What can civil society expect from academic macroeconomics?

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Abstract

Academic macroeconomics as it has been practiced for the last three decades has a bad reputation, especially after the onset of the 2008 recession. The aim of this paper is to reflect on this state of affairs. To begin, I draw a comparison between Keynesian and Lucasian macroeconomics, bringing to light that they are based on different tenets. Next, I claim that because of its higher internal consistency, Lucasian macroeconomics is superior to Keynesian. However, I also claim that espousing it bears a heavy price — in particular a limited usefulness for policymaking and an inability to come to grips with economic crises.

Keywords: Keynesian macroeconomics, Lucas, Real Business Cycle models
JEL: B 22, E 12, E 13

◊ IRES, University of Louvain. This paper originates from two lectures given at the University of Gent and at the Paris-Sorbonne University. Lectures are lighter than articles as their aim is to convey a general message without bothering too much about loose ends. I thank the reader in advance for keeping this feature in mind when reading this paper. I also wish express my gratitude to Luca Pensieroso and Mara Squicciarini for their comments. Correspondence: michel.devroey@uclouvain.be

"Most macroeconomics of the past 30 years was spectacularly useless at best, and positively harmful at worst" (Paul Krugman 2009)

"All men know the use of the useful, but nobody knows the use of the useless." Chuang Tzu (c.360 BC - c. 275 BC)

Introduction

The question I wish to address in this paper surely makes sense in the context of the times we live in. For the past five years, our economies have experienced a deep recession, an event that strongly influenced the general perception of academic macroeconomics. In a nutshell, its reputation is at low ebb. Is this judgment justified? On a broader level, what can civil society gain from macroeconomic theory? Is the discussion triggered by this last question riddled with ambiguities and misunderstandings, and if so, of which nature? These are the questions that I want tackle. To that end, I will follow a historical thread starting with an evocation of the rise of macroeconomics and continuing with the confrontation between Keynesian and Lucasian macroeconomics, the two successive paradigms that have dominated the field over the past fifty years. These characterization and confrontation tasks settled, the last three sections of the paper are concerned with assessment matters. There I raise three points. The first answers the naïve and blunt question of which, in the contest between old Keynesian (a label which somebody like James Tobin had no qualms with) and Lucasian macroeconomics is the winner. Next, after having submitted that Lucasian is superior to Keynesian macroeconomics, I highlight the limitations that burden this approach. Finally, in my concluding comments, I submit my answer to the question that forms the paper's title.

I. The rise of macroeconomics

The sub-discipline of macroeconomics studies aggregate economic variables such as employment, output, the general price level, the interest rate, etc. It came to the light of day in the wake of WW II. Previously, it existed under the name of monetary theory, and its concern was the study of how money, in particular the supply of money, had an impact on 'real' economic outcomes as studied by pure economics.

The piece that, rightly enough, is considered its starting point is John Maynard Keynes's 1936 book, *The General Theory of Employment, Money and Interest* (in short *The General Theory*). Before writing it, Keynes was already a towering figure within the economic profession and a widely recognized expert on monetary matters. His main concern was practical policy but this changed with the onset of the Great Depression. This dramatic event

— unemployment, peaking above 20 % in several countries — triggered Keynes to become more concerned with high theory. He felt that the prevailing economic theory of the time (mainly Marshallian neoclassical theory) proved unable to come to grips with the occurrence of mass unemployment. The 1930s were also a time when Russia experienced strong economic growth, to the effect that an electoral victory of parties leaning towards communism (or their taking power in more unorthodox ways) was a possibility that could not be disregarded. In short, capitalism was in peril, both economically and politically, and Keynes realized that its survival required important modifications of its functioning. While these elements, beautifully expressed in the concluding chapter of *The General Theory*, loomed in the back of Keynes's mind, his endeavor was mainly theoretical. For him, the economic theory of the time was wanting and the policy conclusions that it reached were exactly the opposite of what needed be done. This was the state of affairs he wanted to change. As aptly noted by Robert Skidelsky, Keynes's biographer, this venture intertwined theory and persuasion:

> Keynes understood that his theory had to be usable for politicians and administrators: easily applied, offering political dividends. But he also understood that, before he could win the political argument, he had to win the intellectual argument (Skidelsky 1992, p. 344).

The main diagnosis of the crisis available to economists at the time was of 'Austrian' origin. In this account, the crisis signaled a situation of overinvestment and misallocation of resources, a state of affairs that required a process of 'liquidation' for its solution. This was to be achieved by a real wage deflation and the elimination of failing firms. Flexibility was thus the motto. The more flexible prices and wages could be, the faster the liquidation process could come to an end and conditions for prosperity be re-established. However, when the depression kept its course without wages deflation exerting its proclaimed effect, economists started to waver about the virtues of 'laissez faire' and began to envisage that the state should engage more actively in the economy. Thus, economists were torn between the policy conclusions that accepted theory dictated and their gut feeling that another policy should be adopted. Keynes's aim was to remove this contradiction by providing a theoretical argument in favor of this gut feeling. He did not envision a total overhaul of standard economic theory, but rather its emendation. The task he set for himself was to demonstrate that the economy could be stuck in a state of equilibrium while also featuring the presence of involuntary unemployment, a notion that had no place in the lexicon of accepted economic theory. In a nutshell, his explanation was that involuntary unemployment resulted from a deficiency in aggregate demand, itself the result of insufficient investment. To move the economy away from its involuntary unemployment equilibrium, a policy of autonomous demand activation was needed.

