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*Original Citation:*

*Availability:*

This version is available <http://hdl.handle.net/2318/1632349> since 2018-04-24T15:28:04Z

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**Is neo-Walrasian Macroeconomics a Dead End?  
An Assessment of Recent Criticisms of DSGE models**

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**Abstract:** *After the 2008 'new Great Crisis', it is widely recognized that mainstream macroeconom(etr)ics - 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 macroeconom(etr)ics 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 macroeconom(etr)ics - 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.*

**Key Words:** *Neo-Walrasian Macroeconom(etr)ics, Lucas, DSGE Models, Theory-driven Econometrics*

**JEL Classification:** *B22, B23, B31, D50*

## 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 transformation that has taken place within mainstream economic thought in the past forty years since Lucas’s anti-Keynesian revolution. According to many economists, they symbolize the general consensus, which arose at the beginning of the new millennium, 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).<sup>1</sup> At the same time, the empirical implementation of the new theoretical models has become a very common tool of analysis used by central banks. 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 ‘great recession’ exploded in 2008, it is widely recognised that mainstream macroeconomists have failed to anticipate and then appraise the crisis. 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.<sup>2</sup>

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 Lawson 2009a and b; Kirman, 2010; Caballero, 2010; Heckman, 2010; Stiglitz, 2011; Krugman, 2012; Solow, 2012, Romer 2017), not to say post-Keynesian economists? Furthermore, is not this failure more critical if core institutions (e.g., central banks) have designed their intervention on the basis of such theory? Mainstream macroeconomists recognize that theoretical changes are necessary, but they are confident that the approach does not need to be discarded. For example Chari (2010, p. 1), after recognizing that 'our models failed to see the recent crisis coming', asserts that this failure is not 'because we did not have the right tools in our toolbox' (*ibid*, p. 7); rather, the problem is that of improving the current mainstream macroeconomic theory, 'to devote more resources to it' (*ibid.*, p. 10), not of discarding it.

The issue, however, is considered to be less simple than Chari puts it, also in the mainstream territory. In fact, if we look at what Robert Lucas himself 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, p. 23). *A fortiori*, this judgement

applies to the new great crisis. In other words, systemic crises are other-worldly events absent from these models, and their most eminent theorists were aware of it.

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. The thesis of this paper is that this 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. Throughout the paper it is discussed that, 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 and its policy implications. In other words, neo-Walrasian macroeconomic models are subject to Lucas's critique as well as their predecessors. Lucas's critique becomes a boomeranging razor.

This paper is an attempt to demonstrate our thesis essentially grounding on the manifold criticisms of mainstream macroeconomics that have emerged in recent years. It focuses on both the theoretical foundations of mainstream macroeconomics, offering a revisitation of Lucas's neo-Walrasian research programme, and the related macroeconometric modelling and tools, thus providing an original 'double perspective' that coordinates both theoretical and econometric issues, showing how the two sides of the story strictly resemble each

other. Section 2 considers the state of mainstream macroeconomics and macroeconometrics and their origin. Sections 3 is devoted to the criticism of mainstream macroeconom(etr)ics. The final section concludes.

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

### *A premise*

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, a real anti-Keynesian revolution came about, with Lucas as its main proponent and theoretician. Based on an extensive criticism of both the theoretical apparatus of the neoclassical synthesis and the Keynesian macroeconometric 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 'a dead end', and Keynes's *General Theory* was dismissed as an example of 'bad social science' (Lucas, 1976; Sargent, 1977). Lucas's new classical macroeconomics inaugurated

a new paradigm, which underwent an evolution that led to Real Business Cycle modelling, and finally to the emergence of DSGE modelling. It was the 'new neoclassical synthesis' (a term coined by Goodfriend and King, 1997) between neo-classicals and neo-Keynesians, at the end of the 1990s. Referring to its foundations, we prefer to label 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.<sup>3</sup>

Early DSGE models make the assumption of the representative agent, justifying this reduction as an analytical convenience. Its main harmful implication is that the structure of agents' interactions is essentially neglected. As a consequence, the model is in its essence a simple analytically tractable macromodel considered by its founders as a first approximation to develop. In its first formulation, there is no room for heterogeneity, a simplification that has been recently abandoned: heterogeneity in behavior and decisions is now considered, assuming the existence of different agents with a fixed distribution of characteristics making 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.

