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The new classical/real business cycle revolution: a matter of microfoundations?

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Abstract

The aim of the present paper is to assess the new classical/real business cycle revolution, which dethroned Keynesian macroeconomics. In its first part, I critically discuss the microfoundations requirement that constitutes a cornerstone of the new approach and suggest an alternative, softer, formulation of it. The conclusion of this discussion is that the new classical/real business cycle revolution marked a transition from the soft to the demanding understanding of the microfoundations requirement. This amounted to the expulsion of market non-clearing from macroeconomics. In the second part of the paper, I present the salient traits of the new classical and the real business cycle stages of the revolution. While each of these stages brought a specific contribution to the revolution, I emphasize the decisive role played by Kydland and Prescott in re-orienting the type of work in which macroeconomists were engaged. Finally, in part three, I ponder upon the causes of this revolution. After studying Prescott's and Lucas's accounts of the factors which gave rise to the new approach, I venture into muddier waters by raising the question of whether a political agenda underpinned the NC/RBC revolution.

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INTRODUCTION

As will be documented in this paper, there are good reasons for viewing the transition from Keynesian IS-LM macroeconomics to dynamic stochastic macroeconomics as a scientific revolution *à la* Kuhn. This expression refers to an episode in the history of a given discipline where a period of normal science is disturbed because of the persistent existence of apparently unsolvable puzzles and a drive to move the agenda and the research methods into new directions. This goes along with thundering declarations of war — Keynesian theory is dead — a confrontation between the young and the old generation, the rise of new stars in the profession and the eclipse of old ones. The relevance of the scientific revolution hinges on the existence of a 'before' and an 'after', with a well delineated series of events in between, so that the type of work members of the community are engaged in after the revolution bears little resemblance to earlier practices.

This revolution in macroeconomics resulted from a sequence of episodes related both to the intricacies of the internal development of the discipline and to outside events. Friedman (Friedman 1968) and Lucas (Lucas [1972] 1981) had recounted the story of the real effects of monetary expansion in a non-Keynesian way, thereby disqualifying the policy-menu idea associated with the Phillips curve. The emergence of stagflation in the 1970s was proclaimed to be a real-time experiment that confirmed Friedman's predictions about the inability of monetary policy to have a long-lasting effect on employment (Friedman 1968). Lucas and Rapping's work (Lucas and Rapping 1969) extending the sphere of equilibrium analysis started the downfall of the neoclassical synthesis — why still try to graft disequilibrium onto an equilibrium theory if the equilibrium theory is all-inclusive? The blending of rational expectations and the time inconsistency led to the dismissal of state interventions in the economy that were previously believed to be effective in increasing social welfare. Last but not least, Lucas's critique (Lucas [1976] 1981) questioned the ability of traditional macroeconomic models to serve the purpose of choosing between alternative policy actions. All these factors brought the traditional Keynesian approach to its knees. As stated by Samuelson, this process had a ring of revenge: "The new classical economics of rational expectationists is a return with a vengeance to the pre-Keynesian verities" (Samuelson 1983, p. 212).

This revolution — at present often viewed as having led to the rise of DSGE (dynamic-stochastic general equilibrium) macroeconomics — occurred in two stages. The first is associated with Robert Lucas's work and has often been labeled the 'new classical revolution'. The second is associated with Kydland and Prescott and real business cycle models. To capture this two-step process, I shall refer to it as the new classical/real business cycle revolution (henceforth NC/RBC) rather than the DSGE revolution.[1] In a nutshell, Lucas did the job of attacking the Keynesian paradigm and of introducing a series of new concepts and principles. Kydland and Prescott's transformed Lucas's qualitative modeling into a quantitative research program — as stated by Greenwood ([1994] 2005, p.1), they took macroeconomics to the computer. Of course, many other researchers played an important role in this revolution, in particular Sargent, Barro, Wallace, Plosser and Long. Lucas and Kydland and Prescott have nonetheless been its towering figures, with Kydland playing a more subdued role in the Kydland and Prescott duo, at least as far as the defense of their joint work is concerned.

Several questions arise: What were the causes of the NC/RBC revolution? What changes did it bring about? Did it have a political dimension? These are the issues I wish to tackle in this paper. It comprises three parts. The NC/RBC revolution is often presented as having consisted of giving macroeconomics the microfoundations it lacked. I appraise this characterization in the first part of the paper. In the second, I bring out the different points on which the new way of doing macroeconomics differed from the old one. Finally, in part three, I ponder upon the causes of this revolution. I start with a discussion of Prescott's and Lucas's accounts of the revolution, after which, entering into more troubled waters, I raise the question of whether there was a political agenda underpinning the NC/RBC revolution.

MICROFOUNDATIONS

A new methodological requirement

From the 1970s onwards, a new methodological principle came to prominence in macroeconomics, the microfoundations requirement. It became the *sine qua non* of valid theoretical practice: the condition for a macroeconomic model to be microfounded is that it starts with the description of how agents make their choices, these being made in an optimizing way. An objective function is to be maximized or minimized under given constraints.

For all its generality, this condition is nonetheless deemed sufficient to identify models that do not accord with it and thence ought to be rejected. The same requirement has been expressed differently by Lucas and Sargent (and Lucas on his own) under the name of 'equilibrium discipline'.[2] It states that, to be valid, economic models should rest on two

[1] An additional reason for not using the DSGE terminology is that with the appearance of new neoclassical synthesis models it extends beyond real business cycle models.

[2] Henceforth the terms 'microfoundations' and 'equilibrium discipline' will be used interchangeably.

postulates: (a) that agents act in their own self-interest and their behavior is optimal; and (b) that markets clear (Lucas and Sargent, [1979] 1994, p. 15). The 'discipline' term is used to convey the view that this is a rule that economists impose upon themselves and which stamps their specific way of looking at social reality. Accepting such a standpoint results in proclaiming that the notion of disequilibrium, which before was widely used, should be banned from the economic lexicon.

Two additional remarks are worth making. First, the insistence on the microfoundations requirement did not stem from microeconomists wanting to assess the good practice of macroeconomists from the standpoint of their own sub-discipline. Rather, it originated from within the macroeconomic community, the result of a gradual awareness of some of its members that something was wrong with the existing practice of the discipline, and that this malaise had to do with its drift away from microeconomics. Second, it is sometimes claimed that new classicists invented market clearing. This is not true. Market clearing (i.e. the idea that supply and demand always match in a given period of exchange) has a long standing in economics. Its presence in Walrasian theory is beyond dispute. But the same is true for Marshallian theory (with the additional complication that market clearing and disequilibrium can coexist; see De Vroey, 2007). By challenging this consensus in the profession, Keynes was clearly thinking out of the box. Thus, rather than having invented market clearing, new classicists have just restored it at a higher level, signaling the end of the Keynesian recess. Still, the pace at which the microfoundations requirement conquered the profession is impressive. More curiously, this conquest occurred without any justification being provided, as if the case was so obvious that none was needed.

Lucas's criticism of Keynesian theory and modeling

The gist of Lucas's criticism of Keynesian theory is that it does not abide by the equilibrium discipline. His attack develops at two levels. The first pertains to the general way in which Keynes addressed the issue of unemployment in his *General Theory.* In Lucas's eyes, the mere aim of wanting to produce a theory of involuntary unemployment constitutes an infringement of the equilibrium discipline (Lucas [1977] 1981).

