

# Not Going Away: Representative-Agent Models and Microfoundations in the Making of a New Consensus in Macroeconomics<sup>1</sup>

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## *ABSTRACT:*

Macroeconomics, or the science of economic aggregate fluctuations, has always been portrayed as a field composed by competing schools and in a somewhat recurrent state of disarray. Nowadays, macroeconomists are proud to announce that a new synthesis is what characterizes their field: there are no longer fights and disarray, but rather convergence and progress. I want to discuss how modern macroeconomists see the emergence of such consensus and, therefore, how they see the history of their sub-discipline. In particular, I shall stress the role played in the making of such consensus by both the hypothesis of a representative agent and by a particular understanding of the microfoundations that macroeconomics need. I then use De Vroey's (2004) Marshall-Walras divide to analyze further the new neoclassical synthesis.

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## Not Going Away: Representative-Agent Models and Microfoundations in the Making of a New Consensus in Macroeconomics

*Macroeconomics is now, as it has always been, the subject of intense controversy.* Robert Solow (1979, 340)

*When the outside world tells us that no two economists ever agree on anything, it is talking about macroeconomics.* Stanley Fischer (1983, 275)

*While macroeconomics is often thought as a deeply divided field, with less of a shared core and correspondingly less cumulative progress than in other areas of economics, in fact, there are fewer fundamental disagreements among macroeconomists now than in the past decades. This is due to important progress in resolving seemingly intractable debates.* Michael Woodford (2009, 267)

### Introduction

Macroeconomics here, according to the economists discussed in this essay, refers to “the branch of economics concerned with fluctuations in the overall level of business activity, with the determinants of inflation, interest rates, and exchange rates, and with the effects of government policies ... that are considered mainly with regard to their effects upon the economy as a whole” (Woodford 2000, 1). Therefore, the focus is on the theory of economic stabilization (with greater emphasis on monetary economics) rather than of growth, although macroeconomists now claim to have a single model used both for short and long-run analyses.

In contrast to economists working in other areas of economics, macroeconomists perceive their field to be not only composed of competing schools of thought but also characterized by a somewhat recurrent state of disarray.<sup>2</sup> For instance, Edmund Phelps (1990) identified seven schools of macroeconomic thought: the macroeconomics of Keynes, monetarism, the new classical school, the new Keynesians, supply-side macroeconomists, neoclassical and neo-neoclassical real business cycle theory (RBC), and the structuralist school. Brian Snowdon, Robert Vane, and Peter Wynarczyk (1994) identified the same number of schools (or paradigms, as macroeconomists, we shall see, seem to appreciate Thomas Kuhn’s 1970 concepts).<sup>3</sup> Perhaps some would add the post Keynesian and the Austrian schools.<sup>4</sup>

<sup>2</sup> Here I borrow William Nordhaus’s (1983, 247) term used later by Karl Brunner 1989 and N. Gregory Mankiw 1992 right on the title of their papers (the latter paper draws heavily on Mankiw 1990).

<sup>3</sup> Kevin Hoover (1988, chap. 1) places the new classical economists “among the principal schools of macroeconomic thought” (6). Howard Vane and John Thompson (1992) kept it simple: just three “mainstream schools of thought” (keynesian economics, monetarism, and new classical economics). There are too many articles by macroeconomists in which several schools of thought are appraised. For the most recent ones, many of the papers I shall cite follow this tradition: see in particular Goodfriend and King (1997), Taylor (1997), Woodford (2000), Mankiw (2006), and Akerlof (2007).

<sup>4</sup> Would either the Marxist or the institutionalist theories be included as a school of macroeconomic thought? Sheila Dow (1996, chap. 4) did include them (and others).

As for the understanding that the history of other areas of economics, in contrast to that of macroeconomics, is a steady progress within an unchanged explanatory framework, here I quote at length Michael Woodford's (2000, 2) account of the evolution of the fields of macro and microeconomics:

A discussion of the century's progress in general economic theory—with primary emphasis upon what is taught in courses on “microeconomic theory”, which emphasize the decisions of individual households and firms—would surely be more suitable if my aim were to boost the prestige of my own field among the many distinguished representatives of other disciplines present here. But the story would be one with little suspense. For it would not be too much of an oversimplification to present the field as having progressed smoothly and steadily, developing theories of ever greater power and broader scope within an essentially unchanged explanatory framework, based on the concepts of optimizing individual behavior and market equilibrium, that were already central to economic thought in the previous century. Macroeconomics instead has been famously controversial.... Discussions of twentieth-century developments in macroeconomics make frequent references to “revolutions” and “counter-revolutions”, and the question of whether there has been progress at all (or which broad developments should count as progress) is a more lively topic of debate among economists than one might believe would be possible in the case of a topic with such a canonical status in the curriculum.

We then come to another central element of the macroeconomists' comprehension about the nature and evolution of their field: macroeconomics has not only several competing schools and from time to time is in a state of disarray, but it also has moments of consensus when knowledge seems to progress at a faster rate: the two consensus so far are the neoclassical synthesis of the 1950s and 1960s and the recent new consensus (from the late 1990s to the present day), labelled as the new neoclassical synthesis by Marvin Goodfriend and Robert King (1997). During such a synthesis, the intellectual disarray and the untamed competition among schools—both with respect to macroeconomic theories and of policies—are replaced by balanced conversations, points of convergence and scientific progress.

The concern with progress and secure knowledge is pervasive among macroeconomists. On the one hand, in the ivory tower of academia, having schools competing in a state of disarray is a synonym for weak science. Recurring to Kuhn (1970), when there is no dominant paradigm, there is no normal science: macroeconomists have an epistemological fear, that the scientific foundation of their studies are weak or absent if they are always in a state of intellectual disorder.<sup>5</sup> Additionally, their scientific and academic prestige among both economists in general and other scientists could be boosted if they had a story of steady progress and secure knowledge to tell (see

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<sup>5</sup> As Robert Solow (1983, 279) wrote: “Why ... is macroeconomics in disarray? ‘Disarray’ is an understatement. Thoughtful people in other university departments look on with wonder. Professional disagreements exist in their field too—at the frontier there is always disagreement—but as outsiders they are shocked at the way alternative schools of thought in macroeconomics describe each other as wrong from the ground up. They wonder what kind of subject economics is. (Some of them are not above a little *Schadenfreude* either.)”

