Schumpeter's Plea for Reasoned History

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Abstract and Keywords
The chapter summarizes some of the main characteristics of Schumpeter's evolutionism, discussing his own evolution in some detail, since the early days of the Methodenstreit between Menger and Schmoller, and the participation of Schumpeter along the Walrasian side.

Yet he tried to surpass what he recognized as the limits of that side: the analysis of capitalism from the point of view of statics, and consequently the understatement of the dynamic role of innovation, the main endogenous drive for change and adaptation of the economic system.

Schumpeter's non-Darwinist evolutionism is analysed, and his contributions are compared to his main rival and bête-noir, J. M. Keynes; in spite of the conflicting views of both men, some essential points of proximity are indicated: both interpreted modern industrial relations as part of a dynamic, nonlinear,
complex system, emphasizing the emergence of uncertainty (Keynes) or change (Schumpeter).

Their closely related understanding of the cyclical nature of capitalism is indeed one of the crucial contributions of the first half-century economics.

Keywords: economic history, evolutionism, innovation, John Maynard, Joseph Schumpeter, Keynes, Léon, Methodenstreit, Walras

2.1 Introduction
The econometric revolution of the 1930s and 1940s re-established the dominance of neoclassical economics against the challenge of institutionalism. Paradoxically, one orthodox economist, although a leading figure in the new Econometric Society, which was instrumental in the establishment of the neoclassical dominance, resisted the new orthodoxy. The paradox of Schumpeter's plea for reasoned history is the theme of this second chapter.

As a man and as a scientist, Schumpeter was marked by the difficult times through which he lived: the second industrial revolution, the fall of the Austrian Empire, revolution in Russia and then its extensions to central Europe, the Great Crisis, the victory of Nazism, emigration to America—the first half of the century was a turbulent period indeed. Schumpeter tried to influence these events—in 1919 he was a minister in the new republican government in Vienna, then he was a banker, then he was an abundantly productive writer on economics—but he tried first of all to understand the nature of the changes. This was why his research led directly to the problem we are dealing with in this book: how can economics help to explain history, and how can history help to explain economics?

The origin of Schumpeter's first theoretical sketches can be traced to two fundamental roots. (1) The first was the work of J. B. Clark, who was deeply aware of, but unable to overcome the limits of, the static equilibrium analysis: Schumpeter inherited his research programme and developed it throughout his life, but he too was unable to solve the enigma. (2) The second major influence on Schumpeter's evolution was the Methodenstreit debate, and the growing importance accorded to the historicist arguments. Both of these roots establish a research programme at the margin of the equilibrium paradigm, and this explains the paradoxical attitude of Schumpeter in relation to the economics of his own
time: the most Walrasian and orthodox of all neoclassicals, he was also the only mainstream economist to challenge the foundations of the equilibrium paradigm and was therefore the most heterodox.

Consequently, Rosenberg went as far as to argue that Schumpeter was the ‘most radical scholar in the discipline of economics in the twentieth century’, since ‘he urged the rejection of the most central and precious tenets of neoclassical theory’ (Rosenberg 1994: 41). Rosenberg is very emphatic: ‘Indeed, I (p.43) want to insist that very little of the complex edifice of neoclassical economics, as it existed in the late 1930s and 1940s, survived the sweep of Schumpeter's devastating assaults’ (p. 41). Based essentially on Schumpeter's *Capitalism, Socialism and Democracy* (1942; henceforth CSD) and on the preface to the Japanese edition (1937) of *Theory of Economic Development*, Rosenberg identifies the alleged devastating assaults by Schumpeter against the neoclassical paradigm: since change is the decisive feature of capitalism and it means a permanent tendency to disruption, and since equilibrium has no welfare advantage as it means no progress, innovation is alien to rational-equilibrating decision making. In this context, the ‘circular flow’ described a capitalism deprived of the essential movements of change and one that is therefore merely a simplification; as a consequence, Schumpeter committed himself to the historical analysis of this process of mutation as an alternative to the equilibrium paradigm (Rosenberg 1994: 44–5, 48, 50, 56). Unfortunately, the whole case is based on partial and circumstantial evidence, given the fact that the same Schumpeter denied all these claims at other times, including in posterior writings.

Certainly, one very essential point is clear: Schumpeter was opposed to the neoclassicals in the very definition of the research programme--his *explanandum* was technological and institutional change, and these features are ignored and annihilated in the orthodox view by the *ceteris paribus* conditions (Rosenberg 1994: 50–1). Indeed, the *ceteris paribus* is inoperative in order to study irreversible processes (*CSD*: 95). As a consequence, the real problem for the interpretation of Schumpeter is why did he not break with the neoclassical paradigm he could neither use nor follow. Indeed, he held to the framework of the equilibrium paradigm through all his scientific writings. This paradox, which is not recognized by
Rosenberg in his radical remarks, is the essential question or, as Allen puts it in the most complete and authoritative biography of Schumpeter:

Paradox, failure, disaster, and disappointment were the keynotes of Schumpeter's life and work. He lived a paradoxical life and had a paradoxical career. He thought paradoxical ideas and wrote paradoxical books. Time and time again he failed as a scientist, scholar, politician, businessman, and even as a human being. . . . Yet, paradoxically this career of failure was, in its totality, a success. (Allen 1991: i. 4)

This paradox was never clearer than in regard to the grand schema of general equilibrium and its application to the domain of cycles and change, or to history.

2.2 From the Methodenstreit to the Sozialökonomie: The Role of History
The first book by Schumpeter, Das Wesen (1908; henceforth DW), was actually a large dissertation on the methodological debate opposing Menger (p.44) and Schmoller, the ‘theorists’ and the ‘historical school’.¹ Schumpeter, who entered Vienna University just after the retirement of Menger and studied under the supervision of his successor, Wieser, supported the ‘theoretical’ side of the Methodenstreit and praised the importance of the abstract and naturalistic approach in economics: ‘From a methodological and epistemological viewpoint, pure economics is a “natural science” and its theorems are laws of nature’ (DW; quoted in Swedberg 1991: 28), having nothing to learn from biology (p. 27).

Schumpeter was at the time considered a faithful supporter of the Mengerian side, although with strong Walrasian intonations: Hildebrand in 1911 opposed his appointment as teacher in Graz because he did not want the university to be occupied by anti-‘historians’. Later on, after the failure of his political career (as Minister of Finance in the republican government of Karl Renner from March to October 1919) as well as of his banking career, the fame of the marginalist ‘theorist’ would precede him to Germany, the stronghold of the Historical School. Only the influence of his friend Spiethoff, a disciple of Schmoller, assured him a badly needed appointment at Bonn University (Swedberg 1991: 69–70). Schumpeter considered himself a marginalist by that time: in a note in
History of Economic Analysis (Schumpeter 1954; henceforth HEA), he presented his positions as a 'strong partisan of economic theory', just like Wieser (HEA: 819 n.). He was also, throughout his life, an extreme positivist, supporting a strong demarcation between positive science and normative intervention. Under the influence of logical positivism, then dominant in Vienna, Schumpeter always defended this form of ‘therapeutical nihilism’.

But his balance‐sheet of the Methodenstreit changed during the years, and this was already obvious when, three years after the publication of The Theory of Economic Development (Schumpeter 1911; henceforth TED), Schumpeter again discussed the problem in his Economic Doctrines and Methods (Schumpeter 1914; henceforth EDM). By that time his position was much more cautious, even though he still supported the marginalist side: the whole debate was considered useless and exaggerated, and he presented ‘an explanation for the controversy: it was a struggle between two methods of work, between people of different mental habits, who fought for elbow room or for domination’ (EDM: 167). It is hard to consider this as a compliment for either side.

