HISTORY OF ECONOMIC IDEAS

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TENDENCY TO EQUILIBRIUM, THE POSSIBILITY OF CRISIS, AND THE HISTORY OF BUSINESS CYCLE THEORIES*

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> I find myself still clinging to the idea of 'normalcy' or 'equilibrium' as meaning something, provided one recognises that that something is different in a society which is the heir of past fluctuations from what it would be in the stable society of the Continental text-books.
>
> D. H. Robertson to G. Haberler, 16 April 1936

In 1926 Adolf Löwe suggested that business cycle theories are fundamentally incompatible with the idea that the system tends towards equilibrium. Hayek, his disagreement with such a conclusion notwithstanding, recognised that the issue is central to business cycle theorizing, and agreed with Löwe that the proper way to classify business cycle theories is to examine how writers stand on this point. This paper is a preliminary attempt to take Hayek and Löwe seriously on this historiographical issue. After Löwe's and Hayek's positions are examined in context, the paper shows that Löwe's problem had been, implicitly or explicitly, at the heart of theoretical treatment since the early debates on crises. Next, the paper discusses how some crises and cycle theorists gradually switched from considering a stationary equilibrium as a theoretical norm to the idea of cyclical fluctuations as the natural state of the system, while others continued to focus on a stable equilibrium and explained movement as the result of exogenous events, frictions or mismanagements. Finally, the merits of Löwe's and Hayek's suggestions are examined in light of this dichotomy.

1. INTRODUCTION**

A METHODOLOGICAL debate that took place in Weimar Germany has recently been revisited in connection to business cycles.

Robert Lucas had interpreted his own contribution to business cycle theory as stemming from Hayek's proposition that business cycle theory should be made consistent with equilibrium theory. Later Lucas admitted he had misunderstood Hayek, but nevertheless his remark stimulated the translation of an article by Adolf Löwe, to which Hayek's statement was a response, and the re-examination of its intellectual roots and of the lively debate that followed especially in the German-speaking world.[1]

In spite of the contemporary and recent interest in these writings, one of the outcomes of this debates remained largely unnoticed. Löwe had observed that business cycle theory was, and had to be, inconsistent with equilibrium economics, so that a proper theory of the cycle would have required an altogether different kind of approach. Hayek, whose *Monetary Theory and the Trade Cycle* was to a large extent a reply to this challenge, recognised that Löwe's problem is indeed at the heart of business cycle theorizing, and accordingly suggested (taking up another of Löwe's hints) that a classification of business cycle theories should focus precisely on how the problem of the relationships of cycles and the 'normal state of the system' is theoretically encompassed. This explicit invitation does not seem to have been taken up. Histories of business cycle theories are almost exclusively based on the grouping of theories by the *cause* of fluctuations they advocate, or how they explain this or that phase of the cycle.

Yet Hayek's suggestion deserves to be taken seriously. Löwe's problem is logically prior to the identification of the specific causes of fluctuations and of the construction of a model of the cycle: it regards the *possibility* of understanding crises, their recurrence and persistency, within a system of thought; or, seen from a different angle, requires one to devise a system of thought that makes crises and cycles theoretically possible. Hayek is thus inviting his readers to look at how the perception of what cycles *are* shapes the premises of their theoretical rendition and how, in turns, these premises shape the construction of the theory.

on «Business cycles and equilibrium» and by a number of essays scattered in the 8 volumes, quickly led the discussion rather far away from the original purpose, turning the piece into an essay on the historiography of business cycle. The first set (Hagemann 2002) was reviewed for this journal by the present author (Besomi 2002a). Of the second, it is worth pointing out that besides the reprint of a number of classical contributions (rendered rather useless by the omission of the original pagination) it includes first time English translations of Cassel 1904, Schumpeter 1910, Aftalion 1808-1809, Pareto 1897, Tinbergen 1929, Frisch 1931 and Amoroso 1932. In the bibliography to this article, the items included (in full or in part) in the Hagemann and Boianovsky collections are marked with an asterisk next to the date.

[1] Löwe's 1926 article was translated in 1997, with an introduction by Gehrke 1997 describing its reception and the debates that followed. The main responses to Löwe's challenge are analysed by Hagemann 2002 (introduction to vol. IV) and Dal-Pont and Hagemann 2005. The debate is taken up, with a view to Lucas's reference to Hayek, by Rühl 1994 and 1996. Besomi 2002 refers to the debate as a source of Harrod's instability principle.

This article is a preliminary attempt to explore the implications and the potentialities of Hayek's proposal. It begins by expounding it in the context of Löwe's criticism (sect. 2). The following section offers a cursory survey of how Löwe's problem had been perceived as a logical and epistemic issue since the early stages of the theories of crises up to the most recent formulations; this demonstrates that a number of important authors were aware, long before and well after Löwe's criticism, that the issue is at the heart of theorizing on the subject. Section 4 shows, again by means of examples, how the idea that equilibrium is the theoretical norm towards which the system gravitates, was gradually substituted, by a small but growing number of authors, with the idea that the cycle itself (or, if not the cycle, then movement as opposed to a stationary state) should become the theoretical norm. Section 5 discusses the opposite viewpoint, according to which equilibrium remains the system's attractor while fluctuations are explained by some of several possible kinds of frictions, lags, errors of judgment, or mismanagement. While sections 4 and 5 focus on the early stages of cycle theories, section 6 examines in the same light some of the prevailing post-war approaches. Finally, section 7 compares merits and difficulties in Hayek's proposed method of classification of business cycle theories, concluding that the dichotomy implicit in Löwe's criticism, and well epitomized by Keynes's (otherwise rough) line dividing 'orthodox' from 'heretic' approaches according to whether or not their authors believe in the self-adjusting properties of the system, is still useful for understanding the general development of ideas in the subject, prior to the specific examination of the analytical developments which are, of course, equally necessary.

The procedure followed in this paper, consisting, as it does, of a discussion of a number of relevant examples pertaining to each of the topics examined, is of course open to the charge of partiality, as other relevant authors or schools are bound to be left out. While exhaustiveness would require a voluminous book, it is nevertheless necessary to supply abundant instances of the awareness of the epistemic nature of this problem and of both the main lines of approach that have been followed, even at the risk of an overly lengthy presentation, in order to provide convincing evidence that Hayek's suggestion is well rooted in the history of the subject and deserves to be explored further.[1]

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[1] Some of my previous work on business cycle theories and theorists has focused precisely on the points indicated here (BESOMI 1999, 2002, 2006, 2006a, 2007). I have found it extremely useful to examine the work of individual authors in the broader perspective outlined here: it will be argued below that such analyses are a complement to the broad approach presented here.

2. The possibility of trade cycle theorizing, and the history of trade cycle theory

In his article «How is Business Cycle Theory Possible at all?», Adolf Löwe diagnosed that static theoretical economics were «based on the idea of tendencies to equilibrium» (Löwe 1997 [1926], 251), and pointed out that they are, for this reason, incompatible with the trade cycle: as «the structure of a *process* which is always in equilibrium over time cannot undergo any change by definition», «movement in the sense of a change in the process form is impossible in the static system» (*ibidem*, 269). In Löwe's view, therefore,

The business cycle problem is not a reproach *for*, but a reproach *against* the static system, because in it it is [sic] 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.

(*ibidem*, 267)

In *Monetary Theory and the Trade Cycle* Hayek reported and further discussed Löwe's criticism, and admitted that

There is a fundamental difficulty inherent in all Trade Cycle theories which take as their starting point an empirically ascertained disturbance of the equilibrium of the various branches of production. This difficulty arises because, in stating the effects of that disturbance, they have to make use of the logic of equilibrium theory. Yet this logic, properly followed through, can do no more than demonstrate that such disturbances of equilibrium can only come from outside […] 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 […]. The problem before us cannot be solved by examining the effect of a certain cause within the framework, and by the methods, of equilibrium theory.

(Hayek 1933 [1929], 42-44)

Löwe maintained that this antinomy could only be solved in an altogether different theoretical framework: a «dynamic system [in which] the *total* movement of the business cycle is the quintessential movement form» (Löwe 1997 [1926], 269). Hayek regarded the difficulty as only concerning the *departure* from equilibrium, rather than the *permanence* of the system in a disequilibrium state, and attributed the source of the disturbance to monetary causes.

If Löwe had touched upon a real point, as Hayek and a number of their German-speaking contemporaries conceded by accepting the cogency of Löwe's criticism (even if they did not agree with his solution[1]), one should expect it to recur in the literature on trade cycle

[1] See the literature cited in footnote 1 to p. 54.

and crises, whether recognizedly or not. Hayek indeed went so far as to recognize that this problem lies at the very heart of trade cycle theorizing, and accordingly agreed with Löwe's suggestion that responses to this problem are the only proper key for writing an history of business cycle theories:

As Professor Löwe has correctly emphasized … the only classification [of trade cycle theories] which could be really unobjectionable would be one which proceeded according to the manner in which such theories explain the absence of the 'normal course' of economic events, as presented by static theory.

(Hayek 1933 [1929], 53-54, with reference to Löwe 1925)

Given some provisos, this proposed approach deserves, I think, to be taken seriously. Let us begin from its negative implication. Hayek and Löwe agreed that the existing accounts of business cycle theories did not provide a

theoretically satisfactory classification. The latest attempts at such classifications, by Mr. W. M. Persons, Professor W. C. Mitchell, and Mr. A. H. Hansen, show that the usual division, which relies on external features and hardly touches the solution of fundamental problems, gives too wide a scope for arbitrary decisions

(Hayek 1933 [1929], 53, with reference to Persons 1926, Mitchell 1927 and Hansen 1927)

Löwe actually specified that it would be

methodologically wrong to undertake a classification of the various theories by authors or by any concrete contents. The formal-logical methods which make up our problem must also serve as the principle by which the authors and propositions are ordered, even if this may bring some theory into a logical proximity to others that is at first sight invisible from its material content. Such a categorization [would be] based on the purely formal viewpoint of the *logical* structure.

(Löwe 1997 [1926], 253)

After the attempts at classification mentioned by Hayek, a number of others have been produced, the most eminent of which, still a classic, was Haberler's *Prosperity and Depression* (1937, updated in 1938, 1941, 1958, 1964). A few of these are book-length, some are in the form of articles surveying specific approaches or developments in certain epochs, others are chapters in treatises propounding new trade cycle theories and placing them in context with other developments, others still are parts of textbooks on trade cycles, monetary theories or the history of economic thought. Most of these histories of business cycle theories continue to follow the scheme despised by Hayek and Löwe:[1] a typical table of

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[1] A survey of these surveys would not be a useless task as it could seem at first sight, and is indeed strictly related to the history of trade cycle theories themselves. The interpretation of existing theories and their classification reflect in fact the authors' interpretation of the phenomenon (this is especially true in the case of surveys appended to, or prefacing, monographs expounding business cycle theories), but also of its relation to the existing body of theory. It

contents includes in fact chapters on monetary theories, oversaving and underconsumption theories, overinvestment and capital shortage theories, psychological approaches and so forth, later complemented with chapters on Keynesian theories, multiplier-accelerator models, real and equilibrium business cycle models, and on the exotic findings of non-linear dynamics. Little or no attention is devoted to the fundamental problem of the cycle in relation to a 'normal' (intended as a *theoretical* norm)[1] state of the system: there are, of course, a few observations along these lines,[2] but there does not seem to exist a systematic attempt to examine the history of business cycle theories in this manner.[3]

Acknowledging, however, that the relationship between cycles and equilibrium lies at the heart of the *logical* and *epistemic* problem of how a trade cycle theory should be constructed, and thus also of how it should be interpreted afterwards, is not sufficient. The notion of 'normal course' of events, in fact, is not free of ambiguity. Hayek interpreted it as a theoretical norm, that is, as the course of events described by static theory, namely, a position of stable equilibrium. Löwe insisted instead that the notion of 'normal' should itself be the subject of discussion, for if we can agree that the normal state of the system is the one conforming to the regularity described by economic theory, then there is no unique and universally agreed upon body of theory as different authors or schools express different theses regarding the prevailing tendencies of an undisturbed economic sys-

is actually often the case that an author's interpretation of someone else's theory is not very informative regarding the latter, but is very informative regarding the commentator: as a well-known example I would cite Keynes's reading of Malthus and of the mercantilists; more to the point, Mitchell's outline of the development of trade cycle theories (some passages of which are cited in section 4. 3., below) is illuminating with respect to his own approach and more generally to his epoch's perception of the cycle problem.

[1] The concept of 'normal' is sometimes interpreted as «the datum line from which we measure the 'plus and minus departures' of a variable» (Persons 1926, 96; see also Macgregor 1934, 66). This is of no interest in connection with the present essay where the focus is entirely on the theoretical norm – as clearly intended by Hayek and Löwe in the passages cited above.

[2] Equilibrium Business Cycle Theory, having made of the conciliation of equilibrium and trade cycle its scientific research programme, gives ground to some attempts in this sense: see for instance Kim 1988, who also dedicates a chapter to a cursory survey of earlier cycle theories. His «retrospective perspective» on the problem, however, which consists in molding the interwar economic thinking into the frame of today's EBCT (16), is questionable to historians of thought, as it divorces theories of the past from their legitimate context and forces them into an intellectual and factual world which is foreign to them. (See also footnote 3 to p. 72).

