpose, and that alternative and simpler ways existed to account for stagflation, such as the introduction of supply shocks.

1 The Lucas Critique in the history of macroeconomics

The widespread interpretation of the Critique

Consistently with Lucas's own summary of his argument as a syllogism (*cf. supra*), the Lucas Critique is often understood as a postulate for economic reasoning—a kind of logical axiom or a fundamental principle for producing consistent policy evaluation.

Lucas argued that models conceived for policy evaluation should necessarily involve a careful description of the changes in the behavior of economic agents as a reaction to changes in economic policy rules. Although this argument is usually associated with the rational expectations hypothesis, it actually relies on a simpler and more intuitive idea: that since economic agents take into account government decisions to adjust their behavior, the government should formulate its policy considering people's reactions to its decisions. This simple interpretation of the Lucas Critique is widespread among macroeconomists and historians.

For instance, in his 1985 Preface to *Rational Expectations and Inflation* (a collection of his works of the early 1980s), Thomas Sargent reformulated Lucas's fundamental principle into the idea of "strategic interdependence":

one person's pattern of behaviour depends on the behaviour patterns of those forming his environment. When behaviour patterns of those forming a person's

environment change, the individual can usually profit by adjusting his or her own behaviour pattern (Sargent, 2013, xxii).⁷

Already in his “Rational Expectations and the Reconstruction of Macroeconomics,” Sargent (1980) had presented his idea of “strategic interdependence” through an example drawn from football (namely, how a change in the offside rule would affect players’ behavior). According to Sargent, that example indicated that:

historical patterns of human behaviour often depend on the rules of the game in which people are participating. Since much human behaviour is purposeful, it makes sense to expect that it will change to take advantage of changes in the rules (*ibid.*, 15).

Sargent endorsed the syllogism underlying the Lucas Critique, resulting in the fundamental principle that individuals adapt their behavior in order “to take advantage of changes in the rules.” Although it might appear “trivial to football supporters,” this principle brings important consequences for macroeconometric modeling:

This [...] principle very much deserves mentioning in the context of economic policy because here it has been routinely ignored—and with some devastating results. Adherents of the theory of rational expectations believe, in fact, that no less than the field of macroeconomics must be reconstructed in order to take account of this principle of human behaviour (*ibid.*).

Hence, strategic interdependence constituted a fundamental principle of “human individual behaviour.” According to Sargent, macroeconomists who ignored that principle (as the Keynesians allegedly did), built their expertise on fallacious foundations, and caused “devastating results.”

⁷ The main practical implication of this argument is that “monetary and fiscal policies must be coordinated” (*ibid.*): .): if public deficit increases because of an expansionary fiscal policy, private agents will mistrust

Reading the Lucas Critique as a fundamental postulate for economic reasoning is equally current in today's economics. In his entry in the *New Palgrave Dictionary of Economics* "Lucas Critique," Lars Ljungqvist (2008) presents the problem raised by Lucas as a problem of "economic *logic*." According to Ljungqvist, Lucas criticized "the prevailing approach to quantitative macroeconomic policy evaluation for ignoring this *logic* [and for] being *fundamentally inconsistent* with economic theory" (Ljungqvist, 2008, 1, our emphases). In this sense, the Critique is considered a matter of internal consistency of the logical structure of a macroeconometric model.

Finally, some historians of macroeconomics hold the same interpretation of the Lucas Critique as a postulate. Michel De Vroey (2015, 166), for instance, presents the Critique as an argument about internal consistency, and suggests that the "best summary" of Lucas's argument is a "syllogism". Furthermore, in accordance to Sargent's and Ljungqvist's interpretations, De Vroey (*ibid.*) argues that, for Lucas, Keynesian macroeconometric models were unable to provide sound policy evaluations, because they failed to take into account "the fact that agents change their decisions when faced with a change in the policy regime" (*ibid.*).

The Lucas Critique: A cornerstone in the standard narrative of the history of macroeconomics

Interpreted as a fundamental principle, the Lucas Critique is supposed to have provided the ultimate argument against Keynesian macroeconomics. Preston Miller, for instance, argues that "the Lucas Critique was fatal and [that after it] new approaches had to be developed" (Miller, 1994, xv). To the followers of this view, the Critique was a point of no return for macroeconomics that created the opportunity for the rise of New Classical macroeconomics

monetary policy (fearing debt monetization) and will not adapt their behaviour in a way that enables to reduce inflation.

while disqualifying the Keynesian approach. Robert Hall underlined the “revolutionary” nature of Lucas’s argument as well, after his awarding of the Nobel Prize in economics:

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, 38).⁸

This interpretation corresponds to the standard narrative of the history of macroeconomics which is so widespread among modern macroeconomists. This standard account arises from the interpretation of the Lucas Critique as a postulate, as a matter of logic, and thus as a “fatal” argument against the Keynesian macroeconomic practice. Following this perspective, any reply to the Lucas Critique by Keynesian macroeconomists should be disregarded as some desperate (and perhaps intellectually dishonest) attempt to preserve a flawed and degenerative research program. In terms of logic, indeed, one cannot argue against Lucas’s syllogism, which is partly why the standard narrative has ignored the Keynesians responses of the 1970s and 1980s. And yet, again, Lucas’s explicit target in 1976 was Keynesian macroeconomic modeling, and so this approach and its responses should not be ignored in the history of the discipline.

Furthermore, the standard narrative plays a crucial role in legitimizing the current “standard” modeling practices. Indeed, such a narrative endorses the current status of macroeconomic modeling in which Lucas (1976) is considered a methodological cornerstone,

⁸ Note that the standard history of the Lucas Critique is also accompanied by a standard history of the Phillips Curve that Forder (2014) has called the “Phillips Curve Myth.” We do not address this issue here, among other reasons because many of the authors here studied bought into this history.

an indispensable postulate for a scientific macroeconomic practice. In his emblematic book *Interest and Prices*, Michael Woodford states, for instance, that the first basic principle for building consistent macroeconomic models today 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, 13).

