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Impulses and Propagation Mechanisms in Equilibrium Business Cycles

Theories: From interwar debates to DSGE “consensus”¹

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Abstract. It is tempting to understand the DSGE (Dynamic Stochastic General Equilibrium) approach as a refinement of earlier contributions, namely Slutsky (1927) and Frisch (1933), and to a lesser extent Hayek, Hicks, and Lutz. By analyzing the debates in these periods, we try to show that the modern tools from which our theories benefit, far from being neutral, have deeply changed the nature of business cycles theories. We identify the reduced role of propagation mechanisms in DSGE models and their consequences for current debates. The overemphasis on the capacity of models to mimic cyclical fluctuations on the one side, and the clear incapacity of these models to explain (even to mimic) large scale crises on the other, are distinct but convergent elements which reveal a deep change in the relationship between the empirical findings (data, stylized facts) and the theory. These elements are deserving more attention from historians of economic thought.

Keywords: equilibrium business cycles theories, dynamics, impulse, shocks, propagation mechanism(s), empirical validation, calibration, crisis.

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1. Introduction

There is no doubt that DSGE (Dynamic Stochastic General Equilibrium) models which were considered benchmark models during the Great Moderation period, were challenged enormously when the global financial crisis developed. As business cycles models, their capacity to provide insights into the origins and mechanisms of propagation failed in the context of the crisis. This questions their validity also as a basis for economic policy advice. As a consequence, many economists are pleading for new benchmarks or for a deep reconsideration of both the theoretical and empirical sides of the arguments.² Although no new consensus has yet emerged on possible “solutions” to or reorientations of the research program in this field, many economists are trying to understand *Where modern macroeconomics went wrong*³. As historians of economic thought, we propose to retrace the evolution of business cycles theory and of its empirical practices in order to better understand the way this literature today addresses macroeconomic volatility and eventually crises.

Although endogenous and exogenous explanations of business cycles were for a long time considered to be competing views, today it is clear that the impulse/propagation scheme dominates. Under the combination of various influences, in the 1980s, a consensus had emerged about the need for “sound” microeconomic foundations⁴ of models based on equilibrium, alongside empirical studies and computational tools to allow a better understanding of the nature of shocks (Duarte and Hoover, 2012) and how they affect the economy. The fact that pioneering contributions within the impulse/propagation tradition are rooted in the interwar period, produced a temptation to interpret the DSGE approach as purely a refinement of the contributions provided by Slutsky (1927) and Frisch (1933), and to a lesser extent Hayek, Hicks and Lutz, and benefiting more recently from the technical improvements provided by the New Classical School⁵.

² See the Special Issue of the *Oxford Review of Economic Policy*, Vol. 34, Nos. 1-2, Spring and Summer 2018.

³ This is the title of one of Joseph Stiglitz’s contributions in the Special Issue of the *Oxford Review of Economic Policy*.

⁴ See King (2012) for a survey analyzing the variety of programs on microfoundations.

⁵ This means first Lucas’s contribution to business cycles theory (1972, 1973) and his methodology (1981) and later the Real Business Cycles models by Kydland and Prescott (1982) or Long and Plosser (1983).

By analyzing the debates in both periods, we try to show that there is of course similarity in the nature of the debates but that the modern tools from which our theories benefit, far from being neutral, have also deeply changed the nature of business cycles theories. More precisely, the more shocks became essential elements of business cycles theories, the more drastically the role of propagation mechanisms is reduced (e.g. Cogley and Nason, 1995). This reduced role of propagation mechanisms in DSGE models has two important consequences for current debates. First, there is an overemphasis on the capacity of the models to mimic (versus to explain) cyclical fluctuations. Second, there is a clear incapacity of these models to explain (even to mimic) large scale crises, i.e. the major crises that result from relatively small shocks (Stiglitz, 2015, 2018). Both elements, although apparently distinct, reveal a deep change in the relationship between the empirical findings (data, stylized facts) and the theory (here models) which should get more attention from historians of economic thought.

By investigating the methodological and theoretical debates that occurred between those two periods, we try to identify how the nature of these models has changed, and how this change has affected both the questions that can be addressed (or not) *via* the models and how those questions are formulated. By emphasizing the relative importance of impulse and propagation mechanisms in business cycles (equilibrium) models, we examine the status of those models, i.e. their linking of the theory with the constraints imposed by their empirical validation method(s).

The paper is organized as follows. Section 2 analyzes the debates that occurred in the interwar period comprising the variety of approaches dealing with the methods and problems related to business cycles theory. In this period there was a lack of consensus on the question whether the equilibrium method is a proper framework for business cycles analysis. Furthermore, different concepts of equilibrium existed, and it is in this context that we discuss their implications on other debates - dynamic *versus* static analysis of fluctuations, real *versus* monetary explanations of business cycles, and exogenous *versus* endogenous business cycles models – and measure the relative importance of impulses and propagation mechanisms in the explanation of business cycles. In section 3 we discuss the rational expectations hypothesis

