Nikolai Kondratiev: A New Approach to History and Statistics

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Abstract and Keywords
Nikolai Kondratiev was one of the Russian economists whose work had most impact on the academic profession in the early twentieth century; after following courses by Tugan-Baranovsky, he joined in the development of new approaches to economic statistics.

As the natural follower of Clément Juglar, who first analysed the industrial or business cycle, he led the Institute of Conjuncture in Moscow in the twenties while analysing the statistics of long cycles in prices, interest rates, and trade in the principal countries.

Kondratiev was later a victim of one of the bloody purges of the Stalinist era and, after eight years in prison, was finally shot.

Yet his short period in the Institute and his rare publications, some of them translated into German and then into English, became known to many western colleagues: Simon Kuznets,
Wesley Mitchell, Ragnar Frisch, Jan Tinbergen, and especially Joseph Schumpeter, who named the long waves of industrial change ‘Kondratievs’ or ‘Kondratiev cycles’.

Kondratiev became the only Russian-born economist to be part of the foundation of the Econometric Society in spite of living in jail at that time and his contribution, his hypotheses and statistical demonstrations, and the fierce debate with his own colleagues at the Institute are discussed in detail in this chapter.

Keywords: innovation, Joseph Schumpeter, Nikolai Kondratiev, Long Wave, prison, statistics, structural change

3.1 Introduction

As we saw in the previous chapter, when he needed an inspiration for his life-long attempts to recombine history and economics, Schumpeter looked to Kondratiev for applied research on reasoned history and statistics. This chapter presents and discusses what he could have learnt, and what we can use nowadays from that inheritance.*

Nikolai Kondratiev (1892–1938) was one of the, if not the most, influential of the talented young Russian economists working in the first third of the century, and certainly the best known internationally at that time. His decisive contribution was the presentation of the hypothesis of long waves in capitalist development—named by Schumpeter and known thereafter as ‘Kondratiev waves’—which for some time represented an important topic in the research agenda of economics. Nevertheless, the contemporary dominance of equilibrium economics exiled this research to the fringes of economic history, which is still considered to be a secondary, and not entirely scientific, distant relative of the discipline.

Yet, at least for a few decades, there was widespread agreement about (a) the relevance of the long-term movements identified by Kondratiev, since the existence of long periods with impressively distinctive patterns of development was widely recognized, and (b) the relevance of the newly developed statistical methods for checking the existence of the long waves. Such a consensus did not, however, extend to the precise explanation of the causes of these long phases or cycles.
This chapter presents an inquiry into the disputes, the syntheses available during the first half of the last century, and the problems emerging from this research, which are still crucial for any inquiry into the dynamics of capitalism, as Kondratiev suggested a challenging agenda for there combination of history and economics. Section 3.2 indicates the direct inheritance of Kondratiev's research. Then the predecessors, the key features of Kondratiev's analysis, and the elements of the consensus and dissension it generated among contemporary (p.67) authors are in turn discussed in Sections 3.3 and 3.4. Section 3.5 deals with the decomposition problem. Finally, Section 3.6 considers the impact of his work at the time, and Section 3.7 presents some conclusions.
3.2 Kondratiev’s Life and Work
Nikolai Dimitrievich Kondratiev was born on 4 March 1892 in the province of Kostroma, north of Moscow, into a peasant family. He studied at the University of St Petersburg, following courses given by Tugan-Baranowsky and other economists, epistemologists, and historians. A member of the Revolutionary Socialist Party, his initial professional work was in the area of agricultural economics and statistics and the important problem of food supplies. On 5 October 1917, at the age of 25, he was appointed Minister of Supply of the last Kerensky government, which however only lasted for a few days.

After the revolution he dedicated his attention to academic research. In 1919 he was appointed to a teaching post at the Agricultural Academy of Peter the Great, and in October 1920 he founded the Institute of Conjuncture, in Moscow. As its first director, he managed to develop the Institute, from a small body boasting just a couple of scientists into a large and respected centre with fifty-one researchers in 1923.

In 1923 Kondratiev intervened in the debate about the ‘scissors crisis’ (the growing divergence between prices of agricultural and industrial products), adopting the line taken by most of his colleagues. In 1923–5 he worked on a five-year plan for the development of Soviet agriculture. In 1924, after publishing a book in 1922 presenting the first tentative version of his theory of the major cycles (see Table 3.1), Kondratiev travelled to England, Germany, Canada, and the United States, and visited several universities before returning to Russia. As a supporter of the ‘New Economic Policy’ (NEP), he favoured the strategic option for the primacy of agriculture and the industrial production of consumer goods over the development of heavy industry.

Kondratiev’s influence on Russian economic policy lasted until 1925, declined in 1926, and was over by 1927 (V. Barnett 1995: 431). By that time, the NEP had been abandoned after a political shift in the leadership of the CPSU. Kondratiev was removed from the directorship of the Institute in 1928 and arrested in July 1930, accused of being a member of an illegal but probably non-existent ‘Peasants’ Labour Party’. As early as August of that year, Stalin wrote a letter to Molotov asking for his execution (V. Barnett 1995: 437).
Condemned to eight years in prison, Kondratiev served his sentence, from February 1932 onwards, at Souzdal, near Moscow. Although his health deteriorated and the conditions were bad, he still managed to continue his research and had even decided to prepare new books. Some of these texts were indeed completed and were published in Russian in the early 1990s and later abroad (Kondratiev 1998). While in prison, Kondratiev sent his wife the plan for a five-volume work that would include discussions on statistics, methods for social sciences, long waves, and other matters. He would never conclude that work.

His last letter was to his daughter, Elena Kondratieva, on 31 August 1938. Shortly afterwards, on 17 September, he was subjected to a second trial, condemned, and executed by firing squad. Kondratiev was 46 at the time of his murder and was rehabilitated only after almost forty years, on 16 July 1987.

