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## **Equilibrium and the business cycle: From the inter-war debates to modern approaches**

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### **Introduction**

When business-cycle theory, after dominance of growth theory for more than two decades, entered centre stage again in the 1970s the assumption of continuous market clearing (as a methodological principle) in the New Classical paradigm and real business-cycle models became an issue of heated controversies<sup>1</sup>. That paradigm describes economic fluctuations as a purely equilibrium phenomenon in the monetary or in the real business cycles theory, and more specifically, in the latter one, fluctuations became optimal since they are no more than the expression of the agents’ optimal reaction to exogenous (real) shocks. In the opening passage to ‘Understanding Business Cycles’, one of his mostly read contributions, Lucas asks the question ”Why is it that, in capitalist

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<sup>1</sup> See, *e.g.*, Buiter (1980).

economies, aggregate variables undergo repeated fluctuations about trend, all of essentially the same character?" (Lucas 1977, 7). He continues to quote with approval Hayek's statement in *Monetary Theory and the Trade Cycle* "that the incorporation of cyclical phenomena into the system of economic equilibrium theory, with which they are in apparent contradiction, remains the crucial problem of Trade Cycle theory "(Hayek 1933, 33n). This clearly refers to the central question of an (in)compatibility of an explanation of the business cycle with the dominant general equilibrium approach in economics. Lucas, like Hayek, adheres to the concept of equilibrium as an indispensable tool for economic theory. However, a closer look into the quotation from Hayek shows that the latter here explicitly refers to Löwe's 1926 article 'How is business cycle theory possible at all?' "which remains, however, despite his analysis, the basis of my own work" (*ibid*). It is tempting then to identify the research programme of equilibrium business cycle theory launched by Lucas as another attempt to escape the fundamental methodological dilemma which had been so clearly stated by Löwe half-a-century before.

Such a view is not really straightforward. Indeed, in the meantime, arguments have been developed stressing the contradictions between Lucas' contribution and Hayek's initial project (Hoover [1988] and Arena [1994]). Lucas himself [1994] minimizes the Austrian aspects of his contribution telling that "I once thought of myself as a kind of Austrian, but Kevin Hoover's book persuaded me that this was just a result of my misreading of Hayek and others" (p. 222). In the *Nobel Lecture*, all references to Hayek have disappeared: Lucas henceforth introduces his work as a contribution for better understanding of the *effects of changes in money*. Here he identifies the challenge for monetary theory, *i.e.* the reconciliation of two incompatible ideas

*"Changes in money are neutral units changes and those changes induce movements in employment and production in the same direction" (Lucas [1996], quoting Hume [1752])*

Lucas argues that the quotation taken from Hume could be slipped into Keynes' *Treatise* and Hayek's *Monetary Theory and the Trade Cycle*, "without inducing any sense of anachronism" (*ibid*, p. 669). He is convinced by the idea that

*"Yet all of these theorists [mainly Keynes[1930 ]and Hayek[1933]] want to think in general equilibrium terms, to think people as maximizing over time, as*

*substituting intertemporally. They resort to disequilibrium dynamics only because the analytical equipment available to them offers no alternative” (Lucas [1996], p. 669)*

Lucas is indeed clearly convinced that improvements in mathematical and computational methods have a tendency to be neglected which is contradictory with the deep change they can induce in the economic thinking of some issues.

If we believe that such an argument is of great importance, it would nevertheless be misleading to analyse its contribution to macroeconomics independently of other theoretical influences. Indeed the exclusive focus on *technical tools improvements* could monopolise the attention to the detriment of a better understanding of the origins of the Equilibrium Business Cycles (EBC) approach.

Our objective is then to examine whether our understanding of modern EBC models can be improved by a closer look at the inter-war period, leading then to a better identification of the theoretical and historical roots of contemporary approaches. Such comparison of the inter-war debates with the modern Equilibrium Business Cycle theories should permit to evaluate the nature of the progresses proposed by the modern Equilibrium Business Cycle approaches. Obviously, that objective supposes to depart from the temptation to restrict our comparison to the traditional major references like for instance the one to Hayek and then to include in our set of reference a larger number of contributions (cf. Hagemann (2002)). More precisely, Lucas’ consecutive statements and the widespread neglect of pioneering authors, like among others Lutz or Slutsky, show the need for a systematic and accurate comparison of modern equilibrium theories of the business cycle with some interwar contributions which are still relevant today. The rich economic content of some earlier contributions focussing on the complex interrelation between real and monetary as well as structural change factors in economic systems, confronted with the greater analytical rigour and the more elaborated techniques of the modern approaches, may lead us to a better understanding of the fundamental methodological dilemma of business-cycle theory to integrate the phenomenon of cyclical fluctuations into the system of equilibrium theory, with which it is in apparent contradiction.

In *section 2* we define the literature more explicitly which the paper aims to analyse concerning both the modern approach and the already existing debates and papers which have tried to determine the possible ancestors of the modern EBC approach. *Section 3* reflects the literature of the interwar period focusing on the main contributions which stimulated research

on the challenging issue represented by an equilibrium business-cycle theory. *Section 4* then develops the “Methods and problems in business cycles theory” as they have been settled by Lucas and his followers. We then examine whether the use of the modern technical tools influenced the way we think of equilibrium business cycles. We also analyse the influence of those hypotheses on the role of the monetary sphere as well as on the notion of business-cycle this framework develops. *Section 5* analyses the contribution of the EBC approach developing the idea that although there is no doubt that such a view is of great importance for the development of macroeconomics, the most fundamental progresses are perhaps not exactly where they are supposed to be.

## 2. The scope of the paper

We can identify four different ways of dealing with the methodological problem - originally posed by Lowe - characterising the (in)compatibility of business-cycle theory with the theory of general economic equilibrium<sup>2</sup>. First of all, the abandonment of static equilibrium theory as it had been proposed by Lowe and, among others, endorsed by Kuznets. Second, the defence of the equilibrium method by dichotomizing the cycle from the trend and by introducing money and credit as a propagation mechanism for cyclical fluctuations in an existing equilibrium configuration, as it was the case in Hayek's response to Lowe's challenge. Third, a path-dependent approach without an *a priori* trend line, as it had been developed by Schumpeter. The fourth and last way out consists of the defence of the prevailing equilibrium approach by denying the need for the development of a general theory of the business cycle since *the* business cycle does not exist and the real problem of analyzing concrete-historical cases of fluctuations with a variety of different factors at work, could be completely dealt with within the framework of a modern static equilibrium approach. The latter route was taken by Lutz (1932) who also analysed the other variants in great detail.

One cannot read those statements without thinking about the modern approaches and debates concerning how to deal with the business-cycles issue. Among them, Lucas's contributions are essential not only because he contributed to the modelling process of the New Classical School but because he was almost the only one who really posed and tried to deal with the issues raised by equilibrium business cycle models. In “Understanding Business

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<sup>2</sup> See also. Rühl (1994).

Cycles” (1977)<sup>3</sup>, when he aims to define the modern EBC approach, Lucas considers “the prospects of accounting for cyclical phenomena by an economic theory, in the narrow sense in which Hayek and other business cycle theorists have used that term” (*ibid.* p.217 (1977) reprinted in Lucas (1981)). He then refers explicitly to the pre-Keynesian period only mentioning Hayek explicitly. Our opinion is that his reference to the authors of the thirties is too narrow and then misleading. There are at least essentially two motivations to enlarge the references to other pioneering contributions on business cycle theory.

The first motivation comes from the debate questioning the Austrian roots of the “New Classical Theory”. It seems that there was first a general agreement among commentators to consider the NCT as the revival of an older project. One can summarise that opinion quoting Scheide (1986) “This is not to say that new classical theory completely follows Austrian traditions. But many of the differences appear to be small or are only semantic in character” (p. 595). Two papers did not consider that opinion as self-evident and investigated the core of hypotheses of those approaches in greater detail. These are the contributions by Hoover (1988) and Arena (1994)<sup>4</sup>. Even if Hoover’s analysis is more nuanced, recognising that “the similarities between the Austrian and the new classical research programmes are striking” (p. 10), they both conclude that if we accept the view that the modern equilibrium business cycles and Austrian approaches belong to a common great tradition, on the fundamental points they only reveal apparent similarities. Indeed, both papers underline the superficiality of those similarities. We could sometimes develop similar positions, but our purpose is to enlarge the comparison of the modern equilibrium business cycles with older contributions in order to discuss the relative progress produced by the modern literature.

Our second motivation consists in thinking that the modern theory of business cycles could be better understood and evaluated if we enlarge the comparison to the general debates which animated the 1930s concerning the challenging issue of building a business cycle theory. Instead of focusing on a specific theory or approach, our purpose is to evaluate whether the nature of the debate on business-cycle theories has undergone drastic changes or whether they belong to the same line. Indeed, although the progresses in technique are obvious, it is not clear whether the fundamental issues which animated the interwar period

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<sup>3</sup> That paper has been reprinted in the book published in (1981) listed in our references.