(REH) associated to microeconomic foundations *à la* Lucas, and how it affected the way those equilibrium business cycles models consider fluctuations, *i.e.* the nature of the possible origins (shocks) and their capacity to propagate. A stream of the literature presents a systematic comparison between that first generation of Equilibrium Business Cycles (EBC) models⁶ and the equilibrium approaches in the interwar period⁷. This literature has already proved that comparisons are delicate⁸. We add to this by comparing with the latest version of the EBC models, *i.e.* DSGE models. Those models deserve attention for several reasons. First, they were at the basis of a large consensus among macroeconomists⁹ before the emergence of the most recent crisis and were considered suitable (and sufficiently simple) models for policy analysis. Second, their macroeconomic structure allowed them to handle a large set of economic issues within the same model. The canonical model combined three important elements namely i) the intertemporal optimization program (the well-identified core of real business cycle - RBC models), ii) a new Keynesian Phillips curve, *i.e.* introducing rigidities, iii) policy reaction *via* the incorporation of the Taylor rule. A strand of literature emerged which presented that core model as the new consensus or the New Synthesis¹⁰. Those models were presented as encompassing all relevant New-Classical and New-Keynesian building blocks. They were based on sound micro-foundations, built within a framework that deals jointly with business cycles and growth dynamics, appeared empirically relevant since they exhibited the capacity to mimic (reproduce) observed fluctuations, and were able to evaluate stabilization policy efficiency in conducting welfare analyses.

⁶ It is usual to distinguish among different generations of EBC models. The first, initiated by Lucas's seminal work in 1972, is known also as Monetary Equilibrium Business Cycle models. The second developed from Kydland and Prescott's (1982) paper which found unexpected empirical support from Nelson and Plosser (1982) by showing that real shocks were the only ones that could explain the permanent impact on output, *i.e.* so-called RBC (Real Business Cycles) models. Following this was a brief episode when many macroeconomists tried to analyze the possible impact of the Keynesian hypothesis (*i.e.* diverse sources of rigidities) on the explanatory power of those models (see *e.g.*, Hénin 1995, Cooley 1995). These models came to be known as Augmented RBC (ARBC) models and heavily influenced what now we consider DSGE models, the focus of this paper (see Dal Pont Legrand and Hagemann (2010) for more details on these successive EBC models).

⁷ See Scheide (1986), Hoover (1988), Arena (1994), Laidler (1986 [2001]), Dal Pont Legrand and Hagemann (2010, 2013).

⁸ Hoover (1988) and Arena (1994) have shown that Lucas's model is very far from Hayek's theoretical project and Dal Pont Legrand and Hagemann (2010) have documented deep differences in the debate confronting real versus monetary business cycles models in the two periods, *i.e.* the interwar period opposing Hicks *versus* Hayek and the more recent one opposing Lucas to the first RBC models.

⁹ See de Vroey (2016) p. 309 and more specifically pp. 325-327 for detailed developments explaining how the two sides had to abandon several elements.

¹⁰ See Woodford (2009) and Blanchard (2009).

Today the consensus has eroded. Since the crisis, not only have their proponents been forced to reevaluate what they considered as largely theoretically accepted¹¹ and empirically proved but also their opponents have seized the opportunity for stronger criticism. The main weakness identified is their incapacity to explain how large (deep and long-lasting) fluctuations can be generated by relatively small(er) shocks¹². This is due to the lack of capacity of those models to explain propagation, and more specifically the amplification of the initial shock. We show how models that overemphasize the role of shocks are more easily calibrated but lose some capacity to explain. This is due to their ability to mimic, which characteristically deeply affects their relationship to the empirical facts (or data).

2. Reflections on the Interwar Debates

Business cycles was a dominant theme in theoretical and empirical research conducted in the interwar period. At the origin of intense and heated debates was the central question of the (in) compatibility of the business cycle explanation with the dominant general equilibrium approach in economics. Economists who believed strongly in the need to construct a business cycles theory within the existing general equilibrium framework were opposed by those who were convinced that a ‘realistic’ dynamic theory could only be built upon an out-of-equilibrium concept. In the opening passage to ‘Understanding Business Cycles’, one of his mostly read contributions, Lucas (1977, p. 7) quotes 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, p. 33n).

One of the most influential authors in the interwar period was Joseph Schumpeter (1883-1950) whose monumental *Business Cycles* opened with the statement: “Analyzing business cycles means neither more nor less than analyzing the economic process of the capitalist era” (Schumpeter 1939: V). From the outset, it was central to Schumpeter’s thinking that economic

¹¹ See Blanchard (2018) in Vines and Wills (2018) stressing that those (DSGE) models “must be improved rather than discarded” and continuously recognizing that “(...) we need different types of macroeconomic models for different purposes” (2018: 43).

¹² See Stiglitz (2015).

progress occurs in waves, that crises are turning-points necessary for the re-equilibration process, and that a *Theory of Economic Development* (1911) had to be constructed as a theory of business cycles. Schumpeter's theoretical system is based on the fundamental distinction between statics and dynamics. Whereas his Vienna habilitation thesis on *The Nature and the Main Content of Theoretical Economics* (Schumpeter 1908) inspired by his great hero Walras, focuses on the pure logic of a general equilibrium system, his *Theory of Economic Development* deals with dynamic analysis and was inspired by the challenge posed by Marx's vision of the long-run evolution of the capitalist economy which emphasized capital accumulation and technical progress.