Did Keynes succeed in his theoretical endeavor? In my opinion, he did not (though I hasten to say that he should not be blamed for this, the task was insuperable). Let me just

point out one defect. Keynes definitely wanted to exonerate overly high wages from being the cause of involuntary unemployment. Nonetheless, when constructing his demonstration of the deficiency in effective demand, he made the assumption that wages were rigid, allegedly in order to simplify his argumentation. This is odd: when trying to dethrone a theory, one does not take up its cornerstone assumption as the basis of one's alternative construction. A more convoluted argumentation would be better than adopting the rival assumption. Keynes claimed that he would dispense with the rigidity assumption later in the book, but in my eyes his argumentation, (found in Chapter 19) barely stands up to close scrutiny. So, my claim is that Keynes was unable to vindicate involuntary unemployment without resorting to the wage rigidity assumption, which he outwardly rejected.

Be that as it may, Keynes's book was received with enthusiasm, especially from young economists. Dissatisfied with the existing situation, they were yearning for a new theory that would justify the abandonment of the laissez-faire doctrine. In this respect, Keynes's work delivered beautifully. Small wonder then that it was received as a "liberating revelation" (Leijonhufvud 1968, p. 31). Nevertheless, confusion over the central message of Keynes's book was great, even amongst his admirers. In effect, *The General Theory* was a complex book, hard to read with its intertwining of different types of arguments developed at distinct levels of abstraction, the compatibility of which was by no means obvious.

Progress in this respect came when Hicks succeeded in transforming Keynes's cryptic contribution into a simple simultaneous equations system. This became the renowned IS-LM model, the starting point of what can be called Keynesian macroeconomics — a paradigm that reigned over the profession for the twenty-five years that followed the end of WWII. In this model, effective demand is outstanding for its absence, and the existence of involuntary unemployment is now explicitly attributed to wage rigidity — the very cause that Keynes had desired to dispel! In spite of this drawback, Keynesian macroeconomics underwent tremendous development. The initial model was extended in several directions by giving a more explicit role to the state, integrating the international dimension, etc. Each of its components (consumption, investment, portfolio choice and the labor market) became the object of extended investigation.

Such a success cannot be due to mere luck. The IS-LM model has two main virtues. The first is its ability to model economic interdependence in a simple and intuitive way. In this respect the IS-LM model has been unrivalled. Even in its most elementary form, it lends itself to drawing cogent real-world inferences. Its second key virtue is its plasticity. The IS-LM model provides an architecture that is general enough to allow a quasi-unlimited diversity of specifications. A corollary of this plasticity is pragmatism. Whenever new specifications needed to be added, they originated from the observation of reality rather than theoretical

considerations. This plasticity also applies to policy implications since supporters and foes of Keynesian policy alike could use it to promote or refute policy prescriptions.

The IS-LM started as an abstract model but under the stewardship of another great Keynesian economist, Lawrence Klein, it was transformed into an empirically testable model. The Klein-Goldberger model (1955) marked the start of a long chain of macroeconometric models used for making predictions as well as for assessing alternative policies.

These are the three stages — *The General Theory*, the IS-LM model and the birth of Keynesian econometric models — through which macroeconomics came into existence as a new sub-discipline of economics. The prominent aim of Keynesian macroeconomics was to highlight market failures and to vindicate that they could be remedied by state interventions. So from the onset, Keynesian macroeconomics had a reformist flavor. It soon thrived, being adopted by both universities and public institutions such as central banks.

II. Characterizing Keynesian macroeconomics

To confront Keynesian and Lucasian macroeconomics, I have retained the following benchmarks: (1) macroeconomics' overarching *explanandum*; (2) the underpinning equilibrium concept; (3) whether models belong to general equilibrium analysis, i.e. do they deal with the economy as a whole *and* do they study the interactions between its composing elements? (4) the place given to expectations; (5) the relative importance assigned to either aggregate supply or aggregated demand in determining activity; (6) the crucial methodological principle; (7) the attitude towards the microfoundations requirement; (8) accessibility to laymen; (9) the spontaneous policy conclusion.[1] Let me start with using them for describing Keynesian macroeconomics.

1. The *overarching object of study* of Keynesian macroeconomics is involuntary unemployment, a departure from full employment. It is viewed as the result of some sluggishness in the adjustment of wages. State interventions aiming at activating demand through fiscal policy or monetary activation are the remedy to be resorted to.
2. The underlying *notion of equilibrium* is the traditional stationary equilibrium concept, already used by Adam Smith and other classical economists. Equilibrium is viewed as a state of rest and a center of gravitation. In such a concept the notions of equilibrium and disequilibrium are intertwined. Equilibrium is supposed to exist only tendentially, which means that the economy is practically always in disequilibrium. Disequilibrium, it is declared, exists as a short-period phenomenon, and its study is assigned to Keynesian theory. In turn, the task assigned to classical theory — meaning Walrasian

[1] The 'spontaneous' qualification refers to the vision linked with the basic model, it being admitted that it can be reversed in further models.

theory in its traditional understanding (i.e. distinct from neo-Walrasian theory) — is the study of the long period. A relation of gravitation, it is assumed, links these two time categories. The *neoclassical synthesis* conveys the idea that the two types of analyses can be integrated.[2]