Woodford's quotation above). In this respect, macroeconomists working with a unified framework is what one needs. On the other hand, policymakers keep asking macroeconomists what theory should guide policy, and intellectual disarray is not a good sign here either. Macroeconomists can give a convincing answer as long as they are able to show that there is a core of usable macroeconomics that they all believe in (to use the theme of a session at the AEA meetings of 1997).<sup>6</sup> In this sense, it is symptomatic that Frederic Mishkin (2007), who was member of the Board of Governors of the Federal Reserve System from 2006 to 2008, argued that the major advances in monetary economics have made monetary policy to become more of a science. Mishkin's opinion was first a hope with which Goodfriend and King (1997) closed their article. It is also shared to some degree by V. V. Chari and Patrick Kehoe (2006), by Goodfriend (2007), and by Jordi Galí and Mark Gertler (2007).

In the new wave of consensus in macroeconomics (the new neoclassical synthesis), macroeconomists are given a great emphasis to the progress reached. For them, in essence, there is no Kuhnian substitution of one paradigm by another via revolutions, but rather a merging of previously rival paradigms and a "steady accumulation of knowledge" (Blanchard 2000, 1375). How is that possible? "Largely because facts have a way of not going away" (Blanchard 2008, 2, paraphrasing his earlier sentence "the force of facts is hard to avoid" (Blanchard 1997a, 245)). Said in another way, "to some extent, this is because positions that were vigorously defended in the past have had to be conceded in the face of further argument and experience" (Woodford 2009, 268). Therefore, facts and arguments made economists from different camps develop "a largely shared vision both of fluctuations and of methodology" (Blanchard 2008, 2).<sup>7</sup> In a nutshell, both in theory and practice, advertising a consensus means that macroeconomists now agree on the right way of doing macroeconomics—and it is right because it is generally accepted, after facts and arguments have refuted wrong theories.<sup>8</sup> Knowing how to do is the prerequisite for doing it right and thus for increasing the stock of knowledge.

My goal in this short essay is to discuss how modern macroeconomists see this consensus that emerged in their field, which is intimately related to how they see the history of their sub-discipline. In particular, I shall stress the role played in the making of such consensus by both the

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<sup>6</sup> The participants of this session included Robert Solow, John Taylor, Martin Eichenbaum, Alan Blinder, and Olivier Blanchard, whose essays were published in the Papers and Proceedings issue of the *American Economic Review* of 1997 (vol. 87, no. 2).

<sup>7</sup> Mankiw 2006, 38 states that paralleling the emergence of the new consensus there was the retirement of an older generation of vitriolic macroeconomists and its replacement by "a younger generation of macroeconomists who have adopted a culture of greater civility." You may question Mankiw's view and have trouble identifying this new generation by reading for example the response that Chari and Kehoe gave to Solow's comments on their 2006 article (Chari and Kehoe 2008).

<sup>8</sup> Blanchard 2008, 5 once again exemplifies this understanding among present-day macroeconomists: "Facts have a way of eventually forcing irrelevant theory out (one wishes it happened faster). And good theory also has a way of eventually forcing bad theory out."

hypothesis of a representative agent and by a particular understanding of the microfoundations that macroeconomics needs.<sup>9</sup> I then revisit Michel De Vroey (2004) proposal to see the history of macroeconomics against what he calls the Marshall-Walras divide in order to analyze further the new neoclassical synthesis and raise a few questions about this framework.

## 1. Trading in the Triangle: A New Consensus in Macroeconomics

For many followers of the new neoclassical synthesis the existence of a consensus in their field does not mean that room for disagreement no longer exists (Goodfriend 2007, 3; Blanchard 2008, 2; Woodford 2009, section III; Chari, Kehoe, and McGrattan 2009). After all, as Solow (1983, 279) pointed out, there is always disagreement at the frontier. Nonetheless, they all agree that modern macroeconomics, in theory and in practice, when compared with the 1970s, has changed substantially and for the better:

Over the last three decades, macroeconomic theory and the practice of macroeconomics by economists have changed significantly—for the better. Macroeconomics is now firmly grounded in the principles of economic theory. These advances have not been restricted to the ivory tower. Over the last several decades, the United States and other countries have undertaken a variety of policy changes that are precisely what macroeconomic theory of the last 30 years suggests.

Chari and Kehoe 2006, 3.<sup>10</sup>

In the 1970s and the 1980s, continue those economists, macroeconomics “looked like a battlefield” with “researchers split in different directions, mostly ignoring each other, or else engaging in bitter fights and controversies” (Blanchard 2008, 2): the neoclassical synthesis had broken down and over time monetarists, new classical economists, real business cycle theorists and new Keynesians fought against one another and disagreed on many issues. Macroeconomists see the new synthesis as a bridge between two broad fields, the classical (which incorporates monetarist ideas and is composed of the new classical and real business cycle theorists) and the Keynesian (basically the new Keynesians and the Keynesians of the 1970s associated with the large-scale econometric models).<sup>11</sup> As Woodford (2009, 268) argues, while in the 1970s and 1980s there were “fundamental disagreements among leading macroeconomists about what kind of questions one might reasonably seek to answer or what kinds of theoretical analyses or empirical studies should even be admitted as contributions to knowledge,” nowadays these disagreements and questionings no longer exist.

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<sup>9</sup> I discuss the economists’ perception of the evolution of their field without taking issues with some of their claims. I want to explore the diversity that still exists on their understandings despite the alleged convergence that currently characterizes the field.