In the mid-1920s, Schumpeter's theory had become a central reference point for the more theoretically-minded economists in the German language area. Thus, Adolf Löwe (1893-1995) clearly was inspired by Schumpeter's view that a Walrasian system of general economic equilibrium was inappropriate for business-cycle theory, when he made his claim to the necessary abandonment of static equilibrium theory in the analysis of business cycles which was made explicit in his Kiel habilitation thesis 'How is business-cycle theory possible at all?' (Löwe [1926] 1997). In this "brilliant article" (Kuznets (1930b: 128), Löwe highlighted the fundamental conflict between the equilibrium approach as the dominant method of economic theory and the subject of inquiry, i.e. cyclical fluctuations of the economy which have a genuine out-of-equilibrium character. In his critical investigation of the existing body of business-cycle theories Löwe concluded that all serious approaches either introduce exogenous factors such as wars or bad harvests, or fully or partly abandon the interdependency requirements of a system of general economic equilibrium. He concluded therefore that:

The business cycle problem... 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 [1926] 1997: 267).

If economic theory is to explain the business cycle satisfactorily, 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. In the era of progressive industrialization Löwe identified technological change as this decisive endogenous factor generating the business cycle.

As Hansjoerg Klausinger, the editor of the two volumes on *Business Cycles* of the Collected Works of F.A. Hayek pointed out, “the basic tenet for interpreting Hayek’s writings on money and the cycle in the interwar period is their firm foundation on an equilibrium approach, which served as the benchmark to which cyclical movements are to be related” (Klausinger 2012 I: 12). The challenge arising from Löwe’s attack against the traditional concept of a static equilibrium and his plea for an alternative ‘dynamic’ system approach to explain cyclical fluctuations, posed a major issue to Hayek which is reflected best in chapter I ‘The Problem of the Trade Cycle’ of his *Monetary Theory and the Trade Cycle*.

While Hayek agreed with Löwe’s identification of the incorporation of cyclical phenomena in equilibrium theory as the crucial problem of business cycle theory, the two differed fundamentally in the conclusions drawn from their methodological reflections.¹³ This applies particularly to the role of the concept of equilibrium. In contrast to Löwe, Hayek adhered to this idea as indispensable to economic theory in general, and to an understanding of intertemporal price relationships in particular. Hayek considered the field of application of equilibrium theory as “identical with that of economic theory, since only with its assistance is it possible to give a summary depiction of the very great number of different tendencies of movement which are operative in every economic system at every point in time” (Hayek [1928] 1984, p. 73). Although Hayek understood that the economy was in a state of disequilibrium during an adjustment process, he considered it essential to start the explanation of cyclical fluctuations from an assumption of an economy in equilibrium with full utilization of resources. Thus Hayek was at issue with Wesley C. Mitchell who had pointed out in his *Business Cycles. The Problem and Its Setting* that “it is no part of my task to determine how the fact of cyclical oscillations in economic activity can be reconciled with the general theory of equilibrium or

¹³ For a more detailed comparative analysis see Hagemann (1994). When Lucas presented his “Understanding Business Cycles” (Lucas 1977) at the Kiel Conference on *Growth without Inflation* in June 1976 he seemingly was unaware that in his Vienna habilitation thesis Hayek had reacted to Löwe’s habilitation thesis (1926) which qualified Löwe to become the founding Director of the Department of Business Cycles at the Kiel Institute of World Economics where the 1976 conference took place.

how that theory can be reconciled with facts” (Mitchell 1927, p. 462). Challenging Mitchell, Hayek underlined his “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, p. 34).

Now what is Hayek’s concept of equilibrium? He certainly considered it important to include in the notion of equilibrium the element of time to analyze dynamic questions and to regard differences in the prices of the same goods at different points in time. There is a widespread view that Hayek’s concept of equilibrium originates from Walras as mediated in the German language literature in particular in the works of Schumpeter (1908) and Cassel.¹⁴ Walras’s influence on Hayek is identified usually in the note in *Monetary Theory and the Trade Cycle* in which Hayek states explicitly that “[b]y ‘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, p. 42).¹⁵

The causes of the cycle, or the dynamic impulses can be real as well as monetary. In contrast to Löwe but also Wicksell, Schumpeter and Hicks¹⁶ who emphasized the role of technical progress, Hayek considered cyclical fluctuations to be caused by monetary factors. While in *Monetary Theory and the Trade Cycle* Hayek’s focus is on the monetary factors promoting the cycle, in his subsequent *Prices and Production* lectures the emphasis is on the real structure of production. Agreeing with Löwe’s emphasis on the importance of production structures, Hayek deviated by employing a vertical rather than a horizontal or sectoral disaggregation of production structures, stimulated by Böhm-Bawerk’s Austrian theory of capital stressing the time dimension of the production process as in the famous triangles. Although Hayek’s theory essentially is a monetary (overinvestment) theory, he felt obliged to separate his approach from all monetary trade cycle theories which proceed from changes in the general level of

¹⁴ It is debatable whether Hayek really had a Walrasian notion of equilibrium in mind. See Arena (1994, p. 211) who points out that “[t]here is little common room between Walras’s *tâtonnement* and Hayek’s market discovery”.

¹⁵ However, this interpretation needs to be qualified due to Hayek’s further reference to James Mill and Say, as well as the fact that this note was not included in his original German text *Geldtheorie und Konjunkturtheorie*.

¹⁶ Hicks who considered technological change as more fundamental, had a life-long controversy with Hayek for whom monetary disorders were of first importance. See Hagemann (1998).

prices. In contrast, Cantillon effects of changes in the money supply on the structure of relative prices, and hence, on the structure of production i.e. non-neutrality of money in the short and medium run, and Ricardo effects of a shortage of consumption goods on the production of investment goods, play a decisive role in his business cycle theory.¹⁷ Thus, while monetary factors, in particular excessive credit creation by the banking system, cause the cycle, they are not just the impulse but also are part of the propagation mechanism. The distortion of the structure of relative prices leads to consequential disproportionalities in the structure of production i.e. a misallocation of resources which sooner or later must be corrected.

