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## IS NEO-WALRASIAN MACROECONOMICS A DEAD END?

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# IS NEO-WALRASIAN MACROECONOMICS A DEAD END?

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## Abstract

After the ‘new Great Crisis’ exploded in 2008 it is widely recognized that mainstream macroeconomics - the last result of Lucas’s anti-Keynesian revolution of the 1980s which tried to give macroeconomics sound neo-Walrasian microeconomic bases - has failed to anticipate and then appraise the crisis. Has this crisis revealed a failure of this macroeconomics as a scientific theory? Mainstream macroeconomists defend their models on the basis of their alleged superiority in terms of clarity and coherence. The thesis of this paper is that this claim about superiority is false. The paper argues that the reasons for the failure of mainstream macroeconomics – in particular its poor predictive performance and interpretative weakness - reside in the implications of the neo-Walrasian legacy and the problems connected with the implementation of that programme.

**Keywords:** Neo-Walrasian Macroeconomics, Lucas, DSGE models, theory-driven econometrics

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*“What is the relevance of macroeconomics today?”  
(Robert M. Solow, 2012)*

*“Macroeconomics is not a science, it's a patchwork of theorems and bad data. There is little serious work on the subject, which seems dominated by beliefs”  
(J. Heckman, 2010)*

*“A scientific theory cannot require the facts to conform to its own assumptions”  
(J. M. Keynes, 1936)*

## **1. Introduction**

What has been recently referred to as the ‘new neoclassical synthesis’, or what may more appropriately be called neo-Walrasian macroeconomics, represents the contemporary mainstream form of macroeconomics, i.e. the macroeconomics of the Dynamic Stochastic General Equilibrium (DSGE) models. These latter are the ultimate result of the profound transformation that has taken place within mainstream economic thought in the past forty years since Lucas’ anti-Keynesian revolution. According to many economists, they represent the general consensus, which arose at the beginning of the new millenium, that the fundamental mechanism of macroeconomics has been understood by combining elements of both the new classical and new Keynesian schools (Chari and Kehoe 2006, Blanchard 2009, Woodford 2009). At the same time the empirical implementation of the new theoretical models has become a tool of analysis used by central banks. Hence Blanchard (2009, 209) could say that “the state of macro is good”.<sup>1</sup> This theoretical trend was opposed by discordant voices, but objections on microfoundational legitimacy of the model and its empirical justification went largely unheard until 2008. In 2010, before the US House of Representatives, Varadarajan V. Chari positively referred to what he considered to be a still useful aphorism in macroeconomics:

“If you have an interesting and coherent story to tell, you can tell it in a DSGE model. If you cannot, your story is incoherent” (Chari 2010, p. 2).

This statement still prevails today in the community of economists, even though, after the ‘new Great Crisis’, or the ‘great recession’ as it is often called, exploded in 2008, it is widely recognised that mainstream macroeconomists have failed to anticipate and then appraise the crisis. In fact, they did not predict the financial crisis, and they even failed

to emphasise the risks to which the economic system had been exposed in the period before the crisis. Indeed, the crisis caught them by surprise, as a phenomenon which could neither be predicted, nor understood. Even less did they foresee its transformation into a deep recession or a depression, or consider the ‘return of the great depression’ to be possible.

The following question arises spontaneously: if prediction has to be considered the testbed of a scientific theory, then has this crisis revealed a failure of the contemporary macroeconomic mainstream as a scientific theory - a claim made recently by many eminent economists (see for example Kirman 2010, Caballero 2010, Heckman 2010, Stiglitz 2011, Solow 2012), not to say post-Keynesian economists? Mainstream macroeconomists recognize that theoretical changes are necessary, but they are confident that the model does not need to be discarded. For example Chari (2010), after recognizing that “our models failed to see the recent crisis coming” (p. 1), rhetorically asks: “was this failure because we did not have the right tools in our toolbox?” (p. 7) and answers in the negative. According to Chari, the problem is rather that of improving the current mainstream macroeconomic theory, not of discarding it. He expresses this opinion in a peculiar way:

“I have argued we need more of it. After all, when the AIDS crisis hit, we did not turn over medical research to acupuncturists ... Rather than pursuing elusive chimera dreamt up in remote corners of the profession, the best way of using the power in the modelling style of modern macroeconomics is to devote more resources to it” (Chari, 9-10).

The issue, however, is considered to be less simple and obvious than Chari puts it, also in the mainstream territory. In fact, if we look at what Robert Lucas himself, the most eminent precursor of modern macroeconomics, wrote just few years before the crisis, we have a possible basis for a different answer to the failure question. As he clearly acknowledged,

“there’s a residue [*sic*] of things they [the new theories] don’t let us think about. They don’t let us think about the U.S. experience in the 1930s or about financial crises [of the 1980s] 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” (Lucas 2004, 23).

*A fortiori*, this judgement applies to the new great crisis. In other words, systemic crises are other-worldly events absent from these models.

However, despite these unpleasant results, mainstream economists continue to defend their models on the basis of their alleged superiority in terms of clarity and coherence compared with the alternative models (belonging to the heterodox ‘periphery’ of macroeconomics, to use an expression introduced by Caballero 2010).

The thesis of this paper is that the last claim about superiority is false: on the contrary, it is precisely the theoretical structure of this contemporary mainstream macroeconomics, grounded on the neo-Walrasian general economic equilibrium theory, that is the fundamental cause of its poor predictive performance and interpretative weakness. Starting from the theoretical difficulties of Keynesian economics, Lucas’s revolution has sought to give macroeconomics sound neo-Walrasian microeconomic bases, but this research programme has exhibited difficulties, weaknesses, inconsistencies and failures at the foundational-theoretical level and major problems in the relationship between a highly abstract theoretical model and the real facts. Scholars have attempted to ‘square the circle’, through an endeavour to resolve the inconsistency between the theoretical and the statistical models by fitting the statistical model to the preferred theoretical framework. But here arises the fundamental point: what are the true consequences of such tightrope walking in terms of policy analysis? Whenever the reference models tell imaginative stories, how can the policies based on such models fit reality? To put it in Keynes’ words: what are the policy implications of “adopting a hypothetical world remote from experience as though it were the world of experience and then living in it”? (Keynes 1973 [1936], p. 192).

The paper is essentially an attempt to coordinate the manifold criticisms of mainstream macroeconomics that have emerged in recent years in order to demonstrate our thesis. It focuses on its theoretical foundations and the related macroeconometric modelling. Section 2 considers the state of mainstream macroeconomics and macroeconometrics at the onset of the new great crisis. Section 3 is devoted to the criticism of mainstream macroeconomics. The final section concludes.

## **2 The state of neo-Walrasian macroeconomics**

## 2.1 Premise. From the criticism of Keynesian macroeconomics to the DSGE models

At the end of the Second World War, (macro)economics was largely influenced by Keynes' *General Theory*. At that time it was considered a rich and complex work, but also, according to the majority of economists, theoretically imperfect and too iconoclastic. In fact, the emerging Keynesian economics was an attempted compromise: the so-called 'neoclassical synthesis' that was 'macroeconomics' in the period 1950-1970. Between the end of the 1960s and the beginning of the 1970s, this heterogeneous theoretical Keynesian framework was first challenged by Milton Friedman for its economic policy implications more than its conceptual apparatus. Then, in the second part of that decade, a real anti-Keynesian revolution came about, with Robert Lucas as its main proponent and theoretician. Based on an extensive criticism of the weaknesses of the theoretical apparatus of the neoclassical synthesis and of the Keynesian macroeconomic models, the objective of that revolution was to give macroeconomics a sound neo-Walrasian microeconomic basis, considering this the correct way to give macroeconomics the reputation of being an incontestable science. As a consequence, Keynesian economics was considered dead, or "a dead end", and Keynes's *General Theory* was dismissed as an example of "bad social science" (Lucas 1976 and Sargent 1977). Lucas's new classical macroeconomics inaugurated a new paradigm which underwent an evolution that led to Kydland and Prescott's and Long and Plosser's (1982 and 1983) Real Business Cycle modelling, and finally to the emergence of DSGE modelling, giving rise to the 'new neoclassical synthesis' (a term coined by Goodfriend and King 1997) between neo-classicals and neo-Keynesians, also called 'modern macroeconomics' (Chari and Kehoe, 2006), at the end of the 1990s. Stiglitz (2011) refers to this model, and the standard prescriptions that are associated with it, as the "Standard Model" or the "Conventional Wisdom". Referring to its foundations, we prefer to call it neo-Walrasian Macroeconomics.