The book was careful to insist on the elements of synthesis, namely on Schmoller's alleged acceptance of the similarity of the causal nexus in social and natural sciences and of the definition of laws as the aim of science (EDM: 170). Schumpeter even argued that one of his main previous criticisms of Schmoller's insistence on reform policies could be dropped since the latter had changed his mind (p. 175)––which is not at all evident.

Schumpeter indicated the six major innovative elements of Schmoller's contribution as follows: (1) the relativity of theory, (2) the unity of social life, (3) anti‐rationalism, which Schmoller was supposed to have abandoned, (4) evolution and the role of history, i.e. to be compared with Marx, (5) the affirmation of complexity, and (6) the organic conception, as an analogy of society with a body (EDM: 175ff.). The striking fact is that Schumpeter incorporated many of these features into his own research, namely (2), (4), (5), and (6), even if by the time of publishing EDM he was still fascinated by the marginalist revolution, a ‘purer economics’, ‘incommensurably
more firmly founded’, ‘more correct’, ‘simpler’, and ‘more general’ (pp. 181ff., 189–90).

But the very conception of EDM deserves some attention. The essay was prepared for Max Weber's Grundrisse der Sozialökonomie, a handbook intended to present a new methodology for a transdisciplinary social science. Weber was strongly opposed to the Methodenstreit itself, which he accused of having led to an artificial polarization of the statistical and theoretical methods against the historical method. The influence of this conception on Schumpeter's thought was a lasting one.

In 1926, after a long period without any theoretical intervention, Schumpeter came back to the discussion of Schmoller's theories in an essay, Gustav von Schmoller and the Problems of Today. This represented a major turn, in the sense that it was a very positive assessment of the author and a formal endorsement of the Sozialökonomie, arguing for a fruitful combination of theory, statistics, history, and sociology as the basis for a new economics. And this remained his consistent opinion from then on, with its insistence on the role of history for the understanding of capitalism—this is exactly why Schumpeter is so relevant to the purposes of the argument of this book.

In the opening of Business Cycles (Schumpeter 1939; henceforth BC) this was clearly stated: history has the ‘most important contribution to the understanding of our problem’ (BC: 13). This book is certainly a major piece of economic historical analysis, and we will come back to it later on. As for his magisterial History of Economic Analysis, this is the most complete statement of the programme of Sozialökonomie: the main techniques indicated for research in economics are history ('by far the most important', since 'the subject matter is essentially a unique process in historic time'), then statistics, then theory, and finally economic sociology (HEA: 12).

This methodological indication, combined with the definition of economics as the study of irreversible processes of change, plus the organic vision of evolutionary societies, defines the main conclusion of this section: since Schumpeter incorporated some essential traits of the Historical School in a very distinctive framework, and since he invaded the new
territory of historical mutation in economies, Schumpeter was indeed not a neoclassical economist, but at the same time, he was not able to cease considering himself to be one, since he did not wholly reject, and even tried to incorporate, the paradigm of equilibrium along with the historical forces of mutation. This is more than the simple restatement of the Paradox, as is obvious in his discussion about the role of the Walrasian system: it is also a programmatic conclusion, since the scientific viability of the modern evolutionary programme depends crucially on the rejection of the Schumpeterian compromise.

This dilemma was obvious on several occasions in Schumpeter's work. The cold reception of *BC* in the scientific community was certainly one of the indications of Schumpeter's increasing difficulty in maintaining his profile as a mainstream economist while developing what was considered as an extravagant or esoteric research. As Kuznets put it, cycles are a quantitative phenomenon and should therefore be dealt with by statistical methods, and not as a qualitative phenomenon as Schumpeter implied. Since Schumpeter had been chairman of the founding meeting of the *Econometric Society* (he was, at the time of the publication of *BC*, its vice-president and was to become its president the following year), he was consequently expected to contribute to quantitative economics and to the mathematical formalism he praised so often. He did not; and in fact, of the whole scientific community, only Frisch received the book with enthusiasm (Swedberg 1991: 271 n.).

Schumpeter acknowledged this contradiction and coped with it dramatically. This was evident at one of the last important scientific meetings attended by him, the 1949 NBER Conference on Business Cycles. At this conference, where 'historians' (the NBER researchers, following Mitchell) and 'statisticians' (the Cowles Commission staff) collided, Schumpeter undertook the task of arguing for the historical method. Furthermore, he had the moral duty to represent Mitchell, the creator and director of the NBER, who had recently died. In his uncomfortable double role as the author of *Business Cycles* and a distinguished member of the Econometric Society, Schumpeter began with a defensive declaration: 'I have no wish to (p.47) advocate the historical approach to the phenomenon of the business cycle at the expense, still less to the exclusion, of theoretical or statistical
work upon it’ (Schumpeter 1949: 308). But he then repeated his main definition: ‘Economic life is a unique process that goes on in historical time and in a disturbed environment.’

History is needed for the inquiry into exogenous, occasional events, but also and essentially into the very organism of the cycle:

For historical research is not only required in order to elucidate the nature and importance of the non-essentials dealt with so far, but also in order to elucidate the underlying cyclical process itself. . . . But it would not be quite correct to say that historical analysis gives information as regards impulses and dynamic [theoretical] models as regards the mechanism by which the impulses are propagated . . . Very roughly this is so and I should be quite content if my audience accepts the thesis that the role of the econometric model . . . is to implement the results of historical analysis of the phenomenon and to render the indispensable service for describing the mechanics of aggregates. But the econometric models do more than this—they ‘explain’ situations which in turn ‘explain’ or help to ‘explain’ impulses. And the reverse is also true. (Schumpeter 1949: 311–13)

This is a notorious argument, not only by its search for an incisive counter-logic pedagogy—the listeners should be driven to accept the historical method for the precise reason they were opposing it—or by the acceptance of some sort of Frischian formalism of cycles, but also because it indicates how far Schumpeter was engaged in the defence of the role of historical research and qualitative methods. And certainly, the final advice by Schumpeter did surprise his audience: ‘To let the murder out and to start my final thesis, what is really required is a large collection of industrial and locational monographs . . . [including the historical change and the “behaviour of leading personnel”]’ (Schumpeter 1949: 314).

It is well known that his arguments did not change the course of history, and that the econometric revolution was already well on its way. Although his arguments surprised some of his colleagues—Samuelson, Goodwin (‘it was a great shock to me’, in Swedberg 1991: 176) and Machlup (1951: 95)—they did not stop the attacks by the econometricians against the
historical method (Gordon 1986: 27), nor could they prevent the rise of the new breath of equilibrium economics.\(^6\) Schumpeter could not prevent this, and in fact (p.48) could not challenge it, since it was too late and too little: he was not ready to break away from the equilibrium paradigm, although he suspected he was ‘letting the murder out’. He was right about that.

2.3 Evolution and Evolutionism
Discussing the foundations of economic theory, Schumpeter made a quite bizarre statement at the climax of neoclassicism: without history, our science would not be meaningful. The argument was quite strong: no explanation is legitimate without being contextualized in its proper place in the historical narration. But, then, what is history? Or, more precisely, what do economists need to know about the sequence and temporal structure of events and relations? The answer is simply, yet very demandingly, that they need to understand evolution.

Paradoxical as he was, Schumpeter did not define evolution and his own concept of evolutionism is an entangled mixture of simplifications and confusions. In the first place, evolutionism had no clear status for Schumpeter, in the methodological domain. Indeed, he defined physical science as the authoritative model: ‘And so we have reached a stage, perhaps for the first time, where facts and problems are before all of us in a clear and in the same light, and where analysis and description can cooperate in something like the spirit of physical science’ (Schumpeter 1927: 287).