*At the origin of the DSGE models, 1. The twofold criticism of the Cowles Commission approach and Lucas's razor*



The neo-Walrasian theoretical framework found its statistical counterpart in DSGE macroeconometric models. To understand it fully, its harsh criticism of previous approaches has to be appraised, mostly coming from the super-exogeneity of policy variables and their ineffectiveness in policy evaluation. The criticized models derived from the Cowles Commission approach, which stated a clear dichotomy between theory and econometrics. Theory identified the list of the relevant variables to be included in the analysis by providing some static long-run relationships and some ancillary statistical assumptions (e.g. linearity, homoskedasticity, temporal independence and normality of residuals, etc.), and by focussing on conditional statements specifying the distribution of the relevant variables contingent on the values of all remaining variables (information set). Policy variables were exogenous (instrumental), i.e. either unmodelled or independent from present and past information sets. Then, econometrics entered the scene, providing the methodological tools (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 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 the theory proved to be almost unfalsifiable, since it was unclear whether rejection involved either the theoretical assumptions, or the ancillary statistical assumptions.

Spanos (1990) identified the roots of the Cowles Commission 'fallacy' in the statistical identification problem, i.e. the scant attention paid to the well-definiteness of the statistical model implied by the estimated structural model.

From a statistical point of view, structural models can be conceived as (restricted) reparametrizations of reduced form models (Favero, 2007)<sup>4</sup>. Hence, statistical identification refers to the choice of a well-defined reduced form that satisfies the underlying ancillary statistical assumptions for the measured data. Following this approach, 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 that changes in the economic policy instruments have on outcome variables (Hoover, 2001). But the validity of diagnostics is clearly bound to the adherence of the statistical model to observational data, which in turn determines the model effectiveness in policy analysis.

In the meantime, Lucas's critique pointed out the scant adherence of the Cowles Commission models to theory. The super-exogeneity of policy variables was discussed, underlining that no expectations were explicitly taken into account: since expectations depend upon the policy regime in place, they change with the regime. However, because models were based on reduced forms, they neglected the fact that agents change their decisions when faced with a change in the institutional regime, since their parameters actually described a mixture of the 'deep' parameters (e.g. preferences and technology), thus engendering uneffectiveness in policy evaluation. This is the structural identification problem (Spanos, 1990), which refers to the uniqueness of the structural parameters as defined by the (reparametrization and restriction) mapping from the statistical parameters in the reduced form: only structural models, derived from the fundamentals of the economy as agents' preferences and technological constraints, are able to provide a robust grounding for the evaluation of

alternative policies. In other words, the flaw of Keynesian models was their lack of microfoundations. As a result, the Cowles Commission models were considered as statistically inadequate, theoretically inconsistent, and practically irrelevant (Pesaran and Smith, 1985).

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

As is generally recognized, Robert Lucas had the pre-eminent role in the foundation of the new classical macroeconomic theory, particularly 'at the methodological level' (Vercelli, 1991, p. 129-130; Hartley, 1997; for a reconstruction of Lucas's thought on macroeconomics see Laidler, 1986; Chary, 1998; De Vroey, 2010a, b; De Vroey and Malgrange, 2011). His 1976 paper, 'Econometric Policy Evaluation: A Critique', provided the 'fundamental rationale for microfoundations after 1970' (Hoover, 2013, p.11). As explained, Lucas criticized the neoclassical synthesis as a failure, as far as the assessment of alternative policies was concerned. To overcome that *impasse*, the new macroeconomics (and macroeconometrics as well) had to be based on explicit and fully formulated microfoundations (the deep parameters), and it should try to achieve the standard of rigour imposed by the Neo-Walrasian General Economic Equilibrium Theory, in order to elaborate a valuable macroeconomics of equilibrium. Its predecessors were the pre-Keynesian business cycle theories: according to Lucas (1975), the object of this new macroeconomics had to be business fluctuations again. Hence, the pre-Keynesian literature on business cycles was the necessary reference: 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 reference is to Hayek's business-cycle theory, in particular his 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). Starting from 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>5</sup>

The contradiction could be overcome by applying a different concept of equilibrium: 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' (*ibid.*, p. 707). In other words, in order to adhere to the rigorous tenets of the equilibrium theory, a particular hypothesis or principle was necessary. Lucas found this principle in John Muth's hypothesis of rational expectations, according to which price distributions could

not differ in a systematic way.<sup>6</sup> It is important to note that the hypothesis of rational expectations is not the result of empirical observation: it is a convenient way to close the model. Hence, the conception of equilibrium becomes that of a rest point in the space of decision rules, now considered in mainstream macroeconomics to be the central theoretical breakthrough of the last 50 years, contrary to the conventional conception of equilibrium as a rest point in terms of quantities and prices.<sup>7</sup>

Lucas's (1977) equilibrium model presented a 'representative agent' (for 'helpfulness reasons', Lucas wrote) 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: it is no more the disequilibrium phenomenon *par excellence*, the manifestation of a market failure, but rather the optimizing reaction of agents to outside shocks affecting the economy. 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, p. 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' (Lucas, 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' (*ibid.*).