The canonical DSGE model is essentially a Real Business Cycle Dynamic Stochastic General Equilibrium model rooted in the Arrow-Debreu tradition with some neo-Keynesian ingredients: monopolistic competition and nominal imperfections. Hence, the core of DSGE models is the standard neoclassical growth model developed into a stochastic form: the agents are representative households which maximize their utility under an intertemporal budget constraint, and firms which maximize profits over time.

The system is the sum of these rational agents. The economy is affected by different types of exogenous shocks: supply-side, demand-side and monetary. The framework is designed to capture a *plausible* business cycle dynamic of an economy (i.e. following an exogenous disturbance, the economy would return to the deterministic steady state rather rapidly, avoiding cumulative causation processes): it can be understood as an efficient response to those shocks.

This approach adopts the hypothesis of perfect rational behaviour by economic agents. The rational agents are isolated individuals that interact only through the price system. They manage their affairs as side-aspect of their utility maximization problem. The behavioural patterns that contradict the assumption of rational behaviour – discovered in empirical research and studied by behavioural economics – are considered anomalies or exceptions which are nullified in the aggregate. The crucial assumption concerning rationality is that of rational expectations. Rational expectations constitute a building block of the efficient market hypothesis, this latter being an implication of them. It assumes that financial markets are able to establish prices which correctly reflect the available information.

The DSGE models, in particular in their early versions, make the assumption of the representative agent, i.e. the model assumes that the choices of all the diverse agents can be considered as the choices of one representative maximizing individual whose choices coincide with the aggregate choices of the heterogeneous individuals. This reduction has been usually justified as an analytical convenience. It implies that the behaviour of the aggregate corresponds to the behaviour of its components; consequently, the structure of their interactions is neglected – only the characteristics of the individuals are considered, thus making the hypothesis of a representative agent plausible. The representative agent model takes economy-wide aggregates as if they were the equivalent of the similarly named variables associated with individual agents: “the representative agent is just a microeconomic agent writ large” (Hoover 2009, 389). It maximizes utility subject to a budget constraint given by the national-income identity. It simultaneously maximizes profits subject to an aggregate production function. As a consequence, the model is in its essence a simple analytically tractable macromodel (conceptually simple but mathematically increasingly sophisticated) considered by its founders to be a first approximation to be developed, thus overcoming the analytical and theoretical difficulties of moving beyond the basic model. In this first approximation there is no room for



heterogeneity. Recently, however, the exclusive use of representative agent models has been abandoned: heterogeneity in behavior and decisions is now considered, assuming the existence of different agents with a fixed distribution of characteristics who make decisions independently of each other. In general, advocates of the mainstream macroeconomics have acknowledged the limitations of the models used, but they argue that anything left out will eventually be incorporated.

## *2.2 At the origin of the DSGE models, 1. The criticism of the Cowles Commission approach: the statistical and structural identification problems and the Lucas's razor*

The above theoretical framework found its statistical counterpart in DSGE macroeconometric models. Its advanced evolution, starting from the criticism of the previous approaches, is a sort of 'consensus approach' to macroeconometric model evaluation (Favero, 2007). In fact, previous approaches were harshly criticized, mostly for their evident ineffectiveness in policy evaluation analysis. Such models derived from the Cowles Commission approach, which drew a clear dichotomy between theory and econometrics. In fact, theory identified the list of the relevant variables to be included in the analysis by some static long-run relationships and some ancillary assumptions. Hence, (Keynesian, Neo-classical, New Growth) theory focused on conditional statements. Policy variables were exogenous, i.e. they were either unmodelled or independent from the present and past information sets. Then econometrics entered the scene, providing regression methods to estimate the conditional means, quantifying and testing the theoretical statements about the conditional moments. This was the time of estimators, rather than that of model evaluation; but the substantial lack of consensus on inference methods prevented the profession from establishing whether the evidence actually rejected the theory (Pesaran and Smith, 1992). And theory proved to be almost unfalsifiable, since it was unclear whether rejection involved either the theoretical assumptions or the ancillary statistical assumptions (e.g. linearity, homoskedasticity, absence of autocorrelation, normality of residuals, etc.).

Spanos (1990) identified the roots of the Cowles Commission 'fallacy' in the statistical identification problem, i.e. the scant attention paid to the statistical model implied by the estimated structural model<sup>2</sup>. Identification issues were generally addressed by attributing an exogenous origin to policy variables, and large-scale models were essentially devoted to

quantitative evaluation of the effects exerted by changes in the economic policy instruments have on outcome variables. But the validity of diagnostics is clearly bounded to the adherence of the statistical model to observational data<sup>3</sup>, which in turn determines the model effectiveness in forecasting and policy analysis.

In the meantime, Lucas' critique pointed out the scant adherence of the Cowles Commission models to theory.<sup>4</sup> The super-exogeneity of policy variables was discussed, it was underlined that no expectations were explicitly taken into account and parameters actually described a mixture of the 'deep' parameters (e.g. preferences and technology), engendering ineffectiveness in policy evaluation. This is the 'structural<sup>5</sup> identification problem' (Spanos, 1990). As a result, the Cowles Commission models were considered as *statistically* inadequate, *theoretically* inconsistent, and *practically* irrelevant (Pesaran and Smith, 1985).

### 2.3 *At the origin of DSGE models, 2. Lucas's neo-Walrasian research programme*

Robert Lucas, as is generally recognized, had the pre-eminent role in the foundation of the new classical macroeconomic theory in the 1970s, particularly "at the methodological level" (Vercelli 1991, 129-130, see also Hartley 1997; for a reconstruction of Lucas' thought on macroeconomics see Laidler 1986, Chary 1998, De Vroey 2010a and b, De Vroey and Malgrange 2011).

His famous 1976 paper, "Econometric Policy Evaluation: A Critique", provided the "fundamental rationale for microfoundations after 1970" (Hoover 2013). In the paper Lucas criticized the Keynesian macroeconomic models of the time - i.e. the neoclassical synthesis. These models, Lucas maintained, were a failure as far as the assessment of alternative policies was concerned. The substance of Lucas' critique is that expectations depend upon the policy regime in place; hence they change with the regime. Because they were based on reduced forms, these models neglected the fact that agents change their decisions when faced with a change in the institutional regime; a criticism that ensued from the argument that Lucas developed in his article on expectations and the neutrality of money (Lucas 1972). Only structural models, i.e. those derived from the fundamentals of the economy - agents' preferences and technological constraints - Lucas maintained, are able to provide a robust grounding for the evaluation of alternative policies. In other words, according to Lucas, the flaw of Keynesian models was their lack of

microfoundations. The new macroeconomics had to be based on explicit and fully formulated microfoundations, and it should try to achieve the standard of rigour imposed by the Neo-Walrasian General Economic Equilibrium Theory.

Lucas's aim was to extend the general equilibrium approach to macroeconomics in order to elaborate a macroeconomics of equilibrium, or a Neo-Walrasian macroeconomic equilibrium theory. Its predecessors – Lucas stated – were the pre-Keynesian business cycle theories. In fact, the object *par excellence* of this new macroeconomics had to be, according to Lucas, business fluctuations. Lucas's "An Equilibrium Model of Business Cycle" (1975), an extension of Lucas (1972), is the classical study in the new field: an equilibrium analysis of the business cycle. The model claimed that real output fluctuations are caused by unanticipated monetary-fiscal shocks, the condition for this to happen being that agents' information is imperfect (in the sense that each agent is assumed to have limited information). This model was soon criticized and abandoned by Lucas himself – he recognized that Kyndland and Prescott's (1982) real-business-cycle model was a "far simpler, and more comprehensible structure [resulting from removal of all monetary aspects] that fit postwar US time series data just as well as the original version" (Lucas 2001, p. 28). However, the main point to emphasize here is that with his paper Lucas maintained that business fluctuations were (again) the defining object of macroeconomics.

