Department of Economics- FEA/USP

Not Going Away? Microfoundations in the Making of a New Consensus in Macroeconomics

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WORKING PAPER SERIES Nº 2011-02

DEPARTMENT OF ECONOMICS, FEA-USP
WORKING PAPER Nº 2011-02

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Abstract:

Macroeconomics, or the science of fluctuations in aggregate activity, has always been portrayed as a field composed of competing schools of thought and in a somewhat recurrent state of disarray. Nowadays, macroeconomists are proud to announce that a new synthesis characterizes their field: no longer are there fights and disarray, but rather convergence and progress. I want to discuss how modern macroeconomists see the emergence of such a consensus and, therefore, how they see the history of their sub-discipline. In particular, I stress the role played in the making of such a consensus by a particular understanding of the microfoundations that macroeconomics needs.

Keywords: new neoclassical synthesis; microfoundations; DSGE models; consensus

JEL Codes: B22; B20; E30

Not Going Away? Microfoundations in the Making of a New Consensus in Macroeconomics[1]

Pedro Garcia Duarte[2]

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 of 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)

1. Introduction

According to the economists discussed in this essay, macroeconomics here 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 business cycles and the theory of economic stabilization (with greater emphasis on monetary economics) rather than on growth.

In the last decade a growing number of macroeconomists have presented their field as a steadily progressing enterprise because they now work in a common theoretical framework.

[1] This paper was presented at the First International Symposium on the History of Economics ("The Integration of Micro and Macroeconomics from a Historical Perspective"), University of São Paulo, São Paulo, Brazil, August 3-5, 2009, and at the American Economic Association Meeting in Atlanta, January 2010. I am very grateful for helpful comments I received at both events, especially those from Kevin Hoover, Michel De Vroey, Perry Mehrling and Gilberto Tadeu Lima, and also for those made by Tiago Mata. In addition, I thank Edward Prescott, Michael Lovell, Martin Eichenbaum, Lawrence Christiano, Robert King, and Michael Woodford for kindly answering a few questions. None of them is in any way responsible for the final outcome. I gratefully acknowledge financial support from Fapesp (Brazil) and CNPq (Brazil). This paper will be published by Edward Elgar in 2012 in a volume that Gilberto Tadeu Lima and I are editing, titled "Microfoundations Reconsidered: The Relationship of Micro and Macroeconomics in Historical Perspective."

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Clearly, these economists have an understanding of the current consensus that is inseparable from their view that in the past macroeconomics could not progress much because competing schools fought never-ending theoretical battles. My goal in this essay is to discuss how modern macroeconomists see this consensus that emerged in their field. Because they view this consensus as the last step of a historical ladder, its characterization is intimately related to how they see the history of their sub-discipline.[3] In particular, I will stress how a particular understanding of the microfoundations that macroeconomics needs formed a common ground on which economists could agree and reach a consensus.

In contrast to economists working in other areas of economics, macroeconomists perceive their field as not only composed of competing schools of thought but also characterized by a somewhat recurrent state of disarray.[4] For instance, in the 1970s Robert Hall, "as a gross oversimplification," divided macroeconomic thought in the U.S. at that time into two opposing schools: the freshwater school, which referred to the new classical economists (and later the real business cycle economists) located in universities near lakes or rivers such as the University of Chicago, Carnegie-Mellon, Rochester and Minnesota; and the saltwater school comprising the Keynesians at universities like Harvard, Berkeley, MIT, Princeton, Stanford and UCLA near either coast. Hall (1976, 1) explains:

> The fresh water view holds that fluctuations are largely attributable to supply shifts and the government is essentially incapable of affecting the level of economic activity. The salt water view holds shifts in demand responsible for fluctuations and thinks government policies (at least monetary policy) is [*sic*] capable of affecting demand. Needless to say, individual contributors vary across a spectrum of salinity.

Although Hall's (1976) dichotomy, as Robert Gordon (1989, 177) put it, "has dominated the coffee-break oral tradition of American macroeconomic conferences" for decades, he was not alone in using the idea of a school of thought. Axel Leijonhufvud (1976), among others, explored more systematically how economics and its schools of thought could be interpreted according to the ideas of Thomas Kuhn ([1962] 1970)—which became very popular in economics in the late 1960s and 1970s (Weintraub 2002, 263)[5]—and of Imre Lakatos (1970). Given Kuhn's and Lakatos's notoriety

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[3] It is important to emphasize that I discuss mainstream economists' perceptions of the evolution of their field without taking issues with some of their claims. I want to explore the diversity that still exists in their understanding about both the consensus and the history of macroeconomics despite the alleged convergence that currently characterizes the field.

[4] Here I borrow William Nordhaus's (1983, 247) term used later by Karl Brunner (1989) and N. Gregory Mankiw (1992a) in the very title of their papers (the latter paper draws heavily on Mankiw 1990). This expression was used earlier during the stagflation of the 1970s: see for instance Saul Hymans and Harold Shapiro (1975), a paper presented at the 1975 American Economic Association Meeting but published in 1978 in the *Review of Economics and Statistics* as a different version that did not use the term disarray.

[5] Blaug (1975) argued that by the mid-1970s economists had abandoned Popper and embraced Kuhn. However, some doubts on the applicability of Kuhn's ideas to economics (and to the Keynesian revolution) were already raised, which led Blaug to propose that Lakatos's framework may work better in economics. See Redman (1991, chap. 7) for further references on economists who were the first to apply Kuhn's concepts to understand the evolution of their field. De Vroey in this volume discusses the transition from IS-LM to the new classical/real business cycle (RBC)

among economists from the 1970s onward, and given that the view economists have about how science is conducted shape the history they and historians of economics write, it is not surprising to see macroeconomists increasingly using terms such as schools of thought, normal science, revolutions, research programs and paradigms, and also talking about accumulation of knowledge and progress in their field.[6]

Given that a school of economic thought is a loose concept that can be associated either with a Kuhnian paradigm or a Lakatosian research program, the fact that the concept of a school of thought survived well in economics from the 1970s to the present is not accidental.[7] For example, 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),[8] and some might add the post-Keynesian school to this list.[9] What becomes clear is that macroeconomists have a loose understanding of what makes a school and at the limit classify anyone with a particular idea as a school of thought. As an emblematic example of such a loose use

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macroeconomics in Kuhnian terms. In contrast to this view of revolutions in macroeconomics, Lucas (2004, 21-22) recently has argued that economics is a field technically progressing in a common paradigm that was initiated and structured by Smith and Ricardo. He then denied the existence of either paradigm changes or scientific revolutions. A few years earlier, in an interview with Snowdon and Vane, Lucas (1998, 127) observed that economists have used the concept of scientific revolutions loosely and disagreed that the new classical approach resulted in a revolution in macroeconomics: "Sargent once wrote that you can interpret any scientific development as continuous evolution or discontinuous revolution, at your pleasure. For myself, I do not have any romantic association with the term 'revolution'. To me, it connotes lying, theft and murder, so I would prefer not to be known as a revolutionary."

6 See Weintraub (2002, chap. 9) for a witty discussion on how the kind of historical analysis produced by economists and historians of economics is tied to their methodological conceptions. In fact, the notion of a scientific revolution in economics predates Kuhn and was put forward by Keynes's followers in the late 1940s (Pearce and Hoover 1995, 183)—see also Laidler (1999), who argued that although Keynes himself understood that his work would revolutionize economics, "an element of myth-making is involved whenever the phrase 'Keynesian revolution' is deployed" because "the re-arrangement of ideas to which it refers was neither revolutionary in the usual sense of the word nor by any means uniquely Keynesian in origin" (3). Besides this, as Roger Backhouse (2010, chap. 9) correctly pointed out, Kuhn's ideas were appealing to dissenting economists: through a scientific revolution, a new paradigm emerges, with the old and new paradigms being incommensurable. In the 1970s talk of a crisis in economics was widespread, which led heterodox economists to seek "consciously to create a new paradigm" (159). Although this certainly is important for understanding the popularity of Kuhnian ideas among economists, I want to explore here how mainstream economists, those defending the status quo, loosely used Kuhn ([1962] 1970) and even attributed to him a notion to which he was opposed, that science develops in a cumulative manner.

7 Snowdon and Vane (1996) wrote a history of macroeconomics since the 1970s, when it was in a state of "disarray," characterizing it as a succession of revolutions and counter-revolutions among "conflicting and competing approaches" (382). They then talked about "the new classical *research programme*" that "replaced monetarism as the main rival to Keynesianism." A few lines further they talked about "the new classical and new Keynesian *schools*" (382, emphasis added). This is just one example of economists using a school of thought as a synonym for Lakatos's concept of a research program.

8 Kevin Hoover (1988, chap. 1) placed 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. Among the most recent that follow this tradition, and that I will discuss later, are Goodfriend and King (1997), Taylor (1997), Woodford (2000), Mankiw (2006), and Akerlof (2007). See also Greenwald and Stiglitz (1988, 207) and Romer (1993, 5-6) for a brief overview of recent macroeconomics along these lines.

9 Would either the Marxist or the institutionalist theories be included as schools of macroeconomic thought? Sheila Dow (1996, chap. 4) did include them (and others).

of the term school in macroeconomics, Snowdon asked Leijonhufvud in an interview: "Do you attach any school of thought label to yourself? Do you see yourself as some kind of Leijonhufvudian Keynesian?" To which Leijonhufvud (2004, 127) answered: "In one sense the groupings have disappeared because not many economists are interested anymore. One of my weaknesses is that I am psychologically averse to running with some herd, or even breeding a herd of my own."

As for the understanding that the history of macroeconomics, in contrast to that of other areas of economics, is not characterized by 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, an understanding shared by N. Gregory Mankiw (see Snowdon and Vane 1995, 50-51):[10]

> 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.[11]

We then come to another central element of the mainstream 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

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[10] Michael Woodford received his AB from the University of Chicago in 1977, after which he obtained a JD in 1980 from Yale Law School. He then went to MIT for his PhD in economics, finishing in 1983, with Robert Solow as his official advisor. However, given the technical nature of his work (intertemporal economics), Woodford had Timothy J. Kehoe as his de facto advisor. (Kehoe received his PhD in economics in 1979 from Yale under Herbert Scarf, and he was at MIT from 1980 to 1984 where he taught a graduate course on his field of expertise at the time, general equilibrium theory.) The third member of Woodford's committee was Peter Diamond. As he recently explained to me in an email message (01/31/2011): "My later work on the 'new neoclassical synthesis' had a great deal to do with problems that I encountered in my studies under both Bob Solow and Peter Diamond, as I mention in the preface to my [2003] book. At the same time, what I understand about how to do general equilibrium modeling owes a tremendous amount to Tim Kehoe, who taught general equilibrium theory (not macroeconomics) when I was at MIT." Mankiw also obtained his PhD from MIT in 1984 under Stanley Fischer. Fischer, another PhD from MIT, graduated in 1969 under Franklin Fischer.

[11] Historians of economics have produced narratives that show the complexity of the history of microeconomics, denying that this area progressed smoothly and steadily within an unchanged explanatory framework. For a few examples, see Weintraub (1992), Mirowski and Hands (1998), and Mirowski and Hands (2006).

consensus when knowledge seems to progress at a faster rate.[12] These macroeconomists identify two consensuses in the history of their sub-discipline: the neoclassical synthesis of the 1950s and 1960s and the recent new consensus (from the late 1990s to the present),[13] labeled as the new neoclassical synthesis by Marvin Goodfriend and Robert King (1997).[14] Macroeconomists tend to characterize and tout such periods of synthesis as moments when the intellectual disarray and the untamed competition among schools—both with respect to macroeconomic theories and policies to be prescribed—are replaced by balanced conversation, points of convergence, better policymaking and scientific progress.[15]

Macroeconomists emphasize progress and secure knowledge at times of consensus as a way of stressing that the science of the consensus is good and strong.[16] The flip side of this argument used by macroeconomists in academia is to say that having schools competing in a state of disarray is a synonym for weak science: Stiglitz (1992, 40) pondered whether the fact that macroeconomists' views were so divergent indicates that they are "simply ideologues looking for justifications for

[12] For instance, Mankiw (1992b, 564-565) denied that macroeconomics is like a pendulum that "is destined to oscillate between two irreconcilable extremes," the classical and the Keynesian views, and argued that it does make progress. Bill Gerrard (1996, 54) reviewed the book by Snowdon, Vane and Wynarczyk (1994) and argued that "macroeconomics can be seen as an evolving classical-Keynesian debate from which a developing consensus is ever-emerging as current disagreements are resolved, but new disagreements continually appear requiring the consensus to re-emerge." He then added: "Within mainstream macroeconomics a clear case can be made that the competing schools of thought have generated cumulative progress" (65), and concluded that "macroeconomics is, and will remain, controversial as classical and Keynesian schools provide contending views on the self-adjusting nature of the macro economy and the necessity or otherwise of stabilization policy. This classical-Keynesian debate has been progressive and an ever-emerging, albeit partial, consensus has resulted.…All schools of thought need to recognize more fully the inherent limitations of their own perspectives. Progress in macroeconomics requires competition and co-operation" (66).

[13] Different from this view that the neoclassical synthesis of the 1950s represented economic knowledge in progress, Wade Hands, in this volume, challenges the perception of a synthesis between micro and macroeconomics (as does De Vroey 2004). In his narrative, he proposes that co-evolution, rather than a synthesis, captures much better the relationship between micro and macroeconomics over that period: " although Walrasian economics had a certain core conceptions [*sic*] that were identifiable over time, it also evolved and changed in response to, and because of, its contact with Keynesian economics," while in the end microeconomics and macroeconomics remained identifiable and distinct fields (pp. 6-7, fn. 6). Additionally, Philip Mirowski in his chapter challenges the view that the synthesis was "Keynesian" because there was, according to him, an anti-Keynesian hostility by the major neoclassical schools in the US in the postwar period. In my narrative, notice that I usually refer to the neoclassical (instead of Keynesian) synthesis and I present it according to views held by practicing economists.