<sup>4</sup> Hoover (1988) analyses the possible links between Lucas’s approach and the Austrian school, Arena (1994) concentrates exclusively on a comparison between the approaches by Hayek and Lucas.

have been solved or whether the scope of the debates could have been affected by the new technical tools.

The paper proposes an analysis of the building process of the Equilibrium Business Cycles as they were imagined and produced by the New Classical School. We examine the core of hypotheses which implies to refer to Lucas's 1972 seminal paper but also to his previous work with Rapping (1969) and of the many papers produced after<sup>5</sup>, in the line of the Monetary Business Cycles (MBC). Those papers represent what we call the first wave of EBC models, *i.e.* the *monetary* ones. We also consider in that paper the second wave of EBC models, the *Real Business Cycles* (RBC) theory. The objective is not to try to be exhaustive but to concentrate on the canonical models. Much more than the strict content of those models, we are interested in the study of the theoretical exchanges within the NCS on the fundamental issues raised by the business cycle, by their controversies and finally, by evaluating their contribution to the understanding of business cycles with regards to their possible intellectual ancestors.

### **3. From interwar debates ...<sup>6</sup>**

#### ***Schumpeter***

"Analysing business cycles means neither more nor less than analysing the economic process of the capitalist era." With this statement Schumpeter (1939, p. V) begins his monumental study *Business Cycles*, published at the end of the interwar period and after himself thinking on the subject for more than three decades. It is one of Schumpeter's great merits to have emphasized the importance of integrating the study of business cycles with an analysis of long-run economic development which does not follow a steady-state or balanced growth path. From the very beginning it was central for Schumpeter that a theory of economic development has to be constructed as a theory of business cycles. Economic development initiated by the introduction of innovations by pioneering entrepreneurs and fostered by bank credit implies the concept of endogenous disturbance of equilibrium rather than exogenous impulses, such as wars and bad harvests, which would not explain the regularity of the

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<sup>5</sup> Mainly, the Lucas's papers included in his 1981 book collection.

<sup>6</sup> Section 3 is based on Hagemann (2002).

phenomenon and the permanent struggle for a new equilibrium. Economic progress goes on in waves and crises are regarded as turning-points of economic development, an unavoidable consequence of the preceding prosperity and necessary for the process of reequilibration and future progress and development.

Schumpeter shared the basic idea of his teacher Böhm-Bawerk, who had a major influence on his research programme, that business-cycle theory constitutes the final stage of a fully developed system of economic theory. From the beginning he aimed at creating and designing this final stage of economic theory, exposed in particular in his masterpiece *The Theory of Economic Development* (1911). However, he makes it clear that this book and the earlier one on *Das Wesen und der Hauptinhalt der theoretischen Nationalökonomie* (1908) (The Nature and the Main Content of Theoretical Economics) form an entity. Schumpeter's theoretical system is based on the fundamental distinction between *statics* and *dynamics*, with *Wesen* playing the static and *TED* playing the dynamic part. The first focuses on the pure logic of a general equilibrium system and is mainly inspired by Walras, Schumpeter's great hero. The latter deals with dynamic analysis and is inspired by Marx's vision of the long-run evolution of the capitalist economy which constituted a life-long challenge for Schumpeter, in which emphasis on the causes and effects of technical progress is a key element.

Schumpeter's economic dynamics deals with the changing of the data of the static system, or the destruction of the circular flow by the carrying out of new combinations that include the five cases of the introduction of new methods of production, new products, the opening of new markets, new sources of supply, and new forms of organization. In Schumpeter's system economic dynamics is strongly linked to the phenomenon of economic development. The main carrier of economic development is the pioneering entrepreneur, who is an endogenous force in the economy and in marked contrast to the great majority of people limited to take routine actions only. Bank credit supplies the necessary means for complementing the innovations by a new allocation of resources. Thus innovations, entrepreneurs and credit constitute the three key elements of his theory of economic development, which in his view is endogenous, spontaneous and discontinuous. It is the task of dynamic theory to explain the origin and effects of these transition processes which essentially are a disturbance of equilibrium.

As a consequence of Ragnar Frisch's work on the clarification of the role of statics and dynamics in economics, which shaped the use of these terms in modern economic theory, Schumpeter substituted the terms "statics" and "dynamics" in the 1934 English edition of TED by the concepts of the "circular flow" respectively "economic development". From then onwards Schumpeter – who shared with Frisch the view that a Walrasian system of general economic equilibrium is inappropriate for business-cycle theory -, distinguished between statics as connecting economic quantities that refer to the same point in time, and dynamic theorems which include in their functions values of variables which belong to different points of time.

### *Lowe*

Adolph Lowe's contribution to the study of business cycles, to which the economic profession in Germany had turned in the 1920s, can hardly be overemphasized. With his seminal works, especially his Kiel habilitation thesis 'How is business cycle theory possible at all?' (1926) Lowe established himself as a central figure of the debates on business-cycle theory in Weimar Germany. This is the more remarkable since in those years he never developed his own theory of cyclical fluctuations. However, his thorough analysis of the methodological requirements a theory of the business cycle has to fulfil had a significant impact on the subsequent debates on business-cycle theory in the German language area<sup>7</sup> and, to a lesser extent, even internationally<sup>8</sup>.

In his "brilliant article" (Kuznets 1930b, 128) Lowe pointed out the fundamental conflict between the equilibrium theory on the one hand and the subject of inquiry, exhibiting recurrent fluctuations in macroeconomic key variables, on the other hand. He emphasized that all economic theories since the Physiocrats have centered around the concept of equilibrium and that "this equilibrium idea is axiomatically bound up with the closed, interdependent system ... - in short, a 'static system' " (Löwe 1997, 251). According to Lowe the analytical method which is logically connected with the concept of the static system, is the variation method, as it has been used systematically in Schumpeter's habilitation thesis *Das Wesen und*

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<sup>7</sup> For a detailed survey on contemporary reactions to Löwe's 1926 article see Gehrke (1997, 238-242).

<sup>8</sup> See, for example, the two articles by Kuznets (1930a,b).

*der Hauptinhalt der theoretischen Nationalökonomie* (1908). This "fundamental methodical principle of the general circulation theory" "seeks to determine the adjustment phenomena of the remaining variables on the hypothesis of a change in one of the data elements" (*ibid*, 250). However, although the variation method is capable to explain all the problems following from the interactions and interdependencies in a circular flow economy, it is incapable of explaining an economic history of cyclical fluctuations since more than a century. His analysis of the existing business cycle theories comes to the result that all serious approaches have either given up, at least partly, the interdependency requirements of the economic system or introduced exogenous factors. Furthermore, he stated the problem clearly: If economic theory is satisfactorily to explain the business cycle, it cannot do so simply by outlining the consequences of a disturbing factor exogenously imposed upon an otherwise static economy. Rather, it must seek for some causal factor endogenous to the system itself which can distort the rigid interrelations implied in the system of static equilibrium. Löwe therefore concluded:

"The business cycle problem is not a reproach *for*, but a reproach *against* the static system, because in it it is an antinomic problem. It is solvable only in a system in which the polarity of upswing and crisis arises analytically from the conditions of the system just as the undisturbed adjustment derives from the conditions of the static system. Those who wish to solve the business cycle problem must sacrifice the static system. Those who adhere to the static system must abandon the business cycle problem." (Löwe 1997,267).

He clearly associated himself with the first group. Since the static system were at odds with the economic-historical reality and the dominant equilibrium approach in economics had left open the solution of many problems, not exclusively those of explaining cyclical fluctuations, he favoured "the transformation of our existing static system into a dynamic one" in which "the polarity of upswing and crisis will acquire the same status as a data constellation which the equilibrium has in the static system" (Löwe 1997, 268). He identified technological change in the era of progressive industrialization to be the decisive endogenous factor generating the business cycle. Moreover, stimulated by the works of Marx and contemporary theorists, such as Schumpeter, Spiethoff and Sombart, he considered technological change not only as the decisive factor causing the cycle but also as the key factor for long-run growth.

From the beginning he therefore aimed at a theory of cyclical growth in which no artificial separation is made between the cycle and the trend.<sup>9</sup>

### *Hayek*

The challenge arising from Lowe's attack against the traditional concept of a static equilibrium and his plea for an alternative 'dynamic' system approach to explain business cycles is best reflected in chapter I, 'The Problem of the Trade Cycle', of Hayek's *Monetary Theory and the Trade Cycle* (1933). *Geldtheorie und Konjunkturtheorie* (1929) was originally submitted as his habilitation thesis to the University of Vienna. It clearly shows that Hayek's business-cycle theory was not only strongly influenced by Böhm-Bawerk's theory of capital, Wicksell's distinction between the natural and the money rate of interest and Mises's theory of money and credit, but also by Lowe's methodological challenge and the attacks by him and other members of the Kiel school, in particular Burchardt and Neisser, against monetary theories of the business cycle.<sup>10</sup> At the beginning of 1927 Friedrich August Hayek (1899-1992) had become the first Director of the Austrian Institute for Business Cycle Research in Vienna which had been founded at the initiative of Ludwig von Mises. He stayed in that position until 1931 when he became Professor at the London School of Economics.