During his short and tragic life, Kondratiev gained the respect of academics all over the world. He was a member of several international scientific associations and his papers were translated and published abroad. Political leaders commented on his work, his interpretation of the history of capitalism proved to be a powerful and challenging vision, and he contributed to the early spread, application, and discussion of new statistical methods and concepts. Consequently, when the inaugural list of Fellows of the Econometric Society was to be drawn up, his name was immediately proposed: Frisch wrote to Schumpeter on 7 October 1932 suggesting two Russians, Kondratiev and Slutsky. Subsequently, Kondratiev—who was already in prison—became the sole Russian among the twenty-nine founding Fellows of the Econometric Society elected in August 1933, along with Frisch, Mitchell, Schumpeter, Keynes, Divisia, Bowley, Amoroso, Fisher, Moore, Schultz, Gini, Haberler, Hotelling, and other eminent economists.

Table 3.1 lists Kondratiev’s major works dealing with the problem of long cycles. (Some early work on epistemology is not mentioned and has not yet been translated into non-Russian languages.) Note the early translations into German and English of some parts of his crucial papers.

Kondratiev’s papers had an immediate and major impact when they were published, and the rapidity with which parts of his
papers were translated and published helps to explain his fame and election to the Econometric Society. Moreover, some of the most influential economists, statisticians, and (p.69)

Table 3.1. Main Works by Kondratiev

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<th>Date</th>
<th>Book/paper</th>
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<tr>
<td>1922</td>
<td><em>The World Economy and its Conjunctures During and After the War</em></td>
<td>State edition, Vologda, 258 pp.; P&amp;C</td>
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<tr>
<td>1923</td>
<td>‘Some Controversial Questions Concerning the World Economy and Crisis (Answer to Our Critiques)’</td>
<td>Originally in <em>Socialititcheskoie Khoziaistvo</em>, 4–5: 50–80; LF</td>
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<td>1924</td>
<td>‘On the Notion of Economic Statics, Dynamics and Fluctuations’</td>
<td>Originally in <em>Socialititcheskoie Khoziaistvo</em>; a section published in <em>QJE</em>, 29, 1925: 575–83, under the title ‘The Static and Dynamic Views of Economics’; LF</td>
</tr>
<tr>
<td>1926a</td>
<td>‘About the Question of the Major Cycles of the Conjuncture’</td>
<td>In <em>Planovoe Khoziaistvo</em>, 8: 167–81</td>
</tr>
<tr>
<td>1926b</td>
<td>‘Problems of Forecasting’</td>
<td>In <em>Voprosy Konjunktury</em>, 2(1): 1–42; German version in <em>Annalen der Betriebswirtschaft</em>,</td>
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mathematicians of his time wholeheartedly supported this type of explanation, or at least considered it to be a meaningful and pertinent hypothesis—this was the case with Frisch, Tinbergen, Spiethoff, Kuznets, Mitchell, Schumpeter, Lange, Hansen, and many others. (p.70) Yet what they read was not the whole text, and in some cases was indeed a misrepresentation of Kondratiev's ideas. With the single exceptions of Kuznets and Garvy, who could read Russian and who knew the original contributions, the others read just the German or the American translation of parts of the 1925 paper, and missed out on both the 1926 paper and the debate that took place that same year, not to mention other texts by Kondratiev on central methodological issues. Furthermore, they read inexact translations: as Escudier has shown, ‘long cycles’ were uniformly translated into German as ‘long waves’, whereas Kondratiev preferred to use the concept of waves for the analysis of variables, and to use the concept of long cycles for his interpretation of global movement (Escudier 1990: 128). Moreover, these terminological and conceptual mistakes were later reproduced
in the derived translations, such as the American one. This version of Kondratiev's 1925 paper, which for a long time was the most widely accepted, appeared in 1935 in a major journal, the *Review of Economic Statistics*, but was itself translated from the second-hand German translation, did not include the theoretical part, and was limited to the presentation of the statistical method and empirical laws (Kondratiev 1935; Stolper 1984: 1647). Last but not least, not only were the essential texts not translated at the time, but they were ignored until the 1980s or 1990s, not to mention the papers written by Kondratiev while in gaol and made available in Russian only in the 1990s.

As a consequence, 'Kondratiev waves' have long since been discussed by authors who did not know the most important aspects of Kondratiev's texts. In general, until the 1990s non-Russian-speaking authors knew only the first translations of the 1920s. In fact, a complete English edition of his 1922 paper was published only in 1979, and his 1925 paper was published only in 1984. As a consequence of this ignorance of the original texts, each interpreter could attribute to Kondratiev their own version of part of his work. Only in 1992 was a collection of the main papers of the 1926 debate at the Institute of Conjuncture published in French, and in 1998 published in English.

In spite of this, what may be considered surprising is the resilience of the research programme on long waves, or long cycles as Kondratiev called them. Not only did this research attract various scientists in the 1930s and 1940s, despite their different approaches to economics, but also it was reactivated later on, just before and just after the '30 Golden Years' of postwar expansion in the industrial economies. And more recently, in the entirely new framework of complexity theory, some authors have suggested that the 'long wave' could be thought of as the representation of specific modes of entrainment of oscillations, emerging from the complex nature of economic processes (Mandelbrot 1987: 126; Lo 1991: 1308). The nature of the early consensus generated by Kondratiev in the 1920s and 1930s is the theme of the next section.
Section 3.3 Predecessors: The Early Consensus

The first part of this section briefly presents the contribution made by some of Kondratiev's distinguished predecessors, both in those cases where he was aware of their arguments and built on them and in those where he was ignorant of or completely ignored their writings. The second part of the section presents the 'Kondratiev hypothesis', in keeping with its original formulation and the ensuing debate.