[14] Goodfriend and King both obtained their PhDs in economics from Brown University in 1980. King joined the University of Rochester faculty in 1978 without completing his PhD. He finished his thesis by spring of 1979 but missed the graduation deadlines and, thus, was awarded his degree in 1980.

[15] Lucas (1998, 133) denied the view that there is more consensus among microeconomists compared to macroeconomists, as defended by Woodford (2000). But Lucas agreed that a consensus in macroeconomics was emerging: "What is the microeconomic consensus you are referring to? Does it just mean that microeconomists agree on the Slutsky equation, or other purely mathematical propositions? Macroeconomists all take derivatives in the same way, too. On the matters of application and policy, microeconomists disagree as vehemently as macroeconomists—neither side in an antitrust action has any difficulty in finding expert witnesses. I think there is a tremendous amount of consensus on macroeconomic issues today. But there is much that we don't know, and so—necessarily—a lot to argue about." Lucas also made the point that consensus refers to specific issues, not to a whole area, which needs certain disagreement among its members to progress: "Consensus can be reached on specific issues, but consensus for a research area as a whole is equivalent to stagnation, irrelevance and death" (133).

[16] This is particularly clear in the defense that V. V. Chari made of present-day macroeconomics in a testimony before the Subcommittee on Investigations and Oversight of the Committee on Science and Technology, U.S. House of Representatives (Chari 2010): "I will argue that macroeconomics has made huge progress, especially in the last 25 years or so" (1). This hearing intended to "examine the promise and limits of modern macroeconomic theory in light of the current economic crisis" (Committee on Science and Technology 2010, 1).

[their] political biases, or (no less worse) technicians, taking the assumptions provided to [them] by [their] ideologue brethren and exploring their consequences." Returning to Kuhn ([1962] 1970), when there is no dominant paradigm, there is no normal science: macroeconomists have an epistemological fear that the scientific foundations of their studies are weak or absent if they are always in a state of intellectual disorder.[17] Alan Blinder's (1989, vii) words could not be more emphatic about this perception among macroeconomists:

> macroeconomic debates during my professional career have been distressingly unrelenting, acrimonious, and even ideological. The constant state of intense disputation takes a personal toll and, more importantly, inhibits scientific progress. Too much of our time, it seems to me, is spent defending obvious positions against preposterous challenges, too little doing what T. S. Kuhn called normal science. Sometimes I wonder if we are doing science at all.[18]

As an antidote to such a perception, the scientific and academic prestige of macroeconomists among both economists in general and other scientists could be boosted if they had a story to tell of steady progress and secure knowledge (see Woodford's quotation above and Mankiw's welcoming words about the emerging consensus in Snowdon and Vane 1995, 60). In this respect, one needs to have macroeconomists working within a unified framework.

On the other hand, policymakers keep asking macroeconomists what theory they should use to guide policy, and intellectual disarray here is not good either. Macroeconomists can give a convincing answer as long as they are able to show that there is a core of usable macroeconomics in which they all believe (to use the theme of a session at the 1997 AEA Meeting).[19] In this sense, it is symptomatic that Frederic Mishkin (2007), who was a Member of the Board of Governors of the Federal Reserve System from 2006 to 2008, argued that the major advances in monetary economics

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[17] 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.)" Solow (2000, 151) started his article with similar lines: "These days macroeconomics has become more respectable than it used to be. I can remember when many economists liked to say: Microeconomics is not problematic, but I just can't *understand* macroeconomics." (On page 155 he explicitly identified the current consensus with Kuhn's concept of normal science.) Snowdon and Vane (1995) chose as the epigraph of the article they wrote based on an interview with Gregory Mankiw, Knut Wicksell's (1851-1926) words from a 1904 lecture to an audience interested in science: "in other fields of science these conflicts come to an end…It is only in the field of economics that the state of war seems to persist and remain permanent."

[18] Blinder (1989, viii) argued that the state of macroeconomics in the late 1980s was very different from the early 1970s, after he obtained his PhD from MIT (in 1971 under Robert Solow) and became an assistant professor at Princeton: "The academic world I entered in 1971 was quite different from the one I have inhabited ever since.…The monetarist controversy was simmering, but the Keynesian paradigm reigned supreme." Later, he also referred to the new classical economics as a revolution ("Lucasian revolution") that was largely destructive compared to "the period of normal science that had preceded it" (interview with Snowdon 2001, 112).

[19] The participants in 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).

have allowed monetary policy to become more of a science.[20] Mishkin's opinion was a hope with which Goodfriend and King (1997) closed their article, and it was also shared to some degree by V. V. Chari and Patrick Kehoe (2006), by Goodfriend (2007), and by Jordi Galí and Mark Gertler (2007).[21]

With the new wave of consensus in macroeconomics (the new neoclassical synthesis), mainstream macroeconomists are emphasizing greatly the progress reached nowadays. For them, in essence, there is no Kuhnian substitution of one paradigm for another via revolutions, but rather a merging of previously competing 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 2009, 210, paraphrasing his earlier statement "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 2009, 210).[22]

In a nutshell, both in theory and practice, claiming a consensus means that mainstream macroeconomists now agree on the right way of doing macroeconomics—and it is right because it has become generally accepted, after facts and arguments refuted wrong theories and conventions had become uniform.[23] Knowing how to do something is the prerequisite for doing it right and thus for increasing the stock of knowledge.

In order to proceed with the analysis of the new consensus, I will first point out briefly that macroeconomists understand that disagreements still exist. Following this point I will explore how these economists picture the period of wide disagreement that occurred after the breakdown of the first neoclassical synthesis (roughly from the 1970s to the early 1990s). Having this picture clear in one's mind is important because the progress associated with the new consensus is best identified by comparing the current state of macroeconomics with the preceding disarray. I then discuss how

[20] Mishkin is another of the economists being discussed in this paper who obtained his PhD in economics from MIT (in 1976 under Stanley Fischer). His first position after graduating was as an economist in the summer of 1977 with the Board of Governors of the Federal Reserve System. After this he held several academic and policy-oriented positions.

[21] Continuing to pay attention to the academic training of the economists under study, Galí also received his PhD in economics from MIT, in 1989 under Olivier Blanchard who also has a PhD in economics from MIT (1977, under Stanley Fischer). Chari, Kehoe, and Gertler are all non-MIT graduates: they received their PhDs in economics from Carnegie-Mellon (1980), Harvard (1986), and Stanford (1978), respectively.

[22] See also Blanchard (2008). Mankiw (2006, 38) stated that accompanying the emergence of the new consensus 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 of civilized men after reading the response that Chari and Kehoe gave to Solow's comments on their 2006 article, for example (Chari and Kehoe 2008).

[23] Blanchard (2009, 212) once again exemplified 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."

mainstream macroeconomists saw the emergence of the new synthesis: the development of a theoretical common ground on which different schools could negotiate models and what counts as relevant empirical evidence, and thus resolve disagreements to reach a consensus.[24] Identifying macroeconomics with this area implies that those macroeconomists who are not willing to work in this arena are outsiders and do not count in the consensus. I will stress how such an area was built from a particular understanding of the microfoundations that macroeconomics thus understood needs.

2. A New Consensus in Macroeconomics

For many followers of the new neoclassical synthesis, the existence of a consensus in their field did not mean that room for disagreement no longer existed (Goodfriend 2007, 3; Blanchard 2009, 210; Woodford 2009, section III; Chari, Kehoe, and McGrattan 2009). After all, as Solow (1983, 279) pointed out, there is always (some degree of) disagreement at the frontier. Nonetheless, they all agree that modern macroeconomics, in theory and in practice, has changed substantially when compared with the 1970s, 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).[25]

In the 1970s and the 1980s, the perspective of mainstream economists was well summarized by Blanchard when he said that macroeconomics "looked like a battlefield" with "researchers split in different directions, mostly ignoring each other, or else engaging in bitter fights and controversies" (Blanchard 2009, 210): the neoclassical synthesis had broken down and over time monetarists, new classical and real business cycle (freshwater) economists, and new Keynesians (saltwater) economists fought one another and disagreed on many issues. Mainstream macroeconomists saw the new synthesis as a bridge between two broad fields, the classical (that 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

[24] Zouache (2004, 98) called this a "common methodological reference to the microeconomic foundations of macroeconomics." As will become clear, while I agree with this point, I disagree with the author's view that the new neoclassical synthesis is "more an extension of the Real Business Cycle research programme than a synthesis between two research traditions [RBC and new Keynesian]" (108).

[25] Goodfriend (2004, 21) and Galí and Gertler (2007, 25) share with Chari and Kehoe their enthusiasm about the state of macroeconomics.

associated with the large-scale econometric models).[26] As Woodford (2009, 268) argued, 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 deep disagreements and questionings no longer exist.

Before discussing how macroeconomists characterized the new synthesis, I would like to stress briefly the differences between the new classical and RBC research programs on the one hand, and the new Keynesian program on the other hand.

3. 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.[27] 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 ability of the Keynesian device to incorporate inflation into their IS-LM framework: the Phillips curve (Hoover 1988, chap. 1).[28] 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).[29] New classical macroeconomists like Robert Lucas and Thomas Sargent wanted to bury Keynesian theorizing (as in their famous 1979 article) and to propose a market-clearing approach to economic fluctuations—or "to see whether the 'expectations view' can be pushed far enough to yield something like a full business cycle theory."[30]

Lucas (1976) criticized the use of reduced-form econometric models for policy evaluation (i.e., simulating and comparing paths of endogenous variables under alternative economic policies): the parameters of estimated aggregate relationships are themselves a function of deeper preference and technology parameters and such a function changes when the government adopts a new policy.

[26] See for example Mankiw (1989, 79), Goodfriend and King (1997, 232) and Woodford (2000, 29).

[27] Blanchard (2009, 210) 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 word) of the field in the 1970s."

[28] On the importance of the high inflation of the 1970s to the defeat of Keynesianism and the development of the new classical economics, Lucas (1998, 122) argued that: "The main ideas that are associated with rational expectations were developed by the early 1970s so the importance of the inflation that occurred was that it confirmed some of these theoretical ideas. In a way the timing couldn't have been better."

[29] The point here is not to draw a complete scenario of the evolution of macroeconomics in the postwar period, which should include many issues. I just want to give a rough sense of how practitioners see the major changes in their field. Among the issues that are part of that broader scenario is the general disequilibrium theory of the 1960s and 1970s, which, in contrast to Mankiw (2006, 35), I do not consider to be the first wave of the new Keynesian work—for me this wave was initiated in the 1980s with the work on static (usually partial equilibrium) models done by Mankiw, Romer, Akerlof and others.

[30] This was how Lucas described his intentions in his paper (published later, in 1973) to Otto Eckstein (Harvard University) in a letter dated January 20, 1970, in which he accepted an invitation to present a seminar at Harvard (Folder "1970 Correspondence R. E. Lucas," Box 32; Robert E. Lucas, Jr. Papers, Rare Book, Manuscript, and Special Collections Library, Duke University).

New classical, RBC and new Keynesian economists all worked in a similar fashion to address the Lucas critique by providing the kind of microfoundations that nowadays characterizes not only their research programs but also the models of the new consensus macroeconomics.[31] It is important to add that these economists used the conceptual tool of a representative agent as part of their answer to the Lucas critique without providing clear justification (as pointed out by Hoover in his contribution to this volume).

During the 1970s, the 1980s and early 1990s new classical, real business cycle, and new Keynesian economists were in a battle. They had important points of disagreement, but they also shared some methodological and theoretical elements. To some degree, mostly with respect to policy implications of their models, the new classical and real business cycle macroeconomists, on the one hand, and the new Keynesians on the other hand, seemed to criticize each other from the ground up—to repeat Solow's (1983) words. Or, to use Paul Krugman's (2000, 39) words, there was a great schism in macroeconomics in the 1980s, with on the one hand the RBC economists arguing that "because [they] have not managed to find a micro-foundation for non-neutrality of money, money must be neutral after all," and, on the other hand, the new Keynesians arguing that "[they] need some other explanation of apparent non-neutrality, resting in something like menu costs or bounded rationality."

Even macroeconomists not connected to either of those two approaches understood that these groups differed by virtue of faiths in alternative sets of tenets. For instance, David Cass, a student of Hirofumi Uzawa and an economist who made seminal contributions to the optimal growth literature, the model that is the benchmark of the RBC literature, stated in an interview in 1998:[32]

> But the thing about real business-cycle theory I suppose is that it is almost like a religion. I have talked quite a bit with Victor [Rios-Rull], whom I have a lot of respect for, who has this view, this view that he is convinced quite strongly about, that this is the only way to look at the world, to look at economics.[33] When anybody tells me it's the only thing, I'm skeptical. I don't believe that using general equilibrium theory is the only way of looking at the world….But I also think that the general equilibrium model itself has a role, that it is still an important benchmark, and that there are still a lot of interesting things that can be done with that theory.

What then are the core elements of each of the two groups, new classical/RBC and new Keynesian macroeconomics? 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

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[31] The call for microfoundations is not simply a result of the Lucas critique. Hoover in this volume provides a detailed analysis of the different microfoundational programs that marked the history of macroeconomics since the 1930s and argues that they even have a pre-history. His discussion historicizes the notion of microfoundations. De Vroey, also writing in this volume, explores in detail the microfoundations requirement as expressed by Lucas and the justification he presented for his enterprise.

[32] Spear and Wright (1998, 547).

[33] José-Víctor Ríos-Rull obtained his PhD in 1990 from the University of Minnesota, under Edward Prescott.

(in fact, a representative agent) 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). I add to this set the assumptions about the economic environment that these models commonly assume: an economy working in perfect competition with prices that 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 identified either real shocks (including both aggregate demand and aggregate supply shocks, like fiscal policy and technology shocks) or errors in expectations as the sources of fluctuations.