Whereas the theories of Böhm-Bawerk, Wicksell and Mises contribute important elements to the constructive part of Hayek's business-cycle theory, the role of Lowe's methodological critique is of a completely different nature. As chapter I of *Monetary Theory and the Trade Cycle* documents, Lowe's 1926 essay posed a major challenge to Hayek at an early stage of his thinking on the subject. There are some important elements common to both Hayek's and Lowe's positions as, for example, their views on the relation between *empirical* observation and *theoretical* explanation. In particular Hayek accepted Lowe's seminal argument that all existing theories of the business cycle suffer from the fundamental weakness that they rely on *exogenous* shocks or disturbances and adjustments to such shocks in an equilibrium framework. Such a procedure could hardly result in a satisfactory theory to explain economic fluctuations which occur in a somewhat regular fashion. The logic of equilibrium theory "properly followed through, can do no more than demonstrate that such disturbances of equilibrium can only come from outside – i.e. that they represent a change in the economic

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<sup>9</sup> See also his retrospective reflections in Lowe (1989).

<sup>10</sup> For a more detailed analysis see Hagemann (1994).

data – and that the economic system always reacts to such changes by its well-known methods of adaptation, i.e. by the formation of a new equilibrium." (Hayek 1933, 42-43). Thus Hayek shared Lowe's view that the incorporation of cyclical phenomena into equilibrium theory is the crucial problem of business-cycle theory and that, accordingly, cycles should be explained as *endogenous* outcomes of market processes. However, the two authors differ fundamentally in the conclusions drawn. Whereas Lowe abandoned the traditional concept of a static equilibrium in favour of a new dynamic system in which the polarity of upswing and crisis takes the same position as the equilibrium in the static system, Hayek adhered to the concept of equilibrium as an indispensable tool for economic theory in general and for the understanding of intertemporal price relationships in particular.

To start from the assumption of equilibrium therefore is essential for Hayek's explanation of cyclical fluctuations. *Prices and Production*, which is based on his 1931 LSE lectures, is characterized by Hayek's "conviction that if we want to explain economic phenomena at all, we have no means available but to build on the foundations given by the concept of a tendency towards an equilibrium" (Hayek 1935, 34). For the analysis of dynamic questions it is essential to incorporate the element of time into the notion of equilibrium. Intertemporal general equilibrium then comprises equilibrium in the relative price of any one good at different points of time as well as between different goods at the same point in time. Interestingly, at the same time when his business-cycle theory was shaped, Hayek wrote an article on 'Intertemporal price equilibrium and movements in the value of money' (1928). Hayek's theory rests on the idea that prices determine the direction of production. Influenced by Mises he recognized changes in the relative prices of capital and consumption goods as the key to understand cyclical fluctuations. The function of prices as an intertemporal coordination mechanism is to give entrepreneurs the required information for their investment and allocation decisions. If in an equilibrium framework supply and demand are equilibrated via the price mechanism, how is it possible that cyclical fluctuations are a regular phenomenon since no change within the system can give rise to it?

"The obvious, and (to my mind) the only possible way out of this dilemma, is to explain the difference between the course of events described by static theory ... and the actual course of events, by the fact that, with the introduction of money ... a new determining cause is introduced. Money being a commodity which, unlike all others, is incapable of finally satisfying demand, its introduction does away with the rigid interdependence and self-sufficiency of the 'closed' system of

equilibrium, and makes possible movements which could be excluded from the latter. Here we have a starting-point which fulfils the essential conditions for any satisfactory theory of the Trade Cycle"(Hayek 1933, 44-45).

This is a precise statement that for Hayek the way out of what he regarded to be Lowe's impasse was the integration of money and credit into an equilibrium framework. Lowe, on the other hand, focused on technological change as the decisive endogenous impulse for the disturbance of equilibrium of an economy. Hayek's adherence to the concept of equilibrium in his business-cycle analysis had theoretical as well as political reasons. He regarded the free market economy as inherently stable so that all movements can essentially be seen as equilibrating adjustment processes.

### ***Slutsky***

Eugene Slutsky's 1927 article 'The Summation of Random Causes as the Source of Cyclic Processes' was already regarded among specialists as a classic paper in time-series analysis and business-cycle theory when it was published in its revised 1937 English version in *Econometrica*, due to an earlier English translation which had been initiated by Henry Schultz. Slutsky was one of the pioneers of the theory of stochastic processes, and in his 1937 article, he used serial correlation to prove that the summation of random causes may be the source of cyclic or undulatory processes which show an approximate regularity of the waves. Like Schumpeter he also identified "(t)he presence of waves of definite orders, the long waves embracing decades, shorter cycles from approximately five to ten years in length, and finally the very short waves" (Slutsky 1937, 107). Slutsky's idea that random shocks that were not themselves of a cyclical nature could generate regular oscillations of economic variables was an important and seminal idea. It did not only stimulate further research in time-series analysis but also freed economists from the belief that cyclical fluctuations must be due to causes that were themselves of a periodic nature. Although Slutsky's essay had a considerable influence on contemporary theorists, with the success of Keynesian economics and the decline of business-cycle theory after the war it almost fell into oblivion until the rise of Real Business Cycle theory in the early 1980s when Slutsky's idea that the cumulation of random shocks can produce cyclical fluctuations proved of seminal importance again.

## *Kuznets*

One of the earlier business cycle theorists who immediately recognized the importance of Slutsky's contribution was *Simon Kuznets* who had emigrated from Russia to the United States in 1922. Shortly after the publication of Slutsky's original Russian paper Kuznets wrote an article on 'Random Events and Cyclical Oscillations' in which he emphasized that "if cycles arise from random events, assuming the summation of the latter, then we obviously do not need the hypothesis of an independent regularly recurring cause which is deemed necessary by some theorists of business cycles" (Kuznets 1929, 274). That paper was followed by the article 'Equilibrium Economics and Business-Cycle Theory', in which Kuznets aims at a *general theory of economic change*. In essence he combines three ideas in an innovative way, namely Slutsky's thesis with Paul Rosenstein-Rodan's emphasis on the importance of time differences<sup>11</sup> and Lowe's methodological critique of the existing body of business-cycle theories for relying on the concept of equilibrium which is too rigidly static to grasp essentially dynamic processes such as cyclical fluctuations. Kuznets discusses Lowe's 1926 essay in considerable length and fundamentally endorses the German's position. His wording is even harsher when he concludes that "equilibrium economics was ... adding the dead weight of a barren doctrine to the burdens of a complex reality" (1930a, 390) or regards "the equilibrium approach ... to be a blind alley from the point of view of business-cycle theory" (*ibid*, 399) to end with the statement that "the practice of treating change as a deviation from an imaginary picture of a rigid equilibrium system must be abandoned" (*ibid*, 415).

On the other hand, Kuznets makes it clear that only the concept of a too static equilibrium approach should be discarded not the relations of interdependence involved. It should be supplemented with a much stronger component of the time element. Thus for the constructive part of his argument he aims at a synthesis of the ideas of Slutsky with those of Rosenstein-Rodan, who had made him aware of the importance of different speeds of response to a stimulus in different industries. Differences in time coefficients may not only arise because of differences in timing of the reactions but also because of their disproportionality, which, although not in itself a cause of disequilibrium, may aggravate the effects of time differences. In detailed empirical analysis Kuznets (1933) showed that capital-goods industries have larger

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<sup>11</sup> See Rosenstein-Rodan (1929).

cyclical oscillations than consumer-goods industries. In his earlier investigation on cyclical fluctuations in the retail and wholesale trade for the United States from 1919 to 1925, with which he had begun his work on business cycles, Kuznets (1926) had already come to the result that wholesale sales and manufacturing output have larger oscillations than retail sales. In his critique of business-cycle theories which rely on equilibrium economics he also points out that "(i)f the accumulation of empirical data about economic life teaches us anything, it is that different economic processes have different timing and varying duration" (1930a, 406). Measurement has to be guided by theory, but theory has to respect facts and the accumulation of economic knowledge by empirical work. These guidelines of Kuznets's research strategy are already key ingredients in his early work on business-cycle theory. Accordingly Kuznets did not doubt that random disturbances occur permanently and that there will be a summation of random changes as long as there are differences in time coefficients. "Thus any possibility of an equilibrium becomes exceedingly remote. For the inequality in time coefficients, be it only initial, opens the way to the cumulation of random causes, and they in their turn account for the appearance of cyclical fluctuations. In these conditions any persistent state of equilibrium is completely out of the picture" (*ibid*, 411). However, Kuznets, whose research interests shortly afterwards shifted to the analysis of long-term trends and economic growth processes, is already aware that two complications arise, from a certain skewness, i.e. the trend movement in random events, and from the fact that the stream of random changes cannot be treated as continuously random because after a while the trend component will affect business people's behaviour. Thus relatively small disturbances may cause rather important disproportionalities and may result in prolonged oscillations of formidable magnitude.