3. Does Keynesian macroeconomics qualify as *general equilibrium* according to the two criteria enunciated in the introduction? The answer is 'No'. The IS-LM model is wanting in this respect because when described narratively, it comprises four markets (goods, money, labor, and bonds), while only three of these are part of the analytical device. As far as they are concerned, Keynesian macroeconometric models study an entire economy. However, since the different equations are estimated in isolation with no cross-equations restrictions considered, they fail with respect the second criterion for general equilibrium.
4. *Expectations*. All in all, expectations play a minor role in Keynesian macroeconomics. The usual view is to consider them as backward-looking or adaptive.
5. *Supply or demand as the driving factor*. According to the Keynesian approach, variations in employment result from changes in aggregate demand. The underlying picture is that labor suppliers are passive, employment decisions being made unilaterally by firms. Moreover, this approach tends to consider the supply of labor and the labor force as the same thing, a fixed magnitude.
6. *The central methodological principle* of Keynesian macroeconomics is external consistency. Models are only as good as they are realistic. The prevailing intellectual mood is pragmatic. That several of the basic notions — involuntary unemployment, full employment, rigidity and sluggishness — are defined in a loose way, that the analysis focuses on the short period with no attention being given to the linkage between the short and the long period, are by no means considered harmful methodological practices. Empirical models, the construction of which is often left to engineers rather than economists, are more data- than theory-constrained.
7. *Microfoundations.* They are present in some seminal Keynesian models (e.g. consumption and savings over the life cycle models). Reference to the notion of involuntary unemployment also testifies to a microfoundational perspective, be it only in a negative way. Nonetheless Keynesian macroeconomics usually follows suit with the Marshallian principle that analysis can start at the level of market supply and demand functions rather than at the individual decision-making level.
8. *Accessibility to laymen*. The result of this pragmatic attitude is that Keynesian macroeconomics theory is simple to understand even by non-economists. Its level of technicality is low. Many of its basic notions have made their way into newspapers and political discourses.

[2] Cf. De Vroey and Duarte (2013).

9. *Spontaneous policy conclusion*. The vision of the economy most congenial to Keynesian macroeconomics can be branded 'mitigated liberalism'. It defends the market system as being superior to a planning system without fully advocating laissez faire. Keynesian models tend to support demand activation by the state.

Table 1. Characterizing Keynesian macroeconomics

Criteria	**Keynesian macroeconomics**
1) Overarching *explanandum*	Involuntary unemployment
2) Equilibrium concept	Stationary equilibrium concept
3) General equilibrium analysis	No
4) Expectations	Secondary place
5) Emphasis on supply of or demand for labor	Demand
6) Main methodological priority	External consistency
7) Microfoundations	Not required
8) Accessibility to laymen	Easy
9) Spontaneous policy conclusion	Demand activation

III. Lucasian macroeconomics

Keynesian macroeconomics reigned for a quarter century before losing its grip over the profession. Milton Friedman played an important role in its dismissal. However, his dislike of Keynesian theory was more a matter of disagreement about policy conclusions than about methodology.[3] In contrast, when a new generation of economists led by Robert Lucas and including Thomas Sargent, Neil Wallace, and Robert Barro began to attack Keynesian theory, they were led to envision a total overhaul. As Lucas was the leading figure in this dismissal as well as the methodological spokesperson of the new school, I will be mainly concerned with him.

The change that occurred in macroeconomics deserves to be called a 'scientific revolution' in the sense proposed by Thomas Kuhn, referring to episodes in the history of a discipline where a series of unsolved puzzles pile up, disturbing its normal development. This situation of unrest and dissatisfaction with existing theory triggers a drive to change the agenda, the conceptual toolbox, and the existing research methods in radical ways. This is often accompanied by thundering declarations of war, confrontations between younger and

[3] Friedman's monetarism is based on the same methodological principles as Keynesian macroeconomics with the exception of criteria (1) and (9).

older generations of researchers, the rise of new stars in the profession, and the waning of the previous ones.

Like all scientific revolutions, the new classical revolution was two-legged, with a criticism of the existing paradigm on one side, and the construction of a new one on the other. Lacking time to analyze the critical component, let me just say (a) that Lucas argued that Keynes did not deserve the 'great economist' stature usually ascribed to him; his main contribution, he claimed, was ideological and political; it consisted of having helped to preserve capitalist economies from the socialist temptation; as far as theory was concerned, his contributions were minor; (b) that the central concepts presented by Keynes in *The General Theory* (involuntary unemployment and full employment in particular) were shallow; (c) that as a result of their lack of microfoundations, Keynesian econometric models were of no help for comparing alternative policy measures (the famous 'Lucas critique').

As for aspects of novelty, let me start with a semantic remark. Substantively, the best appellation for the new paradigm would have been 'dynamic stochastic general equilibrium' (DSGE) modeling. As stated by Narayana Kocherlakota:

> Dynamic refers to the forward-looking behavior of households and firms. Stochastic refers to the inclusion of shocks. General refers to the inclusion of the entire economy. Finally, equilibrium refers to the inclusion of explicit constraints and objectives for the households and firms (Kocherlakota 2010, p. 9).

These four elements are indeed central to Lucas's vision of macroeconomics. Unfortunately, this appellation cannot be used. To date, three successive generations of models belonging to the lineage inaugurated by Lucas have surfaced. Lucas's own model branded as 'new classical' macroeconomics was the first. It was succeeded by real business modeling, initiated by Finn Kydland and Edward Prescott (1982), and John Long and Charles Plosser (1983). In the late 1990s, a third generation of models emerged that received the DSGE appellation. The DSGE label being so preempted, it cannot be used to designate the wider program of which so-called DSGE models are just an installment. Therefore I propose to fall back on the 'Lucasian macroeconomics' or 'Lucasian research program' terminology. As the main traits of Lucasian macroeconomics are already present in new classical macroeconomics, much of my description of is also valid for the two subsequent generations of models.