The first wave of predecessors included Hyde Clarke, W. S. Jevons, Karl Marx, and Friedrich Engels. One of these was completely ignored by Kondratiev: this was Hyde Clarke, who in 1847 published a paper in *The British Railway Register* and a short pamphlet (included in Louçã and Reijnders 1999; see also Black and Collison 1992), but who owed his fame mostly to the fact that Jevons pointed to him as the creator of the hypothesis of a long cycle in economic activity (Jevons 1884: 129), and not to his own original contribution, which still remains largely ignored. Indeed, Clarke argued that the crisis of scarcity in 1847 was part of a repetitive phenomenon, and that the approximately ten-year cycles were part of a fifty-four-year movement of the whole economy, mostly motivated by harvest conditions and eventually by the impact of weather conditions. He was inspired by the previous research on time series carried out by Mackenzie (J. Klein 1997: 113-15). Jevons accepted these ideas.

Marx and Engels did not discuss cycles in very much detail, and when they did comment on the topic they essentially referred to industrial, business cycles. While engaged in writing *Das Kapital*, Marx suggested in a letter dated March 1858 that a thirteen-year cycle, obtained from the empirical evidence provided by Engels's experience as a manager, was the convenient unit for his theory explaining the timetable of the crisis by the renewal of fixed capital.

But in the second volume of *Das Kapital* Marx acknowledged Engels's comments on the shortening of the period of the business cycle, while also considering other longer periods. He quoted Scrope at some length; after describing the five to ten-year period for the construction of productive tools and fixed capital, Scrope wrote: 'The capital spent on buildings, e.g. factories, shops . . . seems not to circulate. But, in reality, these premises . . . are used up while in operation and the owner must reproduce them in order to continue his
operation. . . . This invested capital follows a 20 or 50 year rotation’ (Scrope, quoted in Marx 1885: ii. 163; our translation).

Marx commented approvingly on this passage, stating that it presented an organic view. So Marx was aware that the reproduction of capital followed different rhythms, underlying its ‘permanent disharmony’, as he frequently pointed out in the third volume of *Das Kapital* and in *Theories of Surplus Value*.

Schumpeter, who singled out Marx and mainly Engels as the predecessors of the long wave research, argued that Engels’s 1894 editorial notes to the third volume of *Das Kapital* constitute an anticipation of Kondratiev (Schumpeter 1990: 420 n.; see also Chapter 2 above). In fact, Engels simply discussed changes in the rhythms of business cycles in the preceding decades and indicated the possible reason for this—the changes in the world market as a result of the expansion of transport and communication systems—concluding that the alteration in the industrial cycle might explain the increase in the duration of upswings and downswings (Engels, in Marx 1894: iii. 489 n.). In this sense, his intuition was that the 1870s and 1880s were periods of structural change, but there was no further theoretical explanation for this phenomenon. Nevertheless, this early prefiguration of the changes in regimes of accumulation influenced some of his followers.

In an appendix to the 1886 US edition of his early book on the British working class, Engels went so far as to describe different historical periods—1825–42, 1842–68, 1868— that fitted in fairly well with the long wave chronology. Of course, both Marx and Engels were aware of the major changes caused by industrial and technical revolutions, and Marx discussed these processes clearly, outlining a theory of long fluctuations in employment and volumes of production, combined with major technological revolutions. This is indeed the closest indication of any inkling of long waves to be found in Marx’s writings:

There are intervals during which technical revolutions are less notable and accumulation appears to be, above all, a movement of quantitative expansion upon the new technical base already achieved. What begins to operate to a greater or lesser extent in such a case, whatever the actual structure of capital, is a law whereby the demand
for labour rises in the same proportion as capital does. But just when the number of workers attracted by capital reaches its peak, the product becomes so plentiful that the social mechanism seems to have come to a standstill in case of the slightest obstacle arising in the way of their sale; it is the process of alienating labour by capital in great proportions and in the most violent way that comes into operation at once; the very disruption of production makes it imperative for capitalists to strain every nerve to save labour. Detailed improvements building up little by little are concentrated under that high pressure so to speak; they find themselves embodied in the technological modifications which revolutionize the structure of capital throughout the entire periphery of major areas of production. (Marx, *Complete Works*, Russian edn., v. 49: 220–1, as quoted in Menshikov 1987: 69)

Nevertheless, this passage is mainly a descriptive account, and it is well known that this long rhythm was not discussed when Marx formulated his law of the tendency of the rate of profit to fall, or the counter-tendencies to this trend. One possible interpretation for this is that Marx did not consider that these shifts from one period to another affected the outcome of the process of striving for the realization of profit. And although some of the counter-tendencies explicitly dealt with technical change (changes either in the value of the constant capital or in the process of extraction of relative surplus), Marx did not give technological revolutions a prominent role in his theory. Furthermore, he did not explain or define these successive long (p.73) periods—it would have been difficult for him to do so, since he was writing in the years of the second wave, the first one to have a really international character. From this point of view, Marx and Engels cannot be considered direct predecessors of the long wave research, even if their concept of the reproduction cycles of fixed capital influenced most of the forerunners in this research.

Yet, Clarke, Jevons, Marx, and Engels all emphasize the same point: all witnessed periods of unrest, economic turbulence, and great famines in the midst of overproduction and plenty. And they noticed the regularity of these ups and downs, as well as the great structural changes that accompanied capitalist development. Other authors, writing in the following
years, repeatedly arrived at the same conclusion: John Bates Clark detected a period of forty-five years in the maturation of new methods of production (Clark 1899: 429); and Parvus (1901), Tugan-Baranowsky (1901: 52–3), Wicksell, and Pareto used the same calendar for describing the long periods. Thus, the first essential element of the early consensus was the recognition of what may be called the ‘Kondratiev problem’ as well as the dating of the evolutionary processes in the development of nineteenth-century capitalism. The recurrence of long periods marked by expansion as well as long periods marked by depression was believed to be indisputable, although their explanation was not. Several authors followed Marx's insights and endeavoured to explain these processes of change.