<sup>10</sup> Goodfriend (2004, 21) and Galí and Gertler (2007, 25) share with Chari and Kehoe their enthusiasm about the state of macroeconomics.

<sup>11</sup> See for example Mankiw (1989, 79), Goodfriend and King (1997, 232) and Woodford (2000, 29).

Before discussing how macroeconomists characterize the new synthesis I would like to stress briefly how different were the new classical and RBC research programs on the one hand, and the new Keynesian on the other.

### 1.1 Battling Macroeconomics

The Keynesian orthodoxy of the 1950s and 1960s, with the then ubiquitous IS-LM model, was shattered in the 1970s—to use Hoover’s (1988, 3) words.<sup>12</sup> On the theoretical side, weak microfoundations increasingly made professional economists unhappy with this Keynesianism. On the practical side, the stagflation of the 1970s made economists question the stability of the Keynesian device to incorporate inflation into their IS-LM framework: the Phillips curve (Hoover 1988, chap. 1). Milton Friedman (1968) and Edmund Phelps (1967) criticized the Phillips curve for ignoring the long-run neutrality of money and for not incorporating expectations (De Vroey and Hoover 2004, 7).<sup>13</sup>

Robert Lucas (1976) criticized the use of reduced-form econometric models to policy evaluation (i.e., simulate paths of endogenous variables under alternative economic policies): the parameters of estimated aggregate relationships are themselves function of deeper preference and technology parameters, and such function changes when the government adopts a new policy. New classical, RBC and new Keynesian economists all worked in a similar fashion to address the Lucas’s critique, by providing the kind of microfoundations that nowadays characterizes not only their research programs but also the models in the new consensus macroeconomics.<sup>14</sup> These economists used the hypothesis of a representative agent as part of their answer to the Lucas’s critique.

During the 1970s, the 1980s and early 1990s new classicals, real business cycle theorists and new Keynesians were in a battlefield. They had important points of disagreement, but they also shared some methodological and theoretical elements. To some degree, famously with respect to policy implications, the new classical and real business cycle macroeconomists, on the one hand, and the new Keynesians on the other seemed to criticize each other from the ground up—to repeat Solow’s (1983) words.

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<sup>12</sup> Blanchard (2008, 2) referred to the crisis in macroeconomics in the 1970s in more histrionic terms, as the “explosion (in both the positive and negative meaning of the world) of the field in the 1970s.”

<sup>13</sup> The point here is not to draw a complete scenario of the evolution of macroeconomics in the postwar period, which should include the general disequilibrium theory of the 1960s and 1970s (which I do not consider to be the first wave of the new Keynesian work as argued for by Mankiw 2006, 35), and many other issues. I want just to give a rough sense how macroeconomists see the major changes in their field.

<sup>14</sup> The call for microfoundations is not a result of the Lucas’s critique. For example, as early as 1946, Lawrence Klein (1946, 93) expressed his concern about the loose relationship of the business cycle mathematical models and individual behavior (consumers and producers) “which must form the basis of all theories of economic behavior.”

The new classical and RBC followers both worked in a framework with three basic tenets that Hoover (1988, 13-14) used to describe the new classicism. First, agents choose real variables based solely on real, instead of nominal or monetary, factors. Second, agents are continuously in equilibrium because, “to the limits of their information,” they are “consistent and successful optimizers” (14). Third, agents have rational expectations, i.e., they “make no *systematic* errors in evaluating the economic environment” (14). Three other basic tenets should be added to this set: the economy works in a perfect competition environment in which prices are flexible to adjust and clear all markets. Moreover, the new classical/RBC equilibrium intertemporal model of the business cycle that emerged from these hypotheses identifies either real shocks or errors in expectations as the sources of fluctuations.

Over time, several of the models in this tradition generated five neutrality results, as Akerlof (2007, 6) concisely discussed: (a) the independence of consumption and current income through the life-cycle permanent income hypothesis (i.e., a denial of the Keynesian consumption function); (b) the irrelevance of current profits to investment spending through the Modigliani-Miller theorem; (c) the natural rate theory of unemployment that states that the long-run inflation and unemployment are independent (i.e., the long-run Phillips curve is vertical);<sup>15</sup> (d) the inability of monetary policy to stabilize output as a consequence of the rational expectations hypothesis (economic policies can not systematically affect real output, a result known as the policy irrelevance; therefore, actual output is systematically equal to its potential level); and (e) the irrelevance of taxes and budget deficits to consumption (Ricardian equivalence). These neutrality results went against many results either from the Keynesian orthodoxy of the postwar period or, more importantly, from the new Keynesian camp that I shall discuss later.

Although it is sometimes convenient to group together the new classical and the real business cycle macroeconomists—a group to be contrasted to the new Keynesians in this narrative—, there are three important differences between these two approaches. First, in their origins these groups had different methods of bringing their model to the data. New classical economists developed not only solution methods to their model but also estimated them.<sup>16</sup> In contrast, RBC theorists became known for their calibration method and disapproval of econometrics. They generally calibrate the parameters of their models based on equations evaluated at steady-state: the steady-state variables are substituted by their sample means (or other moments) in the data, and the equations are solved for the unknown parameters. After calibrating the parameters, they test the model’s ability to reproduce empirical facts as co-

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<sup>15</sup> In several of rational expectation models the Phillips curve is also vertical in the short run.

<sup>16</sup> See Lucas (1973) and several articles in the volume Lucas and Sargent edited in 1981.

movements, variances and means of aggregate variables in the data (real output, consumption, hours worked, and so on).<sup>17</sup>

A second distinction between new classical and RBC theorists is the inexistence of money in most of the RBC models and, thus, a focus on the real sources of fluctuations. Therefore, with these models real business cycle macroeconomists could in no way talk about monetary policy and inflation stabilization.<sup>18</sup> In contrast, new classical thinkers did have unsystematic monetary shocks in their equilibrium models of the business cycle and talked about “purely monetary cycles” and monetary policy (Lucas 1975).