In spite of this rather strong claim, Schumpeter always attempted to deny the incorporation of mechanistic influences into the province of economics, such as the concepts of ‘force’, ‘equilibrium’, and so forth. Furthermore, his own theoretical reasoning was dominated by the problem of change—of evolution. The cycle—and the whole ‘organic’ process of capitalist development\(^7\)—should be explained by another phenomenon rather than equilibrium: by an ‘industrial mutation—if I may use the biological term—that incessantly revolutionizes the economic structure from within, incessantly destroying the old one, incessantly creating a new one. This process of Creative Destruction is the essential fact about capitalism’ (CSD: 83).
This is indeed a rather exceptional statement, since in general Schumpeter suspected and rejected in the strongest terms any attempt to incorporate an explicit biological metaphor into economics. In 1911, he had written:

Here [in the class of ‘metaphysical’ tendencies], too, belong all kinds of evolutionary thought that centre in Darwin—at least if this means no more than reasoning by analogy. . . . But the evolutionary idea is now discredited in our field, especially with historians and ethnologists, for still another reason. To the reproach of unscientific and extra-scientific mysticism that now surrounds the ‘evolutionary’ ideas, is added that of dilettantism. With all the hasty generalizations in which the word ‘evolution’ plays a part, many of us lost patience. We must get away from such things. (TED: 43)

Almost forty years later, Schumpeter still held the same opinion and expressed it in the same terms. Writing in HEA about the 1870–1914 period, he emphatically described biological evolutionism as ‘a field infected by ideological bias and by dilettantism to an extent that surpasses anything that even we economists are accustomed to’ (HEA: 788). In spite of it, Schumpeter considered Darwin’s Origin as an important scientific achievement, comparable to the definition of the heliocentric system (HEA: 445, 445 n.), and Darwin’s historical sketch of the previous biological theories as a crucial piece for the sociology of science, but he did not indicate any possible kind of influence of theses texts on social sciences: his sympathetic references were probably due mainly to ceremonial reasons. Nevertheless, it is clear that Schumpeter’s purpose was to attack the influential and widespread Spencerian type of evolutionism, which combined ‘naive laissez-faire’ with a simplified version of Darwinism, leading to conclusions such as the ‘silly’ suggestion for the abandonment of sanitary regulations or public systems of education and health (HEA: 773). Schumpeter also cared to inform the reader, in the introduction to BC, that his assumption about the organicity of economic processes did not at all imply his being a supporter of laissez-faire (BC: vi). Therefore, the concept of ‘industrial mutation’ was rather exceptional and had been carefully chosen in order to emphasize the non-equilibrium properties of development and
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In this, Schumpeter was indeed closer to Marx than to Walras.\(^9\)

In fact, Schumpeter's evolutionism was not based on Darwinism or, in general, on the biological metaphor, which played only a minor role, if any, in his system. But it was nevertheless an evolutionary conception, since it was based upon two central concepts. First, the economy was defined as an ‘organic’ whole, propelled by a process of development with mutations. (p.50) Second, this defined a non-mechanistic and historical view of capitalism as one of creation and destruction. Since the concept of ‘mutation’, the change arising from innovation in the core of the system, has been discussed above, this section will now turn to the concept of the organic system.

Here is how Schumpeter presented the concept, criticizing the biological analogy:

> In the first place, we notice the idea that society, being an ‘organic’ system and not a ‘mechanical’ one, can be fruitfully analyzed in terms of an analogy with biological organisms such as the human body. . . . But the obvious puerility of this idea must not blind us to the fact that emphasis upon the ‘organic nature’ of the economic process may be but the means of conveying an eminently sound methodological principle—as it was, for instance, with Marshall. Theorists—specially of the ‘planning’ type—often indulge in the deplorable practice of deriving ‘practical’ results from a few functional relations between a few economic aggregates in utter disregard of the fact that such analytical set-ups are congenitally incapable of taking account of deeper things, the more subtle relations that cannot be weighted and measured . . . ‘Organic’ considerations are perhaps the most obvious antidote—though in themselves hardly an adequate one—against such uncivilized procedure. (HEA: 788–9)

Besides the polemic bias—that the ‘theorists of the planning type’ could be easily replaced by the ‘theorists of the marginalist type’ and the whole paragraph would keep its sense—this is a clear indication of the nature of Schumpeter's thought: organic considerations were supposed to be essential in order to avoid the useless biological analogies and hence to
provide an overall method for the economic inquiry: the solution of a system of equations was unable to represent complex or ‘more subtle’ relations. This explains his approach to causality in economics (BC: 7) and to the analysis of its features (Schumpeter 1949: 313). Evolutionism, then, was for Schumpeter simply the consideration of organic evolution in real time, or of historical and irreversible processes of change:

Social phenomena constitute a unique process in historic time, and incessant and irreversible changes are their most obvious characteristic. If by Evolutionism we mean not more than recognition of this fact, then all reasoning about social phenomena must be either evolutionary in itself or else bear upon evolution. Here, however, evolutionism is to mean more than this. One may recognize the fact without making it the pivot of one's thought and the guiding principle of one's method. . . . [James Mill's] various systems were not evolutionary in the sense that his thought in any of those fields turned upon evolution. And it is this that shall be the criterion of evolutionism for us, both as regards philosophy . . . and as regards any ‘scientific field’. (HEA: 435–6)

It is possible to conclude that Schumpeter defined the social process as an intrinsic dynamic disturbance of equilibrium through the creation of novelty—the innovative mutation—and this was precisely what defined his evolutionary framework. It included stationary processes of equilibrium, the place of Walras, but also forces and processes moving towards disequilibrium, the (p.51) place of Marx. And it was organic, since both processes were considered to be compatible, and since all the relevant variables were considered to be endogenous to the system, which itself generates movement and change. Moreover, this particular combination was considered to be the very specificity of economics, and so Schumpeter believed that his general and historical approach was the only one able to integrate both the statics of general equilibrium and the dynamics of disequilibrating forces: in a superior synthesis, the unscientific bias of the physical and the biological analogies would be prevented, since those analogies took the part as the whole and thus developed dilettante or simplistic views.
Schumpeter's work on the cycle and the long waves of structural change under capitalism provided one of the more creative examples of his own solution to the difficulties of considering economic processes as part of an evolutionary world.

In 1910, while preparing *TED*, Schumpeter summarized his own views in some short theses:

First, the economic processes divide into two different and also in practice clearly discernible classes: static and dynamic. Second, the latter constitutes the pure economic evolution, that is, those changes in the model of the economy which arise from itself. Third, the economic evolution is essentially a disturbance of the static equilibrium of the economy. Fourth, this disturbance provokes a reaction in the static masses of the economy, namely a movement towards a new state of equilibrium. (Schumpeter, quoted in E. Andersen 1994: 41)

For our present purposes, it is essential to emphasize that Schumpeter distinguished between statics and dynamics, as Mill had previously done, as two real processes, related by the conception that without disturbances the system would be ‘static’ but that those disturbances arise from inside the system itself. In 1908, Schumpeter argued that the central question for ‘pure’ economics was statics and equilibrium—which surely deserved the approval of Walras—and considered dynamics as a marginal phenomenon (*DW*, quoted in Bottomore 1992: 171), despite some rhetorical declarations about dynamics as ‘the land of the future’ (*DW*, quoted in Swedberg 1991: 29–30). But he quickly changed his opinion, as indicated by the 1910 thesis.

