

Modern Monetary Policy Evaluation and the Lucas Critique

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Abstract

We can separate modern mainstream approaches to monetary policy evaluation in roughly four groups: large-scale Keynesian macroeconometrics, Monetarism, New Classical macroeconomics, and the most recent consensus approach of New Keynesian DSGE modeling. In a brief historical overview it is shown that the overarching element in all four approaches is a distinctly positivistic framework of mathematical model building and testing in light of continuously gathered macro data. It is argued that this is precisely the reason why the modern consensus approach cannot be seen as an adequate solution to the fundamental problem indicated by the Lucas Critique. The latter must not be seen as a critique of any particular modeling approach within modern mainstream macroeconomics, but rather as one addressing its positivistic research program in general. A more radical critique of positivism is presented in order to clarify the core problem implicit in the Lucas Critique and to hint at a more promising route to circumvent it – a route that has been mostly neglected by mainstream economics.

I. Introduction

Money is one of the most discussed economic topics with a political dimension of ever growing importance. From the beginning of the second half of the 20th century until today we can observe a development towards more political intervention into the monetary sphere of our economies. This development is most notably marked by the suspension of convertibility of the US dollar into gold in 1971 under President Richard Nixon. Although there has already been some room for monetary policy under the *Bretton Woods System* (Bordo and Eichengreen 1993), the fiat money regime established after the Nixon shock rendered monetary policy much more powerful as the money supply then became entirely subject to political will. The natural question is how this additional policy option of a fully flexible money supply can be exploited optimally?

This is one, albeit not the only, important question of modern macroeconomics, which has been described as the sub-discipline of economics that emerged after and out of the publication of Keynes's *General Theory of Employment, Interest, and Money* in 1936 (Vroey and Malgrange, 2011). We there-

fore start our brief historical review of modern macroeconomic approaches to monetary policy analysis, contained in the second part of this paper, with what we call large-scale Keynesian macro-econometrics.¹ The Monetarist school of economic thought as represented most notably by Milton Friedman came to be recognized as the second important branch of modern macroeconomics. Despite certain similarities it was critical towards Keynesianism. Even more recognized and influential, however, was the criticism leveled by the New Classical school, first and foremost representative of which is Robert E. Lucas. The Lucas Critique, so it is frequently claimed, has “transformed macroeconomic analysis and deepened our understanding of economic policy.” (Fischer, 1996, p. 11) This transformation culminated into the current consensus approach of New Keynesian DSGE modeling, the representatives of which regularly claim to have taken account of the Lucas Critique (for example Woodford, 2003, p. 13 and p. 56).²

These four major strands of modern macroeconomics draw diverging conclusions for monetary policy. However, our rough historical sketch shows that the overarching element in all of them is a distinctly positivistic approach to the analysis of political intervention into human interaction in general and monetary affairs in particular. We then ask

1 Vroey and Malgrange (2011) refer to it simply as Keynesian macroeconomics. However, one of the characteristic marks of it is the integration of mathematical and statistical tools into economic theory, that is, the development of econometrics, as well as the attempt to capture the dynamics of the economy in large-scale quantitative models. To emphasize these features we prefer the chosen label. On the interrelationship of Keynesian economics and econometrics see Patinkin (1976).

2 For appraisals of the impact of Lucas’s work see in particular Miller (1994), Fischer (1996), and Hall (1996).

the expedient question whether the most recent approach can really be seen as an adequate solution to the fundamental problem that has been indicated by Lucas (1983 [1976]). In the third part of this paper we will argue that it cannot. Based on a clarification and refinement of the underlying problems derived from the works of Mises (1933, 1962, 2007 [1957]) and Hoppe (1983, 2007) we show that the problem cannot ultimately be solved under a positivistic research paradigm, which holds that policy conclusions are to be drawn from the evidence of observed statistical data. Such an approach will always have to battle with a lack of constancy in human interaction. We will however not engage in a comprehensive outline of an alternative approach.³ This remains to be done in a consecutive paper. The fourth section of the paper concludes.

II. A brief Review of Modern Macroeconomics

As with every retrospective classification of particular branches in economic thought it is very difficult to identify and weigh all relevant factors that influenced their formations. Almost by necessity, we must do some injustice to specific individuals, groups of researchers, institutions, or other lines of economic thinking, when we subsume them into a certain branch for some reason, exclude them

3 The most comprehensive work on monetary theory (which was the commonly used term for what became macroeconomics prior to the *General Theory*) in the tradition of Austrian economics is still Mises (1953). It is the prime example of a non-positivistic analysis of money and monetary policy. See also Hülsmann (2012) for a collection of essays that celebrate the centennial of the publication of the first German edition of this volume in 1912. Salerno (2010) provides further readings in this tradition.

for another, or don't even mention them at all for the sake of brevity. The latter point is of particular importance in a short sketch like the one contained in this paper. Hence, in order to minimize controversy in the following section, we will only focus on the most important branches and follow closely well established history of modern macroeconomic thought. The more controversial content is set aside for section three.

Large-Scale Keynesian Macroeconometrics

With some level of confidence it can be argued that what we call the large-scale Keynesian macroeconomic approach, which became dominant in the 1950s and 60s, is essentially characterized by the conjuncture of two developments.

On the one hand, the attempt to combine economic theory with mathematical and statistical tools gained ever more popularity at that time. The mathematization of economics was most strongly linked to the interrelated foundations of two institutions, namely the *Econometric Society* in 1930 and the *Cowles Commission for Research in Economics* in 1932, both of which proved to be very successful in the promotion of their common motto "Science is Measurement" among professional economists, not only in America but internationally. The result was the newly defined discipline of econometrics (Frisch, 1936),⁴ which saw

one of its first major applications in the work of Tinbergen (1939) who developed the first model of the whole US economy using data from 1919–1932, and who later shared the first Nobel Memorial Prize in economics with Ragnar Frisch in 1969.