The old Keynesians’ replies to the Critique

Despite their being ignored by the standard narrative, the Keynesians did reply to the Lucas Critique during the 1970s and the 1980s. And so, a complete historical treatment of the Critique needs to include these arguments and points of view, starting with the older generation of Keynesians (Klein, Modigliani, Tobin, Solow, and Malinvaud).

Overall, the old Keynesians acknowledged that, *in theory*, Lucas (1976) had pointed out a relevant limitation of their macroeconometric modeling *practice*—namely, the study of agents’ expectations. This is an important point, since the old Keynesians’ reactions to the Critique are entangled with their criticism of the rational expectations hypothesis (Tobin, 1981; Solow, 1978; Malinvaud, 2007). Since Lucas (1976) used the rational expectation hypothesis to present his argument, it is not surprising to see some Keynesians criticizing simultaneously the rational expectations hypothesis and the Lucas Critique. To them, indeed, this hypothesis simply did not solve the problem highlighted by the Critique.⁹ This seems to be quite clear to Modigliani:

[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

⁹ Some Keynesians (especially Klein, Tobin, and Malinvaud) claimed that methods developed by George Katona would better fit to understand the state and evolution of expectations (Goutsmedt *et al.*, 2015).

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 (Modigliani in Klamer, 1984, 125-126).

Yet, the decisive point in the old Keynesians reaction is elsewhere. Keynesians never took for granted the empirical relevance of the Lucas Critique. In fact, they regarded Lucas (1976) essentially as a theoretical postulate, which was still to be tested for its practical relevance for economic policy, and for its ability to be integrated into large scale macroeconomic modeling. To them, the Lucas Critique was regarded as a postulate, whose empirical relevance and scope had to be carefully studied. Hence, the old Keynesians basic reaction to the Lucas Critique consisted simply in asking when (if at all) this principle was relevant to economic policy evaluation.

Old Keynesians also had an *a priori* judgment on the relevance of the Critique for their modeling practices. An insightful illustration of this stance is Malinvaud (1998, 335), who argued that economic agents consider changes in economic policy only when these affect them directly. Furthermore, Malinvaud insisted that neither Lucas nor the New Classical macroeconomists had tried to test the empirical validity of the Critique:

At the time, many macroeconomists, especially [me], were not convinced of the scope of [the Lucas Critique], although they recognized the correctness of the remark that inspired it. Indeed the small illustrative models presented by Lucas and others, showed no more than a possibility and were in no way tested as to their empirical validity (Malinvaud, 1997, 21).

To Malinvaud (1998, 335), this lack of empirical testing was even more deplorable since the recent economic context had provided several testing opportunities—such, for instance, the

shift to an interest rate target in monetary policies, or the change in the wage-setting rules in France in the early 1980s. The important point here is that, to Malinvaud and the old Keynesians, the burden of the proof of the empirical relevance of the Critique rested on its promoters, and so, the Critique should by no means be admitted *a priori* as a good postulate.

The old Keynesians' general stance towards the Lucas Critique can thus be easily characterized as an external criticism, first because the Keynesians did not contest its logical consistency, and, above all, they never took its empirical and practical relevance for granted.¹⁰ By refusing to enter the game of New Classical economists, and by considering that the burden of the proof of the relevance of the Critique rested on the them, the old Keynesians did not go beyond this external criticism. On the contrary, the younger generation did not hesitate to push this external criticism further, testing the empirical relevance of the Critique.

2 Econometric investigations of the younger Keynesians and empirical relevance of the Lucas Critique

Keynesian macroeconometricians replied to the Lucas Critique by questioning its empirical relevance. As just mentioned, this position, however, does not imply that the Critique *per se*, as an axiom for economic reasoning, is not consistent or important. The old Keynesians considered that the burden of proof of empirical relevance rested on the New Classical approach; as such proof was absent, old Keynesians rejected *a priori* the Lucas Critique as not relevant for practical purposes.

Conversely, the younger generation of Keynesians used empirical methods to establish the cases when the Lucas Critique was useful and necessary for economic analysis and when

¹⁰ Another illustration of this general stance is Solow's famous claim that "the last thing I want to do with [someone pretending to be Napoleon Bonaparte] is to get involved in a technical discussion of cavalry tactics at the battle of Austerlitz. [...] Now, Bob Lucas and Tom Sargent like nothing better than to get drawn into technical discussions, because then you have tacitly gone along with their fundamental assumptions" (Solow in Klamer, 1984, 146).

it was not. More precisely, young Keynesians ran econometric tests on the structural relations of their models, and searched for structural breaks and parameters instability after a change in policy rules—as predicted by the Critique. The empirical analysis of the Phillips curve—which was crucial in Keynesian models for discussing the role of fiscal and monetary policies—played a central role in these tests.¹¹ The simple question that Keynesians were willing to address was whether actual changes in policy rules (such as the inflationary monetary policy of the 1970s, or the “Volcker experience” in 1979) had truly led to a change in behavioral relations underlying Keynesian models—as suggested by Lucas.

We would like to emphasize two distinctive characteristics of younger Keynesians’ replies. First, they all were marked by the contemporary policy debates on stagflation and disinflation policy; and second, that they all were largely determined by the Keynesians’ pragmatic view on macroeconomic modeling.

The Keynesian econometric replies to the Critique appeared only at the end of the 1970s with the publication of Blinder (1979), although Lucas’s paper had been well-known and discussed in academia since 1973.¹² That the Keynesian reactions came up relatively late might be explained by the fact that, during the 1970s, Lucas’s argument was getting detached from Lucas’s original paper, and weaponized as a generic argument for academic debates.¹³ Moreover, in the specific context of the US stagflation, the Critique progressively gained influence in the public debate. In fact, it had been discussed at the US House of

¹¹ Note that focusing on structural breaks in the Phillips curve only is a narrow enterprise compared to the global perspective developed by Lucas (1976). Indeed, in Lucas’s paper, the explicit discussion of the output-inflation trade-off was presented in section 5.3 of the paper and only as one illustration (out of three) of the general argument of the Lucas Critique. The Keynesian focus on the Phillips curve is rather related to the crucial role of the curve in other Lucas works Lucas (1972a, 1972b, 1973). In addition, when Lucas revisited his Critique in a joint paper with Sargent (Lucas and Sargent, 1979), they focused on the Phillips curve to illustrate their point. In the early 1980s Sargent’s articles collected in *Rational Expectations and Inflation* (Sargent, 2013) he used the rational expectations and the Lucas critique argument to explain different periods of inflation and to assess different disinflation policies. The Phillips curve is consequently at the heart of Sargent’s work (see Goutsmedt, 2017, Chapter 1).

