Business Cycle Theory: Selected Texts 1860-1939

Equilibrium and the Business Cycle

Edited by
Harald Hagemann

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INTRODUCTION

In the 1970s, after the dominance of growth theory for more than two decades, business cycle theory took centre stage again and the assumption of continuous market clearing (as a methodological principle) in the new classical paradigm and real business cycle models then became an issue of heated controvery. For some adherents, such as Kydland and Prescott (1982), economic fluctuations are a purely equilibrium phenomenon that can be attributed to real disturbances. In the opening passage to 'Understanding Business Cycles', one of his most widely read contributions, Lucas asks the question 'Why is it that, in capitalist economies, aggregate variables undergo repeated fluctuations about trend, all of essentially the same character?'(Lucas 1977, p. 7). He continues by quoting 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'. This clearly refers to the central question of the incompatibility 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. Although a closer look into the quotation from Hayek shows that the latter refers explicitly 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' (Hayek 1933, p. 33n.). We may identify Lucas's research programme of equilibrium business cycle theory as another attempt to escape the fundamental methodological dilemma which had been so clearly stated by Löwe half a century before. Volume IV of this collection therefore starts with Löwe's groundbreaking paper which has only recently been translated into English.

Adolf Löwe (1893–1995) was Head of the International Division of the German Bureau of Statistics from 1924 to 1926 and a close collaborator of its Director, Ernst Wagemann, who founded the Deutsches Institut für Konjunkturforschung in Berlin in 1925 – known as *DIW* today – the first German institute for business cycle research. In 1926 Löwe accepted an offer by Bernhard Harms, the Director of the Kiel Institute of World Economics, to become Director of Research of the newly founded department of statistical international economics and international trade cycles. Within a short period he managed to bring together a group of extremely talented young economists, including Gerhard Colm, Hans Neisser, Fritz (later

Frank) Burchardt and Alfred Kähler. For a period of time Wassily Leontief (1927–8 and 1930–1) and Jacob Marschak (1928–30) were also members of this scientific community. The research work of the Kiel group was supported financially by the Rockefeller Foundation as well as the business cycle research at the institutes in Berlin and Vienna, or at the universities of Bonn (Spiethoff), Oslo (Frisch), Rotterdam (Tinbergen) and Stockholm (Ohlin).[1] With his earlier contribution to the Brentano Festschrift 'The Current State of Research on Business Cycles in Germany' (1925), in which he gave a critical survey of the theoretical and empirical literature, and particularly his Kiel habilitation thesis 'How is Business Cycle Theory Possible at All?' (1926), Löwe established himself as a central figure of the debates on business cycle theory in Weimar Germany. This is particularly remarkable since in those years he never developed his own theory of cyclical fluctuations. However, his thorough analysis of the methodological requirements of a business cycle theory had a significant impact on the subsequent debates on business cycle theory in the German language area[2] and, to a lesser extent, internationally.[3]

In his 'brilliant article' (Kuznets 1930b, p. 128) Löwe 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 emphasised that all economic theories since the time of 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, p. 251). According to Löwe, 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 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' (Löwe 1997, p. 250).

However, although the variation method is capable of explaining all the problems following the interactions and interdependencies in a circular flow economy, it is incapable of explaining an economic history of cyclical fluctuations for more than a century. His analysis of the existing business cycle theories comes to the result that all serious approaches have either abandoned, at least partly, the interdependency requirements of the economic system, or introduced exogenous factors. Furthermore, he stated the following problem: if economic theory is to explain the business cycle satisfactorily, it cannot do so simply by outlining the consequences of a

1 See Craver (1986).

2 For a detailed survey on contemporary reactions to Löwe's 1926 article see Gehrke (1997, pp. 238–42).

3 See, for example, the two articles by Kuznets (1930a, b).

disturbing factor exogenously imposed upon an otherwise static economy; rather, it must seek a causal factor that is endogenous to the system itself in order to 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, p. 267).

He clearly associated himself with the first group. Since the static system was at odds with the economic–historical reality and the dominant equilibrium approach to economics had left the solution to many problems – not exclusively those of explaining cyclical fluctuations – open, 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, p. 268). He identified technological change in the era of progressive industrialisation to be the decisive endogenous factor generating the business cycle. Moreover, stimulated by the works of Marx and contemporary theorists, such as Schumpeter, Spiethoff, Sombart and Oppenheimer, 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, therefore, he aimed to create a theory of cyclical growth in which no artificial separation is made between the cycle and the trend.[1]

The challenge arising from Löwe's attack on the traditional concept of a static equilibrium and his plea for an alternative 'dynamic' systematic 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), which is included as the second paper in this volume. *Geldtheorie und Konjunkturtheorie* (1929), which is the much-enlarged version of his contribution to the 1928 meeting of the Verein für Sozialpolitik in Zurich,[2] 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

1 See also his retrospective reflections in Löwe (1989). The traverse analysis developed by him in *The Path of Economic Growth* (1976) is a late realisation of this research programme; although, for example, the assumption of *exogenous* technological change does not live up to the high methodological standards set in his 1926 paper.

2 See Volume III.

theory of money and credit, but also by Löwe's methodological challenge and the attacks by Löwe and other members of the Kiel school, in particular Burchardt and Neisser, against monetary theories of the business cycle.[1] At the beginning of 1927, Friedrich August von Hayek (1899–1992) had become the first Director of the Austrian Institute for Business Cycle Research in Vienna – known today as Wifo –which had been founded through the initiative of Mises. Hayek stayed in this position until 1931 when he became a 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 Löwe's methodological critique is of a completely different nature. As Chapter I of *Monetary Theory and the Trade Cycle* documents, Löwe'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 Löwe's positions such as, for example, their views on the relationship between *empirical* observation and *theoretical* explanation. In particular Hayek accepted Löwe'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 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, pp. 42–3). Thus Hayek shared Löwe'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 their conclusions. Whereas Löwe 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, in particular, for the understanding of intertemporal price relationships.

It is therefore essential for Hayek's explanation of cyclical fluctuations to start from the assumption of equilibrium. *Prices and Production*, which is based on his 1931 London School of Economics lectures, is characterised 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

1 For a more detailed analysis see Hagemann (1994).

concept of a tendency towards an equilibrium' (Hayek 1935, p. 34).[1] For the analysis of dynamic economic 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, whilst shaping his business cycle theory, Hayek wrote his article '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 recognised that changes in the relative prices of capital and consumption goods were the key to understanding 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, pp. 44–5).

In this statement Hayek shows that the way out of what he regarded to be Löwe's impasse was through the integration of money and credit into an equilibrium framework. Löwe, on the other hand, focused on technological change as the decisive endogenous impulse for the disturbance of the equilibrium of an economy. Hayek's adherence to the concept of equilibrium in his business cycle analysis was due to theoretical as well as political reasons. In contrast to Löwe, Hayek regarded the free market economy as inherently stable so that all movements could essentially be seen as equilibrating adjustment processes. Löwe was certain that disorderly tendencies existed in uncontrolled industrial markets in which profit maximisation had lost its classical determinacy; hence his later plea for interventionism in order to combine political and economic freedom with the goal of collective rationality (Löwe 1965).