On the other hand, there was the rapid spread of Keynesian economics after the publication of the *General Theory of Employment, Interest, and Money* (Keynes 1936), a movement sometimes referred to as the "The Keynesian Revolution" (Kates, 2009; Klein, 1950b) or the "New Economics".⁵ The IS-LM framework developed above all in Hicks (1937) and Modigliani (1944) was the distillation of the most important ideas from the *General Theory* and became the theoretical foundation for large-scale Keynesian macroeconomic models.

some sixteen years later (Frisch 1926). "Oekonomographie" in Ciompa's vision would be a doctrine of illustrating economic phenomena using mathematics and geometry. "Oekonometrie" would be the geometrical representation of "value [Wert]" and directly linked to the principles of accounting. It is very interesting to note that Frisch (1936) laments: "It still seems, however, that, taken in the now accepted meaning, namely, as the unification of economic theory, statistics, and mathematics, the word was first employed in the 1926 paper. Pawel Ciompa seems to emphasise too much the descriptive side of what is now called econometrics." Indeed, Frisch and his followers see mathematical and statistical methods not merely as a means to illustrate or describe economic phenomena, that is, as pedagogical devices, but rather as a veritable tool to develop economic theory. This important difference deserves to be emphasized. Mathematics and geometry as tools for describing and illustrating economic theory is not the same as using mathematical deduction for developing economic theory and empirical-statistical methods to test it. The former use is mostly a matter of style and pedagogy, whereas the latter must be justified by epistemology.

⁵ See for example Harris (2010 [1947]) for a collection of essays that generally praise the impact of the "New Economics", or Hazlitt (1959) for a comprehensive critique thereof. See also Tobin (1974) for a somewhat less resolute reassessment.

⁴ Frisch provides a German language quotation of Polish economist Pawel Ciompa from his book *Grundriss einer Oekonometrie und die auf der Nationaloekonomie aufgebaute natuerliche Theorie der Buchhaltung* (Ciompa 1910) that was brought to his attention by Professor Tomasz Lulek of the University of Cracow. Ciompa was apparently the first to use the terms "Oekonomographie" and "Oekonometrie" and not Frisch himself, who used the French term "économetrie" for the first time

One of the most influential representatives of this first approach, who effectively connected both developments, is Nobel laureate Lawrence R. Klein (1980). He was Paul A. Samuelson's first doctoral student at Massachusetts Institute of Technology, himself a Nobel laureate (1970) and a devoted follower of Keynesian economics. Out of Klein's doctoral dissertation emerged the book *The Keynesian Revolution* (Klein 1950b).⁶ Klein himself underlines the impact that Keynes's theory had on his empirical work and explicitly states that his macroeconomic models were attempts to test Keynesian economics: "Keynes's *General Theory* appeared to cry out for empirical verification (or refutation)" (Klein, 1997, p. 137). According to Vroey and Malgrange (2010, p.1) the empirical extension of Keynesian economics "became his life's work." Interestingly though, they come to the conclusion that Klein's models were partly less Keynesian than he claimed. This however is not surprising given his generally positivistic outlook on economics. Where theory was not confirmed by the data at his disposal or could not be operationalized for quantitative analysis, he tweaked it (Vroey and Malgrange, 2010, pp. 18.). This does not mitigate the inspirational force that the *General Theory* had on his work.

Hired by Jacob Marshak, director of the *Cowles Commission*, Klein started to develop "a new Tinbergen to forecast the performance of the American economy after the War."⁷ Monograph no. 11

6 For a brief overview of Klein's professional career and the impact of his work see for example Visco (2014), and some of the references cited therein, in particular the autobiographical essays Klein (1980, 1986, 1992).

7 These are the words Marshak used to describe Klein's job at the *Cowles Commission*. Klein happily accepted the offer. It is cited in Vroey and Malgrange (2010, fn. 1). See also Klein (1991, p. 108).

of the Commission was Klein's *Economic Fluctuations in the United States* (Klein, 1950a). It was the first in a series of models to be developed over the 50s and 60s, among which were the Klein-Goldberger Model (Klein and Goldberger 1955), the Wharton Models (Evans and Klein, 1967; Evans, 1963; Haitovsky, Treyz, and Su 1974; Klein, 1964), and the Brookings Models (Duesenberry et al., 1965, 1969; Fromm and Klein, 1975; Fromm and Taubman, 1968). Klein was involved in all of them.

In that series there is a clear tendency towards larger and more complex models developed by teams of a growing number of researchers including not only economists, but also statisticians, mathematicians and computer scientists. Whereas the Klein-Goldberger Model "consisted of 15 structural equations, 5 identities and 5 tax-transfer auxiliary relationships" (Bodkin, Klein, & Marwah, 1991, p. 57), the Wharton Model in its different versions grew to more than 50 equations and 40 identities (Haitovsky, Treyz, and Su 1974). The Brookings Model eventually contained more than 400 equations, indicating the prevailing view: "the more complex a model, the better" (Vroey and Malgrange, 2011, p. 4). The coefficients of the various hypothetical equations were estimated on the basis of observed macroeconomic data using different econometric methods. The equations were re-estimated, adjusted or disaggregated into sub-equations, e.g. aggregate demand into demand in different sectors, when the required data was available or when changes in the hitherto observed correlations occurred. But all this growing complexity would never lead to the desired predictive accuracy and could not prevent the dismissal of this approach by many academic economists.

As Webb (1999) points out, these models are elaborated IS-LM models augmented with a Phillips curve, which was initially introduced as an inverse empirical relationship between the rate of money wage growth and unemployment, without any political implications (Phillips, 1958). It was then subsequently popularized as a politically exploitable and constant trade-off between price inflation and unemployment (Samuelson and Solow, 1960). This trade-off became the lynchpin of Keynesian macroeconomic analysis. The simplistic political implication was that expansionary monetary policies by creating price inflation could help diminishing unemployment.⁸ When this empirical relationship broke under the stagflation of the 1970s, during which rising price inflation coincided with rising unemployment over an extended period of time, skepticism towards Keynesian macroeconomics grew stronger. But even prior to the extremely influential Lucas Critique, the Monetarist school effectively qualified the alleged trade-off between price inflation and unemployment.