Parvus was not an economist by training: his 1901 paper was just a short text about the new conditions created by the turning point in the 1896 crisis. Considering the years of prosperity marked by the development or expansion of cities, the increased capital accumulation, and the spread of new inventions (he referred specifically to electricity, typewriters, and bicycles), Parvus argued that there are periods when there are ‘jumps’ in capitalist production, long periods of ‘Sturm und Drang’, i.e. of capital expansion (Parvus 1901: 12, 16, 19, 20, 26), followed by contractions. The undulatory movement of capital accumulation corresponds to the irregular development of the world market, to the ‘laws of capitalist oscillation’ (p. 27).

Parvus acknowledged that Marx and Engels had dealt only with the shorter industrial cycle and had not explained the longer periods of accelerated and retarded development, namely the possibility of these Sturm und Drang periods (p. 27). Yet he used and generalized their theory to explain crises by overproduction, the organic consequence of the enlarged reproduction of capital. Social and political factors were also considered: he pointed out, for instance, that the textile unions' fight for a reduction in working hours had been a major contributory factor in the 1896 crisis. His contribution was, however, superficial and mainly descriptive. He intuitively noted the possibility of periods of general expansion that were longer than the business cycle upswing, but presented no theory or general historical vision to account for this. As a consequence, Duijn's claim that long waves should be called ‘Parvus cycles’ (Duijn 1983: 61) is clearly
exaggerated. Like the early authors who noted the change of tide from the dominance of expansion to the dominance of depression in the long fluctuations, Parvus noticed the striking differences in the transition from one phase to another and registered some of the relevant differences, but he provided no explanation for this.

The next authors to deal with this matter were much more concerned with rigorous proof and statistical identification of the long movements. Van Gelderen's 1913 article was the single most important contribution to the research before Kondratiev's work. He acknowledged Parvus's insights, namely the distinction of the *hausse* years, the *Sturm und Drang* of capital considered to occur because of the capitalist mode of production—unlike Sombart, who noticed the periods of expansion and contraction but considered that these were simple coincidences (van Gelderen 1913: 45, 46). He then analysed price movements as symptoms of the division of productive forces between sectors of production and detected a longer movement than the industrial cycle: ‘Apart from the on-average ten-yearly fluctuations in the general price level, the price-curves also show a longer wave movement, which in the course of its up and downward movement comprises several decades’ (p. 14).

As a consequence, an expansion period from 1850 until 1873, a depression from 1873 until 1895 and an expansion after 1896 were detected. The ‘spring tide’ and the ‘ebb periods’ of expansion and contraction were explained by concrete factors such as the changes in transport costs deriving from the construction of railroads, the consequent increase in the demand for metals, and the emigration to America as far as the expansion of 1850–1873 was concerned (van Gelderen 1913: 15, 22). But this analysis was not limited to the factors influencing price movements, since van Gelderen pointed out the impact of major structural changes in industrial production, namely the development of the electricity sector and the increase in gold production (p. 20).

Van Gelderen then undertook a systematic study of four types of causal factor, whose presence was discussed in several time series (pp. 22 ff.):
1. the acceleration of production, from the ‘sudden emergence of a production-branch, which, in a more powerful way than before, satisfies a certain human need (automobile and electricity industries)’ (p. 40); the emergence of ‘electrical engineering’ was particularly stressed;
2. the expansion of transport systems, especially to colonies;
3. the evolution of the trade turnover through the expansion of the capitalist system to new areas, such as the industrialization of the USA, Russia, and the East Asia regions;
4. the interest rate movements, in connection with the changes taking place in the monetary system, especially the increase in gold production.

The necessary condition for the ‘spring tide’ was considered to be the expansion in aggregate demand caused by the increase in production. The faster rate of growth of production and the cost increase provoked by the inflationary pressures in raw materials were then supposed to create the conditions for a crisis and for the subsequent downswing.

Since it not only considered nominal and real variables, but also explained the evolution of the economic system in a concrete historical context, van Gelderen's paper was in fact the first building block for long wave research. The tragic fate of his work (ignored by most of the writers to follow, and translated into English only in 1996) and of the author himself (he committed suicide in 1940 when the Nazi invasion of the Netherlands was imminent: Reijnders 1990: 54n.) cries out for justice to be done in this regard. The recent publication of his essay (in Freeman 1996), as well as the publication of Clarke's, de Wolff's, and Parvus's works (Louçã and Reijnders 1999), is a step in the direction of highlighting the importance of the contribution of these forerunners.

Working at the same time as van Gelderen, yet ignoring his contribution, some other authors investigating the relation between economic movements and political and institutional conditions produced valuable arguments in support of the long wave hypothesis. Alfonso Pietri-Tonelli, claiming to apply the ‘scientific procedures’ of physics (Pietri-Tonelli 1911: 220), described the economic system as a pendulum; its dynamics was consequently studied as a form of energy propagation
accounting for the waves, which were generated by exogenous factors (p. 222). Pietri-Tonelli considered the interplay of economic and political factors in an attempt to explain the major turning points and, like Pareto, used simple statistical methods (a first-degree polynomial to account for the trend). In 1921, he conducted an extensive investigation into the symptoms of the long fluctuations, namely the time series of prices, theatre tickets, marriages, and criminal activity: his dating scheme included an expansion from 1852 until 1873, a contraction from 1873 until 1897 and a new expansion from 1897 until 1913.