1 Interestingly, Löwe finally summed up his position in *Economics and Sociology* (1935) – which is also based on a series of lectures given at the London School of Economics shortly after his emigration from Nazi Germany – in which he favoured the replacement of the static equilibrium approach with a dynamic conceptualisation of the economy, which makes the fixed sequence of cyclical phases an integral part of general economic theory. See Löwe (1935, pp. 89–90).

In 1926 Eugen Slutsky (1880–1948) had moved from the University of Kiev to Moscow, to become a member of the research staff of the Conjuncture Institute directed by Kondratieff. He was mainly a mathematician and statistician, and became a member of the Mathematical Institute of the Academy of Sciences in Moscow in 1936. Slutsky's fame as an economist is based on his 1915 Italian article on consumer choice, elaborating on the earlier work of Pareto, which itself only became widely known due to the work by Hicks and Allen – who independently arrived at Slutsky's results concerning the income and substitution effects of price changes about two decades later. However, Slutsky's 1927 article, 'The Summation of Random Causes as the Source of Cyclic Processes' (which is included in this volume in its revised 1937 English version), was already regarded among specialists as a classic paper in time-series analysis and business cycle theory when it was published in *Econometrica*, following an earlier English translation 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, p. 107). Slutsky's idea that random shocks, not themselves of a cyclical nature, could generate regular oscillations of economic variables was an important and seminal idea. It not only stimulated further research in time-series analysis, but it also freed economists from the belief that cyclical fluctuations must be due to periodical causes. 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 was almost entirely forgotten until the rise of real business cycle theory in the early 1980s, when Slutsky's idea that the cumulation of random shocks could produce cyclical fluctuations proved to be of seminal importance again.

One of the earlier business cycle theorists who immediately recognised the importance of Slutsky's contribution was Simon Kuznets (1901–85), 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 emphasised 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, p. 274). This paper was followed by his article 'Equilibrium Economics and Business-Cycle Theory', included in this volume, in which Kuznets aims at creating a *general theory of economic change*. In essence he combines three ideas in an innovative way: namely, Slutsky's thesis, Paul Rosenstein-Rodan's emphasis on the importance of

time differences[1] 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 essentially dynamic processes such as cyclical fluctuations. Kuznets discusses Löwe'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', or regards 'the equilibrium approach ... to be a blind alley from the point of view of business-cycle theory', ending with his statement that 'the practice of treating change as a deviation from an imaginary picture of a rigid equilibrium system must be abandoned' (1930a, pp. 390, 399, 415).

On the other hand, Kuznets makes it clear that only the concept of a too static equilibrium approach should be discarded and 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 for a synthesis of Slutsky's ideas and 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 the 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. In his detailed empirical analysis Kuznets (1933) showed that capital-goods industries have larger cyclical oscillations than consumer-goods industries. In his earlier investigation of 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 conclusion that wholesale trade 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, p. 406). Measurement of these factors has to be guided by theory, but theory has to respect facts and the accumulation of economic knowledge by empirical work. These research strategy guidelines were the key ingredients in Kuznets's early work on business cycle theory. Accordingly, Kuznets did not doubt the permanent occurrence of random disturbances or that the summation of random changes will always be present as long as there are differences in time coefficients. Thus, Kuznets writes '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' (1930a, p. 411).

1 See Rosenstein-Rodan (1929).

However, Kuznets, whose research interests shifted shortly afterwards to the analysis of long-term trends and economic growth processes, was already aware that two complications arise from a certain skewness – that is, the trend movement in random events – and from the fact that the stream of random changes cannot be treated as being continuously random because, after a while, the trend component will affect the behaviour of business people. Thus relatively small disturbances may cause rather important disproportionalities and may result in prolonged oscillations of formidable magnitude.

In the same year that Kuznets's article was published, the young Erik Lundberg (1907–87) wrote his licentiate thesis 'On the Concept of Economic Equilibrium' which is included in this volume in its 1995 enlarged English translation. As a survey of 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 Ph.D. thesis *Studies in the Theory of Economic Expansion* which became the highlight of the Stockholm school's works on macroeconomic fluctuations.[1] As the title of the thesis indicates, Lundberg maintained that cyclical fluctuations are 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 on the same topic. Lundberg formulated an early analysis of the multiplier–accelerator type. Some of his 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 provided inspiration for the later work by Metzler (1941). In the same year as publishing his most important work, Lundberg became the Director of the *Konjunkturinstitutet*, the Swedish Institute for Economic Research, where he stayed until 1955. His later book *Business Cycles and Economic Policy* is a kind of legacy of his applied work on business cycles during this period.

Lundberg's thesis 'On the Concept of Economic Equilibrium' shows that the young author was remarkably erudite, well-read of the most recent literature in a variety of 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.[2] In this essay, Lundberg takes up Löwe's fundamental methodological, challenge considering the incompatibility of the static equilibrium system with

1 For a detailed assessment of Lundberg's contribution to the dynamic method of the Stockholm school see Hansson (1982).

2 For an enlightened discussion of the importance of Lundberg's early contribution see Henriksson (1996).

business cycle theory; shows his understanding of the work of Hayek, Kuznets and Rosenstein-Rodan; and extensively scrutinises the contributions of, among others, Schumpeter and John Bates Clark, and the more recent Swedish works of Myrdal (on 'virtual' equilibrium states) and Lindahl (on 'moving' equilibrium, that is, 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 shows that he had fully grasped the essentials of Slutsky's argument when he stated 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, p. 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 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 was only published in Norwegian in 1930 in a Danish journal (Frisch 1929). Following Frisch, Lundberg was concerned with the static method which is essential to all partial and general equilibrium theories. He also saw a decisive distinguishing element in the fact that the static method disregards the idea that supply and demand depend not only 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) states. The main aim is to elaborate upon the hidden premises and limitations of 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, p. 36), but that the two concepts should also be used simultaneously. He saw general equilibrium theory as being indispensable in economic thought since it comprises the interrelatedness of economic variables which cannot, for a long period, considerably diverge from 'normal' positions.

Friedrich A. Lutz (1901–75) is a well-known economist in the areas of monetary, capital and interest theory, who acquired an international reputation after he emigrated from Nazi Germany to the United States because of his liberal convictions in 1938. He continued his academic career in Princeton and moved to Zurich in 1953. At that time he was recognised as a leading expert on the international monetary system and as an early ardent advocate of floating exchange rates. Among his most influential papers were 'The Outcome of the Saving–Investment Discussion'

(Lutz 1938) – reprinted in the American Economic Association's 1944 collection of *Readings in Business Cycle Theory* – and the famous article on the term-structure of interest rates (Lutz 1940). *The Theory of Investment of the Firm* (1951), which he wrote with his wife Vera Smith, involves a fruitful synthesis of neoclassical microeconomic theory and the treatment of capital and time in the Austrian tradition. This book became widely known, as did his later book *The Theory of Interest* (1967), in which Lutz wrote from the perspective of modern economics, giving a survey of the most important approaches to interest theory throughout the history of economic thought, and concluding with his own account of the state of the art in that area since Keynes.