Over time, several of the models in this new classical/RBC 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 consumption function used in the IS-LM model); (b) the irrelevance of current profits to investment spending through the Modigliani-Miller theorem; (c) the natural rate theory of unemployment which states that in the long-run inflation and unemployment are independent (i.e., the long-run Phillips curve is vertical);[34] (d) the inability of monetary policy to stabilize output as a consequence of the rational expectations hypothesis (economic policies cannot systematically affect real output, a result known as policy irrelevance; therefore, actual output is systematically equal to its potential level); and, (e) the irrelevance to consumption of taxes and debt as ways of financing budget deficits (Ricardian equivalence). These neutrality results went against many results associated with either the Keynesian orthodoxy of the postwar period or, more importantly, the new Keynesian camp that I will discuss later.

3.1 Differences Among Siblings: The New Classical and the Real Business Cycle Macroeconomics

Although it is sometimes convenient to group together the new classical and the real business cycle macroeconomists,[35] a group to be contrasted to the new Keynesians in the present narrative, there are three important differences between these two camps. First, at their origins these groups had different methods of bringing their models to the data. New classical economists

[34] In several of the rational expectations models, the Phillips curve is also vertical in the short run. Hoover (1988, 28-31) explained that these models have an “ephemeral Phillips curve.”

[35] In fact, RBC macroeconomists can be seen as continuing the research agenda established by new classical economists like Lucas. This was what Prescott wrote to Lucas in 1991: “much of what we are doing [at Minnesota] is working out the research program that you defined. I wish there were a noun for calibrate or a noun that captured the idea of being rigorous” (letter from Prescott to Lucas, Feb. 19, 1991, folder “1991 Correspondence,” Box 25; Robert E. Lucas, Jr. Papers, Rare Book, Manuscript, and Special Collections Library, Duke University).

developed not only solution methods for their model but also estimated them.[36] In contrast, RBC theorists became known mostly for their calibration method, as clearly defended by Kydland and Prescott (1991a), and for their disapproval of econometrics.[37] They generally calibrate the parameters of their models based on equations evaluated at steady-state so that the business cycle models replicate "stylized facts" of long-run growth theory: the steady-state variables are replaced by their sample means (or other moments) in the data, and the equations are solved for the unknown parameters. Kydland and Prescott (1982, 1346) referred to this procedure as estimating parameters "using steady-state considerations," while Kydland (1992, 477) put it as determining "parameter values on the basis of non-business-cycle measurements." To this end microeconomic empirical evidence can be used also to calibrate some parameters of the model. After calibrating the parameters, they test the model's ability to reproduce short-run facts as co-movements, variances and means of aggregate variables in the data (real output, consumption, hours worked, and so on).[38] Kydland and Prescott (1991a) claimed to be the heirs of a tradition that dates back to Ragnar Frisch in the 1930s (a point criticized by Hartley, Hoover and Salyer 1997, 35). While Frisch and other economists in the 1960s and 1970s calibrated their models by Frisch because it was the only option available, given that they lacked the time series needed to estimate the equations, the RBC theorists of the 1980s and the 1990s chose calibration as their empirical method because they believed that it was the best for measuring the parameters "that characterize preferences, technology, information structure, and institutional arrangements" assumed in their general equilibrium model (Kydland and Prescott 1991a, 168). Kydland and Prescott (1991a, 169) summarized their empirical method as follows:

[36] See Lucas (1973) and several articles in the volume Lucas and Sargent edited in 1981.

[37] This does not mean that there were no estimated RBC models, but rather that calibration was the preferred method used by RBC theorists. Nonetheless, there is a saying that people ascribe to Prescott that embodies the RBC distaste for estimation and support of calibration: "don't regress; progress." Some of Prescott's students tell the story that he had posted this phrase on his office door in Minnesota in the 1970s to tease his then colleague and econometrician Christopher Sims. Confronted with this story, Prescott (email message June 11, 2010) wrote: "My mentor Michael C. Lovell in an interesting paper attributed the quote to me. He told me that I had said it I think in the early 1970s. I like the statement. Measurement without theory has delivered little. As the result of interaction between theory and measurement, great progress has been made and is being made in our science." In fact, this phrase was the epigraph attributed to Prescott in Lovell and Selover's 1994 article, published in the software reviews section of the *Economic Journal* (I could not locate any other reference to this phrase in economic articles available online). Lovell (email message June 11, 2010) explained further: "I don't remember the precise source of 'Progress, Don't Regress.' I don't think it was when we overlapped at Carnegie-Mellon in the 1960's.…Perhaps I picked the quote off of his home page. I looked up his home page just now, and it is not there. I had not heard the office door and Chris Sims story, but it could be true." He then pointed out to me a magazine article published in 2003 by the Minneapolis Federal Reserve Bank, in which it is written: "[some authors] employ a technique Prescott generally scorns: statistical regression. 'Progress, don't regress,' he says with a smile, quoting the slogan featured prominently on his Internet home page" (Clement 2003). Even if this phrase was not on Prescott's office door, it seems nonetheless to have teased Chris Sims (2004), who wrote a critical article on "econometrics for policy analysis" in which he discussed calibration and estimation and used the words progress and regress in the title.

[38] See, for example, Thomas Cooley and Edward Prescott's chapter in Cooley (1995a) and Cooley (1995b). For a thorough discussion of the idealized nature of RBC models, how they fit the data and how they can be tested, see Hoover (1995), Hartley, Hoover, and Salyer (1997), and a symposium published in the *Economic Journal* in 1995 (vol. 105, no. 433). For more on calibration, see also Boumans (2002) and Prescott and Candler (2008).

> The key econometric problem is to use statistical observations to select the parameters for an experimental economy. Once these parameters have been selected, the central part of the econometrics of the general equilibrium approach to business cycles is the computational experiment. This is the vehicle by which theory is made quantitative…The main steps in econometric analyses are as follows: defining the question; setting up the model; calibrating the model; and reporting the findings.

It is important to stress that the opposition between calibration and the use of statistical regressions in macroeconomic models really was not minor. Kydland (1992, 477) implicitly made the case that calibration is better than estimation because "the parameters should not be chosen so as to produce the best fit of the model to the business cycle data. The goal is to provide the clearest possible answer to the question [at hand]. In some cases, deviations of the theory from the data even provide independent verification of the answer." As late as the early 2000s, after looking closely at models used in several central banks, Sims (2004) argued that the probability approach to macroeconomic models had not yet disappeared but "is under siege" (170). He then mentioned the existing "dispute today between econometricians and 'extreme calibrationists'" (171) explaining that:

> By the latter I mean economists who would claim that calibration, i.e. inference without formal appeal to probability-based statistical methods, is not just an occasionally, arguably, necessary expedient when probability-based inference is too complicated, but instead an improved replacement for probability-based inference.

A second distinction between new classical and RBC theorists is the absence of money in most RBC models, which thus focus on the real sources of fluctuations: among these sources Kydland and Prescott (1991b) estimated that technological shocks (summarized in the Solow residual) explain about seventy percent of US business cycle fluctuations in the period after the Korean War. For instance, Lucas (1998, 129) when asked about the differences between Friedman, Tobin and himself on the one hand, who thought (according to Snowdon and Vane, the interviewers) of the economy as fluctuating around a long-run smooth trend, and RBC economists like Prescott and Kydland on the other hand, answered that the difference was in their view of the sources of the trend:

> Well, they [Kydland and Prescott] talk about business cycles in terms of deviations from trend as well. The difference is that Friedman, Tobin and I would think of the sources of the trend as being entirely from the supply side and the fluctuations about the trend as being induced by monetary shocks. Of course, we would think of very different kinds of theoretical models to deal with the long-run and the short-run issues. Kydland and Prescott took the sources that we think of as long-term to see how well they would do for these short-term movements. The surprising thing was how well it worked. I am still mostly on the side of Friedman and Tobin, but there is no question that our thinking has changed a lot on the basis of this work.

The gist behind the RBC position of explaining fluctuations on the basis of real factors lies in two "facts" reported by Kydland and Prescott (1990): first, that prices are countercyclical, rather than procyclical, suggesting that movements in prices are associated with shifts of the aggregate supply function along a given aggregate demand function. The second fact is that monetary aggregates (such as monetary base or M1) do not lead the cycle, a view that was contrary to the prevailing understanding that they do lead it, associated with the works of Milton Friedman and Anna Schwartz (1963) and Chris Sims (1972, 1989). Both facts went against "the prevalence in the 1970s of studies that use equilibrium models with monetary policy or price surprises as the main source of fluctuations," i.e. new classical models (Kydland and Prescott 1990, 7). Although Kydland and Prescott (1991a, 176) affirmed that monetary shocks "are a leading candidate to account for a significant fraction of the unaccounted-for aggregate fluctuations" (that is, fluctuations not explained by technology shocks), and that they foresaw the coming of an RBC model with money, the first RBC models did not have room for money and thus, could in no way analyze monetary policy and inflation stabilization.[39] 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).

The differences in the sources of economic fluctuations between new classical and RBC theoreticians can be seen with crystal clarity in two letters that Lucas and Prescott exchanged in 1990. In the early 1990s Lucas was working with Michael Woodford on a paper in which unanticipated changes in nominal spending flows imply a less-than-proportional adjustment in price levels (i.e., these shocks have real effects).[40] After reading a draft sent to him by Kydland and Prescott, Lucas sent his comments to Prescott (copying Kydland as well) and wrote:

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[39] Nonetheless, in the early 1990s these economists expanded their agenda to include things like money, heterogeneous agents, and imperfect competition, for instance (see Cooley 1995a).

[40] Lucas wrote the first draft alone in 1989 (see draft "The Effects of Monetary Shocks When Prices are Set in Advance," November 1989, in folder "FIXP / BANK," Box 1; Robert E. Lucas, Jr. Papers, Rare Book, Manuscript, and Special Collections Library, Duke University). In the introduction he wrote not only that this model, resembling Lucas (1972), was designed to be "consistent with the centuries old observation, documented most recently and comprehensively by Kormendi and Meguire (1984), that increased monetary instability is associated with increased real instability" (1), but also that "in its reliance on nominal prices that are set in advance, the model in this paper is similar to those of Fischer, Phelps and Taylor" (2). These three papers are part of the new Keynesian literature that advocated the non-neutrality of money in the short run. Woodford joined this project in the second semester of 1990, but their paper has not been published since then (there is a version available as NBER working paper 4250, January 1993, and a 1994 revised version downloadable from Woodford's webpage). There is a curious letter from Lucas to Lars Svensson, on March 20, 1990. Svensson made comments on the paper by Lucas ("The Effects of Monetary Shocks When Prices are Set in Advance") and sent reprints of his papers with sticky-price models, including one that he presented in the same session where Lucas had presented a paper at the "Econometric Society World Meeting in Cambridge around 1985." Lucas then replied (emphasis added): "My apologies for not relating this work to yours on sticky prices, which you had sent me long ago in working paper form. At that time [mid 1980s], *I was so hostile to the idea of pre-set prices* that I simply filed your paper unread and forgot about it!" However, in 1979 he had drafted a paper (titled "Price Fixing") whose introduction shows a Lucas who believed that there was clear evidence that prices do not adjust instantaneously (Folder "Price Fixing," Box 1). Here Lucas talks about "price fixing among sellers" being a common practice "dating at least back to [Adam] Smith" (p. 1). However, this did not mean that Lucas favored the way price stickiness was treated in the so-called disequilibrium macroeconomics. In a report to Henri Theil in 1978, Lucas was uneasy with Negishi's book proposal in which, as in the works of Barro and Grossman and Malinvaud, he attempted "to obtain 'Keynesian' results from a general equilibrium model by arbitrarily fixing some prices." Lucas concluded: "I am not very sympathetic to this line—I think it assumes away the difficult and most crucial issues—and have not followed

> I don't agree with your remark that "persistence" is a difficulty with monetary-shock business cycle theories. Monetary shocks must work because people react to them as if they were taste/technology shocks, because they can't tell the difference or because they are locked in to certain decisions. If so, then *any theory of the persistence of the consequences of actual taste/technology (like yours and Finn's) should be adaptable without change to monetary shocks*. If an investment project, say, is initiated in your model, this has consequences far into the future that should be independent of why the project was initiated. *In Frisch's language, I think the hard part of a monetary theory is getting a coherent picture of the impulses*.[41]

Prescott then replied (also copying Kydland) that he sticks to what he and Kydland have argued in the paper and that they originally had money in the model but decided to drop it because monetary shocks could not generate persistent real effects:

> I stick with my position that *a problem for monetary shock theories of business cycle fluctuations is the lack of a propagation mechanism*. So far, only relatively persistent changes in factors that affect steady state of the deterministic model have been shown to induce business cycle type fluctuations. Time-to-build, staggered contracts, and/or capital accumulation do not provide a mechanism that propagates nonpersistent shocks, (whether they be technology, monetary, preference or something else), in such a way that business cycle type fluctuations are induced. *Incidentally, we dropped the monetary example from the paper*.[42]

There is yet another letter from Lucas to Prescott earlier in the same year in which Lucas showed on the one hand his admiration for the RBC research agenda that he was then teaching to his graduate students, but on the other hand stressed that money for him was an important element missing in RBC macro models:

> My first year macro course is now over half devoted to work by you and your students (here I count Kydland and Cooley, as well as your Minnesota protégés), and if you ever come around to taking money seriously again, I suppose that will go to 100 percent![43]

A third difference between new classical and RBC approaches is the existence of imperfect information in the earlier new classical models and its non-existence in the first RBC models. Lucas's island model (following a metaphor proposed by Phelps 1970, 6-10), for example, makes the imperfect information work as a mechanism propagating the shocks and thus generating serially correlated movements in real output. In contrast, RBC macroeconomists initially developed equilibrium models under perfect competition and information. Consequently, these models "add relatively little to the pattern of fluctuations in real output beyond what is implicit in the technology

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it very closely. Because of this I cannot judge its contribution to this literature very fairly." (Letter from Lucas to Theil, Feb. 24, 1978, folder "Correspondence - 1978-1," Box 30).