¹² On the history of Lucas’s paper, see the Appendix.

¹³ Something that Lucas himself acknowledged later: “My paper, ‘Econometric Policy Evaluation: A Critique’ was written in the early 70s [...] But the term ‘Lucas critique’ has survived, long after that original context has disappeared. It has a life of its own and means different things to different people.” (Lucas, 2012)

Representatives on February 22, 1977, and two articles reporting on this issue had been published in *The Wall Street Journal* on April and June 1977 (Lucas, Archives, Box 3, Folder 1977 2/2). In this sense, the Keynesian replies seem to have arisen as the enlarged audience of the Critique started to challenge Keynesians' influence on policy debates.

The reliability of Keynesian macroeconomic models in terms of policy evaluation became a major issue during the period of 1979-1981. In fact, Paul Volcker's appointment as chairman of the Federal Reserve in August 1979 brought an important change in monetary policy—consisting on the tightening of monetary policy and the switch to inflation targeting. Such change created an opportunity to debate both the efficiency of disinflationary policy, and the empirical relevance of the Lucas Critique.

The second distinctive characteristic of the Keynesians replies, rests on their common view of macroeconomic modeling practice—of its purpose and methods—which was deeply rooted in the macroeconomic approach back in the 1940s and 1950s. In this view, macroeconomic modeling was conceived as a practice driven by econometric work that addressed concrete economic questions within a specific context, and that provided daily expertise in matters of economic policy. In this sense, the Keynesian replies focused on the econometric analysis of parameters' stability. This line of work resulted in the rejection of the Lucas Critique, which did not seem to provide empirically relevant evidence to explain stagflation, evaluate disinflationary policies, or put into question the Phillips curve.¹⁴

In addition, as Lucas himself recognized (Lucas, 1976, 20, fn.2), the problem of structural change in macroeconomic models had already been addressed by the “founding fathers” of econometrics, especially by Trygve Haavelmo (1944, 27), Jan Tinbergen (1956, Chap. 5) and Jacob Marschak (1953, 8, 25). These early econometricians tackled this problem

¹⁴ Lucas's explanation of stagflation, rooted in the Critique argument, could be summarized as follow: due to a more inflationary monetary policy, private agents change their expectations about future prices. As they expect higher prices (and wages) in the future, agents substitute current income with leisure, thus decrease their current

from a pragmatic and empirical perspective, considering it relevant for econometric policy evaluation *in particular cases*.¹⁵ The 1970s-1980s Keynesians replies to Lucas are marked by an important inheritance of this macroeconometric tradition.¹⁶

Alain Blinder on the U.S. stagflation

After the first oil shock of October 1973, the empirical relation between inflation and unemployment—known as the Phillips curve—seemed to disappear. Inflation and unemployment rose simultaneously for several years, which, according to Blinder,¹⁷ generated a favorable context for the emergence of New Classical ideas such as the Lucas Critique.¹⁸ For Blinder, the stability of the Phillips curve was the central issue to be addressed in response to the Lucas Critique. Indeed, by dismissing the traditional inverse relation between inflation and unemployment, the Critique seemed to offer not only an explanation for the observed disappearance of the Phillips curve, but also of the inability of Keynesian models to foresee this event, as well as a reason for dismissing Keynesian expertise in economic policy.¹⁹

However, Blinder's conviction was that this interpretation of stagflation was simply not relevant. Retrospectively, in "The Fall and Rise of Keynesian Economics," he argued that the success of the Lucas Critique had been the result of a bad inference according to which

labor supply. This leads to a simultaneous rise in inflation and a decrease in output—thus expansionary monetary policy is ineffective in boosting output and employment.

¹⁵ See for instance the case of taxation in Tinbergen (1956, 151-153). Conversely, Ragnar Frisch (1938) claimed that the problem of structural change would seriously undermine the macroeconometric program started by Tinbergen (1939). Hence, Frisch suggested an alternative program, relying on the notion of autonomy (Aldrich, 1989). However, Frisch's idea was quickly defeated by Tinbergen's structural models (Qin, 2011).

¹⁶ Further research in this line involves empirical investigations on the Lucas Critique carried out by LSE econometricians in the late 1980s and early 1990 (see Ericsson and Irons, 1995)

¹⁷ In the 1970s, Blinder was Professor at Princeton and Research Associate of the NBER. In 1975, he served as Deputy Assistant Director of the Congressional Budget Office (CBO).

¹⁸ Benjamin Friedman (1985) also emphasized the role played by the economic situation at that time in favoring the ideas of the New Classical macroeconomics.

¹⁹ Blinder's argument refers to Lucas and Sargent (1979)'s claim that the stagflation period represented an "econometric failure on a grand scale" for Keynesian models, and that misleading predictions inspired inflationary policies (Lucas and Sargent, 1979, 6).

economists “put two and two together and jumped like lemmings to the wrong conclusion” (Blinder, 1988, 278). Those economists who saw the rising inflation and the changes in the correlation between inflation and unemployment thought not only that “the government had adopted a more inflationary policy” (*ibid.*), but also that the Lucas Critique would explain all these movements. Blinder revolted against that use of the Critique, and claimed that there was no proof whatsoever that the disappearance of the traditional Phillips curve during the 1970s was the consequence of a change in the behavior of economic agents who responded to a change in economic policy or in the economic environment. Relying on his simple test of the stability of the Phillips curve (*cf. infra*), he denounced that the Lucas critique had been “uncritically accepted,” meaning that it had been accepted without any serious empirical investigation. Indeed, to Blinder (1988), promoters of the Lucas Critique did not seek for evidence, but rather “became econometric nihilists. Theory, not data, was supposed to answer [...] questions” about the changes in the behavior of agents in response to policy changes.²⁰ Blinder had already put forward such lack of empirical evidence in favor of the Lucas Critique, in his 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 (Blinder in Klamer, 1984, 166).