> After freeing himself of the straightjacket (or discipline) imposed by the classical postulates, Keynes described a model in which rules of thumb, such as the consumption function and liquidity preference schedule, took the place of decision functions that a classical economist would insist be derived from the theory of choice. And rather than require that wages and prices be determined by the postulate that markets clear — which for the labor market seemed patently contradicted by the severity of business depressions — Keynes took as an unexamined postulate that money wages are sticky, meaning that they are set at a

> level or by a process that could be taken as uninfluenced by the macroeconomic forces he proposed to analyze (Lucas and Sargent [1979] 1994, p. 15).

Keynes's lapse from the equilibrium discipline, Lucas is ready to admit, is understandable in view of the apparent contradiction between cyclical phenomena and economic equilibrium in the context of the Great Depression. Still, *ex post* it ought to be interpreted as having prompted a long detour in the progress of economic theory. It is an example of "bad social science: an attempt to explain important aspects of human behavior without reference either to what people like or what they are capable of doing" (Lucas, 1981, p. 4).

The second level of criticism is the well-known 'Lucas critique' ([1976] 1981). Here his target is the macroeconomic models of the time, all of which had a Keynesian inspiration. Lucas's claim is that, although they do a fairly good job of forecasting, these models are a failure as far as the assessment of alternative policies is concerned. Their main flaw is their lack of microfoundations. This leads to endogenous variables, sensitive to variations in economic policy, being transformed into exogenous ones. As a result, a model of the economy estimated at a period during which a particular institutional regime holds sway will provide inadequate information for assessing what might occur under a different regime. According to Lucas, to avoid this defect, the parameters of the model need to be 'deeply structural'. That is, they must be derived from the fundamentals of the economy, agents' preferences, and technological constraints.

Assessing the equilibrium discipline principle

Before appraising the validity of Lucas's twofold criticism, it is worth pondering upon the equilibrium discipline in itself, independent of its use as a weapon against Keynesian theory. It is a fact that since the inception of political economy the equilibrium notion has played a central role in it. So the idea of equilibrium discipline as the hallmark of economics makes sense. However, I am unconvinced by the way in which Lucas and Sargent conceive it. First of all, contrary to what they say, it actually contains only one criterion. Optimal behavior and market clearing are two faces of the same coin. They grasp the same reality at two distinct levels: optimal behavior refers to individual or personal equilibrium, while market clearing relates to what could be called 'interactive equilibrium', a state where all individual optimal plans have been made compatible. Moreover, their conception puts under the rug a distinction that I, for one, find crucial. It was expressed long ago by Hayek and Patinkin but subsequently felt into oblivion.

> I have long felt that the concept of equilibrium itself and the methods which we employ in pure analysis have a clear meaning only when confined to the analysis of the action of a single person and that we are really passing into a different sphere

> and silently introducing a new element of altogether different character when we apply it to the explanation of the interactions of a number of different individuals (Hayek [1937] 1948, p. 35).

A similar insight is to be found under Patinkin's name when he draws a distinction between individual experiments and market experiments (1965, pp. 11–12 and 387–392). Yeager aptly commented on this distinction:

> An individual experiment involves discovering, at least conceptually, the desired behavior of an individual person, of a small or large group of individuals, or even of all individuals in the community, acting in certain capacities, under certain specified circumstances. Whether these circumstances are compatible with other economic conditions and whether they can in fact prevail (whether they are genuinely or even conceptually attainable, to use the Chicago terminology) is beside the point: it is not the purpose of an individual experiment, by itself, to describe the economic equilibrium that will tend to emerge. … This other type of analysis, which pulls together the results of various individual experiments, examines the conditions under which the plans of various persons would and would not mesh, and describes the processes at work when plans fail to mesh, and describes the equilibrium position, is what Patinkin means by market experiments (Yeager 1960, p. 59).

It follows from Hayek's and Patinkin's standpoints that the notions of the *optimal plan* and *optimal behavior* designate different realities. Optimal plan refers to agents' intentions before the opening of trading, the solution to the choice-theoretical problem they are faced with.[3] Optimal behavior refers to what is observable after trading has started. Thus, optimal behavior implies that the optimal plan has been realized. The gist of the above quotations is that optimal plan and optimizing behavior need to be logically separated — there is a difference between finding a solution to a choice problem and implementing this solution. Whenever optimizing behavior is the sole concept used, the possibility of there being a difference between them is discarded by definition.

This difference can also be expressed with reference to the notions of equilibrium and disequilibrium. Individual equilibrium is a state where an agent is able to achieve one element of his or her optimal plan. Individual disequilibrium refers to the opposite case, the inability of some agents to have their optimal plans transformed into optimal behavior. Note

[3] In Patinkin's words: "We can consider the individual — with his given indifference map and initial endowment — to be a 'utility-computer' into whom we 'feed' a sequence of market prices and from whom we obtain a corresponding sequence of 'solutions' in the form of specified optimum positions" (1965, p. 7).

that this notion does not run counter to the view that agents are rational and develop optimal plans. Equilibrium *tout court* is what I labeled interactive equilibrium above. “The general equilibrium implies that all subsets of agents are in equilibrium and in particular that all individual agents are in equilibrium” (McKenzie 1987, p. 498). That is, equilibrium requires individual equilibrium. This quotation confirms my view that optimizing behavior and market clearing are one and the same thing. Symmetrically, in the conception that I defend, optimal plan and market clearing are distinct while market non-clearing and individual disequilibrium go hand in hand.

It remains to assess whether the two distinct potential criteria for the microfoundations requirement, optimal planning and market clearing, are valid candidates for this function. Adopting optimal planning as a postulate raises no serious objections. It amounts to assuming that agents have the ability to optimally solve any decision problem they encounter (with the ensuing correlates of rationality, information and rational expectations). This assumption is certainly an exaggeration, yet it is nonetheless acceptable as it is probably better, and certainly more tractable, than alternative assumptions.

But the same is not true for the second component, market clearing. I see no reason to adopt it as a condition for sound economic reasoning. Market clearing is the consequence of some prior assumptions related to ‘trade technology’, i.e. the institutional set-up that is needed to make the realization of equilibrium possible. Like other Walrasian models, Lucas’s models are based on the *tâtonnement* or auctioneer hypothesis. This is a theoretical scenario explaining how the equilibrium values calculated by the economist when studying the logical existence of a general equilibrium could come into existence in the artificial economy described by the model. As soon as this hypothesis is made, the matter is sealed: market clearing always occurs. But then market clearing is the direct consequence of the auctioneer hypothesis rather than a consequence of self-interest and rationality. Moreover, the problem with the auctioneer hypothesis is that it runs counter to the essential nature of the theory’s theoretical *explanandum*, because it amounts to picturing a decentralized system as a centralized organization of trade.[4]

My conclusion is thus: ‘yes’, we can consider optimal plan as the constitutive element of the equilibrium discipline (until procedural rationality theories see the day) but ‘no’, market clearing does not qualify as a condition for the discipline to which economists should submit themselves. Not that its adoption is unjustifiable. It can be justified, but only on the grounds of expediency — the admission that no alternative scenario about the formation of equilibrium exists. I see no reason to transform expediency into methodological virtue.

[4] See De Vroey (1998) for further discussion of this point.