### ***Lundberg***

In the same year when Kuznets's article was published, the young *Erik Lundberg* (1930) wrote his licentiate thesis 'On the Concept of Economic Equilibrium'. Surveying the contemporary literature on the advances of equilibrium economics and the more recent development in economic dynamics, the essay constituted the embryo of his 1937 PhD thesis *Studies in the Theory of Economic Expansion* which became the highlight of the works of the

Stockholm School on macroeconomic fluctuations.<sup>12</sup> As the title of the thesis indicates, Lundberg embedded cyclical fluctuations as inherent in a growing economy. In fact his masterpiece, which became known to a wider audience only after its 1955 reprint, is an innovative and highly original work on the instability of growth which preceded Harrod's contributions. Lundberg formulated an early analysis of the multiplier-accelerator type. Most notable achievements were the explanation of the upper turning point of the business cycle in terms of an imbalance between aggregate investment and overall savings which was inspired by Wicksell, the focus on the lag between changes in demand and the response in output, the famous 'Lundberg lag', his sequence analysis of macroeconomic variables, and his inventory analysis which stimulated the later work by Metzler (1941). In the same year of publishing his most important work Lundberg became the Director of the *Konjunkturinstitutet*, the Swedish Institute for Economic Research, a position he held until 1955.

Lundberg's thesis 'On the Concept of Economic Equilibrium' shows the young author as remarkably erudite, well-read of the most recent literature in different languages and very independent and balanced in his judgements. The core of the essay consists of an assessment of the equilibrium method for a theoretical analysis of economic change.<sup>13</sup> He takes up Lowe's fundamental methodological challenge of the incompatibility of the static equilibrium system with business-cycle theory, is well aware of the contributions by Hayek, Kuznets and Rosenstein-Rodan discussed so far, and scrutinized extensively the contributions by, among others, Schumpeter, John Bates Clark and the more recent Swedish contributions by Myrdal on 'virtual' equilibrium states and Lindahl's 'moving' equilibrium, i.e. the sequence of successive equilibrium states. Although he does not directly refer to Slutsky's contribution, which at that time was only available in the Russian original, Lundberg had fully grasped the essentials of Slutsky's argument when he pointed out that "there are countless possibilities for oscillations which could become cumulative" and that "the proliferation of business cycle theories are to a larger degree due to the many possibilities offered by different time coefficients" (Lundberg 1995, 30). Emphasis on different time coefficients is also the essential argument of his critique of Hayek who relies on changes in the volume of money to

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<sup>12</sup> For a detailed assessment of Lundberg's contribution to the dynamic method of the Stockholm School see Hansson (1982).

<sup>13</sup> For an enlightened discussion of the importance of Lundberg's early contribution see Henriksson (1996).

break the rigid interdependence of the closed equilibrium system but disregards these differences although they are very important for the effects of variations in the money supply.

Lundberg's analysis also benefits from the knowledge of Ragnar Frisch's pioneering work on the concepts of statics and dynamics in economic theory, which constitutes the precise use of these terms in modern economics, but in 1930 was only published in Norwegian in a Danish journal (Frisch 1929). Following Frisch, Lundberg was actually concerned with the static method which is essential to all partial and general equilibrium theory. He also saw a decisive distinguishing element in the fact that the static method disregards that supply and demand do not only depend on the price but also on the rate of change whereas the dynamic method involves the rate of change or speed of reaction. Lundberg's essay also contains a precise distinction between two pairs of concepts: static versus dynamic and stationary versus changing or evolutionary state. The main aim is to elaborate the hidden premises and the limitation in the applicability of the equilibrium concept. The young author succeeds to a remarkable extent. He comes to the conclusion "that a dynamic analysis has to precede static analysis and not vice versa" (Lundberg 1995, 36), but that the two concepts should be used simultaneously and that general equilibrium theory is indispensable to economic thought since it comprises the interrelatedness of economic variables which cannot for a longer period diverge considerably from 'normal' positions.

### ***Lutz***

In *Das Konjunkturproblem in der Nationalökonomie (The Problem of Business Cycles in Economics)* (1932), Friedrich A. Lutz reviewed the existing body of theories of economic fluctuations from the viewpoint of equilibrium economics. Astonishingly, this work, which was submitted as his habilitation thesis to the University of Freiburg and is of great interest for a precise theoretical foundation of business-cycle theory, had been neglected in the modern literature on EBC and RBC models for a long time. Main reasons have to be seen in the fact that it somehow concluded the debate on the (in)compatibility of business-cycle theory with the equilibrium system, initiated by Lowe, in the German language area, where none of the main protagonists was living anymore soon after publication, and for the very same political reasons the German language lost its role as a medium of international scientific discourse. The importance of Lutz's work for modern debates was only rediscovered

recently when one author went even as far as to assess it as "one of the most splendid books on the history of business cycle theory ever written" (Rühl 1994, 188) and rightly emphasized that "Lutz's discussion should be considered indispensable for any truly comprehensive account of the origins of contemporary equilibrium business-cycle theories" (Rühl 1997, 416).

Lutz took the fourth route of dealing with the methodological problem of the (in-)compatibility with economic equilibrium, but analysed the other variants also in great detail. So he confirmed that Lowe had clearly seen and precisely identified the problems which cyclical fluctuations cause for static theory, but that he had proposed a solution, i.e. his demand for a new dynamic theory, which is impossible. Lutz maintained that Lowe had confused 'data' with 'variables', when he regards the data of the theory also subject to the general interdependence, which is definitely not the case. The introduction of technical progress, as it is favoured by Lowe, is nothing else than a change in the data which would lead to a new equilibrium. The analysis of changes in the data, which is the task of business-cycle theory, however, could be done completely within the existing framework of equilibrium theory by the use of the variation method. There would be no need for a 'new dynamic' theory. In the same line of reasoning Lutz criticizes Kuznets for his accusation that static theory had overlooked different reaction speeds. For Lutz, who only concedes that equilibrium theory, particularly in its Lausanne school blend, had not put enough emphasis on adjustment processes, static theory also includes the "investigation of how equilibrium comes about, i.e. the movement towards equilibrium following a change in data" (Lutz 1932, XX).

Lutz's analysis also contains an elaborated critique of Schumpeter's work. Lutz regards Schumpeter's conception of the static theory as being superior to that of Clark since, stimulated by Böhm-Bawerk, emphasis is also on the processes of economic change, caused by data changes, which lead to a new state of equilibrium. According to Lutz "the static theory is well able to deal with the tasks which Clark assigns to the dynamic theory. Its domain necessarily includes problems of change. The same forces operate in dynamics as in statics, but in the latter case they are in balance. Statics is really but a branch of dynamics, as Marshall put it. ... the mere fact that business cycle theory deals with processes of change does not mean that it is part of a dynamic theory rather than the static one" (Lutz 1932, 10, n.2). Lutz shows convincingly that this is carefully considered by Schumpeter who assigns to

dynamic theory the task of explaining the origin and effects of discontinuous changes within the economy. This endogenous part is played by the pioneering entrepreneurs, who implement new combinations of the factors of production. It is they who are responsible for economic development and thus for the difference between dynamics and statics. However, Lutz criticizes Schumpeter for relying on irrational behaviour among the group of entrepreneurs, since in the equilibrium state they miss profitable opportunities. If these opportunities would only arise due to technical progress they would come from without and not from within the economy. Schumpeter then for his dynamic theory would have to rely on exogenous forces, i.e. on changes in data, which do not necessarily imply periodicity. Schumpeter, too, relies on the assumption of rational behaviour by entrepreneurs for his analysis of the reequilibrating process. "In any case, since a theory is only possible on the basis of economic agents who behave rationally, Schumpeter's division between static and dynamic theory must be abandoned" (*ibid*, 14).

For Lutz's own solution of the dilemma posed by Lowe it is decisive to deny the regularity postulate. Time and again Lutz states that there is no general paradigm or 'nature' of business cycles, which therefore cannot be squeezed into an exact scheme. He 'proves' this by referring to many cases where business-cycle analysts depart from their own theoretical approaches in the explanations of concrete cycles, which are always the result of the interplay of a number of factors. Every business cycle is a historical event which constitutes an individual case. It is the task of the theory to explain individual real business-cycle trajectories by applying all the findings of static theory, particularly those of the effects of various data changes. There cannot be a general theory of the business cycle beyond what equilibrium theory has accomplished. Therefore all attempts to develop a 'new dynamic' theory have failed. It is surprising that Lutz's position and his resume, which is summarized in the concluding chapter 6 on 'The nature of the problem of business cycles', has not been referred to by modern representatives of Real or Equilibrium Business Cycle Theory.