Assuming the existence of rational agents able to exploit all 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 unmodelled *a priori* and, on the other hand, that 'the crisis' is explained as resulting from a *random error*. In other words, the crisis is an exogenous phenomenon and the economy is 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's approach is a conception of macroeconomics as part of general economic equilibrium analysis of neo-Walrasian type. In particular, Lucas stressed the importance of discussing macroeconomic ideas 'in the language of Arrow, Debreu and McKenzie': this is 'progress', Lucas (2007) maintains. In this perspective, we can fully appreciate the real role of the representative agent fiction: 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' (*ibid.*, p. 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’ (Lucas, 1980, p. 276).<sup>8</sup>

Lucas writes that 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, Lucas himself recognises that 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 econometric sense). The empirical testing of the theory ‘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, p. 408). Lucas’s 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, p. 25), in order to study the effects of different policies on the economic activity.

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

#### *DSGE models. The macroeconometric modeling*

The premise . Returning to the econometric side of the story, from the statistical and structural identification problems (described in sub-section 2.2), which caused the Cowles Commission twofold ‘fallacy’, two antithetical approaches

derived: an a-theoretical and an a-statistical one. They both made use of tools that finally characterized the DSGE theoretically-founded macroeconometrics, but in a completely different perspective.

On the one hand, Sims (1980) reacted to the statistical identification problem by adopting an a-theoretical approach which used data-driven Vector Autoregressive models (VARs) to capture linear interdependencies among multiple time series and to test economically relevant hypotheses without making use of theoretical identification restrictions, i.e. with no explicit microfoundations. He concluded that no variable can be considered exogenous whenever forward-looking agents solve intertemporal optimization models: such restrictions may induce unsound causal inference.

A similar but different route was followed by some econometricians at the London School of Economics (LSE), who focused on the statistical diagnosis of the failure of the Cowles Commission models by concentrating on dynamic specifications and long-run time-series properties (Juselius, 1999). They introduced long-run equilibrium relations in error-correction models, i.e. multiple time-series models accounting for both short- and long-term effects of one time series on another. Since most macroeconomic time series have stochastic trends, i.e. they are  $I(1)$  processes<sup>9</sup>, conventional econometric theory methods do not apply (Nelson and Plosser, 1982). Hence, the structural cointegrated VAR approach arose. This tested both specification and identification restrictions based on the statistical properties of the general dynamic reduced-form models. The assumption of policy exogeneity was strongly criticized, on the ground that the statistical model implied by the structural econometric model clearly omitted both relevant variables and relevant dynamics (Hendry, 1985).



Under this approach, the Lucas's theoretical 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 null hypothesis of exogeneity could not be rejected, and the resulting 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, which describe the reaction of any dynamic system (in this case, relevant macroeconomic variables) in response to some external change (policy instruments).

Finally, 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).

*The theoretical approach to the Cowles Commission fallacy and the consensus.* On their side, the theoretically-grounded econometricians reacted to the structural identification problem by invoking the help of theoretical models, i.e. by identifying the structural parameters of interest via sound microeconomic foundations. The basic notion was a theoretically-founded identification strategy where the effects of economic policies were estimated through theory-based quantitative models, clearly parametrizing the economic fundamentals (tastes and technology). This approach entailed a backward step towards econometric model evaluation, rather than the diagnostic testing and model selection that characterized previous approaches.

However, it soon became clear that a solved DSGE model is nothing more than a structural VAR model (Christiano *et al.*, 1999), and DSGE began to represent a sort of consensus approach to macroeconomic modelling (Favero, 2007). The DSGE methodology was based on standard numerical techniques, but it proved very difficult to evaluate structural parameters by the reduced-form VAR estimation, since VAR parameters are actually complicated convolutions of the structural deep parameters. Hence, the early DSGE models were simple Real Business Cycle models with no frictions and no role for economic policy, based on a limited number of parameters, whose value could be estimated by linearization around the equilibrium state. However, since most models that are based on dynamic stochastic optimization do not have closed-form solutions, i.e. they cannot be solved in terms of functions and mathematical operations from a given information set, brute force estimation by maximum-likelihood techniques was computationally unattractive, since it is based on maximizing the likelihood function of making the observations given the model parameters.