An appraisal of Lucas's two criticisms

My reconsideration of the equilibrium discipline leaves Lucas's critique intact. There is no need to revise it since it bears on agents' devising their optimal plans. The rational expectations assumption accepted, Lucas is right in claiming that agents should change their optimal plans whenever the policy regime is modified. [5]

Lucas's more general criticism of Keynesian theory is different. If my understanding of equilibrium discipline is adopted, his criticism falls, and so the ban on individual disequilibrium ought to be removed. Keynes's project of constructing a theory of involuntary unemployment as a case of individual disequilibrium does not infringe the equilibrium discipline. Had Lucas contented himself with pointing out that Keynes failed to achieve this project (see De Vroey 2004a), I would have no complaints. My disagreement bears on his further claim that the mere desire to engage in such a project is a sufficient reason for being excommunicated from the economic community.

This being granted, it must be admitted that Keynes's work does not fare well even against my softer definition of the equilibrium principle, at least under retrospective scrutiny. The *General Theory* has sloppy microfoundations. The reason for this is that Keynes was a Marshallian, and followed Marshall in caring little about microfoundations. At the time, microfoundations was the exclusive hallmark of economists of the Lausanne School, and for better or worse, most economists had no acquaintance with them. All in all, Keynes paid no less attention to microfoundations than other Marshallian economists of the time. In fact, his definition of involuntary unemployment does have microfoundations in my sense of the term: agents are described as having an optimal plan, although they are unable to put it into practice.

Conclusion

The question addressed in this first section of the paper is whether it is appropriate to characterize the NC/RBC revolution as having introduced microfoundations into

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[5] However, the first model builders deserve some indulgence because of the undeveloped state of technique at the time. Klein anticipated the need for microfoundations, as is clear from the first book he wrote for t Lucas [1973] 1981) he Cowles Foundation, *Economic Fluctuations in the United States, 1921-1942* (Klein [1950] 1964). New classicists could subscribe to the program he set out there. However, when, in his joint work with Goldberg which became *An Econometric Model of the United States 1929-1952* (Klein and Goldberg, 1955), he came to setting out a full-blown model, he had to back down and adopt pragmatic solutions. Moreover, at the time nobody was aware of the drawbacks of using backward- rather than forward-looking expectations.

[7] My above discussion may give the impression that the microfoundations requirement emerged as a weapon against Keynesian economics. This is not the case. Actually, the first economists who insisted on it were Keynesian economists, such as Patinkin (1965), Clower ([1965] 1984) and Barro and Grossman (1971), who set themselves the task of improving Keynesian theory by giving it more rigorous foundations. It is only in a second stage that the microfoundations requirement came to be associated with an external criticism of Keynesian theory.

macroeconomic theory.[7] It is true that such foundations were introduced, in as far as the narrow Lucas/Sargent definition is adopted. But this definition is too narrow. With my understanding of the microfoundations requirement, Keynesian macroeconomics abides by it, albeit in a sloppy way. So what occurred was a shift from a soft to a strong understanding of the microfoundations requirement (it being furthermore added that, before, the soft microfoundations requirement was only present implicitly).

Another way of posing the contrast between 'old' and 'new' macroeconomics runs as follows. Before the NC/RBC revolution, the agenda of macroeconomics was to explain market non-clearing. The revolution consisted of expelling market non-clearing from macroeconomics. If the microfoundations notion is meant to capture this difference, then it is true that stating that the NC/RBC revolution was a matter of microfoundations captures the gist of what took place.

CHARACTERIZING THE NC/RBC REVOLUTION

A stated above, the NC/RBC revolution was a two-step process, the first being associated with Lucas (the new classical phase of the revolution), and the second with Kydland and Prescott (the real business cycle phase). Below, I will explore the changes in perspective that occurred in each of these two steps.

The new classical phase

Lucas's role was twofold. First, treading in Friedman's footsteps, he launched an all-out attack on Keynesian macroeconomics. Positively, he introduced a series of new concepts and methodological perspectives. They were not necessarily his inventions — the obvious example is the notion of rational expectations, introduced by Muth — nor was he the only person to bring them to the forefront, but he did provide the impulse. The assumption of rational expectations and intertemporal substitution are the cornerstones of the new approach (in addition to the equilibrium discipline). As these two aspects are familiar, I will focus more on some less well-known features of the new approach.[8]

Merging the fields of value theory and business cycle theory

The NC/RBC revolution modified the boundaries between the economic subdisciplines of value theory and business cycle theory, with business cycle theory being absorbed by value

[8] For a pioneering, but still useful, assessment of the new classical approach, see Hoover (1988).

theory.[9] Before the revolution these fields were separate. Evolving at a high level of abstraction, value theory was based on trade technology or information assumptions resulting in the universality of market clearing. By contrast, business cycle theory consisted of qualitative, descriptive accounts of the unfolding of economies over time. Studies in this field aimed to provide specific explanations of fluctuations rather than a general theory. Although the notion of equilibrium was only vaguely referred to, it was taken for granted that the cycle was a manifestation of economic disequilibrium. This split was already present in Marshall's work. Take his treatment of unemployment. No room for it existed in Marshall's value theory, its proper place being in business cycle theory. In Matthews's words (1990, p. 35), unemployment was *par excellence* a 'Vol. II' subject. Against this background, Keynes' aim in the *General Theory* may be reconstructed as an attempt to move unemployment away from the field of business cycle theory into that of value theory without implying that these two fields should be merged.

The new classical revolution took another path, to make the business cycle part of value theory while expelling unemployment altogether from the enlarged field. Two implications of this widening of the scope of value theory are worth mentioning. First, the idea now prevails that a theory of the business cycle can be constructed without resorting to the notion of unemployment, a view that was inconceivable before. The second implication is that the earlier judgments about the harmful character of business cycles were erroneous. Business cycles are no longer considered to be a manifestation of some malfunctioning. The earlier received wisdom that the state should intervene in order to mitigate fluctuations ceases to be valid.

A change in the research agenda

The move described above entailed a radical change in the research agenda of macroeconomics. Macroeconomics arose in the wake of the Great Depression from the wish to bring to the fore the existence of trading failures which, it was presumed, it was the role of the state to act upon. Small wonder that it was supported by social reformers, ??? and in turn supported them. Unemployment was considered the main dysfunction. Hence unemployment was viewed as the central object of analysis. This state of mind was still present at the beginning of the 1970.[10] But then, in a sweeping change, unemployment ceased to be an important preoccupation of macroeconomists. It fell out of fashion, macroeconomists being glad to send it back to labor economists. Issues related to the business cycle and a wider and

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[9] In what follows, value theory, equilibrium theory and price theory will be considered synonymous.
[10] A testimony to this is Tobin's 1971 Presidential Address to the American Economic Association, in which he wrote that macroeconomics deprived of the concept of full employment was unimaginable (1972, p. 1), not realizing that such a state of affairs was just around the corner.

wider spectrum of themes related to growth and development rose to the top of the macroeconomic research agenda.

The replacement of Marshallian macroeconomics with Walrasian macroeconomics

Elsewhere (De Vroey, 2004b) I have claimed that Keynesian macroeconomics ought to be considered as a simplified Marshallian general equilibrium theory. Admittedly, this is an unusual claim. Economists' reflex when seeing the term 'general equilibrium', is to associate it with Walrasian or neo-Walrasian theory as if these were the only ways of studying the economy as a whole. To me, alternative approaches are conceivable, the Marshallian approach being the main one.[11]

The difference between the two approaches results from the consideration of various criteria: the purpose of economic theory; methodology (including the role of mathematics); the ways of looking at the working of the economy as whole; the conception of equilibrium underpinning the theories; and, finally, the trade organization assumptions. Here, I want to focus on the representation of the economy.

