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from *"A History of Macroeconomics, from Keynes to Lucas and Beyond,"*

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A Methodological Breach

The aim of this chapter is to substantiate my claim that Lucas's work marked a turning point in the history of macroeconomics. Any scientific revolution always comprises an important methodological component. I will argue that, in the case at hand, three important shifts stand out: a change in the *quaesitum* of macroeconomics, a change in the conception of the relation between theory and model and, finally, a change in the notion of equilibrium upon which the analysis is based. The overall picture surfacing from my study is that these differences are linked with Lucas's decision to make macroeconomics neo-Walrasian, while traditional Keynesian macroeconomists followed Keynes by adopting a Marshallian methodology.

Lucas had a long-standing interest in methodological problems; in an interview with Snowdon and Vane, Prescott hailed him as "the master of methodology, as well as defining problems" (Snowdon and Vane 2005: 351). While he never wrote a systematic exposition of his methodological standpoint – his "Methods and Problems in Business Cycle Theory" ([1980a] 1981a) comes closest to such an enterprise – it nonetheless worth noticing that about half the essays in his book, *Studies in Business Cycle Theory*, are of a methodological nature. Moreover, these are only the tip of the iceberg. Exploring the Lucas Archives at Duke University's Special Archives division (Lucas. Various) reveals the existence of many drafts and notes by Lucas pertaining to methodological issues. They are invaluable for my inquiry for several reasons: they may complement the published works, they may shed light on the genesis of Lucas's mature vision, and they may reveal Lucas's deep-seated views, what he really believed but preferred not to include in published articles. I will abundantly draw from this little known material.[1]

[1] I am aware that unpublished pieces found in archives also raise difficulties: they must be interpreted cautiously because the reason they have remained unpublished may well be that the author abandoned the views expressed in them. Be that as it may, the greater the congruency of drafts with published pieces, the higher the likelihood that they are more than tentative ideas committed to paper. This is often true for Lucas, a typical situation being one in which ideas that are expressed succinctly, if not inadvertently, in a published article are based on a fuller treatment in earlier notes which remained unpublished.

A CHANGE IN THE RESEARCH AGENDA

Macroeconomics arose in the wake of the Great Depression from the desire to bring to the fore the existence of market failures on which the state should act. It is a small wonder that the field was almost naturally skewed towards social reformers. Unemployment was considered the main of these market failures. Therefore, it became the main research topic of macroeconomics.[2] After the Lucasian revolution, things changed sweepingly, as business fluctuations became the defining object of study of macroeconomics. The unemployment topic was no longer regarded as a priority and disappeared from the radar. Research was redirected towards the level of activity, that is, the total numbers of hours worked. Here is how Lucas justified this move in his book *Models of Business Cycles*:

> In most such models [of the business-cycle] unemployment as a distinct activity plays no role whatever. For many other economists, explaining business cycle is taken to *mean* accounting for recurrent episodes of widespread unemployment. From this alternative viewpoint, a model with cleared markets seems necessarily to miss the main point, however successful it may be accounting for other phenomena, and the work of "equilibrium" macroeconomists is often criticized as though it were a failed attempt to explain unemployment (which it surely does fail to do) instead of as an attempt to explain something else. (Lucas 1987: 48)[3]

Several factors came into play for explaining this sweeping change of agenda. Awareness of the shortcomings of Keynesian macroeconomics was one of them. The availability of new tools allowing a more rigorous study of dynamics, was a second factor. A third one was the emergence of search modeling to the effect that the study of unemployment could be sent back to labor economics. Broader historical events also played a part. The memory of the Great Depression faded away. The contrasted evolution of socialist and capitalist economies strengthened the belief in the superiority of the market system. In many countries, unemployment ceased to be the haunting theme that it used to be.

While earlier the view of business cycle theorists centered more on turning points than on fluctuations, often regarded as caused by idiosyncratic events, Lucas shifted the emphasis toward fluctuations, further arguing

[2] A remnant of the earlier view is Azariadis's statement in an entry in the *New Palgrave Dictionary* that "involuntary unemployment is for many economists the *sine qua non* of modern macroeconomics" (Azariadis 1987: 734).

[3] As will be seen, the unemployment theme made a return in later developments of the DSGE program.

that they exhibited enough regularity to make the construction of a general theory possible.[4]

Lucas's project rested on the premise that neoclassical theory, understood as neo-Walrasian theory, should be taken more in earnest than the macroeconomists believed at the time. It cannot be stated that the historical context was particularly suited to this view. At the turn of the 1970s, neoclassical theory was rather on the defensive. On the one hand, a radical political economy stream was emerging with people who wanted to return to Marxian intuitions.[5] On the other hand, Herbert Simon, also at Carnegie like Muth and Lucas, had suggested that optimizing rationality should be replaced with bounded rationality, a view that at the time was compelling for many.[6] The distinctive feature of Lucas and the likes of him was to have taken the exact counterpoint. They thought, in Sargent's words, that "Keynes and his followers were wrong to give up on the possibility that an equilibrium theory could be account for the business cycle" (Sargent 1977: 14). Positively, they believed that the future of macroeconomics lay in becoming more neoclassical rather than less ('more' is an understatement here, as they meant that macroeconomics should be fully absorbed into neoclassical theory).

> The most interesting recent developments in macroeconomic theory seem to me describable as the reincorporation of aggregative problems such as inflation and the business cycle within the general framework of 'microeconomic' theory. If these developments succeed, the term 'macroeconomic' will simply disappear from use and the modifier 'micro' will become superfluous. We will simply speak, as did Smith, Ricardo, Marshall and Walras of *economic* theory. (Lucas 1987: 107–108)

LUCAS ON METHOD[7]

Standards

Lucas held a narrow view of what macroeconomic theory ought to be. To him what mattered when doing theory was respecting a series of methodological standards.

(a) There should be no split between the principles underpinning microeconomics and those underpinning macroeconomics (Lucas 1987: 107–108). That is, macroeconomics without (choice-theoretical) microfoundations is sub-standard.

[4] "With respect to the qualitative behavior of co-movements among series, *business cycles are alike*. To theoretically incline economists, this conclusion should be attractive and challenging, for it suggests the possibility of a unified explanation of business cycles, grounded on the general laws governing market economies, rather than in political or institutional characteristics specific to the particular countries or periods" (Lucas [1977] 1981a: 218, Lucas's emphasis).

[5] Cf. Piore (2013). [6] Cf. Sent (2002).

[7] This section is drawn from De Vroey (2011a). Other studies of Lucas's epistemology are Vercelli (1991) and Boumans (1999, 2005).

(b) Macroeconomics is part of general equilibrium analysis. Its concern is the working of an entire economy, and it must account for the interactions between the component parts of the economy. It must be dynamic, that is, dealing with the economy over time. Uncertainty is accounted for by ascribing probabilities to future states of the world and to the occurrences of shocks. Thus, the DSGE acronym, which had not yet been invented, fits what Lucas had in mind.

(c) A macroeconomic theory and a mathematical model are one and the same thing. This conception, which can be traced back to Walras, runs counter to another, more widespread, understanding of the relationship between theory and model. According to the latter, they are distinct entities: a theory is a set of propositions about reality while a model, be it mathematical or in prose, is an attempt at rigorously setting out the implications of some part of the theory.[8]

(d) A theory is concerned with imaginary constructions; it is avowedly nonrealistic.

> Insistence on the 'realism' of an economic model subverts its potential usefulness in thinking about reality. . . . On this general view of the nature of economic theory then a 'theory' is not a collection of assertions about the behavior of the actual economy but rather an explicit set of instructions for building a parallel or analogue system – a mechanical, imitation economy. A 'good' model, from this point of view, will not be exactly more 'real' than a poor one, but will provide better imitations. (Lucas [1980a] 1981a: 271–272)

> The central assumptions of macroeconomic models, such as rational expectations, ought thus to be viewed as modeling devices, model-building principles rather than propositions about reality.

> One can ask, for example, whether expectations are rational in the Klein-Goldberger model of the United States economy; one cannot ask whether people in the United States have rational expectations (Lucas. Various. Box 23, Barro Folder).