Consequently, calibration techniques spread. These are strategies that use economic theory as the basis for restricting the general modelling framework and mapping it into the measured data, thus finding numerical values for the model parameters in a well-defined model specification (Cooley, 1997). The underlying models implied the heavily unrealistic assumptions described in sec. 2.3, i.e. simple functional forms for tastes and technology, representative agents with homogeneous information sets, and no institutional constraints (see Summers, 1991; Pesaran, 1987; Kirman, 1992; for discussion). Hence, calibration was not aimed at congruently representing real data, but rather at finding the values of the structural parameters that were 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 – that is to say, more variables than equations – the parameters to be calibrated were selected in order to reproduce the preferred stylized facts. 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, p. 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, it was quite evident that all proposed ‘solutions’ to Lucas’s razor were ‘novel forms of manifestation of a very real, but still inadequately elaborated, fundamental problem’ (Lawson, 1995, p. 260), that Romer (2017) labels ‘post-real’ macroeconomics. In other terms, the macroeconom(etr)ics developed to overcome Lucas’s critique was itself subject to a similar razor. To make an illustrative point, it did not model and provide for systemic crises, which do actually happen and affect economic systems with very crude effects: thus, the effectiveness of DSGE-based policy conclusions was severely discussed.

## **A criticism of neo-Walrasian macroeconomics**

### *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 are often indifferent to these criticisms and quite impenetrable to negative evidence: the point is that for them the story has to be told in a coherent DSGE language to be relevant.

At the theoretical level, the soundness of microfoundations has been finally challenged. On discussing the statement that 'macroeconomics is now firmly grounded in the principles of economic theory' (Chari and Kehoe, 2006), Solow (2008) declared that this 'sentence is simply false', 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, like post-Keynesians had done for decades, charged mainstream macroeconomics with the fallacy of composition, i.e. the attribution of properties to a different level from the one where they are observed. Therefore, Solow asked, in what sense is the DSGE model firmly grounded in the principles of economic theory?<sup>10</sup>

Solow emphasized that the direct application of microeconomic reasoning to the aggregate system may be misleading. However, his criticism (and many others') seems not to have affected most mainstream macroeconomists (see Chari and Kehoe, 2008), who preserve, as Solow emphasises, an hegemonic attitude.

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. Neglecting individual interactions is too strict a convenience assumption to provide satisfying descriptions of economic phenomena.

Despite these criticisms, which undermine both theoretical and empirical aspects, the faith of mainstream economists in the theoretical strength of these models has not been hurt by the evidence or by references to actual behaviour. And the prevailing attitude towards the recent crisis confirms this substantial indifference to most contrary evidence, particularly the evidence that cannot be integrated into the DSGE framework. Some 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 are the questions of the soundness of the microfoundations and the relationship between the model and the real world. To answer these questions we will use many of the current criticisms within a critical framework that is able to show how the theoretical beliefs of mainstream macroeconomics are the consequences of the Lucasian project to make macroeconomics a field embedded in neo-Walrasian microeconomics, and the implication of this. These aspects will be discussed from both a theoretical and econometric point of view.

*Mainstream macroeconometrics. Theory-driven econometrics and policy evaluation, or Lucas's razor returns*

The plentiful attempts to shape theoretical model predictions to empirical data reveals a failure in many essential respects. The fundamental question becomes: Did DSGE models truly resist Lucas's critique?

*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>11</sup> that are 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 inherently 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 approach about the super-exogeneity of theoretically-based policy variables can be made of the DSGE approach, when it asserts the invariance of the deep parameters. Hence, many authors (e.g. Bernanke and Mihov, 1995; Christiano, Eichenbaum, and Evans, 1996a, 1996b, 1998) have made great efforts to show how policy actions can be identified by means of empirically-based restrictions, independently of the underlying theoretical models. This has been an attempt to support the highly abstract DSGE models, which are based on heavily unrealistic (but formally convenient) assumptions, by means of empirically-grounded verifications which would overlook the reference theoretical model.

In practice, the important step consists in the identification of some stylized shocks: in fact, DSGE-VARs do operationalize policy shocks into real economies by means of impulse response functions which describe the reaction of relevant macroeconomic variables to policy instruments. Once model parameters have been estimated, experiments on shocks are performed. Keeping the (estimated) parameters constant, the same experiment is reproduced in model economies, and observed and model-based impulse responses are compared by means of proper objective functions that discriminate among alternative theoretical models with distance minimization criteria. Finally, the intertemporal effect of policy shocks is investigated on the basis of the model chosen.

However, because DSGE models are characterized by a huge number of parameters that have to be estimated with sparse empirical data, model likelihoods are generally ill-behaved. This means that resulting likelihoods are highly dimensional objects, characterized by numerous local minima and maxima and by nearly flat surfaces, that hinder optimization algorithms in model evaluation and cause observational equivalence and other identification problems.

