

Lucas on the Lucasian Transformation of Macroeconomics: an Assessment

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LUCAS ON THE LUCASIAN TRANSFORMATION OF MACROECONOMICS: AN ASSESSMENT

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Abstract

Robert Lucas is rightfully credited with having changed the course of macroeconomic theory. The aim of this paper is to document his transformation from a potential contributor to Keynesian macroeconomics to the master builder of an alternative paradigm, equilibrium macroeconomics. I reconstruct Lucas's theoretical journey as involving seven steps: (1) his pre-macroeconomic years, (2) his early work as a macroeconomist, jointly with Rapping, (3) the 'Expectations and the Neutrality of Money' 1972 article, (4) his inaugural equilibrium model of the business cycle, (5) his all-out attack on Keynesian macroeconomics, (6) the passing of the baton to Kydland and Prescott, and (7) his standpoint after the victory of the approach he so much contributed to launch.

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INTRODUCTION

In the late 1970s and the beginning of the 1980s, macroeconomics underwent a radical change, which resulted in the overthrow of Keynesian IS-LM macroeconomics and its replacement by dynamic stochastic general-equilibrium macroeconomics. While such transformations are always collective enterprises, it is nonetheless widely accepted that one person, Robert Lucas, played a pivotal role in the change.

The aim of my paper is to ponder upon this revolution, if it may be called so.¹ Its originality with respect to most other works on the subject (Blanchard, 2000; Laidler, 2010; Leijonhufvud, 2006; Hoover, 1988; 2003; Snowdon and Vane, 2005; Woodford, 1999) is that it adopts an indirect way of looking at the transformation, since my purpose is to assess Lucas's own account of what caused him to change "from an attempted contributor to Keynesian macroeconomics to that of severe critic..." (Lucas, 1981a, p. 2). To this end, I base my argument on the numerous articles in which Lucas explained his intellectual journey: his introduction to the *Studies in Business Cycle Theory* volume (Lucas, 1981a) and the various methodological papers contained in that volume; his Professional Memoir (Lucas, 2001); his 'My Keynesian Years' (2004) lecture; and the numerous interviews where he commented on his own work (Klamer, 1984; *The Region*, 1993; Snowdon and Vane, 1998; Usubiaga Ibanez, 1999; McCallum, 1999).² Another important source of information is the Lucas archives held at Duke University Rare Book, Manuscript, and Special Collections Library (Lucas, various).

The paper comprises seven parts, each concerned with one stage or facet of Lucas's intellectual journey: (1) his pre-macroeconomist years; (2) the beginnings of Lucas's interest in macroeconomics (the 1969 Lucas-Rapping paper); (3) rational expectations and general equilibrium (the 1972 'Expectations and the Neutrality of Money' article); (4) his inaugural equilibrium model of the business cycle; (5) his all-out attack on Keynesian macroeconomics; (6) the passing of the baton to Kydland and Prescott; (7) and Lucas's standpoint after the victory.

The purpose of this paper is to document Lucas's intellectual transformation, rather than to critically assess his contribution. When presenting this paper in seminars, I have observed how divisive a figure Lucas remains. Some of the audience, those who leant towards Keynes, recoiled whenever I said anything positive about Lucas and rejoiced in any statements they saw as negative, the opposite being true for those who tended to side with Lucas. For my part, I hope that the members of neither camp will end up considering me as one of them, when

¹ Lucas rejects the description of the change as a scientific revolution. For this reason, I have adopted the more neutral term of 'transformation' in the title of this article.

² Fischer (1996) gives a balanced assessment of Lucas's contribution to macroeconomics.

they read this paper. In my opinion, a good historian of economics should remain above the fray.

1. Lucas's pre-macroeconomist years

Having majored in history at the University of Chicago, Lucas started his graduate studies in history at Berkeley. In his Professional Memoir, he recounts that, while writing an essay on 19th century British business cycles for a course in economic history, he came to realize that he needed some economic background (which he totally lacked) to understand the subject fully. This led him to abandon history and shift to economics, not at Berkeley, “where the economic department was not encouraging”, but back in Chicago (2001, p. 6). To make up for his lack of economic education, he taught himself by reading Samuelson's *Foundations of Economic Analysis*. It fascinated him. Love at first sight!

I loved the *Foundations*. Like so many others in my cohort, I internalized its view that if I couldn't formulate a problem in economic theory mathematically, I didn't know what I was doing. I came to the position that mathematical analysis is not one of the many ways of doing economic theory: it is the only way. Economic theory is mathematical analysis. Everything else is just pictures and talk (Lucas, 2001, p. 9).

This put Lucas on the track on which he would remain for the rest of his life, but it also led him away from the Chicago tradition because, in Lucas's terms, Marshall was “the god of Chicago economics” (2001, p. 8). In contrast, Lucas's fascination with mathematical theory *à la* Samuelson tilted him towards Walras, an economist who was not popular in Chicago, castigated as he was by Friedman, the major figure in Chicago economics at the time. Lucas, frequently expressed his admiration for Friedman as an economist and a teacher, as well as a historian of monetary theory. Friedman and Lucas also shared the same vision of economic policy and the role of the state. But in their appreciation of Walras and Marshall, they were poles apart.³

In a letter to Driscoll, dated November 23, 1977, Lucas wrote “I am a hopeless ‘neo-Walrasian’ ” (Lucas (various) Box 30, Correspondence 1977-79 folder). Nonetheless during his early years as an economist, although having a Walrasian predisposition, he did not work in this field. Lucas's dissertation was applied work, an estimation of the degree of capital-labor substitution in U.S. manufacturing. After getting a position at the Business School of

³ The following extract from an interview with Snowdon and Vane is one testimony amongst others: “Question [to Lucas]: *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?* Answer: I like mathematics and general equilibrium theory. Friedman didn't.... Question: *His methodological approach seems more in keeping with Keynes and Marshall.* Answer: He describes himself as Marshallian, although I don't know quite what it means. Whatever it is, it's not what I think of myself (Snowdon and Vane, 1998, p. 132).