Blinder forcefully casts doubt on the legitimacy of this attitude, and brought then the discussion to an epistemological ground. According to him, the success of the Lucas Critique (and broadly, of New Classical macroeconomics) was a theoretical revolution, but without

²⁰ To be sure, however, this remark seems quite unfair, since Lucas and Sargent explicitly advocated the continuation of empirical work on the Lucas Critique (see the Appendix).

any empirical support. Besides, this is, according to Blinder, contrary to what one should expect from the evolution of knowledge in a scientific discipline:

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 [...] (Blinder, 1988, 278).

In short, Blinder argued that empirical evidence (checking whether changes in the economic behavior of agents had a true and substantial effect on the relation of interest) is the cornerstone of scientific practice, and that the Lucas Critique does not abide by this standard. Other Keynesians defended this general epistemological stance, such as Stanley Fischer, who regarded macroeconomics as an empirical science—i.e., as a discipline in which empirical work provided the fundamental basis of scientific method.²¹ To Fischer (1983), it was “indeed remarkable that the Lucas policy evaluation critique” had “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 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, 271).²²

²¹ At this time, Fischer was professor at MIT (1978-1988); later, he served as Chief economist at the World Bank (1988-1990), and as First Deputy Managing Director at the IMF (1994-2000).

²² Lucas responded to Fischer in a letter expressing his “surprise [...] at [Fischer’s] statement that ‘the Lucas policy evaluation critique has triumphed without any detailed empirical support’[...] Really, [...] there is a mountain of evidence on this point.” (Lucas Archives, Box 5, Folder: 1982 1/2, Lucas to Fischer, 17/11/1981). In a further letter, Fischer dismissed this “mountain of evidence” as just “examples, not empirical demonstrations”

These strong statements about the empirical irrelevance of the Lucas Critique were the result of several econometric tests. Blinder (1979) was (chronologically) the first to address the Critique by considering a simple empirical Phillips curve:²³

$$p_t = A(L)p_{t-1} + f(u_t) + e_t$$

Where p_t represents present (t) and past ($t-1$) inflation, $A(L)$ are distributed lags, $f(u_t)$ is a function of current unemployment rate, and e_t a residual. In this equation, $A(L)p_{t-1}$ should represent a good proxy for the expectation variable $E_{t-1}(p_t)$. Let's suppose an increase in the mean level of inflation. If the estimated equation with $A(L)p_{t-1}$ as a proxy continues to fit the data, this means that, despite the change in their environment, there was no fundamental change in the way agents formed their expectations. In his 1988 version of this test, Blinder estimated several simple autoregressions for the period 1955:2 to 1987:4, searching for breaks in parameters' values, as predicted by the Lucas Critique. Yet, a Fisher test showed no statistically significant changes among or within time periods in the sample. Thus, Blinder concluded that "there is no evidence for a shift in the lag coefficients $A(L)$ " (Blinder, 1988, 283), which he considered a direct empirical evidence against the Lucas Critique.

Olivier Blanchard on the Volcker deflation

The second example of Keynesian empirical investigation on the stability of the Phillips curve is Blanchard's paper entitled "The Lucas Critique and the Volcker Deflation" (1984).²⁴ Contrary to Blinder, Blanchard takes as point of departure the change in monetary policy starting with Volcker's appointment as Chairman of the FED. The "Volcker experience," besides tightening monetary policy, constituted a substantial change in policy rules, since the

(*ibid.*, Fischer to Lucas, 31/12/1981). This correspondence illustrates how the very definition of "empirical evidence" was dividing the two sides.

²³ Note that the following formalization is inspired from Blinder (1988) rather than from the first crude version of the test (Blinder, 1979, 92).

FED changed its target from interest rate to money supply in October 1979. According to the Lucas Critique, this should have resulted in a drift in behavioral parameters describing the behavior of private agents. Thus, Blanchard tried to assess the effect of this change in monetary policy on the structure of the Phillips Curve equation, and on the term structure of interest rates.

In this sense, Blanchard analyzed these two relations as they were specified and estimated in two large scale macroeconometric models of the 1970s: (1) the Phillips curve of the Data Resources Incorporated (DRI) model (in use at the CBO), as it was designed and estimated in 1978, before Volcker's appointment; and (2) the term structure drawn from the MPS model of the Federal Reserve Board. He then studied how the estimated coefficients of these relations would change after 1978—introducing additional years, one at the time, into the estimation sample. For the Phillips curve, the results showed roughly unchanged values of coefficients through time. Hence, Blanchard's empirical investigation supported the view of the stability of the Phillips curve relation, arguing that he found "little evidence of a direct effect of the policy change on the Phillips curve, at least until 1982," (Blanchard, 1984, 1). Nevertheless, he warned that his findings did not attest for the accuracy of the DRI's Phillips curve *per se*. Indeed, his findings simply showed that the Lucas critique was not relevant for the analysis of the Volcker experiment (as the behavioral parameters were stable for the period); or, alternatively, that expectations themselves were not an important factor for explaining inflation (disqualifying the relevance of the Lucas Critique):

This in no way implies that the above relation [DRI Phillips curve] 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

²⁴ Blanchard earned his PhD student from MIT (under the supervision of Stanley Fischer). In the 1980s, he was also a Research Associate of the NBER.

due either to unchanged ways of forming expectations, or to expectations playing little role in the determination of wage inflation (Blanchard, 1984, 213-214).

Concerning interest rates, however, Blanchard acknowledged that “expectations appear[ed] to have changed and the term structure [was] very much subject[ed] to the Lucas Critique” (*ibid.*, 214), leaving open the possibility that Lucas’s argument could be relevant for analyzing this particular topic.²⁵

Robert Gordon and Stephen King on the sacrifice ratio

A third example of the pragmatic Keynesian replies to the Lucas Critique is Gordon and King (1982).²⁶ Contrarily to Blinder but similarly to Blanchard, Gordon and King investigated the U.S. disinflation policy of the early 1980, rather than the stagflation of the 1970s.