However, despite the elaboration and quality of Lutz's arguments one may doubt whether one could unconditionally state "that Lutz's criticism 'won' the day" (Rühl 1994, 200, n.24). For example, it can be questioned whether one could grasp capital formation processes simply as data changes (which would also exclude the possibility of endogenous technical progress), as

Lutz did in his attempt to deny a special need for a business-cycle theory and to demonstrate the superiority of the equilibrium approach. A comprehensive explanation of cyclical fluctuations can be neither purely exogenous nor purely endogenous. Data changes always influence the economy, but they cannot by themselves produce recurrent and pervasive fluctuations, and with several changes, as, for example, changes in technology or taste, it is doubtful to regard them as exclusively exogenous. No doubt, however, that interdependency should be a key issue of a theory of business cycles which explains dynamic processes in a modern 'industrial' economy which nowadays furthermore is characterized by an increasing importance of services and knowledge.

### ***Frisch***

In the late 1920s and early 1930s *Ragnar Frisch* focused on the development of dynamic methods in economics and on business-cycle theory. His most significant methodological contribution was the precise distinction between static and dynamic analysis which was already fully developed in his 1929 Norwegian article on 'Statics and Dynamics in Economic Theory' (Frisch 1929).<sup>14</sup> His important definition implies that a genuine dynamic analysis contains at least one economic variable which is related to different points of time. "*Any theoretical law which is such that it involves the notion of rate of change or the notion of speed of reaction (in terms of time), is a dynamic law. All other theoretical laws are static. A static law is a comparison between alternative situations, a dynamic law an analysis of rates of change*" (Frisch 1992, 394). From this it follows that Frisch thought a Walrasian system of general economic equilibrium as inappropriate for business-cycle theory.

The clarification of his views on dynamics versus statics and of other methodological concepts such as, for example, the distinction between micro-dynamic and macro-dynamic analysis was a sound foundation when Frisch embarked for business-cycle analysis in the early 1930s. From that period is his famous contribution to the Cassel Festschrift, 'Propagation Problems and Impulse Problems in Dynamic Economics' (Frisch 1933), which is included in this volume.<sup>15</sup> The main idea of the article, to distinguish between two

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<sup>14</sup> It took until 1992 before the decisive parts were translated into English; see also Andvig (1992).

<sup>15</sup> For a detailed analysis of Frisch's development and his contributions to business-cycle analysis in the interwar period, based on rich sources of original material, see Andvig (1981).

fundamental problems in the analysis of cyclical fluctuations, the propagation problem and the impulse problem, goes back to Wicksell. Frisch<sup>16</sup> also explicitly refers to Slutsky's idea that erratic shocks may cause more or less regular cyclical movements. Frisch's sharp and fruitful distinction between exogenous random disturbances (exterieur impulses) and the intrinsic structure or propagation mechanism, by which the economy transforms them into cyclical fluctuations, has proved seminal for modern business-cycle theory. Frisch assumed the economy to be dynamically stable so that the intrinsic structure is dampening the oscillation caused by a single shock. However, shocks are occurring quite frequently so that the economy keeps fluctuating. While the amplitude of the cyclical swings is mainly determined by the strength of the exogenous impulse, the propagation mechanism accounts for the regularity of alternating movements of expansion and contraction, including the length of the cycles.

Frisch believed in the existence of more than one cycle. In his 1933 essay he distinguishes between three cycles of various length: the primary or classical cycle of 8.57 years, the secondary cycle of 3.50 years and a tertiary one of a little bit more than two years. He later firmly believed in the Kondratieff cycle<sup>17</sup>, which may be caused by the strong exchange of ideas he had over many years with Schumpeter on the role of innovations as a key factor in maintaining oscillations.<sup>18</sup>

Methodologically Frisch tried to bridge the gap, which existed in the late 1920s, between empirical research on business cycles, such as by Wesley Mitchell, favouring the inductionist method, and purely theoretical research, which did not even aim at sound empirical analysis using modern statistical techniques and making their approaches operational. The latter attitude was quite common in particular among monetary theorists of the business cycles, including leading representatives of such diverse schools as the Cambridge and Austrian one.

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<sup>16</sup> See Frisch (1933, 198) and the contribution by Wicksell in Volume II of this collection.

<sup>17</sup> See Andvig (1981, 708).

<sup>18</sup> See the final section 6 of his 1933 essay.

#### **4. ...to modern approaches**

##### ***The core of the new classical doctrine as an answer to the interwar project?***

The modern equilibrium approach was built on a set of hypotheses which fundamentally disturbed the course of the development of macroeconomics. Among those hypotheses one can identify a core of assumptions which structured and provided foundations to the New Classical School. Even if the followers were numerous, such a building process had a pioneering figure in Robert Lucas. His starting point was to consider that macroeconomic theory must be *useful* which means for him that macroeconomic theory must be based on a model in order to allow measurement and discussions. That objective implies some specific choices in terms of modelling. In that section, we propose to examine the influence of the hypotheses and technical tools had on the direction of Lucas's project. Our motivation is partly driven by the fact that we think that traditional history of doctrine has a tendency to neglect the forces of mathematical and computational methods improvements but those elements can induce deep changes in our thinking. Lucas shares that same opinion with the difference that he does not seem to think that the use of some technical tools can deeply modify the fundamental meaning of a theoretical approach. Our purpose in that section is to discuss to what extent the use of the modern technical tools contributed to major changes in the way we think of the equilibrium approach to business cycles.

##### ***"Old" roots for the Lucas project***

Lucas fundamentally believes that the main reason why predecessors either were not successful in reconciling growth and cycle theories within the same equilibrium framework or avoided the issue was because they were not "well enough equipped technically to move to discussion to a sharper or more productive level" (Lucas [1980], page 699). It is in that line that Lucas wants to renew "old" theoretical proposals, thinking that now, he has the technical possibilities to deal with them. He successively stressed his theoretical connection to both Hayek's thought and Hume's one (respectively in Lucas [1977] and Lucas [1996]) arguing that, on his opinion, those authors "resort to disequilibrium dynamics only because the

analytical equipment available to them offers no alternative” (ibid, page 669). He then specifies:

“The intelligence of these attempts to deal theoretically with the real effects of changes in money is still impressive for the modern reader, but serves only to underscore the futility of attempting to talk through hard dynamic problems without any of the equipment of modern mathematical economics.” (ibid, page 669)

The aim of Lucas is a double one. First of all, Lucas rejects what he may consider as a “schizophrenic view”, *i.e.* that there would be one methodology – the equilibrium one – for the analysis of long-run and stable positions and another methodology – the disequilibrium approach – which may explain recurrent fluctuations.

More specifically, the fact that it has been empirically proved that *business cycles are all alike* is of crucial importance since it suggests the possibility of a *unified explanation* of business cycles “grounded in the general laws governing market economies, rather than in political or institutional characteristics specific to particular countries or periods” (Lucas [1977], page 218).

Both positions motivated Lucas to argue in favour of a business-cycle theory compatible with a general equilibrium approach. On that point, there is a clear convergence with Hayek’s project<sup>19</sup>

“(…) the incorporation of cyclical phenomenon into the system of economic equilibrium theory, with which they are in apparent contradiction, remains the crucial problem of the Trade Cycle Theory” (Hayek [1933], page 33n.)

Specifying immediately that

“By “equilibrium theory” we here primarily understand the modern theory of the general interdependence of all economic quantities, which has been most perfectly expressed by the Lausanne School of theoretical economics” (Hayek [1933], 42n.)

Within that equilibrium framework<sup>20</sup>, Lucas presents a second objective (1996). He wants to reconcile in a model two *a priori* incompatible ideas: “changes in money are neutral units changes and those changes induce movements in employment and production in the same direction” (Lucas [1996], quoting Hume [1752]), *i.e.* to reconcile short run disturbing

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<sup>19</sup> One must notice here that Lucas himself in later comments (1994) had recognized that if he had once the impression to work in the Austrian line, it was because he had not read Hayek carefully. This more or less brought to an end the debate on his may-be *filiation* to Hayek’s approach of Trade Cycles.

<sup>20</sup> On the nature of equilibrium in Hayek’s view, one should be careful since there are other interpretations like the one provided by Arena (1994) who considers that “In spite of his reference to Walras or the Lausanne School, the view according to which Hayek would have been a follower of Walras, and Lucas a follower of Hayek, appears to be superficial” (p. 211) and specifies further “in Hayek’s mind, equilibrium theory includes James Mill as well as Say or Walras. Now, those authors have not developed drastically different theories of prices. The reference to equilibrium is therefore more general than a kind of intellectual oath of allegiance to Walras” (p. 211).

monetary changes with long run monetary neutrality. One must notice that such an objective was not present in the Lucas's original contributions (1972, 1973), neither in the following comments written in 1977 nor in 1980. That reference to Hume has been mainly developed in the Nobel Price lecture (1996).

### ***The “essential” requirements***

Lucas considers that a theory cannot be a collection of assertions about the behaviour of the actual economy but rather “an explicit set of instructions” in order to build a parallel system producing a mechanical imitation of the observed movements of key variables of the economy.