Carnegie Institute of Technology in 1963, he worked on the modeling of firms and industry dynamics (a partial-equilibrium topic), following Jorgenson's approach to investment decisions.

In his memoir, Lucas emphasizes that, when he was studying at the University of Chicago (as well as in the beginning of his time at Carnegie), he invested hardly any time in studying macroeconomics.⁴ However, Lucas insists that he had no antagonism to Keynesian economics:

Everyone from Chicago is a Friedman student in some very basic sense, but in terms of macro, I claim that the credentials I'm describing are true-blue Keynesian. When I was done with my graduate education, how did I think of Keynesian economics? I didn't think about it very deeply, to tell you the truth. It wasn't my field. I didn't picture myself as doing research in the area. But I certainly thought of myself as a Keynesian (2004, p. 19).

This declaration should not be understood as meaning that Lucas was a 'fundamentalist Keynesian': "So, when I think of Keynesian economics or at least the Keynesian economics I signed on for, it was part of this econometric model-building tradition" (2004, p. 22).

2. Lucas's initial macroeconomics work

The turning point in Lucas's research interests came when many discussions with Leonard Rapping led them to write a joint paper that became the path breaking 1969, "Real Wages, Employment, and Inflation", *Journal of Political* article. "We got there [to macroeconomics] by the back door, through labor economics" (2001, p. 17).

Triggered by the intense discussions about the Phillips relation that were taking place in the 1960s, their paper studied the wage-employment sector of the broader IS-LM model. Lucas and Rapping wanted to reconcile short-period and long-period observations about the labor supply, which implied the adoption of a dynamic framework. They broke with traditional macroeconomics by adopting a microfoundations perspective, as well as by emphasizing the role of the supply of labor in explaining variations in employment. They depicted labor suppliers as rational optimizing agents engaged in inter-temporal substitution — that is, working more when current wages were high relative to expected wages. The celebrated Lucas supply function ensued. The notion of inter-temporal substitution had gained pride of place in general equilibrium theory and in capital theory, but Lucas and Rapping were the first to apply it to labor supply. Subsequent developments have shown that this had far-reaching consequences.

⁴ He took only the two required macroeconomics courses, taught by Christ and Baily, which centered on IS-LM modeling.

Another original feature of their model was the adoption of the market-clearing assumption. In their paper, Lucas and Rapping trivialized its adoption by noting that some Keynesian models, such as Modigliani's model, were also based on it. They ignored the fact that, for better or worse, this was certainly not the way in which Modigliani interpreted his model. Market clearing meant that no room was left for the notion of involuntary unemployment. This generated a big stir.

Despite being a perfectly standard formulation of labor supply, the model was controversial. The prevailing Keynesian approach had been to assume labor supply was passive, and that movements in the demand for labor determined changes in employment. ... It is the implication that changes in employment may be largely voluntary that made the paper controversial (Fischer 1996, p. 14-15).

Later on, Lucas was to consider market clearing as a compelling part of the equilibrium discipline. However, in the 1969 article, the notion was presented as just one way of proceeding:

Patinkin is correct in asserting that that it is not *necessary* to construct models in which labor markets are continuously cleared, but, as his discussion of the Keynesian literature makes clear, the continuous-equilibrium view is in no sense a radical departure from the views of earlier theorists, not does it have, in itself, any obvious normative consequence (Lucas and Rapping, 1969; reprinted in Lucas, 1981a, p. 48, note 3).

Apart from these two points, the paper was conventional. It was based on adaptive expectations, and adopted the Marshallian strategy of studying one sector of the economy in isolation from the others. Lucas claims that there was no doubt in his and Rappings' minds that their specific model could be combined with other models concerned with other sectors of the economy: "We viewed ourselves as constructing a model of the 'wage-price sector', potentially suitable for combining with other models of other 'sectors' to provide a model of the entire economy" (1981a, p. 6). In the same vein, in Lucas's retrospective papers, we recurrently find the claim that their aim at the time was to enrich the foundations of Keynesian theory rather than to challenge them.⁵

In spite of these outwardly inclusive intentions, with their 1969 paper Lucas and Rapping (actually only Lucas, because Rapping soon lost interest in the subject) were set on a collision course with Keynesian macroeconomics.

We thought of ourselves as engaged in a collective project which engaged efforts of many during 50's and 60's — providing 'microeconomic foundations' for Keynesian macroeconomic models. Many viewed 'wage-price' sector as last frontier in this effort. ... Turned out that this work, in conjunction with similar efforts of others, was deeply

⁵ See Lucas (1981a, p. 3) and Lucas (2004, p. 20).

subversive of Keynesian macroeconomics. [We] were led to complete rejection of this line and its policy implications, obliged to search for quite different ways of thinking about business cycle. Surprisingly, search led back to old-fashioned pre-Keynesian theories — but without rejection of modern analytical methods (extract from a talk given by Lucas at Princeton in 1979, Lucas Archives Box 22).

3. Rational expectations and general equilibrium

By the end of the 60s, I was leading two lives as an economist. With Rapping, I was an empirical macroeconomist, estimating Phillips curves and aggregate labor supply functions. Working with Prescott, I had immersed myself in the mathematics of dynamic programming and general equilibrium theory and was applying these methods to construct tractable, genuinely dynamic models (Lucas 2001, p. 20).