A fine means of capturing the gist of Lucas's contribution can be found in Rodolfo Manuelli and Thomas Sargent's review of Lucas's book, *Models of Business Cycles*. It consists, they state, in setting up "particular sets of rules and techniques to model aggregative economic observations" (Manuelli and Sargent 1988, p. 523). These rules are meant to act as a standard-setter, discriminating between up-to-the standard and sub-standard models. Prominent among these criteria are: the adoption of a general equilibrium perspective,

dynamic analysis, the rational expectations assumption, microfoundations, the presence of shocks in a stochastic environment, and, finally a procedure for empirical confrontation. [4]

More generally, Manuelli and Sargent note that Lucas's contribution manifests "a desire to be intellectually conservative in the sense of preserving as much contact as possible with a quantity theory tradition that emerged from pre-Lucas modeling rules" (Manuelli and Sargent 1988, p. 526). I translate this remark as meaning that macroeconomics ought to be neoclassical (in the narrow sense, i.e. characterized by Walrasian microfoundations, optimizing behavior and market clearance, all these terms being synonomous). Strikingly enough, these recommendations were made in the beginning of the 1970s, which were rather gloomy times for neoclassical theory. I remember my Italian international trade teacher, a Chicago graduate, protesting when we criticized his course for being too neoclassical and his arguing that it was Ricardian rather than neoclassical. Many young students, of which I was one, believed that the future of economics lay in Marxian theory. At a different level and less provocatively, many economists believed that the time was ripe for Herbert Simon's notion of bounded rationality to replace the more extreme notion of optimizing rationality upon which economic theory had traditionally been based. Thus, when Lucas and Sargent expressed the view that macroeconomics should be neoclassical, they were definitely leaning against the wind.

These preliminary points settled, let me now proceed with the characterization of Lucasian macroeconomics.

1. *Overarching aim*. The business cycle replaces unemployment as the central *explanandum* of macroeconomics. Of course, variations in economic activity (and hence in employment) are a central item in the study of economic fluctuations. However, in the new paradigm they are accounted for in terms of hours worked without consideration of the split between the employed and the unemployed.
2. *Equilibrium concept*. According to Lucas, the stationary equilibrium concept is a thing of the past. It should be replaced with a new concept of neo-Walrasian inspiration. No precise name was given, but it could be called 'intertemporal equilibrium', the terminology that John Hicks used in *Value and Capital,* (Hicks 1939), where he blazed the way for neo-Walrasian theory. [5] Intertemporal equilibrium designates a state where all agents succeed in maximizing their intertemporal objective function, utility for households, and profits for firms. Equilibrium thus refers to an equilibrium

[4] This last item is formulated in general terms due to the existence of dissent between defenders of calibration and defenders of econometric testing.

[5] Franco Donzelli (1989, 2007) persuasively argues that this concept is already present in the later editions of Walras's *Elements,* to the effect that the traditional attribution of a state of rest concept of equilibrium to Walras is mistaken. If we follow Donzelli, the stationary equilibrium concept is associated with the traditional, long-period equilibrium interpretation of Walras, while the intertemporal equilibrium concept is associated with an interpretation of Walras viewing him as a forerunner of neo-Walrasian theory.

time path. In this concept, the economy can display changing prices and quantities over time and still be deemed to feature equilibrium. As a result, the disequilibrium notion can be dispensed with. The 'equilibrium theory of the business cycle' label aptly expresses Lucas's project: to construct a general theory of business fluctuations on the premises of optimizing behavior and market clearing — an attempt that was previously considered totally implausible. The merit of Lucas, and later of Kydland and Prescott, was to have invalidated this skeptical opinion. As a result, a totally different picture of the business cycle emerged. Earlier, it was viewed as the disequilibrium phenomenon *par excellence*, the manifestation of a market failure. The mere assertion of its existence was seen as an invitation for the state to take steps in making it disappear. In the new approach, the business cycle expresses the optimizing reactions of agents to outside shocks affecting the economy. Another result of this change in perspective is the dismissal of the neoclassical synthesis idea, now regarded as an unnecessary dichotomy. Once the intertemporal equilibrium concept is adopted, there is no longer any reason for trying to build a synthesis between Keynesian disequilibrium theory and Walrasian equilibrium theory. The latter is able to do what it takes.

3. *General equilibrium.* Unlike Keynesian macroeconomics, Lucasian macroeconomics abides by the criteria for general equilibrium analysis.
4. *Expectations.* Expectations are depicted as forward-looking; the rational expectations hypothesis (the assumption that agents' subjective expectations about coming events coincide with the model-builder's objective expectations) is a *sine qua non* for sound macroeconomics.
5. *Supply and demand*. Lucas's hunch (first presented in a paper co-authored with Rapping) was that changes in the supply of labor, viewed as a result of intertemporal optimizing decision-making, played a central role in explaining fluctuations. This intertemporal substitution phenomenon, Lucas contended, is decisive in explaining variations in the level of activity over time. On the basis of this insight, he constructed a model of the business cycle where variations in activity over time are due to two factors: exogenous monetary shocks, , and agents' imperfect information.
6. *Main methodological principle*. Lucas wants macroeconomics to abide by the Walrasian methodological principles. Accordingly, internal consistency is the alpha and the omega of theoretical construction.
7. *Microfoundations.* Explicit microfoundations are another *sine qua non* for writing sound theory.
8. *Accessibility to laymen*. Lucasian macroeconomics resorts to new complex mathematical techniques, such as dynamic programming. Consequently, the technical barrier to entry is much higher than is the case for Keynesian macroeconomics.