Lutz had already proceeded in the same manner in his earlier 1932 book *Das Konjunkturproblem in der Nationalökonomie* (The Problem of Business Cycles in Economics), in which he reviewed the existing body of theories on economic fluctuations from the viewpoint of equilibrium economics. Astonishingly, this work, which is of great interest for a precise theoretical foundation of business cycle theory, has not yet been translated into English, despite the international recognition Lutz gained soon after publication.[1] The reason this work has not been translated into English was mainly due to the fact that the conclusion of the debate on the (in)compatibility of business cycle theory with the equilibrium system – initiated by Löwe – was written in German. This was published at a time when the main protagonists were no longer living in Germany shortly afterwards and, for the same political reasons, the German language lost its role as a medium of international scientific discourse. The importance of Lutz's work to modern debates was only rediscovered recently when one author went as far as to describe it as 'one of the most splendid books on the history of business cycle theory ever written' (Rühl 1994, p. 188) and rightly emphasised that 'Lutz's discussion should be considered indispensable for any truly comprehensive account of the origins of contemporary equilibrium business-cycle theories' (Rühl 1997, p. 416). This volume contains the first English translation of the three theoretically most important chapters: Chapter 3, 'Return to, and Extension of, the Pure Theory'; Chapter 5, 'Application of the Theory to Explanation of Individual Business Cycles'; and Chapter 6, 'The Nature of the Problem of Business Cycles', in which Lutz draws his final conclusions.

In his systematic comparison of modern equilibrium theories of the business cycle and interwar contributions Rühl (1994) states that there are four different ways of dealing with the methodological problem, posed by

1 Lutz submitted this work as his habilitation thesis to the University of Freiburg, where he had been a research associate of Walter Eucken. Under Eucken's supervision at the University of Tübingen Lutz had already written his 1925 doctoral thesis, *Der Kampf um den Kapitalbegriff in der neuesten Zeit* (The Controversy on the Concept of Capital in Modern Times), which was a complex and difficult work to read, unduly neglected in recent debates on capital theory.

Löwe, of the (in)compatibility of business cycle theory with the theory of general economic equilibrium:

1) the abandonment of static equilibrium theory as it had been proposed by Löwe and, among others, endorsed by Kuznets;

2) the defence of the equilibrium method by dichotomising the cycle from the trend 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 Löwe's challenge;

3) a path-dependent approach without an a priori trend line, as it had been developed by Schumpeter; and

4) 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 analysing 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 last route was taken by Lutz (1932) who analysed the other variants in great detail. He confirmed that although Löwe had clearly identified specific problems with static theory (caused by cyclical fluctuations), the solution he had proposed – that is, his demand for a new dynamic theory – was in fact impossible. Lutz also maintained that Löwe had confused 'data' with 'variables', when he regarded the data of the theory as being subject to the general interdependence, which is definitely not the case. The introduction of technical progress, as favoured by Löwe, is nothing other than a change in the data which would lead to a new equilibrium. However, the analysis of changes in data, which is the task of business cycle theory could be made completely within the existing framework of equilibrium theory by using the variation method. There would therefore be no need for a 'new dynamic' theory. In the same line of reasoning Lutz criticises Kuznets for his accusation that static theory had overlooked different reaction speeds. For Lutz – who only concedes that equilibrium theory, particularly through the influence of the Lausanne school, had not put enough emphasis on adjustment processes – static theory also includes the 'investigation of how equilibrium comes about, that is, the movement towards equilibrium following a change in data' (Lutz 1932, p. 121).

Lutz's analysis also contains an elaborate critique of Schumpeter's work which he rightly regards as being founded on the distinction between statics and dynamics as it was made by John Bates Clark, with *Das Wesen und der Hauptinhalt der theoretischen Nationalökonomie* (Schumpeter 1908) playing the static part and his *Theory of Economic Development* constituting the dynamic part. Lutz regards Schumpeter's conception of the static theory as being superior to that of Clark since, stimulated by the work of Böhm-Bawerk, emphasis is placed 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 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, p. 10, n. 2). Lutz shows convincingly that this is carefully considered by Schumpeter who assigns the task of explaining the origin and effects of discontinuous changes within the economy to dynamic theory. This endogenous part is played by the pioneering entrepreneurs, who implement new combinations of the factors of production. They are also responsible for economic development and thus for explaining the difference between dynamics and statics. However, Lutz criticises 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's dynamic theory would then have to rely on exogenous forces – that is, on changes in data – which do not necessarily imply periodicity. Schumpeter also 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' (Lutz 1932, p. 14).

For Lutz's own solution to the dilemma posed by Löwe 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 an 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 that goes beyond the accomplishments of equilibrium theory. Therefore all attempts to develop a 'new dynamic' theory have failed. It is surprising that Lutz's position and his resumed efforts, which are summarised in the concluding Chapter 6, '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, p. 200, n. 24). For example, it is questionable whether one could grasp capital formation processes by simply considering data changes (thereby excluding 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 produce recurrent and pervasive fluctuations by themselves, and with several changes (such as changes in technology or taste) it is doubtful to regard them as exclusively exogenous. However, interdependency should certainly be a key issue in a business cycle theory which explains dynamic processes in a modern 'industrial' economy which nowadays, furthermore, is characterised by an increasing importance of services and knowledge.

Among the Italian economists who made important contributions to monetary theory and business cycle theory were Costantino Bresciani Turroni, Marco Fanno and Gustavo Del Vecchio (1883–1972). Influenced by Maffeo Pantaleoni, Del Vecchio tried to develop a theory of economic dynamics which was not exclusively understood as a modification of static analysis, although his work on monetary theory was strongly influenced by the theory of money Walras had developed in the fourth edition of his book *Elements d'economie pure* (1900). In a series of papers published from 1909 onwards, Del Vecchio applied the marginal principle to money and incorporated the special case of the value of money into the general structure built upon the law of marginal utility. In his *History of Economic Analysis* Schumpeter (1954, p. 1082) praises 'the original work of Del Vecchio' which was 'summed up in his *Grundlinien der Geldtheorie* and more completely in his *Ricerche sopra la teoria generale della moneta*' (1932). Del Vecchio's German book on the foundations of monetary theory was largely based on his earlier Italian papers but, for the first time, he attempted to form a concise theoretical unity which included problems of bank credit and the determination and effects of changes in the discount rate at the centre of his analysis. Del Vecchio also came very close to a Schumpeterian approach in his 1928 Vienna paper 'Investigations on the Theory of Entrepreneurial Profit', in which he regarded profits as the most evident expression of the dynamic character of the economy, and problems of equilibrium as only being special cases in the study of dynamic theory. Del Vecchio, who had been a professor at the universities of Trieste, Bologna and Rome, was forced to resign from the Bocconi University in Milan, where he had been the Chancellor since 1934, when the fascist government launched racial laws in 1938.

This volume contains the first English translation of Part 7, 'Crises', from Del Vecchio's *Capitale e Interesse* (1956). The first and major section is the article 'On the Economic Theory of Crises', published in Italian in 1914, which begins with a review of Aftalion's book *Les Crises Périodiques de Surproduction* (1913) and continues with a discussion of the necessary ingredients of a sound theoretical explanation of cyclical fluctuations. The article contains a Mengerian theory of the nature of money, a theory of a

flexible multiplier for bank deposits and an overinvestment explanation of the cycle. Section 2 contains a discussion of theoretical aspects of the world crisis in the early 1930s and includes two appendices providing observations on depressed and 'overactive' areas. Section 3, 'The Scientific Construction of Economic Dynamics', is most relevant for our methodological debate, as it reveals Del Vecchio's views that:

1) each cycle must be regarded as an independent one, so that there is no general theory of the business cycle existing, a point on which he is in agreement with Lutz;
2) static phenomena are no more than borderline cases of dynamic situations and cyclical fluctuations are inseparably linked to economic progress, compared to which they are only a secondary phenomenon.