In his 1972 (1981a) ‘Expectations and the Neutrality of Money’ article, the end-result of the most intensive and productive years of his career, Lucas proved able to bring these two separate lines of research together.⁶ This article is a development of the Lucas-Rapping 1969 piece, retaining the idea of the equilibrium discipline (a term that had not been coined at the time) as well as the need to think inter-temporally, and hence use the notion of inter-temporal substitution as a cornerstone on which to build. However, it introduces three new elements. Rational expectations, a general equilibrium perspective are the first two, taking a stance in a policy debate, the endorsement of Friedman’s claim about the inefficiency of monetary policy, is the third.

Rational expectations

Having Muth as a colleague at Carnegie, Lucas became acquainted with the rational-expectations assumption at an early date, but, initially, he saw no way of exploiting it. In his Professional memoirs (2001, p. 13), he explains that the need to use this assumption dawned on him when, working on a paper on adjustment costs and the theory of supply, he came to realize that inconsistencies kept cropping up whenever myopic expectations, i.e. extrapolating the current price into the future, were assumed. This assumption resulted in firms missing profit opportunities which were visible to the model-builder. Such a discrepancy triggered Lucas’s realization that something was wrong: “If your theory reveals [unexploited] profit opportunities, you have the wrong theory” (Lucas 2001, p. 13). Lucas adopted rational

⁶ The speed with which Lucas changed his mind is striking. “In 1963, I had thought of a competitive industry in terms of firms solving short- and long-run, deterministic profit-maximization problems, under the (false) belief that current prices would maintain their current values forever, and with the passage from one to the other and all the effects of unpredictable shocks tacked on as afterthoughts. Five-years later, I thought of the same economics in terms of firms maximizing expect-discounted present value, with rational expectations about the probability distributions of future prices and with stochastic shocks and adjustment costs being fully integrated into theory” (Lucas 2001, p. 16).

expectations in a paper co-authored with Prescott, “Investment Under Uncertainty” (Lucas and Prescott 1971), and then introduced it into the 1972 (1981a) article.

A general equilibrium approach

Lucas credits Phelps with having drawn his attention to the need to adopt such a perspective when organizing a conference around what would become the celebrated ‘Phelps volume’ (Phelps, 1970).⁷ Phelps was also responsible for having spelled out the island metaphor which was to provide Lucas with the signal-extraction idea in an imperfect-information context. Earlier on, Lucas had explored the technicalities of general equilibrium, participating in a workshop conducted by Uzawa and other activities. This was also the time at which he started “working furiously to pick up the mathematics of optimization over time — calculus of variations, the maximum principle of Pontryagin, Bellman’s dynamic programming — and of the differential equations systems these optimization problems produced” (Lucas 2001, p. 14). While working with Prescott, who had been a graduate student at Carnegie when Lucas was an assistant professor there, the idea dawned on them that the Arrow-Debreu model had a previously unrecognized potential for applied work. “Prescott and I saw that it would be helpful to us, and plunged into a self-taught crash course. Putting the pieces together, we wrote ‘Investment under Uncertainty’ ” (Lucas 2001, p. 16).

An endorsement of Friedman and Phelps

Around the time that Lucas and Rapping’s paper was published there were two important developments in macroeconomics: Friedman’s Presidential Address introducing the expectations-augmented Phillips curve (Friedman, 1968), and Phelps’s (1968) *Journal of Political Economy* article. These two papers introduced the notion of a natural rate of unemployment, arguing that there was no long-run tradeoff between output and inflation. Although his address barely departed from the Keynesian framework, Friedman was able to augment the Phillips curve in such a way as to reverse its Keynesian conclusion that monetary policy could improve employment in a durable way into the opposite conclusion that it was ineffective in this respect, Quite a feat! As a result, two opposing interpretations and hence two ‘camps’ were present, one group of economists defending the stable Phillips curve allowing for a trade-off between unemployment and inflation, the other arguing for the natural rate of unemployment hypothesis. This led Lucas to abandon his earlier project of enriching the IS-LM model, and to embark instead on the alternate aim of giving stronger foundations

⁷ “My most influential paper on ‘Expectations and the neutrality of money’ came out of a conference that Phelps organized where Rapping and I were invited to talk about our Phillips curve work. Phelps convinced us that we needed some kind of general equilibrium setting. Rapping and I were just focusing on labor supply decisions. Phelps kept on insisting that these labor suppliers are situated in some economy, and that you have to consider what the whole general equilibrium looks like, not just what the labor supply decision looks like” (Snowdon and Vane 1998, p. 126).

to Friedman's insights. To him, the Friedman paper was, at one and the same time, worth rallying to and in need of improvement, another task attempted in his 1972 paper.

4. An equilibrium model of the business cycle

The next step in Lucas's intellectual development was the extension of the 1972 model into an explicit equilibrium analysis of the business cycle (Lucas [1975] 1981a). This involved less innovation because the 1972 expectations model was already about an economy that "undergoes what is in some sense a business cycle" (Lucas 1981a, p. 8). To Lucas, the possibility of such an easy (conceptually but not technically) extension of the earlier model was a testimony to its potentialities. The model claims that real output fluctuations are caused by unanticipated monetary-fiscal shocks, the condition for this to happen being that production and trade take place in markets that are physically separated and that agents' information is imperfect, the framework that Lucas had adopted in his "Expectations..." paper.

This monetary-shock driven equilibrium model of the business cycle was soon criticized, Lucas himself gradually admitting to its shortcomings.⁸ The important point was that the project of constructing an equilibrium model was on track, and, at a deeper level, that business fluctuations, rather than unemployment, should be the defining object of macroeconomics.⁹ To Lucas, this meant a return to a line of enquiry that Hayek had proposed, but which had been left unexplored as the result of the victory of the Keynesian program of studying involuntary unemployment at one point in time (Lucas [1977] 1981a, p. 215).

5. Lucas's all-out attack on Keynesian macroeconomics

Parallel to devising a new way of doing macroeconomic analysis, Lucas became highly critical of the traditional Keynesian approach. This led him to undertake an all-out offensive on Keynesian economics, incomparably harsher than Friedman's. It evolved on two fronts, substance and polemics. I will present three substantive arguments from this attack, before turning to the polemical aspect.