9. *Spontaneously underpinning ideology*. Keynesian theory was geared toward highlighting market malfunctioning. Here, the opposite conclusion, a defense of laissez faire, emerges, at least initially. Fluctuations in employment reflect rational and optimal reactions by economic agents to changing conditions. If there is no pathology, there is also no need for the state to intervene.

Table 2 summarizes the above argument and offers a comparison between the two paradigms.

Table 2. Characterizing Lucasian in contrast to Keynesian macroeconomics

Criteria	**Keynesian macroeconomics**	**Lucasian macroeconomics**
1) Overarching *explanandum*	Involuntary unemployment	Business fluctuations
2) Equilibrium concept	Stationary equilibrium concept	Intertemporal equilibrium
3) General equilibrium?	No	Yes
4) Expectations	Secondary role	Rational expectations
5) Supply of or demand for labor?	Demand	Supply
6) Main methodological priority	External consistency	Internal consistency
7) Microfoundations	Not strictly required	Strictly required
8) Accessibility to laymen	Easy	Difficult
9) Spontaneous policy conclusion	Demand activation	No rationale for demand activation

Two points stand out. The first is that these two visions of macroeconomics are polar opposites. The second, on which I cannot dwell, is that the transition from Keynesian to new classical macroeconomics can be interpreted as a transition from Marshallian to Walrasian macroeconomics. This conclusion is visible when looking at benchmarks 2, 3, 6, 7 and 8, all elements that shape the difference between the Walrasian and the Marshallian approaches.

IV. Differentiating the three generations of Lucasian macroeconomics

New classical macroeconomics

Lucas's own model of the business cycle, a spin off of his 1972 article "Expectations and the Neutrality of Money", was based on Edmund Phelps' island parable. In Lucas's model, producers receive a signal incorporating two distinct pieces of information. Were they present separately, these two pieces of information would trigger opposite reactions, changing or

maintaining total working hours. Needing to engage in signal extracting, agents will mix the two opposite reactions in some weighted way. Hence the number of hours worked departs from what they would have been with perfect information. Here, Lucas claimed, rests the explanation of the variations in hours worked over the business cycle. Monetary shocks have real effects, but, as argued by Friedman, the government cannot exploit them since they occur only when the changes in money supply are unanticipated.

The prevalence of this *model* was short-lived. Among other things, it was criticized on the grounds that information about money supply changes is rather easily accessible, to the effect that the signaling problem could not be seen as a real burden for producers' decision-making process. However, no abandonment of the Lucasian *program* ensued. Rather, it underwent an inner evolution led by Kydland and Prescott that resulted in the emergence of real business cycle (RBC) modeling.

RBC modeling

Lucas had expressed the view that the task ahead in business cycle theory was to write a FORTRAN program, "a fully articulate artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies" (Lucas [1977] 1981, p. 219). While he himself contributed little to the implementation of this program, Kydland and Prescott took his prescription to heart and did the job with their trailblazing 1982 paper, "Time to Build and Aggregate Fluctuations". The model it comprises has the same ingredients as Lucas's: the equilibrium discipline, microfoundations, rational expectations, a dynamic-stochastic environment, and intertemporal substitution. But there are also striking differences. First, Kydland and Prescott abandoned Lucas's insight that the shocks triggering the business cycle were monetary. Instead their hunch was that technology shocks were the true suspect. Second, they abandoned Lucas's imperfect information line of research. Third and most important, Kydland and Prescott's work was quantitative while Lucas's model was qualitative.[6] Their purpose was to construct a model the simulation of which could replicate important moments of business fluctuations in the US economy from 1950 to 1975. The task was colossal; it involved the need to solve a lot of tricky mathematical, computational and empirical issues. Kydland and Prescott also decided to forego the traditional econometric testing of the models' parameters, resorting instead to the calibration method, a radical methodological break that triggered much controversy. The empirical validation of the model occurred through comparing the volatility, correlation and auto-correlation of output, investment, consumption, hours worked and productivity, as characteristics of the US

[6] In Michael Woodford's words, "The real business cycle literature offered a new methodology, both for theoretical analysis and for empirical testing. … It showed how such models [of the Lucas type] could be made *quantitative*, emphasizing the assignment of realistic numerical parameters values and the computation of numerical solutions to the equations of the model, rather than being content with merely qualitative conclusions derived from more general assumptions" (Woodford 1999, p. 25-26).-.

economy, with the equivalent moments generated by the model. Kydland and Prescott were able to achieve their goal, not fully, but still to a large extent.

While their paper was first met with skepticism, the view grew that it marked a methodological breakthrough. Several routes were proposed to overcome the objections that were leveled against the inaugural model: shocks other than technology were envisioned, and many other extensions were undertaken. From the mid'80s to the mid'90s, RBC modeling witnessed tremendous internal progress, to an extent that few would have predicted at the time Kydland and Prescott published their paper.

To put things in perspective, Lucas initiated the revolution, but Kydland and Prescott brought it to fruition. A scientific revolution succeeds when in a short span of time a large mass of the members of a scientific community shift towards new themes of research and new methods, with the younger generation jumping onto the new train and the older ones often being left on the side of the tracks. Kydland and Prescott's contribution in this respect was decisive. They offered the profession a workhorse model and a road map for further developments, thereby providing bread and butter for regiments of macroeconomists.