It is also certain that *Das Wesen* already presented some clues for the future discussion about the entrepreneur: Schumpeter was influenced by Riedel, an economist of the early nineteenth century, who stressed the role of innovations in economic life; by the previous work on the entrepreneur by Thünen or Bohm Bawerk; and particularly by his teacher, Wieser, under the ultra-romantic influence so important in Germany at the time: a figure of a ‘great man’ and some ‘heroic individualism’ was defined in economics, just as Spencer did in sociology, Nietzsche in philosophy (Streissler 1994: 19 ff., 34;
Allen 1991: 107), and J. B. Clark in economics. Entrepreneurship was interpreted in 1908 as the function of carrying the adventuresome innovation (Allen 1991: 47).

In 1911, in TED, Schumpeter presented these conclusions as the distinction between the ‘circular flow’ and ‘development’, the main economic processes in action (TED: 145). The circular flow, the ‘missing link’ in economic causality (EDM: 43 ff.) supposedly discovered by the physiocrats, described

how each economic period becomes the basis for the subsequent one, not only in a technical sense but also in the sense that it produces exactly such results as induce and enable the members of the economic community to repeat the same process in the same form in the next economic period; how economic production comes about as a social process, how it determines the consumption of every individual and how the latter in its turn determines further production. (EDM: 43)

In other words, this is the stationary process or the condition for equilibrium, which are analytically equivalent (BC: 42 n.; also 68). On the other hand, development was defined as a quantum jump in the social conditions of the system, ‘that kind of change arising from within the system which so displaces its equilibrium point that the new one cannot be reached from the old one by infinitesimal steps’ (TED: 47 n.; also Schumpeter 1935: 4). The ‘static conditions’ exclude the cycle but not growth: in fact, ‘growth’, defined as the combination of the evolution of capital accumulation from savings and of the population, was included in the notion of static equilibrium (Schumpeter 1927: 289 ff.). Equilibrium was thus defined as a ‘shifting centre of gravitation’ in a system that also generates the internal impulse for change, that is, for the rupture of the equilibrium conditions. While development accounted for the nature of the change (BC: 560 n.), equilibrium described the absorption of change (Schumpeter 1937: 159), that is, was defined as the stability property of the system. This topic will be discussed further.

The real economic system cannot be understood without the integration of both processes: in fact, even if Schumpeter sometimes indicated that ‘perfect’ equilibrium was never really present (BC: 52) and that it was a ‘methodo-logical fiction’ (p. 964), or if he criticized Walras's and Clark's
presentation of real prices oscillating around equilibrium (\textit{HEA}: 999, 1000 n.), his general approach was to argue for an integrated account of the development process as including both change and equilibrium. In his ‘first approximation’ to the (\textbf{p.53}) theory of the business cycle, equilibrium existed at the end of the depression and before the prosperity. In the ‘second approximation’, when the ‘secondary wave’ was considered and the cycle was described in four phases, equilibrium conditions were met at two of the inflexion points, namely when the recession leads to depression and when the revival leads to prosperity and a new cycle is supposed to begin.

Three main points should be emphasized. First, this schema considered the stationary process or the equilibrium conditions to be a special case of the dynamic movement, specifically, that corresponding to the discrete points where the movement is null (\textit{BC}: 70–1, 963). This quite closely matches the mechanical Mill–Comte definition of the distinction between ‘statics’ and ‘dynamics’ and the possibility of conducting a static analysis, the ‘bare bones of economic logic’, ‘clearing the ground for rigorous analysis’ (\textit{BC}: 68).

Second, the existence of equilibrium was stated and its stability was defined as the real processes of absorption of change and of disturbance:

The thing that matters to us is nevertheless this tendency [towards equilibrium] considered as an actual force, and not the mere existence of ideal equilibrium points of reference. . . . We wish to distinguish definite periods in which the system embarks upon an excursion away from equilibrium and equally definite periods in which it draws towards equilibrium. (\textit{BC}: 69–70)

Or also, without room for doubt: ‘Common sense tells us that this mechanism for establishing or re-establishing equilibrium is not a figment devised as an exercise in the pure logic of economics but actually operative in the reality around us’ (\textit{BC}: 47, also 56).

In this sense, the mechanism of equilibration provided the resistance to change in the economic system, namely the defence of established business and institutional traditions: it was the creation of order subsuming the creation of novelty,\(^{13}\) for example imitation restoring equilibrium after innovation.
Equilibrium or order would be the moment of the formation of prices, while development or disorder is the evolutionary process: in Schumpeter’s emphatic words, ‘fluctuations must be fluctuations around something’ (BC: 69).

Third, this did not imply that equilibrium was considered the desirable situation. In the first approximation, it was considered the situation where the promises of the boom were fulfilled, that is where the availability of consumption goods increased for the whole community (TED: 161). But in the second approximation, this was certainly more complex, since the system was described as in permanent turmoil, and its change—the disequilibrium (p.54) processes—was the only form of progress. From this point of view, Schumpeter clearly opposed the ‘classics’ and the general equilibrium paradigm, and even condemned their incapacity to incorporate real economic evolution: as he stressed in Economic Doctrines and Method, at the very time when the first modern industrial crises were erupting, the ‘classicals’ were still arguing for Say’s Law and rejecting the theoretical possibility of disequilibrium, against all easily available evidence (EDM: 150). The main achievement of Juglar, by contrast, was precisely to define a new agenda for research, indicating the problem, describing it empirically, and presenting an explanation (Schumpeter 1927: 287). In other words, the tendency to novelty that moves the economic system forward depends on the ability of the entrepreneur to challenge equilibrium: ‘What a miserable figure he is, this economic subject who is always looking so anxiously for an equilibrium. He has no ambition and no entrepreneurial spirit; in brief, he is without force and life’ (DW, quoted in Swedberg 1991: 29).

With these qualifications, Schumpeter’s theory of the cycle can now be reassessed. The motion of the system was analysed under a steady-state representation; then the possibility of change was introduced as an independent and separable dimension, since both correspond to social processes that can be isolated. In other words, Walras indicated a convenient approach to discussing one of the processes (BC: 47), but this was not enough, since evolution should also be explained: for Walras the needs were given, while for Schumpeter the real economic processes created new needs and led to deep transformations. The Schumpeterian research programme
consisted of the bold task of providing the dynamic counterpart of the Walrasian schema, in order to create a truly general theory.

This implied that some sort of logical separability was possible between the problems of growth and cycle, since growth was reduced to the monotonic trend of capital accumulation through savings and to population increase, both being added to an equilibrating process. Of course, this did not solve the statistical problem of the assessment of the trend and cycle, since there was no real trend of equilibrium—only a number of discrete equilibrium points, two for each cycle—and since the cyclical process by itself displaces the centre of gravitation upwards.

And, moreover, the three-cycle schema implied that the equilibria of the shorter cycles were defined in the artificial representation of the trend line of the larger cycles, and that the single true equilibrium occurred at the very beginning of a Kondratiev wave, when prosperity was to commence and the equilibria of the three types of cycles coincided. All other points are ‘neighbourhoods of equilibrium’, therefore unstable for a new, very structural reason: the dynamics of evolution in the larger cycles—the Kondratiev long waves—overdetermined the shorter ones, even when they were in the neighbourhood of equilibrium in their own motion. This was a form of representation of the feedback mechanisms in action in real economies, but it added singular difficulties to the mathematical treatment of the model—and Schumpeter certainly had these in mind when he accepted that his theories were very hostile to mathematical formalism.¹⁴

This permanent tendency to the dislocation of the centre of gravity of the system and the complex interaction of the different cycles account for an original form of instability, created by the system itself. Schumpeter's theory was a system of self-generating complexity and instability, where the equilibrium concept really played only a very subsidiary role. But Schumpeter was not prepared to break with the Walrasian half of his theory, for philosophical rather than theoretical economic reasons. The rationale for this refusal can be discovered in his general view of science and the definition of his own place in economic theory, which we will proceed to
discuss using the illustration provided by the sharp controversy of Schumpeter against Keynes.