From the latter perspective he is rather critical of Keynes's system which he considers as essentially static and which ignores the long-term, and thereby crucial, questions of economic dynamics. For this reason he also assesses developments of Keynesian theory in a more dynamic direction, as multiplier–accelerator models of cyclical fluctuations, as being of only limited use.

One of the fathers of the latter type of models was John Hicks (1904–89), for whom the economic contributions of Keynes and Hayek provided a lifelong challenge in developing his own theoretical explanations. Hicks had always been sceptical about Hayek's claim that the economy would be in equilibrium if there were no monetary disturbances. This is already manifest in his early essay 'Equilibrium and the Trade Cycle' (1933) – included in this volume in its 1980 retranslation into English – which, essentially, is the result of Hicks's grappling with Hayek's *Prices and Production* and Hayek's 1928 concept of intertemporal equilibrium. Here we find Hicks (who was a young lecturer at the London School of Economics when Hayek arrived in 1931) arguing against Hayek's statement 'that a change in the effective volume of monetary circulation is to be regarded as an independent cause of disequilibrium. I cannot accept this in its literal sense, though I am prepared to agree that in a world of imperfect foresight monetary changes are very likely to lead to acute disequilibrium' (Hicks 1980, p. 526, n. 8). Hicks realised that to analyse money one must consider uncertainty and expectations. He had a long struggle to present an inherently dynamic version of the economy in which agents' present decisions represented their attempts to cope with an uncertain future in view of monetary and real constraints imposed upon them by past actions. From his 1935 'Suggestion for Simplifying the Theory of Money', published in his *Critical Essays in Monetary Theory* (1967) – which was a landmark in the evolution of the theory of liquidity preference, and which also contains his personal 'Hayek story' – to his last book, *A Market Theory of Money* (1989), Hicks made important contributions to monetary theory over a period of

more than half a century. Nevertheless he did not become tired of emphasising 'the *real* (non-monetary) character of the cyclical process' (Hicks 1950, p. 136). Hicks always treated the cycle as fundamentally a real phenomenon reflecting technological change and the fluctuations in investment that accompany them. Monetary disorders may be superimposed upon the real disorders but they are only of secondary importance. This is already an issue in his early 1933 article which focuses on the relationship between equilibrium and money. Almost two decades later Hicks made it one of the main objectives of his *Contribution to the Theory of the Trade Cycle* 'to show that the main features of the cycle can be adequately explained in real terms' (Hicks 1950, p. 136).

Ragnar Frisch (1895–1973) had been a professor at the University of Oslo since 1931. In the following year he became the Head of the newly founded Institute of Economics, a position he held until his retirement in 1965. He was one of the key founders of the Econometric Society in 1930 and the first managing editor of *Econometrica* (1933–55). In 1969 he was awarded the first Nobel Memorial Prize in Economics, together with Jan Tinbergen, for his contributions to the development of macrodynamics in the 1930s. In the late 1920s and early 1930s Frisch focused on the development of dynamic methods in economics and in business cycle theory. His most significant methodological contribution was the precise distinction between static and dynamic analysis which was already fully developed in his Norwegian article, 'Statics and Dynamics in Economic Theory' (Frisch 1929).[1] 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, p. 394). From this it follows that Frisch thought that a Walrasian system of general economic equilibrium was inappropriate for business cycle theory.

The clarification of Frisch's views on dynamics versus statics and other methodological concepts – such as, for example, the distinction between micro-dynamic and macro-dynamic analysis – had become a sound foundation by the time he focused on business cycle analysis in the early 1930s. During this period, Frisch made his famous contribution to the Cassel Festschrift 'Propagation Problems and Impulse Problems in Dynamic Economics' (Frisch 1933), which is included in this volume.[2] The main idea of the article goes back to Wicksell (1907): to distinguish between two fundamental problems in the analysis of cyclical fluctuations, the propagation

1 The separate parts were only translated into English in 1992. See also Andvig (1992).

2 For a detailed analysis of Frisch's contributions to business cycle analysis in the interwar period, based on rich sources of original material, see Andvig (1981).

problem and the impulse problem. Frisch[1] 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 dampens the oscillation which is caused by a single shock. However, shocks occur 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 originally 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. However, he later altered this belief by showing a firm preference for the Kondratieff cycle,[2] which may be the result of the strong exchange of ideas he had over many years with Schumpeter concerning the role of innovations as a key factor in maintaining oscillations.[3]

Methodologically Frisch tried to bridge the gap which existed in the late 1920s between empirical research on business cycles (such as by Wesley Mitchell, who favoured the inductionist method) and purely theoretical research (which even avoided sound empirical analysis) using modern statistical techniques in order to make their approaches operational. The latter attitude was quite common particularly among monetary business cycle theorists, including leading representatives of such diverse schools of thought as those of the Cambridge and Austrian schools. In this context it is quite interesting to look at the article 'Tasks and Limits of the Institutes for Business Cycle Research' (1928a), which the young Oskar Morgenstern (1902–77) contributed to the Zurich meeting of the Verein für Sozialpolitik. Morgenstern succeeded Hayek as the Director of the Vienna Institute in the autumn of 1931, where he remained until his emigration to the United States after the Nazis' occupation of Austria in March 1938. He argues that two types of institutes exist: 1) institutes with a strong emphasis on theoretical research, and 2) institutes which have the character of agencies supplying the subscribers of their periodicals with regular information and economic predictions. He counted the National Bureau of Economic Research in New York (founded in 1920 and directed by Wesley Mitchell), and the Kiel Institute (with the research department directed by Löwe) as

1 See Frisch (1933, p. 198) and the contribution by Wicksell in Volume II of this collection.

2 See Andvig (1981, p. 708).

3 See Section 6 of his 1933 essay.

belonging to the first group. To the second group he assigned the two institutes in Berlin and Vienna as well as the Harvard Economic Service (where Warren Persons had developed the barometric approach to forecasting since 1917). Both groups have three practical tasks and one theoretical task:[1] 1) compiling information on the state of the economy; 2) economic forecasting; 3) making specific proposals on concrete economic policy problems; and 4) the advancement of research.

However, the separation of the two types of institutes is not absolute. Some important contributions to business cycle research were made at Berlin, such as the first empirical inquiry into the cyclical behaviour which later became known as the 'cobweb cycle' by Arthur Hanau. An even better demonstration of this is the work of Hayek and Morgenstern, the two directors of the Vienna Institute in the interwar period. Hayek never concealed his animosity against having to regularly report on the state of the economy, or his preference for pure theoretical analysis of business cycles. Morgenstern had dealt with one of the central practical problems himself in *Economic Forecasting. An Investigation of their Conditions and Possibilities* (Morgenstern 1928c), his habilitation thesis which he submitted to the University of Vienna as the result of a two years' research stay in the United States, mainly at Columbia and Harvard. Nevertheless he left no doubt that the attempt to predict future states of the economy has to be based on a sound theory of the business cycle, since 'an adequate picture of the states of the economy cannot be given with statistical methods alone.' (Morgenstern 1928b, p. 87). Morgenstern did not only share Böhm-Bawerk's idea that business cycle theory constitutes the final stage of a fully developed system of economic theory, but he also saw in the solution of the problem of macroeconomic forecasting the last chapter of business cycle theory. However, he was very critical of how contemporary empirical research work was carried out in the United States or by Wagemann and his crew at the Berlin Institute.