Monetarism

Under the leadership of Milton Friedman, Nobel laureate in 1976, the Monetarist school became very influential towards the end of the 1960s and during the 70s. Friedman earned his Ph.D. from Columbia University by submitting a work co-authored with Simon Kuznets in 1946,⁹ and subsequently became pro-

fessor of economics at the University of Chicago for more than 30 years. Incidentally, from 1939 until 1955 the *Cowles Commission* was housed at the University of Chicago (Christ, 1994), which led to a most lively antagonism between the advocates of Keynesian macroeconomics affiliated with the commission and the Monetarists in the faculty.¹⁰

One topic over which the Monetarists' critique of Keynesianism unfolded most strongly was the Phillips curve. Their most important contribution in this respect is the natural rate hypothesis (Friedman, 1968; Phelps, 1967, 1968), which argues that the Phillips curve trade-off is essentially a short-run phenomenon that can only hold as long as actual price inflation rates exceed the inflation expectations of the workers. Once their expectations adjust, which will happen in the long run, higher wages are demanded and the level of unemployment returns to its natural rate as determined by real economic factors, such as labor market regulations, capital endowment, technological change etc. Hence, in the long run there is no trade-off and price inflation is neutral to unemployment.

description of determinants of income for five professions with the aim of drawing conclusions for public policy, such as the role of government in investment into education and professional training. It was published as Friedman and Kuznets (1954).

¹⁰ An illustrative description of the divide into the two camps at Chicago is given by Klein (1991, p. 112): "There were two worlds of economics at Chicago then, 'us' and 'them', the former were the Cowles group, who were overwhelmingly New Deal democrats. I remember, vividly, the shock when Albert Hart came over from his office at CED and interrupted a Cowles Commission seminar one afternoon to tell us that President Roosevelt had passed away. We were all struck with grief. The latter were the stalwarts of the Chicago School [out of which Monetarism emerged], and we nearly always took polarized positions at general economics seminars on campus. Our intellectual opponents were Frank Knight, Henry Simons, Lloyd Mints, and at the end of this period, Milton Friedman."

⁸ It should be pointed out, however, that Keynesian economics generally stressed more the importance of fiscal policies, in particular during economic crises. Stronger emphasis on the monetary side was put by the Monetarist school discussed in the next subsection. Compare the historical overview provided in (Woodford, 1999).

⁹ The work is an empirical investigation and

The curve becomes a vertical line. This led to the notion of the expectations-augmented Phillips curve. According to the monetarist view the role of central banks and monetary policy is thus more limited, but far from negligible.

The main argument presented in Friedman (1960) consists of the claim that there has been much more volatility in the money supply under discretionary central bank policies after the establishment of the Federal Reserve System in 1913 than before. As a result there has also been more volatility in price inflation and unemployment. His *Program for Monetary Stability* boils down to a strict rule for monetary policy, the so-called k-percent rule, according to which the money supply should be expanded “at a fixed rate year-in and year-out” (Friedman, 1960, p. 90). In order to maintain roughly stable long-run prices of final products, he estimates that this rate should have corresponded to slightly above four percent over a time period of nine decades prior to 1960; three percent to account for real economic growth and one percent to account for decreases in the money’s volatility of circulation (see also Friedman and Schwartz, 1963). Friedman argues effectively against discretionary countercyclical monetary policies as advocated by the Keynesians. However, the expectations augmented Phillips curve can still be seen as a backdoor for discretion, as it can hardly be advocated not to engage in discretionary expansion, in particular during crises, if one is convinced that it can diminish unemployment in the short run, and yields no negative consequences in the long run, but is neutral.¹¹

Although both camps, the Keynesians and the Monetarists, were following a positivistic research agenda that is closely connected to the rise of econometrics, they also had some disagreements concerning methodology. Boumans (2013) and Christ (1953, 1994) characterize the *Cowles Commission* approach, i.e. the one that we labeled large-scale Keynesian macroeconometrics, as essentially Walrasian in that their models consist of rather complex systems of simultaneous equations, the solutions to which correspond to general equilibrium. The Monetarist approach in contrast, Boumans (2013) argues, is more in line with the Marshallian idea of breaking up the complex problems of economics, of partitioning and applying the *ceteris paribus* qualifier to specific sub-problems. Hence, Monetarist models often consist of very few or even only one single equation, as opposed to large-scale Keynesian models.

According to Friedman (2008 [1953], p. 146) then, an economic theory or model is not to be tested with respect to its complexity and similarity to a realistic setting, but rather with respect to its predictive power: “Its performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields.” This in turn implies that a theory ought to be constructed in a way that makes comparison to observable phenomena possible, just like theories in the natural sciences. He continues: “In short, positive economics is, or can be, an “objective” science, in precisely the same sense as any of the physical sciences.” Friedman’s contribution has been declared “the most influential work

11 Interestingly Friedman (1977) in his Nobel lecture suggests that the research on the Phillips curve may now enter a third stage, one in which it is tried to explain a positively sloped curve, where

high price inflation corresponds to high unemployment. He pointed to the role the political process might have in explaining this relationship.

on economic methodology of the twentieth century.” (Hausman, 2008, p. 33) In discussing his *A Theory of the Consumption Function* (Friedman, 1957), which he himself calls his best book in technical economics, Friedman (2000) describes the positivistic approach to economics very clearly:

It’s a nice complete whole. It started with an empirical contradiction – data that weren’t consistent with one another. It presented a hypothesis [the permanent income hypothesis] to explain the contradiction. Out of that hypothesis it drew implications capable of being contradicted by further evidence. It analyzed the further evidence that there was and found that it was consistent with the hypothesis. And the hypothesis is by now part of standard economics.