⁸ "That '75 paper was a dead end. I mean, it was attempt to introduce some kind of usual dynamics into my '72 paper, and it didn't work. I think that Kydland and Prescott's 1982 paper followed from that" (Lucas's interview with Usabiaga 1999, p. 181)).

⁹ "Our intention is ... to extend the equilibrium methods which have been applied in many economic problems to cover a phenomenon which has so far resisted their application: the business cycle" Lucas and Sargent ([1979] 1994, p. 28).

Lucas's dismissal of Keynes's General Theory

Keynes's *General Theory* was hardly Lucas's cup of tea.¹⁰ More substantively, Lucas's indictment was that Keynes departed from the equilibrium discipline by proposing his ill-conceived notion of involuntary unemployment (Lucas and Sargent, [1978] 1994, p. 15; Lucas, [1977] 1981a, p. 220; Lucas [1978] 1981a, p. 242-243). Keynes's lapse from the equilibrium discipline, Lucas was ready to admit, was 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 was 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, 1981a, p. 4).

The 'Lucas critique' ([1976a] 1981a)

Lucas's target in his 'Econometric Policy Evaluation: A Critique' article was the macroeconomic models of the time, all of which had a Keynesian inspiration. Although admitting that they did a fairly good job of forecasting, these models, Lucas claimed, were a failure as far as the assessment of alternative policies was concerned. Their main flaw was their lack of microfoundations. Being based on reduced forms, these models missed the fact that agents would change their decisions when faced with a change in the institutional regime. In other words, they just recorded the decision rules used by agents, failing to go deeper and explore the agents' objective functions. As a result, a model of the economy estimated at a period during which a particular institutional regime held sway could not provide adequate information for assessing behavior under a different regime.¹¹

The stagflation experiment

At the end of the 1960s, two distinct theories about the relation between the price level and employment (and at a deeper level about the efficiency of monetary policy) coexisted: the stable Phillips curve trade-off model and Friedman's natural-rate-of-unemployment model. Lucas's claim was that the 1970s phase of stagnation provided a quasi-laboratory experiment for their validity, and the Friedman model won the contest hands down.

¹⁰ Here is how he put it when interviewed by Klamer: "I find it [The *General Theory*] carelessly written, not especially graciously written, sometimes dishonestly written. I don't like the bullying tone. I don't like the sort of British aristocratic stuff" (Klamer, 1984, p. 50).

¹¹ Lucas has been keen to insist that this critique follows from the argument he developed in the 'Expectations' article. "If the theory of 'Expectations and the Neutrality of Money' was the correct way to formulate the Friedman-Phelps natural-rate hypothesis, then it was evident that the econometric methods then being applied to test this hypothesis were entirely missing the point" (Lucas, 1981a, p. 9). "In that model economy [of 'Expectations and the Neutrality of Money'], it is evident that changes in the rules governing monetary policy alter coefficients in what one would ordinarily have thought of as structural equations in the econometric sense, regardless of the stability of these coefficients over a past sample period. Once the reasons for this are understood, it is not difficult to see that the same parameter instability must arise in the actual economy" (Lucas, 1981a, p. 11).

Keynesian theory *is* in deep trouble, the deepest kind of trouble in which an applied body of theory can find itself: It appears to be giving serious wrong answers to the most basic questions of macroeconomic policy. Proponents of a class of models which promised 3½ to 4½ percent unemployment to a society willing to tolerate annual inflation rates of 4 to 5 percent have some explaining to do after a decade such as we have just come through. A forecast error of this magnitude and central importance to policy has consequences, as well it should. (Lucas, 1981b, p. 559-560).

We got the high-inflation decade, and with it as clear-cut an experimental discrimination as macroeconomics is very likely to see, and Friedman and Phelps were right. (Lucas, 1981b, p. 560).¹²

The polemical dimension

Unlike Friedman, whose criticisms of Keynesian theory were usually embedded in flattering wording, Lucas did not pull his punches. Two examples are worth giving. The first is Lucas and Sargent's joint contribution to a Conference organized by the Federal Reserve Bank of Boston in June 1978 (Lucas and Sargent [1978] 1994). Entitled "After Keynesian Macroeconomics", it was a strongly argued paper cutting deep into the Keynesian paradigm. But its rhetoric was virulent. The paper drew strong criticism from Benjamin Friedman, the discussant (Friedman 1978). Never short of mischievous comparisons, Solow, who had been asked to give the concluding remarks, found Lucas and Sargent's "polemical vocabulary reminiscent of Spiro Agnew" (Solow 1978, p. 203), finding the following phrases offensive: "wildly incorrect", "fundamentally flawed", "wreckage", "failure", "fatal", "of no value", "dire implications", "failure on a grand scale", "spectacular recent failure", "no hope" (Solow, 1978, p. 204).¹³

My second example is a talk given by Lucas in 1979 at the Graduate School of Chicago's Annual Management Conference. Entitled "The Death of Keynesian Economics", it was published in the alumni magazine, *Issues and Ideas*, in 1980. It starts as follows:

The main development I want to discuss has already occurred: Keynesian economics is dead — maybe 'disappeared' is a better term. I do not exactly know when this happened but it is true today and it was not true two years ago. This is a sociological not an economic observation, so evidence for it is sociological. For example, one cannot find a

¹² See also Lucas ([1977] 1981a, p. 221).