Furthermore, Morgenstern had a life-long animosity against the static character of general equilibrium theory,[2] which is evident in his habilitation thesis as well as in his 1935 essay, translated by Frank Knight into English as 'Perfect Foresight and Economic Equilibrium', which is the final article in this volume. In this essay, Morgenstern questions a key statement made by Hicks in his 1933 essay 'Equilibrium and the Trade Cycle': that perfect foresight is the condition for equilibrium. Deploring the absence of a systematic investigation into the assumptions underlying general equilibrium theory, Morgenstern comes to the opposite conclusion from Hicks: '*Unlimited foresight and economic equilibrium are thus irreconcilable with one another*'. The reason lies in the fundamental problem that economic decision-makers are not only confronted with 'dead' variables but also with

1 See Morgenstern (1928a, p. 342).
2 See also Morgenstern (1972).

lively ones, which reflect the decisions of other actors; that is, rational individuals have to consider not only the consequences of their own decisions, but also those of all other individuals and the additional consequences of their own behaviour on those of the other actors, etc. This leads to *'an endless chain of reciprocally conjectural reactions and counter-reactions. This chain can never be broken by an act of knowledge but always only through an arbitrary act – a resolution'* (1976, p. 174). It is therefore evident that to expect to have foresight into the probable behaviour of other individuals and subsequent reactions and counter-reactions would seem to be paradoxical. Morgenstern illustrates his central argument, which became known as the *Morgenstern Paradox*, with the famous Sherlock Holmes–Moriarty example. He had already used this idea in his habilitation thesis, but it was only worked out numerically in the pioneering study *Theory of Games and Economic Behaviour* (1944), which was the result of his work with John von Neumann in Princeton.

REFERENCES

Aftalion, A. (1913), *Les Crises Périodiques de Surproduction*, 2 vols, Paris: Marcel Rivière.

Andvig, J. C. (1981), 'Ragnar Frisch and Business Cycle Research during the Interwar Years', *History of Political Economy*, 13, pp. 695–725.

Andvig, J. C. (1992), 'Introduction to Frisch's Article "Statics and Dynamics in Economic Theory"', *Structural Change and Economic Dynamics*, 3, pp. 385–9.

Craver, E. (1986), 'Patronage and the Directions of Research in Economics: The Rockefeller Foundation in Europe 1924–1938', *Minerva*, 24, pp. 205–22.

Del Vecchio, G. (1928), 'Untersuchungen zur Theorie des Unternehmergewinnes', in H. Mayer (ed.), *Die Wirtschaftstheorie der Gegenwart, Vol. III: Einkommensbildung*, Vienna: Julius Springer, pp. 281–92.

Del Vecchio, G. (1930), *Grundlinien der Geldtheorie*, Tübingen: J. C. B. Mohr.

Del Vecchio, G. (1932), *Ricerche sopra le teoria generale della moneta*, Milan: Università Bocconi.

Del Vecchio, G. (1956), *Capitale e Interesse*, Torino: Einaudi.

Frisch, R. (1929), 'Statikk og dynamikk i den ökonomiske teori', *Nationalökonomisk Tidskrift*, 67, pp. 321–79; English translation of Sections 1–3 as 'Statics and Dynamics in Economic Theory', *Structural Change and Economic Dynamics*, 3, 1992, pp. 391–401.

Frisch, R. (1933), 'Propagation Problems and Impulse Problems in Dynamic Economics', in K. Koch (ed.), *Economic Essays in Honour of Gustav Cassel*, London: Allen & Unwin, 171–205.

Frisch, R. (1936), 'On the Notion of Equilibrium and Disequilibrium', *Review of Economic Studies*, 3, pp. 100–5.

Gehrke, C. (1997), 'Introduction to Adolf Löwe's "How is Business Cycle Theory Possible at All?"', *Structural Change and Economic Dynamics*, 8, pp. 233–44.

Hagemann, H. (1994), 'Hayek and the Kiel School: Some Reflections on the German Debate on Business Cycles in the late 1920s and the early 1930s', in

M. Colonna and H. Hagemann (eds), *Money and Business Cycles. The Economics of F. A. Hayek*, vol. I, Aldershot: Edward Elgar, pp. 101–20.

Hansson, B. A. (1982), *The Stockholm School and the Development of the Dynamic Method*, London: Croom Helm.

Hayek, F. A. (1928), 'Das intertemporale Gleichgewichtssystem der Preise und die Bewegungen des Geldwertes', *Weltwirtschaftliches Archiv*, 28, pp. 33–76; English translation as 'Intertemporal Price Equilibrium and Movements in the Value of Money', in R. McCloughry (ed.), F. A Hakek, *Money, Capital and Fluctuations: Early Essays*, London: Routledge and Kegan Paul, 1984, pp. 71–117.

Hayek, F. A. (1929), *Geldtheorie und Konjunkturtheorie*, Vienna: Hölder-Pichler-Tempski; English translation as *Monetary Theory and the Trade Cycle*, London: J. Cape, 1933; New York: Augustus M. Kelley, 1966.

Hayek, F. A. (1931), *Prices and Production*, 2nd edn, London: Routledge & Kegan Paul, 1935.

Hayek, F. A. (1939), *Profits, Interest and Investment*, London: Routledge.

Henriksson, R. G. H. (1996), 'The Early Contribution of Erik Lundberg: A Commentary to his Licentiate Thesis "On the Concept of Economic Equilibrium"(1930)', *Structural Change and Economic Dynamics*, 7, pp. 347–60.

Hicks, J. (1933), 'Gleichgewicht und Konjunktur', *Zeitschrift für Nationalökonomie*, 4, pp. 441–55; English translation as 'Equilibrium and the Trade Cycle', *Economic Inquiry*, 18, 1980, pp. 523–34.

Hicks, J. (1935), 'A Suggestion for Simplifying the Theory of Money', *Economica*, 2, pp. 1–19.

Hicks, J. (1950), *A Contribution to the Theory of the Trade Cycle*, Oxford: Clarendon Press.

Hicks, J. (1967), *Critical Essays in Monetary Theory*, Oxford: Clarendon Press.

Hicks, J. (1989), *A Market Theory of Money*, Oxford: Clarendon Press.

Kuznets, S. (1926), *Cyclical Fluctuations: Retail and Wholesale Trade, United States, 1919–1925*, New York: Adelphi.

Kuznets, S. (1929), 'Random Events and Cyclical Oscillations', *Journal of the American Statistical Association*, September, pp. 258–75.

Kuznets, S. (1930a), 'Equilibrium Economics and Business-Cycle Theory', *Quarterly Journal of Economics*, 44, pp. 381–415.

Kuznets, S. (1930b), 'Monetary Business Cycle Theory in Germany', *Journal of Political Economy*, 38, pp. 125–63.

Kuznets, S. (1933), *Seasonal Variations in Industry and Trade*, New York: National Bureau of Economic Research.

Kydland, F. and Prescott, E. C. (1982), 'Time to Build and Aggregate Fluctuations', *Econometrica*, 50, pp. 1345–70.