[41] Emphasis added. Letter from Lucas to Prescott, October 22, 1990, folder "1990 Correspondence," Box 26; Robert E. Lucas, Jr. Papers, Rare Book, Manuscript, and Special Collections Library, Duke University.

[42] Emphasis added. Letter from Prescott to Lucas, November 1, 1990, folder "1990 Correspondence," Box 26; Robert E. Lucas, Jr. Papers, Rare Book, Manuscript, and Special Collections Library, Duke University.

[43] Letter from Lucas to Prescott, May 18, 1990, folder "1990 Correspondence," Box 26; Robert E. Lucas, Jr. Papers, Rare Book, Manuscript, and Special Collections Library, Duke University.

shocks themselves" as Hartley, Hoover and Salyer (1997, 46) discussed (and as Prescott explained in a previous quote).

3.2 The New Keynesians: Fighting the Opponents and Talking to Them

The new Keynesians wanted to study monetary economies in which nominal rigidities and market failures make fluctuations costly and therefore open the door for stabilization policies. Imperfect competition is the typical environment to get all these elements in place. Moreover, the new Keynesians initially identified monetary shocks as the major source of economic fluctuations, in contrast to the RBC theorists, and they argued that economic (monetary) policy affects real output in the short run as opposed to both the new classical and RBC economists (in the new classical world only unanticipated shocks can affect real variables). Moreover, new Keynesians brought unemployment and the non-neutrality of money in the short run back to macroeconomics. In terms of empirical research, they favored estimation techniques (often equation-by-equation) instead of calibration.

Mankiw (1989, 79) summarized very well in the first paragraphs of his article the major differences between the "classical school" (especially the RBC theory, which share some elements with the new classical macroeconomics) 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, according to this theory, the natural and efficient response of the economy to changes in the available production technology.[44]

Mankiw then stated that he does not believe that real business cycle theory offers an "empirically plausible explanation of economic fluctuations" (79). Moreover, the "facts" that RBC macroeconomists used to criticize the new Keynesians did not serve as conclusive evidence to

[44] Greenwald and Stiglitz (1993, 24) also stressed the differences between the two groups despite their agreement upon two methodological premises -- founding macroeconomics on microeconomic principles and the use of simple general equilibrium models. The authors argued that while RBC and new classical economists "base their theories on simple (we would say simplistic) models of markets that employ perfect information, perfect competition, the absence of transactions costs, and the presence of a complete set of markets.…In contrast, modern Keynesians have identified these real world 'imperfections' as the source of the problem: leaving them out of the model is like leaving Hamlet out of the play."

refute these models. For instance, Ball and Mankiw (1994) examined the major criticisms raised against the new Keynesian agenda and refuted them as being unconvincing: to take one example, the countercyclicality of the price level, for these authors, is an artifact produced by the RBC practice of detrending data with the so-called Hodrick-Prescott filter.[45]

Yet another important illustration of the schism between new classical/RBC macroeconomists and the (new) Keynesians is given by Ball and Mankiw's 1994 article from the saltwater camp and, from the freshwater flank, Lucas's comments on it. Ball and Mankiw (1994, 127) stated that there are two kinds of macroeconomists, those who are "part of a long tradition in macroeconomics," which includes "John Maynard Keynes, Milton Friedman, Franco Modigliani, and James Tobin," who believe "that price stickiness plays a central role in short-run economic fluctuations," and those who do not, whom the authors labeled as "heretics." For the authors "a macroeconomist faces no greater decision than whether to be a traditionalist or a heretic" (128).

On the other hand, Lucas (1994, 154) reproached Ball and Mankiw's characterization of the "heretics" as "'silly' people, 'almost pathological'," on the grounds that "the cost of [this] ideological approach…is that one loses contact with the progressive, cumulative science aspect of macroeconomics." He deplored Ball's and Mankiw's following "the tradition of argument by innuendo, of caricaturing one's unnamed opponents, of using them as foils to dramatize one's own position" and asked that they put all this behind them "and return to the research contributions we know they are capable of making" (155). What is interesting is not only Lucas's call to pay attention to the cumulative knowledge produced in macroeconomics by both freshwater and saltwater camps, but also his criticism of Ball and Mankiw for not perceiving that a "synthesis of old and new ideas" might occur and thus "might leave us better off" (155).

Therefore, despite the major points of disagreement among the new classical and RBC macroeconomists, on the one hand, and the new Keynesians on the other hand, these two camps shared significant methodological and theoretical grounds: they all adopted the rational expectations hypothesis, favored general equilibrium models with microfoundations, and had in their benchmark models a representative agent in an environment of complete markets and complete information. It is true that, for instance, the new Keynesians George Akerlof and Janet Yellen (1985) introduced small deviations from rationality at the individual level that generated significant aggregate effects. Nonetheless, the rational expectations hypothesis was the benchmark from which to deviate.

With respect to the fact that both new classical/RBC and new Keynesian macroeconomists assume rational expectations, it is important to stress that there was a domestication of this hypothesis by mainstream macroeconomists. Initially, in the 1970s, the new classical economists were known as "rational expectationists" (only later were they labeled "new classical") and the

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[45] Hartley, Hoover, and Salyer (1997, 46-8) provided a broader discussion of the problems associated with the way RBC macroeconomists detrend the data.

rational expectations hypothesis was understood to make economic policy ineffective (i.e., only policy surprises could affect real variables like output). This ineffectiveness went against the Keynesian understanding that stabilization policies can be used systematically over the cycle, which made Keynesians oppose rational expectations initially.

The identification of rational expectations with policy ineffectiveness occurred not only in academia but also appeared in magazines like *Business Week* and *Newsweek*. On November 8, 1976 (p. 74), *Business Week* published an article titled “How Expectations Defeat Economic Policy,” which started by announcing that “a controversial new theory called rational expectations is sweeping through the economics profession. It says that economic policy is impotent” because “the public…takes actions that offset [systematic policy changes].” The article continued by stating that “the work of ivory-towered economists Robert E. Lucas Jr. of the University of Chicago and Neil Wallace and Thomas Sargent of the University of Minnesota is giving [Milton] Friedman’s [critique of policy fine-tuning] something it lacked for two decades—a solid theoretical base.” Moreover, the article explained that monetary policy is where “the rationalists are making major breakthroughs:” for it to generate real effects it “must come from out of the blue.…Says Lucas: ‘To affect real output, the monetary authorities must resort to trickery, and how long can you keep pulling that off?’ And Wallace adds: ‘For countercyclical policy to work, it must surprise people, and that’s not a policy, that’s throwing dice.’…Throwing dice is a dangerous game.…[that] increases uncertainty. Uncertainty damps economic activity.” The article also featured the dissenting voices of the time, such as those of Benjamin M. Friedman (Harvard University), Robert E. Hall and Franco Modigliani (MIT), who argued that it takes time for people to adjust their expectations, which means that monetary policies have real effects in the short-run. They agreed with “rationalists” on some issues, but did not accept that the monetary effects would be incorporated into prices fully and instantaneously. “Minnesota’s Sargent and Wallace remain unconvinced. They maintain that time lags in the system are a thin reed on which to hang the success of stabilization policy.” Finally, monetarist Allan H. Meltzer argued for requiring more empirical evidence before, according to his own words, “we can know that rational expectations cripple stabilization policy.” It is important to note that Lucas was pleased with this article.[46]

About two years later, *Newsweek* (June 26, 1978) featured an article titled “The New Economists,” which started: “Most of their names are still obscure, and their work is little known to the public. But in scholarly crannies across the country, a new breed of economists is emerging.” It said that this new generation was then laying the intellectual foundations of a national political shift

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[46] Lucas wrote a letter to Seymour Zucker of *Business Week* showing his satisfaction with the published article: based on “our long and (I thought) confusing conversations, I was dubious that anything coherent would emerge. The article was thus a pleasant surprise.” Letter from Lucas to Seymour, November 11, 1976, folder “Correspondence-1976,” Box 31; Robert E. Lucas, Jr. Papers, Rare Book, Manuscript, and Special Collections Library, Duke University.

to the right: "All of them are basically conservative, though some bristle at the label," but "they are not necessarily conventional political conservatives. Instead they have been inspired by the seeming impotence of Keynesian economic theory." It then went on to say that this group varies ideologically "from a variety of monetarism which spurns activist government intervention in the economy to a pragmatic 'neoclassicism' that accepts some government action but is profoundly critical of past policies." The article then addressed the association of rational expectations to policy ineffectiveness:

> Thomas Sargent represents a more radical branch of the new economists. He and University of Minnesota colleague Neil Wallace, 38, both advisers to the Federal Reserve Bank of Minneapolis, and Robert Lucas, 40, of the University of Chicago, are part of the "school of rational expectations"—a monetarist branch that, says one economist, "out-Friedmans Milton Friedman." In essence the rationalists maintain that the government is impotent in the economic sphere.
> Sheils and Thomas (1978, 59)[47]

Another similar but shorter article was published in 1978 in the newspaper *The Minneapolis Star*, titled "Rational people may be economy's thorn," and collected by Lucas.[48] The article started by asking: "Are you a rational person?" Then it explained the theory that "is almost embarrassingly simple:" the public anticipates a coming policy and acts in a way that eliminates its real effects; the final outcome is only a change in inflation. It adds:

> To call this conclusion radical is an understatement. It challenges views on how to run the economy that have held sway in this country since World War II. And adherence to those views is precisely why we have both high inflation and high unemployment, the rationalists hold.
> Greenwald (1978)

Over time, the "sweeping implications" of the rational expectations hypothesis (Mankiw in Snowdon and Vane 1995, 52), as first believed, were dismissed.[49] New Keynesians could then embrace the rational expectations cause more freely. Alan Blinder (1989, 104, emphasis added) nicely summarized this point:

> It took a while, and some help from Fischer (1977) and Phelps and Taylor (1977), for the profession to get clear that rational expectations (RE) is an assumption about behavior which may be right or wrong but which is logically disconnected from the hypothesis that prices move instantly to clear markets. It is more from the latter than

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47 Sargent and Lucas seemed to embrace fully this missionary activity of advocating rational expectations as the policy ineffectiveness result. It is interesting that this article in *Newsweek* was translated into Spanish and published in a newspaper (not identifiable), which Sargent sent to Lucas with the following undated note: "Dear Bob, I thought you might be interested in learning of Kareken and Wallace's missionary activities. Tom" (folder "1978-2," Box 30; Robert E. Lucas, Jr. Papers, Rare Book, Manuscript, and Special Collections Library, Duke University).

48 Folder "Correspondence-1978-1," Box 30; Robert E. Lucas, Jr. Papers, Rare Book, Manuscript, and Special Collections Library, Duke University.

49 As Blanchard (1992, 123) put it in terms of his preferred understanding of fights and scientific progress: "Initial fights were about the appropriateness of the assumption of rational expectations, as the assumption seemed so damaging to mainstream macroeconomics. But, by the late 1970s, regrouping had occurred, and progress happened in two phases:" the integration of supply shocks and rational expectations, and an analysis of the structure of markets.

> from the former that the new classical economics (NCE) derives its distinctive implications [regarding policy ineffectiveness].
>
> *Separating those two ideas helped spread the RE gospel*, since formal econometric tests of the joint hypothesis of RE and market clearing almost always rejected it. Most economists had a strong suspicion that the market-clearing hypothesis was the weak link in the partnership.[50]

The hypothesis of rational expectations was one of a series of elements shared by new classical/RBC and new Keynesian theorists (others were: general equilibrium models, microfoundations, a representative agent, complete markets and complete information). Microfoundations was the game that the new Keynesians also wanted to play in order to resuscitate Keynesian macroeconomics by supposedly making it immune to the Lucas critique. In fact, Mankiw (1992b) wrote about "the reincarnation of Keynesian economics" after Lucas's announcement that Keynesian economics was dead:

> From our current perspective, it is clear that this obituary was premature. Today Keynesian theorizing does not inspire whispers and giggles from the audience.…If Keynesian economics was dead in 1980, then today it has been reincarnated.
>
> …Yet one can say that the new classical challenge has been met: *Keynesian economics has been reincarnated into a body with firm microeconomic muscle*. Mankiw (1992b, 559-560, emphasis added)

Not all new Keynesians took the call for microfoundations as a justification for the use of a representative agent in their general equilibrium models. Although the new Keynesian work that became part of the new synthesis (as the RBC counterpart) was characterized by that conceptual tool, there were members of this group who subscribed to microfoundations but were against a model with a representative agent in which "problems of coordinating prices and wages simply cannot be studied" (Greenwald and Stiglitz 1993, 42). These were new Keynesians, like Stiglitz, who were interested in studying asymmetric and costly information and associated market failures.[51]

Besides this, one might argue against the view that both groups (new classical/RBC and new Keynesians) shared important methodological elements by noting that Mankiw (1985) and other new Keynesians made a static partial equilibrium analysis instead of using the general equilibrium models for which new classical and RBC theorists were well known. However, Mankiw (1985, 536) explicitly mentioned that it was possible to construct simple general equilibrium models in which his results would not only hold but also would be more pronounced. Moreover, static analysis was just a first step in explaining how nominal rigidity worked in their models; such work was understood to be "largely complete" in the early 1990s (Ball and Mankiw 1994, 137) and the

[50] Blinder would later repeat similar words in an interview with Snowdon (2001, 113). Greenwald and Stiglitz (1993, 41) also observed that the policy ineffectiveness result depends on instantaneous market clearing rather than on rational expectations.

[51] See also Stiglitz (1992) for a thoughtful discussion of his views on microfoundations and asymmetric information.

challenge of the new Keynesians at the time was to construct quantitative general equilibrium models in line with the RBC literature.[52]

Those central elements common to both the new classical/RBC and the new Keynesian camps (rational expectations, general equilibrium monetary models, representative agent, complete markets and complete information) allowed them to communicate, trade ideas and evidence, and negotiate new empirical findings. This way they resolved most of the previously listed points of disagreement that they then turned into core features of the new consensus macroeconomics.