2.4 Schumpeter and Keynes: ‘Semi-Autonomous Variables’ and Reasoned History

Schumpeter and Keynes were born in the same year and, although from distant points, followed the same events, frequented the same theories, and discussed the same topics. But they reacted differently to these times, and formulated quite opposing hypotheses and conjectures. Moreover, there was between them a larger distance than the geographical one: there was misunderstanding and rivalry.

This was mostly true in relation to Schumpeter, who intensely envied the success of his colleague: his review of Keynes's *General Theory*, (Keynes 1936), issued almost immediately after the publication of the book, was a lively and almost incomparable image of bitterness and scientific aggressivity. The short article was full of offensive remarks: Keynes's *General Theory* was based on ‘artificial definitions’, ‘paradoxical-looking tautologies’, ‘treacherous generality’, psychological laws of a ‘bygone age’; it was ‘Ricardian in spirit and intent’, which certainly was not intended to please Keynes (Schumpeter 1936: 792, 793). The final touch is a monument to perversity: challenging Keynes's conceptions of saving and effective demand, Schumpeter gave the example of Louis XV, who was supposed to have called Madame Pompadour in and asked her to spend as much as possible, increasing effective demand in order to avoid depression (p.56) and thereby guarantee the well-being of the people—of course, concluded Schumpeter, if it all finished in a blood-bath, this was a mere coincidence.

But still, this review indicated two important themes. The first was the critique of the short-term view (‘a theory of another world’), which did not make possible any change in the production functions, since ‘reasoning on the assumption that variations in output are uniquely related to variations in employment imposes the further assumption that all the production functions remain invariant. Now the outstanding feature of capitalism is that they do not but that, on the contrary, they are being incessantly revolutionized.’ (Schumpeter 1936: 793). Consequently, a theory of short-term movements would ‘exclude the salient
features of capitalist reality’, as Schumpeter maintained later on (HEA: 1144).

The second theme was the critique, on the same grounds, of the three *dei ex machina* of the *General Theory* (‘there is a whole Olympus of them’, wrote Schumpeter (HEA: 794)): expectations, the psychological law of consumption, and the schedule of the liquidity preference—all three could not be part of an economic explanation, according to Schumpeter. These two main critiques are discussed in the following pages, in order to provide a brief comparison between the research programmes of Keynes and Schumpeter and their assessment of evolution.

In the 1936 review, Schumpeter rejected the use of expectations and of all psychological laws (such as Keynes's explanation of consumption and of liquidity preference), which were introduced as exogenous factors and as ultimate causes for the economic behaviour. Those explanations were considered to be tautological,\(^\text{15}\) like any purely exogenously driven form of causality.

The exact nature of expectations in Keynes's system is a matter of controversy. Mini considers that they are not independent variables, since they are part of the nature of economic agents (Mini 1991: 179). But this is not completely convincing, since economics is not necessarily a global explanation of all the features of real-life agents. O'Donnell, on the other hand, considers that long-term expectations—those concerned with decisions to invest and hence with transformations of the economic system—are typically independent variables, while short-term expectations, concerned with decisions to use the existing capital equipment, are endogenous (O'Donnell 1989: 241, 236). In this interpretation, long-term economic expectations determine the short-term expectations in the specific sector of capital goods, and thus the employment in this sector, while short-term expectations determine the employment in the sector of consumption goods.

\(^\text{p.57}\) Keynes's treatment of expectations was once again deeply rooted in his philosophy. From the first drafts of what would become the *Treatise on Probability*, (Keynes 1921; henceforth *TP*), Keynes insisted that logic should include uncertainty and not only deductive relations, which are certain
or true if logically correct. This general logic implied the passage ‘from the logic of implication and the categories of truth and falsehood to the logic of probability and the categories of knowledge, ignorance and rational belief’ (TP: 62).

In this theory of rational belief, uncertainty can arise from three different sources: from the probability of an event (the measure of the degree of certainty), from the weight of the argument (namely, the nature of the available evidence), and finally from the unknown probabilities of events. But the traditional approach to mathematical expectations assumes the numeric and measurable nature of all variables and events, and thus ignores the weight of the argument and risk.16

In other words, uncertainty was to Keynes the reflection of a world where there are measurable and non-measurable qualities, or quantitative and qualitative phenomena. O'Donnell suggests that Keynes's interpretation of reality distinguished between those features of a quality that could be described by a degree and those that could not, including in this first category those objects or properties describable as the sum of its parts (weight), those for which the degree of the whole equates the degree of the parts (colour), and finally those for which the whole is independent of the parts (beauty) (O'Donnell 1989: 62). In this framework, ‘colour’ and ‘beauty’ are the examples of organic units, while ‘weight’ is a non-organic one. Expectation is typically one of such variables that can only be represented as an organic system, and therefore are non-calculable and impossible to represent in the Cartesian world of purely deductive logic. This corresponds to the notion of semi-autonomous variables, which are to be represented as endogenous or exogenous depending on the scope of the model,17 since they represent the crucial connection between distinct levels of abstraction in the context of non-contained models. The relevance of these variables flows from the fact that they are not compatible with the deterministic view of causality, and that they represent the organic synthesis of network causality and complexity.

Rejecting this whole approach to expectation from his very first writings,18 Schumpeter tried several times to formulate an alternative one. He failed to (p.58) do so. In Business Cycles, dealing with Knight's concept of expectations,
Schumpeter went back to the stationary state in order to indicate that, even without omniscience, expectations are based on experience, and perfect foresight is possible and indeed trivial in that case \((BC: 52)\). But if disturbances affect the system, expectations consequently change: they may either preserve or prevent disequilibria \((p. 53)\). The drastic solution was to treat expectations as equilibrating features, and this was postulated in a very uncomfortable way, which is distinctively dogmatic and neoclassical:

But although they [the disruptive effects of ‘certain types of expectations’] may often temporarily counteract it, they do not in themselves disprove the existence of an equilibrium tendency or the proposition that at times it prevails in such a way as actually to draw the system toward equilibrium. The real trouble to the theorist comes from the fact that introducing expected values in his variables—we will now, on the one hand, assume that they are expected with certainty and, on the other hand, also include past values—changes the whole character of his problem and makes it technically so difficult to handle that he may easily find himself unable to prove an equilibrium tendency which, nevertheless, may exist, or even the existence and stability of the equilibrium position itself. \((BC: 54)\)

In such a framework, expectations can then be treated either as endogenous variables contributing to the equilibrating tendencies, or as exogenous variables fully known; but both solutions are unsatisfactory. Schumpeter clearly preferred the first solution, \(^{19}\) arguing that otherwise expectations would constitute a theoretical blank to fill another blank \((HEA: 312)\). But even in that case, he argued there is no explanation for these variables \(ad \ hoc\) defined as endogenous: the only available interpretation for expectations is to ignore them, since they are so difficult to handle \((BC: 55)\).

In our view, the only solution for this difficulty is that represented by the concept of semi-autonomous variables, those not wholly endogenously explainable by the system and whose behaviour is not autonomously determined by exogenous events in its full extent. In fact, they are not parameters, but the theoretical counterparts of the organic and complex realities; that is, they indicate the building nonlinearities of the system. Now, what is striking is that
Schumpeter in some way touched this exact problem when he left the domain of stationarity—where he discussed Knight's concepts of uncertainty and risk—and considered the notion of development.