To be sure, the Lucas Critique is not the central issue in “The Output Cost of Disinflation in Traditional and Vector Autoregressive Models” (Gordon and King, 1982). Instead, the paper aims at establishing, through VAR models, the value of the sacrifice ratio for the U.S. (i.e., the cost in terms of output gap of a one-point reduction in inflation). An entire section of this article is also dedicated to dismissing the empirical relevance of the Lucas Critique—which is, indeed, crucial for supporting the idea of a positive sacrifice ratio.²⁷

Gordon and King’s econometric strategy to investigate the relevance of the Critique starts with an attempt to spot any change in the policy regime, which contrasts with Blanchard

²⁵ Other empirical works along this line that arrived to similar conclusions, were published by Englander and Los (1983), and Taylor (1984). Note, however, that Taylor’s (1984) results contradicted Blanchard’s results on the Phillips curve even if at a very low level of significance. Yet, Taylor was more inclined to reject his own results, taking into account the small sample used in his estimation.

²⁶ Gordon is a professor at Northwestern University (since 1973), research associate of the NBER (since 1968) and a member of the Brookings Panel on Economic Activity (since 1970). Stephen R. King was PhD student at Northwestern University.

²⁷ Indeed, following the Lucas Critique: if (A) a disinflation policy is put in practice, then (B) agents adapt their behavior (expecting lower prices in the future, they increase their current labor supply), generating a simultaneous fall in inflation and rise in output (so that sacrifice ratio is zero); it follows that (C) if a model does not take into account (B), then the sacrifice ratio associated with (A) is overestimated.

(1984) and Taylor (1984), who simply assume this change from the beginning. Relying on Neftci and Sargent (1978), Gordon and King estimated a feedback monetary growth rule and an inflation equation (both for 1954-1980), then tested the estimated rule with Chow's method. The values of the Chow tests indicated a break in 1967:1 for the equations of quarterly M1 growth (Gordon and King, 1982, 406-407), but no break for the inflation equation.²⁸ These results dismissed the Lucas Critique argument in the estimation of a sacrifice ratio, leaving "no solid reason to think there would be a marked change in the structure of the inflation equation, and thus in the estimated sacrifice ratio" (*ibid.*, 407).²⁹ Consequently, their further estimation, resulting in a positive sacrifice ratio (4.3), would legitimately illustrate the costly inefficiency of Volcker's policy.

Even if they did not target the Lucas Critique directly, further works on the cost of the disinflation policy that used more recent data, supported Gordon and King (1982)'s conclusion. Specifically, George Perry (1983), Otto Eckstein (1985) and Benjamin Friedman (1985)³⁰ empirically showed that the sacrifice ratio during the Volcker-Reagan era was approximately the same as the one estimated in the late 1970s.³¹

²⁸ This method differs both from Blanchard (1984) and Taylor (1984), where structural breaks were investigated by adding progressively a further year into the estimation sample. Gordon and King considered that Blanchard and Taylor's method was weak, considering the small size of the available sample.

²⁹ Further evidence against the Lucas Critique is that the only structural break identified by the Chow test (1967:1 break in M1 growth rule) does not intervene immediately. If the sample is restrained to 1954-1972, the Chow test accepts the null hypothesis at a significance level of 5 percent (*ibid.*).

³⁰ These articles were presented in 1983 and then published in a book in honor of Arthur Okun (Feiwel, 1985).

³¹ Note, however, that these results were contentious, especially because they did not provide *direct* evidence on the empirical relevance of Lucas's argument. McCallum (1984) argued that these tests ignored an underlying assumption in Lucas's argument, namely that policy changes should be credible. Thus, according to him, the cost of the disinflation policy could be explained by the fact that agents did not believe the restrictive commitment of the Federal Reserve and, consequently, did not adjust their expectations quickly enough. Moreover, McCallum expressed his skepticism about the empirical methods used to investigate changes in policy regimes. However, it should also be emphasized that McCallum himself did not suggest any alternative method or proof, simply advocating for a cautious use of empirical results.

3 Some Keynesian alternative explanations to the U.S. stagflation

In the previous section, we presented Keynesian contributions that converged toward a similar conclusion: the Lucas Critique is a syllogism which has no empirical pertinence to explain the economic context of the 1970s and the early 1980s. This conclusion resulted from econometric investigations on the stability of the Phillips curve during the stagflation period and in the immediate aftermath of this episode, i.e. during the disinflation policy.

Lucas reacted to the Keynesian arguments in a letter to Stanley Fischer, in which he argued that there was “a mountain of evidences” supporting the Lucas Critique claim (*cf. supra*). In that piece of correspondence, Lucas tried to disentangle two dimensions of Keynesian replies:

One question is whether the “parameter invariance” issue I stressed in my critique is a quantitatively serious criticism of standard econometric models and their applicability to policy evaluation. A second question is whether the forecast errors of these models in the 1970s was mainly due to their failure to possess the right kind of parameter invariance (Lucas, Archives, Box 5, Folder:1982 1/2, Lucas to Fischer, 02/06/1982).

This section deals precisely with the second question emphasized here by Lucas. Keynesian macroeconometricians did recognize that their models had not performed at their best during the 1970s, failing to predict a simultaneous rise in inflation and unemployment. Yet, Keynesians strongly rejected parameters’ instability (of the kind suggested by Lucas) as a source of this failure. Here, we describe Stanley Fischer’s, Blinder’s, Klein’s, Eckstein’s, Gordon’s, and Solow’s alternative solutions to address stagflation without referring to the Lucas Critique. These solutions shared the common idea that macroeconomic modeling should continue to evolve by adding new empirically features to preexisting models. In

particular, they plead for a better description of the supply side of the U.S. economy, and for the introduction of supply shocks on energy (oil sector) and raw material (food prices).³²

Thus, these Keynesian responses proposed simpler and empirically reliable explanations of the U.S. stagflation that dismissed Lucas's argument. In this sense, again, Lucas's argument appeared as a syllogism with no practical relevance for macroeconomic analysis.