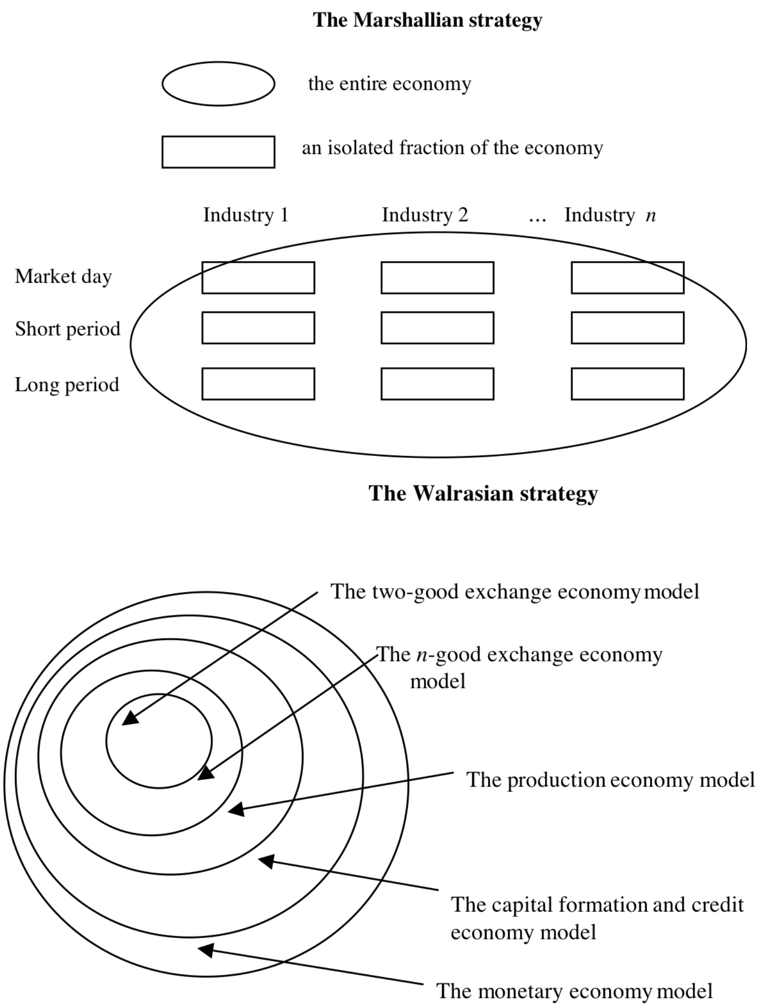
Marshall viewed the analysis of economics as so complex that he thought it wise to approach it gradually.[12] To all intents and purposes, he divided the economy into industries, to be studied separately. Moreover, since the analysis of time was so tricky, he distinguished three time categories: the market day (the unit period of exchange); the short period; and the long period. This strategy amounted to postponing the study of the functioning of the economy as a whole (i.e. the piecing together of these partial results) to a stage at which enough separate understanding of its parts had been reached.

No such two-tier strategy is to be found in Walras's work. This is premised on the view that, from the onset, the object of study should be an entire economy. Simplifications had, of course, to be introduced, but they pertained to the characterization of the economy as a whole and did not involve dividing it into separate sub-entities. Walras inaugurated his analysis with the most rudimentary economy possible, a two-good exchange economy, where the two goods (oats and wheat) constituted the entire economy. He started by deriving the offer and demand curves from agents' optimal plans, in order to study the equilibrium of the economy next. This done, Walras moved on to consider a slightly more complicated economy, an *n*-good exchange economy. His next step was to introduce production into the

[11] Hicks wrote *Value and Capital* ([1939] 1946), a contribution to Walrasian theory, and the IS-LM model, a contribution to Marshallian theory, in roughly the same period. This led many commentators to believe that the IS-LM model was Walrasian, without investigating the matter further.
[12] "Breaking up a complex question, studying one bit at a time, and at last combining his partial solutions with a supreme effort of his whole small strength into some sort of an attempt at a solution of the whole riddle" (Marshall 1920, p 366)

picture. In the end, he had a chain of encompassing models, starting from the simplest and moving towards greater and greater completeness: the two-good exchange-economy model, the *n*-good exchange-economy model, the production model, the capital-formation and credit-economy model and, finally, the monetary-economy model. Figure 1 illustrates Marshall's and Walras's strategies diagrammatically.

Figure 1. Marshall and Walras's models for simplifying the real economy



Marshall made it his priority to study the particular rectangles in the upper part of Figure 1 (branches of the economy during a given time span) separately. The point was not that theory should be confined to the study of a single rectangle, it was rather that economists needed to proceed gradually. For example, in his fishing industry example, Marshall studied the gravitational process between market-day, short-period and long-period equilibrium within an industry — that is, in terms of the Figure, he looked at vertical connections. He also endeavored to construct horizontal connections. Except for his Note XXI in the *Principles*' Mathematical Appendix, he hardy entered into the study of the economy as a whole.

One rarely hears of Marshallian general equilibrium models. Look, however, at Keynesian macroeconomic models as they developed in the line opened by Klein and Goldberg (Klein and Goldberg 1955). It turns out that they fit my description of a Marshallian economy remarkably well. In these models, the economy is sub-divided into separate sectors of activity. Initially these were limited in number, each being accounted for by a few equations: the consumption sector, the investment sector, the monetary sector, the employment sector, the government, the international sector. Progress was seen as consisting of adding new equations to the model. These additions were presumed to make the model more descriptively accurate. They also made them bigger and bigger. Each of these sectors became the object of a separate theoretical treatment. Little consideration was given to overall consistency, that is to the piecing together of sectoral analyses. All these traits make these models belonging to the Marshallian representation of the economy. In his interview with Klamer (Klamer 1984), Sargent pointed out that this is where the new approach made the difference. Although he does not mention either Marshall or Walras by name, what he describes is nothing else than a shift from a Marshallian to a Walrasian framework:

> The earlier literature proceeded as if you could build an optimizing consumption function, an optimizing investment schedule, an optimizing portfolio schedule, in isolation from one another. They are essentially partial equilibrium exercises which were then put together at the end. The Brookings model, built in [19]65, is a good example of this practice. They handed out these various schedules to different people and put them together at the end. The force of rational expectations is that it imposes a general equilibrium discipline. In order to figure out people's expectations you had to assume consistency (Klamer 1984, p. 66).

I see macroeconomics as a simplified general equilibrium model. It is small wonder then that Lucas's "Expectations and the neutrality of money" model ([1972] 1981) can be interpreted as a return to square one of Walras's construction, the two-good exchange-economy model evoked above. Not that these models are a mere copy of Walras's model. Lucas introduced additional elements, several of which were borrowed from the Arrow-Debreu model, such as intertemporal substitution and a more complex notion of commodity. Still, the Walrasian lineage is undeniable.

Table 1 substantiates my claim. It shows that can be viewed as a series of slight modifications to Walras's initial model. Lucas's model basically remains one where two goods are exchanged (they become three due a richer definition of a commodity). Production is already present, which is not the case in Walras's model, yet in the most elementary way, as labor is not marketed. Likewise, money is present in order to have nominal shocks, yet it does not enter the utility function. All these changes are significant, yet they should be seen as elaborations of Walras's initial framework. In fact, the most important departure from Walras's model concerns the information assumption.