In *Business Cycles*, Schumpeter argued that there are three different types of variable: (1) *theoretical*, that is, those related to a law and, consequently, *(p.59)* invariant in their behaviour; (2) *random*; and (3) *historical*, defined as 'hybrid variables', since they represent the 'theoretical law in a process of change' (*BC*: 194–5). 'Hence we may . . . define a historical variable as a variable, the stochastic normal of which changes owing to a change of its theoretical normal' (p. 196). And since 'the very concept of historical sequence implies the occurrence of irreversible changes in the economic structure which must be expected to affect the law of any given quantity' (*CSD*: 72 n.), it implies that very peculiar sort of variables, the kind that cannot be defined as endogenous and cannot be simplified as exogenous (in the Schumpeterian sense of purely stochastic variables, or otherwise as identified factors that are exterior to the scope of the theory). In other words, the morphogenetic process of mutation and evolution cannot be encapsulated in the strict formalism of the mathematical models of simultaneous and linear equations under the current qualifications—since these are not organic representations—and requires the inclusion of a new type of explanation, historical by nature.

Innovation, the key concept of Schumpeter, cannot be fully understood unless in that framework, and it is easily verifiable that all of Schumpeter's arguments against the Keynesian concept of expectation are directly extensible to innovation. Its source (invention) is exogenous and therefore not explained. Its diffusion is endogenous, but it is simultaneously a source of disruption in the system. Nowhere is it fully explained nor is it completely explainable by the system, since it cannot be represented by the postulated relations, as it depends on singular decisions. Innovation is endogenous to the system, but it is finally determined by the entrepreneurial function, that unique capacity to make new combinations, which is clearly outside the domain of the model. And, of course, this boundary between endogenous and exogenous variables can change according to the purposes of each inquiry, as
Schumpeter was aware, and is therefore irrelevant as a classification criterion.\(^{20}\)

In a much more prudent and realist way, Keynes introduced three main expectations-dependent variables: propensity to consume, marginal efficiency of capital, and liquidity preference. He was right to do so and to stress (p.60) the irreducible uncertainty in organic systems.\(^{21}\) The reason he could do so, without being entangled like Schumpeter in a self-contradictory net of explanations, was because Keynes's philosophy suggested the notion of organicity and therefore liberated him from the stringency of the concept of equilibrium, although he was not able to incorporate these notions in a dynamic approach.\(^{22}\)

This is not the end of the story, for in the Schumpeterian explanatory system there is a further complication: he postulated the existence of a tendency to equilibrium, which was off set for some periods by a counter-tendency propelled by innovation, and insisted on the actual existence of some equilibrium points in every cycle. This suggests the application of the traditional statistical methods of analysis of the trend as the loci of those equilibria, and of the cycle as the deviations, as in the mainstream tradition. But Schumpeter did not accept that scheme, in spite of some rhetorics supporting Wicksell's and Frisch's rocking horse metaphor, since his own impulses were defined as endogenous and therefore equilibrium was supposed to be by nature unstable: instability was structural, the disturbances changed the system, and that was the condition for the progress of capitalism. As a consequence, econometricians reacted with great hostility to his formulations, which could not be reduced to a domesticated system of equations.

Tinbergen sharply criticized *Business Cycles*, considering that the book was ‘alien to econometrics’, since for Schumpeter the relevant variables were the shocks, and he ‘belittles the importance of the mechanism’, which for econometricians ‘deserves the main attention’. Tinbergen could not accept Schumpeter's theory of the impulses being endogenous to the system, since this was not compatible with the traditional cycle model and econometric schemata (Tinbergen 1951: 59, 60). Schumpeter implicitly answered this criticism, commenting that Tinbergen's model ‘describes repercussions
and propagations without saying anything about the forces or causes that put them into motion’ (Schumpeter 1937: 162).

In fact, Schumpeter tried to save his allegiance to orthodoxy, stating that the impulses could be of two kinds, both compatible with equilibrium:

Now, what causes economic fluctuations may either be individual shocks which impinge on the system from outside, or a distinct process of change generated by the system itself, but in both cases the theory of equilibrium supplies us with the simplest code of rules according to which the system will respond. This is what we mean by saying that the theory of equilibrium is a description of an apparatus of response. (BC: 68)

(p.61) Later on in the same book, Schumpeter compared the impulses with a water flow (BC: 179), which is close to Frisch's analogy in his 1933 paper. Nevertheless, there remain some remarkable differences: Frisch supposed a damping propagation mechanism, which is the most coherent way to reintroduce the notion of stability of equilibrium, while Schumpeter described a specific oscillator, representing a cyclical and unstable form of growth. And this is why Schumpeter could never accept Frisch's metaphor of the rocking horse as a convenient representation for the process of cycles (Louçã 1997).

In the History of Economic Analysis, there is another metaphor for the explanation of this particular system—and, eventually, for the failure of the current statistical methods—when Schumpeter indicates that the economic system is a resonator for the impulses, just like a violin: the impulse and propagation autonomous systems are clearly stated, but neither the wooden box nor the movements of the fingers of the musician may fully explain the aesthetical pleasure of a concert (HEA: 1167).

Furthermore, the propagation and cyclical mechanisms in Frisch's model may explain the cycle, but they do not explain the trend–cycle behaviour. This is, of course, a major difference with Schumpeter's theory, which was instead concerned with the creative responses of the economic system to whatever impulses may exist (BC: 72). Indeed, when developing his theory, Schumpeter discussed this problem in
detail. The propagation mechanism was traditionally considered as the equilibrating Walrasian feature of the modelled economy, but the theory did not indicate when those equilibrium conditions are fulfilled. Schumpeter argued that the equilibrium force really existed, but he stressed that actual equilibria were attainable only at discrete and rare points, to be immediately abandoned by the motion of the system. Therefore, the equilibrium line indicated in the statistics was only an artificial representation:

They [the neighbourhoods of equilibrium] are the most relevant items of a series . . . A line or curve through those points, or a band or narrow zone through those neighbourhoods, supplies a trend that really has economic significance. . . . We know . . . that this trend does not describe a phenomenon distinct from the cycle. On the contrary, since evolution is essentially a process that moves in cycles, the trend is nothing but the result of the cyclical process or a property of it. . . . Moreover, we also know that it carries realistic meaning only in discrete points or intervals. If we connect them by straight lines . . . it must be borne in mind that the stretches between the neighbourhoods are nothing but a visual help, and devoid of realistic meaning. No fact corresponds to them. Real is only the cycle itself. (BC: 206–7)

Or else, criticizing the statistical methods: ‘if trend-analysis is to have any meaning, it can derive it only from previous theoretical considerations, which must not only guide us in interpreting results, but also in choosing the method. Failing this, a trend is no more than a descriptive device summing up past history with which nothing can be done. It is, in fact, merely formal’ (Schumpeter 1930: 166).

Schumpeter acknowledged the efforts by Mitchell to solve the same problem of the relation between the trend and cycle through the ‘reference cycles’, ‘a judicious compromise between eliminating trend and leaving it in’ (Schumpeter 1952: 339), and he even defined the formal trend as those subintervals where the mean value is monotonically increasing or decreasing (Schumpeter 1935: 3). But the economic meaning of the trend and the applicability of the ‘statistical method’ (of least squares) were supposed to depend on the interpretation of an economic mechanism explaining the
monotonic variation. The only mechanism of that type is growth (savings and population), which constitutes the ‘real trend’ (*BC*: 201ff.); but, as previously indicated, this was considered to be a minor influence in the overall behaviour of the system.  