By prescribing these standards, Lucas was treading Walras's footsteps. However, on two other scores, he adhered to the earlier practice of macroeconomics. First, he held the strong conviction that macroeconomic models are of no interest if they fail to reach policy conclusions.

> 'The central question that macroeconomists need to resolve: Which necessarily abstract models can help us to answer which practical questions of economic policy? (Lucas. Various. Box 26, Reflections on contemporary economics folder).

[8] Thus, in the traditional view, the model is subservient to the theory. The following quote from Leijonhufvud is a fine depiction of this viewpoint: "I propose to conceive of economic 'theories' as a set of beliefs about the economy and how it functions. They refer to the 'real world'. . . . 'Models' are formal but partial representations of theories. A model never encompasses the entire theory to which it refers" (Leijonhufvud 1997). A different approach of the theory-model relationship can be found in Boumans (2005) and Morgan (2012).

Second, he regarded macroeconomics as an applied rather than a purely abstract field. A confrontation between theory and reality needed to be at its heart. What was to be done was 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] 1981a, p. 219), what I call 'Lucas's FORTRAN injunction.'

> Our task as I see it is to write a FORTRAN program that will accept specific economic policy rules as 'inputs' and will generate as 'output' statistics describing the operating characteristics of times series we care about, which are predicted to result from these policies (Lucas [1980a] 1981a: 288).

A theory/model ought to be assessed on its ability to make correct predictions. The better its ability to reproduce past events, the more trustworthy the model is for assessing new policy measures.

Model economies as analogous systems

There is a tension in Lucas's methodological standpoint bearing on how to bring together the disparate sets of requirements I just exposed. Lucas's solution to this riddle was to regard models as analogous systems. Time and again, one finds Lucas writing that economic models should be viewed this way. In Lucas's "Methods and Problems" article alone, I counted seven occurrences of the term 'analogy.' For example:

> Progress in economic thinking means getting better and better abstract, analogue economic models, not better verbal observations about the world. (Lucas [1980a] 1981a: 276)

However, in his published papers, Lucas gave no clue as to what he meant exactly.[9] This gap can be filled by searching through his archives. They contain a series of drafts – some handwritten, some typed – in which he expanded at length on the notion of an analogy, on the role of models, and on the relationship between modeling and economic policy.[10] The subsequent discussion is based on one such fragments.

[9] According to Boumans, Lucas's analogical vision of modeling cane interpreted as a Turing test (Boumans 2005: 92–96).

[10] My guess is that these drafts were written at the end of the 1970s, a period when Lucas gave many seminars on rational expectations and business cycle theory. Unfortunately, the drafts are collected in a disorderly manner in the archives: a series of fragments, often four or five pages long, all related to the same themes, but without any indication as to how they might be combined. To the best of my knowledge, they have never appeared in a finalized form in a published piece. In view of their interest and the lack of easy access to them, I shall quote from them extensively.

Lucas's argumentation starts from two premises, both of which may come as a surprise. The first is that a model, though a fiction, is nonetheless an observable reality:

> We speak of modeling phenomena or models *of* phenomena, suggesting that observed phenomena are one kind of thing and models of them another thing, but I want to define a model to be itself a phenomenon: something the behavior of which can be observed. Then what is the relationship between a set of phenomena and the second set that we call a model of the first set? I will call this relationship *analogy*. (Lucas. Various. Box 27, adaptive behavior folder).

This still does not clarify what Lucas meant by the term 'analogy.' An answer is provided later in the same draft, where Lucas wrote that he took an analogy to "mean a symmetric relationship between two things." These things may either be a thing in the usual sense and a theory, which to Lucas is just another thing, or two distinct procedures for generating observations. In another set of notes, Lucas added that "we must make liberal use of analogies: judgments that one situation is similar enough to another to call for the same reaction."

Lucas's second premise was that economic theory (or at least general equilibrium theory) is utopian in nature, a proposition that he drew from a (bold) comparison between economics and anthropology:

> Economic theory, like anthropology, 'works' by studying societies which are in some relevant sense simpler or more primitive than our own, in the hope either that relations that are important but hidden in our society will be laid bare in simpler ones, or that concrete evidence can be discovered for possibilities which are open to us which are without precedent in our own history. Unlike anthropologists, however, economists simply invent the primitive societies we study, a practice which frees us from limiting ourselves to societies which can be physically visited as sparing us the discomforts of long stays among savages. This method of society-invention is the source of the *utopian* character of economics; and of the mix of distrust and envy with which we are viewed by our fellow social scientists. The point of studying wholly fictional, rather than actual societies, is that it is relatively inexpensive to subject them to external forces of various types and observe the way they react. If, subjected to forces similar to those acting on actual societies, the artificial society reacts in a similar way, we gain confidence that there are useable connections between the invented society and the one we really care about. (Lucas. Various. Box 13, Directions of macroeconomics 1979 folder).

Surely, anthropologists will fail to recognize themselves in Lucas's account but, leaving that aside, the comparison successfully brings out Lucas's message: models are fictitious economies and by manipulating them, we can learn about the functioning of real economies. Lucas's reason for resorting to the use of analogies followed from his view that macroeconomics should be used to assess the impact of policy measures. Normally, these cannot be assessed experimentally, the stagflation episode of the 1970s being a possible exception. So, second-best solutions need to be found. One of these is to look for analogous real-world experiences – has there been another, not too different, country

where the policy under consideration has been tried? If yes, this experience may constitute a valuable benchmark. Unfortunately, such real-world analogies are scarce. Again, a way out exists. The reference for such comparisons does not need to be a real-world experience; a model economy can do the job. Actually, it can do it better, the advantage of the model economy over the real-world economy being that it can be controlled to improve on the similarities it offers. This is how Lucas justified model building.[11] He readily admitted that an analogy which one person finds persuasive will look ridiculous to another. Is there an escape? Here is his answer:

> Well, that is why honest people can disagree. I don't know what one can do about it, except keep trying telling better and better stories, to provide the raw material for better and more instructive analogies. How else can we free ourselves from the limits of historical experiences so as to discover ways in which our society can operate better than it has in the past? (Lucas 1988: 5)

Thus, no matter how arcane mathematical models may seem, in the end they are always a story. At the beginning, it may be vague. Progress consists in 'making it better' in terms both of the concepts introduced and of the logical steps involved in the argumentation.

I started my presentation of Lucas's view of a theory by arguing that, to him, a theoretical proposition is a statement about a fictitious economy rather than about an actual economy. As a result, the right question to ask about such propositions is not whether they are true or false (because the answer is always 'false'). The right way to look at it is: "All we can say about an analogy is that it is good or bad, useful or useless, and such subjective terms only raise further questions: Good for what? Useful for what purpose"? (Lucas. Various. Box 23, Barro folder).

> Do we value this theory (if one can discuss valuing the theory of value!) because we agree that it implies a set of verbal propositions about observations that can be refuted by keeping our eyes open for black swans [because seeing a black swan would indicate that the proposition that all swans are white is refuted (MDV)]? If so, what are these sentences that express – more accurately than the theorems or formulas themselves – what this theory really means, really implies? Shall we test the theory by checking sentences like: "in all economies, production possibility sets have nonempty interiors?" or "People tend to act in their own self-interest"? against what we see, the way we are supposed to check swan colors? Shall we dismiss Arrow and Debreu's theory as vacuous,

[11] "I like to think of theories – economic and psychological, both – as simulatable systems, *analogues* to the actual system we are trying to study. From this point of view, the Wharton model, say, bears the same kind of logical relationship to the United States economy as France, say, does: it is just a different economy, or system, but one that is similar enough to the U.S. economy that we might hope to learn about the properties of one through the study of the other. If our objective is to learn what the consequences of introducing a value added tax in the U.S. might be, we might study its consequences in France or simulate the Wharton system under such a tax or, better still, do both." (Lucas. Various. Box 27, adaptive behavior folder; Lucas's emphasis)

> and Kydland and Prescott's application of it as wrong? (Yes, I think, except for 'dismiss'). An alternative point of view toward things and theory is this: we observe things and events, and we perceive analogies among them. (Lucas. Various. Box 27, adaptive behavior folder).