4 Trading in the Triangle and Synthesizing Macroeconomics

In the early 1990s, the merger of the new Keynesian and RBC models into "some grand synthesis that incorporates the strengths of both approaches" was "just a hope."[53] By the end of that decade it was a reality. Goodfriend and King coined the term new neoclassical synthesis in 1997 when such a synthesis was yet developing: "macroeconomics is moving toward a *New Neoclassical Synthesis*" (231).[54] According to them, the main features of this new consensus macroeconomics were 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).[55]

In that same year Robert Solow, John Taylor, Martin Eichenbaum, Alan Blinder, and Olivier 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 into practice. Solow (1997, 230) asserted that part of the common core of macroeconomics consists of: (a) "trend movement [that] 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"; and, (b) fluctuations around the trend that "are predominantly driven by aggregate demand impulses" best studied with "some model of the various sources of expenditure." Solow recognized that there was some dissent from proposition (b). He explicitly denied the RBC explanation of fluctuations as supply-driven,

[52] This was the opinion voiced by Greenwald and Stiglitz (1993, 35) and Mankiw (in Snowdon and Vane 1995, 56). For Lucas (1998, 130-131), this challenge was still unsettled by the end of the 1990s.

[53] These are Mankiw's words in an interview he gave to Snowdon and Vane in 1993, published later (Snowdon and Vane 1995, 60-61).

[54] Lucas (1998, 130) also saw the synthesis coming. Clarida, Galí, and Gertler (1999) observed that the resurgence of interest in monetary policy was based on both the consensus that empirically monetary policy affects the real economy and on an improvement in the theoretical framework used for policy analysis: one that "incorporated the techniques of dynamic general equilibrium theory pioneered in real business cycle analysis" together with "the explicit incorporation of frictions such as nominal price rigidities" (1661-1662).

[55] 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 argued: (i) give practical macro policy advice; (ii) short-run price stickiness is the major source of economic fluctuations; and, (iii) macro models need microfoundations.

and then commended 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 discuss what is the best way to model such demand-side forces (230).[56] Solow also voiced his disapproval for assuming rational expectations in modeling a 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 recognized that his core of macroeconomics lacked "real coupling between the short-run picture and the long-run picture" (231-2).

Taylor (1997) defined macro as the study of both economic growth and fluctuations and identified a practical core in this field that "is having beneficial effect on macroeconomic policy, especially monetary economics" (233). He then listed five key principles of such a core: (1) long-run growth depends on movements along as well as shifts of a production function (this principle 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 stated that the rational expectations approach is "the most feasible empirical way to model this response" (234); and, (5) evaluating monetary and fiscal policy required thinking in terms of "a series of changes [in instruments] linked by a systematic process or a policy rule" (234).

Blinder (1997) basically followed the line of Taylor and emphasized the utility of such a core in terms of policy analysis, "where contact with reality is a necessity" (240). Eichenbaum (1997) approached the question of the existence of a core in macroeconomics from the perspective of stabilization policy. He then stressed that macroeconomists converge mostly in terms of method. However, they also agree on principles such as those 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"; and, (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 will be discussed later. Finally, Blanchard (1997a) identified only two propositions: (1) short-run movements in economic activity are driven by aggregate demand; and, (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

[56] Eichenbaum received his PhD from the University of Minnesota in 1981, under Thomas Sargent (who received his PhD from Harvard in 1968).

important differences in the key elements of the new consensus, as just discussed. Nevertheless, the majority of these economists agreed not only on the explicitly stated or implicit methodological elements (dynamic general equilibrium models with rational expectations and a representative agent), but also with most of the central principles of such a core of usable macroeconomics. Among these, two are noteworthy: short-run fluctuations are demand driven; and real disturbances are often inefficient—with the degree of inefficiency being a function of the monetary policy response to such disturbances. Thus, monetary policy is non-neutral in the short run as a consequence of nominal rigidity. Clearly, the original advocates of RBC models could not be included in this discussion and provide policy recommendations because their model had no role for money. These models also 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 without considering himself an economist outside the mainstream.[57] The reason was his insistence, first, on using different models for analyzing short and long-run movements in economic activity and, second, on rejecting use of the rational expectations hypothesis in short-run analyses. Later, he commented on Chari and Kehoe's 2006 article (Solow 2008) and repeated his longstanding criticism of using a representative agent in macroeconomics. His comments were not well received by the authors:

> Solow eloquently voices the commonly heard complaint that too much of modern macroeconomics starts with a model with a single type of agent. In our response, we clarify that modern macroeconomics does not end there—and may not end too far from where Solow prefers.…Solow seems to think that using that sort of model requires ignoring all the rich heterogeneity which he sees in the modern economy. While that may have been true many years ago, today it is not.
> Chari and Kehoe (2008, 247).[58]

By the turn of the century the new synthesis more clearly was a reality. According to Stanley Fischer, one of the evolutions that characterized macroeconomics in the early 2000s and made him very happy, "is the beginning of the end of the great split between freshwater and

57 Before he raised once again his criticisms of modern macroeconomics for offering no guidance or insight about the recent crisis and the deep and prolonged recession that many developed countries experienced, Solow (2010, 1) explicitly warned: "Before I go on, there is something preliminary that I want to make clear. I am generally a quite traditional mainstream economist. I think that the body of economic analysis that we have piled up and teach to our students is pretty good; there is no need to overturn it in any wholesale way, and no acceptable suggestion for doing so."

58 In a recent survey, Heathcote, Storesletten, and Violante (2009, 320) made a similar point: due to improvements in numerical methods and faster computers, macroeconomists were now able "to study rich heterogeneous-agent models." They "reached several conclusions about the importance of including household heterogeneity in their models" (320). They recognized that "macroeconomics is expanding from the study of how average values for inputs…and outputs…of production are determined in equilibrium to the study of how the entire distribution of these variables across households is determined" (321). While this broad trend is observable (but leaving open the question of how much this route will be explored by many economists), Solow's point is still valid because the consensus in macroeconomics is one in which the representative agent reigns to this day, despite Chari's (2010, 3) opinion that "any claim that modern macro is dominated by representative agent models is wrong."

saltwater economics. Although the split is still evident, convergence is also clearly under way. And I think that is very healthy for the profession" (interview with Blanchard 2005, 257).

It is important to keep in mind that macroeconomists from both camps, RBC/new classical and new Keynesian, had to negotiate theoretical arguments in the face of growing empirical evidence. These negotiations led to a consensus. Clearly, negotiations can happen only among those who speak the same language and share core theoretical elements. Roughly, the new Keynesian camp had the goal of building dynamic general equilibrium models, then typical of the real-business-cycle literature, with nominal rigidities and other market imperfections.[59]

On the other hand, the proponents of the RBC (and new classical) models were criticized empirically and had to handle growing evidence that monetary policy has real effects on the short-run—implying that they had to go beyond their general equilibrium models with flexible prices and technology shocks.[60] In fact, evidence on price stickiness also came from international data, as Krugman (2000, 39) noted: in the 1980s advanced countries reduced their inflation rates while they experienced wild fluctuations in nominal exchange rates and the implied co-movement of nominal and real exchange rates had to be explained by price stickiness in those countries.[61] In the end, there was a significant change in attitude by some people in the new classical/RBC camp towards price stickiness: after all, they became more comfortable with considering price rigidities as important to fluctuations because they could discuss this issue in a dynamic general equilibrium framework.[62] Therefore, we see that the interplay of opposing forces on both sides was important in the construction of the new synthesis, as well as the existence of "moderates on both sides of the fence," who had done "a little converging; stridency [came] from the extremes" (Solow 2000, 154).[63]

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59 As a result of using dynamic general equilibrium models, these economists became more concerned over time with the discussions about commitment and credibility that they had inherited from the RBC literature.

60 See for instance Greenwald and Stiglitz (1993, 39-41) and Hartley, Hoover, and Salyer (1997), as well as references therein for the major empirical criticisms of real business cycle theories. Sims (1992) challenged RBC modelers to reproduce the multivariate time series facts that he presented in the article and claimed that data are imposing theoretical changes (996).

61 See Obstfeld and Rogoff (1996, chap. 9) for further discussion of price stickiness in international data and open economy models.

62 As Hoover (2006, 144) noted, "even the founders of the new classical macroeconomics, such as Lucas and Sargent, have to come to see that the assumption of sticky prices is essential if models have any hope of capturing observed economic behavior." However, in the 1970s and 1980s the attitude of most new classical/RBC macroeconomists was against price stickiness. As the Keynesian Alan Blinder (interview with Snowdon 2001, 124) pointed out: "The parts of macroeconomics that took off in the 1970s and 1980s were those based on denying sticky prices. The attitude of new classical and real business cycle theorists seemed to be…'if you don't have a coherent theoretical explanation of sticky prices then it cannot be true that prices are sticky'. Personally I found that approach unscientific." Nowadays the often used Calvo price setting would hardly qualify as a "coherent explanation of sticky prices." Even if we take into account that its use is justified for simplifying the model (because it reduces the number of state variables) and for delivering results similar to better models of price stickiness, the wide adoption of Calvo pricing in the consensus macroeconomics is a sign that the opposition to which Blinder referred really weakened over time.

63 Solow (2000, 155) classified Christiano and Eichenbaum as moderate RBC macroeconomists: "My Reading is that the work of Christiano, Eichenbaum and others has moved in the direction of incorporating frictions and imperfections into the real business cycle framework. The result sounds a little more like the observed economy."

Many mainstream macroeconomists believe that the main advantage of such a synthesis is that it bridges "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 dynamic stochastic general equilibrium (DSGE) models that explain not only the evolution of the potential output over time as a mostly supply-side phenomenon, but also short-run and inefficient deviations of the actual output from its "natural" level (the level achieved if prices were flexible) that arise as a consequence of rigidity in wages and prices.[64]

Another very important methodological point of convergence is the empirical approach macroeconomists now use. As previously argued, in the 1980s and early 1990s the real-business-cycle theorists and the new Keynesians were almost in opposite camps: the former defended calibration methods and were against econometric estimation while the latter favored estimation methods.[65] In the new synthesis no such divide exists. As Chari and Kehoe (2008, 248) aptly described, modern mainstream 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." Today not only is there no conflict between calibration and estimation, but both strategies are used complementarily: current practice calibrates a subset of the parameters (those about which economists have more information) and estimates the remaining parameters via likelihood or Bayesian methods.[66]

The other points of disagreement between RBC and new Keynesian economists already mentioned were real versus nominal shocks as sources of fluctuations, the relevance of monetary policy, and the assumption of flexible or sticky prices and wages (perfect versus imperfect competition). Just to give a more vivid example of how divided macroeconomists were at one time about the sources of fluctuations, I take Lucas's insistence that money matters, contrary to what RBC economists would advocate. Lucas (1998, 125) classified himself as an "old-fashioned monetarist," someone who assigns an important role to monetary forces in economic fluctuations. He then added, at the time a consensus had already emerged, that from an econometric perspective, money did not account for most fluctuations in the postwar period:

> There is the real business cycle theory which assigns no importance to monetary forces. This work has been hugely influential, on me as well as on others, although I

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[64] See Duarte (forthcoming) for a detailed discussion of DSGE models.

[65] With hindsight, Woodford (2000, 27) commented that "the rejection of traditional econometric methods by the early RBC literature has surely been overdone."

[66] See Christiano, Eichenbaum, and Evans (2005, 15-17), who followed Rotemberg and Woodford (1997), 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. Even though Bayesian methods in principle can be used to estimate all parameters of a model, in macroeconomics it still is common to calibrate a few parameters that are either hard to estimate or, more importantly, are not identified (see for instance Frank Smets and Raf Wouters 2007).

> still think of myself as a monetarist. Then there are those whom Sargent calls 'fiscalists'…Then there are old-fashioned monetarists, which is where I would class myself, with people like Friedman and Allan Meltzer.…I used to think that monetary shocks were 90 per cent of the story in real variability and I still think they are the central story in the 1930s. But there is no way to get monetary shocks to account for more than about a quarter of real variability in the post-war era. At least, no one has found a way of doing it.[67]

The current consensus is that real shocks account more for the variability of major macroeconomic variables than monetary shocks: for example, Altig *et al.* (2005) showed 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 can be attributed to monetary policy shocks.[68] These numbers can vary from model to model as there are still issues regarding the proper identification of shocks but they give the general picture that real shocks are more important in terms of explaining the variance of real variables.[69] Nonetheless, this result does not mean that monetary policy is irrelevant in explaining economic fluctuations, as stressed by Woodford (2009, 11): not only the existence, uniqueness and stability of equilibrium depend on the policies designed, but also "the equilibrium effects of real disturbances depend substantially on the character of *systematic* monetary policy." Therefore, there is in principle scope for monetary policy design to 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 in major real variables, they still are useful for discriminating among alternative models: nowadays it is common to have papers introduce all kinds of rigidities (such as habit persistence on consumption, price and wage rigidities, adjustment costs on investment, capital utilization, etc.) that serve to smooth the dynamic responses of aggregate variables after a monetary shock. This is done for the models to be able to reproduce the effects of a monetary policy shock identified in the data—so it is not properly an empirical test of these models but rather a reverse engineering strategy to have the models account for features of the data that macroeconomists consider relevant.[70]

Therefore, we can see that the major issues that formerly divided mainstream macroeconomists no longer do so. Nowadays a consensus also has emerged among these

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67 In fact, this association of the new classical economics, particularly Lucas, with monetarism was part of an intense debate in the early 1980s. Hoover (1988, chap. 9), largely reproducing his 1984 article in the *Journal of Economic Literature* (vol. 22, no. 1), clarifies the relationship between monetarism and the new classical school and provides references to that earlier literature.

68 It is interesting to notice the central role played by shocks in modern economics. Duarte and Hoover (2011) analyze how the meaning of shocks has transformed since the 1930s and how they are used nowadays to observe the business cycle phenomena.