DSGE modeling

Successive modifications of the inaugural RBC model brought about the realization that a breach had imperceptibly occurred and that a new, distinct manner of pursuing the Lucasian program had emerged, DSGE modeling. On one hand, money, which had been disregarded in RBC modeling, returned to prominence with a great deal of attention devoted to the task of establishing a foundation for central banks' monetary policymaking. On the other hand, monopolistic competition and price-making behavior replaced perfect competition and price-taking behavior as basic modeling features, a step back towards the Marshallian approach. Moreover, the idea of sluggishness resurfaced in macroeconomics. As these themes had a Keynesian connotation, DSGE models also came to be known as 'new Keynesian' models. In many of them, the policy conclusions amounted to endorse some activation policy. The rise of DSGE modeling was also accompanied with the return to favor of econometric methods, in particular the VAR approach.

The differences between new classical, RBC and DSGE modeling are significant. Nonetheless, from a methodological standpoint, all three belong to the same line of research. In particular, the Keynesian inclination of DSGE models must not conceal the fact that they rest on the basic standards propounded by Lucas. First, to all intents and purposes, DSGE economists accept the hegemony of a single modeling strategy in macroeconomics and hence endorse Lucas's dismissal of the neoclassical synthesis viewpoint. Second, DSGE models are based on the equilibrium discipline and until recently, failed to integrate the possibility of

unemployment.[7] Third, DSGE economists plead allegiance to the internal consistency principle.

V. Keynesian versus Lucasian macroeconomics. Which is the winner?

The question that comes to mind is, which of the two approaches, Keynesian or Lucasian macroeconomics, is best? Richard Lipsey found a nice phrasing in favor of the former by writing that what happened was the "replacement of messy truth by precise error" (Lipsey 2000, p. 76). His point was that Keynesian theory has a strong truth-value; it constitutes a good representation of reality even if the argumentation can be loose. The opposite would be the case for new classical theory.

It would be convenient if things were as simple as Lipsey claims. But how can it be ascertained that truth lies on one side, and hence that the other side is mistaken? Among the many possible bones of contention one is clear: old Keynesians, like Lipsey, are sure that involuntary unemployment is an undeniable fact of life. A related statement pertains to the existence of excess supply of labor. One example among many is the following made by Solow: "It is plain as the nose on my face that the labor market and many markets for produced goods do not clear in any meaningful sense" (Solow 1978, p. 208). For my part, I believe that the matter cannot be settled that easily. Involuntary unemployment is an ambiguous notion. When stating that in any situation of unemployment there are voluntary and involuntary elements (Lucas [1997] 1981, p. 242), Lucas was not saying something new, he was treading the footsteps of no less an important philosopher than Aristotle (De Vroey 2004, p. 173). Ascertaining the existence of excess supply in any labor market is a difficult task. First, strictly speaking, it is impossible to delineate the boundaries of a single market, and second, such an existence is actually difficult to verify since the mere fact that agents are observed as non-participating in a market cannot be taken as a sufficient condition for excess supply.

If assessing the divide between Keynesians and Lucasians in terms of an opposition of truth versus error is difficult, what remains of Lipsey's proposition is the messy/precise opposition. By 'precise', he must have meant 'internally consistent' and this how I would also interpret it. As to 'messy', certainly, for Lipsey, messiness cannot be lethal enough to disqualify the entire Keynesian enterprise. But what if that was the case? Keynes is usually credited for having introduced notions such as full employment, involuntary unemployment, and excess labor supply — all drawn from direct, theoretically unscrutinized observation of reality — into the Marshallian language. These imports have been interpreted as 'making theory more realistic'. The crux is to see whether these notions resist closer scrutiny. Initially,

[7] Unemployment is absent from the index of Woodford's *Interest and Prices* (2011).

there was no objection to their introduction because in Marshallian theory the rules of theoretical admissibility are not as clearly spelled out as they are in Walrasian theory — Marshall disliked sharp definitions and distinctions on the grounds that they betrayed reality. However, when trying to reconstruct Marshall's theory of value rigorously, such rules come into the open. Take his study of the working of markets during a given market period, the corn model in Chapter 2 of Book V of the *Principles* (Marshall 1920, p. 332, seq.). It reveals that Marshall's reasoning is based on the premise that all agents have a perfect knowledge of market conditions. Hence they are able to surmise the equilibrium allocation. As a result, exchanges can only take place at the market-clearing price and quantity.[8] Extending this result to the labor market, — Marshall gave no indication that it should not — it turns out that in a Marshallian set up there is not only no room for involuntary unemployment, but also that there is none for unemployment *tout court*! Keynes was wrong to believe that his task was to add a theory of involuntary unemployment to an existing theory of frictional unemployment.[9]

In short, the introduction into Marshallian theory of notions such as full employment and involuntary unemployment, which are cornerstones of Keynesian thinking, was possible only because of a lack of awareness of this theory's rules. As subsequent history has revealed (Batyra and De Vroey 2012), in order to theoretically produce an unemployment result, it was necessary to leave the Marshallian supply and demand analysis behind and devise a new trade technology. This is exactly what the search models did.