The Popperian idea that economic theory ought to be falsifiable by observable phenomena, that ultimately empirical observation renders theories valid or invalid, has become generally accepted among mainstream economists, and it was not disputed by the economists of the *Cowles Commission* either. Their disputes merely focused on policy conclusions and certain aspects of methodology, but not on epistemology. In contrast, the impact of New Classical economics on the methodology of modern macroeconomics, which we will discuss next, is considered to be much more substantial, even revolutionary (Woodford, 1999).

The Lucas Critique and New Classical Economics

We have mentioned the expectations augmented Phillips curve that the Monetarists introduced. They thought about

expectations as being *adaptive*,¹² and modeled them as functions of the observed lagged values of the relevant variables, for example, expected price inflation rates as a function of the observed price inflation rates in the recent past. The New Classical economists, inspired by Muth (1961), introduced the idea of *rational* expectations into modern macroeconomics. Rational “expectations, since they are informed predictions of future events, are essentially the same as the predictions of the relevant economic theory” (Muth, 1961, p. 316), that is to say, expectations were then modeled as being consistent with the predictions of the models themselves. In its extreme, rational expectations have been applied in a way that renders monetary policy futile, as agents immediately anticipate future price developments in accordance with changes in monetary policy and incorporate those expectations into their buying and selling decisions, and hence it leads to a Phillips curve that is not systematically exploitable at all, neither in the long nor the short run. This idea led to a revival of the classical notion of monetary neutrality (Lucas, 1972, 1996), and to Real Business Cycle Theory that explained macroeconomic fluctuations without recourse to money at all (Kydland and Prescott, 1982).

The most important and most often discussed contribution in the New Classical strand of modern macroeconomics is known as the Lucas Critique, a critique leveled primarily against large-scale Keynesian macroeconometrics. It is however again not a critique of the modern econometric modeling approach in general. Lucas even praises the efforts

12 For early applications of the adaptive expectations hypothesis see for example Cagan (1973 [1957]) and Nerlove (1958) as cited in Gertchev (2007).

of his colleagues: “The Keynesian macroeconomic models were the first to attain this level of explicitness and empirical accuracy; by doing so, they altered the meaning of the term ‘theory’ to such an extent that the older business cycle theories could not really be viewed as ‘theories’ at all.” (Lucas, 1977, p. 11) Yet, he would emphasize an extremely important deficiency to which these models are vulnerable when it comes to counterfactual policy evaluation, namely the likely variability of estimated model coefficients in response to policy changes. Thus, Lucas (1983, [1976], p. 257) started his critique:

The fact that nominal prices and wages tend to rise more rapidly at the peak of the business cycle than they do in the trough has been well recognized from the time when the cycle was first perceived as a distinct phenomenon. The inference that permanent inflation will therefore induce a permanent economic high is no doubt equally ancient, yet it is only recently that this notion has undergone the mysterious transformation from obvious fallacy to cornerstone of the theory of economic policy.

He hints at the naïve Keynesian interpretation of the Phillips curve, the incautious tendency of taking mere empirical observations of correlations between economic variables, uncovered by statistical methods of econometric analysis, as a sufficient basis for theoretical postulates of causality. The idea of the Lucas Critique is that these relationships are subject to change, in particular when policy makers try to exploit them. This is because the behavior of economic agents is contingent on the political environment. Lucas’s contribution is in this sense a substantiation and a generaliza-

tion of the natural rate hypothesis of the Monetarists. He continues:

This clear-cut conflict between two rightly respected traditions – theoretical and econometric – caught those of us who viewed the two as harmoniously complementary quite by surprise. [...] Without underestimating the ingenuity of either econometricians or theorists, it seems to me appropriate to entertain the possibility that reconciliation along both of these lines will fail, and that one of these traditions is fundamentally in error.

The thesis of this essay is that it is the econometric tradition, or more precisely, the “theory of economic policy” based on this tradition, which is in need of major revision. More particularly, 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, and that simulations using these models can, in principle, provide no useful information as to the actual consequences of alternative economic policies. (Lucas 1983, [1976], pp. 257–258)

The basis of his argument is the possibility of structural change, which would lead to changes in the empirical relationships between observed variables, such as price inflation and unemployment, on which these models are built. Such a structural change could be brought about by policy changes, and therefore, these models are not suited to forecasting the impact of different policy alternatives. In order to perform this task we would need models which are built on stable and invariant relationships.

The New Classical solution to the critique was a microfounded optimization-

based approach which incorporates the political conditions as side constraints to the derivation of the optimal “rules of choice” (Sargent 1981). Consistent microeconomic foundations “naturally required an emphasis on intertemporal optimization, which made expectations crucial, and made it natural to assume that the model was also internally consistent in the sense of positing forecasts by agents within the model that agreed with what the model itself would predict” (Woodford, 1999, p. 21), that is, rational expectations.

In a recent publication, Thomas Sargent (2015, p. 43) a longtime collaborator and coauthor of Robert Lucas, summarizes the impact of Lucas’s writings on monetary theory in the following words: “These beautifully written and wisely argued papers integrated macroeconomics, microeconomics, finance, and econometrics in ways that restructured big parts of macroeconomic research.” Lucas, Nobel laureate in 1995, was himself a student under Milton Friedman at the University of Chicago, and it is important to note that although he initiated a modern discussion of monetary neutrality, he considered the works of his professor far too important as to disregard monetary policy altogether. He thought of Real Business Cycle Theory “not as a positive theory suited to all historical time periods but as a normative benchmark providing a good approximation to events when monetary policy is conducted well and a bad approximation when it is not.” (Lucas, 1994, p. 13) This is so because expectation formation is never perfect and adjustments on the market are never immediate, i.e. there are monetary frictions and price and wage stickiness. Those ideas have been incorporated into the New Classical framework, which led to the most recent consensus approach.