[3] Two relevant, albeit partial, exceptions are the attempts to survey post-war trade cycle theories in this light by Gabisch and Lorenz 1989 and Tvede (2001; for a comment in this connection see Besomi 2003). Mirowski 1985 also chose a different approach: he found the organizing principle in the logical 'stages' (not necessarily chronologically developed in the same order) necessary to construct a theory of instability. In a brief article, Kromphardt 1984 classifies post-war theories according to whether they explain the cycle starting from an inherent instability of equilibrium, or whether they assume instead equilibrium to be stable and rely on some exogenous factor. Another exception, according to Gehrke 1997, 238 and Kuznets 1930, 390, fn., seems to be Hirsch 1929.

tem (Löwe 1925, 359-360). Löwe's point is indeed that, as static theory describes a system tending to rest while the facts of the cycle show a remarkable persistence and some regularity in the fluctuations of prices, production, wages and profits, investment and other 'fundamental factors of the circular flow', one should devise a dynamic system capable of contemplating cyclical behaviour as the 'normal' state of the system (Löwe 1925, 1926). Löwe seems to have been envisaging a widening of the scope of economic theory making it capable of including among the 'normal' states of the system not only stationary equilibrium but also other kinds of behaviour. He focused, of course, on the cycle: in a dynamic system

the polarity of upswing and crisis will acquire the same status as a data constellation which the equilibrium has in the static system […]. As opposed to [the static system] in the dynamic system the *total* movement of the business cycle is the quintessential movement form. It can be deduced from the axioms of such a system with the same triviality with which Say deduced the equilibrium from the conditions of the static system.[1]

(Löwe 1997 [1926], 268-269)

Löwe's considerations thus open a number of historical and analytical perspectives and problems, inextricably intermingled and probably impossible to disentangle: for it is necessary not only to understand how trade cycle theorists interpreted the relationship between 'cycle', 'crisis' and 'normal behaviour' of the system, but also how the reference notion of 'normality' evolved under the influence of the emergence of new interpretations of the cycle and of new analytical instruments and concepts for dealing with the problem. The challenge of Löwe's antinomy consists in understanding how, if at all, the tension is resolved between an intuitive and a theoretical notion of 'normality', the former reflecting the interpretation of the facts of the cycle and the latter representing the State towards which the system would tend if undisturbed.[2]

In this respect, it is important to stress that Löwe's formulation of the problem came at a particular and very important juncture of the history of trade cycle theories. On the one hand, after a century of theoretical debates and empirical inquiries, the trade cycle was widely (though not universally) recognized as a phenomenon characterizing capitalist economies, and a number of theoretical accounts had been formulated. This forced theorists to openly reflect on the logical consistency of the whole construction. On the other hand, the era of 'high theory' was at the beginning of the end,[3] to be substituted by

[1] It will be argued below (section 6. 1.) that this notion is surprisingly modern.
[2] The latter is, of course, Hayek's meaning of the expression 'normal course' of economic events.
[3] «For the theory of business cycles the year 1936 was the Apocalypse: the revelation and the end» (SHACKLE 1967, 266).

mathematical modelling of the phenomenon which eventually made the logical syntax of the accounts being formulated more easily and rigorously recognizable.

3. Escaping from equilibrium as a logical and epistemic problem

While Löwe's statement surely was the most explicit and systematic,[1] the problem of the consistency of trade cycle theory with the postulates of the main body of theoretical economic laws troubled a number of other scholars before and after him. The awareness of this problem is indeed part and parcel of business cycle theories (and, before them, crises theories[2]) and their history. It is therefore necessary to begin with an overview of a range of logical and epistemic reflections on this issue, as this lies at the heart of these authors' understanding of the nature of cycles.

3. 1. *Say's Law and the logical impossibility of general gluts*

As early as the two or three decades following the post-Napoleonic wars the obvious incompatibility of the *facts* of crises and the *theoretical* statement of the *impossibility* of general gluts must have struck economists, as the debates on crises seem to have been mainly centred on the implications of Say's law of markets[3] on the one hand and of the character of crises on the other. One could broadly say that those who accepted Say's Law either considered crises as *partial* gluts or attributed them to external events such as wars or bad crops, while those who interpreted crises as *general* gluts rejected Say's Law or reinterpreted it.

Say's Law was widely understood as stating the *logical impossibility* of general overproductions. In the most authoritative formulation by Ricardo, in the opening paragraph of chapter 21 of the *Principles*, Say's law was called on to ensure that every product eventually finds an outlet, so that supply and demand are in principle equal:

M. Say has, however, most satisfactorily shewn, that there is no amount of capital which may not be employed in a country, because demand is only limited by produc-

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[1] «Löwe played a role analogous to that of the child identifying the naked emperor» (Rühl 1994, 175).

[2] Kuznets 1930a, 382-383 argues that the intellectual challenge of incorporating business cycle theory into general economic theory only arose late, as both crises and cycles could be attributed to external phenomena. He does not, however, attempt a survey, which will instead be the task of the present section.

[3] For the present argument it is not necessary to distinguish between the several different meanings of Say's law, as the interpretation of the law of markets (however reductive) as a statement regarding the impossibility of general gluts was quite widespread in the Classical era, and this is how it was normally referred to in connection with the problem of crises.

tion. No man produces, but with a view to consume or sell, and he never sells, but with an intention to purchase some other commodity, which may be immediately useful to him, or which may contribute to future production. By producing, then, he necessarily becomes either the consumer of his own goods, or the purchaser and consumer of the goods of some other person.

(1951 [1817], 290)

Ricardo was more precise in commenting upon Malthus: «if [commodities] are produced we contend that there will always be some who have the will and power to consume them, or in other words that there will be a demand for them» (Ricardo 1951 [1817], 314). The young John Stuart Mill was even more drastic: Say's Law is «not a deduction of probabilities. It possesses all the certainty of a mathematical demonstration for it involves in the very meaning the notion of supply and demand» (Mill 1824, 16). From the premise that production aims at satisfying immediate or remote desires to consume, and that saving only makes sense as provision of resources for investment, the implication was drawn that the system always tends to equilibrium, and any deviation from such a state could only be explained in terms of temporary maladjustments (excess of supply of some goods necessarily have to be compensated by lack of supply of some other goods), or of some disturbance external to the working of the economic system. In Say's words, «It is because the production of some commodities has declined, that other commodities are superabundant», the causes of the obstruction of circulation being «wars, embargoes, oppressive duties, the dangers and difficulties of transportation», social unrest increasing uncertainty, arbitrary exactions, jobbing and speculation (Say 1880 [1803], bk. 1, chs 15 and 16). In this conception, troubles only last while the external disturbance persist.[1] In Say's and Ricardo's view, the normal state of the economic system is characterized by an orderly equilibrium of supply and demand, that can only be disturbed by evaluation errors on the part of entrepreneurs[2] or by external events, both of which give rise to disproportions. As soon as the disturbance ceases, stabilizing forces come into play and bring the system back towards its normal state.

Any successful argument that crises have a general character had

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[1] A passage supporting this view from Say's chapter on outlets is quoted at the beginning of section 4. 1., p. 73.

[2] In his notes on Malthus, Ricardo repeatedly stressed this aspect: «What I wish to impress on the readers mind is that it is at all times the bad adaptation of the commodities produced to the wants of mankind which is the specific evil, and not the abundance of commodities» (RICARDO 1951 [1820], 306). Again: «[a glut] arises always I think from a bad selection of the object produced» (413); «the only cause of stagnation which commerce at different times experiences … may be all traced to miscalculations, and to the production of a commodity which is not wanted instead of one which is wanted» (415).

to begin by the questioning the logical stringency of the equipoise of supply and demand, *i.e.* by the postulation at the outset that the working of the equilibrating mechanism admits exceptions: otherwise, general crises would be inconsistent with theory. Accordingly, Stuart Mill felt entitled to dismiss the doctrines of Lauderdale, Malthus and Sismondi «as confused and erroneous without any attempt to examine their underlying ideas» (Hutchison 1953, 351). Yet he also recognized the recurring character of crises and gave it a place in the explanation of the overcoming of the consequences of the fall in the profit rate and thus of economic progress (Mill 1848, bk. 4, ch. 4), attributing to terminological ambiguities the contradiction between the 'absurdity' of general overproductions and their actual occurrence (1844, essay 2). If Mill did not solve the problem, he had at least the merit of having clearly seen it.[1]

3. 2. *Business cycles vs equilibrium theory*

While in this first phase of the debate the issue at stake was the recognition of the pervasive character of crises, from around the middle of the xix century the awareness of their almost periodical return was growing and spreading. In the early decades of the century some authors had already recognised some regularity in the occurrence of the crises[2] and even attempted to formulate theoretical explanations,[3] but by 1860 the phenomenon had acquired such a dimension that the Académie des Sciences Morales et Politiques offered a prize for a research on the causes and effects of commercial crises (in the plural[4]), whose winner was Clément Juglar's treatise *Des Crises Commerciales et de leur retour périodique en France, en Angleterre et aux Etats-Unis* (1862), the first full-blown treatment of the recurrence of crises.

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[1] See for instance HAGEMANN 2002; BALASSA 1959, SOWELL 1972; PLATTEAU 1985, 15-17, SKAGGS 1994, 555.

[2] Tooke's 'waves' of prices (1823; see ARNON 1991, 74-77), an anonymous report from America in 1829 (possibly by Condy Raguet) that most people believed crises to recur every 14 years approximately, John Wade's observation that the commercial cycle completes in five to seven years (1833, 21), Lord Overstone's and Longfield's descriptions of ten phases of the cycle (OVERSTONE 1837 and LONGFIELD 1840) and Hyde Clarke's suggestion of a 10-years period (CLARKE 1847; see BLACK 1992 and HENDERSON 1992) are to be cited, without forgetting that both sides in the debate between the currency and banking schools had in common the recognition of the almost periodical return of crises.

[3] For instance WADE 1933 and WILSON 1839, expanded in WILSON 1840; for a cursory discussion see JEVONS 1884 [1878a], 217, where it is stressed how the frequent recurring of crises was perceived; for a more detailed examination of Wilson's contribution see LINK 1959, 104-114.

[4] The task assigned was to «Inquire into the causes, and indicate the effects, of commercial crises that took place in Europe and North America during the xix Century. These crises have been frequent at any epoch; but, as commercial relations have expanded, the perturbations crises bring with are also touching more and more regions» (ACADÉMIE DES SCIENCES MORALES ET POLITIQUES 1860).

As crises were increasingly understood to be part of a more complex phenomenon and their causes were less and less be attributed to occasional events but were recognized as somehow systematic, the scope increased for reflections on the antinomy between the tendency to equilibrium predicted by economic laws and the disequilibrium characteristics of crises and cycles. By the early years of the xx century a number of business cycle theories had been formulated and the subject had acquired enough autonomy to become the subject of a course at Harvard (Samuels 1972). After World War I trade cycle theories had fully supplanted crises theories, and were one of the major topics of discussion in academic economics. Institutes of business forecast were founded all over Europe and the United States, and business barometers were regularly published. Time was ripe for a full discussion of the logical foundations of the new discipline.

In Germany this problem was perceived as a particularly urgent one. A number of theorists insisted on the necessity of developing a theoretical scheme capable of overcoming the limits of Say's law, which was criticized not so much for obviously being incompatible with the *facts* of crises and cycles, but for *excluding these phenomena from its own premises*.[1] The charge was therefore *theoretical* rather than regarding the adherence of the theory to the facts. Löwe's criticism thus represents the culmination of a strong tradition of reflections on the theoretical foundations of a theory of crises, which surely was inspired, in part at least, by the Marxist tradition. This deserves a specific reference, as in Marx we find an explicit discussion of the *possibility* of crisis, of its denial by Say's 'metaphysical' law of markets which assumes the conditions ensuring that crises are impossible,[2] and the awareness that these issues ought to be cleared before any attempt is developed to construct a theory of crises. A similar distinction is implicit in the logical structure of Tugan-Baranowsky's theory of industrial crises. His theory of markets, explaining by means of the Marxian schemes of enlarged reproduction how equilibrium can be disrupted and how disequilibrium generalizes to the whole eco-

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[1] GIOIA 2001, with reference in particular to LEXIS 1901, LEDERER 1925 and SPIETHOFF 1925; see also RÜHL 1994, who considers the «primary methodological problem of this period» (focusing, however, on the German developments) «how to reconcile contemporary equilibrium theory and the empirical phenomenon of a regular business cycle» (168). On Lederer in this connection see also KUZNETS 1930a, 386-387.