Table 1 Lucas**'s** neutrality of money model as an amended Walrasian two**-**good exchange model

	Walras's model	Lucas's model
1) Type of economy	A two good-exchange economy; the two goods are physically different and are consumed during the same time period; substitution is intra-period	A three good-production economy (c, c' and leisure); the two goods are physically identical yet are consumed at different dates; the physical good is non-storable; because of the self-employment assumption, only two goods are traded; substitution is both intra and inter-period
2) Price formation	Tâtonnement: an auctioneer presides over a single exchange set-up[13]	One tâtonnement process per point in time
3) Organization of trade arrangement	No medium of exchange	Existence of a medium of exchange which does not enter the utility function and whose magnitude changes stochastically from one point in time to another
4) Agents	*n* different optimizing agents each of whom is endowed with only one of the two goods	2-*n* agents belonging to two overlapping generations; all are identical except for their age
5) Information assumption	Agents hold perfect information about all relevant non-private data (which in this context means the quality of the two goods)	Imperfect information: young agents ignore the present-day location of the two stochastic variables (although they know their density functions)

The real business cycle stage

Beyond doubt, there is a relationship of continuity between real business cycle models and Lucas's work.[14] They are based on the same conceptual ingredients: a perfectly competitive economy, the equilibrium discipline, rational expectations, a stochastic dynamic environment, and intertemporal substitution. The two main departures Kydland and Prescott (1982) made from Lucas's approach were: abandoning his view of a money-driven cycle, an important move, which also meant a departure from the Friedmanian vision; engaging in applied work. This second change deserves further attention.

In his "Methods and problems in business cycle theory" paper, Lucas ([1980] 1981, p. 288) stated that the task ahead was to write a FORTRAN program. According to Lucas, the

[13] Walras did not make this assumption explicitly yet with hindsight it turns out to be a necessary ingredient of his approach.
[14] No surprise here. Lucas and Prescott were in close contact and had engaged in joint work, for example their 1971 paper, "Investment under uncertainty".

macroeconomist's aim must be to construct "a fully articulate artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies" (Lucas [1977] 1981, p. 219). However, Lucas himself contributed little to this enterprise. In contrast, Kydland and Prescott took Lucas's injunction literally and devoted themselves to the task. In Woodford's words:

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

Woodford is right. However, merely asserting that qualitative modeling gave way to quantitative fails to convey the full measure of the change. Behind this contrast lies another, more sociological, difference. While models *à la* Lucas could mobilize only a tiny fraction of the macroeconomic profession, Kydland and Prescott were able to devise a research program which would provide bread and butter to legions of macroeconomists, both top-notch researchers and more run-of-the mill contributors, for decades to come. This is what it means to have a successful scientific revolution.

In a certain way, the relationship between Lucas and Kydland and Prescott replicates that between Keynes and his followers. What would have happened to the *General Theory* if its message had not been transposed into the IS-LM model, and if Klein had not extended this model into an econometric framework? While no answer can be provided to such a question, it reminds us that, in a field such as economics, there is no single compelling way in which theory will evolve. The same conundrum arises over the relationship between Lucas, on the one hand, and Kydland and Prescott, on the other. Without Kydland and Prescott, would the seismic change that macroeconomics underwent have occurred? It is far from sure. Lucas's conceptual papers were impressive but too highbrow to generate a huge following. As to Lucas's criticism, its impact on the profession could have been limited to making modelers more cautious when drawing conclusions from their models, and not produced a radical change in method. To have a scientific revolution, an alternative way of doing applied work needed to be conceived, providing new grist to the mill for the majority of members of the community. This was Kydland and Prescott's main contribution.

To get a taste of the titanic nature of the task Kydland and Prescott set themselves, it suffices to survey their research protocol, i.e. the different steps that needed to be taken to construct and assess a real business cycle model.[15] I will describe them with reference to Kydland and Prescott's initial paper, "Time to build and aggregate fluctuations" (1982).

Two parallel tasks were involved. On the one hand, work had to be done on real-world data, which often needed to be rearranged. On the other hand, the model had to be constructed and computed. The first part of the first task was to gather growth data. At the start of their work, Kydland and Prescott also found it necessary to make the national accounting categories consistent with their theory. In their model, there was no government sector, no household production sector, no inventories, and no foreign sector. As a result, the data relating to these sectors needed to be reassigned. Moreover, national accounts categories are built on the premise that consumer durables are part of consumption. However, from their theoretical viewpoint, they should be included in capital stock, and their flow assigned to GNP. Another important job was to detrend the data. Business cycles are defined as occurring with a frequency of three to five years around the trend. To isolate them, it is necessary to eliminate lower-frequency movements, related to long-run factors. The most widely used filter in the real business cycle literature is the Hodrick-Prescott filter (Hodrick and Prescott 1980).

As to the model economy, several steps are involved. The first is to assign a precise functional form to production and utility that is consistent with the facts on growth. Adhering to the equilibrium discipline, Kydland and Prescott restricted their attention to model economies that displayed balanced growth. They opted for a Cobb-Douglas production function, integrating a productivity variable, and a utility function restricted to a unitary elasticity of substitution between leisure and consumption. These functional forms are still widely used today. The first choice is justified by the fact that capital's and labor's shares of output have been approximately constant in the US in the period since WWII. The second is vindicated by the combination of a roughly constant level of leisure per capita and of a large rise in the real wage rate. Luckily, these two functional forms also happen to be the most tractable ones.

Assigning values to the parameters of the model economy is a crucial step in the program. To this end, Kydland and Prescott resorted to a new methodology, calibration, [16] which differs from econometric testing. This method, they claimed, was already in use in the natural sciences, as well as in computational general equilibrium theory. But, to say the least, they gave it a new impetus. Calibration consists of choosing values for the model economy's parameters either by drawing on existing empirical studies, independent research or national accounting data or by applying economic theory. The more the parameters can be valued in the first way, the better. In their 1982 paper, Kydland and Prescott found themselves with six

[15] See Kydland and Prescott (1991; 1996), Cooley and Prescott (2006).
[16] For a presentation of the calibration method see, for example, Wickens (1995), Cooley (1997) or Prescott and Candler (2008). For its criticism see Hoover (1995).

'free' parameters still in need of receiving a quantitative value. These pertained mainly to intertemporal substitution and technology shocks. Different values could be calculated for their various combinations. Kydland and Prescott choose those that resulted in a close correspondence between the moments predicted by the model and those of the real-world series.

The next task is to solve the model. The equilibrium process ought to be computed and simulated to generate equilibrium paths for the economy, using the recursive competitive equilibrium concept (Stockey and Lucas 1989). Again, this part of the program is central. More often than not, it is a time-consuming activity which turned the earlier pen-and-paper economists into a computational experts. Finally, after an additional filtering procedure, the researcher can proceed to calculate the selected moments both for the model economy and for the rearranged real-world data and compare them.

When all these steps are completed, a final assessment is possible. Kydland and Prescott's account of their own research runs as follows:

> Using the standard neoclassical production function, standard preferences to describe people's willingness to substitute intra- and inter-temporally between consumption and leisure, and an estimate of the technology shock variance, we found that the model economy displays business cycle fluctuations 70 percent as large as did the U.S. economy. This number is our answer to the posed question (Kydland and Prescott 1996, p. 74).

In this description, I have referred to Kydland and Prescott's "Time to build" paper. In subsequent work, things have become easier because of the cumulative development of the approach. However the job to be done is still daunting. Much of it comprises working on the data and making computations, all tasks requiring long hours of tedious calculation.