The Modern Consensus Approach of New Keynesian DSGE Modeling

The models that emerged out of the New Classical contributions are so-called dynamic stochastic general equilibrium (DSGE) models. In any canonical version (Galí and Gertler, 2007; Galí 2008, chapter 3; Walsh, 2010, chapter 8; Woodford, 2003, chapter 3) of those models, representative agents are postulated, such as firms and households that have certain objective functions. The representative household maximizes its utility and the firms their monetary profits. The decisions of the other agents and the policy rule pursued by the central bank, usually a rule for setting short term interest rates, enter the respective optimization problems as side constraints. Firms produce the goods that enter the household’s utility functions in the form of a consumption index. The household supplies the labor that the firms demand for production. Money typically enters the model either as a separately postulated ad-hoc money demand function (Galí, 2008, p. 43), or as an argument of the household’s utility function (Walsh, 2010, p. 331). Firms pay wages to the household and the household pays the prices of the goods consumed. Those models thus attempt to incorporate all feedback effects of policy changes and the behavior of the different parties on one another.

The optimality conditions derived from the optimization problems of the agents, together with conditions of general equilibrium, i.e. the equality of supply and demand on the goods and labor markets, and further auxiliary assumptions are used to derive “truly structural” relationships that hold universally. It is then claimed by the proponents of this approach that the problems indicated by

the Lucas Critique are thereby solved, which renders counterfactual policy evaluation on the basis of these models possible, at least in principle. So Galí and Gertler (2007, p. 26) celebrate the advances:

Overall, the progress has been remarkable. A decade ago it would have been unimaginable that a tightly structured macroeconometric model would have much hope of capturing real-world data, let alone of being of any use in the monetary policy process. However, frameworks have been recently developed that forecast as well as the reduced-form models of an earlier era (for example, Christiano, Eichenbaum, and Evans, 2005; Smets and Wouters 2003, 2007). Because these models have explicit theoretical foundations, they can also be used for counterfactual policy experiments.

Fundamental assumptions of the New Classical variety are perfect competition, perfect price and wage flexibility, intertemporality of optimization (hence the predicate dynamic) and rational expectations. The general framework and some of the auxiliary elements have been incorporated into the recent consensus approach – others have been replaced by more or less Keynesian elements, which led to the term New Keynesian DSGE modeling. Given this combination of elements some economists refer to this conjuncture as the New Neoclassical Synthesis:¹³

13 Like the “old” Neoclassical Synthesis, advanced and popularized for example in Hicks (1937) and the various editions of Samuelson’s textbook (most recently Samuelson and Nordhaus 2009), it combines Keynesian and Classical elements, but no longer in the dichotomic way in which the Keynesian theory is simply viewed as applicable for the short-run analysis and the classical general equilibrium theory as explaining the

The New Neoclassical Synthesis inherits the spirit of the old, in that it combines Keynesian and classical elements. Methodologically, the new synthesis involves the systematic application of intertemporal optimization and rational expectations as stressed by Robert Lucas. In the synthesis, these ideas are applied to the pricing and output decisions at the heart of Keynesian models, new and old, as well as to the consumption, investment, and factor supply decisions that are at the heart of classical and RBC models. (Goodfriend and King, 1997, p. 232)

The price and output decisions are indeed the most characteristic Keynesian features in the new consensus approach. Output follows aggregate demand and instead of fully flexible prices, the New Keynesian DSGE models assume some kind of price and/or wage rigidities à la Calvo (1983), which render real effects of monetary policy within the model possible in the first place. It is simply assumed that only a fixed fraction of firms can adjust their selling prices in any given period.

The alleged success of this most recent approach in overcoming the Lucas Critique is energetically underlined by its proponents. It “has become in recent years the workhorse for the analysis of monetary policy, fluctuations, and welfare.” (Galí, 2008, p. 41) It was able

to show that it is possible to use the tools of modern macroeconomic theory – intertemporal equilibrium modeling, taking

long run. In the new synthesis the classical elements are used to describe the *potential* state of the economy, whereas the Keynesian elements are used to explain deviations from that state, i.e. the *actual* state of the economy. Compare to Woodford (1999, p. 29).

full account of the endogeneity of private-sector expectations – to analyze optimal interest-rate setting in a way that takes the concerns of central bankers seriously, while simultaneously taking account of the “New Classical” [Lucas] critique of traditional policy-evaluation exercises. (Woodford, 2003, p. 4)

The acid test for these new models, in accordance with Friedman (2008 [1953]), is their capability of closely reproducing observed macroeconomic time series, and predicting macroeconomic aggregates, like unemployment, output, and price inflation. Their predictive power is what ultimately justifies or disqualifies the underlying assumptions. Yet, not unlike the stagflation of the 1970s, the “structural change” that induced skepticism towards the traditional Keynesian approach, the most recent economic crisis, often referred to as the Great Recession, which has not been forecasted by any of the New Keynesian DSGE models applied in Central Banks around the world,¹⁴ provides ample reason to question the new orthodoxy. Has the problem raised by the Lucas Critique really been adequately solved by New Keynesian DSGE modeling? Hurtado (2014) provides empirical evidence for an answer in the negative. He finds that

14 The Fed, for example, employs amongst others two DSGE models, which are revised and extended versions of the models presented in Christiano, Eichenbaum, and Evans (2005) and Smets and Wouters (2007), respectively. The SIGMA model is an open economy model with multiple countries (Erceg, Guerrieri, and Gust 2006), whereas the ODE model is a closed economy model for the United States (Edge, Kiley, and Laforge 2008). Two of the most important models used by the ECB include the New Area White Model, based on Smets and Wouters (2003) who estimated a closed economy DSGE model for the euro zone, and the CMR Model based on Christiano, Rostagno, and Motto (2010). For a summary of the use of DSGE models in the ECB see also Smets et al. (2010).

the new models would not have yielded very different results from those of the old macroeconomic models had they been used instead back in the 1970s. In the following section we outline a strictly logical argument to support a negative answer to this important question.