[2] Keynes's argument half a century later recalls Marx's: «the conditions required for the 'neutrality' of money […] are, I suspect, precisely the same as those which will ensure that crises *do not occur*. If this is true, the real-exchange economics […], though a valuable abstraction in itself and perfectly valid as an intellectual conception, is a singularly blunt weapon for dealing with the problem of booms and depressions. For it has assumed away the very matter under investigation» (KEYNES 1973 [1933], 410-411).

nomic system, was the *logical basis* for his theory of the cycle, which accounted by means of the credit mechanism how the disproportions take an almost periodic form.[1]

The problem of the inadequacy of mainstream economic theory was also perceived elsewhere. In the United States, for instance, Uriel Crocker argued that J. S. Mill's doctrine of the impossibility of general overproduction was fallacious, and that to account for production in excess of demand 'a radically different conception' of political economy was necessary (Crocker 1892, 362). Veblen, although critical of Crocker's «overproduction fallacy» (Veblen 1892), blamed current economic theory for having «proceeded by an analysis of industrial life apart from business enterprise», thereby having «sought to explain the occurrence of crises under the old-fashioned 'natural economy' or 'money economy' under which crises do not normally occur». Accordingly he commented that «the theory of crisis and depression has, as is well known, been one of the less happy passages in the economists' repertory of doctrines», and suggested that in this «accident of false start» may lie the cause of «the fact that no tenable theory of these phenomena has yet been offered» (Veblen 1958 [1904], 90). Mitchell was also aware of the problem, but opted for not discussing it: «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 how that theory can be reconciled with facts» (Mitchell 1927, 462).

In France, Aftalion explicitly sought to reconcile the facts of the cycle with Say's law which stated the *logical* impossibility of a general[2] overproduction[3] (and interpreted other cycle theories in the light of this problem):[4]

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[1] Tugan-Baranowsky 1954 [1914], 801, fn. This interpretation of Tugan is presented in Besomi 2006, where references to Marx are also given.

[2] Here the emphasis is on the adjective 'general': while Say's theory of markets (or, more appropriately translated, of outlets) excluded that overproduction originated by a lack of demand and that it could be general, it was widely admitted that partial overproductions could develop due to frictions or maladjustments. Some authors, such as Lescure 1907, argued that such partial overproductions could generalize to the entire system.

[3] Aftalion often insisted on the logically binding character of Say's denial of the possibility of a general overproduction: as «the circulation of goods finally leads to exchanging goods for goods», «an excess of production with respect to the total purchasing power is logically impossible» (see for instance Aftalion 1913, vol. 1, 273-278, 309; vol. 2, 40, 271-272, and Aftalion 1908-1909).

[4] Jeanneney's brief comment on Aftalion's reading of other trade cycle theories is quite interesting in itself. The author noted that Aftalion's distinction between the theories emphasizing problems on the supply side and those focusing instead on the demand side of the economy reflected a division line common at the time of Aftalion's writing, between those who interpreted the crisis as the inevitable price for the progress of production and those who blamed the economic system's incapacity of permitting and diffusing the consumption of

Comment concilier les difficultés logiques inhérentes à la notion de la surproduction générale avec l'assez grande généralité de la baisse des prix dont témoignent les faits?

<div align="right">(Aftalion 1913, vol. 1, 278)</div>

La conciliation pourtant doit se faire. Elle est imposé par la force des choses.

<div align="right">(vol. 2, 271)</div>

In Britain the case of Jevons is quite interesting as an example of one of the forms in which the problem was recast. The apparent conflict between his belief in the absurdity and inconsistency of the idea of general overproductions (Jevons 1911 [1871], 202-203) and his investigations on the effects of sunspots on cycles is in fact resolved by assigning a different logical status to economic theory as a deductive science and to applied economics as inductive inference whose conclusions only bear a probabilistic result.[1] Emphasis on the inductive content of trade cycle theories was quite widespread, especially on the part of those whose conclusions were strongly based on statistical inquiries, so that a related distinction referred to the abstract and ideal character of pure theory, where there are no errors of calculation, speculation or maladjustments, as opposed to the real world where uncertainty, mistakes and frictions are part of the game.

3. 3. *Statics and dynamics*

Other distinctions aimed at resolving (or at any rate reflected) the problem of the incompatibility of cycle and equilibrium, in particular by referring to the need to develop a dynamic theory along with, or in place of, statics. This was a recurrent topic in the inter-war years: not only numerous definitions were attempted, but they were made to encompass a number of other conceptual pairs such as stable/unstable,[2] micro/macro, abstract/concrete, equilibrium/disequilibrium. Numerous examples could be quoted[3] of contributions focusing on

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produce in spite of unlimited needs. He claims that in the three following decades trade cycle theorizing had freed itself from ethical and metaphysical *a priori*, and evolved new methods and priorities. As they proceeded by trying to identify the various mechanisms causing, diffusing, propagating, amplifying and eventually reversing disequilibria, Jeanneney maintains that a classification of theories should focus on the nature of the factor indicated as the main cause of disequilibrium (JEANNENEY 1945, 140).

[1] For comments see for instance MIROWSKI 1988, 46-47; PEART 1991, 257; BLACK 1981, 20-21.

[2] In this connection it is important to recall the cobweb theorem, independently formulated in 1930 by Tinbergen, Ricci and Schultz, by means of which it was possible to conceive of an unstable static equilibrium (for an account see for instance EZEKIEL 1938). On the context of the cobweb theorem see section 5. 1.

[3] A very interesting but far from complete survey is given by MACHLUP 1959. Among the

or at least touching on the problem of the limits of 'statics' so far as cycles are concerned, but two references actually suffice.

The first is a set of Kuznets's contributions to the redefinition of the scope of statics and dynamics (1930a, 1930b, 1930c), which are important for the topic under discussion in this paper because his starting point was Löwe's 1926 article, which is extensively cited and the conclusions of which are shared and defended. These papers contributed to the diffusion of Löwe's explicit criticism to statics in the English-speaking world. An echo also reached France, as one of Kuznets's articles, together with a long reply by Souter (1930), were summarized and commented upon by Weiller, who acutely observed how these debates induced a notable shift of perspective from the concept of dynamics held since Comte and J. S. Mill, for the notion of «rhythm» was added to that of «evolution» (Weiller 1943, 234-235).

All the pairs of concepts mentioned above are represented in J. M. Clark's reflections on the problem «of discrepancies between actual values and their static levels»:

> Why do prices seldom reach their supposed static level and never remain there? The answer involves the whole baffling problem of the business cycle. Among the causes of this phenomenon are, apparently, original disturbances from outside the economic system proper; such as wars or climatic cycles affecting agriculture; but the character of the cycle is more directly determined by the process through which the business system adjusts itself to these disturbing forces. Here it appears that there are not merely forces of the kind which may be described as self-limiting, but others of the cumulative sort, and that the self-limiting factors do not operate effectually until after the cumulative forces have driven things so far that a reaction is produced, which in turn goes so far as to produce another revulsion.
>
> (Clark 1927, 52)

Clark argues that for the resolution of this problem it is not possible to «start with static conclusions, add dynamic elements one at a time and make allowance for the resulting 'disturbancies' of static equilibrium», but it is necessary to «follow a more fundamental method, going back to the premises and replacing static by dynamic assumptions and then building upon them» (*ibidem*, 46). This brought him to realize that deductive methods are necessary for establishing the premises for a dynamic study (46), that «Dynamic economics cannot work successfully with the idea of one 'economic man'» but that 'collective economic personalities' should be considered instead (57-59): dynamics «will necessarily view society as an organic whole, rather than a mechanical summation of the results of the theoretical acts of independent 'free exchange'» (69).

discussions of dynamics of specific interest to the problem under discussion here (not referred to by Machlup) one ought to refer at least to MILLS 1930.

Among the other conceptual innovations being suggested with respect to our problem, Moore's «moving equilibrium» (Moore 1925, 1926, 1929) deserves to be mentioned.[1] Moore considered economic oscillations[2] as «simply the result of perturbations in a system striving, under the influence of statical forces, towards a moving general economic equilibrium» (Moore 1926, 392). Although Moore did not actually develop a proper theory of oscillations, the importance of the new concept – obtained by inserting a trend in the Walrasian system of equations – was immediately recognised by his contemporaries. A new kind of equilibrium, besides a position of rest, was now conceivable,[3] describing a more general 'normal' behaviour of the system to which fluctuations could be compared. Moore believed in the stability of such an equilibrium, and therefore interpreted fluctuations as the result of the tendency to return to the trend path after a disturbance.

The statics/dynamics issue was settled by the end of the decade by the almost universal acceptance[4] of the definitions propounded by Ragnar Frisch in 1933, according to which dynamics is a theory

that explains how one situation grows out of the foregoing. In this type of analysis we consider not only a set of magnitudes in a given point of time and study the interrelations between them, but we consider the magnitudes of certain variables in different points of time, and we introduce certain equations which embrace at the same time several of these magnitudes belonging to different instants […]. Only by a theory of this type we can explain how a situation grows out of the foregoing. This kind of analysis is basically different from the kind of analysis that is represented by a system of Walrasian equations; indeed in such a system all the variables belong to the same point of time.[5]

Frisch also explicitly discussed some of the above-mentioned conceptual pairs, such as concrete/abstract and micro/macro. What matters here, however, is that the dynamic problem was defined as the unfolding of successive states of the system, for which the only appropriate analytical instrument is the theory of functional equations.

[1] As often happens, suggestions in this sense were advanced before Moore, in particular by Pantaleoni (see SCHUMPETER 1954, 967). What matters here is that the importance of the concept was fully appreciated after Moore's *Synthetic Economics*.

[2] To be precise, in this connection Moore refers to non-periodic oscillations only; periodic oscillations (cycles proper) were discussed earlier by the same author with reference to climatic changes. This distinction is not relevant here, although it would be for an evaluation of Moore's business cycle theory. For an interpretation of Moore's cycle theory in relation to equilibrium see RAYBAUT 1991.

[3] The Marxist tradition, of course, had made such a concept available long before Moore, but in an altogether different conceptual frame.

[4] Among the relevant exceptions one should cite Harrod and Hicks.

[5] FRISCH 1933, 171-172. Frisch's definition was actually formulated by 1929 in *Nationalökonomisk Tidskrift*, but was not acknowledged in the west (FRISCH 1929). Schumpeter, however, was influenced by it: see HAGEMANN 2003, sect. III.

This had a profound influence on how the trade cycle problem was formulated from the 1930s on, and also with respect to the problem of the relationship between cycle and equilibrium theories. Frisch himself was fully aware that he was subverting the traditional approach to cycles in this connection:

The essential character of a set of equations that is dynamic in the above sense is, indeed, that it does not *lock* the system (does not stop motion) although it is determinate (*i.e.* although it contains the same number of equations as unknown). In this respect it differs from a set of static equations. The study of the evolution, in particular the study of the time shape of the curves x_t, y_t, z_t … which will follow, can, in most cases, be done directly from the nature of the structural equation without introducing any notion of 'equilibrium' values of the variables. However, although this notion may in point of principle not be *necessary* in a truly dynamic analysis, it may, even here, in many cases, help towards a simpler and clearer systematisation of the various features of the movement.

(Frisch 1936, 100-101)

3. 4. *Instability as a premise to business cycle theorizing*

More or less at the same time when Frisch was discussing these things, Harrod tackled the problem from a different angle. He was not well equipped with mathematical methods, but his logic told him that a system with a stable equilibrium is not suitable to give rise to persistent fluctuations, unless one assumes that it is subject to external shocks, internal frictions or some kind of maladjustment. Early in 1934 (well before he was able to devise a theory of his own) Harrod criticised Pigou's psychological theory of the cycle by arguing that, having assumed that equilibrium is stable, he was forced to resort to the continuous action of some abnormal factor to explain the persistence of a disequilibrium state:

So long as the equilibrium of output as a whole is regarded as stable, departures from it in one direction by conjoint action are essentially connected with the prevalence of *error*. The error is explained by an abnormal psychological condition. The continuance of recession or over-expansion depends on the persistence of error. Once the error is clearly seen the departure from the long-period position of stability is corrected. Other factors may indeed supervene upon the psychological one and be responsible for a longer duration of the departure than the psychological factor would alone account for. But to that extent the explanation of the cycle ceases to be a psychological one.

(Harrod 1934, 469)

Harrod's argument regarded the logical structure and the epistemic foundations of a trade cycle theory, as he made clear a few months later in a letter to Haberler commenting upon Keynes's *Treatise on Money*:

The orthodox view is that if a market price diverges from a natural, forces are set

up to bring the market price towards the natural. The case of interest is a special application. Ah, but in the case of credit, it may be objected, this tendency is obstructed if the banks artificially increase (or decrease) supply by their credit policy. But Keynes claims to show that the rates do not tend to converge even if the banks do *not* artificially alter the supply. Surely if that is right, it is very interesting & important. The puzzle of the cycle is that when a departure from equilibrium occurs, the system tends to move further from and not back to the equilibrium position. This movement seems contrary to the principles of supply & demand. Now if Keynes shows that these principles dont operate in the case of interest, which clearly lies at the heart of the system, it would seem that he is supplying just the very kind of explanation that is required. Why dont the principles of s[upply]. & d[emand] operate? Well, that simply takes one to the heart of his theory, where I cant go in this letter. Suffice it to say here that I do feel a lacuna in your summary, in that you do not note that there is someone professing to give – by reasoning not obviously and palpably absurd – just the *very kind of explanation* which a *rational account* of the trade cycle requires.