69 See Dupor, Han, and Tsai (2009), Canova and Sala (2009), and references therein.

70 See Duarte (forthcoming). Blanchard (2009, 224) considered the strategy of uncritically introducing such features to reconcile theory with data as "clearly wrongheaded."

economists in terms of stabilization policies, as summarized by Eichenbaum (1997). The common principles that characterize the new synthesis and the fact that these economists believe such principles make their models immune to the Lucas critique give them confidence to apply their models to policy analysis, especially to monetary policy. Woodford's precise motivation in his 2003 book was to show how economists can now discuss monetary policy in practice by drawing on theoretical principles of the new consensus macroeconomics.[71] But Woodford (2003, 11-12) carefully stated that "no attempt is made to set out a model that is sufficiently realistic to be used for actual policy analysis in a central bank. Nonetheless, the basic elements of an optimizing model…are ones that I believe are representative of crucial elements of a realistic model"—and these crucial elements include being free of Lucas's 1976 curse, according to the author. As Hoover (2006, 146) argued, mainstream economists like Woodford present here an "eschatological justification" for constructing simple general equilibrium models (with a representative agent), which are used as guides for policymaking not because they provide an adequate description of the economy, but because they are seen as "the starting point for a series of fuller and richer models that eventually will provide the basis for" such an adequate description.

Going beyond Woodford's (2003) simple model, Christiano, Eichenbaum, and Evans (2005), and Smets and Wouters (2003, 2007) enriched the basic model in several dimensions to make it replicate features of the data that mainstream macroeconomists consider important (as already mentioned, these features are usually summarized in impulse response functions to a monetary policy shock, but there are correlations among variables as well). Both the basic and the larger models are based on representative households and firms. Although there are two types of firms (one that produces an intermediate good and one that assembles the final good), and a continuum of intermediate firms (each producing one of the infinite number of intermediate goods), heterogeneity plays no substantial role in these models.[72] By expanding the basic model and enriching its dynamics, those authors presented the clearest incarnations of quantitative general equilibrium models oriented to policy applications typical of the new synthesis.

As mentioned in the introduction to the present essay, claiming a consensus in macroeconomics—a field often seen as composed of rival schools—has the advantage of making the case for both academic cohesion and scientific progress, as well as for a unified body of knowledge from which to prescribe policies. In his comments regarding Goodfriend and King's

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[71] For a discussion on whether Woodford has succeeded see Hoover (2006), Laidler (2006), Mehrling (2006) and Woodford's (2006) own comments on these papers.

[72] Hoover (2006, 145) stated that although these models have "a measure of stylized heterogeneity...there is no agent-by-agent modeling of the sort that would really qualify as microeconomics." Besides this, the central point is that the focus of the consensus model is on the symmetric equilibrium in which intermediate firms only differ because they do not charge the same price, and in which the dispersion of the relative price does not affect the dynamics of the variables because these models are usually solved with first-order approximation methods (and price dispersion has an effect that is second order and thus is ignored in these methods).

1997 article, Blanchard (1997b) questioned both the labels "new" and "synthesis" on the grounds that the principles behind such a synthesis 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 starts anew "every twenty years or so" and has "little or no common core," but is rather a history of "a surprisingly steady accumulation of knowledge" (Blanchard 2000, 1375).[73] According to the author, macroeconomists differ in the weights they attach to the different ingredients of their models (intertemporal optimization, nominal rigidities, and imperfect competition), but they live in the same world, which he described as being like 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, with its focus on imperfections in the goods and the labor markets. Most of us live somewhere in the triangle. So do Goodfriend and King. Seen 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 trading among economists working in a narrowly defined area, one in which a representative agent was often assumed to answer the Lucas critique in their dynamic general equilibrium models based on optimizing individual agents. Questions about the non-neutrality of money in the long run, the appropriate type of microfoundations, non-market clearing models, and the limitations of assuming a representative agent, for example, are simply not in that triangle and thus, are not central to the new synthesis. In this sense it is not surprising that Leijonhufvud's concern with the ability of a market economy to coordinate the economic activities of decentralized agents, and his "corridor hypothesis," which states that the market mechanism generates coordination only within certain limits but not outside these limits (Leijonhufvud 1973), are both outside the triangle. As he later explained his opposition to Lucas's resolution of the tension between micro and macro:

> That cut the Gordian knot, all right. But why was this path not taken before? Let me give a personal answer: because macroeconomics (I thought) is about system co-ordination, and one should not adopt a method that threatens to define away the main problem. The New Classical economics has the priorities the other way around—and carrying through from individual optimizing behaviour, it all but eliminates the co-ordination problem.
> Leijonhufvud (1992, 28-29)

And he adds that since publishing his 1973 paper, he became interested in behavior outside the corridor and in understanding agents' behavior as the search for or computation of an

[73] But the same author went back to Kuhn's notion of revolution in his 2009 essay, in order to argue that the macroeconomics of the 1970s had exploded and converged to the new synthesis, as previously mentioned. He then wrote: "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).

equilibrium price vector (instead of knowing it, as in a Walrasian general equilibrium model). Because of his interests, he became a macroeconomist outside Blanchard's triangle:

> Since that time, I have been particularly interested in 'out-of bounds' behavior, i.e., in what happens in economics under extreme conditions: hyperinflations, great depressions, transformation from socialism.…
>
> My disenchantment with this [Lucas's] brand of microfoundations…was such that, to be frank, I drifted out of the professional mainstream from the mid-70s onwards, as intertemporal optimization became all the rage.
> Leijonhufvud (1993, 8, 11)

Not surprisingly, Leijonhufvud (2004, 139) saw the new neoclassical synthesis as an enterprise that misses the crucial elements of how the economy works:

> Conceptually, the recent trend you mention, the so-called new neoclassical synthesis, reminds one of the discussions that took place in the 1920's and early 1930's. It's *classical economics with frictions*. Again we have come full circle. But few economists these days want to think or talk about the corridor problem.

It is curious that Blanchard (1997b) chose a triangle to summarize the core elements behind macroeconomics and the new synthesis because it parallels a common manner by which microeconomists geometrically represent preferences under uncertainty in the case of lotteries with three possible outcomes: the so-called simplex has at each vertex a lottery in which one outcome is certain and the other two have zero probability; macroeconomists of the new consensus have theoretical preferences over a two dimensional simplex, Blanchard's triangle.

However, the existence of a consensus in mainstream macroeconomics does not mean either that even within such a 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 vertices. The dissent inside the triangle relates to two major points: first, that the new consensus models are not yet ready for policy analysis because they introduce many shocks that are not invariant to policy (Chari, Kehoe, and McGrattan 2009);[74]and, second, that the development of macroeconomics since the 1970s has emphasized mostly theoretical issues at the expense of practical ones. Therefore, the recent theoretical developments have had little effect on macroeconomists in charge of conducting actual economic policies. In other words, macroeconomists privileged the development of macroeconomics more as a science, devoted to "understand how the world works" (Mankiw 2006, 29-30), than as a type of engineering, concerned with solving practical problems.[75]

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[74] These authors strengthened their criticisms by questioning the existence of a broad consensus in macroeconomics 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).

[75] Mankiw (2006) here reiterated and updated a concern already present in his 1990 article (repeated in his 1992a essay). See Woodford (2009, 275-7) for a criticism of Mankiw's view.

In terms of expanding the triangle or transforming it into a polyhedron, macroeconomists living in the triangle have their lists of improvements ready.[76] Chari and Kehoe (2006, 21-6), in a more standard RBC vein, listed: (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; and, (3) introduce unemployment benefits to understand differences in unemployment between countries.

Mishkin (2007, 27-30) went in the direction of improvements to make monetary policymaking a science more than an art: (1) enrich estimated DSGE models so as to make them more realistic in the eyes of central bankers; (2) improve or extend the way nominal rigidities are usually incorporated into such models; (3) move from models with a representative agent to ones with heterogeneity of agents; (4) incorporate (and understand better the role of) financial frictions; (5) go beyond rational expectations and embed behavioral economics in macroeconomics; (6) introduce learning into macro models; and, (7) keep a sense of 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) echoed 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 listed as new directions of research: (1) substitute Calvo’s time-dependent pricing scheme, in which the timing of adjustment of prices is exogenous, by state-dependent pricing that makes that timing depend on the endogenous evolution of the economy; (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 Galí and Gertler’s points (2) and (3), Blanchard (2009, 216-22) argued that macroeconomists should answer questions of how markups move, in response to what, and why. He also repeated Chari, Kehoe, and McGrattan’s criticism that several shocks in a DSGE model may not be invariant to the policy adopted—but he, in contrast to them, proposed the use of less structural approaches—and urged economists to pay more attention to the role of anticipations, suggesting a departure from rational expectations.

Before concluding this section I would like to discuss in more detail the major criticisms that Solow raised regarding the new synthesis in general and regarding the idea that modern macroeconomics has changed for the better because it is now “firmly grounded on the principles of economic theory” (Chari and Kehoe 2006, 3). Solow (2008) commented on Chari and Kehoe’s 2006 article that started, according to him, with a self-congratulatory phrase and with a statement

[76] 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.

about macro having firm microfoundations. To Solow, the last sentence was "simply false" (243). He then criticized 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 was explicit about not being against the idea that, as a first approximation, "individual agents optimize as best they can," which does not imply that the whole economy "acts like a single optimizer under the simplest possible constraints" (244). He stressed that the Sonnenschein-Mantel-Debreu theorems establish that "the only universal empirical aggregative implications 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 continued, 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 presentation at the 1997 AEA Meeting (stressing his preference for small, tailored, partial equilibrium models), Solow (2008, 245) came up with this 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.

As already mentioned, Chari and Kehoe responded only to Solow's criticisms that they considered to be of substance.[77] They recognized that the challenges facing modern

[77] Chari and Kehoe (2008, 248) wrote: "Analogies about school colors and carrots aside, there does not seem to be much of substance here to argue about." On a side note, it is interesting to note that they published their response as an

macroeconomics are not small but rejected Solow's criticisms of the use of a representative agent and of their claim that macroeconomics is now firmly grounded in economic theory. With respect to the representative agent hypothesis, Chari and Kehoe (2008, 247) stated that modern macroeconomics does not end with such a 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 argued 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 for the 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 [*sic*] invaluable" (247).

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

> 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.

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 grounded on Lucas's call for microfoundations and the use of the representative agent apparatus, among other things.

5. Some Final Thoughts

Nowadays, mainstream macroeconomists assert that there is a methodological consensus in their field to the extent that "there are really no longer alternative approaches to the resolution of macroeconomic issues," as Woodford (2009, 274) argued. The new consensus is perceived to have promoted 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."

NBER Working Paper (13655, Nov. 2007), which had as its title "The heterogeneous state of modern macroeconomics: A reply to Solow." The reply was later published in the *Journal of Economic Perspectives* without a title.

One may wonder whether defining a common area, Blanchard's triangle, in which macroeconomists could trade and narrow disagreements, implies a synthesis by definition, since it leaves outside the triangle those who disagree more widely. There is even something stronger: defining macroeconomics by that area implies, therefore, that dissenters are either not macroeconomists or not scientists to begin with. That was the case of Leijonhufvud, just to give one example, as I argued earlier: disenchanted with intertemporal optimization of rational agents, he drifted away from mainstream macroeconomics. But what is interesting from a historical point of view, as I have tried to argue, is that the definition of that common area is not immutable and that it left some room for disagreement.

It is tempting to consider the role of MIT in promoting the new synthesis. Paul Samuelson had announced in the second edition (1951) of his bestselling textbook *Economics* that economists had worked toward a synthesis. But it was in its third edition (1955) that he coined "one of the most famous phrases in the history of macroeconomics" (Pearce and Hoover 1995, 202): the neoclassical synthesis. The old synthesis vanished with the new classical conquest of macroeconomics in the 1970s and 1980s and nowadays we are back to a synthesis, the new neoclassical one. Although this term was first used by Goodfriend and King (1997), the idea of a new synthesis was welcomed in print by economists like Blanchard, Woodford, Mishkin, Romer and Galí and Gertler (and Mankiw and Fischer in interviews). All these enthusiasts, except Gertler, obtained their PhDs at MIT: Blanchard, Mishkin, Romer and Mankiw were all advised by Stanley Fischer, while Galí was advised by Olivier Blanchard, and Woodford by Robert Solow (officially, by Tim Kehoe de facto). On the other hand, the group of adherents to the new synthesis went beyond MIT graduates and included people like Goodfriend (Brown University), King (Brown University), and Gertler (Stanford University). It is indeed hard to strongly indentify the new synthesis with MIT, as was the case with the old neoclassical synthesis. Nonetheless, the fact that Blanchard and Woodford (and later Galí) were the leading writers of narratives about the evolution of the new synthesis and that they both studied at MIT has nice connotations that suggest that they are perhaps the Samuelsons of the new synthesis, even if unintentionally.

As I have tried to show in this essay, the acceptance of the idea of a synthesis did not mean that mainstream macroeconomists all had the same view and all agreed about which policies to prescribe. The somewhat nuanced views that these economists have about such a consensus reflect a deeper understanding about how their field evolves. Having a synthesis is so much valued by them because they see that, in contrast to microeconomists, they work on a battlefield, with alternative schools constantly debunking one another. A synthesis is understood by them as implying that the fights are over—similar to Samuelson's view of the old neoclassical synthesis, when "90 per cent of American economists have stopped being 'Keynesian economists' or 'anti-

Keynesian economists'" (Samuelson 1955, 260)—and that knowledge progresses at a faster rate. Consequently, a narrative of consensus is a way of boosting credibility, both in academia and in the policymaking arena: if you tell the story of a synthesis that merges the strengths of the competing schools that preceded it, how can one oppose the view that the synthesis means progress?