In short, Schumpeter claimed that there was a real trend of growth that was a secondary feature of the model, that there were discrete points where the tendency towards equilibrium was achieved, and that there was a causal process inseparably explaining both cycle and evolution. Thus, the meaningful trend, the ‘trend-result’ describing evolution and the cycle, synthesized one and the same process, and therefore no multiple regression could be successfully applied to it, according to Schumpeter, since it implied the decomposability of this process. But, according to the theory, there was no meaningful separation of the variables of impulse (innovation) and its propagation mechanism, since both are endogenous, and every decomposition would be arbitrary: ‘It follows that barring the elements of growth the trends of our times series are not due to influences distinct from those that create the cyclical fluctuations but simply embody the results of the latter. To these “result-trends” . . . it is entirely unwarranted to apply formal methods of the type of least squares’ (Schumpeter 1935: 6).

The same argument was given in other works (Schumpeter 1930: 167; *BC*: 198). This was a substantial reason for the rejection of the Slutsky effect of the impact of random shocks on the propagation mechanism (*BC*: 180–1). Here is the heart of Schumpeter's model: equilibrium was the reference to the trend, but this was without implication in the choice of method of analysis, since it could not in any case be meaningfully separated from the cycle itself. Furthermore, to add to the analytical difficulty, the theory provided an explanation for the automatic disturbance of equilibrium and indicated the relevant process to be the disequilibrating and innovative process of creative destruction, the central feature of capitalism—and the very reason for its survival and adaptation.

This was in fact the consequence of the centrality of ‘hybrid variables’, whose ‘theoretical norm’ changes following the irreversible process of mutation (*BC*: 196, 198): the historical approach is therefore necessary to a general theory of the
cycle, since the cycle is always a ‘historical individual’ (p.63) (Schumpeter 1935: 2). The trend exists, but in our analytical mind: it is the ‘gravitational axis of the smoothed curve’ in our own representation (BC: 210). Equilibrium exists, but it is exiled to the domain of the secondary and artificial representations. The cycle is the only persistent and meaningful reality: it is the name of progress.

And with the cycle we are back to the concept of evolution, or history: ‘The essential point to grasp is that in dealing with capitalism we are dealing with an evolutionary process’ (CSD: 82).
2.5 Conclusions
Schumpeter's ambition was immense: he intended to do no less than formulate a theory sufficiently general to encompass the determinateness of general equilibrium and the indeterminateness of general disequilibrium. 'I was trying to construct a theoretical model of the process of economic change in time, or perhaps more clearly, to answer the question how the economic system generates the force which incessantly transforms it' (Schumpeter 1951: 158–9). This revolutionizing force, the *alma mater* of capitalism, was innovation, and Schumpeter had been after it since 1911–12 (Schumpeter 1927: 292).

But innovation is historical by nature, and can be understood only as a historical process: its clustering and non-random distribution (*BC*: 75), and its relation to the changes in organizational and institutional structure, are part of the organic functioning of modern capitalism. This is why reasoned history should be used in economics as part of the core function of theorizing, in a triple sense. First, theory itself can be understood, according to Schumpeter, in two different senses: either as a *corpus* of explanatory hypotheses, or as a body of conceptual tools (Schumpeter 1952: 326–7), and his preference for the former interpretation was based on a consideration of the nature of economics itself as an evolutionary process, resistant to the simplistic tools of applied mechanics. Second, the theory of changes could itself be self-defeating, based as it was on occasional disturbances—the analytical form to introduce explanatory events, abandoning any hope to explain them—but it could also choose an alternative strategy, and study the movements of disequilibrium: ‘the cycles are the form of capitalist evolution’ (Schumpeter 1952: 333, or 1927: 295), and this is why reasoned history is the ultimate complete theory (1927: 298). Third, and because of the previous arguments, the formal methods, based on the rigour of statistics, could fail us just when they are most needed to prove a theory: reasoned history is required since the subject matter of this research, i.e. (p.64) change and evolution, is by nature historical—the cycles ‘carry historical meaning which . . . is much more important than fulfilment of any formal criterion’ (Schumpeter 1935: 7).
Following these assumptions and intuitions, Schumpeter pleaded for the ‘coalition’, or combination, of different types of economists in his introductory paper to *Econometrica* (Schumpeter 1933: 7): rigour and proof depended, he claimed, on this articulation of knowledge, essential for the explanation of real economies.

In all this inquiry, one forerunner, who was also a contemporary, played a major role in the definition of Schumpeter's vision: that was Kondratiev, a Russian economist widely known at the time for his hypothesis on long cycles. Schumpeter endorsed this theory and indeed became its major proponent: it completed his own vision of the evolutionary process of capitalist development, providing the historical framework for the process of structural change provoked by major innovations. Kondratiev provided the bridge to Marx: accumulation explained the inherently disequilibrating and catastrophic process (*HEA*: 749).

Unlike Kondratiev, Schumpeter did not use large statistical evidence to corroborate his theory: historical theory, abstract models, and description of the evolution of real series and events were the dominant methods in *Business Cycles*. For one reason, he was not convinced about the qualities of the available statistical devices, and he rejected the methods of least squares and polynomial fitting, given that they require uniform variance and normal distribution of the residuals, which could not be taken for granted (*BC*: 201). For another reason, he believed that the entangled cycles could be approximately decomposed, since he presented the cycles as perfect multiples of one another and argued that the level of the \( n \)th cycle is the equilibrium level for the trend of the \( (n+1) \)th cycle; but he also accepted that the results could not be unambiguous, since he simultaneously suspected that nonlinear phenomena (‘mutations’) could occur and could dominate historical-economic evolution. The lack of regularity in the real cycles accentuated this suspicion (Fellner 1956: 44–5). In this sense, both Schumpeter and Mitchell accepted that the best fit could lie.

On the other hand, Schumpeter presented the trend as the result of the cycles, and defined it as a line passing through certain neighbourhoods of equilibria—and a line with the property of being economically meaningful only at those discrete points. He therefore had substantial reasons for
rejecting the principle of decomposition, since no available
technique could deliver those results without a shadow of a
doubt. But Schumpeter did not develop a new theory of statics
and dynamics and accepted the dominant views. He was
bounded by the ideology of equilibrium and considered that
the irreversible processes were representable by a moving
equilibrium frontier, as Kondratiev did.

Schumpeter knew that the reversible movements are actions
without change and thus fictitious entities: reversibility does
not exist in economics. (p.65) Last but not least, it is
irrelevant to explain cycles as fluctuations over a trend if the
basic historical process, which generates the trend and the
cycle itself, is unexplained. This had been the vexatio questio
of Kondratiev's whole academic life and work.

Notes:
(1) Schmoller developed a historical method in economics,
describing the successive stages of development of societies
with a combination of sociological, ethical, and historical
insights. His long time perspective was invoked by Marshall
when he criticized the Comtian distinction between statics and
dynamics: Schmoller's Grundrisse was 'an unsurpassed
embodiment of wide knowledge and subtle thought' (1907
preface, in Marshall 1890: 48). Nevertheless, Keynes indicated
that Marshall had all his life been dissatisfied with the 'learned
but half-muddled work of the German Historical
School' (Keynes 1972: 210).

(2) Schumpeter maintained this opinion much later: in HEA he
presented the whole polemic as 'a history of wasted
energies' (HEA: 814ff.).

(3) This was also another influence of Comte, which
Schumpeter registered in EDM: the philosopher insisted on
the 'altogetherness of social life and the need for an historical
method for other problems other than the purely economic
ones' (EDM: 96). Of course, later Schumpeter developed this
programme much further, since historical methods were
considered essential to account even for 'purely economic'
problems.

(4) One can hypothesize that the rejection of DW, the 1908
book on the Methodenstreit, was connected to this important
change in Schumpeter's opinion and his later incorporation of
essential elements of the Historical School into his own system. Simultaneously, important elements of differentiation with the marginalist school were developed by Schumpeter: unlike Menger, he did not consider that the value theory required any psychological foundation (Bottomore 1992: 19), and he praised Pareto for getting rid of the concept of ‘utility’ and suggested that maximizing rationality was not a realist feature (Ten Great Economists (TGE): 179, 192).