Hayek perceived the introduction of money as the proper starting point for a satisfactory theory of the business cycle and as overcoming the methodological dilemma highlighted by Löwe because money “does away with the rigid interdependence and self-sufficiency of the ‘closed’ system of equilibrium and makes possible movements which would be excluded from the latter” (Hayek 1933, pp. 44-45). According to Hayek, a Walrasian system of general economic equilibrium alone would be insufficient to explain cyclical fluctuations, because

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 data – and that the economic system always reacts to such changes by its well-known methods of adaption, i.e. by the formation of a new equilibrium. (Ibid: 42-43)

What Hayek regarded as a weakness, Friedrich Lutz (1901-1975) considered to be a strength. Lutz concluded the German language debate on the (in)compatibility of business cycle theory with the theory of general economic equilibrium before the Nazis’ rise to power with his Freiburg habilitation *The Problem of Business Cycles in Economics* (Lutz 1932).

Lutz’s position can be summarized as follows:¹⁸

1. Analysis of the effects of changes in the data which is the task of business-cycle theory, could be done within the existing equilibrium theory framework by *the use of the variation method*. There would be no need for a ‘new dynamic’ theory.

¹⁷ See Hagemann and Trautwein (1998) for a more detailed analysis of Cantillon and Ricardo effects in Hayek’s theory.

¹⁸ For a more detailed analysis of Lutz’s contribution see Dal Pont Legrand and Hagemann (2013). All original emphases.

2. *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.
3. *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.

Lutz also deals intensively with Löwe’s analysis of the (in)compatibility of the business cycle problem and the equilibrium method, and Löwe’s claim to a new dynamic theory (see Lutz 1932: 114-122, 2002: 202-209). He confirms that Löwe clearly had seen and precisely identified the problems that cyclical fluctuations cause for static theory but that he had proposed a solution, i.e. his demand for a new dynamic theory, which was impossible. According to Lutz,

the idea of equilibrium is the result of thinking through processes of economic change, it is the product of the recognition that the economy tends towards equilibrium... 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 puts 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]2002: 187-188 n.2)

The introduction of technical progress favored by Löwe is nothing more than a change in the data which would lead to a new equilibrium *“In any case, what must be stressed is that since ‘independent variables’ only represent changes in data, introducing them can neither help show a way out of the static system nor resolve the difficulties posed by the problem of business cycles”* (Ibid: 205).

Following the same reasoning, Lutz criticized Kuznets’s (1930a) accusation that static theory overlooked the different reaction speeds. Kuznets regarded “the equilibrium approach... to be a blind alley from the point of view of business-cycle theory” (Kuznets 1930a, 399), and even more radical than Löwe in his conclusions, stated that “...the practice of treating change as a deviation from an imaginary picture of a rigid equilibrium system must be abandoned” (ibid: 415).

Lutz concedes only that equilibrium theory particularly in its Lausanne school blend, which does not include the time element, does not sufficiently emphasize adjustment processes, while static theory also includes “investigation of how equilibrium comes about, i.e. the movement towards equilibrium following a change in data” (Lutz [1932] 2002:208). However, the introduction of the time element does not rule out general interdependence.

Time and again, Lutz states that there is no general paradigm or ‘nature’ of business cycles, and that therefore they cannot be squeezed into a unifying scheme. An endogenous inevitability of business cycles is incompatible with modern economic theory which is based on the central idea that the economy tends towards equilibrium¹⁹. To ascribe the periodicity of the business cycle to internal economic forces would imply abandoning the equilibrium concept. Lutz was not willing to do this.

The arguments developed by Eugen Slutsky in his 1927 article 'The Summation of Random Causes as the Source of Cyclic Processes'²⁰ deeply changed the debate traditionally opposing exogenous versus endogenous business cycles models. Slutsky's contribution 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 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 can be the source of cyclic or undulatory processes which show an approximate regularity of the waves. Like Schumpeter, he 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). Like Lutz, Slutsky was convinced that cyclical fluctuations never exactly repeat earlier ones in either duration or in amplitude. Nevertheless, he considered it possible to identify specific approximate uniformities and regularities in empirical investigations. Having in 1926 joined the

¹⁹ Hayek, in a lecture delivered in Copenhagen in December 1933, switched explicitly from his earlier view “that the theory of the trade cycle at which we were aiming ought to be organically superimposed upon the existing theory of equilibrium” to Lutz's view that “our task is not to construct a separate theory of the trade cycle... but rather a development of those sections of general theory which we need in the analysis of particular cycles- which often differ from one another very considerably” (Hayek 1939, pp. 137-8).

²⁰ The following passages are based on Hagemann (2002).

research staff of the Conjecture Institute directed by Kondratieff, Slutsky was interested in empirical validation of business-cycle theories. Slutsky's idea that random shocks that were not themselves cyclical in nature could generate regular oscillations of the economic variables was important and seminal. It not only stimulated 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 and this way, affected the traditional divide between endogenous and exogenous business cycles explanations.