In his correspondence with Lucas, Fischer (Lucas Archives, Box 5, Folder:1982 1/2, Fischer to Lucas, 31/12/1981) made very clear that he considered the Critique as "one possible source" of the "bad" predictions of the econometric models "for the early seventies." However, "there were many other potential sources of errors" such as "single equation estimations methods; simple misspecification in [structural] equations," and yet "no one that [Fischer] [knew] of ha[d] made the empirical connection between the bad forecasts of the models and [Lucas's] critique."³³

Fischer returned in later works to what he thought were the most important "other potential sources" to be investigated. "Misspecification in individual equations" came on top of the agenda. According to him, the Phillips curve needed especially to be reformulated by adding an expectational term to the equation, following the works of Phelps (1967) and Friedman (1968). This improvement had been at the disposal of modelers since the late sixties, and it has been integrated to major models so far (in 1974, in the MPS model, for

³² Note that this attitude is closely related to the Keynesian pragmatic view of macroeconomics, which we emphasized in the previous sections. In addition, the idea of an incremental development of models (adding new features in order to take into account new situations) was a distinctive characteristic of the style of modeling advocated by Keynesians (see in particular Klein's position in Pinzón-Fuchs, 2017).

³³ Lucas's answers to this remarks is quite condescending, since he argued that the existence of other problems in Keynesian models is in no way evidence against the Lucas Critique: "I certainly agree with you that in principle there are many possibilities besides the failure to possess parameter invariance. There are a lot of ways to do economics badly, and I am willing to believe that one can find all of them in these large-scale models." (*ibid.*, Lucas to Fischer, 02/07/1982).

instance).³⁴ However, this does not prevent from other sources of misspecification, causing the kind of parameters' variability pointed out by the Lucas Critique:

The 1973 supply shock also led to an under-prediction 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, 302).

On this issue, Blinder (1987, 133) argued too that once expectational variables were added to the Phillips curve equation (to get the “augmented” Phillips curve), as well as “supply shocks,” major models would fit the 1970s data pretty accurately, contrasting with Lucas and Sargent (1979)'s argument. Such contradictory arguments illustrate the disagreement that existed at the time between New Classical and Keynesians macroeconomists on the interpretation of the prediction failures associated with parameters' variability.³⁵ Fischer considered here the misspecification of the Phillips Curve (an omitted expectation variable) to be the source of prediction failures. Consequently, the problem has to be solved by adding further, relevant variables into the estimated equation to explain (better) the inflation pattern. Conversely, Lucas argues for a change in the whole structure of the estimated equation, where the variability of some parameters has to be included as a representation of changes in the agents behavior.³⁶

Like Fischer, Klein was persuaded that the Lucas Critique was not a relevant point to overcome the prediction problems presented by the Keynesian models during the stagflation

³⁴ On the history of the Phillips curve and its multiple avatars, see for instance Qin (2014) and Forder (2010a, 2010b, 2014).

³⁵ An issue already emphasized by Qin (2013) in her history of econometrics after 1970s.

³⁶ Today, Lucas's interpretation is dominant, as emphasized by Qin: “[time-varying parameter estimates are] no longer associates with the omitted-variable problem. Instead, time-varying parameters are regarded as the structural representation of the changing behavior of agents as they adapt to changing economic reality” (Qin, 2013, 120).

period.³⁷ In a paper written in 1976,³⁸ Klein had already showed how these problems could be easily fixed by adding supply shocks to the model. His argument resulted from simulations of the Wharton model, and illustrated that³⁹

given a conventional fiscal shock, [the Wharton model] would generate the usual trade-off relationship, but *if given a food or fuel price shock [the model] would generate a situation of rising unemployment and rising inflation* (Klein, 1985, 293, our emphasis).

Hence, the neglect of the energy and food sectors constituted for Klein the source of underestimation of the inflation rate in the forecasts of the Wharton model. According to Klein, the prediction problem was about to be solved by the end of the 1970s, when Keynesian macroeconomic models had managed to introduce the energy and agricultural sectors in a satisfactory manner (*ibid.*, 292). Furthermore, this new available feature 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”—even if this extension still yielded a “moderate” forecast error (*ibid.*). This point brings the debate back to the question on the *originating source* of the 1970s stagflation: which was responsible for inflation? Was it economic policy, as Lucas argued, or were it oil and food price shocks? According to Klein, relying on the Wharton simulations, “inflation was not policy induced,” or at least, it was “certainly not [induced] from the policies that were coming from the neoclassical-Keynesian model.” Instead, inflation “was purely exaggerated by the food and oil shocks” (*ibid.*).

³⁷ Klein had been a key figure for the development of the Keynesian macroeconomic approach in which the Klein-Goldberger (1955) model had marked the cornerstone for this practice. For an account on Klein's influence in the construction of macroeconomic modeling, and on Klein's early work and career see Pinzón-Fuchs (2017).

³⁸ “Longevity of Economic Theory,” 1976, box 31, Lawrence R. Klein Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University.

³⁹ This large-scale Keynesian macroeconomic model mainly arose from Klein's work within the Wharton School of Business at the University of Pennsylvania. Like the DRI model (*cf. infra*), the Wharton model played

Eckstein (1978, 1983) also investigated the different potential sources of the 1970s stagflation in the U.S.⁴⁰ Similarly to Klein's work on the Wharton model, Eckstein ran comparative simulations of the DRI model in which he addressed specific scenarios, capturing a major shock to the economic system. Eckstein (1978) suggested six scenarios or shocks: an energy crisis, a raise in agricultural prices, changes in monetary policy, devaluation of the dollar, price control, price decontrol, and changes in fiscal policies. When trying to reproduce the simultaneous rise of inflation and unemployment observed in 1969-1974 macroeconomic time series, four scenarios performed satisfactorily in accounting for the rise in inflation (agricultural prices, monetary policy, devaluation, price decontrol, and fiscal policy), while price control only accounted for the rise in unemployment. Yet, the "energy crisis" scenario (consisting in an increase of oil prices) was the only one that reproduced both observed high inflation and unemployment. These results supported the idea that changes in policy rules that resulted in changes in individual behavior, were not a pertinent source for the stagflation phenomenon, and, conversely, that supply shocks in energy prices were the most likely source.⁴¹ Klein's and Eckstein's simulations were actually framed into a broader project, which Klein further developed in his presidential address to the American Economic Association, "The Supply Side" (Klein, 1978). He defended the idea that the supply side of macroeconomic models was underdeveloped in comparison to the demand side, and proposed to combine standard Keynesian models with the "Leontief model of interindustrial flows" (*ibid.*, 1). And Klein explained, retrospectively, 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, 290). Developing this "supply side" was hence

a central role in the development of Keynesian macroeconomic practices and their spreading in different fields of expertise.