A third difference is the existence of imperfect information in the earlier new classical models and its inexistence in the first RBC models. Lucas’s islands (following Phelps 1969, introductory chapter) work as a mechanism propagating the shocks and thus generating serially correlated movements in real output, for instance.

As for the new Keynesians, they wanted to study monetary economies in which nominal rigidities and market failures that make fluctuations costly and therefore open the door for stabilization policies. The typical environment to get all these elements in place is one of imperfect competition. Moreover, in contrast mostly to the RBC theorists, the new Keynesians identified monetary shocks as the major source of economic fluctuations, and against both the new classical and RBC economists they argued that economic (monetary) policy can affect real output at least in the short run (not as surprise shocks). They brought back to macroeconomics unemployment and the non-neutrality of money in the short run. In terms of empirical research, the new Keynesians favored estimation (often equation-by-equation) techniques.

Mankiw (1989, 79) does summarize very well in the first paragraphs of his article the major differences between the “classical school” (especially the RBC theory, but with some elements in common with the new classical) and the “Keynesian school” (the new Keynesians):

The debate over the source and propagation of economic fluctuations rages as fiercely today as it did 50 years ago in the aftermath of Keynes’s *The General Theory* and in the midst of the Great Depression. Today, as then, there are two schools of thought. The classical school emphasizes the optimization of private economic actors, the adjustment of relative prices to equate supply and demand, and the efficiency of unfettered markets. The Keynesian school believes that understanding economic fluctuations requires not just studying the intricacies of general equilibrium, but also appreciating the possibility of market failure on a grand scale.

Real business cycle theory is the latest incarnation of the classical view of economic fluctuations. It assumes that there are large random fluctuations in the rate of technological change. In response to these fluctuations, individuals rationally alter their levels of labor supply and consumption. The business cycle is,

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<sup>17</sup> See, for example, Thomas Cooley and Edward Prescott’s chapter in Cooley (1995).

<sup>18</sup> Nonetheless, in the early 1990s these economists expanded their agenda to include things like money, heterogeneous agents, and imperfect competition, for instance (see Cooley 1995).

according to this theory, the natural and efficient response of the economy to changes in the available production technology.

Mankiw then states that he does not believe that real business cycle theory offers an “empirically plausible explanation of economic fluctuations” (79).

Despite the major points of disagreement among the new classical and RBC macroeconomists, on the one hand, and the new Keynesians on the other, these two camps shared significant methodological and theoretical grounds: they all adopt the rational expectations hypothesis, favor general equilibrium models, and have in the benchmark models a representative agent in an environment of complete markets. It is true that, for instance, the new Keynesians George Akerlof and Janet Yellen (1985) introduced small deviations from rationality at the individual level and that Mankiw (1985) made a static partial equilibrium analysis. Nonetheless, rational expectations were, for the former, the benchmark from which to deviate, and the latter explicitly mentioned that it was possible to construct simple general equilibrium models in which his results would not only hold but be more pronounced (536). This central elements common to both the new classical/RBC and the new Keynesian camps allowed them to trade and communicate and to resolve most of the previously listed points of disagreements and thus turn them into core features of the new consensus macroeconomics.

## 1.2 Synthesizing Macroeconomics

Goodfriend and King coined the term new neoclassical synthesis in 1997 when such synthesis was still on the way: “macroeconomics is moving toward a *New Neoclassical Synthesis*” (231). According to them, the main features of this new consensus macroeconomics are methodological: “the systematic application of intertemporal optimization and rational expectations.” But the synthesis also “embodies the insights of monetarists ... regarding the theory and practice of monetary policy” (232).<sup>19</sup>

In that same year Solow, Taylor, Eichenbaum, Blinder, and Blanchard all tried to answer the question “is there a core of usable macroeconomics we should all believe in?”, which was the theme of a session at that year’s AEA meeting—with a clear emphasis on how models come to practice. Robert Solow (1997, 230) advocated that part of the common core of macroeconomics consists of: (a) “trend movement is predominantly driven by the supply side of the economy (the supply of factors of production and total factor productivity)” and that it is best analyzed “in some sort of growth model, preferably mine”; (b) fluctuations around the trend “are predominantly

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<sup>19</sup> According to these authors, the new synthesis “inherits the spirit of the old, in that it combines Keynesian and classical elements” (232). The old was characterized by three principles, they argue: (i) give practical macro policy advice; (ii) short-run price stickiness is the major source of economic fluctuations; (iii) macro models need microfoundations.

driven by aggregate demand impulses” best studied with “some model of the various sources of expenditure”. Solow recognizes that there is some dissent from proposition (b). He explicitly denies the RBC explanation of fluctuations, as supply-driven, and then commends the “flexible, observant members of the real-business-cycle school, like Martin Eichenbaum and his coworkers” for opening up “the fabric of their underlying model so that it will allow—or insist—that demand-side impulses play the dominant role in the short-run macroeconomic fluctuations. Then this proposition is indeed part of the usable core of macroeconomics” and economists can thus argue the best way of modeling such demand-side forces (230). Solow also voices his disapproval of assuming rational expectations in the modeling of short-run equilibrium: “I can see a role for rational expectations in the modeling of long-run equilibrium. In the short-run part of macroeconomics, the rational expectations hypothesis seems to have little to recommend” (231). He then recognizes that his core of macroeconomics lacks of “real coupling between the short-run picture and the long-run picture” (231-2).