According to Lucas the 'sophisticated' pre-Keynesian literature on business cycles was the necessary reference and a line of inquiry left unexplored: it emphasized

"the recurrent character of business cycles, the necessity of viewing these recurrences as mistakes and attempts to rationalize these mistakes as intelligent responses to movements in nominal 'signals' of movements in the underlying 'real' events we care about and want to react to" (Lucas 1981, p. 9).

Lucas's main (really the only) reference is to Hayek's business-cycle theory. Lucas referred to Hayek's famous statement about "the crucial problem" of the theory:

"The incorporation of cyclical phenomena into the system of economic equilibrium theory, with which they are in apparent contradiction, remains the *crucial problem of Trade Cycle Theory*" (Hayek 1933, p. 33 note, our italics).

Starting from this Hayek's "crucial problem", progress in macroeconomics was interpreted by Lucas essentially as a matter of discovering and applying new tools for treating old issues. Following Hayek, to deal with his problem it was necessary to overcome the "apparent contradiction" between the statics of the traditional Walrasian general economic equilibrium and the dynamics of the economic cycle.<sup>6</sup> According to Lucas the contradiction could be overcome by applying a different concept of equilibrium – a rest point in the space of decision rules, now considered in mainstream macroeconomics the central theoretical breakthrough of the last 50 years.<sup>7</sup> Lucas (1972) developed an equilibrium model of the business cycle which utilizes "the contingent-claim general equilibrium formalism" – originally proposed by Arrow and Debreu as an interpretation of a competitive equilibrium that takes all information to be simultaneously and freely available to all traders. Lucas interpreted the contingent claim equilibrium "as being determined via a sequence of spot markets, in which current prices are set given certain expectations about future prices". To do so, he needed "a principle to reconcile the price distributions implied by the market equilibrium with the distributions used by agents to form their own views of the future" (p. 707). In other words, in order to adhere to the tenets of equilibrium theory, a particular hypothesis or principle was necessary. This principle was found by Lucas in John Muth's hypothesis of rational expectations, according to which these distributions could not differ in a systematic way.<sup>8</sup> It is important to note that the hypothesis of rational expectations is not the result of empirical observation of the formation of human actors' expectations: it is a way to close a model. Hence the conception of equilibrium becomes that of a rest point in the space of decision rules – contrary to the conventional conception of equilibrium as a rest point in terms of quantities and prices. This makes it possible to use the maximization postulate to analyze a world which is continually buffeted by shocks.

Lucas (1977) equilibrium model presented a 'representative agent' (for "helpfulness reasons", Lucas writes) with rational expectations who decides how much to work and how much to produce, given his tastes and the available technology, and responds to movements in relative prices, which transmit information. Monetary shocks cause movements in the price level and the agent's reaction causes the business cycles. In this context, a new picture of the business cycle emerged. Previously, the business cycle had been viewed as the disequilibrium phenomenon *par excellence*, the manifestation of a

market failure. In the new Lucasian approach, the business cycle expressed the optimizing reactions of agents to outside shocks affecting the economy.<sup>9</sup> It was an equilibrium model precisely because it describes a state in which agents act in their own best interest and markets clear. And it should show, as Sargent emphasized, that “Keynes and his followers were wrong to give up on the possibility that an equilibrium theory could account for the business cycle” Sargent 1977, 14). Moreover, with Lucas the empirical idea that business cycles are all alike returned. Lucas wrote that “[an] influential feature of post-World War II time series has been the return to a pattern of recurrent, roughly similar cycles in Mitchell sense” (p. 706). This pattern had been abandoned when “the magnitude of the Great Depression dealt a serious blow to the idea of the business cycle as a repeated occurrence of the same event”. But, Lucas thought, “the postwar experience has to some degree restored respectability to this idea”.

Assuming the existence of rational agents able to exploit all the relevant information available and to form correct expectations on the average, and considering deviations from this pattern to be random, it follows, on the one hand, that the occurrence of systemic crisis is excluded – the concept itself of crisis is absent in this framework - and on the other hand, that the fact of ‘the crisis’ is explained as resulting from a *random error*. In other words, the crisis is explained as an exogenous phenomenon and the economy is considered to be inherently stable: only temporarily can it go off track when perturbed by external shocks (another idea existing in the pre-Keynesian literature, see for example Pigou’s theory of fluctuations).

The fundamental theoretical component of Lucas’ approach is its conception of macroeconomics as a part of general economic equilibrium analysis of neo-Walrasian type. In particular, Lucas stressed the importance of using appropriate language:

“Macroeconomists today are expected to be able to discuss their ideas in the language of Arrow, Debreu and McKenzie. This is progress” (Lucas 2007).

Hence, the progress of macroeconomics lies in its being formulated as a mathematical model of Arrow-Debreu type. In this perspective, we can fully appreciate the real role of the adoption of the representative agent fiction (Lucas’ “helpfulness reasons” for its adoption): it makes it possible to build the model mathematically without foregoing the rigour of microfoundations (see Hartley 1997, in particular ch. 2). As a consequence, the

model of the representative optimizing agent is unavoidably highly abstract. According to Lucas (who on this point takes Friedman's methodological perspective), this is a positive fact. "Progress in economic thinking means getting better and better abstract, analogue economic models, not better verbal observations about the world" (276). A theory is concerned with imaginary constructions; it is avowedly non-realistic:

"A theory is not a collection of assertions about the behavior of the actual economy but rather an explicit set of instructions for building a parallel or analogue system – a mechanical, imitation economy. A good model, from this point of view, will not be exactly more 'real' than a poor one, but will provide better imitations" (1980, 276 (697)).

It should be emphasized that this does not mean that the theory lives exclusively in a hypothetical world like Debreu's world of general economic equilibrium: Lucas stressed that macroeconomic models must reach practical conclusions. He writes:

"the central question that macroeconomists need to resolve [is]: which necessarily abstract models can help us to answer which practical questions of economic policy?" (cited in De Vroey 2010b, p. 3).

Hence, theory ought to be tested against facts. Like Friedman, Lucas thought that the appropriate criterion for establishing the fruitfulness of a theory is the degree of empirical corroboration received by its predictions. In Lucas's words, its aim is to construct "a fully articulate artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies" (Lucas 1977, 219). The empirical testing of the theory, Lucas writes, "is critical precisely because we know that the axioms are abstractions, necessarily 'false', so we need to know whether and *under what range of circumstances these abstractions are adequate*" (Lucas 1986, 408, our italics). Lucas' program was implemented by Kydland and Prescott (1982): assigning realistic numerical parameter values to the model and computing numerical solutions to its equations, they tried to show how Lucas-type models could be made "quantitative" (Woodford 1999, 25).

The challenge of Lucas's approach is therefore to build a highly abstract (microeconomically founded) but relevant (empirically tested) model in order to reach policy conclusions: consequently, it must be judged in relation to how far it achieves this

aim.

## 2.4 DSGE models. *The (consensus approach to) macroeconometric modeling*

### 2.4.1 Premise

From the two distinct identification problems described in subsection 2.2 (statistical and structural), two antithetical approaches derived: an a-theoretical and an a-statistical one. They made use of tools that finally characterized DSGE macroeconometrics in a completely different perspective.

On the one hand, Sims (1980) reacted to the statistical identification problem by adopting an a-theoretical approach that used data-driven VARs to test economically relevant hypotheses with no theoretical identification restrictions, i.e. no explicit microfoundations.<sup>10</sup> He concluded that no variable can be considered exogenous whenever forward-looking agents solve intertemporal optimization models: such silly restrictions may induce unsound causal inference<sup>11</sup> about the real effectiveness of policy instruments.