(Harrod to Haberler, 19 October 1934, in Harrod 2003, 304; emphasis added to the last sentence)

Accordingly, in Harrod's view instability must be incorporated in the model at the outset, precisely in order to solve the cycles and equilibrium problem. He thus embodied the instability principle twice, ensuring that the *level* of output was neutrally stable, so that dynamic forces could determine movement, and that the dynamic (moving) equilibrium[1] is unstable so that fluctuations can occur. Harrod was aware that the whole construction is somehow artificial in the assumption of the neutral stability of static equilibrium, and explicitly stated that the «change of method» he had to introduce was adopted in deference to the epistemic principle, to which he gave logical priority (for a full discussion see Besomi 1999, ch. 4)

Harrod's point was implicitly taken up by Kaldor (1940) and more explicitly by Knight (1941) and Hicks (1950), before appearing as a result of nonlinear modelling of cycles. Kaldor constructed a trade cycle model, based on the saving-investment relationship, that included the instability of equilibrium among the necessary and sufficient conditions for giving rise to persistent fluctuations: if the conditions guaranteeing instability were absent, «the 'normal' equilibrium position […] would be stable» and the behaviour of the variables «would merely lead to gradual changes in the level of activity until a position of stationariness is reached; they would not generate cyclical movements» (Kaldor 1940, 85-86). Kaldor criticised Kalecki's apparently similar model (1937, 1939) for having assumed a stable equilibrium, so that the persistence of fluctuations required the introduction of lags forcing the system to 'overshoot the mark' rather than settling on a

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[1] This is quite a different usage of the analogous concept for a disaggregated system developed by Moore a few years before.

stationary state (Kaldor 1940, 91). Kaldor's introduction of the *assumption* of an unstable equilibrium was motivated precisely because it introduced the possibility of *explaining* cycles of constant amplitude irrespective of the values of lags and parameters (92).[1]

Knight, outlining a 'methodological approach' to business cycles, pointed out that the insertion of a lag in the supply or demand mechanism, coupled with the psychology of operators, determines a 'perverse' response of the regulation mechanism. Lags in the response of consumption, and even more of production, to price changes determine 'overdone' responses; these are amplified by the tendency to mistake upward movements for trends, which induces further movements in the same direction. Equilibrium is therefore unstable, although «this cumulative tendency can operate only within fairly narrow limits» (1941, 55-56). But if we also consider the speculative demand for money, «which predominates over the real demand», the cumulative tendency «is subject to no such effective check» (58). The explanation of fluctuations lies in this general condition of instability: «In practically every economic adjustment […] conditions are present which clearly involve a tendency to oscillation» (55); thus the «cycle does not call for any concrete causal explanation beyond the fact of unstable equilibrium itself» (56).[2]

Hicks's *Contribution to the Theory of the Trade Cycle* also consists in a model incorporating instability at the outset. Having noted that the possible solutions of linear functional equations are either exponential growth or decay, or fluctuations at an increasing, decreasing or constant amplitude, Hicks discarded all configurations of parameters except one as inadequate to explain the elementary facts of the cycle, in particular its persistence. Growth or decay obviously do not represent cycles; constant amplitude fluctuations rely on a special set of parameters, but «it would be an extraordinary coincidence if we lived in a world which had got stuck for two centuries with an investment coefficient which was always equal to this precise value! The chances against a hypothesis of this sort are quite overwhelming» (Hicks 1950, 89). Damped fluctuations, kept alive by exogenous shocks (as propounded by Frisch 1933 and taken up by Kalecki 1939 and 1954),[3] are

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[1] For a discussion of Kaldor's position in relation to Kalecki's struggle to solve the problem of the relation between cycles and the stability of equilibrium see Besomi 2006a.

[2] Or again: «Too much attention has been given to this problem of the cause of the collapse. The essential fact is merely the unstable equilibrium. As already noted, we do not try to find out what particular cause upsets an object balanced upon a sharp point or a knife edge» (Knight 1941, 60).

[3] For a discussion of the context of such a proposal see below, section 5. 2. On Kalecki see Besomi 2006a.

not satisfactory either, for «the current level of output will be mainly a matter of what the recent shocks have happened to be – that is, it will be mainly a matter of chance». This assumption is thus rejected on *epistemic* grounds: it «leaves us with fluctuations which are mainly random – with fluctuations, that is, which remain unexplained» (Hicks 1950, 91). The remaining alternative is the hypothesis that the coefficients must be such as to determine instability in the system, which is prevented from exploding by limits to excessive growth ('ceilings' and 'floors'). In spite of seeming at first «quite ridiculous» (91), this assumption belongs to the basic sets of Hick's hypotheses (95), precisely because it is the only one with «direct power to explain what happens» (94).

The problem was taken up again by Goodwin who, in a series of contributions beginning in 1951, clearly saw that the problem lay in the assumption of linearity at the basis of the Frisch and Kalecki models, and that Harrod and Hicks were instead struggling with non-linearities. Goodwin's rejection of linear approaches is based on their intrinsic impossibility of explaining persistent fluctuations unless resorting to an external source of maintenance; he concluded that «in this sense [a linear theory] is not a complete theory» (Goodwin 1951, 1-3). The conclusion (with respect to our problem of the relation of cycles and equilibrium) to which Goodwin's studies led is that

What is required is that the equilibrium must be unstable, so that the system will *never* be found there or be found approaching it. For large values of the variable the system must be asymptotically stable. It follows then that there must be at least one closed orbit separating the stable from the unstable region. One thus has a stable equilibrium motion, towards which the system is always tending and which is a generalisation of the idea of a stable equilibrium point (the notion had been developed long ago by Poincaré).

(Goodwin 1989, 250)

In this view, permitted by nonlinear dynamics, cyclical fluctuations themselves become an equilibrium motion, a state or better, a process towards which the system tends, while the 'old' equilibrium (the stationary state where the system reproduces its own conditions, whether of simple or enlarged reproduction, to use Marxian terminology) must be conceived to be unstable in order to permit fluctuations.

3. 5. *Equilibrium back at the center of the stage*

While Goodwin and nonlinear dynamics were exploring the dynamics of cycles and of more complex creatures (strange attractors, chaotic dynamics, catastrophes) whose existence is made possible by the instability of stationary states, neoclassical economics also returned

to the epistemic aspect of the original problem of the relation of cycles and equilibrium, but completely reversing the perspective.

Lucas honored Hayek as his intellectual ancestor (1977, 8)[1] by approvingly quoting (7) Hayek's view that «the incorporation of cyclical phenomena into the system of economic equilibrium theory» (by which Hayek meant «the modern theory of the general interdependence of all economic quantities, which has been most perfectly expressed by the Lausanne School of theoretical economics»), «with which they are in apparent contradiction, remains the crucial problem of Trade Cycle Theory» (Hayek 1933 [1929], 33, fn. and 42, fn.). Lucas's argument explicitly tackles the logical and epistemic part of the issue: his emphasis is on the effort to «explain business cycles» (Lucas 1977, 8, twice, and *passim*), for which the elaboration of microeconomic foundations to macroeconomics is a necessity.[2] The alternative approach, attributed to the Keynesian revolution coupled with Tinbergen's modelling, is charged with having «altered the meaning of the term 'theory' to such an extent that the older business cycle theories could not really be viewed as 'theories' at all» (11). This approach, however, is not an 'equilibrium theory' in Hayek's sense, and indeed Keynes stated that «an equilibrium theory was unattainable» (11-12). But the construction of a proper theory requires that the «'apparent contradiction' between 'cyclical phenomena' and 'economic equilibrium'» is resolved at its root, by formulating «an explicit, equilibrium account of the business cycle» (12-14).[3]

<div align="center">

4. FROM CRISES TO CYCLES.
OR, FROM THE 'NORMALITY' OF EQUILIBRIUM TO
THE 'NORMALITY' OF FLUCTUATIONS

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Whether or not theorists explicitly recognized the relationship between cycles and equilibrium to be an epistemic or a logical problem,

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[1] It has been pointed out (ARENA 1994, HOOVER 1988) that generally speaking Lucas's interpretation of Hayek is based on an inaccurate reading of the Austrian author; LUCAS 1994 admitted the charge (for a discussion see HAGEMANN and DAL-PONT 2005 and RÜHL 1996). His considerations, however, are right to the point discussed in these pages.

[2] «Whether or not [modern economists accept Hayek's statement], I wish in this essay to argue that it *should* be so» (LUCAS 1977, 8).

[3] Lucas's characterization of Hayek's statement as a 'leading example' of a proposition on which «there was wide agreement» among interwar business cycle theorists (1977, 8) is therefore not a statement of fact, and is, in fact contradicted by the opinions cited in this paper. Kim, in his 'historical perspective' on Equilibrium business cycle theories, describes the history of business cycle theories in the inter-war years as «the history of theoretical attempts to incorporate business cycles into classical equilibrium theory» (1988, 80). This perspective could be easily reversed, and the history of the subject examined in terms of the attempts to *escape* equilibrium theory. The most suitable approach, which is proposed here, is probably to examine the history of the subject in terms of the clash between those who sought to harmonize cycles and equilibrium, and those who rejected the attempt as epistemically improper.

they often offered neatly drawn considerations on the cycle in relation to what is 'normal' in the working of the economic system. A cursory enumeration of some such statements developed up to and around the time Löwe was writing already indicates some lines of approach and 'philosophies' followed in trade cycle theorizing, thereby giving some inklings as to the fruitfulness of Hayek's and Löwe's suggestion to look into this problem for a key to the history of such theories.

4. 1. *Crises* vs *normality*

Let us begin with J. B. Say. In his chapter «Des Débouchés» (Say 1880 [1803], bk. 1, ch. xv) he writes:

precisely at the same time, that one commodity makes a loss, another commodity is making excessive profit. And, since such profits must operate as a powerful stimulus to the cultivation of that particular kind of products, there […] needs be some violent means, or some extraordinary cause, a political or natural convulsion, or the avarice or ignorance of authority, to perpetuate this scarcity on the one hand, and consequent glut on the other. No sooner is the cause of this political disease removed, than the means of production feel a natural impulse towards the vacant channels, the replenishment of which restores activity to all the others. One kind of production would seldom outstrip every other, and its products be disproportionately cheapened, were production left entirely free.

<div align="right">(Say 1880 [1803], 57)</div>

In this passage gluts are qualified as temporary and partial exceptions to the state of things which would rule if the system were left entirely free; the term used, a 'political disease', indicates both the cause of the disturbance, extraneous to the proper working of the system, and an abnormal state of 'illness', as opposed to the healthiness of a free system. The medical metaphor, besides distinguishing what is normal from what is not, also carries a value judgement and policy advice.

On the opposite side we find Marx, who thought that «equilibrium is itself an accident» (1867-1894, vol. 2, ch. 21:1:1). In his writings, the possibility of crisis inherent in the separation of buyer and seller turns into actuality whenever the profitability of the production process is jeopardized. On the other hand, equilibrium must be re-established, otherwise the system would break down. Crises are the violent and sudden process of destruction of capital (accompanied by mass unemployment) by means of which the profitable conditions of production are temporarily reinstated.

4. 2. *From the pathology of crises to the physiology of cycles*

The medical metaphor is, perhaps naturally, used also by Juglar who was trained as a medical doctor, but in a sense somehow different

from Say's. Juglar points out that the word 'crisis' indicates a disease or sufferance, but also indicates that in spite of being opposed to a 'healthy' state it is part of the physiology of the system, as it is necessary to remedy the «excesses» and «abuses» of credit that build up in the prosperity phase, and that crisis is a temporary state:

> Les crises, commes les maladies, paraissent une des conditions de l'existence des sociétés où le commerce et l'industrie dominent. On peut les prévoir, les adoucir, s'en préserver jusqu'à un certain point, faciliter la reprise des affaires; mais les supprimer, c'est ce qui jusqu'à ici, malgré les combinaisons les plus diverses, n'a été donné à personne. Proposer un remède à notre tour, quand nous reconnaissons le peu d'efficacité de ceux des autres, n'était pas possible, d'autant que leur évolution naturelle rétablit l'équilibre et prépare un sol ferme sur lequel on peut s'appuyer sans crainte pour parcourir une nouvelle période.

> (Juglar 1862, vii, and 1889, x)

The 'normal' state of an economic system is prosperity:

> Dans le langage vulgaire, la période prospère n'a pas de nom; c'est ce que l'on regarde comme l'état normal, on n'en parle même pas; il en est de la prosperité comme la santé, rien ne paraît plus naturel.

> (Juglar 1889, 16)

The three phases of the cycle recognised by Juglar, prosperity, crisis and liquidation, thus correspond to the normal state of health, a pathological state caused by exaggerations and abuses, and a process of recovery during which the impurities are eliminated and the return to normality is prepared. The cycle is therefore an alternation of normal states, punctuated by crises and subsequent recoveries.[1]

A similar expression, «malady of commercial crisis» as opposed to prosperity as «the healthiest period of our commercial life» – a state characterized by «healthy confidence» which eventually degenerates «into the disease of a too facile faith» – was used by John Mills (1868, 17, 24 and 27). Mills pursues the medical metaphor even further, mocking 'homeopathic' remedies to crises: «It is exactly the relative excess of promises over the power of performance that is the most salient symptom of the disease; a proposal to add more promises to the number is not a very hopeful application of the doctrine similia similibus curantur» (*ibidem*, 36; on p. 29 there is a further reference to the distinction between symptom and disease).

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[1] «The medical man, who survived in him, there rediscovered the metaphor of the illness, the pains generated therefrom, the elimination without which good health is not restored» (Beauregard 1908, 156, cited and further discussed in Groenewegen 2001, 118). The metaphor is taken up and extended by Wolowski, in his commentary on Juglar's book: «Ce n'est pas sans raison que le corps social est souvent comparé à l'organisme de l'homme: il a ses maladies et il doit avoir son hygiène. Cette hygiène à ses règles et ses lois, et M. le docteur Juglar a su très habilement les exposer» (Wolowski 1862, 482). For a more detailed discussion see Besomi 2007a.