John Taylor (1997) defined macro as studying both economic growth and fluctuations and identified a practical core in this field because “it is having beneficial effect on macroeconomic policy, especially monetary economics” (233). He then lists five key principles of such core: (1) long-run growth depends on movements along as well as shifts of a production function (which corresponds to Solow’s proposition (a)); (2) there is no long-run trade-off between inflation and unemployment; (3) there is a short-run trade-off between inflation and unemployment (rationalized either by new Keynesian sticky prices or by asymmetric information *à la* Lucas); (4) expectations matter because they are highly responsive to economic policy—he then states that the rational expectation approach is “the most feasible empirical way to model this response” (234); and (5) evaluating monetary and fiscal policy requires thinking in terms of “a series of changes [in instruments] linked by a systematic process or a policy rule” (234).

Alan Blinder (1997) follows basically the line of Taylor and emphasizes how useful such core is in terms of policy analysis, “where contact with reality is a necessity” (240). Martin Eichenbaum (1997) approaches the question of the existence of a core in macroeconomics from the perspective of stabilization policy. He then stresses that macroeconomists converge mostly in terms of method, but also on principles like the ones listed by Taylor: (1) “monetary policy is neutral in the long run”; (2) “*persistent* inflation is always a monetary phenomenon”; (3) “monetary policy is not neutral in the short run”; (4) “most aggregate economic fluctuations are not due to monetary policy shocks” (236). All these points are at the core of the new synthesis, as discussed later. Finally, Blanchard (1997a) identifies two propositions only: (1) short-run movements in economic activity are driven by aggregate demand; (2) “over time, the economy tends to return to a steady-state growth path” (244).

What I want to underline is that although many macroeconomists wanted to answer with an “unambiguous yes” the question of the AEA session (Blanchard 1997a, 244), there were still important differences in the key elements of the new consensus, as shown before. Nevertheless, the majority agreed on the methodological elements—dynamic stochastic general equilibrium models (DSGE) with rational expectations (and a representative agent)—and with most of the key principles, especially with short-run fluctuations being demand driven and that real disturbances are often inefficient, with the degree of inefficiency being a function of the response of the monetary shock to such disturbances. Thus, monetary policy is non-neutral in the short run as consequence of some sort of nominal rigidity. Clearly, the original RBC models could not join this discussion and give policy recommendations because they had no role for money and treated short-run fluctuations as optimal supply-side adjustments that occur in an environment of flexible prices in which all markets clear.

However, Solow was a dissonant voice in this group of people. The reason was his insistence on, first, using different models for analyzing short and long-run movements in economic activity and, second, on rejecting the rational expectations hypothesis for short-run analysis. Later, when he commented on Chari and Kehoe’s 2006 article and repeated his longstanding criticism to the use of a representative agent in macroeconomics, his comments were not well received by these authors (Solow 2008).

By the turn of the century the new synthesis was more clearly a reality. Its main advantage is, according to many macroeconomists, “to bridge the methodological divide between microeconomics and macroeconomics, by using the tools of general equilibrium theory to model Keynesian insights,” in Woodford’s (2000, 29) words. The central point of convergence in the new synthesis was methodological: the use of a single structural general equilibrium model that explains not only the evolution of the potential output over time as a supply-side phenomenon, but also short-run deviations of the actual output from its “natural” level (the level achieved if prices were flexible) as a consequence of wages and prices being not fully flexible.

Another very important methodological point of convergence is the empirical approach macroeconomists now use. In the 1980s and early 1990s the real-business-cycle theorists and the new Keynesians were in opposite camps: the former defended calibration methods and were against econometric estimation while the latter favored estimation methods.<sup>20</sup> In the new synthesis no such divide exists. As Chari and Kehoe (2008, 248) aptly described, modern macroeconomists have now a “big-tent approach to data analysis” through which they “confront both the micro aspects and the macro implications of general equilibrium models with data.”

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<sup>20</sup> With hindsight, Woodford (2000, 27) commented that “the rejection of traditional econometric methods by the early RBC literature has surely been overdone.”

Today not only there is no opposition between calibration and estimation (of a system of equations) but both strategies are complementarily used: the current practice is to calibrate a subset of the parameters (those that economists feel they have more information about) and to estimate the remaining parameters via likelihood or Bayesian methods.<sup>21</sup>

The other points of disagreement between RBC and new Keynesian economists already mentioned were real versus nominal shocks as sources of fluctuations, the irrelevance or not of monetary policy, and the assumption of flexible or sticky prices and wages (perfect versus imperfect competition). The current consensus view is that real shocks account more for the variability of major macroeconomic variables than monetary shocks: for example, Altig *et al.* (2005) show that about 28 percent of each of the variances of output, inflation and average hours worked is explained by real shocks in their model, while only 14 percent of each of these variances can be attributed to monetary policy shocks. These numbers can vary from model to model as there are still issues about how to properly identify shocks but they give the general picture that real shocks are more important in this dimension.<sup>22</sup> Nonetheless, this result does not mean that is irrelevant in explaining economic fluctuations, as stressed by Woodford (2009, 11): not only the existence, unicity and stability of equilibrium depend on the policies designed, but also “the equilibrium effects of real disturbances depends substantially on the character of *systematic* monetary policy.” Therefore, monetary policy design should be part of a stabilization program.

Monetary shocks have well documented effects on real variables in the short run (Christiano, Eichenbaum, and Evans 1999). While they do not explain much of the variability of major real variables, they still are very useful as an empirical test to discriminate among alternative models: nowadays it is common to have papers introducing all kinds of rigidities and smooth dynamics in order to be able to reproduce the effects of a monetary policy shock identified in the data—so it is not properly an empirical test but rather a reverse engineering strategy to account for features of the data. Lastly, monetary policy does have real effects thanks to the existence of nominal rigidities. Therefore, all previous issues that divided macroeconomists in the past are now part of a consensus to discuss stabilization policies, as summarized by Eichenbaum (1997)—there is yet a very important element of the new synthesis: the concern with credibility and discussions about commitment inherited from the real-business-cycle literature.