Thus, with the benefit of hindsight it surfaces that Keynes's blind spot was to have wrongly believed that it was possible to produce an involuntary unemployment result within the Marshallian framework, while in fact this was not the case, except by resorting to tricks such as wage floor or some ersatz of the latter such as sluggishness. Keynesian macroeconomists followed suit. This, in my mind, explains why from the start the Keynesian program evolved on murky ground, making it easy prey for Lucas's critical investigation. Most of the criticisms he made were right on the mark. What was to the Keynesians a dramatic result, was not so for Lucas. Let's get rid of the ill-founded notion of unemployment, he claimed: as far as the theory of business fluctuations is concerned, it can operate without reference to unemployment; as far as the explanation of unemployment is concerned, partial equilibrium search models can do the job.

[8] Cf. De Vroey (2007).

[9] Economists such as Pigou and Hicks may well have used the term 'frictional unemployment', but it was merely a tag with no theory behind it.

To conclude, if the proposition that truth-value cannot be a decisive criterion for assessing which of the Keynesian and the Lucasian macroeconomics is superior, is accepted, the matter must be settled on the matter of logical consistency. Against this benchmark, Lucasian macroeconomics comes out as displaying higher consistency.

VI. A frustrating victory?

While I find that the ascent of Lucasian macroeconomics has marked a significant progress, I also believe that there is another side to the coin, namely that Lucas's choice involves drastic limitations. At least three of them are worth dwelling on. I expose them here in ascending order of importance.

The first limitation is that Lucasian macroeconomics provides a poor explanation of business fluctuations if by 'explanation' we mean exposing the causes underlying the rise of given historical events. On one hand, general explanations are by definition incomplete. Lucas's starting point was that all business cycles were alike. This, he argued, was what allowed the creation of a general theory of business fluctuations. Fine, but the counterpart is that such a theory by definition offers little light on the specificities of particular cycles, their causes, their pattern, etc. On the other hand, we tend to believe that the explanatory value of theories increases when they are confronted with reality. Limiting myself here to RBC modeling, such a belief ought to be qualified. It cannot be declared that a model, which succeeds in simulating the data using the calibration methodology, provides a compelling explanation of reality. Moreover, language abuse needs to be avoided. For example, few people would have qualms with the following proposition: the Kydland-Prescott model explains that for the period studied, US business fluctuations up to a certain percentage resulted from agents optimizing reactions to exogenous shocks. To me, such a proposition is an abuse of language. The correct phrasing should rather be: Kydland and Prescott have constructed a model economy in which agents are by definition in equilibrium; when simulating this fictive economy, they found that several of its moments replicate the moments calculated from US statistics over the period considered. In this phrasing, it is admitted that a replication has successfully taken place, but one stops short of claiming that reality has been explained.

The second limitation takes the form of a duty of abstention: the policy conclusion of so-constructed macroeconomic model cannot be peddled to policymakers. Rightly in my eyes, Lucas insists on the view that macroeconomics ought to be geared towards policy conclusions. “The central question that macroeconomists need to resolve: Which necessarily abstract models can help us to answer which practical questions of economic policy?” (Lucas Archives (various) Box 23, Tobin folder). This feature, he acknowledges, was a positive

aspect of Keynesian macroeconomics, which should not be lost. However, Lucas reckons that the policy conclusions of his model are embedded in its premises:

> By seeking an equilibrium account of business cycles, one accepts *in advance* rather severe limitations on the scope of governmental countercyclical policy which might be rationalized by the theory (Lucas [1977] 1981a, p. 234, Lucas's emphasis).

Thus, Lucas recognizes that although his model reaches the conclusion that government should not engage in countercyclical policy, he cannot recommend policymakers to engage in this policy.[10] Lucas's standpoint is definitely commendable. Unfortunately, it is hard to follow, and especially frustrating for macroeconomists holding strong ideological views.

In recent years macroeconomics have been lambasted for not having predicted the current crisis. This sort of criticism is somewhat misplaced because, by definition, crises cannot be predicted. But it comprises an important element of truth: Lucasian macroeconomics is ill equipped for tackling serious pathologies. This is the third limitation that I want to bring to light. Adopting the equilibrium discipline bears the burden that the explanatory capacity of macroeconomics is circumscribed to the explanation of mild or normal business fluctuations while more dramatic episodes such as the Great Depression (or for that matter, the 2008 recession) fall out of its scope. Again, Lucas ought to be credited for the fact that he was aware of this limitation.

> In Kydland and Prescott's original model, and in many (though not all) of its descendants, the equilibrium allocation coincides with the optimal allocation: fluctuations generated by the model represent an efficient response to unavoidable shocks to productivity. One may thus think of the model 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; my emphasis).

> The problem is that the new theories, the theories embedded in general equilibrium dynamics of the sort that we know how to use pretty well now — there's a residue of things they don't let us think about. They don't let us think about the U.S. experience in the 1930s or about financial crises and their real consequences in Asia and Latin America. They don't let us think, I don't think, very well about Japan in the 1990s. We may be disillusioned with the Keynesian apparatus for thinking about those things, but it doesn't mean that this replacement apparatus can do it either. It can't ((2004, p. 23).[11]

I doubt whether the majority of macroeconomists would endorse Lucas's subdued viewpoint. Be that as it may, admitting this limitation during 'the great moderation' had little consequence, in as far as it was believed that great depressions were a thing of the past.[12] In

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[10] "There is something wrong, and necessarily transient, with this easy translation of a technical contribution to economic theory into a platform for economic policy. … There can be no simple connection between what appears on the scratch pads of professional economists, however original, and important conclusions about the way our society ought to operate" (Lucas Archives, various, Box 23, Barro folder).