Hence, the Bayesian approach and its suitable small sample properties are regarded as being very attractive (Fernandez-Villaverde, 2009). In the Bayesian approach to DSGE, the observational data are treated as given, i.e. they are no longer regarded as the product of some underlying data-generating process. The stylized model is chosen by specifying, apart from the parameter set and the likelihood function, a prior distribution that describes the pre-sample beliefs about the true parameter values. From the Bayes's 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 (e.g., nonstationarity) emerges through this process, priors are changed accordingly. On the contrary, 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.

*The 'Parameters-trap': identification issues, micro-macro inconsistencies, and the structural problem.* Both approaches to DSGE macroeconometrics (VAR and Bayesian) have evident vulnerabilities, which substantially derive from how parameters are handled in the technique. In brief, parameters from formally elegant models are calibrated in order to obtain simulated values that reproduce some stylized fact and/or some empirical data distribution, thus relating the underlying 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 substantial difficulties in taking account of many important mechanisms that actually govern real economies, e.g. institutional constraints like the tax system, thereby reducing DSGE power in policy analysis (Pesaran and Smith, 1995). In the attempt to deal with this serious problem, various parameter constraints on the model policy block are provided. They derive from institutional analysis and reflect policy-makers' operational procedures. However such model extensions, which are intended to reshape **its** predictions to reality and to deal with the underlying optimization problem, prove to be highly inflexible, turning DSGE into a 'straitjacket tool' (Pesaran and Smith, *ibid.*). In particular, the structure imposed on DSGE parameters entails



various identification problems, such as observational equivalence, underidentification, partial and weak identification.<sup>12</sup>

These problems affect both empirical DSGE approaches. Fundamentally, they are ascribable to the likelihoods to estimate. In fact, the range of structural parameters that generate impulse response functions and data distributions fitting very close to the true ones does include model specifications that show very different features and welfare properties.<sup>13</sup> So which is the right model specification (i.e., parameter set) to choose? As a consequence, reasonable estimates do not derive from the informative contents of models and data, but rather from the ancillary restrictions that are necessary to make the likelihoods informative, which are often arbitrary. Thus, after the Lucas's super-exogeneity 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.*, p. 375) on the processes generating the exogenous variables and disturbances. Clearly, such empirical equivalence has fundamental and quite puzzling 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. Canova and Sala (2009) underline that arbitrarily chosen priors may hide severe identification problems,<sup>14</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.*, p. 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, p. 7).

However, 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, which mainly relies on the mechanisms, 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, the Bayes's theorem is applied to 'a set of axioms that decision theorists have proposed to characterize rational behaviour' (Fernandez-Villaverde *et al.*, 2009, p. 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, p. 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 feeding 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 the synthesis of the micro evidence, since a plethora of micro studies often implies 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>15</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 (relating the expected value of a variable to its conditional expected value given the information set) 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' (*ibid.*, p. 13).

*The neo-Walrasian legacy: theoretical attitude and theoretical status.*

*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 founding principle, that

rigour should prevail over realism. It is not a defect in principle, as long as the rigorous unrealistic economic theory is empirically verified, providing valuable policy analysis instruments that can be usefully applied to real macroeconomic problems. But this is not the case, as previous concerns about DSGE econometrics have explored.

This rigour-over-realism prevalence is a crucial methodological legacy of the Neo-Walrasian theory of general economic equilibrium: from John von Neumann (1937) – and then, above all, Gerard Debreu (1950), who represented the radical extension of mathematical formalism in economics – 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.<sup>16,17</sup> This is the essential point.

The main and fundamental implication of this foundational approach is the pre-eminence of the mathematical proof, which is considered the model's main validation, more than any empirical evidence. In other words, evidence is dominated by theory. 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 mathematician-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.

Some critical implications of the theoretical attitude: excess of abstraction and irrelevance. The historical roots of an actual critique. Economists who put their 'faith' in mathematical models often forget the warnings of Francis Y. Edgeworth, Alfred Marshall and Vilfredo Pareto concerning the use of the mathematical engine in economics: the resistance of economic material to pure reduction by means of mathematical treatment, i.e. 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* (1889), in the period of the first spread and consolidation of mathematical economics (Marchionatti, 2007). The controversy reveals the clash of two different methodological requirements. On the one hand was 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 was Edgeworth's requirement of greater realism for the model. He rejected the Walrasian level of abstraction as a representation of the 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. Edgeworth did not accept Walras's rational mechanics reductionism. He emphasised that mathematics has an instrumental use in economics. 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, p. 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: the nature of economic material is what limits the use of mathematics. 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. The crucial issue concerning the role and extent of the use of mathematics in economics reappeared in the work of Vilfredo Pareto (Marchionatti and Gambino, 1997). In the mid-1890s the methodological difference became explicit in Pareto's critique of Walras's *économie sociale*, which he believed to be vitiated by metaphysics. 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, mathematical economics was dominated by the problem of the relation between the model and the real world, and the question of the irrelevance of the theory – an implication of the excess of abstraction - was held to be crucial. In fact,

the opinion was widely shared 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 – hence their irrelevance – rather than its formal aspects. In the 1930s, the Viennese debate at Menger's *Mathematische Kolloquium* (Marchionatti and Mornati, 2016) 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 model 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 emphasised above: this was the epistemological context of contemporary mainstream macroeconomics.