In the early years of the xx century, when the idea that crises are not exceptions but the rule and that they recur with some regularity, the notion of 'normality' was also changing.[1] With Wicksell, for instance, the medical metaphor takes another turn: «We have now studied trade and circulation in its normal state, its physiology as it were. What remains is to take a look at its pathology, and especially its acute disturbances which are, by a term borrowed from medicine, called crises (κρίσις = decisive struggle)» (Wicksell 1902-1905, 356). We still have the opposition between a 'normal' and a 'pathological' state, but cycles and crises have acquired a different status. While crises are the acute disturbance, which are not the product of some inherent logical necessity but of a number of miscalculations on the part of both entrepreneurs and bankers, the alternation of 'good' and 'bad' time is a *necessary* consequence of a growing economy: the expansion of production, due for instance to population growth, would meet decreasing returns, unless accompanied by technological progress. This, however, is not a smooth process: inventions come in at irregular intervals, and the necessary premises for their introduction into the productive process are met only when producers become optimistic and when a sufficient amount of stocks has been accumulated during depression that can easily be converted into liquid capital. The depression thus needs to be reinterpreted:

Bad times are certainly to some extent a reaction to good times, especially from a psychological point of view. But bad times are to a far greater extent the necessary prerequisite for good times, and to the extent we recognize this, the former lose, both theoretically and practically, their 'bad' character. Overproduction no longer appears as a frightening spectre, an evil which must be avoided at all costs, but rather as a good and a necessary condition which ought to be encouraged as much as possible.

<div align="right">(Wicksell 1953 [1907a], 67-68)</div>

Wicksell believed that the economic system is stable, and that fluctuations would eventually die out. But the recurrence of inventions would give the system new kicks, which the internal structure of the economy would turn into an almost periodical fluctuation.[2] The cycle is still seen as a pathology, but its occurrence is a necessary form taken by the physiology of an economy in progress.

Oppenheimer (1911) pointed out that crises have always been understood as a disturbance to the normal economic process and quali-

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[1] This is not to say, of course, that there were no earlier examples, as the already cited conclusion of J. S. Mill that crises are necessary to the accumulation process.

[2] This was illustrated by means of the well-known metaphor of the rocking horse (see BOIANOVSKY 1995, 399-400), which was later taken up and developed by Frisch (see section 5. 2., below).

fied as diseases, in contrast to the healthy state of the economy. Yet this 'normal' state has not been well defined, if not negatively as the lack of crises, so that economists did not really have a 'normal' state with which to compare the abnormal ones. Oppenheimer reviews some preceding discussions of the relationship of normality and crisis, with special reference to Fischer (1911), Pinkus (1906) and Sombart (1904). His remarks on Sombart are particularly interesting. Sombart posed the problem but believed it not to be solvable: he maintained that the normal capitalistic process inevitably leads to crises, which are thus understood as its normal consequence precisely as a hangover is the ordinary after-effect of an alcohol poisoning. But this leaves the problem of whether alcohol intoxications are normal, revealing that Sombart mixes up two notions of 'normal': the regular operational sequence and repetition of features of the same kind, and on the other hand a 'correct', ideal process. Fischer is also guilty of the same confusion, and his methodological conclusion that the determination of the 'normal' can only be a result of a theory of crisis, but never its condition (Fischer 1911, 12), is, in Oppenheimer's view, untenable. Oppenheimer argues that this would be analogous to the case of an isolated island, where everyone including the only physician suffers of malaria: the physician's determination of the 'normal' state would reflect the island's condition. He thus concludes that inquiries on crises need an accurate picture of the normal, healthy state as a basis for comparison.

Spiethoff, whose theory of crises deeply influenced Wicksell's explanation of cycles (see Wicksell 2001 [1907b], 335 and 1934-1935 [1906], 209), concluded that in a capitalistic system in which entrepreneurship is driven by the profit motit, «Overproduction is part and parcel of highly capitalist production; in a system based on money, this overproduction must lead to a downswing».

> The 'normal state' is neither the upswing, nor the downswing, nor, least of all, the crisis. What is normal in a free, highly capitalist market system based on money, is the business cycle.
>
> (Spiethoff 1953 [1925], 166)

Accordingly, disequilibrium is not a pathological state, but a feature pervading all the phases of the cycle:

> Lack of balance occurs not only in the overproduction which brings about the change from prosperity to depression; it characterizes the entire business cycle. The balanced profitable production mentioned above as a contrast to overproduction is a phenomenon of short duration occurring only at the height of prosperity. Perhaps indeed it is only an ideal which is never actually realized.
>
> (Spiethoff 1933, 515)

The crisis, on the contrary, is for Spiethoff, the result of «breaches of the rules»: it is «that period during which an unhealthy economic state of affairs is suddenly and violently transformed. This unhealthy state is overspeculation linked with overexpansion of credit» (Spiethoff 1953 [1925], 93 and 80).

4. 3. *Fluctuations as the normal state of the system*

Statements such as Spiethoff's regarding the 'normality' of a cyclical development became more and more frequent in the literature.[1] Mitchell, for instance, strongly emphasized that «the 'normal state of trade' [is] a figment'»: business annals show that «the only normal condition is a state of change», «the theorist's 'normal state' … is not to be looked for in an historical record[, n]or can we take for granted the existence of a moving 'normal state of trade' of such a nature that departures from it tend to correct themselves» (Mitchell 1927, 376).[2] Hawtrey also argued that «Trade is never normal; in times of average prosperity it is always on either the upward or the downward path; when it ceases to improve it is on the verge of collapse, when it ceases to slacken it is beginning to recover» (Hawtrey 1913, 75-76).

Veblen similarly wrote that «Crises, depressions, hard times, dull times, brisk times, periods of speculative advance, 'eras of prosperity', are primarily phenomena of business. …The true, or what may be called the normal, crises, depressions and exaltations in the business world are not the results of accidents, such as the failure of crops. They come in the regular course of business» (1904, 88-89). But he went a step further. Having noticed that since the last quarter of the XIX century crises and depressions had become more accentuated, he concluded that «with increasing persistency, chronic depression has been the rule rather than the exception in business», and accordingly reversed the traditional explanation which saw the cause of crises in external events and suggested that «Seasons of easy times, 'ordinary prosperity,' during this period are pretty uniformly traceable to specific causes extraneous to the process of industrial business proper […]. If the outside stimulus from which the present prosperity takes its impulse be continued at an adequate pitch, the season of prosperity may be prolonged; otherwise there seems little reason to expect

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[1] The earliest statement I have found so far is by Arthur Ellis (whose endeavour was «to preach not political economy, but business»: 1879, v): «There are waves in prices in a normal state of trade» (159). Significantly, his book is titled *The Rationale of Market Fluctuations* (Ellis also used, well ahead of other authors, the expression 'trade cycle').

[2] These passages will be further discussed at the end of this section.

any other outcome than a more or less abrupt and searching liquidation.» (*ibidem*, 120)

The notion of the 'normality' of fluctuations was also shared by Robertson. Already in 1915 he had pointed out that, besides errors of judgement and miscalculations, the conditions determining the rational inter-temporal optimization of the level of investment were changing in the course of the cycle, being subject to both unidirectional change (as a consequence of technological progress) and of fluctuations in the degree of productivity during the cycle. Equilibrium is thus not stationary, but itself oscillatory. This notion was further specified in *Banking policy and the price level* (1926), where Robertson distinguished between «justified fluctuations», *i.e.* the fluctuations due to changes in the equilibrium conditions, and ampler fluctuations attributed to errors and frictions and to mistaken banking policies. Robertson considers the cycle as the necessary price to pay for economic growth: «To a large extent, in the writer's view, fluctuations in the desirability of acquiring instruments are the inevitable penalty of industrial progress» (1926, 94); it is therefore necessary to treat the cycle «not as a passing rash on the fair face of a static equilibrium but as a deep seated functional disorder of the endocrine glands which control the state of organic growth» (Robertson 1933, 241).

Johan Åkerman saw the cycle as part of the natural rhythmic of economic life. He summarized as follows the nature of the problem to be explored:

The nature of changes. In stead of the classical doctrine of equilibrium and the theoretically unimportant factors, disturbing this equilibrium it is proposed to use the wave or the sin[e] curve as standard. The frame of the investigation is thus formed by periods of different length and amplitude. Just as Fourier's theorem states that every sort of curve can be resolved in a number of sine curves, all economic changes may be reconstructed as a sum of periods of different length.

<div align="right">(Åkerman 1928)</div>

The transition was summarized by Mitchell at the eve of the war in an interesting passage, revealing the point of view of one of the participants in the process convinced that an almost natural progress from a partial to a general kind of explanation was under way, with his implicit judgement that this is an improvement:[1]

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[1] Jevons, more drastically, mocked «the variety of the explanations offered by commercial writers concerning the cause of the present state of trade. Foreign competition, beer-drinking, overproduction, trade-unionism, war, peace, want of gold, superabundance of silver, Lord Beaconsfield, Sir Stafford Northcote, their extravagant expenditure, the Government policy, the Glasgow Bank directors, Mr. Edison and the electric light, are a few of the happy and consistent suggestions continually made to explain the present disastrous collapse of industry and credit» (JEVONS 1884 [1878b,], 221). Juglar also frequently insisted on the necessity of looking for what predisposes the system to crises rather than blaming the specific incident

Wide divergences of opinion continue to exist among competent writers upon crises; but in recent years substantial agreement has been reached upon two points of fundamental importance.

Crises are no longer treated as sudden catastrophes which interrupt the 'normal' course of business, as episodes which can be understood without investigation of the intervening years. On the contrary, the crisis is regarded as but the most dramatic and briefest of the three phases of a business cycle – prosperity, crisis, and depression. Modern discussions endeavor to show why a crisis is followed by a depression, and depression by prosperity, quite as much as to show why prosperity is followed by a crisis. In a word, the theory of crises has grown into the theory of business cycles.

The wider grasp of the problem has discredited the view that crises are due to abnormal conditions which tempt industry and trade to forsake their beaten paths and temporarily befog the judgement of business men and investors, or to misguided legislation, unsound business practices, imperfect banking organization, and the like. As business cycles have continued to run their round decade after decade in all nations of highly developed business organization, the idea that each crisis may be accounted for by some special cause has become less tenable. On the contrary, the explanations in favor today ascribe the recurrence of crises after periods of prosperity to some inherent characteristic of economic organization or activity. The complex processes which make up business life are analyzed to discover why they inevitably work out a change from good times to bad and from bad times to good. The influence of special conditions is admitted, of course, but rather as a factor which complicates the process than as the leading cause of crises.[1]

(Mitchell 1913, 5-6)

Mitchell is describing, and fully supporting, the tendency to abandon the idea that equilibrium is the normal state of the system and that fluctuations are just accidents and to introduce in its stead the conception that other kinds of behaviour are not only admissible but in turn 'normal'. At this juncture, Mitchell had in mind cycles only: but soon other, wilder kinds of movement became conceivable and suitable to treatment. Before examining these developments, however, we should pause on the then mainstream currents of thought that have never abandoned the idea that the driving force of the system is the tendency towards equilibrium.

bursting forth each individual crisis (see, e.g., Juglar 1862, iii, ix-xi, 1-2, 5-6, and *passim*; 1889, 5, 27-29, 36 and *passim*).

[1] Similar considerations were expressed by Patterson, in one of the first English-language attempts of classifying crises theories: «Explanations of crises may be divided conveniently into two general groups. The first includes those which find that each disturbance is due to some special cause. Modern industry is viewed as in a state of stable equilibrium. This condition is 'normal' and tends to continue, but numerous influences, which are for the most part unpredictable, are apt to disturb it and bring about an 'abnormal' situation. Crises thus have little or nothing in common except their abnormality. They are pathological phenomena and each has its special, unpredictable cause. When the cause has been removed or when the industrial structure has become adjusted to its presence, disturbances disappear and a state of normality again exists.» But this method of accounting for crises […] overlooks or chooses

5. Frictions, maladjustments, discontinuities, mismanagements, miscalculations, shocks and lags

On the front of the scholars who conceived of the cycle as *the* 'normal' phenomenon, exogenous disturbances were changing their theoretical status. While they were seen by most classical economists as the cause of crises, they were now advocated to explain the historical differences between cycles, as an antidote to the excess of regularity postulated in the explanation of all cycles in terms of a common cause or set of causes.[1] Yet this view was far less generally accepted than the passage by Mitchell cited at the end of section 4. 3. would suggest. Important currents of thought maintained in fact that rather than being the 'norm', cycles are (temporary) deviations from (or oscillations around) equilibrium, a state towards which the economic system would tend if undisturbed and if only economic forces could freely act.[2]

These authors reconciled the facts of the cycle with the system's tendency towards equilibrium by allowing impediments or external influences to prevent the system from actually settling into its 'natural' state.[3] The inventory of possible frictions and imperfections advocated for this purpose is quite rich, and a sample of a few representative ones must suffice.