¹³ Lucas and Sargent reacted strongly to Friedman's comments by declaring that he focused on minor points and their 'rhetorical profile' instead of rebutting their main arguments. "The 'rhetorical profile' adopted in our paper was not chosen independently of the arguments developed using more precise and technical language in the text, and more fully developed by each of us in earlier writings. ... If this research is flawed in some essential way, it is difficult to see how softening our rhetoric will help matters. If the implications we have drawn are close to the mark, how can 'the cause of scientific interchange' be best served by summarizing them in a way which averages what we believe to be true with what others find pleasant or familiar?" (Lucas and Sargent, 1979, p. 82).

good, under-40 economist who identifies himself, works as ‘Keynesian’. Indeed, people even take offence if referred to in this way. At research seminars, people do not take Keynesian theorizing seriously any more — audience starts to whisper, giggles to one another. Leading journals are not getting Keynesian papers submitted any more. I suppose I, and with many others, were in on the kill in an intellectual sense, but I do not say this as any kind of boast, even with much pleasure. Just a fact. True, there are still leading Keynesians — in academics and government circles — so Keynesian economics is alive in this sense — but this is transient, because there is no fresh source of supply. Only way to produce a sixty year old Keynesian is to produce a thirty year old Keynesian, and wait thirty years. So implications for policy will take a while to be evident — but they can be very accurately predicted.

This blunt passage has been posted on the internet on several occasions in the last years as a charge against Lucas. However, getting hold of the full paper proved to be difficult. My requests to the people who had posted the passage for a copy of it were always met with the answer that they had none! Lucas gave me a similar answer. So the passage, cut loose from the rest of the paper, achieved a life on its own.¹⁴ Luckily, I was able to find a draft version of the paper in the Lucas Archives at Duke (various, Box 22).¹⁵ When reading it in full, a rather different picture emerges. In particular, it turns out that its real subject matter is the demise of the neoclassical synthesis.

The idea of a neoclassical synthesis, introduced by Samuelson in 1955 in the third edition of his *Economics* textbook, covers a multi-faceted reality. It has a sociological dimension in its description of a *modus vivendi* between the two communities of micro- and macroeconomists. It also refers to a theoretical program, already introduced in Samuelson’s *Foundations* (1947), aimed at bringing together Walrasian and Keynesian theory, the domain of long-period equilibrium being assigned to the former, and that of short-period disequilibrium to the latter. In practice, this program had not been achieved, but this failure was hardly recognized.

Lucas’s purpose in his lecture was to ponder upon the ideological function played by the neoclassical synthesis. He viewed it as having arisen as a by-product of the Great Depression, which had led to a widespread loss of faith in market forces, and its replacement by the belief that the economy needed to be managed on a year-to-year basis. Lucas’s story is so vivid that it is best to quote him directly:

The central message of Keynes was that there existed a middle ground between the extremes of socialism and laissez faire capitalism. ... True that economy cannot be left to its own device, but *all* that we need to do to manage it is to manipulate the general

¹⁴ Mankiw quotes it in his paper, “The Macroeconomists as a Scientist and an Engineer” (2006, p. 34). In Google Scholar, it is mentioned as being quoted thirty-five times, but the paper itself is listed as unavailable.

¹⁵ The above excerpt is drawn from this draft version. I have slightly amended it by ingsuppress abbreviations.

level of fiscal and monetary policy. If this is done right, all that elegant 19th century economics will be valid and individual markets can be left to take care of themselves. In effect, Samuelson told his colleagues: “Face it — you live in a world where virtually nobody has any faith in this laissez faire religion of yours. I am offering a substitute ideology which concedes the inability of a competitive economy to take care of itself, but which also offers a management system which is, say, 95% consistent with laissez faire”. These were hard times, and this was too good a deal to pass up. We took it. So did society as a whole. What I meant by saying that Keynesian economics is dead at the beginning of my talk is just that this middle ground is dead. Not because people do not *like* the middle ground anymore but because its intellectual rationale has eroded to the point where it is no longer serviceable (Lucas Archives Box 22, Lucas’s emphasis).

By his own admission, Lucas played a crucial role in this erosion since his work showed that the new synthesis program was unachievable from the intellectual point of view.¹⁶ To Lucas, this conclusion was good news, good riddance to a mistaken line of research. Intellectually, nothing was lost since the dismissal of the neoclassical synthesis was accompanied by the realization that classical theory alone could tackle that part of the *explanandum* that had previously been assigned to Keynesian theory. Nonetheless, he reckoned that this collapse led to a feeling of disarray, a painful loss of consensus among macroeconomists.

Had Lucas entitled his lecture ‘The fall of the neoclassical synthesis’ instead of ‘The death of Keynesian economics’ (and had he deleted its first paragraph) not only would it have caused less outrage, but the title would have been more accurate. In effect, the ‘Keynesian’ modifier (and the term ‘Keynesianism’) can have two meanings. First, they can designate a specific vision of the ideal organization of society, i.e. a limited belief in the supremacy of market forces, the effect of which is to assign governments the auxiliary role of engaging in stabilization policies. Keynes pinpointed this as a “moderately conservative’ viewpoint (Keynes 1936, p.377). Second, they can indicate the Keynesian conceptual apparatus, which to all intents and purposes can be identified with the IS-LM model and associated with the neoclassical synthesis. This is Keynesianism as a theory, and, for that matter, Keynesian macroeconomics. Whenever these meanings are not separated, confusion occurs.

In the light of this distinction, it turns out that that Lucas’s judgment in his lecture was basically correct for Keynesianism as a conceptual apparatus, but wide off the mark for Keynesianism as a vision of the market system. Oddly enough, Lucas had made this

¹⁶ Lucas, himself, claimed that his theory was not responsible for the fall of the neoclassical synthesis but was rather a reaction to it. “[Tobin’s negative reaction] reflects the view that ‘new classical’ models are a *cause* of the decay in confidence in the neoclassical synthesis, as opposed to a *response* to what I see as the failure of Keynesian models to deal with events in the 1970s” (Lucas, 1981b, pp. 562-3).

distinction a few years earlier in his review of McCracken et al.'s "Towards Full Employment and Price Stability" OECD report (Lucas [1976b] 1981a).¹⁷

To sum up, the 'Death of Keynesianism' lecture is akin to the 'After Keynesian macroeconomics' paper, although it operates at a different level of technicality. Both make a well-argued substantive claim, but wrap it up in such provocative wording that it inevitably caused vehement outrage.