Löwe, A. (1925), 'Der gegenwärtige Stand der Konjunkturforschung in Deutschland', in M. J. Bonn and M. Palyi (eds), *Die Wirtschaftswissenschaft nach dem Kriege. Festgabe für Lujo Brentano zum 80. Geburtstag*, vol. 2, Munich and Leipzig: Duncker & Humblot, pp. 329–77.

Löwe, A. (1926), 'Wie ist Konjunkturtheorie überhaupt möglich?', *Weltwirtschaftliches Archiv*, 24, pp. 165–97; English translation as 'How is Business Cycle Theory Possible at All?' in *Structural Change and Economic Dynamics*, 8, 1997, pp. 245–70.

Löwe, A. (1935), *Economics and Sociology: a Plea for Cooperation in the Social Sciences*, London: George Allen & Unwin.
Löwe, A. (1965), *On Economic Knowledge. Toward a Science of Political Economics*, New York: Harper & Row.
Löwe, A. (1976), *The Path of Economic Growth*, Cambridge: Cambridge University Press.
Löwe, A. (1989), 'Konjunkturtheorie in Deutschland in den Zwanziger Jahren', in B. Schefold (ed.), *Studien zur Entwicklung der ökonomischen Theorie VIII*, Berlin: Duncker & Humblot, pp. 75–86.
Lucas, R. E. (1977), 'Understanding Business Cycles', in K. Brunner and A. H. Meltzer (eds), *Stabilization of the Domestic and International Economy*, Carnegie-Rochester Conference Series on Public Policy, vol. 5, Amsterdam: North-Holland, pp. 7–29.
Lundberg, E. (1930), 'Om begreppet ekonomisk jämvikt', *Ekonomisk Tidskrift*, 32; English translation as 'On the Concept of Economic Equilibrium' in R. G. H. Henriksson (ed.), *Studies in Economic Instability and Change*, Stockholm: SNS Förlag, 1995, pp. 13–47.
Lundberg, E. (1937), *Studies in the Theory of Economic Expansion*, London: P.S. King & Sons; reprinted with a new preface, Oxford: Basil Blackwell, 1955.
Lundberg, E. (1953), *Konjunkturer och Ekonomisk Politik*, Stockholm: SNS Förlag; English translation as *Business Cycles and Economic Policy*, London: Allen & Unwin.
Lutz, F. A. (1925), *Der Kampf um den Kapitalbegriff in der neuesten Zeit*, Tübingen: Mohr Siebeck.
Lutz, F. A. (1932), *Das Konjunkturproblem in der Nationalökonomie*, Jena: Gustav Fischer.
Lutz, F. A. (1938), 'The Outcome of the Saving–Investment Discussion', *Quarterly Journal of Economics*, 52, pp. 588–614.
Lutz, F. A. (1940), 'The Structure of Interest Rates', *Quarterly Journal of Economics*, 55, pp. 499–529.
Lutz, F. A. (1967), *The Theory of Interest*, Dordrecht: D. Reidel.
Lutz, F. A. and Lutz, V. S. (1951), *The Theory of Investment of the Firm*, Princeton: Princeton University Press.
Metzler, L. A. (1941), 'The Nature and Stability of Inventory Cycles', *Review of Economics and Statistics*, 23, pp. 113–29.
Morgenstern, O. (1928a), 'Aufgaben und Grenzen der Institute für Konjunkturforschung', in K. Diehl (ed.), *Beiträge zur Wirtschaftstheorie*. Vol. II: *Konjunkturforschung und Konjunkturtheorie*, Schriften des Vereins für Sozialpolitik vol. 173/II, Munich and Leipzig: Duncker & Humblot, pp. 337–53.
Morgenstern, O. (1928b), 'Qualitative und quantitative Konjunkturforschung', *Zeitschrift für die gesamte Staatswissenschaft*, 85, pp. 54–88.
Morgenstern, O. (1928c), *Wirtschaftsprognose. Eine Untersuchung ihrer Voraussetzungen und Möglichkeiten*, Vienna.
Morgenstern, O. (1935), 'Vollkommene Voraussicht und wirtschaftliches Gleichgewicht', *Zeitschrift für Nationalökonomie*, 6, pp. 196–208; English translation as 'Perfect Foresight and Economic Equilibrium' by Frank Knight, in A. Schotter (ed.), *Selected Economic Writings of Oskar Morgenstern*, New York: New York University Press, 1976, pp. 169–83.

Morgenstern, O. (1972), 'Thirteen Critical Points in Contemporary Economic Theory: An Interpretation', *Journal of Economic Literature*, 10, pp. 1163–89.
Neumann, J. von, and Morgenstern, O. (1944), *Theory of Games and Economic Behavior*, Princeton: Princeton University Press.
Rosenstein-Rodan, P. (1929), 'Das Zeitmoment in der mathematischen Theorie des wirtschaftlichen Gleichgewichts', *Zeitschrift für Nationalökonomie*, 1, pp. 129–42.
Rühl, C. (1994), 'The Transformation of Business-Cycle Theory: Hayek, Lucas and a Change in the Notion of Equilibrium', in M. Colonna and H. Hagemann (eds), *Money and Business Cycles. The Economics of F. A. Hayek*, Vol. I, Aldershot: Edward Elgar, pp. 168–202.
Rühl, C. (1997), 'Lutz, Friedrich August (1901–1975)', in D. Glasner (ed.), *Business Cycles and Depressions. An Encyclopedia*, New York and London: Garland Publishing, Inc., pp. 415–16.
Schumpeter, J. A. (1908), *Das Wesen und der Hauptinhalt der theoretischen Nationalökonomie*, Leipzig: Duncker & Humblot.
Schumpeter, J. A. (1911), *Theorie der wirtschaftlichen Entwicklung*, Munich and Leipzig: Duncker & Humblot, 2nd edn published in 1926; English translation as *The Theory of Economic Development*, Cambridge: Harvard University Press, 1934.
Schumpeter, J. A. (1954), *History of Economic Analysis*, London: Allen & Unwin.
Slutsky, E. (1915), 'Sulla teoria del bilancio del consumatore', *Giornale degli Economisti e Rivista di Statistica*, 51, pp. 1–26; English translation as 'On the Theory of the Budget of the Consumer' in K. E. Boulding and G. J. Stigler (eds), *Readings in Price Theory*, London: Allen & Unwin, 1953, pp. 26–56.
Slutsky, E. (1927), 'The Summation of Random Causes as the Source of Cyclic Processes' (originally published in Russian) in *Problems of Economic Conditions*, The Conjuncture Institute, Moscow, vol. 3, no. 1; revised English version in *Econometrica*, 5, 1937, pp. 105–46.
Wicksell, K. (1898), *Geldzins und Güterpreise*, Jena: Gustav Fischer; English translation by R. F. Kahn as *Interest and Prices*, London: Macmillan, 1936.

Volume IV
Equilibrium and the Business Cycle

Löwe

'How is Business Cycle Theory Possible at All?'

Adolf Löwe, 'Wie ist Konjunkturtheorie überhaupt möglich?', *Weltwitschaftliches Archiv*, 24 (1926), pp. 165–97; translated by Christian Gehrke as 'How is Business Cycle Theory Possible at All?', *Structural Change and Economic Dynamics*, 8 (1997), pp. 245–70.