Simon Kuznets was one of the few theorists who immediately recognized the importance of Slutsky's contribution. Shortly after publication of Slutsky's original paper in Russian Kuznets wrote his 'Random Events and Cyclical Oscillations' article 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. He combines three ideas in an innovative way, namely Slutsky's thesis with Paul Rosenstein-Rodan's (1929) emphasis on the importance of time differences and Löwe'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 dynamic processes such as cyclical fluctuations.

Kuznets makes it clear that it is only the concept of a too static equilibrium approach that should be discarded not the relations of interdependence involved. It should be supplemented with a much stronger time element component. Thus, for the constructive part of his argument he aims at a synthesis of Slutsky's and Rosenstein-Rodan's ideas which made him aware of the importance of different speeds of response to a stimulus in different industries. Time coefficient differences may arise not only because of differences in timing of reactions but also because of their disproportionality which although not in itself a cause of disequilibrium, may aggravate the effects of time differences. 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" (Kuznets 1930a: 411). However, Kuznets whose research interests shortly afterwards shifted to analysis of long-term trends and economic growth processes, was already aware of two complications 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 some time the trend component will affect business people's behavior. Thus, relatively small disturbances may cause rather important disproportionalities and may result in prolonged oscillations of formidable magnitude.

In the same year Erik Lundberg (1907-1987) wrote his licentiate thesis 'On the Concept of Economic Equilibrium'(1930). The core of the essay consists of an assessment of the equilibrium method for a theoretical analysis of economic change.²¹ Although he does not refer directly to Slutsky's contribution which at that time was available only in the Russian original, Lundberg had completely grasped the essentials of Slutsky's argument, pointing 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 [1930] 1995, 30).

Lundberg's analysis also benefited from the knowledge of 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 first was only published in Norwegian in a Danish journal (Frisch 1929). Frisch's precise distinction between static and dynamic analysis implies that a genuine dynamic analysis contains at least one economic variable which is related to different points in 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 [1929] 1992: 394). From this it follows that Frisch thought a Walrasian system of general economic equilibrium inappropriate for business-cycle theory.

²¹ For the importance of Lundberg's early contribution see Henriksson (1996).

Following Frisch, Lundberg was concerned with the static method which is essential to all partial and general equilibrium theories. He also saw a distinguishing element as decisive in the fact that the static method disregards that supply and demand 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 includes a precise distinction between two pairs of concepts: static versus dynamic, and stationary versus evolutionary states. The main aim is to elaborate the hidden premises and the limitation to 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 [1930], 1995, p. 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.

Ragnar Frisch (1895–1973) embarked on his business-cycles analysis in the early 1930s. In his famous contribution to the Cassel Festschrift, "Propagation Problems and Impulse Problems in Dynamic Economics" Frisch (1933)²² distinguishes between two fundamental problems in the analysis of cyclical fluctuations: the propagation problem and the impulse problem, elaborating an idea which he attributes to Wicksell (1907). Frisch (p. 198) also refers explicitly to Slutsky's idea that erratic shocks may cause more or less regular cyclical movements but his contribution to business cycles analysis is of a different nature. Frisch develops a sharp and fruitful distinction between exogenous random disturbances (exterior disturbances, called also impulses), and the intrinsic structure or propagation mechanism by which the economy transforms them into cyclical fluctuations. This distinction has proved seminal to modern business-cycle theory. Frisch assumes the economy to be dynamically stable so that the intrinsic structure is dampening the oscillation caused by a single shock. However, shocks occur quite frequently so the economy keeps on fluctuating. While the amplitude of the cyclical swings is determined mainly by the strength of the exogenous impulse, the propagation

²² For a detailed analysis of Frisch's development and his contributions to business-cycle analysis in the interwar period see Andvig (1981).

mechanism accounts for the regularity of alternating movements of expansion and contraction including the length of the cycles.

Whereas in his 1933 essay Frisch distinguishes between three cycles of various lengths, he later believed firmly in the Kondratieff cycle²³ which can be caused by his detailed exchange of ideas with Schumpeter on the role of innovations as key to maintaining oscillations.²⁴ Methodologically, Frisch tried to bridge the gap which existed in the late 1920s between empirical research on business cycles such as that conducted by Mitchell favoring the inductive method, and purely theoretical research which did not even aim at sound empirical analysis using modern statistical techniques and making their approaches operational²⁵. It is then natural that he was involved in the debates around Tinbergen's 1938 report. Tinbergen was commissioned by the League of Nations to implement statistical tests of business cycles theories and Frisch (1938)²⁶ discussed the method in a paper²⁷. This debate reveals an important point. Frisch and Tinbergen both shared the view that "the explanation of business cycle phenomena can be summarized as follows: a phenomenon is explained when its endogenous mechanism is given. That is the phenomenon should be explained by the mechanism itself and not by external phenomena" (Boumans 1995: 147). However, Frisch showed that using difference equations (instead of differential equations) does not allow to construct a model with such an objective. Indeed, "a necessary condition for identification of difference equations is the presence of exogenous variables" (ibid: 147). The constraints associated to the identification had a deep influence on modeling choices. Boumans concludes that the progressive abandonment of models based on both component cycles and difference but also differential equations for models based on stochastic variables and linear difference equations led to the abandonment of the endogenous mechanistic explanation of fluctuations. More than that, in the light of this debate, one can easily recognize the weight Frisch granted to the propagation mechanism. This is clearly distinct from Slutsky. As Lines (1990) showed,

²³ See Andvig (1981:708).