6. The passing of the baton to Kydland and Prescott

As hinted above, Lucas gradually admitted that his own equilibrium business-cycle model did not hold water. Here is how he put it in his Professional Memoir:

Though I did not see it at the time, the Bald Peak conference [organized by the Federal Reserve Bank of Boston in 1978] also marked the beginning of the end for my attempts to account for the business cycle in terms of monetary shocks. At that conference, Ed Prescott presented a model of his and Finn Kydland's that was a kind mixture of Brock and Mirman's model of growth subject to stochastic technology shocks and my model of monetary shocks. When Ed presented his results, everyone could see they were important but the paper was so novel and complicated that no one could see exactly what they were. Later on, as they gained more experience through numerical simulations of their Bad Peak model, Kydland and Prescott found that the monetary shocks were just not pulling their weight: by removing all monetary aspects of the theory they obtained a far simpler and more comprehensible structure that fit postwar U.S. time series data just as well as the original version (Lucas 2001, p. 28).

In effect, Kydland and Prescott's 1982 real-business-cycle model (Kydland and Prescott 1982) became the workhorse of macroeconomics in the late 1980s and 1990s. The relationship between Lucas, and Kydland and Prescott, is, I surmise, reminiscent of 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 inevitable way for theory to evolve. The same questions 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 certain. Lucas's conceptual papers were impressive but too abstract to generate a huge following. As to Lucas's

¹⁷ "It seems certain that Keynes's thought will continue to stimulate economic theorists in various and unpredictable ways for the foreseeable future, so much that many economists will think of themselves as 'Keynesians'. In advance of seeing these developments, one cannot presume to pronounce them failed. I am here using the term 'Keynesian' much more narrowly, to refer to the multiplier calculation, which all of us understood Heller to be discussing and applying, together with the underlying, if less precisely specified, theory which provided guidance as to the range of circumstances under which these calculations might be expected to yield accurate answers" (Lucas [1976b] 1981a, pp. 265-66).

criticisms, their impact on the profession could have been limited to making modelers more cautious about drawing practical conclusions from their models, without producing a radical change in method. To generate a scientific revolution, an alternative way of doing applied work, providing new grist to the mill for the majority of members of the community, must be made available. This was Kydland and Prescott's main contribution.

7. Stage seven: after the victory

In his Professional Memoir, when discussing the Bald Peak Colony Club Conference mentioned above, Lucas depicts himself as a rebel fighting the establishment (2001, p. 26). It did not take long before the positions were reversed. Frequent invitations to comment on the history of macroeconomics accompanied Lucas's new status, and he cannot be faulted for shirking this responsibility! By doing so, he was led to take the role of the commentator of the theoretical innovations he had impulsed.

Lucas's 'Methods and Problems in Business Cycle Theory' article

In his article entitled 'Methods and Problems in Business Cycle Theory', Lucas ([1980] 1981a) aimed to elucidate the factors explaining the rise of equilibrium business-cycle models. He considered three possible factors (technical developments, outside events and the internal development of the discipline), and concluded that the first of these played the dominant role. Progress in macroeconomics has mainly been a matter of discovering or applying new tools, new techniques for treating old issues.¹⁸ With respect to the transformation of macroeconomics, two technical innovations were particularly important: the use of new mathematical tools, borrowed from engineering; and the increased computational ability associated with the tremendous progress that had taken place in computer science, paving the way for large-scale simulation work. It is their absence that explains earlier theoretical stalemates. Taking Keynes's *Treatise on Money*, the aim of which was to construct a theory of the business cycle, as an example, Lucas notes that:

The difficulty is that Keynes has no apparatus for dealing with these problems. Though he discusses them verbally as well as his contemporaries, neither he nor anyone else was well enough equipped technically to move the discussion to a sharper or more productive level (Lucas [1980] 1981a, p. 275).

¹⁸ The same point is expressed more lyrically in 'My Keynesian Education': "I see the progressive development in economics as entirely technical: better mathematics, better mathematical formulation, better data, better data-processing methods, better statistical methods, better computational methods. I think of all progress in economic thinking, in the kind of basic core of economic theory, as developing entirely as learning how to do what Hume and Smith and Ricardo wanted to do, only better: more empirically founded, more powerful solution methods, and so on" (Lucas, 2004, p. 22). See also Lucas ([1980] 1981a, p. 275), Lucas (1987, p. 2), and Lucas (1996, p. 669).

As to new developments “thrown at us by the real world” (Lucas [1980] 1981a, p. 272), Lucas dislikes giving them too much importance because this would run counter to his premise that all business cycles are basically alike.¹⁹ He nonetheless considers two external influences that may have played a role in the rise of new classical macroeconomics. The first, already evoked above, is the stagflation period that characterized the 1970s. In this paper, however, unlike some of his others, Lucas stops short of claiming that this was the decisive factor in the theoretical transformation ([1980] 1981a, pp. 282-283). The second is the fact that in the post-World War II period, business cycles have followed a fairly regular pattern, giving weight to the view that they are a repeated occurrence of the ‘same’ event.