Once a scientific revolution has been successful, it is part of the job of its initiators to emphasize the differences between the 'before' and the 'after'. Kydland and Prescott (in practice, often Prescott alone) took this task on board. Take the inaugural paragraph of Prescott's Nobel Prize lecture:

> … And that is the story I am going to tell: how macroeconomic policy and research changed as a result of the transformation of macroeconomics from constructing a system of equations of the national accounts to an investigation of dynamic stochastic economies (Prescott, 2006, p. 203).

In their 1991 paper, Kydland and Prescott drew a contrast between the pre- and post-revolutionary paradigm by coining the terms *system-of-equations approach* and *general equilibrium approach* respectively for them.

> General equilibrium models have people or agents who have preferences and technologies, and who use some allocation mechanism. The crucial difference between the general equilibrium and the system-of-equations approaches is that which is assumed invariant and about which we organize our empirical knowledge. With the system-of-equations approach, it is behavioral equations which are invariant and are measured. With the general equilibrium approach, on the other hand, it is the willingness and ability of people to substitute that is measured (Kydland and Prescott 1991, p. 163).[17]

To me the above characterization is insufficient.[18] A reference to the Marshall-Walras contrast would be more to the point. Moreover, some additional features pertaining to the methodological implications of the transition from qualitative to quantitative modeling are worht bringing out.

Economists engaging in qualitative work are usually modest about their work. They are aware that their model is just a thought experiment providing a benchmark for looking at reality. Arrow and Debreu never claimed that their model explained the working of real-world economies. With quantitative modeling, a different tune emerges, at least when it comes to Kydland and Prescott. Modesty has gone, being replaced by bluntness. To them, the equilibrium business cycle model is no longer just a conjecture about reality; it constitutes a successful explanation of real-world business fluctuations. To the long-standing question of what explains these, they have an answer, equilibrium responses to real shocks — an answer which they claim to have, to a large extent, empirically vindicated.

Kydland and Prescott's method turns out to be a marriage of sorts between Lausanne and Chicago economics, a move that purists of the two schools used to disapprove of. Earlier neo-Walrasian authors were skeptical of, if not opposed to, any direct transposition from a model to reality. To them, their models were evolving at a level of abstraction which made them

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[17] "By general equilibrium we mean a framework in which there is an explicit and consistent account of the household sector as well as the business sector. To answer some research questions, one must also include a sector for the government, which is subject to its own budget constraint. A model within this framework is specified in terms of the parameters that characterize preferences, technology, information structure, and institutional arrangements. It is these parameters that must be measured, and not some set of equations" (Kydland and Prescott 1991, p. 168).

[18] A minor point is that the terminology is unconvincing. Why the label 'system of equations'? After all, general equilibrium theory is also a system of equations. Why are the equations in the traditional system more 'behavioral' than those in the new one?

unfit for empirical testing. For example, Cass, whom Kydland and Prescott hail as their forerunner, expressed his dissatisfaction with seeing his model transformed in the Kydland and Prescott way (Cass 1998). [20] On the other side of the fence, Friedman, the emblematic Chicago economist, was radically opposed to Walrasian theory.[21] In contrast, neither Kydland and Prescott nor Lucas have any qualms about such a mingling of the two traditions.

A related striking trait of Kydland and Prescott's methodological standpoint is their unshakeable faith in neoclassical theory, that is any model with "agents maximizing, subject to constraints and market clearing" (Kydland and Prescott 1991, p. 164). This is just Lucas's equilibrium discipline. When it comes to macroeconomics, they view the neoclassical growth model as the 'established theory'.

> I view the growth model as a paradigm for macro analysis — analogous to the supply and demand construct of price theory (Prescott [1986] 1994, p. 266).

> Macroeconomics has progressed beyond the stage of searching for a theory to deriving the implications of theory (Prescott 2006 p. 203-4).

Klein constructed his models with the purpose of assessing the validity of Keynes's 'hypothetical system' (Klein 1955, p. 280). True, he had a somewhat biased way of engaging in this exercise by contriving to put the economy in a Keynesian regime with excess supply in the labor and the goods markets. Nonetheless the official aim of the enterprise was to put the theory to the test. This is no longer the case for Kydland and Prescott. The shift that occurred with them is from a methodological standpoint where the validity of a theory has to be established to one where the aim is to apply a well-established theory.

Earlier on, macroeconomists 'believed' in neoclassical theory but took it with a large pinch of salt — Solow being a fine example. This was the reason for their adhering to the neoclassical synthesis view. The latter encapsulated the idea that 'classical' theory was valid only in the long term, which in turn suggested that a non-classical theory (Keynesian theory) was needed to explain the here and now. This attitude of conditional or mitigated adhesion to neoclassical theory has now disappeared.

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[20] Hahn is another example. "It is for all these reasons that I have always held the view that the Walrasian theory in all of its manifestations is an important theoretical benchmark but that a vast and unruly terrain had to be traversed before one understood, let alone predict, the behavior of an actual economy. No economist and certainly no theorist should be ignorant of the Walrasian theory, and no economist and certainly no theorist should pronounce on actual economies and policies on its basis alone" (Hahn 1983, p. 224)

[21] See De Vroey (2009a) for further discussion of this point.

EXPLAINING THE NC/RBC REVOLUTION

Lucas's and Prescott's views

As we have already seen, in his Nobel lecture, Prescott expresses his pride at having led a revolution that transformed macroeconomics. He described the state of affairs that prevailed before the revolution as follows:

> Prior to the transformation, macroeconomics was largely separate from the rest of economics. Indeed, some considered the study of macroeconomics fundamentally different and thought there was no hope of integrating macroeconomics with the rest of economics, that is with neoclassical economics. Others held the view that neoclassical foundations for the empirically-determined macro relations would in time be developed. Neither view was correct (Prescott 2006, p. 203).

To Prescott, the change was mainly methodological. In his eyes, it was so radical that the term revolution is deserved, and so compelling that it could not but win. Beyond these broad declarations, he devoted little attention to what actually happened. Basically, it was scientific progress in action.

> The reign of this system-of-equations macroeconomic approach was not long. One reason for its demise was the spectacular predictive failure of the approach. As Lucas and Sargent point out, in 1969 these models predicted high unemployment would be associated with low inflation. Counter to this prediction, the 1970s saw a combination of both high unemployment and high inflation. Another reason for the demise of this approach was the general recognition that policy-invariant behavioral equations are inconsistent with the maximization postulate in dynamic settings. The principal reason for the abandonment of the system-of-equations approach, however, was advances in neoclassical theory that permitted the application of the paradigm in dynamic stochastic settings. Once the neoclassical tools needed for modeling business cycle fluctuations existed, their application to this problem and their ultimate domination over any other method was inevitable (Kydland and Prescott 1991, pp. 166-167).

For a more detailed account of the changes that went on, we may turn to Lucas's brilliant article, "Methods and problems in business cycle theory" ([1980] 1981).[22] He considers three

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[22] Lucas was opposed to the terminology of revolution: "Research in my field of specialization — macroeconomics, or monetary and business cycle theory — has undergone rapid change in the past 15 years. One way of describing some of these changes is in terms of ideological contests between rival schools of thought: the 'Keynesian revolution', the 'monetarist counter-revolution', and so on. There is no doubt something to be learned by tracing the main ideological currents in macroeconomic research, but I myself find

forces that may have been active: technical developments, outside events and the internal development of the discipline. In his eyes, the first of these was most important. He views progress in economic theory mainly as a matter of discovering or applying new tools, new techniques for treating old issues. In effect, one leitmotif of Lucas's methodological writings is that earlier economists felt the need to study the economy in a dynamic way but lacked the necessary tools.[23] Two important transformations took place to change this state of affairs. The first was the possibility of resorting to new mathematical tools, borrowed from engineering, such as control theory. They allowed for the construction of dynamic theory. The second was the increased computational ability associated with the tremendous progress that took place in computer science, paving the way for large-scale simulation work.