A NEW EQUILIBRIUM CONCEPT

From Adam Smith onward, equilibrium has played a central role in economic theory, one of the main features differentiating economics from the other social sciences. The traditional view of equilibrium was that it means a state of rest. This was also how it was understood in Keynesian macroeconomics until the Lucasian revolution. The latter introduced a new equilibrium concept in macroeconomics. It could be named 'dynamic equilibrium,' but this label is equivocal since the state of rest view also comprises a dynamic dimension (yet an unsatisfactory one). I will therefore use the 'intertemporal equilibrium' terminology. Lucas did not invent the new concept; he imported it into macroeconomics from Arrow and Debreu's theory of competitive equilibrium. My aim in this section is to contrast the two concepts.

The traditional understanding of equilibrium: equilibrium as a state of rest

First of all, it must be noticed that traditional equilibrium theory is a set of propositions about reality. It is taken for granted that the functioning of real economies can be interpreted as manifesting that equilibration forces, the so-called 'market forces,' are at work in them. Among many others, here are three quotations, drawn from economists of different allegiances, written in support of the state of rest concept.

> Such is the continuous market, which is perpetually tending towards equilibrium without ever actually attaining it, because the market has no other way of approaching equilibrium except by groping. . . . Viewed in this way, the market is like a lake agitated by the wind, where the water is incessantly seeking its level without ever reaching it. (Walras1954: 380)[12]

> The ordinary economic situation is one of disequilibrium moving in the direction of equilibrium rather than of realised equilibrium. (Viner [1931] 1953: 206)

> If we are to make empirically interesting statements about disequilibrium and equilibrium, statements that have potential empirical content, we must define these terms so that both are meaningful and both can be observed – in order to say that in fact we do not observe one of them. (Lipsey 2000: 72)

[12] It seems odd to state that the intertemporal equilibrium concept was formulated by Debreu, the emblematic neo-Walrasian theorist, and give a quotation from Walras in support of the old concept. As argued aptly by Donzelli (1989), the fact is that Walras wavered between the old and the new concept.

In an article entitled, "Alfred Marshall's Theory of Value," Frisch (1950) compared the state of rest conception of equilibrium to a pendulum. Like a pendulum, the industry studied is supposed to have a single standstill position. When we observe that the pendulum is moving, we can infer that disequilibrium exists. The same holds for the industry. If prices and/or quantities change over time, it means that the industry is in disequilibrium. This view can be extended to the economy as a whole. In short, in Friedman's words, "an equilibrium position is one that, if attained, will be maintained" (Friedman 1976: 19).

The central feature of the state of rest conception of equilibrium is its static character: its object of analysis is what happens in an industry or an economy over a given period of time during which a single equilibrium allocation acts as a center of gravity. Here is how Marshall, who reflected a lot on the issues of time and adjustment, posited the framework in which this concept must be analyzed:

> The unit of time may be chosen according to the circumstances of each particular problem: it may be a day, a month, a year, or even a generation: but in every case it must be short relative to the *period of the market* under discussion. It is to be assumed that the general circumstances of the market remain unchanged throughout this period; that there is, for instance, no change in fashion or taste, no new substitute which might affect the demand, no new invention to disturb the supply. (Marshall 1920: 342, my emphasis)

Marshall proposed the 'period of the market' terminology. I prefer to call it the 'period of analysis.' I also find it misleading to call the center of gravity allocation a 'long period equilibrium,' as is often done. In my eyes, a better terminology is 'normal equilibrium' or 'period-of-analysis equilibrium.'

When the state of rest equilibrium concept is adopted, it must be assumed that the same equilibrium allocation exists at the beginning and at the end of the period of analysis. In other words, the initial configuration of data characterizing the economy or the market studied (technology, preferences, endowments, population, and states of the world) ought to still be found at the end of the period.

Another basic trait of this concept is that the notions of equilibrium and disequilibrium are organically linked. When the question is asked of whether equilibrium exists at a given date, the answer will usually be 'No': the economy generally is out of equilibrium, changes in prices or quantities being the indicators of such a state of affairs. There is nothing dramatic about such a situation, however. What matters is the existence of efficient re-equilibrating forces.

A final characterization is that, though basically static, this equilibrium notion comprises a dynamic dimension. It is related to stability, that is, the issue of how, after a shock, the unchanged equilibrium allocation is restored. In his *Foundations of Economic Analysis* (1947), discussing the economy as a whole rather than an industry as Marshall did, Samuelson encapsulated this re-equilibration process in a set of differential equations:

$$\frac{dp_i}{dt} = a_i E_i(p_i, \ldots, p_{m-1})$$

These equations refer to an exchange economy comprising m goods, with good m acting as numéraire; p_i is the numéraire price of good i, every a_i is a positive constant expressing the speed of adjustment in the i-th market, E_i is the excess demand function for good i.

This traditional conception of equilibrium may well receive some vindication from elementary physics, but the main factor explaining its adoption is that it corresponds to our common-sense understanding of equilibrium. It is thus no surprise that the founding fathers of economic theory adopted it. However, on reflection, its appositeness is far from obvious. The state of rest equilibrium concept captures one aspect of time, namely duration – it takes time for the effects of decisions to be realized. Unfortunately, it cannot come to grips with another of its dimensions, namely that the passage of time is accompanied by incessant irreversible changes, large or small. Therefore, as argued by Donzelli, it turns out to be a-temporal. As a result, any models based on it

> ... are structurally incapable of providing the slightest explanation of any economic phenomenon whose occurrence essentially depends on economic activities taking place in time. (Donzelli 1989: 158)

Changes in these data can occur during the period of analysis, but they must be reversible and temporary. Irreversible changes can only enter the picture in the interstices between periods of analysis, which means that that they cannot be part of price theory.

A related problem is that the length of the period of analysis cannot be inferred from observation but is decided by economists (this explains why I dislike Marshall's expression 'period of the market'). Yet, they face an impossible dilemma as they need to find a compromise between two opposite criteria: on the one hand, the period of analysis must be long enough to allow adjustment processes to take place, on the other hand, the longer it is, the more contrived the assumption that only reversible shocks can be considered. No satisfying solution to this dilemma can be found.

A last problem is that the speed of the adjustment process is a 'free parameter'; assigning a value to it is a decision left to the economist. If the latter is eager to argue that the economy evolves in disequilibrium, she just has to assign the speed of adjustment a low value, the opposite being true is she likes the idea of a quick return to equilibrium.

Lucas's equilibrium conception

In their "After Keynesian Macroeconomics" paper, Lucas and Sargent insisted that the new paradigm they advocated was based on a different equilibrium concept, intertemporal equilibrium, arguing as follows:

When Keynes wrote, the terms *equilibrium* and *classical* carried certain positive and normative connotations which seemed to rule out either modifier being applied to business cycle theory. The term equilibrium was thought to refer to a system at rest, and some used both *equilibrium* and *classical* interchangeably with *ideal*. Thus an economy in classical equilibrium would be both unchanging an unimprovable by policy interventions. With terms used in this way, it is no wonder that few economists regarded equilibrium theory as a promising starting point to understand business cycles and design policy to mitigate or eliminate them. In recent years, the meaning of the term *equilibrium* has changed so dramatically that a theorist of the 1930s would not recognize it. An economy following a multivariate stochastic process is now routinely described as being in equilibrium, by which it is meant nothing more than at each point in time, postulates (a) [that markets clear, MDV] and (b) [that agents display optimizing behavior, MDV] above are satisfied. This development, which stemmed mainly from work by K. J. Arrow and G. Debreu, implies that simply to look at any economic time series and conclude that it is a disequilibrium phenomenon is a meaningless observation. (Lucas and Sargent [1979a] 1994: 15)

Before discussing Lucas's equilibrium concept, I have one remark to make about the two postulates Lucas and Sargent mention. These postulates would have been better captured by writing that their approach rests on (a) optimizing *planning* and (b) a trade technology allowing for a generalized transformation of optimizing planning into optimizing *behavior*. Since in their framework (b) is taken care of by the auctioneer assumption, these two postulates can be combined into a single one, namely that all agents experience optimal behavior, in the spirit of McKenzie's observation, mentioned in Chapter 7 that general equilibrium is generalized individual equilibrium.