The set of solid macroeconomic principles that characterize the new synthesis gave economists confidence to apply their models, which are immune to the Lucas’s critique, to policy

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<sup>21</sup> See Christiano, Eichenbaum, and Evans (2005, 15-17) for a description of the econometric methodology of calibrating a subset of parameters and estimating the others by minimizing the distance between the impulse response functions from the data and the model.

<sup>22</sup> See Dupor, Han, and Tsai (2009), Canova and Sala (2009), and references therein.

analysis, especially to monetary policy. The motivation of Woodford in his 2003 book is exactly to show how economists can now discuss monetary policy in practice by drawing from theoretical principles of the new consensus macroeconomics. Frank Smets and Raf Wouters (2003, 2007) and Christiano, Eichenbaum, and Evans (2005) present the clearest incarnations of quantitative general equilibrium models oriented to policy application typical of the new synthesis.

As mentioned in the introduction, advertizing a consensus in macroeconomics—a field seen as composed by rival schools— has the advantage of making the case for both academic cohesion and scientific progress, and for providing a unified body of knowledge from which to prescribe policies. In his comments to Goodfriend and King’s 1997 article, Blanchard (1997b) questioned both the labels “new” and “synthesis” on the ground that the principles behind it were always part of macroeconomics—later he explicitly elaborated on his view that the history of macroeconomics in the twentieth century is not a “series of battles, revolutions and counterrevolutions,” which suggests that the field start anew “every twenty years or so” and has “little or no common core,” but it is rather a history of “a surprisingly steady accumulation of knowledge” (Blanchard 2000, 1375).<sup>23</sup> According to the author, macroeconomists differ by the weights they attach to the different ingredients in their models, but they live in the same world, which he characterizes by a triangle:

Think of a triangle. At the top is the Ramsey-Prescott model, with its emphasis on intertemporal choice. At the bottom left is the Taylor model, with its emphasis on nominal rigidities. At the bottom right is the Akerlof-Yellen model, which its focus on imperfections in the goods and the labor markets. Most of us live somewhere in the triangle. So do Goodfriend and King. See in this light, “new” and “synthesis” may be both a bit of an overstatement.  
Blanchard (1997b, 290)

Blanchard’s triangle is indeed very useful for my point here: that the synthesis emerged from a trade among economists working in a narrowly defined area, one in which a representative agent was often assumed and which answered Lucas’s critique by constructing general equilibrium models based on optimizing agents, as in microeconomics. Questions about the non-neutrality of money (in the long run), on the appropriate type of microfoundations, on non-market clearing models, and about the limitations of assuming a representative agent, or about richer types of dynamics, for example, are simply not in that triangle and thus, are not central to the new synthesis. As microeconomists represent geometrically the choice under uncertainty by means of a simplex, which has as vertex degenerated lotteries where one outcome is certain and the others have zero probability (such simplex is a triangle in the case of three possible outcomes),

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<sup>23</sup> But the same author did recur to Kuhn’s notion of revolution in his 2008 essay, in order to argue that the macroeconomics of the 1970s has exploded and converged to the new synthesis, as previously mentioned. He then writes: “Not everything is fine. Like all revolutions, this one has come with the destruction of some knowledge, and suffers from extremism, herding, and fashion. But none of this is deadly. The state of macro is good” (2).

macroeconomists of the new synthesis have theoretical preferences over a two dimensional simplex—Blanchard’s triangle.

However, this does not mean that even in such triangle macroeconomists do not disagree. Or that they are not trying to reshape their space and transform the triangle into a geometric figure with more vertexes. The dissent inside the triangle relates to two points: first, that the new consensus models are not yet ready for policy analysis because it introduced many shocks that are not invariant to policy (Chari, Kehoe, and McGrattan 2009)<sup>24</sup> and, second, that the development of macroeconomics since the 1970s have emphasized mostly theoretical at the expense of practical issues—therefore, the recent theoretical developments have had little impact on practical macroeconomists in charge of conducting actual economic policies; in other words, macroeconomists privileged the developing of macroeconomics as science instead of engineering (Mankiw 2006).<sup>25</sup>

In terms of expanding the triangle or transforming it into a polyhedron, macroeconomists living in the triangle have their lists of improvements ready.<sup>26</sup> Chari and Kehoe (2006, 21-6), in a more standard RBC vein, list: (1) work more on labor market rigidities; (2) incorporate the idea that differences in taxes are a key source of the differences in the labor markets in Europe and the US; (3) introduce unemployment benefits to understand cross-country differences in unemployment.

Mishkin (2007, 27-30) goes in the direction of improvements to make monetary policy more of a science: (1) enrich estimated DSGE models so that to make them more realistic to the eyes of central bankers; (2) improve or extend the way nominal rigidities are usually incorporated in such models; (3) move from models with a representative agent to ones with heterogeneity of agents; (4) incorporate (and understand better its role) financial frictions; (5) go beyond rational expectations and embed behavioral economics into macroeconomics; (6) introduce learning in macro models; (7) keep art in monetary policymaking because economists “can never be sure what is the right model of the economy” (30).

Galí and Gertler (2007, 41-3) echo the aim to make “the model more realistic, by adding a variety of features that are likely to enhance its fit of the data” (41), and list as new directions of research: (1) replace Calvo’s time-dependent pricing scheme, in which the timing of adjustment

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<sup>24</sup> These authors strengthen their criticisms by questioning the view of a broad consensus in macroeconomics right in the opening paragraph: “Viewed from a distance, modern macroeconomics, whether New Keynesian or neoclassical, are all alike. ... Viewed up close, however, we disagree considerably” (242).

<sup>25</sup> Mankiw (2006) retakes and updates a concern already present in his 1990 article (repeated in his 1992 essay). See Woodford (2009, 275-7) for a criticism to Mankiw’s view.