With respect to the views these macroeconomists have about their field, 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.[78] Moreover, implicitly or explicitly, most accounts are centered on internal progress: new theories improving on older ones by fixing logical and empirical flaws. Empirical facts are central to the construction of a synthesis because they do not go away, as Blanchard said, and therefore they force economists to change their theories and views in order to account for them. Mainstream macroeconomists see the merging of the new classical/RBC and new Keynesian theories as driven by facts: a fight of convincing RBC followers that prices are sticky and that monetary policy matters in the short run. This was possible because both camps shared important methodological and theoretical convictions: they both distinguished rational expectations from the policy ineffectiveness result; they took up Lucas's microfoundational program; and they both, over time, leaned toward quantitative general equilibrium models (usually with a representative agent) that were typical of the RBC approach.

Mankiw (2006), however, favored a bit broader understanding of the evolution of macroeconomics by stressing that the field has a dual role, as previously mentioned: macroeconomics as science, concerned with understanding how the world works, and as a type of engineering that solves practical problems.[79]

While Blanchard (2009) at once cast the new synthesis as a revolution with a 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 identified with the new Keynesian macroeconomics to the extent that most RBC models are simply silent about monetary policy, RBC followers make the case that the new consensus is just a convergence to the use of dynamic general equilibrium models and to "policy recommendations

[78] Transposing Weintraub's (1979, 6) words to this context, mainstream economists need to guarantee that progress and synthesis are logically possible in order to look to the past and tell stories of progress and synthesis in their field: "Writing doctrinal history with an eye to progress and synthesis becomes difficult if progress was absent and synthesis is logically impossible."

[79] Goodfriend (2007) consciously tried to go beyond purely internal histories of macroeconomics by relating theoretical developments to historical events. Mishkin (2007) also went in the same direction.

similar to those made by neoclassical economists like Lucas and Stokey 25 years ago" (Chari, Kehoe and McGrattan 2009, 265). Neither side is willing to accept that they had to concede and change their models since the models have failed in some dimension.

The differing views that mainstream macroeconomists have about the state of their field and about possible areas of improvement should not diminish the degree to which they converged methodologically in studying fluctuations. They all analyze such phenomena usually through a dynamic stochastic general equilibrium model with a representative agent and nominal rigidities, firmly grounded in microeconomic principles. Moreover, several of them agree with Chari (2010, 2) that "any interesting model must be a dynamic stochastic general equilibrium model. From this perspective, there is no other game in town." Therefore, he continued, "a useful aphorism in macroeconomics is: '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.'"[80]

To historians of economics, not only is this consensus an interesting object of study but also the histories about its evolution that already have been produced, mostly by economists:[81] such narratives illustrate that the history of macroeconomics can be used purposefully to organize the present developments in ways that reaffirm the solidness of the new consensus and its policy prescriptions, despite the fact that macroeconomists recognize that further work is necessary to improve the new consensus even more. The challenge of a serious recession such as the one initiated in 2007 may threaten macroeconomics with another period of disarray. Time will tell, as usual.

References:

Akerlof, G. A. (2007, March). The Missing Motivation in Macroeconomics. *American Economic Review* 97 (1), pp. 5-36.

Akerlof, G. A., & Yellen, J. L. (1985). A Near-Rational Model of the Business Cycle, with Wage and Price Inertia. *Quarterly Journal of Economics*, 100 (Supplement), pp. 823-38.

Altig, D., Christiano, L. J., Eichenbaum, M., & Linde, J. (2005). Firm-Specific Capital, Nominal Rigidities and the Business Cycle. *NBER Working Paper* (11034).

[80] Nevertheless, nowadays there are voices in favor of more pragmatism, of exploring ideas with partial equilibrium models, and of facing the pretense-of-knowledge syndrome of DSGE macroeconomists: see for instance Krugman (2000), Colander (2006), Colander *et al* (2008), and Caballero (2010). Even Blanchard (1992) was once more skeptical of the microfoundations of macreoconomics as dictated by Lucas.

[81] It is curious to observe that some of the articles written by economists and constructing such histories are published in major economics journals under the classification of either "general economics and teaching" or "macroeconomics and monetary economics" (as Woodford 2009), but not as "history of economics" (or history of economic thought).

Arestis, P. (Ed.). (2007). *Is there a New Consensus in Macroeconomics?* New York: Palgrave Macmillan.

Arestis, P., & Sawyer, M. (2008). A Critical Reconsideration of the Foundations of Monetary Policy in the New Consensus Macroeconomics Framework. *Cambridge Journal of Economics*, 32 (5), pp. 761-79.

Backhouse, R. E. (2010). *The Puzzle of Modern Economics.* Cambridge: Cambridge University Press.

Ball, L., & Mankiw, N. G. (1994). A Sticky-Price Manifesto. *Carnegie-Rochester Conference Series on Public Policy*, 41, pp. 127-51.

Blanchard, O. J. (1992). For a Return to Pragmatism. In M. T. Belongia, & M. R. Garfinkel (Eds.), *The Business Cycle: Theories and Evidence* (pp. 121-132). Boston: Kluwer Academic Publishers.

____________ (1997a). Is There a Core of Usable Macroeconomics? *American Economic Review, Papers and Proceedings*, 87 (2), pp. 244-6.

____________ (1997b). Comment on Goodfriend and King: "The New Neoclassical Synthesis and the Role of Monetary Policy." *NBER Macroeconomics Annual*, pp. 289-93.

____________ (2000). What Do We Know About Macroeconomics that Fisher and Wicksell Did Not? *Quarterly Journal of Economics*, 115 (4), pp. 1375-409.

____________ (2005). An Interview with Stanley Fischer. *Macroeconomic Dynamics*, 9 (2), pp. 244-62.

____________ (2008). Neoclassical Synthesis., In S. N. Durlauf, & L. E. Blume (Eds.) *The New Palgrave Dictionary of Economics*. Palgrave Macmillan. (available at: http://www.dictionaryofeconomics.com/article?id=pde2008_N000041, accessed Nov. 10, 2009).

____________ (2009). The State of Macro. *Annual Review of Economics*, 1, pp. 209-28.

Blaug, M. (1975). Kuhn versus Lakatos, or Paradigms versus Research Programmes in the History of Economics. *HOPE*, 7 (4), pp. 399-433.

Blinder, A. S. (1989). *Macroeconomics Under Debate.* New York: Harvester Wheatsheaf.

____________ (1997). Is There a Core of Practical Macroeconomics That We Should All Believe? *American Economic Review*, 87 (2, Papers and Proceedings), pp. 240-3.

Boumans, M. (2002). Calibration. In B. Snowdon, & H. R. Vane (Eds.), *An Encyclopedia of Macroeconomics* (pp. 105-9). Edward Elgar.

Brunner, K. (1989). The Disarray in Macroeconomics. In F. Capie, & G. Wood (Eds.), *Monetary Economics in the 1980s.* London: Macmillan.

Business Week. (1976). How Expectations Defeat Economic Policy. *Business Week*, November 8, 74-75.

Caballero, R. J. (1992). A Fallacy of Composition. *American Economic Review*, 82 (5), pp. 1279-92.

____________ (2010). Macroeconomics After the Crisis: Time to Deal With the Pretense-of-Knowledge Syndrome. *NBER working paper*, 16429.

Canova, F., & Sala, L. (2009). Back to Square One: Identification Issues in DSGE Models. *Journal of Monetary Economics*, 56 (4), pp. 431-49.

Chari, V. V. (2010). *Testimony before the Committee on Science and Technology*. Subcommittee on Investigations and Oversight, U.S. House of Representatives, July 20. Washington, D. C. (available at: http://science.house.gov/publications/hearings_markups_details.aspx?NewsID=2916, accessed on Oct. 13, 2010).

Chari, V. V., & Kehoe, P. J. (2006). Modern Macroeconomics in Practice: How Theory Is Shaping Policy. *Journal of Economic Perspectives*, 20 (4), pp. 3-28.

____________ (2008). Response from V. V. Chari and Patrick J. Kehoe. *Journal of Economic Perspectives*, 22 (1), pp. 247-9.

Chari, V. V., Kehoe, P. J., & McGrattan, E. R. (2009). New Keynesian Models: Not Yet Useful for Policy Analysis. *American Economic Journal: Macroeconomics*, 1 (1), pp. 242-66.

Christiano, L. J., Eichenbaum, M., & Evans, C. L. (1999). Monetary Policy Shocks: What Have We Learned and to What End? In: J. B. Taylor, & M. Woodford (Eds.), *Handbook of Macroeconomics* (pp. 65-148). Amsterdam: North-Holland.

____________ (2005). Nominal Rigidities and the Dynamic Effects of a Shock to Monetary Policy. *Journal of Political Economy*, 113 (1), pp. 1-45.

Clarida, R., Galí, J., & Gertler, M. (1999). The Science of Monetary Policy: A New Keynesian Perspective. *Journal of Economic Literature*, 37 (4), pp. 1661-1707.

Clement, D. (2003). European Vacation: Why Americans Work More Than Europeans. *The Region - Banking and Policy Issues Magazine*. December.

Colander, D., (Ed.). (2006). *Post Walrasian Macroeconomics.* Cambridge: Cambridge University Press.

Colander, D., Howitt, P., Kirman, A., Leijonhufvud, A., & Mehrling, P. (2008). Beyond DSGE Models: Toward an Empirically Based Macroeconomics. *American Economic Review, Papers and Proceedings*, 98 (2), pp. 236-240.

Committee on Science and Technology. (2010). *Hearing Charter: Building a Science of Economics for the Real World.* U.S. House of Representatives. July 20. Washington, D.C. (available at: http://science.house.gov/publications/hearings_markups_details.aspx?NewsID=2916, accessed on Oct. 13, 2010).

Cooley, T. F. (Ed.). (1995a). *Frontiers of Business Cycle Research.* Princeton: Princeton University Press.

____________ (1995b). Calibrated Models. *Oxford Review of Economic Policy*, 13 (3), pp. 55-69.

De Vroey, M. (2004). The History of Macroeconomics Viewed Against the Background of the Marshall-Walras Divide. In M. De Vroey, & K. D. Hoover (Eds.), The IS-LM Model: Its Rise, Fall, and Strange Persistence. *HOPE*, 36, pp. 57-91.

De Vroey, M., & Hoover, K. D. (2004). Introduction: Seven Decades of the IS-LM Model, in M. De Vroey, & K. D. Hoover (Eds.), The IS-LM Model: Its Rise, Fall, and Strange Persistence. *HOPE*, 36, pp. 1-11.

Dow, S. C. (1996). *The Methodology of Macroeconomic Thought--A Conceptual Analysis of Schools of Thought in Economics.* Cheltenham, U.K.: Edward Elgar.

Duarte, Pedro Garcia (forthcoming). Recent Developments in Macroeconomics: The DSGE Approach to Business Cycles in Perspective. In Wade Hands and John Davis (Eds.), *The Elgar Companion to Recent Economic Methodology*. Edward Elgar.

Duarte, Pedro Garcia, & Hoover, Kevin D. (2011). Observing Shocks. SSRN working paper, available at: http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1840705

Dupor, B., Han, J., & Tsai, Y.-C. (2009). What Do Technology Shocks Tell Us About the New Keynesian Paradigm? *Journal of Monetary Economics*, 56 (4), pp. 560-9.

Eichenbaum, M. (1997). Some Thoughts on Pratical Stabilization Policy. *American Economic Review*, 87 (2, Papers and Proceedings), pp. 236-9.

Fischer, S. (1977). Long Term Contracts, Rational Expectations, and the Optimal Money Supply Rule. *Journal of Political Economy*, 85(1), pp. 191-205.

____________ (1983). Comment on Nordhaus's "Macroconfusion: The Dilemmas of Economic Policy." In J. Tobin (Ed.), *Macroeconomics, Prices, and Quantities---Essays in Memory of Arthur M. Okun* (pp. 267-76). Washington, D.C.: The Brookings Institution.

Friedman, M. (1968). The Role of Monetary Policy. *American Economic Review*, 58 (1), 1-17.

Friedman, M., & Schwartz, A. J. (1963). *A Monetary History of the United States.* Princeton: Princeton University Press.

Galí, J., & Gertler, M. (2007). Macroeconomic Modeling for Monetary Policy Evaluation. *Journal of Economic Perspectives*, 21 (4), pp. 25-45.

Gerrard, B. (1996). Competing Schools of Thought in Macroeconomics - An Ever Emerging Consensus? (Review Article). *Journal of Economic Studies*, 23(1), pp. 53-69.

Goodfriend, M. (2004). Monetary Policy in the New Neoclassical Synthesis: A Primer. *Federal Reserve Bank of Richmond Economic Quarterly*, 90 (3), pp. 21-45.

____________ (2007). How the World Achieved Consensus on Monetary Policy. *Journal of Economic Perspectives*, 21 (4), pp. 47-68.

Goodfriend, M., & King, R. G. (1997). The New Neoclassical Synthesis and the Role of Monetary Policy. *NBER Macroeconomics Annual*, 12, pp. 231-83.

Gordon, R. (1989). Fresh Water, Salt Water, and Other Macroeconomic Elixirs. *Economic Record*, 65 (2), pp. 177-84.

Greenwald, B. C., & Stiglitz, J. E. (1988). Examining Alternative Macroeconomic Theories. *Brookings Papers on Economic Activity*, (1), pp. 207-260.

Greenwald, B. C., & Stiglitz, J. E. (1993). New and Old Keynesians. *Journal of Economic Perspectives*, 7 (1), pp. 23-44.

Greenwald, J. (1978, May 18). Rational people may be economy's thorn. *The Minneapolis Star*.

Hall, R. E. (1976). *Notes on the Current State of Empirical Macroeconomics.* Available at: http://www.stanford.edu/~rehall/Notes%20Current%20State%20Empirical%201976.pdf (accessed on 12/18/2009).

Hartley, J. E. (1997). *The Representative Agent in Macroeconomics.* London: Routledge.

Hartley, J. E., Hoover, K. D., & Salyer, K. D. (1997). The Limits of Business Cycle Research: Assessing the Real Business Cycle Model. *Oxford Review of Economic Policy*, 13 (3), pp. 34-54.