III. A Refinement of the fundamental Problem implied in the Lucas Critique

As mentioned above, our brief history of modern mainstream macroeconomic thought is of rather uncontroversial content as it closely follows the history of thought established by economists who could themselves be considered part of the mainstream. In this section however, we want to leave this calm waters and discuss much more controversial and less accepted ideas, namely those of economists Ludwig von Mises and Hans-Hermann Hoppe, both of which are representatives of the Austrian School of economic thought.¹⁵

According to his biographer Mises’s works on the epistemological and methodological foundations of economics are

15 Interestingly, those ideas are so controversial that they are not even accepted by all economists who consider themselves to stand in the tradition of Austrian economics. For example, Friedrich von Hayek was in his later works influenced by Popper and his idea of falsification as argued in Hutchison (1981), who even called Hayek’s changing thought on methodology towards Popperian ideas a “U-turn”. Although Caldwell (1992) argues that Hutchison overstated Popper’s influence on Hayek’s methodological thought, it is without doubt that the late Hayek (“Hayek II”) and his intellectual followers disagree on Mises’s methodological approach. In his intellectual biography of Hayek Caldwell (2004, p. 420) claims that even the early Hayek (“Hayek I”) was opposed to Mises’s views. He writes: “Both Hutchison and I agreed that ‘Economics and Knowledge’ [(Hayek 1937)] contains a criticism by Hayek of Mises’s position. But I claimed that Hayek had never been a follower of Mises’s apriorist approach.”

the most neglected and least well understood among his many contributions (Hülsmann, 2007, p. 950; see also Murray Rothbard's preface to Mises, 2007, p. 12). Yet, as we will show, his views can help shed new light on one of the most important and acknowledged methodological contributions in modern macroeconomics – the Lucas Critique. It will help us substantiate the claim that New Keynesian DSGE modeling is not a solution to the fundamental problem implied in the critique, and that the problem will remain as long as the positivistic research agenda is pursued in macroeconomic analysis.

The Misesian Core of the Lucas Critique

Mises is a proponent of *methodological dualism*, that is, he believes that economics, which deals with human choice and action, as a discipline is categorically different from the natural sciences, which deal with inanimate objects. Therefore, a different method of enquiry is appropriate in economics. His view is opposed to *methodological monism*, which holds that the same methods used in the natural sciences are also applicable to economics. Advocates of this view consider the positivistic approach of hypotheses building and testing against empirical data as adequate across all scientific fields.

Notice, that the importance and adequacy of purely naturalistic analyses of the human body are thereby not disputed. Physiology and modern neuroscience analyze certain functions of the human body from the point of view of natural sciences and follow a positivistic approach. Yet, the important elements of the subject matter of economics, i.e. human action defined as purposeful behavior, cannot be observed. We cannot

observe purposes, goals, motives and motivation, and we cannot, at least not yet, physiologically explain those phenomena. We cannot physiologically explain ideas, thinking, choice and action. Hence, we take them as ultimate givens and make them the starting point of economic analysis (Mises, 2007, p. 3).

This is not to deny determinism. Mises (1962, p. 57) even writes that a human being “is at any instant of his life – his earthly pilgrimage – a product of the whole history of the universe. All his actions are the inevitable result of his individuality as shaped by all that preceded.” Human choice and action might therefore not be free, but we can stay agnostic about those puzzling questions. The fact is that we are still far away from being able to explain them in naturalistic terms. It is impossible to relate actions to any sensible number of ascertainable external factors in the sense that the action is the inevitable effect of the external factors as the cause. “There is nothing else that could be said about a definite instance of a man's acting and choosing than to ascribe it to this man's individuality.” (Mises, 1962, p. 58) Individuality then implies an absence of constant relationships between observable variables as the potential causes and other variables that are the products of human action as effects. This lack of constancy in economics poses serious and unavoidable problems for the positivistic research program followed by mainstream economics. It is the Misesian core of the Lucas Critique.¹⁶

16 Of course Lucas did not consider himself to be a follower of Misesian economics and this is not what we want to suggest with the phrase “Misesian core of the Lucas Critique”. Lucas acknowledged Friedrich von Hayek as an intellectual influence on him (Lucas, 1977). Hayek, another economist of the Austrian School, did not accept Mises's methodological position either, at least in his later

Empirical relationships observed in the past are subject to change in the future as long as at least one of those variables is the outcome of human action. Yet, those kind of relationships are an essential part of all modern mainstream macroeconomic models, either in the form of ad-hoc assumptions like the price setting behavior of agents (Calvo, 1983) or as derived “rules of choice” from an optimization problem.¹⁷ The New Keynesian DSGE models are then still vulnerable to the Lucas Critique, to the invariance of estimated model coefficients, as were the old Keynesian models. If one still advocates the positivistic approach of mainstream economics, it would be important to at least acknowledge this fundamental problem and not to claim, as has been done frequently, that the most recent models are no longer affected by it.

This is by no means the only deficiency of the mainstream approach. Others concern the measurability of certain concepts, measurement errors, and the pecuniary and non-pecuniary costs of data collection. However, the most fundamental problem still lies in the way we look at human beings; whether we assume their individuality away and look at them much like atoms, or whether we accept their capacity to think, reason, chose and act. Mises (1962, p. 25; 2007 [1957], p. 91) contrasts his views of methodological dualism with the monistic interpretation of philosopher Bertrand

Russell, who with some sense of nuance admits that there still is a difference between the behavior of atoms and human beings. However, Mises decidedly rejects Russell’s views. It is worthwhile quoting both authors at some length to illustrate the conflict. Russell (1997 [1935], pp. 152–153) writes:

According to quantum mechanics, it cannot be known what an atom will do in given circumstances; there are a definite set of alternatives open to it, and it chooses sometimes one, sometimes another. We know in what proportion of cases one choice will be made, in what proportion a second, or a third, and so on. But we do not know any law determining the choice in an individual instance. We are in the same position as a booking-office clerk at Paddington, who can discover, if he chooses, what proportion of travelers from that station go to Birmingham, what proportion to Exeter, and so on, but knows nothing of the individual reasons which lead to one choice in one case and another in another. The cases are, however, not *wholly* analogous, because the booking-office clerk has his non-professional moments, during which he can find out things about human beings which they do not mention when they are taking tickets. The physicist has no such advantage, because in his unprofessional moments he has no chance to observe atoms; when he is not in his laboratory, he can only observe what is done by large masses, consisting of many millions of atoms. And in his laboratory the atoms are scarcely more communicative than the people who take tickets in a hurry just before the train starts. His knowledge, therefore, is such as the booking-office clerk’s would be if he were always asleep except in working hours.

career (see footnote 15). Strictly speaking there are two different traditions within the Austrian School, the Hayekian and the Misesian. On this divide see Salerno (1993).