In Lucas and Sargent's new approach, time is framed as a succession of points in time which need to be dated. It is assumed that exchanges are confined to some of these. Decisions are made before trade. As a result, the analysis can be considered as an intertemporal planning problem. Equilibrium is defined as a state where all the agents (of the fictitious model) follow an optimizing consumption/leisure intertemporal path. It is no longer equated with 'being at rest,' and the impossible task of defining an adequate period of analysis vanishes. Observing that quantities and prices change across time no longer needs to be interpreted as meaning that the economy displays disequilibrium.

It is possible to construct systems in competitive equilibrium in a contingent-claim sense which exhibit a vast variety of dynamic behavior. The idea that an economic system in equilibrium is in any sense 'at rest' is simply an anachronism. (Lucas [1980] 1981a: 287)

Two traits of the new equilibrium concept must be underlined. The first one is that equilibrium is declared to exist as a postulate. This is well encapsulated in Lucas's 'equilibrium discipline' expression. The term 'discipline' must be understood as conveying the view that the equilibrium postulate is a rule that economists impose upon themselves when constructing their models. What matters for judging the appositeness of adopting such a stringent postulate is what can be done with models based on it (and what are the drawbacks of not adopting it). The second trait pertains to another important breach from the traditional conception of equilibrium. Supporters of stationary equilibrium take it for granted that equilibrium and disequilibrium are features of reality. Here, Lucas also embraces the opposite viewpoint. His view is that equilibrium ought to be understood as a characteristic of the way in which economists look at reality rather than as a characteristic of reality.

Cleared markets is simply a principle, not verifiable by direct observation, which may or may not be useful in constructing successful hypotheses about the behavior of these series. Alternative principles, such as the postulate of the existence of a third-party auctioneer inducing wage rigidity and uncleared markets, are similarly 'unrealistic,' in the not especially important sense of not offering a good description of observed labor market institutions. (Lucas and Sargent [1978] 1994: 21)

I think general discussions, especially by non-economists, of whether the system is in equilibrium or not are almost entirely nonsense. You can't look out of this window and ask whether New Orleans is in equilibrium. What does that mean? Equilibrium is a property of the way we look at things, not a property of reality. (Lucas's interview with Snowdon and Vane, 1998: 127)[13]

This change of equilibrium concept proposed by Lucas amounted to a Copernican revolution. The transformation involved comprises several dimensions: (a) from the state of rest to the intertemporal concept of equilibrium, (b) from an approach wherein equilibrium and disequilibrium are organically linked to an approach using only the equilibrium category, and (c) from an approach wherein the assessment of the existence of equilibrium or disequilibrium is a matter of characterizing reality to an approach where instead the equilibrium concept pertains only to the fictitious model economy. Keynesian economists had a hard time accepting this threefold modification. For example, as will be seen in Chapter 12, time and again they criticized Lucas on the grounds that it was obvious that in reality markets, and in particular the labor market, are in a state of disequilibrium.[14]

In light of these remarks, it proves difficult to assess whether the new conception of equilibrium, with its exclusion of the disequilibrium notion, amounts to attributing a higher or a lower role to the notion of equilibrium. Getting disequilibrium out of the picture may suggest a higher role for equilibrium. But there is another side to the picture: the fact that equilibrium has

[13] In other words, to use Weintraub's apt formulation, equilibrium is imposed upon the world: "This symposium provided additional examples of such argumentations: the discussions generated by McCallum's paper, and Grandmont's, contained various appeals to the 'Principle' that the world either was or was not in equilibrium. The commentators in this audience seemed to think that they had a way of discussing the truth of the idea that observed states were equilibria without committing themselves to any particular theory of macroeconomics. This is, of course, an illusion: equilibrium states, or disequilibria are characteristics of our theories, and are thus imposed on the world" (Weintraub 1990: 273).

[14] As stated by Farmer: "When economists before Lucas saw unemployment in the labor market, they thought that they were observing a market in disequilibrium" (Farmer 2010b: 70).

become a postulate and that equilibrium or disequilibrium are no longer claimed to be characteristics of reality amounts to shrinking the pretense of equilibrium theory. The assessment to make about the equilibrium discipline then does not consist in pondering whether it is realistic but whether it is an efficient way of constructing economic theory. Moreover, when every outcome is by construction an equilibrium outcome, the normative connotation that was associated earlier with equilibrium vanishes. Welfare considerations now need to focus on the comparison of alternative equilibrium positions.

According to Lucas, there were at least two reasons, both related to 'theoretical efficiency,' why macroeconomics must shift towards the intertemporal equilibrium approach. The first is that the traditional conception is wanting as explained above. The second reason is that dropping the disequilibrium notion is good riddance. It must be banned because it refers to 'unintelligent behavior' (Lucas [1977] 1981a: 225) or, in other words, it lacks microfoundations. Now that the new concept is available (thanks to him), Lucas stated, not using it would make no sense:

> To ask why the monetary theorists of the 1940s did not make use of the contingent-claim view of equilibrium is, it seems to me, like asking why Hannibal did not use tanks against the Romans instead of elephants. ([1980] 1981a: 286)

COMPARING KEYNESIAN AND NEW CLASSICAL MACROECONOMICS

In the above sections, I have dwelled on what are in my eyes the two central programmatic bifurcations made by Lucas. In this last section, I want to integrate these in a broader comparison of Keynesian macroeconomics and DSGE macroeconomics in its first installment, that is, new classical macroeconomics. Table 10.1 summarizes the contrast between them.

Several of these benchmarks have already been discussed earlier. Therefore, I will content myself with commenting the remaining ones.

The starting point

Keynes's *General Theory* lay the foundations for Keynesian macroeconomics, while Lucas's "Expectations and the Neutrality of Money" article did so for Lucasian macroeconomics. As noticed by Sargent, it is difficult to imagine two works that are more opposed, a complex kaleidoscopic 384-page book and a 21-page mathematical article:

> Many of us regard Lucas's 1972 *Journal of Economic Theory* paper as the flagship of the Revolution; it is different than the flagship of that earlier revolution, Keynes's General *Theory of Employment Interest and Money*, which was ambitious, wide ranging, imprecise and vague enough to induce twenty-five years of controversy about what the book really meant. Lucas's paper was a narrow, technical study. . . . There was never any confusion about what Lucas's paper meant. (Sargent 1996: 537)

TABLE 10.1 *Comparing Keynesian and new classical macroeconomics*

	Keynesian macroeconomics	New classical macroeconomics
1. Starting point	Keynes's *General Theory* (1936)	Lucas's "Expectations and the Neutrality of Money" (1972)
2. Overarching aim of macro	Explaining unemployment or underemployment	Explaining the business cycle
3. Main explanatory factor	Wage rigidity or sluggishness	Agents' response to monetary shocks in a signal extracting context
4. Equilibrium concept	State of rest equilibrium	Intertemporal equilibrium
5. General equilibrium	'Incomplete' general equilibrium	General equilibrium
6. Type of economy	Monetary economy	Monetary economy
7. Microfoundations	Not strictly required	Strictly required
8. Expectations	Adaptive expectations	Rational expectations
9. Driving factor, demand or supply	Demand	Supply
10. Real effects of monetary changes	Monetary activation works	Monetary activation does not work
11. Neoclassical synthesis	Support	Rejection
12. Empirical method	Cowles Commission method	Lucas critique
13. Methodological priority	External consistency	Internal consistency
14. Standard policy conclusion	Demand activation	Laissez faire
15. Intuitiveness and accessibility	High	Low

A general equilibrium approach?

Two criteria must be fulfilled for models to qualify as general equilibrium models. First, they must be concerned with the economy in its entity rather than with particular sections of it. Second, the analysis must encompass the study of the interactions between the composing branches of the economy. A general equilibrium intention was already present in Keynes's *General Theory*. But a full implementation did not follow up. Keynesian econometric models took entire economies as objects of analysis, but dealt with across

equations relations in an offhand way.[15] As for the DSGE program, it is fully general equilibrium.[16]

Money

Both modeling strategies are concerned with a monetary economy. Contrary to the other items, there is a similarity here.