⁴⁰ Eckstein had been professor at Harvard, member of the Council of Economic Advisors and one of the co-founders of the DRI model, in use at the CBO (*cf. supra*).

⁴¹ A few years later, Eckstein conducted some new tests and drew a clear general conclusion: "changes in policy regime seem to have been among the minor sources of structural change of the economy and of forecasting error

on the top of his research agenda, and was, actually, already followed by other researchers at the time. This was for instance the case of Eckstein (1976), who considered as well that “the most exciting econometric research today is in the fields of energy, capacity planning, food supply” (*ibid.*, 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 order to cope better with the new sources of instability” (*ibid.*, 17). This research program represented a pragmatic alternative path to Lucas’s approach. The relevant way of developing and improving macroeconomic modelling was to develop concrete, empirical-grounded extensions of the economy described by the model.

Gordon followed a research path similar to the one advocated by Klein. In a series of articles, he developed an elaborated “triangle” model to explain stagflation (Gordon et al., 1975; Gordon 1977; Gordon, 1984). In his framework, inflation was explained in addition to the employment rate by price inertia (a form of adaptive expectations in which inflation yesterday explains inflation today), and supply shocks. Gordon (2011, 2013) defends his model still today, showing its capacity to fit the data, and to explain indirectly the failures of the Keynesian predictions of the end of the 1960s and 1970s.⁴²

More generally, not only these authors but most Keynesians defended the view that the Lucas Critique was not relevant to address stagflation. For them, the true challenge on Keynesian macroeconomic models given by stagflation was elsewhere. In his conclusive statement to the conference “After the Phillips Curve,” held at the Boston Fed in June 1978, Solow suggested indeed—in the same line of Klein and Eckstein—that the main priority for Keynesian macroeconomicians was to improve their models by emphasizing the supply

in the actual historical record” (Eckstein, 1983, xi-xii). Again, Eckstein’s empirical work clearly aimed at discrediting the change in economic policies as an explanation of the rise in both inflation and unemployment.

⁴² On Gordon’s analysis of the Phillips Curve in the 1970s, see Goutsmedt (2017, chap. 3).

side of the economy and carry on with empirical work on “the side of food, oil, nonfuel minerals, and the depreciation of the dollar” (Solow, 1978, 205).

Concluding Remarks

In this article, we studied some of the 1970s and 1980s Keynesian replies to the Lucas Critique. So far, these replies had been either ignored or disqualified by the standard history of macroeconomics, which relies on the interpretation of Lucas (1976) as a logical argument that inevitably dismissed the internal consistency of Keynesian models. We argued instead that this interpretation misses the main point of the Keynesian replies, which consisted in questioning the empirical relevance of Lucas’s argument to account for the macroeconomic context of the time.

As Blinder (1983, 14-15) put it, the “criticisms of the old econometrics made by Lucas, Sargent and others are not wrong; they are absolutely correct.” Yet, “saying this in no way denies the validity of the Lucas critique, but merely points out that it may not always be of great empirical importance.” To Blinder, “the critique should take its place as one among many serious problems that confront the applied econometrician” together “with violations of the assumptions of the Gauss-Markov theorem,” which have “not stopped applied econometrics in its tracks (though it has given cause for humility)”:

Perhaps the Lucas critique should be treated in the same way. This broader perspective dictates that we follow a more pragmatic, case by case, approach in which we recognize that other problems may be more important than the Lucas critique in particular cases (Blinder, 1983, 14-15).

The case of the reception of the Lucas Critique serves as an illustration of a general historiographical point that we want to make for the history of macroeconomics. If one took the spontaneous histories told by macroeconomists today as faithful accounts of the history of

the discipline, one would discover a narrative in which the old Keynesians had suddenly stopped contributing to macroeconomics during the 1970s, and a history in which their works had been erased. Here, we hope to have made the case that, if there was a Keynesian retreat, this was a structured one. Their empirical work should not be perceived as an attempt to simply preserve the *status quo* of the discipline, nor should their refusal to take into consideration Lucas's arguments be seen as a kind of theoretical conservatism or ideological bias. The main claim of this paper is that their replies were motivated by the very nature of the Keynesian macroeconometric tradition, which consisted in a pragmatic approach to modeling that addressed concrete economic situations with empirical rigor. In this sense, testing econometrically the relevance of Lucas's argument was a natural way of approaching the question on a scientific basis. Similarly, neglecting rational expectations as the main point of their research agenda was natural, since empirical evidence indicated that supply shocks accounted as the main factor of the 1970s stagflation. These replies, however, were neither sufficient to take down the Lucas Critique nor to stop the development of New Classical macroeconomics, which was perceived by some Keynesians such as Blinder or Malinvaud (*cf. supra*), as an illegitimate "palace coup" that lacked support from decisive empirical results.

Also Fischer, in his correspondence with Lucas, (Lucas's Archives, Box 5, Folder:1982 1/2, Fischer to Lucas, 31/12/1981) bitterly insisted on his point that the Keynesian models had been "discarded by serious researchers on the basis of [Lucas's] critique and not because the estimation methods were inappropriate." Fischer attributed this not only to the fact that Lucas's "theoretical critique was persuasive," but also that "an unexciting prospect for bright young minds" was "toiling in the vineyards of the econometric models." Most importantly, however, Fischer found it "remarkable that an ostensibly empirical profession [paid] very little attention to empirical work."

Appendix

Precisions on *Econometric Policy Evaluation: A Critique*

This appendix provides some precisions on the origins, content, and scope of Lucas's (1976) paper.