A similar but slightly different route was followed by some econometricians at the London School of Economics (LSE), who focused on statistical diagnosis of the failure of the Cowles Commission models. Based on the belief that economic theory is mainly informative about long-run relationships (Pesaran and Shin, 2002), the LSE group focused on dynamic specification and the long-run properties of the models used by policy-makers (Juselius, 1999). The introduction of some simple long-run equilibrium relations in error correction models gave rise to the structural cointegrated VAR approach. This tested specification and identification restrictions based on the statistical properties of the general dynamic reduced form models.<sup>12</sup> The policy exogeneity assumption was strongly criticized, on the ground that the statistical models (implied by the structural econometric models) clearly omitted both relevant variables and relevant dynamics (Hendry, 1995).

The reduced form became prominent in both these a-theoretical approaches,<sup>13</sup> reversing the Cowles Commission approach based on the structural form. Lucas's critique became a testable concept (Hendry, 1988; Engle and Hendry, 1993): once the baseline model was validated, its dimensionality was reduced by omitting the equations for which the exogeneity null hypothesis could not be rejected, and the statistical model was finally

estimated. Except for the lag length, no restrictions were imposed on the short-run dynamics, which were used to both forecast and evaluate the effects of policies, via the generalized impulse-response functions.

On the other hand, many theorists preferred an a-statistical approach, either relying on 'stylized facts' (Summers 1991) or calibrating their nonlinear stochastic optimization models (RBC and DSGE) by matching the unconditional sample moments with the corresponding model simulated moments (Kydland and Prescott, 1982; King, Plosser and Rebelo, 1988).

#### 2.4.2 The theoretical approach to the Cowles Commission fallacy and the consensus

These theorists reacted to the structural identification problem by identifying the structural parameters of interest via microeconomic foundations. Hence, the basic notion was a theoretically-founded identification strategy, where the effects of economic policies were estimated through theory-based quantitative models, clearly parametrizing tastes and technology. This approach entailed a backward step towards model evaluation, rather than diagnostic testing and model selection. However, it became clear that a solved DSGE is a structural VAR (Christiano *et al.*, 1999), and DSGE began to represent a sort of consensus approach to macroeconomic modelling.

The DSGE methodology was based on standard numerical techniques, but it proved very difficult to evaluate parameters by the reduced-form VAR estimation, since VAR parameters are actually complicated convolutions of the structural parameters. The early DSGE models were, as already said, RBC models, with no frictions and (consequently) no role for economic policy. They were based on a limited number of parameters, whose value could be estimated by linearization around the equilibrium. However, since most models based on dynamic stochastic optimization did not have closed-form solutions<sup>14</sup>, brute force estimation by maximum-likelihood simulation was computationally unattractive. Then, calibration techniques spread. They implied some heavily unrealistic assumptions, i.e. simple functional forms describing tastes and technology, representative agents with homogeneous information sets, and no institutional constraints<sup>15</sup>. However, calibration<sup>16</sup> was not aimed at congruently representing real data, but rather at finding the values of the structural parameters jointly compatible with both theory and real data in a particular well-specified dimension (Cooley, 1997). Since there were fewer invertible relations than unknown parameters, the parameters to be calibrated were chosen in order



to reproduce the specific stylized fact. Their one-to-one relationship with the empirical features were investigated, and then inverted. The strongest supporters of DSGE models asserted that:

The combination of rich structural models, novel solution algorithms, and powerful simulation techniques has allowed researchers to transform the quantitative implementation of equilibrium models from a disparate collection of *ad hoc* procedures to a systematic discipline where progress is fast and prospects entrancing (Fernandez-Villaverde, 2010, 4).

DSGE was advocated as a flexible and powerful approach yielding high performance forecasts and tools essential for applied policy analysis (Adolfson *et al.*, 2007; Smets and Wouters, 2007; Edge *et al.*, 2010). It was quickly accepted by practitioners, and many central banks adopted their own DSGE model. However, due to its various inconsistencies and the weak link between theory and observational data, the widespread adoption of DSGE modelling in policy analysis had serious consequences, especially an incapability to anticipate heavy imminent structural changes in actual economies and to adopt proper policy instruments to soften the impacts of the crisis.

### 3. A criticism of the neo-Walrasian macroeconomics

#### 3.1 Premise

Manifold criticisms at theoretical and empirical verification levels have been brought against the neo-Walrasian programme in macroeconomics and its realizations. However, the mainstream macroeconomists have been largely indifferent to these criticisms and impenetrable to negative evidence.

At the theoretical level, the soundness of the microfoundations has been challenged. On discussing the statement that “macroeconomics is now firmly grounded in the principles of economic theory”, affirmed by the mainstream macroeconomists (Chari and Kehoe 2006), Solow (2008) declared that this “sentence is simply false” (p. 243), because the assumption that individual agents optimize as best as they can does not imply that the

overall economy acts as a single optimizer under the simplest possible constraints. In other words, Solow charged mainstream macroeconomics with the fallacy of composition, i.e. the attribution of properties to a different level from which the property is observed. Therefore, Solow asked, in what sense is the DSGE model firmly grounded in the principles of economic theory? In fact, when economic decision-makers interact with each other, the outcome for the overall economy may be different from what was intended by the individual decision-makers: a classic example is the paradox of thrift as formulated by Mandeville and then Keynes (see also Caballero 1992 and Kirman 1992). In other words, Solow emphasized that the direct application of microeconomic reasoning to the aggregate system may be misleading. However, Solow's criticism (and of many others') seems not to have affected mainstream macroeconomists (see Chari and Kehoe 2008).

At the empirical verification level, another classic strand of criticism has been founded on the fact that empirical research often does not confirm the theories, as in the case of the two pillars of these models: the Rational Expectations and the Efficient Market hypotheses. The behavioural literature shows that individuals act in a way that bears no resemblance to the rational expectations paradigm: they display various forms of bounded rationality and are strongly influenced by emotions. And the Efficient Market Hypothesis has been proved false many times: individuals do not observe information independently and then act on it; rather, they are constantly influenced by others and are prone to herd behaviour.

Despite these criticisms, the faith of mainstream economists in the theoretical strength of these models has not been undermined by the evidence or by references to actual behaviour. And the prevailing attitude towards the recent crisis confirms this substantial indifference to any contrary evidence.

Two questions arise. On the one hand, there is the question of the attitude in the face of criticism and the connected alleged superiority of mainstream macroeconomics: how can this be explained? On the other hand, there is the question of the soundness of the microfoundations and the relationship between the model and the real world, or, one might say, the impenetrability of the model to evidence.

To answer these questions we will use many of the current criticisms within a critical framework able to show that the theoretical beliefs and choices of mainstream macroeconomics are the consequences of the Lucasian project to make macroeconomics not a specific field but one embedded in neo-Walrasian microeconomics, and the

implication of this.

### 3.2 *The neo-Walrasian legacy: theoretical attitude and theoretical status*

#### 3.2.1 Theoretical attitude: The prevalence of rigour over realism

The alleged superiority and the substantial impenetrability to evidence shown by mainstream economic theory is essentially rooted in its theoretical belief, its real founding principle, that rigour should prevail over realism. This is a crucial methodological legacy of the Neo-Walrasian theory of general economic equilibrium (see Marchionatti and Mornati 2015): from von Neumann (1937) – and then, above all, Gerard Debreu (1950), who represented the radical extension of mathematical formalism in economics<sup>7</sup> – the choice of extremely artificial assumptions was considered a necessary condition for obtaining mathematical solutions of theoretical economic problems characterized by elegance, logical completeness, concision and rigour. Models were conceived as formal structures whose legitimacy and cogency depend on their internal consistency, not on their verification. This theoretical attitude, appropriately called the “Walrasian detour” of the post-Marshallian era by Samuel Bowles and Herbert Gintis (2000), originated in the reaction to Marshallian economics in the 1920s and 1930s (see Marchionatti 2003 and 2004), and greatly influenced post-war mainstream economics giving origin to what was called the ‘neo-Walrasian research programme’ by Weintraub (1985; see also Backhouse 1995). But its strong influence in macroeconomics dates from the 1970s.