As far as internal developments are concerned, Lucas emphasizes the usefulness of connecting theoretical elements that at first look remote, and in particular of importing ideas from neo-Walrasian theory into macroeconomics. These two fields were previously isolated from each other. To illustrate this point, Lucas returns to the topic of the neoclassical synthesis, this time broaching it in its technical, rather than its ideological, aspect. The neoclassical synthesis rested on an old-style interpretation of Walrasian theory, where equilibrium is conceived of as a static, or state of rest, notion, acting as a center of gravity for disequilibrium states.²⁰ Such a conception of equilibrium, intertwining equilibrium and disequilibrium, has many disadvantages (for example, it cannot account for irreversible changes in the data), but the drawback to which Lucas seems to be most sensitive is the arbitrariness of the speed of adjustment. That is, it is up to the economist to decide on the speed, to the effect that he or she can give as much or as little importance to disequilibrium as he or she cares. To Lucas, this is a poor conception of dynamics. But macroeconomists of the time, he argues in this paper, should not be blamed for adopting it. They could not do better, first, because they lacked the technical tools and, second, because the usefulness of borrowing from neo-Walrasian theory, as distinct from the traditional interpretation of Walras, had not dawned on them. Taking these steps, as Lucas did, leads to the introduction of new equilibrium concepts to the effect that “the idea that an economic system in equilibrium is in any sense ‘at rest’ is simply an anachronism” (Lucas ([1980] 1981a p. 287). Once the new trail had been blazed, the old path ought to be abandoned. In Lucas’s superb metaphor:

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, p. 286).

¹⁹ Lucas 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] 1981a, p. 273).

²⁰ “I refer to this theory [the neoclassical synthesis] as static.... The underlying idea seems to be taken from physics, as referring to a system “at rest.” In economics, I suppose such a static equilibrium corresponds to a prediction as to how an economy would behave should external shocks remain fixed over a long period, so that households and firms would adjust to facing the same set of prices over and over again and attune their behavior accordingly” (Lucas, ([1980] 1981a, p. 278).

A different assessment of Keynes's contribution to economics?

It may be wondered whether Lucas's judgment of Keynes mellowed after he had joined him in the pantheon of great economists. The answer is nuanced. As far as the issue of Keynes's contribution to economic theory is concerned, Lucas has stuck to his view that Keynes was a poor technical economist. However, he has also insisted that, in the 1930s, Keynes played a crucial positive role in the ideological sphere by helping to ensure the survival of democratic capitalism, which was threatened at the time.

I think Keynes's actual influence as a technical economist is pretty close to zero, and it has been close to zero for 50 years. Keynes was not a very good technical economist. He didn't contribute much to the development of the field. (Interview with Usabiaga Ibanez, 1999, p. 180).

[Keynes's real contribution] is not Einstein-level theory, new paradigm, all this. ... I think that in writing the *General Theory*, Keynes was viewing himself as a spokesman for a discredited profession. ... He is writing in a situation where people are ready to throw in the towel on capitalism and liberal democracy and go with fascism or corporatism, protectionism, socialist planning. Keynes's first objective is to say, 'Look, there's got to be a way to respond to depressions that's consistent with capitalist democracy'. What he hits on is that the government should take some new responsibilities, but the responsibilities are for stabilizing overall spending flows. You don't have to plan the economy in detail to meet this objective. ... So, I think this was a great political achievement. ... [Keynes] was a political activist from beginning to end. What he was concerned about when he wrote the *General Theory* was convincing people that there was a way to deal with the Depression that was forceful and effective but didn't involve scrapping the capitalist system. Maybe we could have done without him but I'm glad we didn't have to try (2004, p. 24).²¹

Lucas's 'My Keynesian Education' lecture

In 2003, Lucas gave a lecture given at Duke University at a Conference on the IS-LM model. It made a strong impression on many in the audience. First Lucas admitted to the limits of the real-business-cycle type of modeling.²² These models, he stated, were a fine tool for understanding business fluctuations in periods of plain sailing, but were inappropriate for coming to grips with big depressions or financial crises. Second, as the last quotation above illustrates, Lucas eulogized Keynes for having contributed through his work to the survival of capitalism and liberal democracy in the face of the dual threat from fascism and communism. Moreover, Lucas admitted that Keynes had made the important

²¹ See also Lucas (1995, pp. 916-917).

²² Lucas had already made this point in his anniversary review of the Friedman-Schwartz book on the monetary history of the US (Lucas 1994).

policy claim that spending flows should be stabilized, a claim which, Lucas said he agreed with, adding that the real question was how to do it (2004, p. 24). Finally, Lucas declared that he had also had his Keynesian years, which he evoked fondly. He had already made this claim *in passim* in his Professional Memoir (2001), but now it was expressed in a more resonant way.

What exactly did Lucas have in mind when speaking of his Keynesian years? Did he refer to Keynesianism as a vision of the working of the market system or as a conceptual apparatus? As a vision of the working of the market, Lucas's reminiscences about his Keynesian years means that there was a time when he shared this vision. In his Professional Memoir, he makes it clear that political discussions abounded in his family and that a left-of-center viewpoint dominated. He may have retained these ideas when he entered graduate school, before shifting towards more free-market views, possibly under Friedman's influence and the general atmosphere in Chicago. As a conceptual apparatus, the 'Keynesian years' refers to the short time when Lucas worked with Rapping on the wage-employment sector of an IS-LM model. For my purposes, the two interpretations amount to the same thing. They both indicate that Lucas initially had nothing against Keynesian theory, either ideologically or theoretically.

If this is accepted, the lecture was more than a gratuitous reminiscence about past days. It served the purpose of making the point that Lucas's dissatisfaction with Keynesian theory, rather of being due to some *a priori* antagonism, happened despite his Keynesian inclination, as a result of his having worked with the theory and uncovered its flaws. Had he admitted that he had always been anti-Keynesian, his criticism could have been dismissed as purely based on prejudice.²³

Lucas's rejection of the scientific revolution interpretation

Another of Lucas's recurrent themes is his lack of sympathy for interpreting the unfolding of macroeconomics (or for that matter economics in general) in terms of the Kuhnian notion of a scientific revolution. To him, little, if anything, is gained by invoking such a framework.