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 (Sargent, 1996, 539). The first draft of the paper was presented on April 20, 1973 at the University of Rochester, during the first *Carnegie-Rochester Conferences on Public Policy*. The main subject of the session in which Lucas’s paper was presented dealt with the Phillips Curve. Karl Brunner, organizer of the conference, had asked Lucas for “a survey of the empirical evidence on the Phillips curve” (King, 2003, 249). After the conference and after some modifications, Lucas published this version of the paper as a Carnegie Mellon working paper (Lucas, 1973a) in May 1973. 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 the Lucas’s Critique was in circulation since 1973, and was even quoted by some authors (Cooley and Prescott, 1976; Sargent, 1976).⁴³

In his 1976 paper, Lucas presents both a general discussion of his argument (sections 2 to 4, section 6) and three specific examples (sub-sections 5.1 to 5.3). The first example discusses Friedman (1957) permanent income hypothesis, in a similar vein of the discussion

⁴³ “My copy of Lucas (1973a) is wearing out from people reading it, unstapling it, Xeroxing it, and restapling it” (Lucas, Archives, Box 2, Folder: 1975 1/2, William Poole to Lucas, 01/11/1975). In Lucas archives (Box 2, Folders 1973-1976), one can find many other letters to Lucas asking for a copy of the paper.

given by Muth (1960). The second example is borrowed from Lucas and Prescott (1971), and deals with investment. The third example deals with the determination of the Phillips curve.

From a general perspective, the Lucas Critique is formalized as follows. The evolution of the relevant variables of the economic system s_t can be described by a function f :

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

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.). Macroeconometricians identify f (not directly known) through a distribution function F and through the estimation of a vector of behavioral relations θ :

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

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

Lucas points out that the behavioral parameters in θ are not fixed, since they are not invariant for every $\{z_i\}_t$.⁴⁴ Thus, the vector θ should be *a function of the individual decision rules* (λ), which react to changes in z_t . This relation between government decisions and individuals’ decisions 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:

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

and the econometric problem is to estimate the function $\theta(\lambda)$. According to Lucas, the specification of $\theta(\lambda)$ must deal with two questions (to which “Econometric Policy Evaluation”

⁴⁴ To rephrase in the terms of the Lucas’s syllogism presented in the introduction of our article: (A) the structure of an econometric model ($s_{t+1}=F(.)$) is based on relations describing the behaviour of economic agents (θ); (B) this behaviour changes along with changes in policy-makers’ decisions (θ is not the same for all $\{z_i\}_t$); it follows that (C) the structure of the model will also change along with changes in $\{z_i\}_t$.

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 the optimizing behavior of agents. 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), meaning that the subjective probability distribution (agents' expectations) equals the objective probability distribution of the model. At time $t-1$, the expected value of a variable x at time t equals the mathematical expected value of x , conditionally to the set of available past information Ω_{t-1} :

$$x_t^e(t-1) = E_{t-1}(x_t | \Omega_{t-1}) \quad (4)$$

Lucas emphasizes 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, 41).

The specification of $\theta(\lambda)$ under optimizing behavior and rational expectations was addressed by a full macroeconometric program mainly developed by Sargent. These works have been collected in Lucas and Sargent (1981).⁴⁵ The Lucas Critique provided a cornerstone for these contributions, as it established the guidelines for building “true” structural econometric models. Presenting the Lucas Critique as a “simple syllogism” is certainly an accurate summary of the content of *Econometric Policy Evaluation*, since the article contains no econometrics. In addition, the article underestimates the emphasis on empirical corroboration

⁴⁵ For a comprehensive overview of this New Classical macroeconometric program, see Sergi (2015).

put by Lucas himself in later writings, and, as we argued, leaves aside the important amount of econometric contributions that had been developed by other New Classical macroeconomists.

Two examples illustrate Lucas's position on the empirical corroboration. First, in the introduction to his collected works, *Studies in Business Cycle Theory* Lucas emphasized the idea that his models contain policy-invariant parameters (such as tastes and technologies). Yet, he argued that "this presumption [about policy-invariance] seems [...] sound [...], but [...] must be defended on empirical, not logical grounds," and added that "the nature of such a defense [would] presumably [...] vary with the particular application one has in mind" (Lucas, 1981, 11-12). This argument meets the Keynesian position that invariance in the parameters is an empirical issue that depends on the scope of the models. Moreover, Lucas and Sargent made the same point in their polemical piece "After Keynesian Macroeconomics:"

[there is] a number of theoretical reasons for believing that the parameters identified as structural by the methods which are in current use in macroeconomics are not structural in fact. That is, there is no reason, in our opinion, to believe that these models have isolated structures which will remain invariant across the class of interventions that figure in contemporary discussions of economic policy. *Yet the question of whether a particular model is structural is an empirical, not a theoretical, one* (Lucas and Sargent, 1979, 56, our emphasis).

Indeed, Lucas did consider that his Critique ought to be empirically corroborated. Moreover, he also thought that this corroboration had already been achieved. This is why he strongly reacted to Fischer's (1983) claim that the Lucas Critique had been accepted without the support of any empirical evidence (*cf. supra, section 3*). In fact, many of the evidences referred to by Lucas in his letter to Fischer (like Lucas, 1973b, Sargent and Wallace, 1973; Sargent, 1976, and Barro, 1977) were indirect tests that addressed the "natural rate

hypothesis,” a vertical Phillips-curve with no exploitable trade-off between inflation and output.

However, some New Classical macroeconomists did provide more indirect “tests” of the Lucas Critique. The most illustrative examples are the simulations ran by Paul Anderson (1978, 1979). To Anderson, who worked at the time in the Research Department of the Federal Reserve Bank of Minneapolis, finding out whether the “changes in forecasting rules hypothesized” by the Lucas Critique were “important enough to invalidate current simulation methodology” was indeed “an empirical question which should be of great concern to model builders and users” (Anderson, 1979, 68). Through the simulation of the models’ responses to different changes in policy, his goal was to show that a model involving rational expectations would provide more reliable predictions compared to traditional Keynesian models. In Anderson’s own words, “it seem[ed] reasonable to maintain that macroeconometric models in which forecasting rules adjust to policy changes have the potential to represent the responses of the real economy more accurately than the static expectations models now employed” (*ibid.*). In this sense, Anderson defended the augmented rational expectations approach on empirical ground.

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