The main and fundamental implication of this foundational approach is the pre-eminence of the mathematical proof: the mathematical proof within the model is considered its main validation, more than any empirical evidence. In other words, the evidence is dominated by the theory. This theoretical approach has modelled itself more on mathematics than any of the natural sciences. This sanctions a substantial divorce between theory and empirical observation: the analysis of empirical data becomes a separate, subordinate, subject. However, differently from the neo-Walrasian General Economic Equilibrium field, where the economist occupies the territory of pure economics and halts at the threshold of the real world, in macroeconomics the relationship with the real world is inescapable. This is the issue discussed in subsection 3.4.

3.2.2 On some implications (risks) of the theoretical attitude: excess of abstraction and irrelevance. An historical excursus

Economists who put their ‘faith’ in mathematical models often forgets the warnings of Francis Y. Edgeworth, Alfred Marshall and Vilfredo Pareto – needless to say, three founders of the neoclassical mathematical economics – concerning the use of the mathematical engine in economics:<sup>18</sup> the resistance of economic material to pure reduction by means of mathematical treatment, the risk of omitting certain important factors too difficult to reduce to the mathematical treatment; the idea that formal abstractions are legitimate as long as they do not lose their experimental character (a point strongly stressed by Pareto); in general the limitation of deductive reasoning unsupported by specific experience.

A consequence of this attitude is the risk of an “excess of abstraction”, as Edgeworth wrote in his controversy with Walras at the time of the second edition of the *Éléments d'économie politique pure*, at the end of the nineteenth century, in the period of the first spread and consolidation of mathematical economics (see Marchionatti 2004 and 2007). The controversy between Edgeworth and Walras reveals the clash of two different methodological requirements. On the one hand, there is Walras’s requirement of the rigour and simplicity allowed by the reduction of economics to mathematical treatment. Walras considered his simple model of free competition to be the general case. On the other hand, there is Edgeworth’s requirement of greater realism for the model. He rejected the Walrasian level of abstraction as a representation of the general case: according to Edgeworth, the Walrasian case was acceptable only as a case of extreme simplification, not as a general case. The dispute can be understood as centred on the conception of economics. Walras considered economics to be a physical-mathematical science like mechanics. Hence he saw the mathematical method and language as the natural expression of reasoning in political economy. The entire theory had to be mathematical, and the mathematical expression of the theory was considered a condition of its intelligibility. Walras defined the mathematical method a “rational method”: “The mathematical method is not an experimental method; it is a rational method” (Walras, 1889, p. 71), he wrote. Edgeworth did not accept Walras’s rational mechanics reductionism. He emphasised that mathematics has an instrumental use in economics. He agreed with Walras that mathematics is necessary for deductive reasoning, but he limited its use to simple cases. This position was not due to a different knowledge or a

different image of mathematics, but to a different idea of economics as a science. Edgeworth's position was close to Marshall's. In the *Principles* Marshall maintained that economic science "must never lose sight of the real issues of life; and these are all... affected more or less by motives that are not measurable" (Marshall 1890, 78). He emphasised the complexity of human and social subjects, which implies that 'economic laws' have some limitations as to exactness, certitude and precision. Edgeworth and Marshall emphasised that economic material is often unable to resist the strains of the mathematician's machinery – a judgement then adopted by Keynes (see Marchionatti 2010): the nature of economic material is what limits the use of mathematics and the fundamental part of a complex real-life problem rarely can be grasped by a series of equations. The greater realism of the hypotheses and models that is implicit in Marshall and Edgeworth's thought made them consider Walrasian theories to be spoiled by an excess of abstraction. This crucial issue concerning the role and extent of the use of mathematics in economics reappeared in the work of Vilfredo Pareto (Marchionatti & Gambino 1997; Marchionatti & Mornati 2003), the leading figure in mathematical economics between the 1890s and the first World War. His position had many points of contact with those of the two great English economists. In his "Considerazioni sui principi fondamentali dell'economia politica pura" (Pareto 2007 [1893–94]), his first fundamental systematic reflection on pure economics, Pareto's remarks followed Edgeworth's and Marshall's argument. In the mid-1890s the methodological difference between Walras and Pareto became explicit in Pareto's critique of Walras' *économie sociale*, which he believed to be vitiated by metaphysics, consequently, on a different conception of applied economics. Pareto was an advocate of the prevailing standard of natural sciences as a practice associated with experimental verification. Therefore, he adopted a strongly antimetaphysical attitude. As a consequence, in the Paretian era (the period between the 1890s and the first World War), mathematical economics was dominated by the problem of the relation between the model and the real world (see Marchionatti 2004 and 2006), and the question of the irrelevance of the theory – an implication of the excess of abstraction – was crucial. In fact, the opinion was widely shared the opinion that the abstractness of mathematical economics made it extremely difficult to apply its conclusions to the explanation of actual facts. The main issues under discussion in the early years of the new century were the excessive abstraction of theory and the unreality of its assumptions and models, rather than its formal aspects. These economists were chiefly

interested in the problems connected with the relationship between mathematical expression and experimental reality. In the 1930s, the Viennese debate at Menger's *Mathematische Colloquium* (Marchionatti and Mornati 2015) modified the theoretical and methodological framework. With the axiomatisation of economic theory and the emergence of a neo-Walrasian approach, mathematical developments free from problems of the model's realism were adopted, and they influenced the emergence of a different idea of the nature and method of economics as a science, with the crucial implications we have emphasised: this was the epistemological context of contemporary mainstream macroeconomics.

### 3.2.3 Theoretical status: unsolved problems and inconsistencies, or pseudo-foundations of macroeconomics

Since the 1970s, the general equilibrium of neo-Walrasian origin has been considered the fundamental framework for theoretical discourse in neoclassical perspective, and it has colonized macroeconomics as well. Many authors have recently emphasized that despite its success, this theory “is not exactly alive and well” (Ackerman 2002, 120): in fact, the equilibrium in a general equilibrium model is not necessarily either unique or stable, and “there are apparently no grounds for dismissing such ill-behaved outcomes as implausible special cases” (ibid.). The story begins in the 1970s, when some theorists reached negative conclusions about both the uniqueness and the stability of general equilibrium. As for the uniqueness, Debreu (1970) demonstrated that it was ensured by certain unrealistic restrictions on the nature of aggregate demand. As for the stability, Sonnenschein (1972), Mantel (1974) and Debreu (1974) - since then their results have been known as the Sonnenschein-Mantel-Debreu (SMD) theorem -, found that almost any continuous pattern of price movements can occur in a general equilibrium model and that *tâtonnement* does not lead to convergence to equilibrium. Kirman and Koch (1986) generalised the SMD theorem, i.e. they proved that virtually any continuous price dynamic can occur even if all consumers have identical preferences and any arbitrarily chosen income distribution is used. Saari (1992) showed that the ‘SMD instability’ may be a property of an economy as a whole even if it is not present in any part of the economy. Saari (1985) showed that any price adjustment process that converges to an equilibrium has infinite information requirements. Recognizing this failure of General Economic Equilibrium theory, Kirman (1989 and 1992) and Saari (1995) wrote fundamental analyses

on the reasons for it. Kirman (1989) argued that the problem lay in treating individuals as acting independently of each other – “this plays an essential role in the construction of economies generating arbitrary excess demand functions” (p. 138). Saari (1995) emphasized the unlimited variety of individual preferences that, when aggregated, “can generate all imaginable forms of pathological behaviour” (p. 229). In other words, instability arises because aggregate demand is not as well behaved as individual demand. This signals a problem of aggregation, a well known problem in the literature (see for example Stoker 1995). It is quite obvious to say, as for example Ackerman does, that the aggregation problem makes “the pursuit of microfoundations for macroeconomics futile” (Ackerman 2002, 127). That is, it is impossible to draw useful conclusions about macroeconomics directly from the understanding of individual behaviour, owing to the problem of aggregation (see also Rizvi 1994). As already Arrow (1986) wrote, in the aggregate, the hypothesis of rational behaviour has in general no implications.