²⁴ See the final section 6 of his 1933 essay.

²⁵ The latter attitude was quite common, in particular among monetary theorists of business cycles, including leading representatives of schools as diverse as the Cambridge and Austrian ones.

²⁶ Frisch's 1938 paper is considered by many historians of economic thought (Morgan 1990) but also by Koopmans, Rubin and Leipnik (1950) as a seminal contribution to the problem of identification (see Boumans 1995: 130).

²⁷ For a detailed analysis of the importance of these econometric developments and their influence on business cycle theories, see Boumans (1995).

Frisch considers shocks as a necessary reinvigoration element of the oscillatory system based on lags, accelerator and limiting factors, so a way to maintain the swing²⁸. Then Frisch was superimposing random disturbances on deterministic oscillations. Slutsky held a different view. Considering fluctuations as *the summation of random causes*, he studied under which conditions random factors can be transformed into a regular wave (cyclical) movement. To this respect “his work is familiar to us today as an analysis of linear filtering” (ibid: 360). He then exhibited autocorrelated stochastic fluctuations. So random disturbances clearly do not have the same status in both kinds of models.

In the earlier attempts to provide business cycles models, the status of shocks can depend ambiguously on the nature of the mathematical tools involved, and the choice between these tools is closely related to the constraints imposed by the identification method. It is only the beginning of the story. Duarte and Hoover (2012) have documented how, from the early 1970s, the status of shocks evolved from observed data to inferable phenomena. It is the objective of the next section to examine symmetrically, how propagation mechanisms were progressively discarded, shocks being considered as key-elements to understand business cycles.

3. DGSE: rise and ... fall?

Although economists never ceased to work on business cycles theory, it came back center stage only when Lucas started to look for some new foundations which could explain the emerging puzzling relationship between inflation and unemployment, i.e. Stagflation. It is well-known today that the main influence of Lucas’s 1972 paper on macroeconomics research lies much more in his methodological contribution than in his business cycles model²⁹, although it was the first successful attempt to model monetary equilibrium business cycles. Under the hypotheses of rational expectations and continuous market clearing, fluctuations are

²⁸ See Lines (1990: 370).

²⁹ We do not focus here on the core hypotheses of the Lucas 1972 model but more on his methodology since they are not of fundamental importance for the development of business cycles models. The important elements are those finally selected by the RBC and then DSGE models.

considered as the manifestation of changes of equilibrium positions³⁰. Nevertheless, the business cycles theory suffered from other important weaknesses. First, the model did not allow persistency in output dynamics to be exhibited. Second (and consequently), in order to generate fluctuations with the characteristics of the observed output movements, there must be an assumption of i) the occurrence of regular unexpected monetary shocks, and ii) the admission that agents are never well informed about monetary policy, a highly questionable point already in the 1970s. Tobin (1980), certainly unintentionally, delivered one of the cleverest analyses, offering justifications for the forthcoming RBC counter- proposition arguing that it would have been much more convincing to provide a model based on a shift in productivity. What changed fundamentally was that as soon as the rational expectations hypothesis (REH) became a core element of the new modeling strategy of macroeconomists, shocks ceased to have a secondary role, and instead became central elements of business cycles analysis. First, systematic use of REH, especially when associated to a perfect information hypothesis, prevented analysis of fluctuations which “previously” could have been generated by the discrepancy between expected and realized variables. The idea of rational expectations implies also a stochastic characterization of the agents’ environment.

(...) viewing a commodity as a function of stochastically determined shocks... in situation in which information differs in various ways among traders... permits one to use economic theory to make precise what one means by information, and to determine how it is valued economically. (Lucas 1981: 707)

³⁰ The notion of equilibrium is here quite different from the one used in the interwar period. For many economists of that period (except for economists such as Lowe or Kuznets), equilibrium and disequilibrium were encapsulated concepts. This is not anymore the case for Lucas’s REH equilibrium (see de Vroey 2016: 176-186). As David Laidler emphasized, “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” (1986: 349). For more details on the building process of the new classical school, see Dal Pont (1999) or de Vroey (2016).

More than that, as emphasized by Duarte and Hoover (2012), shocks progressively became modelers' instruments to introduce a (sort of) manageable instability. In selecting the stochastic properties of the shocks, the modeler can capture / characterize different artificial economies.

In 1982, Kydland and Prescott published their pioneering model "Time to build and aggregate fluctuations" but in the same year, a more "empirical" paper was of fundamental importance: the contribution by Nelson and Plosser. This paper provided an empirical justification for Kydland and Prescott's RBC model, giving support to the fact that only real shocks can have persistent effects. However, there was some opposition to that vision. This result was challenged sharply by Stadler (1990) who demonstrated that the "empirical" findings of Nelson and Plosser were in fact deeply rooted (only!) in their model. Endogenizing technology, the model provides a (powerful) propagation mechanism to shocks and reveals that monetary shocks also can have permanent effects. The second criticism was an internal one. In 1995, Cogley and Nason aimed at investigating whether the RBC models were consistent with the stylized facts. Surveying an already vast literature, they first replicated time-series evidence, and then discussed the capacity of different RBC models to exhibit this empirical evidence. Their conclusions were straightforward: RBC models relied too heavily on exogenous shocks in order to replicate those stylized facts: "in models that rely on intertemporal substitution, capital accumulation and costs of adjusting the capital stock, output dynamics are nearly the same as impulse dynamics" (ibid. p. 509). Their final conclusion was that the RBC research program should consist of devoting "further attention to modeling **internal** source of propagation" (our bold letters, ibid. p. 509). The problem was then clearly identified by several

economists but is evidence also that the concept of propagation had been radically transformed: now, the main source of propagation of their model is **external** (i.e. due to the shocks' stochastic properties).