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How is business cycle theory possible at all?[1]

A. Löwe

1. The problem

Business cycle research is in a curious state. It has become a fashionable field of inquiry and it arouses the general interest of politicians and of the practical business community. Its topic as well as its newly developed methods have predestined it for this role. To depict exactly the current market situation as a whole as well as in all its details and, according to the pronouncements of the adacious innovators, even to determine it in advance – what could bring economics, often scolded as an unrealistic science, closer to the men of business than such promises? The business cycle researcher of today refuses to think in terms of the abstractions of the elder crisis theories. The realistic knowledge of the last generation has taught him the insight into complicated phase relationships which are not exhausted by the twin notions of boom and crisis. No longer is a single element of the market situation, like some price or the total purchasing power or the circulating amount of money, pulled out of the context of the overall relationships and discussed in isolation. The relationship between the prices of the agricultural and the industrial products, between the productive goods and the consumption goods, between the wholesale and the retail commodities, the relation of the production quantities of those same groups and their connection to the price relations, the structure of the 'horizontally connected markets'[2] (such as the markets for money, securities and commodities in the Harvard barometer) or the 'vertically connected markets' (for example, the investment goods and the consumption goods sphere), the relation between the capital market and the production of investment goods, between short- and long-term credit, between the price movement of the fixed interest securities and that of the dividend securities, or between the income categories of wages and profits – all this is just a small segment of the abundance of empirical facts which today have become problems of business cycle research. When American researchers, following some European developments of the last century, finally applied the statistical laws of 'time series' with undoubted success to the empirical data of the business cycle

[1] Translated by Christian Gehrke, University of Graz, from the original German article 'Wie ist Konjunkturtheorie überhaupt möglich?', *Weltwirtschaftliches Archiv*. 24 (1926) 165–197.
[2] See 'Vierteljahreshefte zur Konjunkturforschung', Berlin, Vol. 1, 1926, p. 32.

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and developed a new branch of business cycle statistics with 'barometers' from it, business cycle research seemed finally to have succeeded in constructing a bridge between theory and reality – a project, which had occupied generations of scholars in the sphere of economic inquiry.

One would be very wrong if one were simply to condemn this development from the standpoint of economic theory. This new realistic upswing is by no means only a late blossom of the historical school. It is, like the latter had formerly been itself, a reaction to a certain self-contentedness of pure theory and to its still undecided battle over the explanation of the business cycle problem. In this connection one must certainly acknowledge that the new statistical methods – which are far ahead of the monographical atomistics of the historical school and the vague surface descriptions of its imitators – have greatly deepened our perception of reality, in particular in so far as they are directed to the recording of typical patterns and regularities.

Yet all this conceded, one must still conclude that our insight into the *theoretical* interconnections of economic cycles and into the structural laws of circulation has not been enriched at all by all these phase descriptions and calculations of correlation. Business cycle theory has in principle not advanced a single step in the last decade. Three in terms of their history similarly honourable system types still confront each other irreconcilably. Whether the business cycle is to be explained from the monetary side or the real side, and if from the latter, whether from the conditions of production or those of consumption – each of these views has recently found new proponents in Germany. One may think of the quantity theory-based explanations of, for example, Hahn, Mises, Budge or Stucken, of the disproportionality theories of Spiethoff and Cassel, or of the underconsumption theory of Lederer. Or, if one wanted to distinguish by countries one could counterpose the quantity theory, as the dominant doctrine among the Anglo-Saxon economists, with the continental viewpoint, which still largely fixes upon the real or commodity side.

Now it would of course mean to misunderstand the logical relationship between theory and empirical research to expect an immediate furtherance of the *theoretical* system from an increase in *empirical* insight. The theorems of pure economic theory have of course only hypothetical relevance vis-à-vis reality, in so far as their validity is bound up with specific data, the selection and the ordering of which cannot be decided by means of economic theory. The propositions, however, which are deduced from these data in a logically unassailable way can neither be proved nor disproved by empirical findings in their theoretical validity. Nevertheless, social economics, as opposed to mathematics, is of course an empirical science. It must therefore react to contradictions between its propositions and economic reality. It does not do this, however, if in such a case it simply changes the data *combination*, always supposing, of course, that no formal errors have been committed. It must rather seek to respond to a revised form idea (Gestalturteil) by substituting the data itself either partly or completely, that is, by changing its axioms[3] rather than its method. *Verification* of

[3] 'Axiom' is here used in a sense that exceeds its philosophical usage. While the notion is commonly restricted to aprioristic basic premises, it is in the following used to designate all statements that are, within a given economic system, not capable of being further explained, even if in the system of knowledge as a whole they may only have aposterioric character.

the *data* and of the *propositions*: that is all that empirical insight can do for an individual theoretical doctrine and for the theoretical system of economics as a whole. Within its own systematic field, theory is autonomous.

For our specific subject this means that even the most assiduous empirical research cannot resolve a theoretical dispute, since, contrary to Mitchell's hypothesis, a "theory of the business cycle" can never be a "descriptive analysis". The logical relationship is just the reverse, in as much as an appropriate selective description of the chaotic jumble of empirical facts requires itself some conscious or unconscious theoretical 'prejudice'. There is one exception, however. One can select a purely *formal* element as a guiding principle for the business cycle description: the movement of the factors over time. By recording, in order to cast the net of knowledge as tight as possible, in as small time segments as possible the respectively chosen elements of the business cycle pattern in their absolute movements as well as in their relative relationship to one another, one can indeed obtain a picture of the business cycle which is free of any economic a priori. One could only wish that the statistical verification of theoretical results in business cycle theory were also carried out in such an unprejudiced way. Never, however, can the *statistical* method by itself generate *theoretical* insights. It has recently been argued[4] that in this way one could detect regularities in a statistical sense, and thereby obtain a type of surrogate for the theoretical law. The current business cycle statistics cannot even fulfil this hope. It still lacks the multiplicity of experiences which would allow application of the law of large numbers to its results. Yet even if future generations should be able to undertake this attempt with more justification, it is clear that the insight that is gained in this way will never be capable of replacing theoretical knowledge. The validity and unassailability of nomological insights cannot, due to the structure of our thinking, be reached by however many ontological statements.

The area in which business cycle theory can expect an enhancement from the upswing of empirical research is much smaller. It has already been specified: verification of the data and of the final conclusions of the theoretical deductions. However, business cycle theory has so far not even been able to successfully make use of this indirect help, although all modern cycle theories are backed up by large numbers of empirical facts and of time series for their verification and also attempt to fight their critics by using the weapon of empirical data. Yet the abundance of material which has been invoked has not led to an approximation of the divergent views in any important point. One is therefore led to suspect that business cycle theory, understood as the overall complex of the divergent theories, has perhaps not yet reached that degree of systematic unification which could offer the starting point for an indirect confirmation or rejection by means of empirical findings. May the business cycle theoretician look jealously at the empirical business cycle researcher for his luck of being able to grapple the full economic life at the point where it is most interesting – nobody can suspend him from his real task: to search within the relationships of theory itself for the critical cause that precludes the overall consistency of the individual theoretical insights.

[4] See 'Vierteljahrshefte zur Konjunkturforschung', Berlin, Vol. 1, 1926, p. 5.