In the DSGE models with representative agent this problem “is not solved, just treated as if it were solved” (Kirman 2010, p. 508). In fact, the assumption of a representative agent generates a unique equilibrium, but this assumption is open to the criticism that there is no simple relation between individual and aggregate behaviour. Hence assuming that behaviour at individual level can be likened to that at aggregate level is erroneous: it is a not theoretically justified assumption. As Kirman writes,

“the assumption of a representative individuals is far from innocent; it is the fiction by which macroeconomists can justify equilibrium analysis and provide pseudo-microfoundations. I refer to those as pseudo-foundations, since the very restrictions placed on the behavior of the aggregate system are those which are obtained in the individual case and ... there is no formal justification for this” (Kirman 1992, 125).

Therefore the neo-Walrasian programme in macroeconomics is trapped between the Scylla of the aggregation problem and the Charybdis of the unjustified fiction of the representative agent.

To complicate the picture, in the mainstream programme there is the issue of adding ingredients to the basic model with the risk of having to deal with a problem of inconsistency. Acemoglu (2009) has summarised the predominant opinion in the mainstream since the 2007-8 crisis, maintaining that some theoretical changes are

necessary. In particular, the main suggestion is to make some aspect of the financial sector endogenous to the model.<sup>19</sup> Is the excess of abstraction and the consequent inadequacy of mainstream models – particularly, in the case of the current crisis, their insufficient consideration of the financial sector – surmountable by refining the existing models, in particular by retaining the existing framework of DSGE models but making the financial sector endogenous to the model?

In general, the strategy of adding new ingredients and parts to the model (often insights from the periphery of macroeconomics) is considered hardly feasible by many authors. Caballero (2010) is pessimistic about the possibility of answering ‘yes’ to the above question, and he thinks that this strategy “is plagued by internal inconsistencies and pretence-of-knowledge problems” (p. 88). “The pretence-of-knowledge” is the title of Hayek’s 1974 Nobel Price Lecture: it refers to the dangerous belief that we possess a degree of knowledge and precision that we do not have. DSGE macroeconomics is affected by this syndrome, Caballero maintains. He thinks that it would be better to consider the basic model (where the equilibrium is described in a frictionless world) as a benchmark, “not a shell or a steppingstone for everything we study in macroeconomics” (p. 90). Caballero emphasises another crucial methodological problem, an internal inconsistency: the core assumptions – *in primis* the Rational Expectations Hypothesis – become “increasingly untenable” as we continue to add realism into the core. More generally, these inconsistencies refer to the difficulty of using the ‘successive approximation method’, a well-known problem in Marshall’s and Pareto’s works. It is a method, those old economists wisely maintained, useful only in the “early stages of economics” for the reasons concerning the difficulty of using long chains of deductive reasoning. The risk of using this method, to use Caballero’s expression, is that of entering ‘Fantasyland’.

Actually, as Stiglitz (2011) effectively writes, such Ptolemaic exercises in economics “will be no more successful than they were in astronomy in dealing with the facts of the Copernical revolution” (p. 593). Hence, a dilemma arises on using DSGE models: the basic model seems to be simple and elegant, but substantially irrelevant, because what is left out is essential; on the other hand, adding ingredients – so making the model more realistic – gives rise to inconsistencies. A strong doubt emerges: that the essential problem is that the standard model is not a good starting point.

Another aspect of the inconsistency problem is emphasised by De Grauwe (2010). He points out the inconsistencies due to the introduction of *ad hoc* assumptions in the



process of model extension. He writes that on the one hand, “the micro-founded macroeconomic models had to be sent back to the repair shop” (p. 417) but, on the other, once in the repair shop, the ambition of micro-founding the macrotheory is diluted “by introducing *ad hoc* assumptions about why agents do not adjust their plans instantaneously and why prices are rigid” (ibid.). However, De Grauwe notes, in the context of DSGE models the assumption of “super-rational agents” but “prone to strange habits” (ibid.) that prevent them from acting as optimizing agents (ibid.) is inconsistent. Hence, he concludes, “when the models came out of the repair shops, they were changed fundamentally by the addition of features that have no micro-foundations” (ibid.). These changes, of course, “were not just innocent ones”: on the contrary, “they were crucial in making the model fit the data ... ways to introduce heuristics into the DSGE models through the back door” (ibid.).<sup>20</sup>

This set of unsolved (or unsolvable) problems justifies the assertion that mainstream macroeconomics is founded, to use Kirman’s expression, on pseudo-microfoundations.

### *3.3 Mainstream macroeconometrics. Theory-driven econometrics and policy implications, or Lucas’s razor returns*

On becoming familiar with the main tools of DSGE modeling, the role of parameters in linking theory to reality becomes evident. However, the plentiful attempts to shape theoretical model predictions to empirical data reveals a failure in many fundamental respects. Did DSGE models truly resist Lucas’s critique?

#### *3.3.1 Bayesian and VAR DSGE: some ‘parametric’ tools*

As already pointed out, solved DSGE models are equivalent to structural VARs, based on cross-equation parametric restrictions<sup>21</sup> consistent with the rational expectations hypothesis (Chari *et al.*, 2008). This hypothesis is necessary to identify (and estimate/calibrate) the ‘deep’ structural parameters, which are meant to be invariant to both natural and policy shocks.

At this point, the role of the empirical analysis emerges. In principle, the same criticism brought against the Cowles Commission about the super-exogeneity of theoretically-

based policy variables can be made of the DSGE approach. Hence, many authors (e.g. Bernanke and Mihov, 1995; Christiano, Eichenbaum, and Evans, 1996a, 1996b, 1998) have made great effort to show how policy actions can be identified by means of empirically-based restrictions, independently of the underlying theoretical models.

In practice, the important step consists in the identification of shocks: in fact, DSGE-VARs operationalize policy shocks<sup>22</sup> into actual economies by means of impulse response functions describing the reaction of the relevant macroeconomic variables to policy instruments. Once parameters have been estimated,<sup>23</sup> experiments on shocks are performed. Keeping the (estimated) parameters constant, the same experiment is reproduced in model economies, and actual and model-based impulse responses are compared by means of proper objective functions making it possible to discriminate among alternative theoretical models. Finally, the intertemporal effect of policy shocks is investigated by means of variance and historical decomposition techniques.<sup>24</sup>

Because DSGE models are characterized by a huge number of parameters that have to be estimated with sparse empirical data, model likelihoods<sup>25</sup> are generally ill-behaved, i.e. they are highly dimensional objects characterized by numerous local minima and maxima and by nearly flat surfaces, that hinder optimization algorithms in model evaluation. Hence, the suitable small sample properties of the Bayesian techniques prove to be very attractive (Fernandez-Villaverde, 2009).

In the Bayesian approach to DSGE, the observational data are given, i.e. they are no longer regarded as the product of some underlying data-generating process. The model is chosen by specifying, apart from the parameter set and the likelihood function, a prior distribution describing the pre-sample beliefs about the true parameter values. From Bayes' theorem, the posterior distribution is derived, i.e. a new set of beliefs that combines the priors with the sample information in the likelihood. Whenever a new belief emerges (e.g., nonstationarity), priors are changed accordingly, while classical inference generally requires the adoption of specific methods (Sims and Uhlig, 1991). At this point, inference (point estimation and model comparison) can be performed.

### 3.3.2 The 'Parameters-trap': identification issues, micro-macro inconsistencies, and the structural problem

Both approaches to DSGE macroeconometrics have evident vulnerabilities that substantially derive from how parameters are handled. In brief, parameter values from

formally elegant models are ‘calibrated’ in order to reproduce some stylized fact, hence suggesting a sort of consistency between the theoretical model and the observational data. But there are at least three main respects in which this practice fails.

First of all, DSGE models have difficulties in taking account of many important mechanisms governing real economies, e.g. institutional constraints like the tax system, thereby reducing DSGE power in policy analysis. In an attempt to deal with this problem, various parameter constraints on the model policy block are provided. They derive from institutional analysis and reflect policy-makers’ operational procedures. In addition, proper assumptions on the lag of policy impacts impose that macroeconomic variables do not simultaneously react to policy variables. However such extensions, intended to reshape model predictions to the reality and to deal with the underlying optimization problem, prove to be highly inflexible, turning DSGE into a ‘straitjacket tool’ (Pesaran and Smith, 1995).