Business cycles models experienced a double evolution: the more they relied on shocks to reproduce the observed fluctuations, the more they overlooked endogenous propagation mechanisms. This modeling strategy had a profound effect on the questions addressed by macroeconomic models, preventing the analysis of numerous coordination issues which were dominating the debates during the interwar period. As Romer (2016:4) emphasizes: "Macroeconomists got comfortable with the idea that fluctuations in macroeconomic aggregates are caused by imaginary shocks, instead of actions that people take, after Kydland and Prescott (1982) launched the real business cycles (RBC) model." It should be noted that despite critiques of this empirical approach, the practical dimension of RBC became not only central to academic research strategy but was characterized by convergence to a unique potential method; the measurement of the co-movements among aggregate variables. As Zarnowitz (1985: 524) identifies with precision, "it is the 'stylized facts' which it provides that ought to be explained by the theory".

The transition from RBC to DSGE models, and the Keynesian influence on that process benefited from the attention given by historians of economic thought³¹. We can identify two phases of this "convergence" process. The first consists of the introduction of frictions in RBC models, the so-called ARBC (Augmented Real Business Cycles). Research along these lines was

³¹ See Duarte (2015).

described as ‘Keynesian’ until 2003 when Woodford published his important *Interest and Prices*. This was the origin of a New Synthesis which combined three important elements: i) the intertemporal optimization program (the core of RBC models), ii) a New Keynesian Phillips curve, i.e. introducing rigidities, and iii) policy reaction via integration of the Taylor rule. Those models, i.e. DSGE models, were presented as encompassing all relevant New Classical and New Keynesian building blocks. This New Synthesis emerged at a moment identified by both New Keynesians (Bernanke) and New Classical economists (Lucas) as the “Great moderation”, a period characterized by decreasing volatility of the macroeconomic variables attributed to successful economic policies, driven by our modern tools (DSGE models). These models were not only largely adopted by the academic community but also quickly diffused through more institutional networks such as the Central Banks’ economic departments. Although debate continued, it involved economists who followed different lines of research.

The direct consequence of this consensus was that the DSGE model was used to address various economic issues. This was an important change to the discipline compared to the diversity of approaches which characterized the interwar period. It should be recalled that numerous economists from Keynes to Solow and Leijonhufvud, considered that a large part of economists’ expertise consisted not of building models but of the capacity to select the most appropriate one for specific situations. Thus, Solow argues:

I cannot say the same about the use made of the intertemporally- optimizing representative agent. Maybe I reveal myself merely as old-fashioned, but I see no redeeming social value in using this construction, which Ramsey intended as a representation of the decision-making of an idealized policy - maker, as if it were a descriptive model of an industrial capitalist economy. It adds little or nothing to the story anyway, while encumbering it with unnecessary implausibilities and complexities. (Solow 1994:49)

Convergence towards a single model was accompanied by acceptance of new criteria determining “the” good model. Clearly for supporters of the DSGE approach, a good model was a workable model which behaves according to the observed data. The plausibility of the hypotheses cannot depend on reasonable criteria. In line with Friedman’s 1953 statement, “the more significant the theory, the more unrealistic the assumptions” (p. 14), the DSGE proponents avoid discussion of the realism of hypotheses and consider that ultimately all models are false and their “reliability” should be evaluated not by considering each equation independently but by considering the behavior of the system (of equations). So, DSGE economists judge a model as “useful” as soon as it can replicate observed fluctuations, i.e. as soon as it produces data with the same stochastic properties as the observed data. More specifically, the typical test of a RBC model once calibrated, consists of checking “whether simulations of the model with reasonable disturbances can reproduce a few of the low moments of observed time series: ratios of variances or correlation coefficients, for instance” (Solow 2008: 245). Today, with DSGE models, empirical validation has evolved. First, the number of autoregressive shocks involved in the models has increased to be equal to the number of forward variables. Second, “identification” of parameters (instead of only calibration) is now the main method³². This means that the modelers calculate Bayesian estimates, a method very often (almost systematically) presented as an improvement compared to the calibration method, although as shown (among others) by Romer (2016: 6-7), the identification method is possible only if the DSGE econometrician introduces prior(s):

The identification problem means that to get results, an econometrician has to feed in something other than data on the variables in the simultaneous system. (...) The current practice in DSGE econometrics is feed in some FWUTV’s (facts with unknown truth value) by “calibrating” the values of some parameters and to feed in others tight Bayesian Priors. As Olivier Blanchard (2016) observes with his typical

³² See Sergi (2017) for debates on empirical validation methods with the DSGE approach.

understatement, 'in many cases, the justification of the tight prior is weak at best, and what is estimated reflects more the prior of the researcher than the likelihood function'.