Microfoundations

Microfoundations, as understood by Lucas, relate to Walras's method of starting the analysis by analyzing how agents make optimal choices. With a few exceptions, Keynesian models usually followed the Marshallian principle that analysis can start at the level of market supply and demand functions rather than at the individual decision-making level. It is not that agents are assumed to behave in a non-optimizing way, but rather that this stage of the reasoning is skipped. On the contrary Lucas regarded microfoundations *à la* Walras as a *sine qua non* for writing sound theory. To Lucas, microfoundations were more than the choice-theoretical apparatus. They also involved bringing people into the picture, a perspective that he claimed was absent from Keynesian macroeconomics.

> I think a lot of the work in Keynesian economics has gotten too far away from thinking about individuals and their decisions at all. Keynesians don't often worry about what actual individuals are doing. They look at mechanical statistical relationships that have no connection with what real individuals are actually doing. (Lucas. Various. The Margin's interview. Box 7, Correspondence 1989 folder).

The driving factor, supply or demand?

According to the Keynesian approach, variations in output and employment result from changes in aggregate demand. The underlying picture is that labor suppliers are passive, employment decisions being made unilaterally by firms. Moreover, this approach tends to consider the supply of labor and the labor force as the same thing, a fixed magnitude. By contrast, in the DSGE program the driving force of economic activity is the supply of labor. This change in emphasis, forward-looking behavior

[15] "*Sargent*: 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" (Sargent's interview by Klamer 1984: 66).

[16] As seen, non-Walrasian equilibrium models anticipated this move.

being combined with the idea of intertemporal leisure substitution, is a main ingredient in Lucas's view of macroeconomics. An implication of this move from demand to supply is a shift of emphasis from firms' to households' decision-making process.

Real effects of monetary changes

While Hicks favored demand activation under the form of fiscal policy, Modigliani argued in favor of monetary activation. From then on, Keynesians held the conviction that monetary expansion can increase the level of employment in a sustained way. Friedman and Lucas shared the goal of demonstrating that Keynesians were wrong.

Empirical method

Keynesian macroeconomics embraced the Cowles Commission's simultaneous equation method, witnessing to impressive developments. The Lucas critique brought out its flaws.

Methodological priority

The central methodological principle of Keynesian macroeconomics was external consistency. In this view, models are only as good as they are realistic. The prevailing intellectual mood was pragmatic. That several of the basic notions – involuntary unemployment, full employment, rigidity, and sluggishness – were defined in a loose way, or that the analysis focused on the short period with no attention being given to the linkage between the short and the long period, were by no means considered harmful methodological practices. Empirical models, the construction of which was often left to engineers rather than economists, were more data-than theory-constrained. Lucas wanted macroeconomics to abide by the Walrasian methodological principles. Accordingly, internal consistency became the alpha and the omega of theoretical construction.

Natural policy conclusions

The vision of the economy closest to Keynesian macroeconomics can be branded 'mitigated liberalism.' It defends the market system as being superior to a planning system without fully advocating laissez-faire. Keynesian models tend to support demand activation by the state. The opposite conclusion, a defense of laissez-faire, emerges from new classical macroeconomics. Fluctuations in employment reflect rational and optimal reactions by economic agents to changing conditions. If there is no market failure, there is also no need for the state to intervene.

Intuitiveness of the approach taken and accessibility to the layman

Keynesian macroeconomics theory is simple to understand even by non-economists. Its level of technicality is low. Many of its basic notions have made their way into newspapers and political discourse. Lucasian macroeconomics resorts to new, complex mathematical techniques, such as dynamic programming. Consequently, the technical barrier to entry is much higher than for Keynesian macroeconomics. The Lucasian conception of theory and model is counter-intuitive and of little appeal to the layman.

*
* *

These are, in my view, the most salient traits on which the two approaches of macroeconomics can be compared. They stand in sharp contrast. However, the breach is not total. They still have a monetary economy as their object of analysis in common. This makes for one precise object of disputation between them, the issue of the real effects of monetary policy.

Three final remarks must be made about this comparison. First, one clue to understand what underpins this contrast and at which I have hinted on several occasions is that the change that occurred can be interpreted as a transition from Marshallian to neo-Walrasian macroeconomics. Second, I need to underline that my comparison only serves a pedagogical purpose. Its main drawback is probably that it is static. This does a disservice to the presentation of Keynesian macroeconomics. It extended over more than two decades, and many improvements took place over this period, so that a snapshot such as mine is necessarily a simplification. My last remark is that the comparison above concerns only the first wave of modeling in the DSGE program. When I will come to the study of the subsequent waves, it will be seen that most of the specificities of the first one remained valid, but that there were significant shifts nonetheless.

11

Assessing Lucas

In this chapter I assess Lucas's contribution to macroeconomics. In the first section, I express my disagreement with his judgment about Keynes. In the second, I briefly comment on intertemporal substitution, the cornerstone of DSGE macroeconomics. In the third, I discuss whether Lucas's attempt of making macroeconomics Walrasian may be regarded a valid move. In the fourth, I probe into Lucas's methodological standpoint by displaying what I consider to be two ambiguities in his approach. Lucas's opponents have often argued that his work was ideologically motivated. This is a point that should not be swept under the rug. Therefore, in the penultimate section I consider whether it is a valid contention. Finally, the chapter ends with a few concluding remarks.

LUCAS ON KEYNES

Lucas's claim, that *The General Theory* is a minor contribution to economic theory – that "Keynes was not a very good technical economist" (Usabiaga Ibanez 1999: 180) – has the merit of being original. Lucas may have been led to make such a judgment because of his conviction that economic theory ought to be mathematical. Still, I find it difficult to agree with him. By passing such an a-historical judgment, Lucas went against what he wrote in his "Problems and Methods" article, namely, that economists of the past, as clever as they may have been, could not do much better than what the contemporary level of development of the discipline allowed. With respect to the standards of his time, there is no reason to state that Keynes was a bad technical economist. On the contrary, gauged against the writings on unemployment at the time— say, Hicks's *Theory of Wages* (1932) and Pigou's *Theory of Unemployment* (1933) – Keynes's work seems to me much superior as far as breadth of inspiration and conceptual innovations are concerned.

Let me give just one example of Keynes's sharp mind. That the notion of rational expectations could have been conceived of in Keynes's time is

unimaginable. Still, Keynes hinted at the gist of it when criticizing Tinbergen's *Statistical Testing of Business Cycle Theories* (1939).[1] One of Keynes's complaints concerned the absence of expectations in Tinbergen's estimations: "Is it assumed that the future is a determinate function of past statistics? What place is left for expectations and the state of confidence relating to the future?" (Moggridge 1973: 287). Amazingly enough, Keynes's criticism of Tinbergen's model makes one think of the Lucas Critique.

> I also want to emphasize strongly the point about economics being a moral science. I mentioned before that it deals with introspection and with values. I might have added that it deals with motives, expectations, psychological uncertainties. One has to be constantly on guard against treating the material as constant and homogeneous. It is as though the fall of the apple to the ground depended on the apple's motives, on whether the ground wanted the apple to fall, and on mistaken calculations on the part of the apple as to how far it was from the center of the earth. (A letter from Keynes to Harrod, dated July 16, 1928, quoted in Moggridge 1973: 300)

In light of this quotation, the difference between Keynes's and Lucas's results largely from the state of development of economics they each faced. Observing that there was no way to integrate expectations in Tinbergen's econometric model, Keynes declared that one should dispense with econometrics. He was not followed however. Decades later, Lucas found that what needed to be done was to drive econometrics in a direction enabling it to come to grips with Keynes's preoccupation.

INTERTEMPORAL SUBSTITUTION

No economist denies the existence of the phenomenon of intertemporal substitution. Incorporating it into macroeconomics was long overdue. The problem lies elsewhere, in that DSGE macroeconomics needs a strong intertemporal leisure elasticity of substitution and hence a high instantaneous elasticity of labor supply. Lucas had initially dealt with the issue in an offhand manner.[2] Soon, however, the validity of this assumption became the subject of hot debates. Several econometric studies argued that no elasticity of labor supply of the size needed for Lucas's claim to hold could be found in the data.[3] Although this criticism was crucial as it touched on the cornerstone of the new classical paradigm, it hardly stopped the momentum of the Lucasian revolution. The reason is that intertemporal substitution is such a crucial ingredient of the whole program that it cannot do without it, while alternative mechanisms are scarce. In Lucas's words:

> I see no way to account for observed employment patterns that does not rest on an understanding of the intertemporal substitutability of labor. The literature contains innumerable examples of possible additional, supplementary considerations, but to my knowledge no alternatives. (Lucas 1981a: 4)

The debate about the empirical validity of the assumption of a high leisure intertemporal substitutability has persisted up to the present (although interesting progress has taken place; more about this in subsequent chapters). For the sake of the argumentation, let us assume that the defenders of low elasticity are right. Would this suffice to condemn the DSGE program? My answer, based on the conclusion of my discussion of monetarism, is 'No': demonstrating the empirical invalidity of a theoretical proposition that is a part of a wider theoretical paradigm (however central it may be) is not a sufficient condition for dismissing the whole paradigm.