In particular, the structure imposed on DSGE parameters entails various identification problems, such as observational equivalence, underidentification, partial and weak identification. Fundamentally, they are ascribable to the likelihoods to estimate. Using the Christiano et al. - Smets and Wouters model, Canova and Sala (2009) show that the range of structural parameters generating impulse response functions very close to the true ones includes model specifications with very different features and welfare properties.<sup>26</sup> Hence, reasonable estimates do not derive from the informative contents of models and data, but rather from the ancillary restrictions that make the likelihood informative. Thus, after Lucas’s superexogeneity critique has been thrown out the door, it comes back through the window. Already Pesaran (1981) stated the identification problem in DSGEs, assessing the observational equivalence between the rational expectations model and the general distributed lag model in the absence of *a priori* restrictions (“untestable and often quite arbitrary”, *ibid.*, 375) on the processes generating the exogenous variables and disturbances. Clearly, such empirical equivalence has fundamental implications for policy analysis, since the distributed lag model asserts the effect of fiscal and monetary policies on output and employment targets, while the rational expectation model denies any intended and systematic impact of the policy-makers’ tools on those macroeconomic indicators.

Analogous problems affect the Bayesian approach to DSGE. In fact, Canova and Sala (2009) underline that arbitrarily chosen priors may hide severe identification problems<sup>27</sup>,

causing the same informational deficiencies as in the classic likelihood estimation:

“The common practice of fixing some troublesome parameters to arbitrary (calibrated) values may create distortions in the distribution of the estimated parameters, making the results difficult to interpret” (*ibid.*, 432).

This is acknowledged by the ‘New Macroeconometrists’, who claim that:

“a DSGE model is a very stylized and simplified view of the economy that focuses only on the most important mechanisms at play. Hence, the model is false by construction and we need to keep this notion constantly in view” (Fernandez-Villaverde *et al.*, 2009, 7).

But the DSGE agenda has been mainly concerned with matching the facts, rather than with identifying the mechanisms explaining such facts. This circumstance has serious implications in terms of policy analysis, since many large-scale models suited for that purpose have been subject neither to identification checks nor to evaluation analysis. But, under observational equivalence, it is impossible to distinguish between important and unimportant features, while under weak and partial identification problems, policy conclusions become questionable (Canova and Sala, 2009). Hence, although the Bayesian approach is useful for directly answering practical policy questions (e.g., what is the probability that the selected instrument will produce the desired result conditional on the observed data?), Bayes’ theorem is applied to “a set of axioms that decision theorists have proposed to characterize rational behaviour” (Fernandez-Villaverde *et al.*, 2009, 9). But widely acknowledged is the gap between micro evidence and macro priors, which causes the above specification problems. More generally, Browning *et al.* (1999, 545) recognize in regard to DSGEs that “the microeconomic evidence is often incompatible with the macroeconomic model being calibrated”. These authors highlight three main criticalities, that feed the micro-macro gap and that DSGEs largely neglect for reasons of computational tractability: heterogeneity, which empirical evidence widely documents, in preferences, constraints, and skills; uncertainty, for it is fundamental to distinguish between micro and macro uncertainty, and to deduce it from measurement error and model misspecification; and synthesis of the micro evidence, since a plethora of micro studies often imply very different assumptions that prevent the (estimated) parameters

from fitting any kind of context.

Finally, worth recalling is the problem of the intertemporal inconsistency of the rational expectations hypothesis with unanticipated structural breaks.<sup>28</sup> The empirical importance of this point is very evident on considering the effects of the latest financial and economic crisis. In this regard, Hendry and Mizon (2010) prove that the conditional expectation is not an unbiased predictor and that the law of iterated expectations does not hold intertemporally. *Hence, Lucas's critique still applies to DSGEs, which require no changes in the expectations distribution: DSGEs are intrinsically non-structural.* This point further prevents the use of DSGEs for policy analysis:

“Although no model is perfect, choosing amongst the available models on the basis of economic theory coherence, no matter how inconsistent the result is with empirical evidence, has little to recommend it for economic policy and forecasting” (Hendry and Mizon, 2010, 13).

#### **4. Conclusions. The dead end of neo-Walrasian Macroeconomics**

In the years of the new great economic crisis, the contemporary mainstream macroeconomics - the last result of Lucas's anti-Keynesian revolution of the 1980s which tried to give macroeconomics sound neo-Walrasian microeconomic bases - seems to reveal a serious failure. This paper has argued that the reasons for this failure reside in the implications of the neo-Walrasian legacy and the problems connected with the implementation of that programme. First of all, its theoretical attitude: the prevalence of rigour over realism. Then its theoretical status: the aggregation problems and difficulties of adding realism in the model. Finally the theory-driven econometrics.

The prevalence of rigour over realism entails the risk of “excessive abstraction”: a vice which seems intrinsic to mainstream macroeconomics. The aggregation problems and the methodological problems determine the pseudo-microfoundations of macroeconomics. The question of its relationship with the real world and the falsifiability of the model is crucial in this regard because it unifies, in a certain sense, the previous theoretical issues and has serious implications in terms of policy analysis. To be stressed is that the new macroeconometrists do not actually ignore real data. Rather, they exhibit their own peculiar *Anschaung*: observational data are used to calibrate the structural parameters in

order to make the model adhere to indicators from the real world. Unfortunately, the formal DSGE mathematical framework mostly supplies ill-behaved functions, i.e. DSGE calibration practice does generally select sets of parameter values generating model specifications that exhibit very different features and welfare properties. That is to say, without arbitrary and untestable restrictions, information cannot be adequately processed by DSGE tools in order to discriminate among competing model specifications. *Hence, all conjugations of such identification problem make DSGEs subject to the same criticism that they were conceived to resolve, i.e. Lucas's razor.* This fact is particularly alarming when considering that policy-makers have made profuse use of large-scale models that have never been subjected to either identification checks or evaluation and sensitivity analysis. The same reasoning applies to rational expectations, which are manifestly untrue when accounting for unanticipated structural breaks: their assumed structural nature, i.e. time invariance, proves to be intrinsically unstructural. Overall, we may conclude that modern macroeconomics tortures data to demonstrate consistency with an *a priori* world view. According to Keynes, the (neo)classical theory of his time had become a purely formal analysis with little empirical relevance, and he traced the origins of this fact back to Ricardo:

“Ricardo offers us the supreme intellectual achievement, unattainable by weaker spirits, of adopting a hypothetical world remote from experience as though it were the world of experience and then living in it consistently. With most of his successors common sense cannot help breaking it – with injury to their logical consistency” (Keynes 1936, 192).

Similarly, mainstream macroeconomics, along the line of Lucas's anti-Keynesian revolution, has adopted as its basic model an artificial world remote from the reality in which to live, and it has failed to account for the actual evolution of the real-world economy. Because mainstream macroeconomics has taken the “wrong line”<sup>29</sup> of constructing a macroeconomics aspiring to the precision of microeconomics of perfect rationality - this is our thesis - it is unable to handle the complexity of the real world. Therefore, our answer to the question ‘Is the neo-Walrasian macroeconomics a dead end?’ has to be positive. The endeavour by Lucas's approach - to build a highly abstract (microeconomically founded) but relevant (empirically tested) model in order to reach policy conclusions - has had quite disappointing outcomes and the new great crisis has

marked its defeat.

An appropriate aphorism in the field of macroeconomics could now declare: if you have an interesting story to tell, you can no longer tell it in a DSGE model.

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## Notes

<sup>1</sup> It is fair to say that Blanchard, in the wake of the crisis, changed his opinion and acknowledged the existence of serious problems in mainstream macroeconomic theory (Blanchard *et alii*, 2010).

<sup>2</sup> Structural models can be interpreted as reparametrizations of the reduced form, where restrictions apply to the over-identified specifications. Hence, diagnostics compare the restricted reduced form with the just-identified reduced form, where endogenous variables depend on all exogenous variables with unrestricted coefficients.