On that point one can see a connection with Hayek but the convergence with Lutz may be stronger:

“Firstly, [trade cycles theory] must be deduced with an exceptionable logic from the fundamental notions of the theoretical system; and secondly, it must explain by a purely deductive method those phenomena with all their peculiarities which we observe in the actual cycle” (Hayek [1966], pp.32-33)

Such a view has been criticized. Adopting that view means that a “good” model is a model which provides the best imitation of the economy whatever the degree of realism of its hypothesis. As a direct consequence, this statement means that Lucas considers as important the models with a great degree of prediction.

Lucas's objective is to explain how cycles can emerge from an *equilibrium* theory; he considers fluctuations as the manifestations of changes of the equilibrium positions. Since in an equilibrium theory, disturbances can only come from “outside”, *i.e.* from changes in the economic data. Such a change provokes the formation of a new equilibrium. In monetary business-cycle theory, changes in the equilibrium are provoked by a signal extraction issue *i.e.* by an imperfect information hypothesis which corresponds to the fact, at the moment the shock occurs (increasing price for instance), that agents cannot distinguish between an increase of the general price level and an increase of the demand for their own good. In the Real Business cycles models, changes in the equilibrium would also come from “outside”: information is perfect and when the shocks are identified as temporary ones, they activate an *intertemporal substitution mechanism* which affects the equilibrium position. One must notice here that even if intertemporal substitution is the fundamental mechanism used by the RBC

models, it has been first introduced in Lucas and Rapping (1969) and also has been intensively used in Barro (1981) who underlines that the substitution will strongly depend on the nature of the shocks, *i.e.* with a temporary or permanent impact. On a purely theoretical side, Barro (1981) contributed a lot to the evolution from M.B.C. to R.B.C.. He analyses the real impact of changes in government expenditures-budget and uses for that intertemporal substitution mechanism. Barro (1981) then can be seen as the bridge between the two waves of the EBC approach. Lucas seems to have adopted intertemporal substitution mechanism after RBC emerged. Indeed after criticism addressed to the empirical relevance of his monetary analysis of business cycles, he abandoned purely monetary explanations and moved to more real-monetary explanations of business cycles (what he calls “hybrid models”<sup>21</sup>) and as, the RBC models, abandoned the imperfect information hypothesis and replaced it by intertemporal substitution mechanism

“Though the theoretical formalism on which they draw involves a static equilibrium combined with a mechanical process to describe dynamic adjustments, their verbal descriptions of periods of transitions, like Hume’s before them, show that they are in fact thinking of people solving intertemporal decision problems.” (Lucas [1996], page 669)

The use of that mechanism appears convenient when the model will have to fit actual data movements: The result of the simulation of the model heavily depends on the value of the elasticity of substitution. One must notice here that in the 1972 article, when Lucas does not use intertemporal substitution in order to move the equilibrium but imperfect information, those two mechanisms are in fact the possibilities EBC has in order to produce (apparent) cyclical movements of equilibrium.

The third point is that *useful* discussions of economic policy must ultimately be based on models since they necessarily involve *quantitative assessments*. This arguments means in fact that participants in the discussion must have “explicitly or implicitly, some way of making a quantitative connection between policies and their consequences” (Lucas [1987], page 6). The model must be an explicit description of the way the economy evolves through time: here is the appeal for the fourth point, the need for *microeconomic foundations*. In order to know the impact of an economic policy the decision problem must be identified<sup>22</sup>. At this point, we

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<sup>21</sup> See Lucas (1987).

<sup>22</sup> Lucas does not believe that it is the whole macroeconomic analysis which has to be based on choice theory. In an interview made by Snowdon, Vane and Wynarczyk and published in their book (*ibid*, 1997), Lucas mentioned the works of Sims and Litterman as good extrapolation methods and nevertheless without any microeconomic foundations.

understand better why Lucas considers that his approach is so deeply dependent on progress in dynamics and mathematics in general.

Since the model has to be set within an equilibrium framework, the first direct implication is that the origin of fluctuations is exogenous<sup>23</sup>.

“All it does mean is that, in the model, the objectives of each agent and the situation he faces are made explicit, that each agent is doing the best he can in light of the actions taken by others, and that these actions taken together are technologically feasible.” (Lucas 1987, page 16)

The next step consists in setting a *Markovian* environment in which the knowledge agents have is condensed in a vector of state variables. The agents’ pay-offs are separable and only depend on state variables and current actions. Agents decide strategies taking into account the Markov process followed by those variables. Individual actions cannot influence pay-offs of other agents. Then variables which are the outcome of aggregate behaviour actually are in fact state variables resulting from individual decisions issues. Their evolution is exogenous for each individual agent but endogenous for the system itself.

As it can be guessed, the use of rational expectations has another direct implication, the introduction of agents’ *subjective probabilities* with observed frequencies of the events to be forecast or with ‘true’ probabilities. Evidently, this hypothesis cannot help to understand situations in which one cannot guess which observable frequencies are relevant. It will be useful only in situations in which probabilities concern a fairly well defined recurrent event, i.e. situations of *risk* in Knight’s terminology. Lucas restrains himself to that context because he considers it is the only case which can be *useful*. “In case of uncertainty, economic reasoning will be of no value.” (Lucas [1977], page 224). He then underlines

“Insofar as business cycles can be viewed as repeated instances of essentially similar events, it will be reasonable to treat agents as reacting to cyclical changes as “risk”, or to assume their expectations are rational, that they have fairly stable arrangements for collecting and processing information, and that they utilize this information in forecasting the future in a stable way, free of systematic and easily correctable biases.” (Ibid, page 224)

That core hypothesis allows now Lucas and his followers to deal with a business cycle theory compatible with an equilibrium analysis. What is nevertheless the nature of such equilibria? What sort of business cycles or instability are we considering there?

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<sup>23</sup> That point is clearly seen by Hayek (1933) as the major problem of the Trade Cycle Theories which take as a starting point a disturbance of equilibrium: “This difficulty arises because, in stating the effects of that disturbance, they have to make use of the logic of equilibrium theory. Yet this logic, properly followed through, can do no more than demonstrate that such disturbances of equilibrium can only come from outside- *i.e.* they represent a change in economic data- and that the economic system always reacts to such changes by its well-known methods of adaptation, *i.e.* by the formation of a new equilibrium” (*ibid*, page 43).

Our aim in the next points is not to focus on the strict implications of the various EBC models for the understanding of specific features of business cycles, or for the analysis of the impact of economic policies. Those points are already well-known by both supporters and opponents of that approach. We want to focus on the EBC theory contribution to the debates in business cycles. Indeed, the integration of business cycle in an equilibrium analytical framework is a theoretical challenge of great importance which has direct consequences on the way we consider then the influence of the monetary sphere as well as on the nature of the fluctuations the models describe.

### ***Real versus monetary theories of Business cycle: a traditional debate?***

Lucas' contribution proposed an explanation for fluctuations in equilibrium terms. More explicitly, his model is based on the idea that there are strong (equilibrium) forces which represent the attractor of the economy to its natural equilibrium. The stability is rooted in the market forces which means, *in the real sphere*. The monetary sphere is exclusively seen as the source of temporary disturbances. Real variables are then the only ones to determine growth and fluctuations are seen as purely random fluctuations around the steady state, *i.e.* around the long-term trend.

The statute of monetary shocks. Lucas's purpose - as it was developed in the 1996 Nobel Price lecture - was to overcome a *Classical* limit, precisely the dichotomy between the theory of value and the theory of money. In the 1972 model, he does not really produce such a theory but he introduces a hypothesis on the nature of information which allows him to draw a link between both theories. Indeed, the hypothesis of imperfect information prevents agents to discriminate instantaneously between monetary and real disturbances. It is the so-called problem of the "signal extraction". Of course, "at the moment" people get the right information, they determine the nature of shock and then, optimize. If the monetary shock was a random walk, it had of course no consequence on the trend level and agents immediately adjust their decisions, coming back to the initial equilibrium. If the monetary shock was an unanticipated monetary policy decision –in that case there is a rule behind the monetary decision – then, the new monetary rule becomes endogenous for agents and then the impact such a monetary policy could have on the level of the economic activity would be

fully “neutralized”. Here is one of the major direct implications of rational expectations: agents are able to learn and then make endogenous the behavior of monetary authorities. The combination of that hypothesis with the one of continuous market-clearing equilibrium leads to the conclusion that anticipated monetary<sup>24</sup> policies will have no real impact.

The main problem that first model had to face is that such a *pure* New Classical model cannot explain what observations tell us: fluctuations are persistent. More than that, it does not seem that monetary shocks are so recurrent that they could explain regular fluctuations. Lucas was then forced to resort to an additional – and external – assumption: the use of time lags which could explain recurrent fluctuations provoked by single (and simple) monetary shocks. Introducing a so *ad hoc* assumption led Lucas to recognize the empirical weakness of his theory. Lucas wanted to provide a pure theoretical explanation of business cycles and then, he finally rejected any use of additional hypotheses<sup>25</sup>. The introduction of lags was interpreted by Lucas himself as the proof that his model lacks for a propagation mechanism. The main issue is that such a model denies any temporal dimension, all events can be undone. In fact, it is logical since the informational problem cannot last over time (it is not a *structural* informational problem (like asymmetry could be): agents are learning and the model is fully invariant.