THE PROS AND CONS OF WALRASIAN MACROECONOMICS

Lucas's methodological standpoint can be summarized in the statement that macroeconomics should be based on neo-Walrasian principles. He once wrote "I am a hopeless 'neo-Walrasian'" (letter to Driscoll, dated November 23, 1977 Lucas. Various. Box 30). He also set himself apart from Friedman on the grounds that the latter was Marshallian whereas he was Walrasian.[4] When thinking of neo-Walrasian theory, the names which come to mind are those of Kenneth Arrow, Gérard Debreu, Lionel McKensie, Frank Hahn, David Cass, and Karl Shell, among others. These economists are also those Lucas seemed to refer to in his "Method and Problem in Business Cycle Theory" article. Yet it is unsure whether neo-Walrasians were ready to consider him as one of their own. At least, somebody like Cass was not:

> Bob [Lucas] was in the Chicago tradition and was very concerned about empirical testing – whatever the hell that means – something that I have little sympathy for and very little interest in, to be perfectly honest. So there was quite a difference in viewpoints about why you did theory and what the relevance of theory is (Cass's interview with Spear and Wright 1988: 546).

[1] The context of Keynes's criticism has been described in Note 24, chapter 1.

[2] "What we do know indicates that leisure in one period is an excellent substitute for leisure in other, nearly periods. ... The small premium required to induce workers to shift holidays and vacations (take Monday off instead of Sunday, two weeks in March rather than in August) point to the same conclusion, and this 'causal' evidence is somewhat more impressive because of its probabilistic simplicity: holidays are *known* to be transitory. On the basis of this evidence, one would predict a *highly elastic* response to transitory price changes" ([1977] 1981a, p. 224).

[3] Ashenfelter (1984) surveys this literature.

[4] "*Snowdon and Vane*: You acknowledge that Friedman has had a great influence on you, yet his methodological approach is completely different to your own approach to macroeconomics. Why did his methodological approach not appeal to you? *Lucas*: I like mathematics and general equilibrium theory. Friedman didn't." (Lucas's interview by Snowdon and Vane 1998: 132).

The problem did not lie in Lucas's "Expectations and the Neutrality of Money" paper in itself. It was acceptable to general equilibrium theorists as it resembled the incursions into neighboring territories in which they sometimes indulged. What matters in Cass's quotation is the last sentence, that is, his perception that neo-Walrasian theory and macroeconomics pursue different purposes and hence must not be mingled. Contemporary neo-Walrasian economists regarded general equilibrium theory as an abstract construction, the strength of which lay in its ability to posit issues in a rigorous way. They were also aware of its limits. In their view, the best it could provide was to be a negative benchmark.

In a nutshell, behind the veil of their mathematical language, neo-Walrasians do political philosophy *à la* Rawls, while macroeconomists engage in applied work. Lucas may well have wanted macroeconomics to become Walrasian, but he also wanted it to keep its long-standing traits of empirical verification and policy conclusions. This was not to neo-Walrasian economists' liking.[5] To them, testing their models empirically made little sense. As noticed by Roy Weintraub, "empirical work, ideas of fact and falsifications, played no role at all" in Walrasian theory (Weintraub 1983: 37). Or, in Hahn's words:

> 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 predicted) 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: 224).

In neo-Walrasian theory agents are price-makers. Hence there must be someone else announcing prices, the auctioneer. The auctioneer hypothesis, though necessary to explain the formation of equilibrium in neo-Walrasian theory, has little to commend it.[6] Indeed, it amounts to pre-empting the main issue that should be addressed, that is, whether market forces are able to bring the economy to a state of efficiency.

As for policy conclusions, the problem is not just that neo-Walrasian theory evolves at a stratospheric level of abstraction. It is also that Keynes's indictment of classical theory because it relied on a 'Panglossian' vision of the economy (Pangloss is Candide's tutor in Voltaire's eponymous book) fully applies to neo-Walrasian theory:

> The celebrated optimism of traditional economic theory, which as led to economists being looked upon as Candides, who, having left this world for the cultivation of their gardens, teach that all is for the best in the best of all possible worlds provided we will let well alone ... (Keynes 1936: 53).[7]

[5] In this respect, Lucas was more in the line of the vision held at the Cowles Commission.

[6] Solow declared it a 'swindle' in his review of the English translation of Walras's *Elements* (Solow 1956: 88).

Leaving the imaginary, best-of-all-possible worlds behind in order to study the malfunctions likely to arise in real-world economies is certainly not what occurs when taking the Walrasian track. Thus, with respect to Keynes's call to forgo the Panglossian vision, Lucasian macroeconomics is clearly a step backward. Economists such as Hahn and Solow, and many others, found this move outrageous:

> The irony is that macroeconomics began as the study of large-scale economic pathologies: prolonged depressions, mass unemployment, persistent inflation, etc. This focus was not invented by Keynes (although the depression of the 1930s did not pass without notice). After all, most of Haberler's classic *Prosperity and Depression* is about ideas that were in circulation before *The General Theory*. Now, at last, macroeconomic theory has as its central conception a model in which such pathologies are, strictly speaking, unmentionable. There is no legal way to talk about them (Hahn and Solow 1995: 2-3).

Though I can understand Hahn and Solow's viewpoint, I am less categorical than they are. At stake is the internal/external consistency dilemma, the worthiness of engaging in theoretical detours and ... patience – in one of his *notes d'humeur* (casual annotations scribbled on pieces of paper) Walras wrote:

> One must know what one is doing. If one wants to harvest promptly, one should plant carrots and salads; if one has the ambition to plant oak trees, one must be wise enough to say: [posterity] will owe me this shade (Baranzini and Allison (2014: 1).

A possible justification of Lucas's standpoint can be found in a 1989 *Journal of Economic Perspectives* by Charles Plosser. According to Plosser, the results of Keynesian economists' attempts to theorize malfunctions were disappointing because they were introduced in an *ad hoc* way, instead of being systematically derived from a theoretical core model. The lesson he drew was that it is wiser to start the analysis with the study of an idealized state of the economy. In his words:

> Progress towards understanding this idealized state is essential because it is logically impossible to attribute an important portion of fluctuations to market failure without an understanding of the sorts of fluctuations that would be observed in the absence of the hypothesized market failure. Keynesian models started out asserting market failures (like unexplained and unexploited gains from trade) and thus could offer no such understanding. (Plosser 1989: 53)

Taking such a position is admissible at a condition that I suggest to call the 'non-exploitation principle.' Models *à la* Lucas may well have policy

[7] Willhem Buiter dubbed his criticism of new classical macroeconomics, "The Macroeconomics of Dr Pangloss" (1980).

conclusions as their normal outlet. Yet, their builders should refrain from recommending these to decision makers. Clearly, this implies a strong dose of stoicism. To his credit, Lucas expressed awareness of this precept as the following passage from the concluding section of his "Understanding Business Cycles" article testifies:

> By seeking an equilibrium account of business cycles, one accepts *in advance* rather severe limitations on the scope of governmental countercyclical policy which might be rationalized by the theory. (Lucas [1977] 1981a: 234, Lucas's emphasis)

The important element of this quote is the term 'in advance,' italicized by Lucas. By stressing this, he admitted hat the limitation on countercyclical policy, the policy conclusion of his model, followed from its premises. Again, while this point is only hinted at in Lucas's published papers, more is to be found in the archives:

> One now reads of rational expectations not in *Econometrica* but in *Time* and *Business Week*, where it appears as a 'school' or 'theory' with apparently sweeping implications for important issues of economic policy. These implications seem primarily of a 'conservative' cast, favoring a reducing role for government, balanced budget fiscal policy, and tight and 'unaccommodating' monetary policy. Now the idea of limited government, budget balance and tight money are not unimportant to me; they are high on the list of values I carry into the voting booth every year, and for reasons I am willing to defend in some detail. These developments are not, then, ones which I find unwelcome or displeasing, nor do I find the journalistic treatment of rational expectations any less accurate than similar treatments of other developments in economics. . . . There can be no simple connection between what appears on the scratch pads of professional economists, however original, and important conclusions about the way our society ought to operate. (Lucas. Various. Box 13, Directions of macroeconomics 1979 folder).