<sup>3</sup> Increasing evidence that such models did not fit real data led to the proliferation of diagnostic and misspecification testing and it raised the issues of dynamic specification and model selection within an agreed inference framework.

<sup>4</sup> “Given that the structure of an econometric model consists of optimal decision rules of economic agents, and that optimal decision rules vary systematically with changes in the structure of series relevant to the decision maker, it follows that any change in policy will systematically alter the structure of econometric models” (Lucas, 1976, 41). The empirical significance of Lucas’s critique went largely unrecognized by the profession. Alogoskoufis and Smith (1991) empirically illustrate the point by investigating the shifts in the persistence of inflation following the collapse of fixed-exchange-rate regimes.

<sup>5</sup> Henceforth, the term ‘structure’ specifically refers to the set of basic model features that are invariant to changes in the economy.

<sup>6</sup> Chary 1998 (p. 172), emphasized this point as crucial: “The chief theoretical difficulty in developing these foundations was that macroeconomic questions necessarily involve dealing with dynamics and uncertainty”.

<sup>7</sup> In his article “Methods and Problems in business Cycle Theory” (1980), Lucas criticised the neoclassical synthesis because it rested on a old-style interpretation of Walrasian theory, where equilibrium was conceived as a static notion, acting as a center of gravity for disequilibrium states: “I refer to this theory as static ... The underlying idea seems to be taken from physics, as referring to a system ‘at rest’. In economics, I suppose such a static equilibrium corresponds to a prediction as to how an economy would behave should external shocks remain fixed over a long period, so that households and firms would adjust to facing the same set of prices over and over again and attune their behavior accordingly” (278). Lucas maintained that “the idea that an economic system in equilibrium is in any sense ‘at rest’ is simply an anachronism” (ibid.).

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<sup>8</sup> As is well known, the assumption asserts that individuals use their information in an efficient way, without systematic errors, in the formation of their expectations. More specifically, it assumes that expectations correctly identify the mean and variance of stochastic variables which influence future contingencies. It does not deny that individuals can make forecasting errors, but it suggests that errors will not persistently occur, i.e. the deviations from the pattern of correct forecasting are random: in fact, if they were systematically biased, they would be incorporated into the agents' expectations.

<sup>9</sup> According to Solow (2000, 152), this is “the most spectacularly implausible” welfare conclusion reached by these models.

<sup>10</sup> Cooley and Leroy (1985) strongly criticized Sims' a-theoretical approach, maintaining that Granger's (1969) and Sims' (1972) causality tests were very different from the Cowles tests, which were based on the structural vs. non-structural interpretation of the models. Since causality tests are structural in nature, conclusions about causality were unsupported as long as VARs were interpreted as inherently non-structural. Conversely, if VARs were considered structural, the restrictions on the residual distributions imposed by the a-theoretical approach were not *de facto* arbitrary renormalizations, but rather prior theoretically-based identifying restrictions, which the a-theoretical approach did not intrinsically support.

<sup>11</sup> In fact, the true endogeneity of policy reflects on the observed correlation between macroeconomic and policy variables. Whenever policy instruments are assumed to be exogenous, this correlation can be mistakenly attributed to their efficacy on macroeconomic indicators.

<sup>12</sup> Alogoskoufis and Smith (1991) criticized the approach both for its unconscious use of non linear restrictions and for the fact that its modelling of expectations was a mere reparametrization of dynamic linear regression and VAR models.

<sup>13</sup> Model congruency was directly tested against the true unobservable data-generating process, through the vector of random residuals: whenever it did not follow a (multivariate) normal distribution, the model was misspecified. Moreover, stationarity was a crucial statistical property: non-stationary models were treated as cointegrated VARs testing and imposing theory-based rank-reduction restrictions on the matrix of long-run equilibria, in order to provide a statistically coherent framework for the short-run analysis (Johansen, 1995).

<sup>14</sup> In fact, they are not LQ (linear-quadratic) form (Whittle, 1981), i.e. they did not rely on quadratic objective functions and linear constraints.

<sup>15</sup> See Summers (1991), Pesaran (1987), Kirman (1992) for discussion.

<sup>16</sup> Calibrated models were developed in several steps (Favero, 2007): a. a preliminary a-theoretical inspection of the real data to derive the stylized facts of interest; b. the selection of a relevant class of parametric models, combining the preferred theoretical framework and the observed stylized facts; c. based on the selected model, the theory-based definition of the quantities of interest, and the reorganization of available measurements, whenever theoretically inconsistent; d. based on measurements, the



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empirically-based evaluation of specific unknown parameters; e. model simulation and model evaluation based on the comparison between the actual and the simulated series, relying on some informal measure of their distance. In order to measure such distance, the method of moments was applied, setting the discrepancy between the observed and the simulated moments to zero. Christiano and Eichenbaum (1992) derived a variant of Hansen's (1982) GMM to estimate DSGE using just specific moments of the real data. By contrast, standard econometric methods (like ML estimation) use the entire available information set, weighting the different moments according to how much of their information is contained in the real data.

<sup>17</sup> On Debreu and his influence on economics see Düppe 2010.

<sup>18</sup> Their wise advice has been recently echoed by Lawson (2009).

<sup>19</sup> Indeed, the main weakness of current DSGEs is considered to be the absence of an appropriate way to model financial markets. In fact, by excluding the formal modelling of financial markets, the current benchmark DSGE models fail to explain important regularities in the business cycle (Tovar 2008).

<sup>20</sup> The attempt to incorporate ideas developed in other contexts (not-mainstream) into mainstream economics, ignoring those contexts and thus leaving mainstream analysis essentially unchanged, is effectively described by Palley (2013) as "gattopardo economics".

<sup>21</sup> Cross-equation restrictions have three main implications (Piazzesi, 2007). First, they constrain the rational expectation parameters to be consistent with the parameters from the equilibrium probability distribution, removing free parameters describing prerational expectations (Evans and Houkapohja, 2005). Second, they tie together processes describing different endogenous variables that involve the same parameters and shocks, thereby increasing estimation efficiency for different data series containing information about the same parameters. Finally, rational expectations imply that the data-generating process underlying agents' beliefs is the same as the true data-generating process, hence justifying GMM estimation on moments derived from the Euler equations.

<sup>22</sup> In order to be identified, the shocks must be reciprocally orthogonal, i.e. independent. However, since orthogonality is not an intrinsic VAR property, some structure is needed. This is generally tested via the  $\chi^2$ -test, where the number of degrees of freedom corresponds to the number of over-identifying restrictions.

<sup>23</sup> Parameter estimation/calibration is implemented via the Generalized Method of Moments (GMM), i.e. by selecting those parameter values that better fit some property (moment) of the data. Then, models are evaluated on their likelihood, i.e. their ability to reproduce data properties other than those used in GMM estimation.

<sup>24</sup> Variance decomposition techniques investigate in different time horizons which fraction of the forecasting error variance of the relevant variables can be attributed to the policy shocks, while historical decomposition methods evaluate the effect of zeroing the policy shocks.

<sup>25</sup> The Likelihood Principle states that the entire sample information is embodied in the

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likelihood function (Berger and Wolpert, 1988). It represents the probability assigned to each observation given a chosen set of parameter values.

<sup>26</sup> This is due to the ill-behaved mapping between the structural parameters and the coefficients of the stationary rational expectations solution, which means that the model transition laws are relatively insensitive to changes in many parameter values.

<sup>27</sup> The posterior distribution is proportional to the likelihood times the prior. Hence, in the case of variation-free parameters, the likelihood conveys information about the parameters whenever the prior and the posterior differ (Poirier, 1998). Conversely, in the case of parameter constraints, these shift the posterior away from the prior, hence differences between the priors and the posteriors do not guarantee parameter identification.

<sup>28</sup> Hall (1978) shows that unanticipated changes are unpredictable one-step ahead by rational expectations. Hence, structural breaks cause serious forecast errors.

<sup>29</sup> The term “wrong line” was used in his *Theory of Political Economy* (1871) by William S. Jevons in his criticism of Ricardo, who, according to Jevons, “shunted the car of Economic Science on to a wrong line”.

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