Taussig supplies a clear example of this attitude. «The causes of the larger oscillations […] are to be found partly in the division of labor and the time-using or capitalistic method of production», which introduce «a possibility of mistake and maladjustment, and also the possibility that maladjustment will not be promptly ascertained»,[4] and partly in the psychology of entrepreneurs and middlemen. An al-

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to ignore a certain regularity in the appearance of crises – a regularity that may be significant and call for explanation». Yet, «to most writers in recent years, there seems to be a distinct uniformity in the intervals between crises and in certain of the accompanying phenomena. This has led to the suggestion that there may perhaps be formulated a law of their periodicity. It is said that crises tend to recur and that prosperity, crisis, and depression succeed each other with such regularity as to warrant the use of the word 'cycle.'». This second sort of explanation is in turn subdivided into three main branches: the psychological theories, those relying on the regularity of external events, and those «placing the emphasis upon the structure of our modern economic life» (Patterson 1915).

[1] This shift of perspective was even taken as a criterion for classifying trade cycle theories, according to the mathematical and statistical rigour in the separation of the causes of secular, cyclical and random changes (Moore 1923, 2-5).

[2] For an exposition of «The problem of business equilibration» in these terms see Mc-Cracken 1933, ch. xvi.

[3] For a general discussion of this point see Lachmann 1939.

[4] One can thus say that «one great cause of the industrial crisis» is «ill-adjusted production»: Taussig 1921, 390-391.

ternation of «a pervading spirit of optimism […] in times of activity» and «a spirit of pessimism […] in time of depression» is unleashed by «some new event – a good crop, the unexpected profitableness of a fresh venture, a turn in foreign trade»,[1] capable both of ending prosperity and starting the revival. Once these waves are off the ground, they spread not by mere contagion but because of the 'real interdependence' of businesses (393-396). The elasticity of credit, by permitting expansions of capital beyond what «the accrued savings of the community make possible», «both facilitates and conceals an overshooting of the mark»: it is precisely because the excess is concealed that the self-correcting mechanisms fail to work (398-399).

The premises of Irving Fisher's debt-deflation theory further illustrate the point. He starts from the assumption that «ordinarily and within wide limits, all, or almost all, economic variables tend, in a general way, toward a stable equilibrium». Yet there «may be equilibrium which, though stable, is so delicately poised that, after departure from it beyond certain limits, instability ensues». Equilibrium is constantly disturbed by excesses or shortages in almost any variable, giving rise to small oscillations; most of these, on their own, are incapable of explaining major fluctuations, except for over-indebtedness and deflation, which set up serious disturbances and amplify the effect of other disturbances (Fisher 1933, 338-340). These major disruptions of equilibrium can «be started by many causes, of which the most common appears to be *new opportunities to invest at a big prospective profit*», given for instance by «new inventions, new industries, development of new resources, opening of new land or new markets» (348).

Another example is Hayek's development of the Austrians' approach. He also believed that the system tends to equilibrium and that the cycle results from the disruptions caused by monetary factors and the tendency to adjust to the new conditions. He argued that after an initial departure from equilibrium has occurred, the cycle is maintained by the play of the fundamentally stable forces tending to bring the system back to equilibrium but failing to do so effectively as the individual agent's expectations are systematically frustrated by «'wrong' prices» (Hayek 1933 [1929], 84-85).

In Italy a number of authors also treated the crisis, *i.e.* the general overproduction, as «a disequilibrium of production and consumption» and considered it as a «'deviation' from the 'normal path' of economic life, *i.e.* from the path dictated by the natural laws which, if free to act, would realize equilibrium and progress» (Gobello 1937,

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[1] «The immediate occasion of the turn one way or the other, seems to rest on accident—that is, on irregular and unpredictable causes» (TAUSSIG 1921, 395-396).

196, surveying the theories of Ferrara, Della Bona, Montemartini, Cognetti de Martiis, Ottolenghi and Supino).

5. 1. *The passing of time*

The list of possible factors disrupting equilibrium is almost endless: «the discrepancy between saving and investment», «over-production, under-consumption, over-capacity, price-dislocation, maladjustment between agricultural and industrial prices, over-confidence, over-investment, over-saving, over-spending» and «over and under everything else» (Fisher 1933, 340, 339). Various vertical and horizontal maladjustments and disproportions could be added to this list: imperfections in competition, the fixity of fixed capital, lack of perfect information, political interferences and numerous others. One of the most prolific originators of disturbances to the equilibrating tendency deserves to be mentioned for its prospective importance – the influence of the passing of time.

A first aspect was discussed by Rosenstein-Rodan, who pointed out that equilibrium theory assumes that adjustments between supply and price movements, and demand and prices, have the same speed; this, however, is not normally the case, and may bring relevant consequences. It can either set up damped oscillations tending towards equilibrium, or persistent oscillations (Rosenstein-Rodan 1934, 91), as described in the cobweb cycle. Moreover, if there is a change in external data while the adjustment is ongoing, cumulative effects could take place generating other possible outcomes, such as e.g. changes in the equilibrium position towards which the system will eventually tend but also persistent cycles or increasing disequilibria (95-96).[1]

Also the troubles evidenced by Pigou originate in the lapse of time. Pigou began by distinguishing the role of initiating impulses (which can be of a 'real' or psychological nature) and the «complex of industrial and monetary conditions» which «determine the nature of the effects that it produces, and are, in this sense, causes of industrial fluctuations» (Pigou 1927, 8).[2] 'Real' impulses, consist in the «changes that have occurred, or are about to occur, in actual industrial conditions» (*ibidem*, 30), with reference to crop variations, inventions, discoveries of mineral deposits, industrial disputes, changes of taste or in foreign demand. If these real impulses only operated in a «state peopled exclusively by perfectly intelligent persons» (*ibidem*, 31), thus apart from

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[1] Rosenstein's argument, as previously formulated in a German version of this part of his article, is taken up by Kuznets 1930.

[2] On the 'impulse' and 'propagation' distinction see section 5. 2.

their influence on people's psychology, their effect would die out (ch. v).[1] But the time necessary to implement production decisions, which varies considerably from one branch of business to another, gives scope for errors of forecast; and, because of the cumulative and general implications of the psychological reactions to mistakes, these errors can be invoked to explain large and systematic deviations from equilibrium. Psychology also explains how the direction of errors is suddenly reversed as soon as confidence is shaken.[2]

5. 2. *Time lags and cycles*

Time lags are the most interesting time-related friction, as they have found diverse uses in this connection. The first influential attempt was by Albert Aftalion, who reconciled cycles with his belief in the economic system's self-adjusting tendency by considering the effect of the time necessary to build capital equipment. Entrepreneurs decide the volume of production by examining price movements; if prices are growing they infer that demand is also growing larger than supply, and accordingly increase production. But the implementation of such a decision requires time, during which demand keeps running ahead of supply, sending further wrong signals to entrepreneurs; fluctuations are precisely the result of the system's tendency to correct disequilibrium (Aftalion 1913; for a discussion see Dangel and Raybaut 1997). A similar argument was put forward by Mentor Bouniatian, although it only played a subordinate role in his explanation of the cycle (Bouniatian 1908, 120; 1927, 132-133; 1924, 665-666; 1928, 68-69; 1934, 4; for a discussion see Besomi 2007). Both Aftalion and Bouniatian were preceded by Charles Conant. This author pointed out that as the relation between production and consumption are constantly changing, due e.g. to changes in fashion, tastes or technology, so that there is much scope for miscalculations. Among the several systematic causes of maladjustment that reside in the 'modern organization of industry',[3] Conant focused on the long construction

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[1] As Harrod pointed out, in the relevant passages Pigou described «the economics of a stable equilibrium with admirable lucidity» (1934, 469. Pigou's psychological theory was Harrod's first critical reference point in the elaboration of his epistemic premise to trade cycle theorizing: see section 3. 4., above).

[2] Psychological explanations of the cycle originated of course well before Pigou. He quotes many passages in his book of authors who expounded considerations similar to his own. Indeed, the first British attempted full explanation of the cycle as such, by John Mills (1868; cited by Pigou only second hand), relied on psychological mechanisms. On some American contributions, see BARNETT 1941, ch. II.

[3] Conant also mentions the sub-division of labour, which makes it difficult to for the correct expectations as to the price that can be realized for one's own product, and the fixity of fixed capital (1901, 375-357).

period of plants («months, and in many cases, years»). From order to actual production there is a considerable delay, during which «demand runs a little ahead of the supply»; if «considerable profits are realized for a brief period, some capitalist, tempted by the profits, puts up another mill and the supply again threatens to become excessive» (Conant 1901, 376).[1]

The full development of the potentialities of time-lags in the generation of cyclical movements took place in the hands of the 'econometricians', who framed the problem in terms of models of linear difference or mixed difference-differential equations,[2] whose unknown is a function (or a system of functions) representing the development in time of a variable (or a set of variables). The solution is in the form of an exponential function, possibly combined with (or substituted by) sine oscillations; the latter would not be possible in first-order systems, were it not for the lag. This can give rise to monotonic growth, monotonic decrease, amplified fluctuations, damped fluctuations, or fluctuations of a constant amplitude, according to the parameters' values. The latter case, of oscillations of constant amplitude, lies on the border line between amplified and damped fluctuations, and as such is an infinitely improbable case.

In such a framework, characterized by the assumption of linearity, the development of trade cycle theories only had two choices. The theoretically weakest one consisted in assuming such values of the parameters making the amplitude of the oscillations constant; but there is no reason at all why economic systems should be precisely characterized by that configuration of parameters.[3] The alternative was to assume that oscillations are damped, but kept alive by exogenous shocks. In both cases, the fluctuations are symmetric, both in terms of appearance (a sine curve) and of causation: there is no other

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[1] Barnett interpreted in a similar way the contribution of George Dixwell, who maintained that the gestation lag implies that, as workers are engaged in the construction of capital goods, they exert demand of consumption goods, giving rise to an unbalance: prices of consumption goods increase; this further stimulates production of capital goods during the period of gestation (BARNETT 1941, 63-69, with reference to DIXWELL 1881, 8-9). Although this interpretation is not incompatible with Dixwell's wording, the reference to the lag is not explicit as it is in Conant.

[2] A typical model, eventually giving rise to endless variations, was given by the interaction of the multiplier and the acceleration principle. The idea was first explicitly expounded, although not in mathematical form, by Harrod.

[3] I recollect only one such attempt, devised by KALECKI 1935. It was immediately criticised for this reason by FRISCH and HOLME 1935; Kalecki himself accepted the criticism (1936), and struggled for years to find a solution: at first he just expounded Frisch's propounded solution, consisting in superimposing exogenous shocks upon a damped fluctuations mechanism (KALECKI 1939), then inserted a variable parameter into his equation (1943) and eventually returned to a linear damped model to which he superimposed normally distributed shocks (1954). For a detailed discussion see BESOMI 2006a.

room for crises except as the name for the upper turning point;[1] all the previous emphasis on the suddenness and violence of the crisis altogether disappeared.[2]

Ragnar Frisch opted for fluctuations of decreasing amplitude coupled with external shocks, which he expounded by formalising the distinction between exogenous 'impulses' and their 'propagation' effects.[3] His interpretation of the economic mechanism by analogy with a dissipative pendulum[4] was extremely successful, and became the focus of the later developments of business cycle theory: it was taken up (as a reference system, if not formally: Lines 1990 and 1990a) by the Equilibrium business cycle models, although it was rejected by nonlinear dynamics.

6. Resetting the problem: post-war developments

Post-war theories of business cycles develop the two above-mentioned main lines of thought regarding cycles and equilibrium, but somehow altering the setting of the problem by changing both the notion of the cycle and the notion of theoretical norm. Two lines of approach are particularly interesting in this connection: nonlinear dynamics and real business cycles.

6. 1. *Nonlinear dynamics, and Löwe's intuition*

One of the two main lines of development of business cycle theory in post-war years elaborated on the kind of models propounded by the econometricians. It was soon perceived that these models were incapable of determining self-generated fluctuations (Goodwin 1951), and gradually the impediment which was seen as an epistemic hindrance to correct theorizing (see sections 3. 3. and 3. 4. above) was identi-

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[1] Lescure 1907, quickly followed by Aftalion 1913 and Mitchell 1913.

[2] The phenomenon has been recently resurrected in the form of asymmetry of the cycle; curiously, its discovery is attributed to Burns and Mitchell 1946 (Ramsey and Rothman 1996, 1) or to Keynes (Neftçi 1984, 307; Sichel 1993, 224; McQueen and Thorley 1993, 342).

[3] Although we find a kindred discussion in Pigou, as seen above, Frisch's starting point were Wicksell and Åkerman: see Frisch 1931; for a discussion, see Boianovsky and Trautwein 2005.