*Theoretical status: unsolved problems and inconsistencies, or pseudo-foundations of macroeconomics.* Since the 1950s, the general equilibrium of neo-Walrasian origin has been considered the fundamental framework for theoretical discourse in neoclassical perspective. Many authors have recently emphasized that, despite its success, this theory 'is not exactly alive and well' (Ackerman, 2002, p. 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 uniqueness, Debreu (1970) demonstrated that it was ensured by certain unrealistic restrictions on the

nature of aggregate demand. As for 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. Saari (1985) showed that any price adjustment process that converges to an equilibrium has infinite information requirements. Kirman and Koch (1986) generalised the SMD theorem, proving 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. Recognizing this failure of General Economic Equilibrium theory, 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' and Saari (1995, p. 229) emphasized the unlimited variety of individual preferences that, when aggregated, 'can generate all imaginable forms of pathological behaviour'. 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, and Fisher and Monz, 1992, specifically on production functions). It makes 'the pursuit of microfoundations for macroeconomics futile' (Ackerman, 2002, p. 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 observed by Arrow (1986), in the aggregate the hypothesis of rational behaviour has in general no implications.

In the DSGE models with a 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.

'the assumption of a representative individual 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, p. 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 more aspects of the real world endogenous to the model. Is the excess of abstraction and the consequent inadequacy of mainstream models surmountable by refining the existing models, in particular by retaining the existing framework of DSGE models but making crucial real facts endogenous to the model? In

general, the strategy of adding new ingredients and parts to the model has to be considered hardly feasible, as many authors have emphasized (for example Caballero 2010, De Grauwe 2010, Stiglitz 2011, Rogers 2013 and 2014). In fact this strategy is plagued by internal inconsistencies:

- the core assumptions – *in primis* the Rational Expectations Hypothesis – become ‘increasingly untenable’ as we continue to add realism into the core;

- the introduction of *ad hoc* assumptions on the behaviours of the agents, not derived by the maximizing behaviour of the agents, in the process of model extension, creates inconsistencies between the assumption of rational agents and the habits to which they are prone. These additional assumptions, as De Grauwe notes, ‘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>18</sup>

- the incorporation of money and banking system into the neo-Walrasian general equilibrium model is an inherently theoretical contradiction. The embrace of Walrasian microfoundations has devastating consequences for monetary theory (Rogers 2013 and 2014; see also Buiter 2002 and 2008). Money and credit have no role in that theoretical context because the Walrasian auction reduces the model to perfect barter, and the Arrow-Debreu extension to a world of complete markets with Arrow securities eliminates uncertainty. As Rogers (2013) writes, to seek the microfoundations of monetary theory in the Walrasian framework creates a dilemma: either the model is logically consistent but with no foundations for monetary theory, or an essential monetary component can be added and logical inconsistency results.

As Stiglitz (2011, p. 593) effectively writes, such Ptolemaic exercises in economics ‘will be no more successful than they were in astronomy in dealing with the facts of the Copernican revolution’. 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. Actually, this set of unsolved (or unsolvable) problems justifies the assertion that mainstream macroeconomics is founded, to use Kirman’s expression, on pseudo-microfoundations.

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

### **Macroeconom(etr)ics**

In the years of the new great economic crisis, mainstream macroeconom(etr)ics seems to reveal a serious failure. This paper has argued that the reasons for this failure, both at the theoretical and empirical levels, reside in the implications of the neo-Walrasian legacy and the problems connected with the practical implementation of that programme. First of all, its theoretical attitude: the prevalence of rigour over realism, i.e. rigorous mathematical formulation versus empirical verification. Then its theoretical status: the aggregation problems and difficulties of adding realism in the model (imperfections introduced *ex-post* but not justified at the theoretical level). Finally the theory-driven econometrics and the empirical validation of the models. The prevalence of rigour over realism entails the risk of ‘excessive abstraction’: a vice which seems intrinsic to

mainstream macroeconomics. Aggregation and methodological problems undermine the microfoundations of macroeconomics, which become pseudo-microfoundations. The question of the model's relationship with the real world and its falsifiability involves serious implications in terms of policy analysis: Lucas's critique becomes a boomeranging razor, which applies to its models as well as to the Cowles Commission's.