As to new developments "thrown at us by the real world" (Lucas [1980] 1981, p. 272), Lucas dislikes giving them too much importance because this would run counter to his premise that all business cycles are basically alike.[24] He nonetheless considers two external influences that may have played a role in the rise of new classical macroeconomics. The first is the stagflation period that characterized the 1970s. To him, it constituted a dramatic quasi-laboratory experiment that confirmed the rightness of Friedman's intuition ([1977] 1981, p. 221). However, Lucas stops short of claiming that this was a decisive cause of the change ([1980] 1981, pp. 282-283). The second is the fact that in the post-World War II period business cycles have followed a regular pattern, giving weight to the view that they are a repeated occurrence of the 'same' event. All in all, however, he sees technical progress as the driving force:

> These new observations have been influential (as new observation should be to empirical researchers) but it seems to me that the main outside influences have been, and will continue to be, changes in available theoretical methods. In business cycle theory, it appears not to be the problem that changes but rather the way we look at it. Of changes in methods, certainly the most central have been postwar developments in general equilibrium theory (Lucas [1980] 1981, p. 284).

While the appearance of new tools made the change in approach possible, this change manifested itself in internal theoretical developments. Most of Lucas's paper on 'Methods and problems' is devoted to these. He shows how post-WWII economists relied upon the stationary equilibrium concept. To them, the economy was in a state of disequilibrium as long as it departed from the stationary state. The business cycle was viewed as a manifestation of

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most of this discussion of crises, revolutions and so on, unintelligible, and almost wholly unconnected with the most interesting current research" (Lucas 1987, p. 1).

[23] See Lucas ([1980] 1981, p. 275), Lucas (1987, p. 2), Lucas (1996, p. 669-.

[24] He admits that the Great Depression remains a "formidable barrier to a completely unbending application of the view that business cycles are all alike" (Lucas ([1980] 1981, p. 273).

such a departure. Since disequilibrium was furthermore equated with a mismatch between supply and demand, the main form of which was the Keynesian case of excess supply, the business cycle was deemed to be a market failure. This was how an essentially static theory was supplemented with short-run dynamics, the basic insight of the neoclassical synthesis. Lucas's claim is that this framework, combining a static long-run equilibrium with short-run disequilibrium dynamics, was used for lack of a better one. This lack in turn was explained by the fact that a series of advances had not yet been made. In particular, possible lessons for the study of the business cycle had not yet been drawn from the Arrow-Debreu model. Once this was done, thanks to Lucas and his associates' contributions, in conjunction with the technical advances that had become available, a radically new framework became possible, wherein " the idea that an economic system in equilibrium is in any sense 'at rest' is simply an anachronism" (Lucas p. 207).

This is how Prescott and Lucas account for the emergence of the new approach in macroeconomics. Two remarks are called forth. First, the notion of a scientific revolution has a relativistic ring attached to it. It suggests that any new paradigm that emerges, having dethroned a pre-existing one, will be dethroned in its turn in the future. We may envisage, for example, that one day it will be possible to conceptualize the idea of individual disequilibrium so that the equilibrium approach will become *passé*. This is not Lucas and Prescott's view. They see the science of economics as being on a path of continuous progress. Hence, although Prescott uses the revolution term, he is no more a follower of Kuhn than Lucas is. Second, it is clear that what we have here is history written from the point of view of the 'winners'. What Prescott and Lucas see as theoretical advances is considered a backward step by the 'losers', the defenders of the neoclassical synthesis. For lack of space, I shall content myself with giving two quotations retorts to NC/RBC economists:

> I argue … that there was no anomaly, that the ascendancy of new classicism in academia was instead a triumph of *a priori* theorizing over empiricism, of intellectual aesthetics over observation and, in some measure, of conservative ideology over liberalism (Blinder [1988] 1997, p. 110).

> To many Keynesians, the new classical programmed replaced messy truth by precise error (Lipsey 2000, p. 76).[25]

A political agenda?

In the quotation above, Blinder evokes a factor of which, not surprisingly, no trace is to be found in Lucas's or Kydland and Prescott's accounts. Blinder's accusation is that the

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[25] Other critical articles are Summers ([1986] 1994) and Solow (2001).

NC/RBC revolution, to some extent at least, resulted from the desire to promote a laissez faire approach and to dismiss the more interventionist Keynesian ideology that had previously prevailed. This is a point that is worth investigating, although such a line of thought will hardly be popular amongst economists. Most will reject it, be they friend or foe of the NC/RBC revolution. One underlying reason is the tremendous influence exerted by Friedman's conception of the methodology of economics, asserting its positivistic character. A basic tenant of this view is that value judgments have no place in economic theory.

For sure, it will be difficult to find NC/RBC macroeconomists claiming that they pursue a political agenda. Let me give just two examples of denials of the presence of such a purpose. The first is from Klamer's interview with Sargent.

> [Klamer]: *Are the political aspects an important question?*
>
> [Sargent]: I am not really interested in politics. The rational expectations stuff is clearly not politically motivated. People from all sorts of different political perspectives contribute to it. It's more a technical revolution.… No, it's certainly not politically motivated (Klamer 1984, p. 80).

My second example is Kehoe and Prescott's reply to Temin's (2008) criticism of their work in the *Journal of Economic Literature*. One of Temin's indictments was that real business cycle authors were pursuing a political agenda. Kehoe and Prescott's reacted by writing:

> We can assure Temin and readers of our book is that there is no agenda in the book but a scientific one. The suggestion that we have such an agenda is yet another example of Temin's lack of understanding of the book (Kehoe and Prescott 2009, p. 21).[26]

I have no reason to question the sincerity of Sargent, Prescott, and other authors in the NC/RBC tradition. But for several reasons that does not close the question. First of all, returning to square one, we must remember that political economy started with Adam Smith as a plea in favor of a system of economic liberty. In the *Wealth of Nations* positive and normative aspects are intertwined. Still today, an important part of the theoretical conversation among economists bears on whether the invisible hand is doing its job. This is true, not for all fields of economics, but for those that deal with the economy as a whole, as is the case with macroeconomics. The issue at stake is how the economy can best be organized — with or without state intervention, and, if with, to what extent? If an opinion about the

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[26] Lucas lapsed at least once into taking a more relativistic standpoint when he said in an interview that: "In economic policy, the frontier never changes. The issue is always mercantilism and government intervention vs. laissez faire and free market" (Lucas 1993: 3).

ideal organization of the economy is ideological, then ideology cannot be absent from macroeconomics. To me, this is not a stigma. It just means that economics is less remote from political philosophy than is usually believed.[27] As to Friedman, for all my admiration for him, I find it difficult to swallow that he was a value-judgment-free economist. All his life he passionately pleaded the cause of laissez faire, using every type of argument he came across.