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.

²³ We can, I believe, safely extend to Lucas the following observation that Sargent made about himself in an interview with Sent: "*But isn't the case that what you define as a problem depends on what your starting position is?* Absolutely. That's exactly why rational expectations stuff was developed by people within the Keynesian tradition. There were people trying to knock off and destroy the Keynesian tradition from the outside, who weren't sympathetic enough to it to learn it. And I mean it was the monetarist tradition or something like that. And the paradox is that, I would say what's ended up being perceived as the most destructive in Keynesian tradition is from its own children. You know? Because, if you look at what, say Lucas, Prescott, Wallace, and Barro were working on, those were all pieces of a Keynesian model, a high-tech Keynesian style" (Sent 1998, pp. 165-166).

There is no doubt something to be learned by tracing the main ideological currents in macroeconomic research, but I myself find 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).²⁴

It is true that the notion of a scientific revolution has been overused. Still, if there is one episode in the history of macroeconomics which seems to deserve the appellation, it is the transformation triggered by Lucas's work. It has all the trappings of a change in paradigm: a shift in the object of analysis, a shift from the alleged neglect of microfoundations to allotting them a central role, the replacement of Keynesian concepts with new ones, unheard of before (such as rational expectations and inter-temporal substitution), the introduction of a new dynamic perspective putting an end to the neoclassical-synthesis perspective, a change in the equilibrium notion, and a move away from a Marshallian and towards a Walrasian approach. From the sociological point of view, the picture also fits: thundering declarations of war, the displacement of the stars of the profession by new idols, the publication of new journals, etc.

None of this impresses Lucas. Nor, I suspect, would he be seduced by Axel Leijonhufvud's account of the development of macroeconomics using a decision-tree metaphor.²⁵ According to Leijonhufvud, progress along a branch of the tree may come to an end because more or less insuperable puzzles arise, or because objections leveled against it persuade researchers to abandon this approach. The Lucasian transformation is a case in point. Keynesian macroeconomics seemed to be alive and well. Lucas and a few other troublemakers came along, declaring that it was plagued with inconsistencies and methodological flaws. They engaged in a backtracking process, returning to an earlier node at which they felt the wrong turning had been taken. Another approach, which for one reason or another had looked impracticable at an earlier date, suddenly turns out to be appealing and worth exploring.²⁶

Two reasons may explain Lucas's reluctance to endorse the Kuhnian or Leijonhufvud explanations. The first is that, to him, the transformation is unequivocal progress. In both the Kuhnian and the Leijonhufvud models, progress is relative. The decision-tree approach

²⁴ See also Klamer (1984, p. 50).

²⁵ "It is useful to think of the history of our subject as forming a *decision tree*. Major economists force their contemporaries to face choices – the choice of what to ask, what to assume, what to regard as evidence and what methods and models to employ – and persuade the profession or some fraction of it to follow the choice they make. The path that any particular school has followed traces a sequence of such decisions. Many of the choices faced in such a sequence were not anticipated by the founder to which we trace the development in question but were created by subsequent contributors; some of the decisions made we may judge to have been wrong in hindsight" (Leijonhufvud 1994, p. 148).

²⁶ The following extract from the introduction to *Studies in Business Cycle Theory* shows that Lucas did indeed practice backtracking: "The apparent novelty of the model of 'Expectations and the neutrality of money' ... renewed my interest in the vast pre-Keynesian literature on business-cycle theory. There I found ... a sophisticated literature, however unaided by modern theoretical technology, emphasizing the recurrent character of business cycles, the necessity of viewing these recurrences as mistakes, and attempts to rationalize these mistakes as intelligent responses to movements in nominal 'signals' of movements in the underlying 'real' events we care about and want to react to" (Lucas, 1981a, p. 9).

incorporates the idea of progress only within the confines of a given research route.²⁷ When a scientific revolution (i.e. a radical backtracking) occurs, the winners claim that returning to a previous node and taking a different track constitutes progress. By the same taken, they declare that the earlier paradigm was, if not a dead end, at least a detour. But the mere fact that one scientific revolution has occurred creates the possibility of another, with the new winners engaging in the same dismissive discourse about the previous line of research. This is not the monotonic view of progress that Lucas seems to espouse. The second reason that he rejects this approach is that he dislikes the idea of ‘school’ or a ‘paradigm’. To him, they are ideological entities, and reasoning in terms of such notions is unproductive.

Concluding remarks

The aim of this paper has been to document the different steps by which Lucas moved away from Keynesian macroeconomics to become the leading figure in a new approach to macroeconomics. To the best of my knowledge, such a task has not previously been addressed. Having documented the stages, there are several paths along which the research could continue. The first is to critically assess Lucas’s account. I find Lucas’s narration of his intellectual journey honest and highly instructive. Nonetheless, in view of his stakeholder position, we cannot accept it uncritically. The fact that he took the trouble to write these meta-theoretical papers witnesses to his interest in setting the record straight for future historians. To get the full picture, complementary investigations are now needed to compare Lucas’s account with other testimonies and available documents.²⁸ A second possible line of research is to enter into a discussion about the validity of the often blunt, views expressed by Lucas in the papers under review – for example, his assessment of Keynesian theory. Finally, a critical study of the methodological underpinnings of Lucas’s account would constitute a third interesting development.²⁹

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²⁷ An old-style Keynesian economist would have to admit that a lot of progress in macroeconomics has taken place between, say, the Kydland-Prescott 1982 model and the present. But this is progress within the branch that Lucas pioneered, progress within a given research direction. The Keynesians would claim that exploring this branch is in itself a diversion, leading only to dead ends.

²⁸ To this end, the literature on autobiographical memory should be used. See Forget and Weintraub (2007).

²⁹ This last task is undertaken in De Vroey (2010).

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