Interpreted in a loose way, this passage states that economists should be cautious when extending the policy conclusions of their models into direct advice to governments. Taken strictly, it means that economists should totally refrain from politically exploiting the results of their models.

LUCAS'S AMBIGUITIES

Wavering between monetarism and DSGE reasoning

Like Friedman's 1968 article, Lucas's 1972 paper aimed at demonstrating that monetary shocks can have real effects only in as far as they cannot be perfectly unanticipated. When Lucas expanded this into a model of the business cycle, he kept money shocks as the causal factor of fluctuations. However, a few years later, he admitted that RBC modeling, wherefrom money is absent, was the right line to follow. Lucas dated the 'beginning of the end' of his viewing the explanation of business fluctuations in terms of monetary shocks to the 1978 Bald Peak Conference where Kydland and Prescott presented an early

version of their "Time to Build and Aggregate Fluctuations" paper. Nonetheless, in an October 1982 correspondence with Leijonhufvud, who wrote in a letter to him that he did not understand why he was such a monetarist, Lucas kept claiming his monetarist allegiance.

> 'My' monetarism is simply Friedman's. I've tried over and over again to make this clear, but I don't think people take it seriously. I suppose this is because Friedman and I have such different styles of doing economics, but really, style has nothing to do with it. (Lucas. Various. Box 3. Letter to Leijonhufvud, October 28, 1982. Correspondence 1982 folder).[8]

True to himself, Lucas expressed the same viewpoint three decades later in the Introduction of the volume collecting his papers on monetary theory (Lucas 2013):

> Now toward the end of my career as at the beginning, I see myself as a monetarist, a student of Milton Friedman and Allan Meltzer. My contributions to monetary theory have been in incorporating the quantity theory of money into modern, explicitly dynamic modeling. . . . It is understandable that in the leading operational macroeconomics models today – real business cycle models and new Keynesian models – money as a measurable magnitude plays no role at all, but I hope we can do better than that in the models of the future. (Lucas 2013: XXVI–XXVII)

These two statements must be related to an intriguing observation made by Sargent in an article commemorating the twenty-fifth anniversary of Lucas's "Expectations . . ." article (Sargent 1996). Sargent's point was that this article, which played such a seminal role, was actually "the first and last paper [Lucas] would write in this line" – a line that Sargent defined as "an unrelentlessly 'deep' approach to modeling monetary and macroeconomic phenomena in terms of explicitly spelled out phenomena" (Sargent 1996: 544), the very program that he set for himself.

> The link to monetarism in Lucas's *Journal of Economic Theory* paper was incidental to the methodology of the rational expectations program, but integral to the substance of Lucas's own research program. (Sargent 1996: 544)

The first part of the quotation was confirmed by what happened afterwards, Kydland and Prescott's transformation of Lucas's model into RBC modeling from which the monetary dimension was absent, while nonetheless abiding by the DSGE program as set out by Lucas. As for Lucas's own and distinct research program, Sargent characterized it as mere monetarism, that is, (a)

[8] While nobody could be further from Lucas than Leijonhufvud, the latter nonetheless wrote in the letter to which the excerpt above was Lucas's answer: "I haven't read a better book (in economics) for years" referring to *Studies in Business Cycle Theory* (Lucas 1981a). The moral to be drawn is that economic theory is also a gentleman's sport.

trying to integrate money in price theory while keeping the latter intact, (b) using quantity theory to explain inflation and (c) regarding business fluctuation as resulting from monetary disturbances. According to Sargent, the 1972 article was hardly the best vehicle to carry this monetarist project forward. This, he claimed, explains why afterward Lucas returned to "a more superficial and workable approach using arbitrary cash in advance restrictions" (Sargent 1996: 544).

Replacing the neoclassical synthesis dichotomy with another one

One of the criticisms of the Lucas-Rapping 1969 paper, voiced by Albert Rees (1970), was that Lucas and Rapping's analysis implicitly assumed that all unemployment during the Great Depression was voluntary:

> Though scientific discussion is supposed to be dispassionate, it is hard for one old enough to remember the Great Depression not to regard as monstrous the implication that the unemployment of that period could have been eliminated if only all the unemployed had been more willing to sell apples or to shine shoes. (Rees 1970: 308)

In their answer, Lucas and Rapping (1972) avoided discussing the meaning of involuntary unemployment, but admitted that their model was inadequate for tackling unemployment during the Great Depression.[9] Lucas returned to the issue of the Great Depression on several occasions, mainly in interviews or book reviews, although never in much detail. Referring to RBC models, he reiterated the same view that such models were unable to explain the Great Depression and that one needed to return to Friedman and Schwartz's analysis for such an explanation.[10] That is, RBC models are apt to study periods of plain sailing (what later became known as 'periods of moderation') but ill-suited when it comes to more dramatic events such as the Great Depression:

> In Kydland and Prescott's original model, and in many (though not all) of its descendants, the equilibrium allocation coincides with the optimal allocation: fluctuations generated by the model represent an efficient response to unavoidable shocks to productivity. One may thus think of the model not as a positive theory suited to all historical time periods but as a normative benchmark providing a good approximation to events when monetary policy is conducted well and a bad approximation when it is not. Viewed in this way, the theory's relative success in accounting for post-war experience can be interpreted as evidence that post-war monetary policy has resulted in near-efficient behaviour, not as evidence that money does not matter. (Lucas, 1994: 13)

[9] Actually, Lucas wrote this response alone because Rapping had lost interest in the subject.

[10] Other references are Lucas [1980a] 1981: 273, 284), Lucas (1987: 87), Klamer (1984: 41–2), Snowndon and Vane (1998: 125) and McCallum (1999: 284).

I personally agree with Lucas's standpoint. However, there is s a flip side to it, which Obstfeld and Rogoff aptly pointed out:

> A theory of business cycles that has nothing to say about the Great Depression is like a theory of earthquakes that explains only small tremors. (Obstfeld and Rogoff 1996: 627)

So, Lucas ended up supporting a divide between types of explanations according to the subject dealt with, the very idea that he strongly rejected *apropos* of the neoclassical synthesis![11] This is another ambiguity in Lucas's thinking.

A POLITICAL AGENDA?

In an article entitled "The Fall and Rise of Keynesian Economics," Alan Blinder, an eminent Keynesian economist from Princeton University who also held important political appointments at the Council of Economic Advisers and the Board of Governors of the Federal Reserve Bank, assessed new classical macroeconomics with the following words:

> I argue . . . 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] 2001: 110)[12]

In this quotation Blinder made two points. First, he expressed his regret about the 'Walrasation' of macroeconomics spearheaded by Lucas; Blinder's words are close to those which Friedman used to dismiss Walrasian theory. His second point may be interpreted in two ways – either as meaning that the conclusions of new classical models were more conservative than those of Keynesian models, or as meaning that the ascent of new classical models resulted from a political motivation. The first of these statements is obviously right. The second amounts to stating that Lucas pursued a political agenda. In Chapter 4, I concluded that this was the case for Friedman, his declarations to the contrary notwithstanding. Can the same conclusion be reached about Lucas?

When trying to answer this question, the first observation to be made is that Lucas departed from Friedman's assertion that theory and ideology could be radically split. In Lucas's mind, macroeconomics is geared towards producing policy conclusions, and such conclusions necessarily support a particular ideological vision, to simplify either the free market solution or the Keynesian one. The two following quotations the first drawn from an interview with *The*

[11] This dichotomy led Prescott to depart from the Lucasian standpoint, which he had initially endorsed, by claiming that the RBC conceptual apparatus, if slightly changed, can account for great depressions – now without capital letters! Cf. De Vroey and Pensieroso (2006).