<sup>26</sup> For critical assessments of the new consensus and the role of monetary policy in it, see Arestis (2007) and Arestis and Sawyer (2008). The literature critical on the use of a representative agent in macroeconomics is extensive. See Kirman (1992), Caballero (1992) (who is someone not exactly living outside the triangle), Janssen (1993), Hartley (1997), Hoover (2001), van den Bergh and Gowdy (2003), and references therein.

of prices is exogenous, for state-dependent pricing, which makes that timing depend on the evolution of the economy (endogenous); (2) incorporate labor market frictions that help economists account for the observed fluctuations in employment and job flows; and (3) abandon the complete markets hypothesis and introduce financial market imperfections. To Gali and Gertler's points (2) and (3), Blanchard (2008, 12-21) argues that macroeconomists should answer the questions of how markups move, in response to what, and why (he also repeats Chari, Kehoe, and McGrattan's criticism about shocks—proposing the use of a less structural approaches, in contrast to them—and he urges economists to pay more attention to the role of anticipations, suggesting departures from rational expectations).

Before concluding this section I would like to discuss with more detail the major criticisms that Solow raised against the new synthesis in general, and to the idea that modern macroeconomics has changed for better because it is now “firmly grounded on the principles of economic theory” (Chari and Kehoe 2006, 3). Solow (2008) comments on Chari and Kehoe's 2006 article, which they start, according to him with a self-congratulatory phrase and with the statement about macro having firm microfoundations. To Solow, the last sentence is “simply false” (243). He then criticizes the consensus macroeconomics through Chari and Kehoe's positions:

When Chari and Kehoe speak of macroeconomics as being firmly grounded in economic theory, we know what they mean. They are not being idiosyncratic; they are speaking as able representatives of a school of macroeconomic thought that dominates many of the leading university departments and some of the best journals, not to mention the Federal Reserve Bank of Minneapolis. They mean a macroeconomics that is deduced from a model in which a single immortal consumer–worker–owner maximizes a perfectly conventional time-additive utility function over an infinite horizon, under perfect foresight or rational expectations, and in an institutional and technological environment that favors universal price-taking behavior. In effect, the industrial side of the economy carries out the representative consumer–worker–owner's wishes. It has been possible to incorporate some frictions and price rigidities with the usual consequences—and this is surely a good thing—but basically this is the Ramsey model transformed from a normative account of socially optimal growth into a positive story that is supposed to describe day-to-day behavior in a modern industrial capitalist economy. It is taken as an advantage that the same model applies in the short run, the long run, and every run with no awkward shifting of gears. And the whole thing is given the honorific label of “dynamic stochastic general equilibrium.”

Solow (2008, 243)

Solow is explicit about not being against the idea that, as a first approximation, “individual agents optimize as best as they can,” which does not imply that the whole economy “acts like a single optimizer under the simplest possible constraints” (244). He stresses that the Sonnenschein-Mantel-Debreu theorems that “the only universal empirical aggregative implication

of general equilibrium theory are that excess demand functions should be continuous and homogeneous of degree zero in prices, and should satisfy Walras' Law." Many macro models, Solow continues, can satisfy these requirements "without imposing anything as extreme and prejudicial as a representative agent in a favorable environment" (244). In addition to retaking his preferred view on macroeconomics already sketched in his 1997 presentation at the AEA meetings (stressing his preference to small, tailored, partial equilibrium models), Solow (2008, 245) comes up with his irony:

I suppose it could also be true that the bow to the Ramsey model is like wearing the school colors or singing the Notre Dame fight song: a harmless way of providing some apparent intellectual unity, and maybe even a minimal commonality of approach. That seems hardly worthy of grown-ups, especially because there is always a danger that some of the in-group come to believe the slogans, and it distorts their work.

Chari and Kehoe responded to Solow's criticisms.<sup>27</sup> They recognize that the challenges facing modern macroeconomics are not small, but reject Solow's criticisms to the use of a representative agent and to their claim that macroeconomics is now firmly grounded in economic theory. With respect to the representative agent hypothesis, Chari and Kehoe (2008, 247) state that modern macroeconomics do not end with such hypothesis, and in fact it does not end "too far from where Solow prefers": "Most of macroeconomic research over the last 20 years has precisely been about incorporating the heterogeneity and the rich interactions that Solow seems to think it needs." They argue that macroeconomists just start with a representative agent and then enrich the model "with the detail necessary to answer the question at hand" (248). They also criticized Solow back, for his use in his growth papers of a single production function with aggregate labor and stock of capital, with which he "sacrificed realism for an abstraction that has proven invaluable" (247).

In relation to Solow's point about aggregation and the Sonnenschein-Mantel-Debreu implications, Chari and Kehoe (2008, 248) found a way out:

Solow's argument is based on an appeal to the Sonnenschein-Mantel-Debreu result, which implies that if we have only aggregate data, then theory imposes little discipline on how we model aggregates. Fortunately for macroeconomics, the Sonnenschein-Mantel-Debreu result notwithstanding, discipline is available elsewhere. If we have microeconomic data on how individual households and firms behave, then theory imposes discipline on the behavior of aggregates over and above Walras' Law and zero-degree homogeneity.

The way macroeconomists use microeconomic data to discipline their models is still developing.

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<sup>27</sup> It is interesting to note that they published their response as an NBER Working Paper (13655, Nov. 2007), which had as title "the heterogeneous state of modern macroeconomics: a reply to Solow". The reply was published without a title.

The debate between Solow and Chari and Kehoe illustrates that macroeconomists can disagree more widely when they distance themselves a bit from the narrow definition of macroeconomics aptly captured by Blanchard in his triangle, which is supported by microfoundations and the representative agent hypothesis, among other things.

## 2. Hybridism in the Marshall-Walras Divide?

Michel De Vroey (2004) has proposed a framework for reading the history of macroeconomics after Keynes: the so called Marshall-Walras divide. He started by noting that general equilibrium has different variants that he along two lines. First, general equilibrium models can be either Walrasian, the hegemonic one, or Marshallian. Second, they can be either complex or simplified/pragmatic depending on the degree of abstraction and the model's applicability to real-world phenomena.