Bresciani-Turroni wrote an article in 1913 identifying long waves in prices (with a trough around 1850, a peak around 1870 and a new trough around 1895), which he explained through Cassel's theory of the impact of the volume of gold. But in a later paper, he instead considered the fluctuations of profits—including certain factors such as the costs of production, technical (p. 76) advancement, the discovery and exploitation of new territories—as the central cause for the observed long fluctuations (Bresciani-Turroni 1917: 9).

In 1913 Pareto wrote a paper explaining the long waves in an economy by the social conflict inside the elite, i.e. the ruling class, between entrepreneurs (speculators) and rentiers (traditional capitalists): their alternating domination explained the successive periods of daring expansion and timid contraction. He developed the same theme in his subsequent work (Pareto 1916). At the same time, Aftalion (1913: 1–7), Lenoir (1913: 148–9), and Lescure (1912: 452–90) detected and discussed these long movements. Aftalion followed the Marxian tradition in his 1913 book, explaining the cyclical fluctuations by the long periods required for the reproduction of fixed capital, influencing the formation of expectations and creating ‘wave movements of wide amplitude’ which caused the ‘fairly long cyclical variations’ (Aftalion 1927: 165). Kondratiev developed this approach later on.

In spite of the diversity and importance of these insights, a large part of this debate was lost, since most of these papers were not widely publicized, partly because of the language barrier. As an illustration, when the Institute of Conjuncture organized a debate about Kondratiev's 1926 paper, Spektator referred to Parvus, and Falkner criticized Kondratiev for not acknowledging the works of de Wolff, Bresciani-Turroni, and
Pietri-Tonelli. In his reply, Kondratiev indicated that, after the preparation of the 1926 draft, he had read Bresciani-Turroni (Kondratiev 1992: 244, 250, 289), but not the others. No one yet referred to van Gelderen. Later Kondratiev at least read de Wolff (and became acquainted with van Gelderen’s arguments through de Wolff’s) and Pietri-Tonelli, so that only in 1928 could he consider and classify all these contributions according to the nature of their explanations.

The main exception is the work of van Gelderen, since his paper was partially accessible abroad through the reference made by his friend Sam de Wolff (1924, appearing for the first time in English in Louçã and Reijnders 1999). De Wolff was a Dutch social democrat who published an account of van Gelderen’s theory on long waves in a book that was widely known, since it was the Festschrift for Karl Kautsky. De Wolff adopted the same dating (1825–49, ebb tide; 1850–73, spring tide; 1873–95, ebb tide; 1895 and afterwards, spring tide or Sturm und Drang) and used sophisticated descriptive statistical methods following van Gelderen.

These authors are important forerunners: they indicate a broad consensus on the calendar of the long waves, showing that price oscillations and (at least for some authors) the impact of new industrial branches were so noticeable that they accounted for them by each reaching the same conclusions independently. Such initial consensus established the main agenda for future research, including topics such as the place of social and political factors, particularly for the explanation of the turning points (as the Italians argued for), the historical role of innovation and structural change (van Gelderen), the relationship between price and production series, single (Cassel’s monetary theory) or multi-causal explanations, and the statistical treatment of the series in order to detect and demonstrate regularity and recurrence.
3.4 Kondratiev: An Organic Approach
In spite of the importance of the previous writers, it was Kondratiev who established the foundations of the research, since his works were more complete and general—having been developed independently—than those of van Gelderen. Kondratiev's ideas had a greater impact, since some of them were soon translated and frequently discussed in broader scientific circles. But his theoretical argument could not be studied in detail, as it was not translated and the Russian debate was almost completely ignored. For a long time Garvy’s 1943 paper was the most precise and complete source of reference in this debate—and a still rare reference in English, together with Day’s (1981) and V. Barnett’s (1998) books and a few other contributions—but it is a somewhat biased summary of the arguments. In short, not only was Kondratiev condemned by the Stalinist courts for crimes he did not commit, but his work has been discussed for at least five or six decades on the basis of incomplete and incoherent versions of his original writings. This section provides a short review of that work, briefly outlining the main theses, while some of the analytical contributions will be examined in the following section.

In 1922, Kondratiev published a book formulating in passing the long cycle hypothesis based on his inspection of some statistical series. His conclusion was very tentative and amounted to his claiming that there were long periods of upswing and downswing in historical data. In the paper prepared the following year as an answer to his critics, Kondratiev emphasized that the ‘major cycles of the conjuncture were only considered as probable’ (Kondratiev 1923: 524). This was interpreted by some as implying a simple mechanical recurrence so that, after the First World War and the severe depression of the subsequent postwar years, a longer period of recovery would necessarily occur.

On the basis of his previous work on epistemology and the analytical representation of history, Kondratiev argued that irreversible and reversible processes coexisted, although ‘the evolution of the economy as a whole is an irreversible process’ (1923: 496), comparable to that of an organism. Although declaring not to be a Marxist, Kondratiev insisted that he was precisely following Marx’s understanding of the genetic process of capitalism, in keeping with the analysis of major cycles by Lescure, Aftalion, Trotsky, Panekoek, and
Kautsky. Apparently Kondratiev just wanted to claim to be part of a much larger research into reversible processes (such as those encapsulated in the concepts of the transformation of the commodity, the reproduction of fixed capital and the crises) and irreversible processes (such as those accounting for technological and social change). Furthermore, he argued that the major cycles could be organically explained by the action of internal factors of change further affected by secondary environmental circumstances.