(5) Samuelson’s interpretation of this event is that Schumpeter loved to take the ‘unpopular side’ of a dispute (Samuelson 1951: 49–50, 50 n.). Faced with the evidence, this is obviously a minor point.

(6) The impact of the Cowles Commission research programme was by then dominant in the profession: the econometric revolution won the day. Friedman, by then a researcher at the NBER, argued at the conference that a final synthesis would be reached between the NBER method and the Cowles approach (Friedman 1951: 114). But Koopmans was so convinced of the superiority of the econometricians’ approach that he could recommend, in an internal memorandum to the Cowles group with a balance sheet of the meeting, ‘Let’s not fight too much’ (Epstein 1987: 111).

(7) In a letter written in the early 1940s, Schumpeter argued that the organic nature of his thought was responsible for the difficulty of formalization: ‘there is nothing in my structures that has not a living piece of reality behind it. This is not an advantage in every respect. It makes, for instance, my theories so refractory to mathematical formulations’ (quoted in E. Andersen 1994: 2).

(8) This ‘biological term’ was used for the first time in 1941, in the Spanish preface to TED (Schumpeter 1944: 15).

(9) Schumpeter’s main argument was that the nature of the economic reality was a disequilibrating process, just as Marx conceived it (HEA: 77, 774 n.; CSD: 83). In the Japanese preface this was indicated when he argued that Marx was, with Walras, the main source of his thought, and that, unlike the latter, he discussed the dynamical processes of change. In CSD Schumpeter argued that Marx was the first to abandon the concept of ‘crisis’ as an accident, and therefore to anticipate Juglar (CSD: 41). Elizabeth Schumpeter, in her
preface to the 1951 collection of *Essays*, argued that her husband had in common with Marx the vision of capitalism as a dynamical process (Elizabeth Schumpeter; in Schumpeter 1951: 9).

(10) Of course, the ‘organic’ argument may be a trivial declaration of the self-containedness of a system, and in the previous sections several instances of such a stance were found. In this case, the ‘organicity’ of the system is fully identified with its mechanistic character; that is, a ‘natural’ system excludes purposive action. The word is used in this section in a very distinct sense, still indicating the indirect influence of the biological metaphor: an organic system includes complex and indeterminate interactions and feedbacks including with the environment: it is an open system. This is the sense used in connection with the Schumpeterian concept of an organic whole.

(11) In the first edition of *TED* (1911) Schumpeter used the distinction ‘circular flow’/‘development’; in the second edition (1926) these were replaced by ‘statics’/‘dynamics’. But since 1934, as indicated in the preface to the English edition of *TED*, ‘in deference to Professor Frisch’, and also in *BC*, Schumpeter used the distinction between static and dynamic *forms of analysis*, and stationary or development *processes in nature* (*TED* 1934 edn.: 6).

(12) Schumpeter's concept of capital was defined as a flexible resource, distinct from the technical structure of the production process (Oakley 1990: 38). It belongs to the circular flow, and is ‘that part of the social product of preceding economic periods which maintains the production of the current period’ (*EDM*: 54). Thus, there are two sources of accumulation, one being the circular flow and another the development process, which is moved by innovation.

(13) Rosenberg interprets Schumpeter's position on the circular flow as a theoretical description, as opposed to the real processes of change in capitalism (Rosenberg 1994: 43), and Swedberg interprets it as an ideal-type (Swedberg 1991: 32). But the previous quotations refute this interpretation: for Schumpeter, the circular flow was a real process, *simultaneous* with development, and a complete theory should integrate both dimensions in the same framework.
One of the main reasons for the sense of failure Schumpeter felt in his last years was his incapacity to develop a formal model for his theories. His own diary proves that he worked almost daily and helplessly with systems of equations, at least since 1934 when preparing BC and afterwards, looking for a general equilibrium model accounting for the time path of the variables (Allen 1991: ii. 8, 142, 177, 190, 227). But his colleagues, such as Goodwin, witnessed his difficulties with mathematics. In spite of that, he suspected that the available differential and difference equations were unsuited to define an evolutionary system including social relations and complex behaviours.

'But expectations are not linked by Mr Keynes to the cyclical situations that give rise to them and hence become independent variables and ultimate determinants of economic action... An expectation acquires explanatory value if we are made to understand why people expect what they expect. Otherwise expectation is a mere *deus ex machina* that conceals problems instead of solving them' (Schumpeter 1936: 792 n.). Of course, this would be the relevant form of causality if Schumpeter still accepted his previous account in TED, where the cause was considered to be the first relevant exogenous factor for the system (TED: 10).

Risk was defined by Keynes as the mathematical expectation times the probability of failure (Keynes 1921: 348)—this was written approximately one decade before the seminal book by Knight.

In the General Theory (GT) these variables (propensity to consume, marginal efficiency of capital, rate of interest) were formulated as independent, although Keynes recognized that it was an impure solution since they were influenced by other variables (expectations, etc.). The designation of the semi-autonomous variables as exogenous is the *testemonium paupertatis* of modelling.

In TED, before the publication of any of Keynes's writings, Schumpeter had already stated that he was firmly opposed to the 'psychological prejudice which consists in seeing more in 'motives and acts of volition than a reflex of the social process' (TED: 43). But in TGE, Schumpeter credited Keynes with the dynamic feature of the concept of expectations (TGE:}
381, even if this aspect was never developed in Schumpeter's work. As a matter of fact, Schumpeter was generally opposed to the Keynesian concept of expectations.

(19) ‘Unless we know why people expect what they expect, any argument is completely valueless which appeals to them as *causa efficient.* Such appeals enter into the class of pseudo-explanations which already amused Molière’ (*BC*: 140).

(20) ‘What precisely is looked upon as inherent [endogenous] in it [the system] will, of course, depend on how we delimit it and which facts and relations we decide to treat as data, and which are variable’ (*BC*: 7 n.). Just at the end of the book, he indicated that variables such as ‘mentality’ could be considered as exogenous, as usually, or as endogenous, according to the researcher and to the research—*BC* is said to take normally a ‘narrow sense’ (p. 1050 n.). Of course, this created the greatest confusion among the historians of economic thought.

Hansen considered the exogeneity or endogeneity of Schumpeter's theory a ‘perennial and inexhaustible subject for discussion’, not clarified by the author himself. Here is Hansen's own interpretation, a Salomonic solution: ‘It is exogenous in the respect that it places primary emphasis upon changes in the data. Yet it is also an endogenous theory in the respect that it runs in terms of an internal, self-perpetuating system . . . whose impelling force, innovation, cycle after cycle renews the wave-like the movement. . . . [The business cycle] is an endogenous process determined by the inner nature of a dynamic economy, but it is exogenous in the sense that innovation is a change in the basic data’ (Hansen 1951: 80).

(21) In his *GT* Keynes clearly stated that a mathematical model could not represent expectations (Keynes 1936: 162–3).

(22) Because of its statistical framework, in *GT* expectations were modelled as exogenous variables and the explanation was abstracted from uncertainty, except in chap. 22, in which expectations were introduced in order to understand fluctuations. For what matters for this book, cycles and irregular growth cannot be explained unless in a dynamical context.
(23) For reasons of criticism of the correlation methods, Keynes shared such suspicion about decomposition and the concepts of ‘trend’ and ‘residuals’ (Keynes 1921: xiv. 319).

(24) In realist accounts, the problem is further complicated by the abandonment of the hypothesis of perfect competition: under oligopolistic situations equilibrium becomes indeterminate \((CSD: 79–80)\), and the same happens under monopolistic competition \((BC: 57)\).