The first line has to do with the trade technology assumed in the model ("that is, the institutional setup enabling equilibrium outcomes to be arrived at", De Vroey 2004, 64). Walrasian models assume "the auctioneer-let *tâtonnement* process" (64) in which the auctioneer has bilateral relationships with isolated individuals and set prices to clear the markets. Agents do not interact, communicate between themselves and do not know about excess demand functions; the omniscient auctioneer takes care of all this. As De Vroey (2004, 65) stresses, such institutional arrangement preclude agents' ability to influence prices: "the auctioneer hypothesis and imperfect competition, it turns out, are incompatible bedfellows." The Marshallian approach, of a monetary economy, depicts the economy "as composed of separate markets of different subtypes (factors, final goods, etc.)," each of them being "an autonomous locus of information of equilibrium" (66). Moreover, agents are price makers and no auctioneer exists.

With the four categories that emerge from the possible combinations along these two lines, De Vroey goes on to classify the major developments in macroeconomics. In such categorization, the RBC models are Walrasian pragmatic dynamic general equilibrium models (as they assume price flexibility and perfect competition), while the new Keynesian ones are Marshallian pragmatic dynamic general equilibrium models (as they assume price stickiness and imperfect competition).

De Vroey's framework is useful in this narrative because it highlights the fact that RBC theorists and new Keynesians were in different camps, in the two dimensions he considered (Walrasian vs. Marshallian trade technologies and complex vs. pragmatic models). As discussed above, the new synthesis should be assigned to the Marshallian-pragmatic-dynamic bin of De Vroey's structure because these models have sticky prices and imperfect competition (and are

simplified models designed for practical applications, most macroeconomists besides Mankiw would agree).

The major problem I see with De Vroey's analysis applied to the new consensus macroeconomics is that it is not flexible enough to show the merging of the previously antagonistic schools: such convergence goes beyond the dimensions stressed by him. In particular, since the Walrasian auctioneer and the Marshallian imperfect competition are incompatible bedfellows, it is hard to fully appreciate the essence of the new consensus model: it is an RBC model of the natural level of the variables (i.e., the ones under price flexibility) and also a model that explains why actual variables deviate from such levels and, thus, how monetary policy can be used to make the economy approach the second-best equilibrium (the one with price flexibility and imperfect competition).

It should be noted that the overall picture about the new synthesis was not as clear to De Vroey as it is now, and I agree in general with De Vroey when he states that he finds it difficult that people like Lucas and Prescott would not be ready to sign a manifesto for the new synthesis. But looking at the current practice in macroeconomics I have difficulties with the claim that Chicago and Minnesota "may have shifted from Marshall to Walras" but have deep-rooted resistance to imperfect competition (86). This claim led De Vroey (2004, 86) to conclude that:

a merger between [the perfectly competitive and imperfectly competitive models] is hard to envisage. The picture that then emerges is rather one of two rival macroeconomic paradigms, a Marshallian (i.e., imperfect competition) and a Walrasian (i.e., perfect competition), existing side by side.

I see a local merging or hybridism: willing to talk about economic policies (especially monetary policy), new Keynesians and RBC theorists share a common basic model, although they may differ in terms of their preferred additional elements added to it. They trade inside the triangle, establishing a trade zone, to use Peter Galison's (1999) idea. Otherwise, they may well disagree.

### **3. Some Final Remarks**

Nowadays, there is a methodological consensus in macroeconomics to the extent that "there are really no longer alternative approaches to the resolution of macroeconomic issues," as Woodford (2009, 274) writes. The new consensus is perceived to promote a greater merging than the "old" neoclassical synthesis—an idea reinforced by De Vroey's (2004, 75-76) observation that the old one was not truly a synthesis but rather "a metaphorical compromise between two approaches that did not want to enter into an open intellectual fight."

The nuanced views that macroeconomists have about the synthesis reflect a deeper understanding about how their field evolves. While some favor the notion of revolutions and

counter-revolutions, and others deny it, all seem to see macroeconomics progressing over time, with knowledge accumulating and improving. Moreover, implicitly or explicitly, most accounts are centered on internal progress: new theories improving on older ones by fixing logical and empirical flaws.

However, Mankiw (2006) favors a bit broader understanding of the evolution of macroeconomics by stressing that the field has a dual role: macroeconomics as science, devoted to “understand how the world works” (29-30), and as a type of engineering, concerned with solving practical problems. But Goodfriend (2007) makes a clearer tentative of relating theoretical developments with historical events (and Mishkin 2007 as well).

While Blanchard (2008) at once casted the new synthesis as a revolution with mild Kuhnian flavor, Mankiw (2006, 39) preferred to see it as a truce:

It is tempting to describe the emergence of this consensus as great progress. In some ways, it is. But there is also a less sanguine way to view the current state of play. Perhaps what has occurred is not so much a synthesis as a truce between intellectual combatants, followed by a face-saving retreat on both sides. Both new classicals and new Keynesians can look to this new synthesis and claim a degree of victory, while ignoring the more profound defeat that lies beneath the surface.

There is some truth in Mankiw’s truce: while the new neoclassical synthesis can be also called as new Keynesian macroeconomics, to the extent that RBC models were simply silent about monetary policy, RBC followers make the case that the new consensus is just a convergence to “policy recommendations similar to those made by neoclassical economists like Lucas and Stokey 25 years ago” (Chari, Kehoe and McGrattan 2009, 265).

All the different views macroeconomists have should not diminish the degree to which they converged in talking about fluctuations. They all cast such phenomena in terms of a dynamic stochastic general equilibrium model of a representative agent, in its most common specifications. To the historians of economics, not only this consensus is an interesting object to study, but also the history about its evolution that has been already produced—with a fascinating aspect that some of the articles constructing this history that are published in major economics journals are classified as “general economics and teaching” and “macroeconomics and monetary economics” (as Woodford 2009).

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