¹⁷ Price setting can also be modeled endogenously as in Bakhshi et al. (2003), but it does not really make a difference for the problem we are dealing with. It merely pushes the problem one step back. Their analysis still relies on assumed constancy in the factors which determine price setting behavior.

Russell correctly informs his readers that the booking-office clerk may find out about his clients' reasons to go to Birmingham or Exeter, and that the physicist has no such options when investigating the behavior of atoms. This however, is not merely due to a lack of communicativeness on the part of the atoms. The clerk does in fact know that ticket buyers have *reasons* for buying certain tickets. He can find out about their motives for going to one place rather than another. The fundamental difference is that he knows that human beings choose and act, and that their motives for choosing and acting in a certain way may change. Mises (1962, pp. 25–26) counters:

It is characteristic of the reasoning of Russell that he exemplifies his case by referring to the mind of a subaltern clerk to whom the unvarying performance of a strictly limited number of simple operations is assigned. What such a man (whose work could be performed as well by a vending automaton) thinks about things that transcend the narrow sphere of his duties is without avail. To the promoters who took the initiative in advancing the project of the railroad, to the capitalists who invested in the company, and to the managers who administer its operations, the problems involved appear in a quite different light. They built and operate the road because they anticipate the fact that there are certain reasons that will induce a number of people to travel from one point of their route to another. They know the conditions that determine these people's behavior, they know also that these conditions are changing, and they are intent upon influencing the size and the direction of these changes in order to preserve and to increase their patronage and the enterpri-

se's proceeds. Their conduct of business has nothing to do with a reliance upon the existence of a mythical "statistical law." It is guided by the insight that there is a latent demand for travel facilities on the part of such a number of people that it pays to satisfy it by the operation of a railroad. And they are fully aware of the fact that the quantity of service they are able to sell could be drastically reduced one day to such an extent that they would be forced to go out of business.

Bertrand Russell and all other positivists referring to what they call "statistical laws" are committing a serious blunder in commenting upon human statistics, i.e., statistics dealing with facts of human action as distinguished from the facts of human physiology. They do not take into account the fact that all these statistical figures are continually changing, sometimes more, sometimes less rapidly. There is in human valuations and consequently in human actions no such regularity as in the field investigated by the natural sciences. Human behavior is guided by motives, and the historian dealing with the past as well as the businessman intent upon anticipating the future must try to "understand" this behavior.

Statistical and empirical analysis for studying human action, in Mises's view, is then a descriptive tool, applicable only to historical events of the past. It does not allow for inferring universal "statistical laws". It can neither validate nor falsify economic theory, which should instead be derived deductively from the logical implications of human choice and action. But economic theory can help us understand the past. It can help making sense of a negative correlation between price inflation and unemployment over

certain periods of time and a positive link between them in other periods, or with a certain time lag between the time series. The monetary theory of the business cycle developed in Mises (1953) for example can explain the empirical phenomenon of the short-run Phillips curve through the initial boom period that follows monetary expansion.¹⁸

It would however be false to declare the empirical observation of a Phillips curve to be a necessary consequence of any monetary expansion. The observable reality is complex and several forces work at the same time in the same or opposite directions. Technological progress and innovation can have an impact on both employment and prices and change the empirically observable time series and their correlation. Economic theory therefore cannot make exact scientific predictions of the form: policy X will lead to an increase or decrease of variable Y by an amount of Z, as long as Y is the result of individual action. We will now go into a more recent and improved justification of this position.

A Substantiation of the Misesian View

However compelling or non-compelling Mises's views on the foundations of economic theory might be for any contemporary reader, it is true that he did not provide a sufficiently elaborated logical argument for his rejection of monism. According to Hoppe (1983, p. 8) it was Karl R. Popper, by many seen as the intellectual father of modern positivism, who unwillingly provided the groundwork for such an argument. It was Hoppe's achievement to reconstruct and

incorporate this argument into the Misesian framework.

In the preface of Popper (2002 [1957]) we find the crux of his argument in refutation of historicism (in particular the views of Marx and Spengler as he points out in the preface to the German language edition): because human knowledge can grow and we cannot scientifically predict the state of our future knowledge, but our knowledge on the other hand influences the path of history, "there can be no scientific theory of historical development serving as a basis for historical prediction." This insight, however, can not only serve as a basis for the refutation of historicism, but also as a justification for the Misesian claim that there is a lack of constancy in human action – it is after all human action that shapes the path of history. The argument implies a logical refutation of the positivistic approach to economics. But let us now recapitulate the Hoppean argumentation step by step.

The first step in the argument lies in the necessity of the constancy principle for the positivistic approach of hypothesis building and testing. It is in principle only possible to falsify a hypothesis if we assume constancy in the relationship between the observable causes and effects: *the same configuration of causes produces the same effect, and differences in the effects imply different causes.*

If we employ the same econometric method to identify a relationship between one "explained" variable as effect and one or more "explanatory" variables as causes in two different data sets, and find that the relationships are different in the two sets, then we implicitly assume the constancy principle, when we conclude that there must have been at least one ignored causal factor at work in the generation of one data set, but not

¹⁸ As mentioned above we will not go into more detail here. For a complete picture of how Mises thought about and developed economic theory see Mises (1998).

or not to the same extent in the generation of the other. This is exactly what the positivist does when he revises his initial hypothesis in the hope of incorporating those missed factors.