Trotsky reacted in June 1923 and published an article criticizing Kondratiev's hypothesis. This text introduced a rather important debate, since it marked out the boundaries and implications of the controversies, which have frequently been misunderstood, in later interpretations. Trotsky referred to two concepts of equilibrium: (1) the 'secular equilibrium', i.e. the general trend of development encapsulated in the 'curve of capitalist development', and (2) the 'cyclical equilibrium', imposed after the restoration of the system following the elimination of the crises of disproportion (Trotsky 1923: 7 ff.). Equilibrium, in this sense, was an epitome for the general cumulative process of capital transformation and circulation, considered to be inherently unstable although very resistant. In particular, the long-term trend of development could be changed by political events: for Trotsky, long fluctuations were trend variations and not cycles, as they were exogenously generated. The essential difference is that cycles were supposed to be driven by the internal contradictions of the economic system—i.e. determined by the clock of capital reproduction and accumulation—whereas the shifts in the curve of capitalist development were supposed to be brought about by major external events. These major changes were dated according to the general consensus of the time, 1781–1851, 1851–73, 1873–94, 1894–1913, 1913–... (pp. 7 ff.). In order to illustrate his argument, Trotsky used a table published in January 1923 by The Times, describing political, ideological, and economic evolution over more than one hundred years.

This distinction had a political intention, namely to preserve the possibility of ruptures imposed by anti-systemic forces, by conscious social decisions. In this framework, exogeneity once
more emphasized the creative role of strategy and social design. As Day notes, Kondratiev's efforts to 'endogenize' Trotsky's factors of change were contradictory to the very nature of his world vision:

[presenting] a continuous curve generated by a single equation instead of a segmented trend-line, Kondratiev made manifest the ideological assumption implicit in the concept of moving equilibrium: the lack of unevenness in the historical developments of capitalism. By 'internalizing' Trotsky's external conditions, he produced an ultra-deterministic theory of history that few Marxists could contemplate. (Day 1981: 89)

The contradistinction was very sharp, since Kondratiev instead considered an 'irreversible' movement, one that could not be changed by any sort of event and was indeed wholly ignored in the analysis, and political and social factors that were endogenously determined by the very nature of the 'reversible' processes. Furthermore, the relevant features were the reversible oscillations around a moving equilibrium. Trotsky rejected this concept and concluded that the moving equilibrium concept implied some sort of harmonization process.

This criticism quite surprised Kondratiev. Indeed, in his 1923 reply he quite candidly quoted Varga's position and Trotsky's speech at the Third Conference of the Comintern, in which they acknowledged the decisive change in the international conjuncture (Kondratiev 1923: 521–2). In these remarks about the change of the conjuncture Kondratiev saw something else, a more general statement about the possible evolution of a new long-term expansionary wave immediately after the depression years which would not require a new change in political conditions. Furthermore, by 1923 the setting was already completely different and the economic situation had once more deteriorated.

Eventually, because of the political implications of the argument about the nature of equilibrium, Kondratiev preferred not to develop this matter any further. In fact, the 1926 internal debate at the Institute of Conjuncture was more important from the statistical and methodological point of
view, although it merely redefined the earlier questions about broader interpretation issues.

The controversy involved at least four important topics: (1) the legitimacy of the formal analogy, in both methods and theory, between the (Juglar) business cycles and the longer movements, which was implicitly supported by Kondratiev and explicitly criticized by Trotsky; (2) the evaluation of the conjuncture, in order to know if a new long-term revival was emerging in the early 1920s or if the conjuncture was still dominated by a general downturn; (3) the nature of the causes of the ‘reversible’ movements, and thus of equilibrium and of endogenous and exogenous factors; and their links to (4) the nature of the ‘irreversible’ movement.

Refining his argument, Kondratiev maintained later that the ‘essence’ described by static equilibrium was supposed to be the core of the identity and invariance of phenomena, while dynamics was supposed to describe change and difference, under the concept of ‘dynamic equilibrium’ (Kondratiev 1924).

But, according to Kondratiev, change presupposes the ontological identity of the object, and that is why dynamics was considered to include statics. In that sense, he argued that dynamic processes comprise two types of movement: (1) irreversible processes, which have a fixed direction, e.g. the growth of population and the volume of production, the models of enlarged reproduction (p. 17); and (2) reversible processes, which may change direction, e.g. interest rate, prices, employment (p. 12). The long cycle, or the ‘curve of the conjuncture’, belongs naturally to the second type, if one disregards certain irreversible processes. As Kondratiev acknowledged, he was using a metaphor drawn from physics, the concept of sub-stratum, although he recognized that this did not have a convenient analogue in economics (pp. 14–15).

In 1925 and 1926 Kondratiev again elaborated his theoretical approach. The thesis has three main theoretical characteristics. First, Kondratiev argued that crises are ‘organically’ a part of the capitalist mode of production, as Marx and Juglar considered (Kondratiev 1928a: 111). This was an important argument in favour of rejecting simple exogenous causality, but it also had a precise holistic consequence: the organic concept of ‘totality’ implies that there is something more than the simple sum of the components, that there is ‘something new’ in the whole
(Kondratiev 1926b: 63), and Kondratiev was fully aware of this implication. If this is so, no purely atomistic concept is useful or acceptable for the analysis of reality. Consequently, all cycles are part of the same economic process, as he stressed in a debate with Pervushin (V. Barnett 1996: 1021).

Second, Kondratiev considered that this organic, holistic, and non-atomistic epistemology was the necessary counterpart of the reality of social processes, in which the rationality of ‘human interventions’ implies the creation of a greater diversity than in the case studied in natural sciences (Kondratiev 1926b: 83). In other words, unlike the neoclassicists, for whom rationality is typically associated with the pattern of a representative agent, Kondratiev defined economics as a research into the creation of variation.

Third, for Kondratiev such variation was still compatible with equilibrium. The system always tends towards a moving equilibrium: ‘So the long cycles of the conjuncture represent a deviation in the real level of the elements of the capitalist system in relation to this same system's equilibrium . . . a process in which the level of equilibrium itself changes’ (1928a: 159). So impulses were conceived of as disequilibrium processes, caused by ‘radical changes in the conditions of production’ through infrastructural investment in essential capital goods (1928a: 158, 160). Kondratiev did not discuss in any detail this equilibrium around which the reversible processes were supposed to be organized. He just implied that equilibrium represented the most probable state of the system, and did not deal with the changes in the system itself.