[4] The success of Frisch's mechanical metaphor marked the definite death of the medical analogy evoked by many of the authors referred to in section 4. For an exhaustive and fascinating discussion of Frisch's pendula in connection with the impulse/propagation dichotomy see Louçã 2001. Occasional references to the pendulum in connection with cycles, of course appeared early in the literature. Wilson, for instance, laid the principle that the analysis of fluctuations should take as starting point that particular condition that, if maintained, would be the 'correct' (*i.e.*, equilibrium) amount, stressing that the difficulty in identifying that point could be solved, as a first approximation, by taking the middle point in the price fluctuations, in analogy with what is done in the study of the vibrations of the pendulum around its middle point (Wilson 1840, 19).

fied and removed: the assumption of linearity. The simplest attempt (Hicks 1950) consisted in assuming amplified fluctuation contained within boundaries by a 'ceiling' and a 'floor'; Kaldor (1940) used non-linear relationships to generate persistent oscillations consisting in a stable limit cycle revolving around an unstable equilibrium point. Goodwin himself worked out a multiplier-accelerator model with a nonlinearity in the accelerator, also giving rise to a stable limit cycle: a stationary, cyclical process towards which all other initial configurations of the system within a certain range are eventually attracted. As the mathematics of nonlinear functional equations evolved, and computational methods became available, other rather wild patterns emerged, such as the aperiodic movement within a stable orbit represented by strange attractors; moreover, the range of possible movements enable to encompass growth *and* cycles in their interaction, while linear systems only admitted superimposing the cycle upon an independent trend.

The mathematical modelling of trade cycles enables us to reinterpret Löwe's antinomy, which was our point of departure, with more precision. 'Formal dynamics' (the expression is due to Shackle, 1967, ch. 16) basically admits two kinds of cyclical results: if the system tends to a stable equilibrium point, oscillations can only be maintained due to the influence of external disturbances. Endogenous oscillations require some nonlinearity, and the corresponding model will describe a stable orbit – either a closed line in the phase space, as in the case of limit cycles, or a 'fuzzy' ring, as in the case of strange attractors – surrounding an unstable steady point. It is fascinating to observe that Löwe's idea that statics should give pace to a theoretical system capable of encompassing cycles and other kinds of behaviour as 'normal' has a modern counterpart. I cited in section 2 a passage from «How is Business Cycle Theory Possible at all?» where he argued that a dynamic system should be able to encompass the alternation of upswing and downswing as the static system deals with an equilibrium state of rest. He was aware, however, that other kinds of movement must be theoretically conceivable, for he appended a footnote explaining that

the cyclical dynamic motion is probably historically but by no means logically the only possibility of a dynamic economy. Circular flow processes which are influenced by more than one independent variable in more or less complicated structure and rhythm are at least conceivable. The form of its movement would have to be studied in each particular case, although such an inquiry has perhaps for social economics as an empirical science no greater meaning than the non-Euclidean geometries have for the technical sciences.

(Löwe 1997 [1926], 269, fn.)

Löwe seems to have intuitively hit upon the concept that today we would call an 'attractor', that is, a state towards which the system would converge if starting off from within a certain boundary. The implication he drew is surprisingly modern. When he argued that dynamics would focus on «the *total* movement of the business cycle» as the «quintessential movement form» (Löwe 1997 [1926], 268-269), he struck an important point: although the (unstable) steady state around which a limit cycle revolves is an equilibrium point, for there the system reproduces its own conditions, it is no longer taken as a reference point in the explanation, because it is only an unlikely and fragile configuration. The emphasis is not in the repelling properties of the stationary point, but in the attracting properties of the orbit. Movement, and not rest, is the natural state of the system.

6. 2. *Real business cycles*

Another line of approach to business cycles was developed starting from the rational expectations assumption. With reference to our problem, the two main versions of modern neoclassical business cycle theory share the feature of stemming from a microeconomic equilibrium approach, and of recognizing as a consequence that the engine of the cycle must reside in exogenous events. Yet the monetary (Lucas) line is in a way akin to the classical and inter-war theories of cycles based on errors, while real business cycle theories introduce an alteration in the meaning of equilibrium and therefore in its relation to the cycle.

   Lucas's argument is based on a linear model characterized by a strong tendency to equilibrium in real magnitudes. Monetary disturbances, if genuinely surprising and not discounted in advance (*i.e.*, in conditions of imperfect information), act as real shocks and alter the equilibrium position towards which the system moves. This itself is not sufficient to explain persistent fluctuations, because monetary shocks only determine temporary oscillations. The introduction of further assumptions, such as lags, is therefore required. Such an approach thus presupposes the existence of an attractor for the system's dynamics; a non-discounted exogenous disturbance determines a new equilibrium position, towards which the system moves in a temporarily oscillatory way and which it is prevented from reaching by some kind of friction. The nature of the cycle, in this view, is of system's reaction to disturbances shifting the equilibrium position and determining an oscillatory movement towards it.

   The Real business cycles approach dispenses with the frictions incorporated in the monetary model: information is perfect, monetary

policies can therefore be fully discounted. For the rational expectations hypothesis these policies are thus ineffective, unless the shocks under consideration directly affects the real structure of the system, such as productivity changes, or the like. Shocks are propagated over time by the inter-temporal rational optimizing behaviour of agents; the cycle *is* this optimizing behaviour, so that each state of the system is an equilibrium. Movement thus consists in a succession of equilibrium states, and the persistence of the cycle relies entirely on the external shocks, which cannot be purely random but must be AR(1). In such a view, and in contrast to Lucas's and the more traditional 'frictional' approaches illustrated in section 5, equilibrium is not a privileged state of the system, as any state that is actually reached is itself an equilibrium. This recalls the notion of 'neutral equilibrium' of a ball lying on a billiard table: any position is an equilibrium, and all kinds of movements are possible if the ball is subjected to an external force. But if equilibrium is everywhere, then the notion cannot act as a reference state.

7. Orthodox and heretics?

The preceding discussion gives some ground for taking seriously Hayek's and Löwe's proposition, that a classification of business cycle theories should be undertaken looking at the relationship of cycle and equilibrium: Löwe's problem of the impossibility for equilibrium theory to account for fluctuations without recurring to exogenous elements, errors or frictions was recognised by most participants in the debate on cycles as an *epistemic* issue, logically prior to the formulation of models or the identification of causal relationships. Accordingly, these theorists made a deliberate choice. Some of them struggled to develop approaches that would enable one to conceive of the cycle, or of economic change more generally, as a 'normal' process, and therefore renounced, even if sometimes only partially, to the idea that the system tends towards a state of equilibrium. Others clung instead to the equilibrium approach, and developed models describing a tendency towards a state of equilibrium but subject to disturbances or impediments enabling to justify temporary disruptions of 'normalcy'.

7. 1. *A blurred distinction*

The line of division between these approaches can hardly be so clear cut. Some people were 'treading the narrow path'[1] between accept-

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[1] The expression was used by Morishima and Catephores 1988, 24, fn. with reference to J. S. Mill's attitude towards Say's Law. Mill is indeed a case in point in the present connection (see section 3. 1., above).

ing and rejecting an equilibrium approach. Schumpeter, for instance, viewed the cycle as consisting in booms induced by *per saltum* incorporations of innovations in the industrial and commercial organism, to which 'depressions' follow; the latter are understood «as the reaction of business life to the situation created by the boom or, more precisely, as the movement of business life towards a new state of equilibrium conforming to the data created by the boom»[1] (Schumpeter 1927, 294-295).[2] His point is that most people adapt their behaviour to the current economic situation in passive ways, as described by (static) equilibrium theory. But a minority sometimes exhibit other kinds of responses to stimuli, «not gripped by our analytical machine»: in certain situations «they can react by doing new things or things in a different way, incompatible with the fundamental arrangements that exist» (292). Schumpeter resolved Löwe's dilemma[3] by stressing that «it is only to routine work that received theory applies» (298), while for times of innovational activity theory needs «a new arm» (which Schumpeter had tried to supply since 1912: 292) capable of producing «different consequences». The boom cannot, therefore, be explained by means of the existing (equilibrium) theoretical apparatus, «although its consequences [that is, the depression] are» (292). In other words, Schumpeter did not fully accept the idea that the economic system is systematically unstable, nor did he fully espouse the view that it is intrinsically stable. His argument is not only that innovations, by changing the conditions, displace the system from one equi-

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[1] In other words, «industrial and commercial change […] proceeds by leaps which […] must fundamentally alter the bases of calculation and upset the existing equilibrium beyond the possibility of all people adapting themselves successfully by marginal variations» (SCHUMPETER 1927, 297).

[2] Schumpeter adds a distinction between the 'normal' depression resulting from the tendency to move towards the new equilibrium and the 'abnormal' havoc wrought by panics (1927, 294); this echoes the similar distinction suggested by Spiethoff (see section 4. 2.).

[3] Schumpeter (without explicit reference to Löwe) formulated it as follows: «I always thought, and still think, that in order to find out whether or not cycles are a phenomenon *sui generis,* clearly standing out as such from the rest of industrial fluctuations and arising from *within* the economic system, we ought, in the first instance, to assume the absence of outside disturbances – non-economic ones, or economic ones which cannot be produced or avoided by economic action, both of which we are going to call 'casual' – acting on the system. We shall, then, see either that the economic system *never* (and not only not under «static» conditions) evolves that particular kind of fluctuations of itself, in which case outside disturbances *must* be looked upon as responsible for them; or else that the economic system would of itself display 'cyclical' movement, in which case we should have to recognise the presence of a problem of a 'normal cycle'; we should, moreover, have to conclude that the whole of purely economic phenomena cannot be exhausted by means of the 'static' apparatus; and we should, finally, have to look upon the influence of outside disturbances as a fifth set of problems within the genus of industrial fluctuations, which would, indeed, also form part of any comprehensive survey of all that happens in cycles (because outside disturbances of some kind never fail to arise and always must react upon the cyclical movement), but which would have to be kept aloof in a theory of causation, in a sense which I hope is now quite clear» (1927, 290-291).

librium position to another, but that innovations temporarily topple the stability properties of the system.

Another reason why the dividing line is somehow blurred is that not only the notion of 'normality' changed over the years and was not shared by all economists, but the analytical notion of 'equilibrium' also underwent radical transformations.[1] The most relevant transition, but by no means the only one,[2] was brought about by the marginalist revolution, with its shift of focus from the idea of the system's reproduction of its own state to individual choices and with the use of new analytical instruments; and this «renewal of the theory of value implied a correspondent renewal of crises theory» (Courtin 1945, 124).[3] Some of the theories mentioned above refer to equilibrium as the reproduction of the system's conditions (whether or not this implies full employment), others to the individualistic optimization process, and it is therefore difficult to subsume them under a common heading.

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[1] Both these facts further complicate very much the relationship between the intuitive notion of 'normal' and the analytical notion of 'equilibrium'. This is witnessed for instance in the passages by Mitchell cited at the end of section 4. 3. Mitchell himself points out that the notion of a stable 'moving state of trade' should be expressed in terms of 'equilibrium'. But the latter is a theoretical term and not one of the measurable concepts in which he was interested (Morgan 1990, 52). On the other hand, he was skeptic of the mechanical notions of equilibrium due to his institutionalist background, which brought him to emphasize evolutionary adaptations instead, and more generally of the speculative character of theoretical economics (see for instance Klein 1983, 875; Adair 1994, 118; Kieffer-Dupont 2001, 607). Schumpeter even doubted that Mitchell had properly understood the role of equilibrium and of models in theoretical economics (Schumpeter 1952 [1950], 329). A part at least of these difficulties seem however to reside in the fact that Mitchell does not appear to have been able to conceive of theoretical 'normal states' different from equilibria, as witnessed by his doubts on the possibility of an unstable moving equilibrium.

[2] For comparisons of some notions of 'equilibrium' of notable importance for trade cycle theory see for instance Merlin 1949, 145-153; Vercelli 1997, with reference to 'formal dynamics'; and Rühl 1994, with reference to Hayek and equilibrium business cycle theory.

[3] See also Pribram 1951, 34-35. One of the most evident consequences of this switch was a revaluation of the relevance of Say's law: «Say's law was acknowledged as 'doctrine' but […] it coexisted with additional lines of reasoning attempting to explain fluctuations and cycles; […] Say's law itself was seen as enjoining overproduction as a cause of cycles; […] overproduction could be the result of instability whose genesis still had to be explained; […] Say's law did not signify for the writers involved the impossibility of either unemployment or of cycles nor for that matter the impossibility of internally generated cycles per se. Say's law, in other words, while of doctrinal status, applied to only a small range of considerations and was neither an absolution for the need for nor a substitute for a business cycle theory. […] Say's law was treated in neither a cavalier nor an adversary manner; in no small part, it seems to have been left neatly on the shelf, applied negatively to one line of reasoning (overproduction as a cause of cycles), as something inadequate to the problem at hand – in effect, as part of a developing collection of lines of reasoning and approaches, all attempting to grapple with an understanding of a serious problem, a problem whose nature was approached and seen from different facets. These economists were seeking explanation from crises to cycles, for periodicity and recurrence […]; in short, to account for under-full-employment equilibrium» (Samuels 1972, 156-157 and 160; see also Sowell 1972, ch. 7, on the changing status of Say's law with the advent of the marginalist era).