However, this constancy assumption can neither be falsified nor verified by experience. If we observe two different effects, we cannot exclude the possibility of eventually identifying different causes in accordance with that assumption that produced the different effects. If we want to conclude from observing two equal effects that their causes must have been equal, we implicitly assume the constancy principle. If we do not assume it, then, from observing equal effects nothing follows about the causes, and for a verification of the principle we would have to empirically investigate *all* potential causes in the whole universe and identify them as being equal, which is in principle impossible (Hoppe, 1983, pp. 11–12). Now if this is so, then how can one justify the Misesian claim that the constancy principle does not hold in the realm of human choice and action? It can only be justified through a logical argument.

The second step consists of such a logical argument. As Popper claimed, human knowledge can change and we cannot scientifically predict our future state of knowledge, that is in other words, we can learn new things and we cannot scientifically predict what we will learn in the future, not even the near future. If we could it would precisely not be learning, since we would have to know what we will “learn” beforehand.¹⁹ The state-

ment that human beings learn is again not falsifiable, since its falsification would precisely be an act of learning. The truth of the statement, like for any other logically true statement, is not dependent on experience. It is *a priori* true. One cannot argumentatively deny the truth of the statement that human beings learn, without implicitly assuming its truth, since any argumentation presupposes the possibility of answers and rebottles that are contingent on the arguments presented – even if it is only: “Okay, I see you are right.” And then in fact someone would have learned something, which is a contradiction, since we cannot learn that we cannot learn. We may in fact consider every conscious sense experience and every thought as an act of learning – learning the fact that a certain sense experience was made or that a certain thought occurred in our mind at definite points in time and space. And positivistic enquiries in the social sciences all have the declared purpose of learning something about the world.

Human action, as the employment of means to attain chosen ends, is contingent on our knowledge and believes over what means are suitable to attain certain ends. If knowledge is not scientifically predictable and human action is contingent on knowledge, then human action itself is not scientifically predictable. The constancy principle applied to the field of human action would imply that human beings cannot learn. It would imply that human beings cannot incorporate new information and knowledge into their choices and actions. Since this is an indefensible position and human beings

19 In fact knowledge can not only grow, but we could also lose or forget already acquired knowledge. Every student of any subject who is not exceptionally gifted knows that acquired pieces of knowledge may get lost quickly, if they are not repeatedly and actively kept in mind. On a larger

scale, the Renaissance, for example, is considered to be a period in Western history that is marked by a rebirth of antique Roman and Greek culture and knowledge that has been lost or forgotten during the Middle Ages.

can in fact learn, the constancy principle must be rejected as false by logical contradiction.

The third and final step is simply the logical combination of the first two steps. The constancy principle is the necessary condition for falsification, and thus for the positivistic approach in general. Yet, the constancy principle must be rejected if the “explained” empirical phenomenon is the result of human action. This is because human beings learn. Hence, the positivistic approach in economics is contradictory.

Strictly speaking, we couldn’t even think of certain individuals as literally being in a state of “non-learning”, a state of “evenly rotating” daily routine for a time period of some length, as intuitively illustrative this description might seem in certain cases. The idea of “non-learning”, of no change, becomes even more absurd for any person over the span of his lifetime, or for all individuals forming a society over time, from one generation to the next. There is significant change in the ends we value, in our knowledge about the means suitable to attain these ends, in fashion, in ideology, and in our culture in general. In particular, there are changes in the way we are dealing with money, not exclusively because of technological progress and learning, but also because of the changing political and societal environment into which we are born.²⁰ It is therefore rather inappropriate to treat macroeconomic time series of the past 100 years as being generated by some homogenous and self-repeating mechanism that would allow generalizations of the kind commonly drawn in modern macroeconomics. To the con-

trary, the lack of constancy explodes the basis for this positivistic research program.

IV. Concluding Remarks

The important conclusion that we tried to convey in this paper is that the hard core of the Lucas Critique, carved out using the epistemological and methodological contributions of Ludwig von Mises and Hans-Hermann Hoppe, remains relevant for any model of modern mainstream macroeconomics, even those advocated by Lucas himself. It should therefore not come as a surprise that traditional Keynesian models, as well as New Classical and New Keynesian models are all used side by side in public policy institutions, including central banks, and that at least some researchers were reluctant to incorporate the lessons from the Lucas Critique into their models (Ericsson and Irons, 1995; Goutsmedt, et al. 2015), as the radical implication would have been to abandon their positivistic foundations altogether, if one really wanted to avoid the problem. Ultimately, all of these models suffer from the same deficiency. The critique as understood and refined in this paper is not so much a critique of any particular modern macroeconomic models, but rather one of the entire positivistic research agenda.

Even though monism is from the point of view of the learning human actor a path riddled with contradictions, we can of course not eliminate the possibility that all phenomena ultimately follow the same laws and that there is in fact a monistic structure underlying everything, the external world as well as the human mind. As Ludwig von Mises (2007 [1957], p. 1) writes:

²⁰ For a description of the cultural changes in an inflationary environment for example, see Hülsmann (2008, chapter 13) and Hülsmann (2013, chapter 10).

Mortal man does not know how the universe and all that it contains may appear to a superhuman intelligence. Perhaps such an exalted mind is in a position to elaborate a coherent and comprehensive monistic interpretation of all phenomena. Man – up to now, at least – has always gone lamentably amiss in his attempts to bridge the gulf that he sees yawning between mind and matter, between the rider and the horse, between the mason and the stone. It would be preposterous to view this failure as a sufficient demonstration of the soundness of a dualistic philosophy. All that we can infer from it is that science – at least for the time being – must adopt a dualistic approach, less as a philosophical explanation than as a methodological device.

The argument presented in this paper is not considered the be-all and end-all of the theory of everything. It is valid only from the perspective of a learning human being. Since presumably we all belong to that group, it carries some weight. It is then a curious diagnosis to note that modern mainstream macroeconomists, to the extent that they look at their fellow human beings – their objects of enquiry – as inanimate, constant automatons, are implicitly elevating themselves to the ranks of such a superhuman intelligence Mises mentioned. They put themselves in the position of a zoologist who studies some species distinct from his own – and presumably inferior. We suggest that it is time to get off that high horse and honestly admit the fundamental problems involved in such an approach.

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