Overall, this was a contradictory and incomplete vision. The holistic and organic view that Kondratiev endorsed does in fact preclude the decomposition procedure or absolute distinction between different types of dynamic movement, as if they were atomistic and unrelated phenomena. Equilibrium was assumed, but one of its empirical counterparts, the irreversible process or the trend line in which it was supposed to be located, was absent from the inquiry. Furthermore, the concept did not explain the change of structure from one long cycle to the next; Kondratiev was forced by the logic of his argument to assume a strict separability between irreversible and reversible movements, and to ignore the effect of cycles on the trend and vice versa. (p.81) This implies a major
contradiction, since some of the structural factors that were supposed to influence the longer-term evolution of productive forces were then defined as mere endogenous consequences of the cycle itself.

Many authors centred their criticism of Kondratiev on the imprecision and vagueness of his causal explanation for the long cycle, which was based on two essential factors that could account for the revival: Tugan-Baranowsky's theory of ‘free loanable funds’, and Marx's theory of the echo-cycle of fixed capital reproduction. Our argument is that this debate was indeed relevant, yet it was of secondary importance: the main limitations of Kondratiev's theory were not the rather fascinating explanatory hypotheses he created, but the very concepts of statics and dynamics, of irreversible and reversible movements, and, as a consequence, of equilibrium. These formed the basis for his trend-decomposition procedures, a contradictory and puzzling technique with dubious epistemological foundations.

On the other hand, Kondratiev detected long-term fluctuations that could not be explained by general equilibrium macroeconomics; he described such fluctuations as specific phenomena in distinct epochs in the history of capitalism. This led him to carry out an impressive and detailed inductive research, presented in his 1926 paper, and producing vast amounts of evidence and statistical as well as graphical information.8

In this study, just as van Gelderen had done fifteen years earlier, Kondratiev identified some major transformations in productive forces, such as the new industrial revolution that was driving the transition from the II to the III long wave, based on the chemical, electrical, and motor industries (Kondratiev 1928a: 140). He established the first rigorous dating scheme for the long wave: the upswing of the first long wave from the end of the 1780s or the beginning of the 1790s until 1810–17, and the downswing from 1810–17 until 1844–51; the upswing of the next wave from 1844–51 until 1870–5, and the downswing from then until 1890–5; the upswing of the third wave from 1891–6 until 1914–20, and the downswing from 1914–20 onwards. (See the comparison between Kondratiev's dating scheme and those of his forerunners and contemporaries in Table 3.2.) Furthermore, Kondratiev made a valuable contribution to the research when he decided to
include in his explanatory model different technological, economic, social, and political factors: as in the case of the previous authors, from van Gelderen to Pareto, this interconnection became an important part of research.

(p.82)
### Table 3.2. Dating of Long Waves

<table>
<thead>
<tr>
<th>Author/period</th>
<th>First LW</th>
<th></th>
<th>Second LW</th>
<th></th>
<th>Third LW</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Upswing</td>
<td>Downswing</td>
<td>Upswing</td>
<td>Downswing</td>
<td>Upswing</td>
<td>Downswing</td>
</tr>
<tr>
<td>Pietri-Tonelli</td>
<td>1852–73</td>
<td>1873–97</td>
<td>1897–1913</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bresciani-Turroni</td>
<td>1852–73</td>
<td>1873–97</td>
<td>1897–1913</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Van Gelderen</td>
<td>1850–70</td>
<td>1870–95</td>
<td>1895–. . .</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>De Wolff</td>
<td>1825–49</td>
<td>1850–73</td>
<td>1873–95</td>
<td>1895–. . .</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Trotsky</td>
<td>1781–1851</td>
<td>1851–73</td>
<td>1873–94</td>
<td>1894–1913</td>
<td>1913–. . .</td>
<td></td>
</tr>
</tbody>
</table>

*Note:* As the table shows, Kondratiev prudently indicated his periodization with large intervals for the starting and ending periods.
The crucial discussion of these ideas was centred upon the paper presented by Kondratiev on 6 February 1926 at a seminar of the Institute of Economics of the Association of Social Science Research Institutes. One week later, assisted by a large staff, Oparin presented his own counter-report, and in 1928 a pamphlet was published including both Kondratiev's and Oparin's contributions and the minutes of the seminars. Kondratiev's confrontation with Oparin was mainly about statistical methodology, since Oparin supported an alternative theory—Cassel's monetary theory—but was not very emphatic about it, and Kondratiev very easily showed that such a theory of equilibrium produced the same type of statistical problems, if not worse. Many of Oparin's points were, however, fully justified, such as the lack of theoretical justification and the arbitrariness of the choice of the detrending functions, producing some sort of 'perspectivist distortion'. This topic will be discussed in more detail in the next section.

Eventov and Bogdanov, who unlike Oparin did not have to formulate alternatives, presented interesting arguments against detrending, namely that the trend (the growth of the economy) and the cycles (the acceleration and deceleration of growth) are quite simply the same phenomenon (Kondratiev 1992: 246 ff.; Garvy 1943: 210), therefore implying that decomposition was not justified. Sukhanov endorsed Kondratiev's argument about the organic nature of social systems in order to argue, on the basis of a life-cycle concept, that no further explanation of the long-term changes was necessary: 'The physiology of an organism in evolution is different in the successive stages of its evolution. Capitalist evolution is an organic process with definite different stages: youth, maturity, decline . . . and even death' (quoted in Garvy 1943: 214).