[11] In an interview with the Minneapolis FED journal, *The Region* (2010), Sargent makes the same point. "RBC models and their close cousins, the so-called New Keynesian models… these models are designed to describe aggregate economic fluctuations during normal times; these economists are aware of the limits of their works" (p. 4).

[12] Lucas fell prey to this opinion in his 2003 Presidential Address to the American Economic Association "Macroeconomics was born as a distinct field in the 1940's, as a part of the intellectual response to the Great Depression. The term then referred to the body of knowledge and expertise that we hoped would prevent the recurrence of that economic disaster. My thesis in this lecture is that macroeconomics in this original sense has

contrast, nowadays, with the recession on their hands, macroeconomists must recognize this deep shortcoming of their approach, the emperor's nakedness. No surprise, they are deeply frustrated and on the defensive, their haughtiness lost.

If it was only that macroeconomics cannot take everything in its stride, only frustration would be involved. But much more is at stake. There is also a substantive problem, which Obstfeld and Rogoff nicely captured by writing:

> A theory of business cycles that has nothing to say about the Great Depression is like a theory of earthquakes that explains only small tremors (Obstfeld and Rogoff 1996, p. 627).

Obstfeld and Rogoff have a strong point. Lucas's standpoint amounts to assigning the study of moderate fluctuations to RBC macroeconomics and that of great recessions to economic historians. Lucas fiercely attacked Keynesians, who argued when defending the neoclassical synthesis that different methodologies should coexist within macroeconomics according to the problems at hand. Ironically enough, here we observe him doing the same.[13]

Concluding remarks

Let me conclude by making four remarks. First, it may be wondered whether the present state of macroeconomics augurs a new scientific revolution. Of course, nobody can answer. Some take advantage of this disarray to plead for a return to Keynesian macroeconomics, but the latter is no better equipped for dealing with great recessions. The path taken by the macroeconomic profession after the recession has been to explore the workings of the financial sector. Will such a limited backtracking process suffice to get the field off the hook? If the aim is to explaining crises endogenously, the answer is probably 'No', because at a certain point such an endeavor might have to go beyond equilibrium theory — a daunting task that would require new tools and concepts. Therefore it might take a long time for the dichotomy between macroeconomics and economic history to be solved.

Turning to my second remark, I have argued that Lucas induced a strong shift in macroeconomics towards the Walrasian approach (with a caveat *à propos* DSGE modeling). Yet he did not want to forego the applied character of the discipline. The result is that macroeconomics has become a somewhat odd object. It definitely gives off an air of an applied discipline through its concern with empirical confrontations and its inclination for policymaking. Nonetheless, I, for one, rather regard it as a 'pure' discipline in Walras's sense, i.e. concerned with matters of principles, and giving priority to internal over external consistency. This is so, for better and for worse. For the better, if one believes that conceptual

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succeeded: Its central problem of depression prevention has been solved, for all practical purposes, and has in fact been solved for many decades" (Lucas 2003, p.1).

[13] Prescott was alert to the problem. After having first declared that the Great Depression was beyond the reach of equilibrium business cycle theory, he changed his mind and tried to extend the RBC framework in order to make it capable of coming to grips with great recessions. Cf. De Vroey and Pensieroso (2006).

work, formalization, and the internal consistency principle pay off in the long run. The strides made between Lucas's initial paper and Kydland and Prescott's model to present-day DSGE models are a fine testimony of the progress that ensued from adopting the Lucasian program. The flip side consists of the different limitations discussed above. In view of these, the conclusion to be drawn is that in spite of the progress made, civil society should not have too high expectations about what present-day macroeconomic theory can deliver as far as policymaking is concerned. Likewise, macroeconomists should avoid pretending that they have an edge on policy matters and endorsing the role of expert that they are often unduly invited to play.

My third remark, related to the previous one, is that the path taken by macroeconomics has widened the gap between the production of economic science and the art of economic policy.[14] The latter has become a specialization on its own, where the economists are the middlemen between the academic and policy worlds. A standard Lucasian macroeconomist produces models and has little to say about real-world policy issues. For him or her, being mute on these issues is more honest than expressing his or her prejudices. In contrast, those economists who have significant things to say on policy (although knowing the theory and drawing from it, while often being lukewarm about its achievements) — say a Charles Goodhart or a Paul De Grauwe — are not producers of theory in the strict sense. The posture that Keynes was able to adopt of practicing both economic theory and policy advice is no longer viable today.

My last remark relates to the two epigrams with which I have opened this paper. Is Krugman right in declaring that macroeconomics has been spectacularly useless? It all depends on what it is put to use for. Krugman is right when it comes to explaining recessions and great crises. He is also right if one takes the 'old' Keynesian standpoint (what I believe one should not do). In contrast, if one accepts (as I do) that there is a role to be played by pure theory, it makes sense to adhere to the subtext of Chuang Tzu's epigram: apparently useless things can actually be useful. Finally, is modern macroeconomics positively harmful, as claimed by Krugman? My analysis has suggested that macroeconomists should take stock of their Walrasian methodology and as a result, follow suit with earlier neo-Walrasian economists by adopting an abstentionist attitude towards policy. I admit this is probably a tall order, at least from those who hold a strong sense of policy. However, once entering the ideological fray, Krugman's 'harmfulness' characterization becomes trivial since it is, almost by definition, an attribute of one's opposite ideological camp.

[14] This is a point that Gregory Mankiw and David Colander have made repeatedly. Cf. Mankiw (2006) and Colander (2010, p. 41).

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