[12] The title of Blinder is not a typo. A few sentences after those I quote, Blinder writes: "macroeconomics is already in the midst of another revolution which amounts to a return to Keynesianism – but with a much more rigorous theoretical flavor" (Blinder 1988: 110).

Region, the second from a draft of his review of Tobin's Yrjö Jahnson lectures (Tobin 1982) – make the point:

Lucas: In economic policy, the frontier never changes. The issue is always mercantilism and government intervention vs. laissez faire and free market. (Lucas interviewed in *The Region* 1993: 3)

There are, it seems to me, two schools of macroeconomic (and perhaps all) social policies: one which keeps the power of government to injure in the front of its mind, and stresses policies which take the form of institutional constraints on government action, and another which focuses on the power of government to improve welfare, and seeks methods by which this power may be exercised more effectively. (Lucas. Various. Box 26, Directions of macroeconomics 1979 folder).

Admitting that there is inevitably an ideological dimension to economic discussions is one thing, liking it is another, and Lucas surely did not.[13] Thus, since the ideological dimension could not be dispensed with, it had to be tamed.

Using the mathematical language, which allowed keeping hermeneutic and ideological discussions at bay, was according to Lucas a first way of doing so.[14] A second way was to draw a sharp separation between those propositions that pertain to the fictitious model economy and those pertaining to the real world. This is supposedly true for the premises of a model, for example, rational expectations, but also for its policy conclusions. Making the methodological standards ruling model construction explicit was a third barrier. Under these conditions, Lucas argued, fruitful conversations can take place even if they will be unable to settle ideological disputes. The following two quotations, the first drawn from a draft fragment, the second from Lucas's correspondence, make the point:

The classical issue of the proper role of government in a democratic society, of 'laws versus men' or 'rules versus authority,' are not going to be settled by technical advances in economics. It follows that no one's position on such basic questions needs to be threatened by such new technologies as may come to be at our disposal [Lucas had in mind here rational expectations]. (Lucas. Various. Box 26, Directions of macroeconomics 1979 folder).

[13] In a deleted passage from the introduction of his *Models of the Business Cycle* book, Lucas wrote: "I think it is fair to say that ideology plays an important role in contemporary discussion. There is probably no point in getting angry about this, but I do." (Lucas. Various. Box 13, Models of business cycles 1985-87 folder).

[14] Lucas, quite bluntly, considers that economists such as Coase and Hayek, who broadly speaking share his vision of the economy, are more engaged in ideological rather than theoretical activities. "What I want from economics is a set of principles I can use to evaluate proposed government interventions, case by case, on their individual merits. I agree that explicit modeling can give a spurious sense of omniscience that one has to guard against, ... *But if we give up explicit modeling, what have we got left except ideology?* I don't think either Hayek or Coase have faced up to this question" (Lucas. Various. Box 8. Letter to K. Matsuyama, March 29, 1995. Correspondence 1995 folder; my emphasis).

Really, there is never going to be such a thing as an uncontroversial way to settle disputes over economic policy, nor do I see why one would hope for such a state of affairs. It seems to that our job is to try to make controversy *useful*, by focusing on discussable, analyzeable issues. In Taylor's first paper, for example, contract length was selected arbitrarily (hence 'controversially') and was central to the operating characteristics of the model. But labor contracts are something we can [?] independent evidence on (as John did) or theorize about (as many people are not doing). Work like this is productive not because it settles policy issues in a way that honest people can't disagree over but because it channels controversy onto potentially productive tracks, because it gets us talking and thinking about issues that our equipment may let us make some progress on. (Lucas. Various. Box 5. Letter to Sims, July 15, 1982. Correspondence 1983 folder).[15]

These different elements point to the conclusion that the criticism I address to Friedman of mixing ideology and theory must not be extended to Lucas. An additional factor in favor of this judgment is the observation made earlier about Lucas's awareness that the policy conclusions of models are embedded in their premises and must not be peddled to policymakers. This is a statement that Friedman would not have uttered.

CONCLUDING REMARKS

Let me begin with a general impression about the present-day perception of Lucas's role in the history of macroeconomics. As mentioned in the Preface, last year I taught a graduate course on the subject matter of this book to third- or fourth-year doctoral students. It made me realize how unfamiliar they were with Lucas's work. They knew his name, they were aware that he had played a pioneering role in macroeconomics, but that was it. I see this as a testimony to how fast one may become *passé*. Leaving nostalgia aside, I find that a loss in knowledge ensues, which I regret. Of course, this is not the students' fault, but the result of how graduate programs are conceived with too much focus on technique. For their part, economists working at the cutting edge of the profession tread in Lucas's footsteps, but feel no need to refer to his views. They just look forward. The result is that most of what is written about Lucas comes from opponents to his views. On the one hand, there are those who dislike Lucas's work for ideological reasons. They take it for granted that it is part of the weaponry of 'ultra-liberalism,' which they regard as a sufficient condition for dismissing it without delving into its intricacies. On the other hand, there is also a dissension within the macroeconomics profession. It comes from defenders of the neoclassical synthesis viewpoint (meaning a defense of a pluralistic macroeconomics field). The objection here is against the Walrasian hegemony that Lucas wants to impose on the field. My surmise is thus that many of the critical positions taken against Lucas within the macroeconomics profession

[15] The work by Taylor to which Lucas refers is staggering contract modeling; it will be studied in Chapter 13.

have to do with the Marshall-Walras divide. I would not be surprised if a high correlation between a disinclination for Walrasian theory and one for Lucas's approach existed.

My own viewpoint about this divide is relativistic as behooves a historian of economics. Where I depart from the critics of Lucas is that I do not share their view that following the Walrasian paradigm is per se a flaw, while monetarists and Keynesians see eye to eye in this respect. Therefore, I have no objections of principle against Lucas's methodological canons, which have a Walrasian affiliation (except for what concerns empirical verification). What I want to insist on is the need to acknowledge the limitations of adopting the Walrasian approach. This is often not the case, Lucas himself being an exception here.

My general standpoint is that programs must be judged on their results. More generally, what I find important when assessing theoretical lines is less their value at the time of their rise than their posterity, that is, whether they have led to cumulative developments – in terms of Leijonhufvud's metaphor, whether a frail branch developed into a sturdy one.[16] Sargent put this nicely in an interview with Klamer:

> When we do research, the idea is that you don't produce a finished produce. You produce an input. You write the paper with the hope that it will be superseded. It's not a success unless it's superseded. Research is a living process involving other people. (Klamer 1984: 74)

The subsequent chapters will amply attest that this has been the case for the DSGE program.

I have also shown that Lucas, in spite of his strong desire for consistency, has been unable to avoid ambiguity. This is of course a flaw, but it is an understandable one – after all economics is a social science and the quest for epistemological purity may be fruitless if pushed too far. Lucas's main ambiguity was his twofold allegiance to neo-Walrasian theory and to empirical verification. Did this amount to wanting to have the best of both worlds, or was it a bold move that would later prove to have been productive? As Lucas himself did not engage in implementing his empirical injunction, again the answer to this question must await my examination of the works of those who set this task out for themselves. Still, I may already ponder on his view of models as analogous systems. I surmise that thereby Lucas was hoping to break the deadlock of the non-exploitation principle. If analogous models can be proven to be empirically robust, they can be used to compare the outcomes of alternative policy measures. As a result, policy recommendations would receive some 'scientific'

[16] Compare Hayek's papers on knowledge and its role in the functioning of market economies (Hayek 1948) with Walras's *Elements of Pure Economics*. The immediate value of the former to understand the economy is incomparably superior to that of Walras's *Elements*. Yet, as far as cumulative development is concerned, at the risk of upsetting my Austrian economist friends, I find that the latter has fared much better than the former.

vindication allowing to soften the non-exploitation principle. It remains that, to me, this ambition to straddle external and internal consistency is utopian. The replication discipline is surely useful, yet not to the point of lifting the non-exploitation requirement. As for the other ambiguities, let me just say that I find Sargent's remark about Lucas's 1972 paper, stating that it was almost an accident with regard to his perennial monetarist vision, enlightening. I like what it suggests, namely that models have a life of their own that may evolve independently from the motivation and vision of those who created them.