It is important to say that the new macroeconom(etr)ists do not actually ignore real data. Rather, they exhibit their own peculiar *Anschauung*: observational data are used to calibrate the inherent 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 by construction, i.e. DSGE calibration practice does generally select sets of parameter values that generate model specifications exhibiting 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, and hence among different policy instruments. Therefore, all conjugations of such identification problem make DSGEs subject to the same criticism that they were conceived to resolve: it is 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 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. Similarly, the new-neoclassical macroeconomics, along the lines of Lucas's anti-Keynesian revolution, has adopted as its basic model an artificial world remote from the reality in which to live, excluding the possibility of any pathology in the working of the market system – a Panglossian view of the market mechanism – and it has failed to account for the actual evolution of the real-world economy. Mainstream macroeconom(etr)ics has taken the 'wrong line' of constructing a macroeconomics aspiring to the precision of microeconomics of perfect rationality – a reductionist programme of research – and as a result it is unable to handle the complexity of the real world. Therefore, paraphrasing Sargent who forty years ago rhetorically asked: 'Is Keynesian Economics a Dead End?', our answer to the question 'Is the neo-Walrasian macroeconom(etr)ics a dead end?' has to be positive. The endeavour by Lucas's (and Sargent'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 could now declare: if you have an interesting story to tell, you can no longer tell it in a DSGE model. To overcome the disappointing outcomes of Lucas's revolution and to handle the complexity of real world we probably need, once again, to return to Keynes and his methodological and theoretical approach to complexity.

## **Acknowledgements**

A earlier version of this paper has been presented at the STOREP Annual Conference (Turin, June 2015). We are very grateful to the participants for their comments and suggestions. In particular we thank Alan Kirman and Cristina Marcuzzo. Moreover, we thank Chris Flinn, Mario Cedrini, Terenzio Cozzi and Jonathan Kirshner for their useful remarks. We also gratefully acknowledge useful comments from three anonymous referees.

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## FOOTNOTES



1. We refer first of all, to Olivier Blanchard (2009) who wrote that “The state of Macro is good” and Michael Woodford (2009, p. 268) who maintained that “there has been a considerable convergence of opinion among macroeconomists over the past ten or fifteen years”. More generally, at the beginning of the new millenium, a general consensus emerged that the fundamental mechanism of macroeconomics was undestood. See the optimistic declaration of Robert Lucas (2003) in his Presidential Address to the American Economic Association and Ben Bernanke (2004) who celebrated the great moderation in economic performance over the previous two decades attributed in large part to the improved economic policy-making resulting from better understanding of how the economy works.

2. In November 18, 2010, at the annual Central Banking Conference, Jean Claude Trichet, then governor of the European Central Bank, expressing what was considered a largely accepted opinion not only among heterodox economists, said that “macromodels failed to predict the crisis and seemed incapable of explaining what was happening in the economy in a convincing matter” – quoted again in Trichet (2013, p. 244). Specifically he was referred to Caballero (2010). A bit paradoxically it was Lucas (2009) to first recognise that the neoclassical model was unable to predict the crisis. In fact, since 2009 a great number of papers and books referred to the interpretative and predictable limited ability of the contemporary neoclassical model: for example, Leijonhufvud (2009), Buitier (2009) Krugman (2009), Caballero (2010), Taylor (2010), Stiglitz (2011), not to say all those books which accompanied the Keynes’s resurgence, in particular Clarke (2009), Skidelski (2009), Davidson (2009), Backhouse and Bateman (2011). Rogers (2013 and 2014) is a good example of the further in-depth analysis of the shortcomings of the contemporary neoclassical macroeconomics.

3. Agents are representative households, which maximize their utility under an intertemporal budget constraint, and firms, which maximize profits over time. The economy is affected by different types of exogenous shocks. 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.

4. Reduced forms are obtained by solving the system of structural equations for the endogenous variables. I.e., being  $Y$  the vector of endogenous variables to be explained by a statistical model,  $X$  the vector of explanatory exogenous variables and  $e$  a vector of error terms, the structural form is  $f(Y,X,e)=0$  and the reduced form is  $Y=g(X,e)$ , with  $f$  and  $g$  functions.

5. Lucas (1980) 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. He maintained that 'the idea that an economic system in equilibrium is in any sense at rest is simply an anachronism'.