7. 2. *The role of cycles in the working of the system*

In spite of these difficulties, Hayek's and Löwe's suggestion that a history of cycle theories should focus on the relationship of cycles and equilibrium induces me to explore two related issues. The first consists in the fact that the views regarding the position of cycles with respect to some 'normal' state of the system reflect the views of what cycles essentially *are*: whether they are expressions of some deep-seated disorder in the working of the capitalist system; or, alternatively, whether they are the result of external events or of some friction, maladjustment, or government interference disturbing the good working of the self-adjusting properties of the system.[1] This, I suspect, is not an issue that can be solved once and for all, because cycles and crises, in their relation to some notion of equilibrium can be interpreted in either of two irreconcilable ways. Few in the history of economic thought seem to have denied that equilibrium is sometimes, or even often, disrupted in an evident way by crises or cycles;[2] equally, no one ever denied that the economic system must, at some point, get close to some kind of equilibrium (at least in the sense of the system's reproduction conditions), otherwise it could not endure. Both crises and equilibrium are therefore well rooted in two centuries of historical evidence. But this evidence admits *both* the above-men-

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[1] Schumpeter's comment in this connection is quite important as it brings the equilibrium-crisis relationship in the context of the logical scheme developed to study the economic system: «economists who had developed economic statics as the centerpiece of their science […] naturally exaggerated the importance of their central achievement. They saw more in it than do we, that is, more than a logical schema that is useful for clearing up certain equilibrium relations but is not in itself directly applicable to the given processes of real life. They did not realize how many and how important the phenomena are that escape this logical schema and loved to believe that they had got hold of all that was essential and 'normal'. Now, from the standpoint of this type of analysis, it is natural to locate the 'causes' of observed disturbances either outside of the economic system or in the fact that the economic engine, like any engine, never works with precision» (SCHUMPETER 1954, 1132-1133).

[2] It is hard to deny the existence of crises when they occur, although they can be interpreted as partial only, e.g. as relating to financial markets or to some specific sector of the economy. The cycle, on the contrary, has found a number of detractors who denied the almost-periodical recurrence of crises or a semi-regular alternation of phases of prosperity and depression, or interpreted it as the economy's response to exogenous shocks, or argued that it was a relic of the past, beginning from an anonymous 'political economist' writing in 1864 two letters to the Editor of *The Economist* criticising «a current theory, more or less distinctly conceived and firmly believed, that periods of commercial prosperity and adversity recur as regularly as the tides» (anonymous, 1864, with reference to Jevons's 1863 essay on the corn question and to an *Economist*'s correspondent writing in 1863 that there was evidence of «an alternation of periods of prosperity and extension recurring apparently with the regularity and activity of economic laws»: 1428) to a number of books bearing titles inquiring whether the business cycle is dead (for instance BRONFENBRENNER 1969, BALDASSARRI and ANNUNZIATO 1994, and FUHRER and SCHUH 1998), not to mention countless other contributions along the same lines (FISHER 1925 being perhaps the most eminent representative of this tradition).

tioned alternative interpretations:[1] one can prove that a certain *model* does (or does not) give rise to a stable equilibrium, but one cannot prove that *actual* economic systems are, or are not, stable.[2] And a shift of emphasis on the importance or unimportance of certain causal factors, or a simple alterations in the form of equations, may lead to altogether different results, switching from one interpretation to the other. For instance, Goodwin's dropping of the assumption of linearity in one term of the multiplier-accelerator model turned it from a stable system with fluctuations dying out (and therefore to be sustained by exogenous impulses) into one describing persistent endogenous oscillations (Goodwin 1951).[3]

The second issue raised by Löwe's and Hayek's historiographical suggestion is directly related to this point. Although the issue is un-decidable, theorists (admittedly not all of them, but surely most of those listed in these pages) *did* make up their minds: some on one side, others on the opposite; some more radically than others; some explicitly, others tacitly.[4] And they have framed their theoretical systems accordingly.[5] The pre-analytical belief in the self-adjusting properties of the system is a *premise* to theorizing, as it guides the choice of the model, either mathematical or verbal, from which the results follow. This aspect is not captured, and may even generate hopeless confusion, by the widely followed alternative historiographical approaches based on the pigeon-holing of theories depending of the

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[1] For a more detailed discussion see Lunghini 1997.

[2] On the kindred issue of the endogenity or exogenity of the cycle, Sims points out that «whether fluctuations are endogenously or exogenously generated, stochastic or determin-istic, is a property of the model, not of the real world» (Sims 1994, 1886). Sims thus argues that the distinction is semantic only, and «for the most part no more than a distraction»; the issue is nevertheless far from irrelevant in understanding an author's interpretation of the phenomenon.

[3] The debates on the stability of the so-called Harrod-Domar model is a case in point: by slightly altering a few assumptions regarding the functional relationships, opposite results were obtained (see, for a survey, Besomi 2001). This is another instance of McCloskey's *A*-prime/*C*-prime conjecture, which states that «for each and every set of assumptions *A* imply-ing a conclusion *C*, there exists a set of alternative assumptions, *A'*, arbitrarily close to *A*, such that *A'* implies an alternative conclusion, *C'*, arbitrarily far from *C*» (1993, 235).

[4] As open discussions of viewpoints and premises are exceptions rather than the norm (especially since mathematical modelling has become the only admissible way of theorizing), there is often scope for exploring the question of whether (or how far) the interpretation of cycles and crises as a 'normal' or 'abnormal' condition is a purely analytical result, or is hidden among the pre-analytical postulates.

[5] Possible exceptions made by theorists who *believe* they have represented their views but in fact have not, reflect either errors in the logical development of the argument from premises to result, or a bad choice of the model coupled with the lack of awareness of its full implica-tions. As an example of the latter I would cite Kalecki, whose usage of a linear model repre-senting damped fluctuations maintained alive by exogenous shocks oddly contrasts with his view of the cycle as the form taken by the contradictions of capitalism (for a full discussion see Besomi 2006a).

cause, or causes, of the cycle, or on analytical *results*. The same causal relationships, combined in different ways, with altered emphasis, or with different parameters (e.g., with or without a lag or a non-linear term), can give rise to wildly different representations of the cycle.[1]

7. 3. *Heretics* vs *orthodoxy*

Reflection on the premises of trade cycle theories cannot be dispensed with, even though there is not unambiguous correspondence between analytically precise notions of what the theoretical norm is: after all, at the pre-analytical stage there is nevertheless an intuitive notion of 'normality'. It could be of some use, then, to look at the history of business cycle and crises theories keeping in mind the intuitive and apparently simplistic dichotomic categorization propounded by Keynes:

On the one side are those who believe that the existing economic system is, in the long-run, a self-adjusting system, though with creaks and groans and jerks, and interrupted by time lags, outside interference and mistakes […]. These authorities do not, of course, believe that the system is automatically or immediately self-adjusting. But they do believe that it has an inherent tendency towards self-adjustment, if it is not interfered with and if the action of change is not too rapid. On the other side of the gulf are those who reject the idea that the existing economic system is, in any significant sense, self-adjusting. They believe that the failure of effective demand to reach the full potentialities of supply, in spite of human psychological demand being immensely far from satisfied for the vast majority of individuals, is due to much more fundamental causes.

(Keynes 1934, 486-487)

Keynes's distinction between orthodox (the first group) and heretics (among whom he ranged himself) of course only covers one form of dissent from the mainstream; in this sense, the proposed division is doubtless simplistic. This proposal was formulated in a radio talk,

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[1] Keynes's diffidence towards a memorandum by Haberler, being the very first draft of *Prosperity and Depression*, the volume epitomizing this historiographical approach, well expresses this point: «[Haberler] is attempting something which is not worth while. Working within such narrow limits of space he is condemned to producing an account which is very far from clear of theories which are themselves confused. On a plane so near the surface as that which he is deliberately occupying there is obviously nothing useful to be said. Obviously the truth must lie somewhere much deeper down and the source of so much confusion could only be found by clearing up the terms and concepts involved right from the beginning. There is hardly a phrase of which the meaning is unambiguously clear. Moreover, in order to get his authors into his schematism he has had to make some of them appear sillier, others less silly, than they really are. I doubt if many of them would recognise themselves as they are here presented, with their bodies cut up into small cubes which are then made up into lb packages and put into pigeonholes. Two trunk murders were bad enough, but here are dozens with an assorted collection of limbs in each trunk» (Keynes to Felkin, 30 August 1934, in League of Nations Archives, Geneva; carbon copy in Keynes's papers, King's College, Cambridge, CO/3/33; further copy in Haberler papers, Hoover Foundation, Stanford, Box 66).

rather than in an academic publication. This is surely one of the reasons for the use of the intuitive notion of 'self-adjusting' system and the absence of the technical terms 'equilibrium' and 'stability'. But there is more in this choice. Keynes believed that orthodox theory had constructed in a century a strong citadel of «organized economic thinking and doctrine», while the heretics had only «survived as isolated groups of cranks» who could only argue that *facts* do not conform to the theory. He maintained that to defeat the citadel it had to be assailed from inside, and at that point «no successful attack [had] been made» (*ibidem*, 488-489), but he was himself not ready for the final assault. If one wants to take Keynes seriously on this point, one should be careful not to give too much weight to his conclusion. Some of those he would have unhesitatingly classified among the orthodox were aware of the antinomy between the stability of equilibrium postulated by the mainstream approach and cycle or crisis *theory* (and not only of the *facts* of the cycle), and were prepared to recognize the 'normality' of cycles as opposed to the 'naturality' of equilibrium.[1] Robertson, as we have seen, is a case in point, and his complaints were to some extent justified.[2]

Yet the proposed line of division made sense to other commentators as well.[3] A couple of examples will suffice. Mitchell, for one, stressed that while «orthodox economists» focused on «the long run» and «normal states», «to force into prominence the fact that economic activities are subject to recurring phases of contraction and expansion was the work primarily of men who were critics, not merely of orthodox economics, but also of modern society» (1927, 3-4). Similarly, Mentor Bouniatian perceived a line of division between the «pre-

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[1] The opposite is also true: authors who are unorthodox in Keynes's sense could be orthodox in other respects; an example is Bouniatian: see Besomi 2007.

[2] Robertson's grievance is well expressed in this passage: «But in my heart I do think (though I don't expect you [Harrod] to agree) that ch. 23 of the General Theory is rather an outrage. If K[eynes]. was going in for Dogmengeschichte at all at this stage, he had no business to stop short at Mummery and Gesell, thereby giving the impression that apart from a handful of dead cranks he was the first person to question the alleged 'classical' hypothesis of an automatically and instantaneously self-righting economy. He ought to have gone on to say something serious and appreciative of the work of his contemporaries – the Swedes, Haberler, myself; and a repetition of the pat on the back for Abbati would then have been in place. K. found it easier to be generous to cranks than to professional economists, but I think it is not unfair to say that he preferred even his cranks to be dead» (Robertson to Harrod, 4 April 1950, cited in Besomi 2004, 44).

[3] Schumpeter, albeit not labelling the two currents as orthodox and heretics, maintained that «there are only two – fundamentally different groups of cycle theories. There is the 'theory' that the economic process is essentially non-oscillatory and that the explanation of cyclical as well as other fluctuations must therefore be sought in particular circumstances (monetary or other) which disturb that even flow. Marshall stands out in the large crowd that represents this 'hypothesis.' And there is the 'theory' that the economic process itself is essentially wave-like – that cycles are the form of capitalist evolution» (Schumpeter 1950, 149).

vailing doctrine» and its critics. The former «denied the possibility of a general overproduction, arguing that riches are produced to exchange them with other goods and production of the former creates an outlet for the latter (James Mill's and Say's law of markets)». In this view, a general overproduction is a *contradictio in adjecto*, and crises can only be explained as the consequence of errors, miscalculations, excess of credit, overspeculation or some other kind of disproportion, that can, however, lead at most to *partial* overproduction, for the regularity of which no explanation was suggested. Only the critics, by explicitly rejecting the idea that accumulation has no intrinsic limits, could provide endogenous, 'organic' theories of crises (Bouniatian 1922, 3-4; see Besomi, forthcoming).

I would like to conclude where I began, namely with Löwe himself. Four decades after his original statement, he reformulated the problem in terms of the stability of equilibrium:

[A] general diagnosis of instability … has been the constant theme of critics … from Marx to Keynes. Now what unites these … and even allies them with the champions of capitalist stability, is their belief in an inexorable mechanism which is supposed to direct all market motions. The critics differ from theoretical orthodoxy only in the manner in which they see the mechanism operate. It is the conviction of classical and neoclassical orthodoxy that market processes bear all the characteristics of a negative feedback mechanism, which automatically corrects any deviation from [equilibrium[1]]. The critics, on the other hand, point to certain institutional factors, such as property relations, income distribution, absence of investment opportunities, which are alleged to produce positive feedbacks, amplifying partial distortions into general disequilibrium.

(Lowe 1987, 238)

I do not want to deduce from these arguments that analytical results are not relevant for a history of trade cycle theories,[2] but only that more weight should be given to the largely ignored question «what *are* business cycles»? Hayek's and Löwe's suggestion to classify trade cycle theories in terms of the relationship of cycle to 'normality' achieves its fullest sense if the intuitive notions of cycles and normality are also compared, rather than only the analytical results that reflects, among other things, the choice of the logico-mathematical formulation of the basic relationships. The history of the solutions to the trade cycle problem cannot be separated from the history of what the question is, and how it is asked.

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[1] In the original the word 'stability' is used, but of course it makes no sense.
[2] The analytical results and formulations are, needless to say, the key for understanding what hidden assumptions lie behind a model and what kind of view it supports. Accordingly, Keynes's method of criticism of 'classical' theories is based on the working out the argument backwards, identifying the implicit hypotheses necessary to reach the result, and examining whether these are compatible with the internal logic of the theory of whether they assume away the facts they want to disprove (see CARABELLI 1991).

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[1] Unless otherwise indicated, all translations are mine.

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