As Romer emphasizes, since such priors have a direct impact on the model conclusions, one cannot exclude that the modeler finally selects the priors for parameters that might seem unimportant but which will produce the expected result for the parameter of interest. This point is investigated in detail by Chatelain and Ralf (2018). Since Lucas (1976), microfoundations are "empirically justified by the endogeneity of macroeconomic policy instruments leading to parameter identification problems" (Ibid:3). More than that they emphasize that often microfoundations imply too many structural parameters so that we are in a situation of under-identification of the statistical model with the direct consequence that a "useful normative macroeconomic theory can be a useless positive macroeconomic theory" (ibid). Indeed, observationally equivalent models can offer different predictions about policy Interventions. More than that, and perhaps surprisingly, the increasing reliance to empirical validation in order to measure the relevance of a theoretical contribution was done in a way which allowed these models to avoid falsification method and then, to disentangle theory's diffusion from its capacity to settle clear empirical facts.-It is surprising to note that-this point is not addressed more often in discussions of the relative merits of competing models. The existence of such a blind spot and especially the quasi-absence of debates on its consequences are puzzling.

Due to the recent crisis, already existing critics got a greater resonance. In 1996 Hansen and Heckman (p.87) noted "(...) that as a paradigm for organizing and synthesizing economic data, it (General Equilibrium Theory) poses some arduous challenges". Simulations are not new, Tinbergen proceeded in this way, and Frisch and Klein refer to a few. The point is not about the

usefulness of simulations but rather that the quality of the simulation is completely dependent on the quality of the data. So perhaps macroeconomists should be more aware concerning the trade-off between ambitious *versus* reliable models.

With the increasing role of shocks in business cycles models and the emphasis put on models' capacity to replicate (*versus* to explain) observed fluctuations, it became necessary for modelers to rely on "external" propagation mechanisms (the system being described as intrinsically stable) which can be controlled. This is achieved using the identification method.

The direct counterpart is that macroeconomists, whatever their response to critics such as Solow, must admit that they ceased to dedicate serious effort to improving the theoretical roots of their models. This can be partly explained by the feeling of confidence during the Great Moderation period: the main economic indicators (at least those on which they tended to focus) seemed to confirm the performance of their economic policy guidance.

However, on many occasions (and even before the crisis), economists were questioning their micro-foundations and could not understand why more attention was not being paid to the transition from micro to macro in the RBC literature. Indeed,

(...) microeconomic estimates routinely incorporate heterogeneity that is often abstracted from the specification of dynamic, stochastic general equilibrium models (...), given that understanding the distribution of heterogeneity is central to making this transition (...)" (Hansen and Heckman 1996: 101).

This argument echoes that one already developed by Cogley and Nason (1995) when they emphasized the need for an internal propagation mechanism. As Solow (2008: 246) perfectly

summarizes, questions about theoretical foundations and empirical validation methods are strongly linked since modelers, if they are not careful, can be constrained:

My general preference is for small, transparent, tailored models, often partial equilibrium, usually aimed at understanding some little piece of the (macro-) economic mechanism. I would also be for broadening the kinds of data that are eligible for use in estimation and testing. One of the advantages of this alternative style of research is that it should be easier to accommodate relevant empirical regularities derived from behavioral economics as they become established.

What did the crisis reveal that we did not know before? Possibly, not much. However, it perhaps helped macroeconomists to define better where they want to go. The fact that DSGE cannot predict crises is one point but may not be a sufficient argument on its own to dismiss those contributions. The fact that they cannot provide, even *ex post*, convincing explanations of what happened, or economic policy advice, may be a bigger flaw.

Ten years after the crisis various special issues have been published. Some authors urge a deep renewal of macroeconomic theory³³ while others fight to save (part of) their contributions³⁴. It is not our object here to examine the relative truths of their respective arguments but we must agree with Stiglitz that we need a business cycles theory which can explain deep downturns , and in the absence of proper internal (and micro- based) propagation mechanisms, we can have reasonable doubts about the capacity of the current DSGE models to produce such an explanation. Indeed, DSGE modeling choices are under a twofold influence namely, a strict definition for acceptable microfoundations and restrictions dictated by empirical validation methods. Such an acknowledgment should push macroeconomists to consider how to offer

³³ See the special issue *Rebuilding Macroeconomic Theory* edited by Vines and Wills (2018)

³⁴ See the last symposia "Macroeconomics a Decade after the great Recession" in the *Journal of Economic Perspectives*, 32:3, summer 2018.

models with perhaps more modest but certainly more robust empirical results and perhaps also how to introduce again diversity in macroeconomic modelling.

4. Concluding Remarks

Duarte and Hoover (2012) provide a history of shocks analysis in macroeconomics, underlining how the nature and the status of the shocks involved in business cycles analysis changed progressively. Open questions still were why macroeconomists started invoking shocks as “imaginary driving forces” (Romer 2016) in order to produce business cycles analysis, why they did it to such an extent that it led to total neglect of internal propagation mechanisms, and to what extent this progressive shift affected the research program of business cycle analysis?

We believe that the investigation of the two periods draw attention to some missing links. It explains first how propagation mechanisms were progressively overlooked but it also points out that such a view (i.e. based on external propagation mechanisms) was already present in Slutsky’s pioneering contribution. With no doubt the nature of the models has so much evolved that no strict comparison would be useful. However, it is interesting to note that despite technical progresses, macroeconomists are today still facing a trade-off between theoretically satisfying and empirically testable models.

This short episode perhaps reveals that it might be time for historians of economic thought to emphasize in the “stories” they tell, how academics decide to focus on one sort of model compared to others, i.e. what are their theoretical and empirical criteria, and how those criteria evolved. This remains part of the history of modern macroeconomics which is not often carefully documented by historians of economic thought.

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