6. As it is well known, the assumption asserts that individuals use their information in an efficient way, without systematic errors, in the formation of their expectations. 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. However, Muth himself argued that rational expectations would fail if people's 'errors' were to be correlated. So people could have different expectations but their differences should cancel out and if they do not, think of herd behaviour, the Rational Expectations Hypothesis is in trouble (We are ourselves indebted to Alan Kirman for this latter valuable point).

7. This makes it possible to use the maximization postulate to analyze a world which is continually buffeted by shocks.

8. This means that the theory does not live exclusively in a hypothetical world like Debreu's world of general economic equilibrium. Macroeconomic models must reach practical conclusions.

9. The order of integration  $I(d)$  is the minimum number of differences required to obtain a covariance stationary time series. A collection of time series is said to be cointegrated if their linear combination is  $I(0)$ .

10. 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; Kirman, 1992).

11. 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 pre-rational expectations. 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 the agents' beliefs is the same as the true data-generating process, hence justifying GMM estimation on moments derived from the Euler equations.

12. Identification problems arise whenever it is not possible to identify the best estimate of one or more model parameters. In particular, observational equivalence

occurs when more than a single parameter set generates the same observed distributions.

13. This is due to the ill-behaved mapping between the structural parameters and the coefficients of the solution, which means that the model transition laws are relatively insensitive to changes in many parameter values. See Canova and Sala (2009) for an extensive analysis of the standard setting of a unique solution to the problem of the representative agent under rational expectations.

14. 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 prior and the posterior do not guarantee parameter identification.

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

16. This theoretical attitude originated in the reaction to Marshallian economics in the 1920s and 1930s (Marchionatti 2003), and greatly influenced post-war mainstream economics, giving origin to what Weintraub (1985) called the 'neo-Walrasian research programme' (see also Backhouse, 1995). But its strong influence in macroeconomics dates from the 1970s.

17. The concept of rigor we refer is that introduced in the Hilbert – Menger (Karl) – VonNeumann – Debreu (and Arrow) tradition (see Marchionatti and Mornati 2016). Its peculiarity is to consider rigor entirely divorced from empirical corroboration. In this construction, Karl Menger can be considered the point of departure in economics. His 1936

paper was the first instance in economics of a clear separation between the question of logical interrelations among various propositions and the question of empirical validity. According to Menger, it was the key point needed to transform economics into a science. This “clear separation” between the question of logic and the question of empirical validity, was described by Schumpeter (1954, p. 1037) as “a shining example of the general tendency towards increased rigor that is an important characteristic of the economics of our own period”. Menger’s paper was at the basis of Wald’s work on the existence and uniqueness of GEE equation solutions, and of the programme for the new mathematization of Walrasian general economic equilibrium theory. The axiomatic approach in economics was applied in von Neumann’s 1937 paper in a “totally coherent way”, in the sense that the concern for the economic interpretation of the model – still existing in Wald (1936) - disappears. This theoretical attitude derived, firstly, from the fact that von Neumann dealt with the economic question as a mathematician: in this way he obtained a mathematical solution of (to quote von Neumann himself) a “highly generalised problem in theoretical economics” characterized by “the elegance of its solution, logical completeness, concision and rigor”, but he had to adopt “extremely artificial assumptions” or “idealizations” as von Neumann termed them. Secondly, von Neumann’s attitude derived from the fact that he dealt with theoretical economic problems like a *formalist mathematician* – i.e. he conceived the model as a formal structure whose legitimacy and cogency depend on its internal consistency. The radical extension of formalism in economics definitively affirmed with Debreu (1959) and the Arrow-Debreu model of the GEE. As Weintraub (2002) wrote: “From Hilbert to von Neumann, to the *Mengerkries* and Wald, to Bourbaki and thence to Debreu runs the chain of causality, the development of modern economic theory in its unconcern to study real economies”. Debreu effected the complete shift to Hilbert’s definition of mathematical rigor: ‘demand’, ‘supply’ and ‘equilibrium’ became nothing more referentially real than letter sequences. As a consequence it emerge the trade-off of rigor and relevance: “Allegiance to rigor dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations”, states

Debreu (1959), adding that such a dichotomy between the theory in the strict sense and its interpretation “reveals all the assumptions and the logical structure of the analysis” and “makes possible immediate extensions of that analysis without modification of the theory by simple reinterpretations of the concepts” (p. x). This implies that its actual aim is not realism but understanding the implications of axioms and assumptions for the results. This is Lucas’ theoretical tradition and within it has to be considered the concept of rigor by him used (and implicitly assumed by neo-Walrasian macroeconomists).

18. 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 described by Palley (2013) as ‘gattopardo economics’.

19. This idea is developed in Marchionatti 2010.