One could consider that such a view of “monetary business cycles” gives a paradoxical role to money. On the one side, one cannot deny that Lucas developed a monetary theory, but it is not so much money which is essential for the emergence of fluctuations but the “surprise effect”. For some economists, the Lucas’s model is a “progress” in the super-neutralization of money since compared to the monetarist analysis, the new-classical school presents the monetary authorities activity as an object on which agents have expectations. Then when the behaviour of those authorities is rational, it is logically expected and neutralized. There is no possibility to conceive a monetary policy with the aim to modify agents’ behaviour. Indeed, money never participates to the determination of the activity level – only to its temporary fluctuations. On the other side, money is a crucial element of that theory since, because the real sphere is fundamentally stable (even the rate of growth is given), disturbances can only come from the monetary side. In fact, money is used in order to introduce imperfect information and disturbances in agents’ expectations. The way Lucas (1987) will propose to integrate money in latter work was far more convincing but nevertheless did not provoke a

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<sup>24</sup> That result will be expanded to other demand shocks in general.

<sup>25</sup> One must notice that the performances of the time-lagged model were not far better.

great enthusiasm: perhaps economists were too much attracted by the rapid development of the RBC models to feel interested by that line of research. Then, Lucas's monetary theory is far more explicit and based on sound and deep theoretical roots than his RBC companions, nevertheless in the pure core of EBC approach we are focusing in that paper, monetary issues were never of major importance *per se*.

The "real" answer. In response to these new developments, some classical theorists have tried to provide "a more rigorous equilibrium analysis" which could offer a solution to the theoretical weaknesses of Lucas's first attempt, expecting also that the new class of models could fit better with the empirical data.

RBC approach represents an extreme radicalisation of the EBC initial project. Adopting the same framework as Lucas, they excluded monetary shocks and they introduced the hypothesis of perfect information<sup>26</sup>. Now, agents are not disturbed anymore in their analysis of the disturbances: they are only affected by technological (real) shocks<sup>27</sup> and the magnitude of their reaction depends on the temporary *versus* permanent impact that shock is supposed to have on the economy. The most important direct consequence of that second wave of models is that they totally rejected the idea that the cycle has to be dampened. The fluctuations are the optimal reactions of rational and perfectly informed agents.

Another important contribution of the RBC theory certainly lies in its capacity to generate recurrent fluctuations, the artificial economy being only affected by a "one shot" shock. The problem is that those models exhibit persistence only if the shocks themselves are defined with such a property<sup>28</sup>. The persistence is rooted in the sphere of impulsion not in the sphere of propagation. One must notice here that such hypothesis received a great support from the econometric work of Nelson and Plosser (1982)<sup>29</sup>. Then, the recurrence and

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<sup>26</sup> Paradoxically, the idea has been developed by Tobin (1981) when addressing his critique of the Lucas 1972 model.

<sup>27</sup> Formally speaking other shocks have been introduced but in any case those models could convincingly reproduce the fluctuations observed which is the criterion recognized by EBC approach allowing to validate the "good" model.

<sup>28</sup> It has been shown that a shock defines as a simple random walk cannot generate the persistence observed in fluctuations; in order to do so, one has to suppose that the shocks are AR (1).

<sup>29</sup> Since 2003 many econometric papers have been published which underline the importance of demand shocks on long run dynamics. For a survey as well as an original contribution, one can refer to Hartley and Whitt (2003) and other studies which underlines that there can be permanent demand shocks. Allowing whether demand or supply shocks of either type - temporary or permanent - to disturb the economy, they find that permanent and temporary demand shocks have been the dominant source of invariance in output growth in United States and in the five European countries they tested (France, Germany, Italy, United Kingdom and Netherlands).

persistence of fluctuations is much more posed as a prerequisite hypothesis on shocks stochastic properties than it is the outcome of the computation of the model itself.

Another issue is problematic: none of those models do consider the determinant of turning points of business cycles. Impulse but also propagation and turning points depend on stochastic shocks. Then, RBC models had to convince that long periods of recession were due to lasting negative technological shocks... Those models appeared just after the petroleum shocks which perfectly fitted with the way they considered the nature of the shocks. On that point one can consider that the modern stochastic tools totally ignored that issue which was so fundamental for past research programmes.

The RBC approach introduced money in some models but their monetary analysis was not very convincing<sup>30</sup> way for economists interested in those issues and not in a satisfying way for a RBC modeller whose final aim is to build an artificial economy reproducing the observed fluctuations<sup>31</sup>. The point has been defended heavily by Plosser.

*“The nature and magnitude, however, of the fluctuations and responses in the real neoclassical model means that real business research poses a challenge to conventional views regarding the relative importance of money”. (1989, p.70)*

Plosser refers intensively to Hicks “*Value and Capital*” and to Hicks (1933) underlines the role of analysing “idealized state of dynamic equilibrium...(which) give(s) us a way of assessing the extent and degree of disequilibrium”. Plosser argues that

*“Progress towards understanding this idealized state is essential because it is logically impossible to attribute an important portion of fluctuations to market failure without understanding of the sorts of fluctuations that would be observed in the absence of the hypothesized market failure” ((1989) p. 52).*

Gaffard (2000) argues that such a reference to Hicks in order to justify a pure real analysis of business cycles can be misleading<sup>32</sup>. Hicks himself explains that

*“A more general, and more convincing, approach would be to assume that both sources of instability are, at least potentially, at work- that monetary instability, of the kind we have just considered, is surimposed upon the real instability which we analysed in the earlier chapter of this book (Hicks 1950, p. 153)*

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<sup>30</sup> One could nevertheless distinguish the King and Plosser model (1984) as well as the Williamson (1987a) and (1987b). Nevertheless, the latter one introduced imperfect information in order to give a more heavy weight to for instance, financial intermediation.

<sup>31</sup> See Cooley and Hansen (1995) in Henin (1995) for a survey of those attempts.

<sup>32</sup> For Gaffard, Hicks project has never been to oppose real and monetary explanations but to understand each mechanism separately in order after, to be able to combine their interactions.

The fundamental problem for business-cycle theories in general and for the EBC approach in particular is not so much the origin of the shocks but is to account for large real fluctuations without having being able to identify shocks that are of the right order of magnitude. Taking that perspective, there is not a great gap between those two waves of EBC models. We do not want to minimise the difference concerning the origin of the fluctuations between EBC I and EBC II, but Lucas did not try really to oppose to the RBC objecting them the non importance of monetary factors.

### ***Equilibrium analysis and Business Cycles theory***

The main theoretical outcome of this approach as a whole is that stabilisation policies are powerless. In Lucas's monetary business-cycle theory, monetary policy rules are fully anticipated and then their impact strictly becomes neutralized. When the monetary authorities' interventions are strictly discretionary, then the policy surprises the agents but nevertheless has no persistent impact on output levels or fluctuations.

In the RBC models, intervention can only be damageable since agents reactions to exogenous shocks are the optimal reactions which are translated into variations of the trend itself (instead of variations around the trend, the trend being always considered as the optimal reference path). Such a view draws a link with the statements of Lowe we presented in the introduction *i.e.* to develop and study a path-dependent approach without an *a priori* trend line, as it had been developed by Schumpeter. Arguing for some of them that they were pursuing the same line as Schumpeter, they underlined the fact that their model does reproduce that artificial distinction between trend and fluctuations. Nevertheless, trend remains fundamentally exogenous in this approach. The dichotomy between growth and cycles is eliminated but replaced by a dichotomy between permanent and transitory shocks<sup>33</sup> which is not less arbitrary.

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<sup>33</sup> This can be easily shown by the analysis of Nelson and Plosser (1982).

## Conclusion

To propose an overall assessment of the impact of the equilibrium business cycles approach would be a difficult task considering the great variety of models<sup>34</sup> and of topics those models focused on. Our purpose was to examine the impact of the EBC theory on the understanding of the fundamental methodological dilemma of business-cycle theory to integrate the phenomenon of cyclical fluctuations into the system of equilibrium theory, with which it is in apparent contradiction. The solutions proposed by the various approaches in order to deal with that theoretical challenge have direct consequences on the way the influence of the monetary sphere as well as the nature of the fluctuations can be considered.