Heathcote, J., Storesletten, K., & Violante, G. L. (2009). Quantitative Macroeconomics with Heterogeneous Households. In K. J. Arrow, & T. F. Bresnahan (Eds.), *Annual Review of Economics* (Vol. 1, pp. 319-354).

Hoover, K. D. (1988). *The New Classical Macroeconomics: A Sceptical Inquiry.* Oxford: Basil Blackwell.

____________ (1995). Facts and Artifacts: Calibration and the Empirical Assessment of Real-Business-Cycle Models. *Oxford Economic Papers*, 47 (1), pp. 24-44.

____________ (2001). *The Methodology of Empirical Macroeconomics.* Cambridge: Cambridge University Press.

____________ (2006). A Neowicksellian in a New Classical World: The Methodology of Michael Woodford's Interest and Prices. *Journal of the History of Economic Thought*, 28 (2), pp. 143-49.

Hymans, S. H., & Shapiro, H. T. (1975). Econometric Review of Alternative Fiscal and Monetary Policies, 1966–75: Part II. *Franco Modigliani Papers. Rare Book, Manuscript, and Special Collections Library, Duke University* , pp. Draft, box RW27, folder "'Models of the Economy ... ,' -Russian Paper, Notes, 1976.".

Janssen, M. (1993). *Microfoundations: A Critical Inquiry.* London: Routledge.

Kirman, A. P. (1992). Whom or What Does the Representative Individual Represent? *Journal of Economic Perspectives*, 6 (2), pp. 117-36.

Krugman, P. (2000). How Complicated Does the Model Have to Be? *Oxford Review of Economic Policy*, 16 (4), pp. 33-42.

Kuhn, T. ([1962] 1970). *The Structure of Scientific Revolutions* (2nd ed.). Chicago: University of Chicago Press.

Kydland, F. E. (1992). On the Econometrics of World Business Cycles. *European Economic Review*, 36 (2-3), pp. 476-482.

Kydland, F. E., & Prescott, E. C. (1982). Time to Build and Aggregate Fluctuations. *Econometrica*, 50 (6), pp. 1345-70.

____________ (1990). Business Cycles: Real Facts and a Monetary Myth. *Federal Reserve Bank of Minneapolis Quarterly Review*, 14 (2), pp. 3-18.

____________ (1991a). The Econometrics of the General Equilibrium Approach to Business Cycles. *Scandinavian Journal of Economics*, 93 (2), pp. 161-78.

____________ (1991b). Hours and employment variation in business cycle theory. *Economic Theory, 1* (1): 63-81.

Laidler, D. (1999). *Fabricating the Keynesian Revolution - Studies of the Inter-War Literature on Money, the Cycle, and Unemployment.* Cambridge: Cambridge University Press.

____________ (2006). Woodford and Wicksell on Interest and Prices: The Place of the Pure Credit Economy in the Theory of Monetary Policy. *Journal of the History of Economic Thought*, 28 (2), pp. 151-9.

Lakatos, I. (1970). Falsification and the Methodology of Scientific Research Programmes. In I. Lakatos, & A. Musgrave (Eds.), *Criticism and the Growth of Knowledge* (pp. 91-196). London: Cambridge University Press.

Leijonhufvud, A. (1973). Effective Demand Failures. *Swedish Journal of Economics*, 75 (1), pp. 27-48.

____________ (1976). Schools, "Revolutions", and Research Programmes in Economic Theory. In S. Latsis (Ed.), *Method and Appraisal in Economics* (pp. 65-108). Cambridge: Cambridge University Press.

____________ (1992). Keynesian Economics: Past Confusions, Future Prospects. In A. Vercelli, & N. Dimitri (Eds.), *Macroeconomics - A Survey of Research Strategies* (pp. 16-37). Oxford: Oxford University Press.

____________ (1993). Towards a Not-Too-Rational Macroeconomics. *Southern Economic Journal*, 60 (1), pp. 1-13.

____________ (2004). Outside the Mainstream: An Interview with Axel Leijonhufvud. *Macroeconomic Dynamics*, 8 (1), pp. 117-45.

Lovell, M. C., & Selover, D. D. (1994). Econometric Software Accidents. *Economic Journal*, 104 (424), pp. 713-725.

Lucas Jr., R. E. (1972). Expectations and the Neutrality of Money. *Journal of Economic Theory*, 4 (2), pp. 103-124.

____________ (1973). Some International Evidence on Output-Inflation Tradeoffs. *American Economic Review*, 63 (3), pp. 326-34.

____________ (1975). An Equilibrium Model of the Business Cycle. *Journal of Political Economy*, pp. 1113-44.

____________ (1976). Econometric Policy Evaluation: A Critique. *Carnegie-Rochester Conference Series on Public Policy*, 11, 19-46.

____________ (1994). Comments on Ball and Mankiw. *Carnegie-Rochester Conference Series on Public Policy*, 41, pp. 153-5.

____________ (1998). Transforming Macroeconomics: An Interview with Robert E. Lucas Jr. *Journal of Economic Methodology*, 5 (1), pp. 115-46.

____________ (2004). My Keynesian Education. In M. De Vroey, & K. D. Hoover (Eds.), The IS-LM Model: Its Rise, Fall, and Strange Persistence. *HOPE*, 36 (Annual Supplement): 12-24.

Lucas Jr., R. E., & Sargent, T. J. (1979). After Keynesian Macroeconomics. *The Federal Reserve Bank of Minneapolis Quarterly Review*, 3 (2).

____________ (Eds.). (1981). *Rational Expectations and Econometric Practice.* London: George Allen & Unwin.

Mankiw, N. G. (1985). Small Menu Costs and Large Business Cycles: A Macroeconomic Model of Monopoly. *Quarterly Journal of Economics*, 100 (2), pp. 529-37.

____________ (1989). Real Business Cycle: A New Keynesian Perspective. *Journal of Economic Perspectives,* 3 (3), 79-90.

____________ (1990). A Quick Refresher Course in Macroeconomics. *Journal of Economic Literature*, 28 (4), pp. 1645-60.

____________ (1992). Macroeconomics in Disarray. *Society*, 29 (4), pp. 19-24.

Mankiw, N. G. (1992). The Reincarnation of Keynesian Economics. *European Economic Review*, 36 (2-3), pp. 559-565.

____________ (2006). The Macroeconomist as Scientist and Engineer. *Journal of Economic Perspectives*, 20 (4), pp. 29-46.

Mehrling, P. (2006). Mr. Woodford and the Challenge of Finance. *Journal of the History of Economic Thought,* 28 (2), pp. 161-70.

Mirowski, P., & Hands, D. W. (1998). A Paradox of Budgets: The Postwar Stabilization of American Neoclassical Demand Theory. *HOPE,* 30 (Annual Supplement), pp. 260-292.

____________ (Eds.). (2006). Agreement on Demand: Consumer Theory in the Twentieth Century. *HOPE,* 38 (Annual Supplement).

Mishkin, F. S. (2007). Will Monetary Policy Become More of a Science? *NBER Working Paper 13566* .

Nordhaus, W. D. (1983). Macroconfusion: The Dilemmas of Economic Policy. In J. Tobin (Ed.), *Macroeconomics, Prices, and Quantities---Essays in Memory of Arthur M. Okun* (pp. 247-67). Washington, D.C.: The Brookings Institution.

Obstfeld, M., & Rogoff, K. (1996). *Foundations of International Macroeconomics.* Cambridge: The MIT Press.

Pearce, K. A., & Hoover, K. D. (1995). After the Revolution: Paul Samuelson and the Textbook Keynesian Model. In A. R. Cottrell, & M. S. Lawlor, New Perspectives on Keynes. *HOPE*, 27 (Annual Supplement), pp. 183-216.

Phelps, E. S. (1967). Phillips Curves, Expectations of Inflation, and Optimal Unemployment Over Time. *Economica*, 34 (3), 254-81.

____________ (Ed.). (1969). *Microeconomic Foundations of Employment and Inflation Theory.* New York: Norton W. W.

____________ (1990). *Seven Schools of Macroeconomic Thought.* Oxford: Oxford University Press.

Phelps, E. S., & Taylor, J. B. (1977). Stabilizing Powers of Monetary Policy Under Rational Expectations. *Journal of Political Economy*, 85 (1), pp. 163-190.

Prescott, E. C., & Candler, G. V. (2008). Calibration. In S. N. Durlauf, & L. E. Blume (Eds.), *The New Palgrave Dictionary of Economics* (2nd ed.). Palgrave Macmillan.

Redman, D. A. (1991). *Economics and the Philosophy of Science.* Oxford: Oxford University Press.

Romer, D. (1993). The New Keynesian Synthesis. *Journal of Economic Perspectives*, 7(1), pp. 5-22.

Rotemberg, J., & Woodford, M. (1997). An Optimization-Based Econometric Framework for the Evaluation of Monetary Policy. In B. S. Bernanke (Ed.), *NBER Macroeconomics Annual* (pp. 297-346). Cambridge: MIT Press.

Samuelson, P. A. (1955). *Economics* (2nd ed.). New York: McGraw-Hill.

Sheils, M., & Thomas, R. (1978, June 26). The New Economists. *Newsweek*, 59-60.

Sims, C. (1972). Money, Income, and Causality. *American Economic Review*, 62 (4), pp. 540-52.

____________ (1989). Models and Their Uses. *American Journal of Agricultural Economics*, 71 (2), pp. 489-94.

____________ (1992). Interpreting the Macroeconomic Time Series Facts - the Effects of Monetary Policy. *European Economic Review*, 36 (5), pp. 975-1000.

____________ (2004). Econometrics for Policy Analysis: Progress and Regress. *De Economist, 152*(2), pp. 167-175.

Smets, F., & Wouters, R. (2003). An Estimated Dynamic Stochastic General Equilibrium Model of the Euro Area. *Journal of the European Economic Association*, 1 (5), pp. 1123-75.

____________ (2007). Shocks and Frictions in US Business Cycles: A Bayesian DSGE Approach. *American Economic Review*, 97 (3), pp. 586-606.

Snowdon, B. (2001). Keeping the Keynesian Faith - Alan Blinder on the Evolution of Macroeconomics. *World Economics*, 2 (2), pp. 105-140.

Snowdon, B., & Vane, H. (1995). New-Keynesian Economics Today: The Empire Strikes Back. *American Economist*, 39 (1), pp. 48-65.

____________ (1996). The Development of Modern Macroeconomics: Reflections in the Light of Johnson's Analysis After Twenty-Five Years. *Journal of Macroeconomics*, 18 (3), pp. 381-401.

Snowdon, B., Vane, H., & Wynarczyk, P. (1994). *A Modern Guide to Macroeconomics: An Introduction to Competing Schools of Thought.* Aldershot: Edward Elgar.

Solow, R. M. (1979). Alternative Approaches to Macroeconomic Theory: A Partial View. *Canadian Journal of Economics*, 12 (3), pp. 339-54.

____________ (1983). Comment on Nordhaus's "Macroconfusion: The Dilemmas of Economic Policy." In J. Tobin (Ed.), *Macroeconomics, Prices, and Quantities---Essays in Memory of Arthur M. Okun* (pp. 279-84). Washington, D.C.: The Brookings Institution.

____________ (1997). Is There a Core of Usable Macroeconomics We Should All Believe In? *American Economic Review*, 87 (2, Papers and Proceedings), pp. 230-2.

____________ (2000). Toward a Macroeconomics of the Medium Run. *Journal of Economic Perspectives*, 14 (1), pp. 151-158.

____________ (2008). The State of Macroeconomics. *Journal of Economic Perspectives*, 22 (1), pp. 243-6.

_____________ (2010). *Building a Science of Economics for the Real World.* House Committee on Science and Technology, U.S. House of Representatives - Subcommittee on Investigations and Oversight. July 20. Washington, D. C. (available at: http://science.house.gov/publications/hearings_markups_details.aspx?NewsID=2916, accessed on Oct. 13, 2010).

Spear, S. E., & Wright, R. (1998). Interview with David Cass. *Macroeconomic Dynamics*, 2 (4), pp. 533-58.

Stiglitz, J. E. (1992). Methodological Issues and the New Keynesian Economics. In A. Vercelli, & N. Dimitri (Eds.), *Macroeconomics - A Survey of Research Strategies* (pp. 38-86). Oxford: Oxford University Press.

Taylor, J. B. (1997). A Core of Practical Macroeconomics. *American Economic Review, Papers and Proceedings*, 87 (2), pp. 233-5.

van den Bergh, J. C., & Gowdy, J. M. (2003). The Microfoundations of Macroeconomics: An Evolutionary Perspective. *Cambridge Journal of Economics*, 27 (1), pp. 65-84.

Vane, H., & Thompson, J. L. (1992). *Current Controversies in Macroeconomics.* Aldershot: Edward Elgar.

Weintraub, E. R. (1979). *Microfoundations - the Compatibility of Microeconomics and Macroeconomics.* Cambridge: Cambridge University Press.

_____________ (Ed.). (1992). *Toward a History of Game Theory*. *HOPE*, 24 (Annual Supplement).

_____________ (2002). *How Economics Became a Mathematical Science.* Durham: Duke University Press.

Woodford, M. (2000). Revolution and Evolution in Twentieth-Century Macroeconomics. In P. Gifford (Ed.), *Frontiers of the Mind in the Twenty-First Century* (Available at: http://www.columbia.edu/~mw2230/macro20C.pdf). Cambridge: Harvard University Press.

_____________ (2003). *Interest and Prices: Foundations of a Theory of Monetary Policy.* Princeton: Princeton University Press.

_____________ (2006). Comments on the Symposium on Interest and Prices. *Journal of the History of Economic Thought*, 28 (2), pp. 187-98.

_____________ (2009). Convergence in Macroeconomics: Elements of the New Synthesis. *American Economic Journal: Macroeconomics*, 1 (1), pp. 267–79.

Zouache, A. (2004). Towards a ‘New Neoclassical Synthesis’? An Analysis of the Methodological Convergence Between New Keynesian Economics and Real Business Cycle Theory. *History of Economic Ideas*, 12 (1), pp. 95-117.