What are the possible ideological views in presence in macroeconomics? In a recent paper (De Vroey 2009b, written in French) I have distinguished seven degrees of economic liberalism with at its top 'Austrian economic liberalism' and at its bottom 'communism', complete state control of the economy. As far as macroeconomics is concerned, the spectrum can be limited to two levels, the 'laissez faire' and the 'Keynesian' conceptions. The 'Keynesian' modifier can be viewed as a catchword for grouping those authors who, although generally supportive of the market system, nonetheless believe that it can exhibit failures which state intervention, in particular demand stimulation, can remedy. Here is Blinder's account:

> A normative Keynesian believes that government should use its leverage over aggregate demand to reduce the amplitude of business cycles. He or she is probably far more interested in filling in cyclical troughs than in shaving off peaks. These normative propositions are based on judgments that (a) macroeconomic fluctuations significantly reduce social welfare, (b) the government is knowledgeable and capable enough to improve upon free-market outcomes, and (c) unemployment is a more important problem than inflation (Blinder 1988: 112–3).

The laissez faire (or anti-Keynesian) view is that the unfettered working of competition will lead the economy to the best attainable position. To repeat a well-known aphorism, the state is the problem, not the solution. Stabilization policies are neither necessary nor efficient.

A second factor that casts doubt on the non-ideological character of macroeconomics appears when the genealogy of the main models that have succeeded each other over the history of macroeconomics is retraced, and their policy origins explored. The results are summarized in Table 2.

[27] My understanding of the term 'ideology' is relativistic, unlike its common-sense understanding which is often pejorative, pointing to the alleged bad faith of political opponents.

Table 2 The policy cause defended

	Justifying demand activation	Laissez-faire
The *General Theory*	♦	
The IS-LM model	♦	
Disequilibrium theory (Patinkin and Leijonhufvud) and non-Walrasian equilibrium models	♦	
Monetarism		♦
New classical models (Lucas)		♦
New Keynesian models	♦	
Real business cycle models (Kydland & Prescott)		♦
The new neoclassical synthesis	♦	♦

The to-and-fro process illustrated in the table looks incompatible with the hypothesis of an unwavering march towards increased knowledge based exclusively on conceptual, technical and factual advances. Some additional factors must have played a role. Political agendas are a candidate.

Finally, a third argument is that the policy conclusions which a given theory or model can support are constrained by their premises. According to the type of model adopted, certain results are possible, others are excluded. To wit, an elementary Walrasian model with complete markets and without externalities excludes coordination failure and states of underemployment. Of course, researchers can enter into a theoretical framework that has policy implications without having a political motivation. More importantly, they can be blind to the predetermined character of the policy conclusions of the model they use. Let me refer again to Kehoe and Prescott's answer to Temin. Having stated that they are engaged in science and not politics, they make the following statement:

> The underlying hypothesis of the book is that the general equilibrium growth model is a useful tool for studying great depression episodes. The tentative findings are that bad government policies can turn ordinary economic downturns into great depressions. These finding are especially relevant now, late 2008 (Kehoe and Prescott, 2009, p. 21).

Following in Friedman and Schwartz's footsteps, Kehoe and Prescott view government failures (i.e. mistaken policy measures) as responsible for great depressions. They may be right, but the problem is that this conclusion is worth little if it is based on a model that excludes any other causes, in particular trading malfunctioning, in an *a priori* way. To really determine the reasons for great depressions we should have models that consider all the main suspects. In other words, as soon as an equilibrium model is used, the dice are stacked. I admit that, at present, disequilibrium models are too hard to construct. In this situation, two attitudes are possible. The first is to admit to the limitations of the type of model employed, and, as a result, to avoid drawing definite conclusions. The second, taken by Kehoe and Prescott, is to be oblivious of the implications of the approach, and to voice blunt conclusion). To me, this is a methodological fraud, a case of theoretical hubris, and it has an ideological ring. This is possibly what Blinder and Temin had in mind.

It is thus well worth pondering the role of ideology in the NC/RBC revolution. The challenge is to be able to combine the statements made by its leaders (that they had no political motivation) and the presence of an ideological dimension in their approach. One view that needs to be discarded from the outset is that all practitioners of macroeconomics are aware of its ideological dimension. Most of them are mere foot soldiers in the regiment. They march in step without any need to be aware of where the regiment is coming from or where it is heading. This being said, most macroeconomists do have ideas about the best economic policy. So, individually, a large number of them could place themselves on the spectrum from laissez faire to Keynesian; but often this has no impact on their theoretical practice. There may be exceptions, people who consider themselves pure technicians. Some may change their minds over time (possibly because of the study of economics). None of this matters for my claim.

I do not want to claim that every theoretical move should be viewed as the result of a political agenda. The starting point for engaging in theoretical innovations under the impulse of a political agenda is finding that the prevailing paradigm leads to policy conclusions that you dislike. But these are unusual situations. Only a few episodes in the history of macroeconomics, to my mind three, qualify. We have Keynes, who wanted to amend what he called classical theory; Friedman, who wanted to recast Keynesian macroeconomics in order to reach laissez faire conclusions; and New Keynesian economists who wanted to import Keynesian themes into the new theoretical discourse introduced by new classicists and real business cycle theorists.

Note that I have not included either the new classical or the real business cycle episodes in this category of politically-motivated theoretical moves (let me repeat that I have nothing against these). The reason is that Lucas, Sargent, Kydland and Prescott did not need to act upon a political agenda, because much of the job of gearing theory towards a political agenda other than the Keynesian had already been carried out by Milton Friedman!

Friedman had a political agenda and used macroeconomics in a way that was not conceptually revolutionary but marked a radical shift in terms of policy conclusions. Economists like Lucas and Sargent boarded the train after it had left. While they held the same laissez faire ideology as Friedman, they no longer needed to promulgate it. Rather they could concentrate on working, as pure technicians, on the conceptual and technical modifications that were necessary to de fiprovirm ground for Friedman's policy conclusions. In other words, a division of labor occurred, with Friedman doing the political job, and the next generation undertaking the theoretical developments needed to underpin the political agenda inaugurated by Friedman.

CONCLUDING REMARKS

In this paper, I have tried to make the following points. First, I have criticized the standard understanding of the requirement for microfoundations, and argued that a softer understanding of it, admitting states of individual disequilibrium, is more appropriate. Unfortunately, theories based on it are still non-existent. The NC/RBC revolution can be described as a transition from the soft to the demanding understanding of this requirement. It implied the expulsion of market non-clearing from macroeconomics. Second, I have characterized the new classical stage of the revolution as having introduced a series of novelties: a merger of the fields of value theory and business cycle theory that were previously separate, the expulsion of unemployment from the agenda of macroeconomics, the replacement of Marshallian by Walrasian macroeconomics. Third, I have shown how Kydland and Prescott made Lucas's qualitative model quantitative. I have suggested that if this had not been done, a scientific revolution *à la* Kuhn might not have occurred. Fourth, my study Lucas's and Kydland and Prescott's accounts of the NC/RBC shows that, although Prescott used the term 'revolution', what these authors had in mind was just the uninterrupted march of progress, in contrast to my own interpretation and a far cry from a Kuhnian perspective. Fifth, I have addressed the challenge of conciliating the assertion made by the leaders of the NC/RBC revolution that they were doing science without a political agenda with the fact that laissez faire policies are a foregone conclusion in their models. This has been possible, I have argued, because the job of putting forward the laissez faire political agenda had previously been carried out by Friedman, so that his disciples could devote themselves to the task of solving theoretical puzzles and doing applied work.

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