Paraphrasing Johnson (1971), the most helpful circumstances for the rapid propagation of a new revolutionary theory is certainly the existence of an established orthodoxy which is clearly inconsistent with the most salient facts of reality. In the context of Keynesianism facing the puzzling economic issues of the 1970's, particularly the stagflation problem, one must recognize that the diffusion of the EBC approach was extremely rapid, not only in a strict reference to the so-called New Classical School but also considering its opponents, the New Keynesians. Indeed, if the latter were - and still are - strong adversaries of the continuous market clearing hypothesis, they did build their theory nevertheless in reaction to the NCS and in that sense contributed also to the spreading of those ideas and debates among academics. The major difference between the modern classical and Keynesian streams comes from the fact that Keynesians rejected the traditional micro economic theory in order to deal with macroeconomic issues which led them to adapt micro to macroeconomics and not the reverse. Nevertheless, despite the dominance of the contributions by Lucas, Sargent and Wallace, throughout the 1970's, in the early 1990's several weaknesses appeared which participated to the persistent interest of economists for Keynesian but in general for competing approaches.

Now, if we strictly focus on the contribution of EBC to the evolution of the debates we can examine the current structuring lines of research in business-cycle theory. First of all, the

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<sup>34</sup> From the pure RBC to the ones including monetary shocks, international trade (cf. Cooley 1995)... etc. one should also mention the models named the *Augmented Real Business Cycles models* (ARBC) which, without departing from the core of hypotheses of the RBC theory, included Keynesian characteristics like price rigidities, demand shocks... etc. This literature is very well represented in the Henin (1995) volume. Without being considered as Keynesian models, those contributions have nevertheless different conclusions from the ones obtained by the original RBC models.

notion of equilibrium developed within that modern framework, has drastic implications on the way the economy works. One can easily recognize that this concept of equilibrium is important when the aim is to build economic models with predictive contents. Indeed, to allow disequilibrium affects the capacity to forecast agents' actions. Here the core of the common goal of the classical and the Austrian school can be found: the need for rational micro-foundations of macroeconomics. Logically, the intensive use of rational expectations (Muth 1961) is consistent with the idea of building a *useful* model, but the hypothesis of complete - and sometimes perfect – information remains highly questionable. The continuous market clearing assumption clearly depends on the structure of the available information. As it is noticed by Laidler (1986),

*“the difficulty with the new-classical economics lies not in the equilibrium postulate per se, but in its insistence that we model the economy as a whole as if the equilibrium strategies of individuals were formulated and executed in an institutional framework characterized by continuously clearing competitive markets” (p. 349)*

The best evidence of this can be seen in the nature of debates which opposed the new classical against the new Keynesian view: Stiglitz (1991) never refused the rational expectations hypothesis, but introduced asymmetric information. He reduced its implications and explained how disequilibrium (in the strict sense, when supply differs from demand) can nevertheless be an optimal solution, and another sort of equilibrium. Such a view allows also makes it possible to abandon the contestable framework of an economy built on the concept of the representative agent: asymmetric information implies agents who differ according to their information set. Despite that important debate, it would be difficult to deny the relevance of the introduction of rational expectations hypothesis on the way economists think of economic policies. Indeed the problem posed by the credibility of economic policies for the predictive contents, emphasized also in Laidler (1986), is one of the most important questions on which the new-classical tools enlightened our view even they have also stimulated highly critical reactions.

Perhaps a more surprising outcome of that line of research is the fact that the opposition between real and monetary analysis does not finally represent a fundamental split within the EBC framework. Nowadays, Lucas's paper of 1972 appears much more as the first attempt and then the cornerstone of a new approach than a strict and long lasting contribution in itself

representing the observed fluctuations. The importance of Lucas's first contribution lies in its ability to reconcile equilibrium analysis and business-cycle theory within the same framework. However, weaknesses were underlined concerning the performance of such a model to explain the observed fluctuations characterised by high persistence. It finally did not give a great support to the monetary explanation of the fluctuations and gave the floor to the RBC. Today, it is certainly much more obvious that the Lucas's 1972 contribution was much more fruitful through the interrogations revealed by the Lucas critic and its implications for monetary policy<sup>35</sup>.

Logically, the RBC models were seen as an answer to the weaknesses of the first version of EBC. Most of the time, they denied any role to the monetary sphere, they focused on a perfectly adjusted economy, translated in the RBC terms by continuously clearing markets with perfectly informed agents reacting optimally to exogenous real shocks. It is only a second range of models which tries to introduce monetary influence<sup>36</sup>, but their attempts were not considered as really improving the capacity of those models to mimic the observed fluctuations. The latter was interpreted by some of the proponents as the evidence that monetary factors are only of poor importance for the understanding of business cycles movements.

Certainly the remarkable contribution of the RBC models lies in the capacity to generate recurrent fluctuations, being only affected by a single shock. The problem is that those models exhibit persistence only if the shocks themselves are persistent<sup>37</sup>. This property is rooted in the sphere of impulsion not in the sphere of propagation. This hypothesis received a great support from Nelson and Plosser's (1982) econometric work. Then, the recurrence of fluctuations is posed much more as a prerequisite hypothesis than it is the outcome of the RBC.

Instead of distinguishing strongly between real *versus* monetary origins of the business cycles, the two waves constituting the EBC approach can be seen as homogenous since they both focus on propagation mechanisms which are fundamentally real. Indeed, even in Lucas, the origin of the cyclical fluctuations comes from the problem of "signal extraction" created by monetary disturbances but the transmission depends on the (optimal) behaviour of agents

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<sup>35</sup> We can mention the literature on policy ineffectiveness within the new classical macroeconomics (Sargent and Wallace 1975, 1976), the one on credibility (Fellner 1976, 1979), the other on time inconsistency and monetary rules (Kydland and Prescott (1977) or Barro and Gordon 1983).

<sup>36</sup> For a survey of those various models, one should refer to Cooley and Hansen (1995) in Cooley (1995).

<sup>37</sup> It has been shown that a shock defined as a simple random walk cannot generate the persistence observed in fluctuations; in order to do so, one has to suppose that the shocks are AR (1).

facing anticipated or unanticipated monetary shocks<sup>38</sup>. In the RBC scenario, the reaction of the agents is affected by the temporary or permanent nature of the shocks. The problem which still remains is that the propagation mechanisms do not affect the nature of fluctuations: no amplification, no change in the dynamics, all is in the shocks.

Thus in some sense, the EBC models focused on propagation mechanisms much more than on the origins of fluctuations, but not in a very satisfying way.

The contribution by Stadler (1990) has deeply changed this view. Indeed he showed by the introduction of endogenous growth mechanism(s), that the two origins of fluctuations can be considered within the same analytical framework. Furthermore, that analytical framework has the advantage to offer a more complete and interconnected view of business cycles and also to be more compatible with recent econometrical works that have the tendency to underline the interconnection of real and monetary shocks in the long run<sup>39</sup>. One must notice here another consequence: building an analysis of fluctuations strongly based on propagation mechanisms leads to a “semi-endogenous theory” or “incomplete theory of business cycles” (Greenwald and Stiglitz, 1987) which could be the hint that debates on cycles are just continuing.

Lucas’s consecutive statements<sup>40</sup> and the widespread neglect of Lutz and other authors of the interwar period show the need for a systematic and accurate comparison of modern equilibrium theories of the business cycle with some interwar contributions that are still relevant today. The position of Lucas is not so clear. Lucas (1977 and 1980) refers intensively to the *Hayekian* project consisting in bringing together business cycles analysis and the equilibrium approach. However, in the same period he wrote:

*“If an historical approach cannot guarantee an ability to foresee the future, it does seem to me to aid in distinguishing those elements in past thinking that remain useful from those that do not. The neoclassical synthesis arose, as does all useful economics, from a compromise between what we would like to have known and what the methods at our disposal seemed to make it possible to know. Nothing*

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<sup>38</sup> A class of models is a little bit different but did not get a large success, the so-called hybrid models which appeared for the first time in Lucas (1987). The idea is that the monetary factors could be seen as key elements affecting the accumulation of capital and then could be responsible of long lasting fluctuations.

<sup>39</sup> For a survey as well as an original contribution, one can refer to Hartley and Whitt (2003) and other studies which underlines that there can be permanent demand shocks. Allowing whether demand or supply shocks of either type - temporary or permanent – to disturb the economy, they find that permanent and temporary demand shocks have been the dominant source of invariance in output growth in United States and in the five European countries they tested (France, Germany, Italy, United Kingdom and Netherlands).

<sup>40</sup> Quoting Hayek in “Understanding Business Cycles”, then minimising that reference in 1994 and finally referring intensively to Hume in the Nobel Price lecture (1996).

*could be more detrimental to the productive use of methods more recently developed than to view the categories and constructs that were produced by this compromise as constraints on the way we think about business cycles today.”*  
(Lucas (1980) reprinted in Lucas (1981) p. 292)

Certainly the problem is not to determine whether the interwar period was more interesting to understand business cycles, or to feel as constrained minds because of the way they dealt with economic issues. On the contrary, it would represent a great deficit to inquire in business-cycle theory without considering any knowledge from the past contributions. The rich economic content of some of those earlier contributions focussing on the complex interrelation between real and monetary as well as structural change factors in economic systems, confronted with the greater analytical rigour and the more elaborated techniques of the modern approaches, may be a fruitful starting point for further research.

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