Disagreements within a theoretical system are only possible for two reasons. One can either disagree about the *object* of the theoretical problem, that is in our case about which phenomena of the cyclical movement are in need of a theoretical explanation at all, and therefore start the theoretical discussion from different *data*. Or one employs, surprisingly as this may sound in view of the apparently unfailing method of nomological thinking, different *logical constructions* in the generation of the problem-solving deductions and the connecting hypotheses. In the first case the difference is in the problem *perception*, whereas in the second one it is in the methods of the problem *solution*.

Whichever of these two possibilities may be present in a particular case, it is at least clear that the immanent critique of a theory by another one on the basis of the latter's own problem perception or method cannot be decisive. If business cycle theory wants to master the prevailing linguistic confusion, there will be no other way than to make the content of the business cycle problem and the methods previously applied to its solution itself a matter of critique. It will first have to ask: what meaning has the business cycle as an epistemological concept of economic theory; which features of this concept are problematic; and finally, which logical requirements must a theoretical deduction fulfil that attempts to incorporate those features into the overall system relations?

To provide a first answer to these *pre-questions* of any business cycle theory is the object of the following discussion. It is thus an inquiry which in the field of epistemology is called 'axiomatic'. The purpose is not to solve a problem of economic theory *in terms of content*. Rather, it is to show, in order to clear the path for this real task, the *formal* order of the data which would make a solution in accordance with the laws of logic possible at all.

From where does the axiomatic critique derive its standard in order to determine the formal requirements of the business cycle problem and its solution? Where is the eternally valid reference frame to which problem perception and problem solution have to adhere?

If the business cycle problem were the only problem of economic theory, this question would be unanswerable within the confines of economic inquiry. We would have to find, in order to provide a standard for the axiomatics, the more general reference frame into which economic theory as theory would have to submit. Business cycle theory may indeed be "the last or the last but one chapter of a written or unwritten socio-economic system, the ripe fruit of the knowledge of all socio-economic processes and their reciprocal relationships" (Böhm-Bawerk). It in any case forms only a part of the theoretical economic system as a whole. The logical structure of this overall system therefore has to provide the standard by means of which the axiomatic study of the problem of the business cycle and of the methods of its solution can be determined. What is specific about the business cycle and requires a particular explanation in relation to the logical requirements of the general economic system – that alone can be the object of a particular business cycle theory. On the other hand a business cycle theory as a part of this superordinate total system can only be constructed, if it can be demonstrated that each contradiction of its specific features with the system conditions is merely superficial; that is, if the

business cycle problem can ultimately be resolved in accordance with the logic of the overall system.

If this is not attainable, then the results of the axiomatic critique can mean two things: either the business cycle problem lies outside of the field of economic theory and belongs to that large number of facts which, such as an earthquake or a war, have important economic consequences, but are for economic theory a data at best but never a *task*. For this option one would have to decide if the axiomatics shows that the problem in question cannot be demonstrated as *necessary* within the confines of the overall system relations (but, of course, is also not contradictory in it). Much more difficult is the second possibility, namely that the problem solution is, in principle, possible, yet only in contradiction to the logical conditions of the higher-ordered system. This antinomic state of the problem would destroy the system as a whole. It would mean in our case that business cycle theory is possible at all only on the basis of a system logic which is different from the one that prevails today.

After the field of the axiomatic *problematique* has thus been described, we will begin the further inquiry by first formulating the theoretical notion of the business cycle and by specifying the more comprehensive systematic relation, in which this notion exhibits problematic features. When we have thus defined the content of the business cycle problem, we will then turn to the existing theories in order to learn about the logical constructions by means of which these attempt to solve the problem. What emerges from the critique of those attempts for the structure of the theoretical system as a whole will be discussed in the concluding remarks.

2. The notion and the problem of the business cycle

Like any theoretical concept, the notion of the business cycle is first and foremost a designation of a fact of economic experience. As with most empirical forms, however, the factual object 'business cycle' is very hard to pin down in view of the open and infinite form of the overall world of appearances. One will have to interpret business cycle as an empirical fact: the 'overall market situation in each moment', where the notion of the 'overall market situation' directs the view to the *combining* of the single market phenomena, while the complement 'in each moment' seeks to emphasize the aspect of *mutability* as an important characteristic. Economic theory is not using this notion, however. It transforms the many-faceted object of experience into the unique dimension of its system by noting all and only those characteristics of the complex fact 'business cycle' that are meaningful in the theoretical system as a whole. In accordance with the formal structure of this overall system only quantitative elements enter into the theoretical notion of the business cycle, and these also only insofar as they are suitable for a nomological combination. Of the overall market situation in each moment only that is explainable by means of economic theory which can be grasped as '*price*' in the widest sense and which at the same time is integrable into the *laws* of prices – and which on the other hand also must be explainable by these means, if the business cycle problem is to be a problem of theory at all.

The regular relationship of all quantitatively specifiable factors of the overall market situation in each moment – this is the theoretical problem of the business cycle. With this definition, however, the business cycle problem seems to coincide with the problem of circulation as such, and business cycle theory to be nothing but the general theory of the economic circular flow. The common denomination of the sequence of the market situations as business *cycle* seems to confirm this identity.

The circulation problem itself has been one of the earliest objects of theoretical interest in economics. Admittedly, Quesnay's attempt to illuminate the structure of the economic process as a whole has found a continuation only with Marx. However, the price theory of the classics and of the marginal utility school, Say's law of markets, Mill's distinction between statics and dynamics, and even the functional equations of the mathematical school – they all have the methodological purpose of illuminating the structure of the economic circular flow, even if only as the requirement for the working-out of equilibrating tendencies. Now it is by no means true that each of these systems would depict the regular interrelationships of the economic process in a different way. Since the new marginal utility doctrine (Clark, Wicksell, Schumpeter) has removed all qualitative determinateness from the data of the circular flow and revealed the pure quantity relations, it has become ever more clear that all the general circulation theories from the classics onwards are in principle identical. In fact in the entire field of socio-economic theory the 'variation method', which seeks to determine the adjustment phenomena of the remaining variables on the hypothesis of a change in one of the data elements, is employed as the fundamental methodical principle of the general circulation theory with the same unanimity as conversely the combination of the data itself is controversial.

General approvement, however, is not a logical justification of the dominating circulation theory. In order to be justified theoretically it must have successfully passed the same axiomatic test that we must also demand of business cycle theory in particular. This means that the variation method must be incorporable into the logic of the total system without contradictions. Whether this is the case can easily be decided, if one first determines the system type to which the variation method logically belongs and then examines whether this system type is identical with the one generally applied in economic theory.

The first question can be answered unequivocally. The famous *ceteris paribus* clause of the variation method acquires complete and exclusive validity in the *closed, interdependent* system. Only if the adjustment process that is triggered by a change in a datum is not disturbed by influences from outside of the system, and only if at the same time the mutual interdependence of the data guarantees its complete reagibility with regard to the evoking change, do the movements occur as described by the variation method. The closed, interdependent system need not be thought of as fixed once and for all. Growth or decline neither affect the closedness nor the interdependency, as long as the change of the system elements occurs in a regular fashion or the adjustment of the rest of the data to an irregular change in one datum is not affected by system-external disturbances.

Does the idea of a closed, interdependent system form the basis of the system formation of economic theory in general? This question, on the answer to which