As the next section will indicate, this debate was important, although not conclusive: it detected some of the most important mistakes in Kondratiev's statistical techniques, but could not solve them.
3.5 The Decomposition Problem
Kondratiev dealt at length with the decomposition problem, as a consequence of his early works on statics and dynamics. In 1923, answering to the critiques of his first and very rough sketch of a long wave theory, Kondratiev stated that the economy is an irreversible and dynamic process, comparable to an organism with cyclical functions (blood circulation, nutrition) and irreversible processes (Kondratiev 1923: 496). This vision was compared to that of Marx, who was supposed to have indicated irreversible processes (accumulation of capital, concentration) and their reversible counterparts (crises, metamorphosis of the commodity). The conclusion was that two different domains were open for study: those of the irreversible phenomena (theory of the stages of development) and those of reversible processes (theory of the cycles: 1923: 496–8). Kondratiev recognized that the distinction is not always self-evident: ‘Of course, in practice, in the inquiry it is very difficult to establish perfect limits between both objects’ (p. 497).

But, unlike Schumpeter—and J. B. Clark, who inspired Schumpeter in this particular theme—Kondratiev did not consider that statics described real phenomena; in fact he explicitly criticized Schumpeter on this matter in his 1924 paper (Kondratiev 1924: 7–8, 10–11), stating that only dynamic movements were real: ‘One must not forget that in reality we have one only dynamic process in economic life, and that it is just for the sake of scientific analysis that we may decompose it in irreversible tendencies [the trend] and reversible oscillatory movements’ (1924: 29; also 1923: 496; 1925: 575–6).

The argument was that economic dynamics represented the totality of the social process and therefore the very nature of historical evolution—and that an understanding of its internal mechanism was decisive for the development of a convenient and pertinent explanation. As a consequence, Kondratiev invoked the dogma of endogeneity as the locus of the epistemic legitimacy for a scientific explanation:
These episodic and external causes are also included in the overall process of the socio-economic dynamics and for that reason cannot be considered as external factors causing the cycles. From our point of view, the explanation of the long cycles and in particular of the price movements must be sought in the character of the mechanism and in the internal laws of the general process of socio-economic development. (Kondratiev 1928b: 425)

A pertinent causal claim was consequently described as the set of necessary conditions for an event in a simple and mechanistic framework. Everything was then ready for the use and abuse of the early standard mathematical procedures to decompose the series, to interpret its elements, and to attribute the value of proof to the conclusions emerging from a surgically precise analysis of a split world.

Kondratiev accepted and argued for such a decomposition, although recognizing that the reversible processes always include some irreversible elements, so that decomposition is somewhat artificial (1924: 13), given that no real independence exists between both types of movement: the distinction was merely made ‘for the purpose of the scientific analysis’ (p. 14). From this point of view, the decomposition was a subsidiary problem to that of the distinction between statics and dynamics (to be represented, for instance, by the tendency of the profit rate to fall or by the increase in the organic composition of capital), and statics was a specific dimension of dynamics: ‘if one abstracts from the dynamic processes, we may discover the statical regularities. . . . In this sense, statics is a moment of dynamics and the formula of statical regularity is that of dynamical regularity to which the time element is subtracted or reduced to zero’ (p. 69).

In the 1926 paper Kondratiev again took up the same issues. He began saying that there is no independence of the reversible processes in relation to the irreversible ones, and that the whole is the real process: ‘Indeed, I think (p.85) that the real process of dynamics is unique’ (1926b: 102–3). At the same time, he argued that the decomposition was acceptable just for purposes of study:
Economic evolution as a whole must without doubt be considered as a single and indivisible process, since only by taking it into account is one able to understand completely the characteristics of economic dynamics. Though economic evolution as a whole must doubtless be considered as a single and indivisible process, the acceptance of the classification of economic processes and elements here suggested is absolutely necessary for the purpose of scientific analysis of economic realities. (Kondratiev 1925: 583)

In order to avoid an arbitrary fitting procedure, namely in the choice of the function, Kondratiev argued that he favoured ‘collective work’; in fact, he trusted the least squares method to give him precise guidance (1928a: 115). But he recognized that some arbitrariness was unavoidable: ‘I do not affirm at any moment that the theoretical curves I found represent the real curve of evolution. On the contrary, I do affirm that we do not have for the moment the methods to exactly determine those curves’ (p. 163).

However, Kondratiev trusted his method to detect both the underlying irreversible movement and the long cycles of the reversible conjuncture:9

I suppose that to this curve, theoretical by construction, may correspond the real tendencies of the evolution. But we cannot affirm that the theoretical curves we found correspond exactly to those tendencies . . . We may only say that the theoretical curve exactly represents the tendency of the given empirical curve . . . It is evident that the task is then to determine if there exist long cycles in the deviations of the series in relation to the theoretical series. (Kondratiev 1928a: 116)

According to Garvy's interpretation of Kondratiev's argument, decomposition was always artificial: the distinction between reversible and irreversible movements was technically necessary in order to legitimize the decomposition and to look for long waves in the detrended series, but since the trend indicated some average rate of growth of the system and the long cycles indicate the acceleration or retardation of that growth, both were supposedly created by the same set of
factors and indistinguishable in reality. As a consequence, decomposition was ‘purely artificial’ (Garvy 1943: 210).

**p.86** Of course, it is impossible to accept simultaneously both arguments: either the real process is singular and non-decomposable, and then any procedure of decomposition may create spurious results—and Kondratiev did not indicate any protection against this danger—or the real process is decomposable, and there are real distinct entities which can be meaningfully addressed by separate theoretical explanations and by some precise functions. Although Kondratiev paid lip service to the first interpretation, it is obvious that the second one dominated his work, which would otherwise be unjustifiable and irrelevant. Several distinct functions were used by him in order to extract the trend, and the choice of functions did not obey any uniform criterion, being dictated by mathematical convenience; in no case was an interpretation indicated for those functions or for the values obtained for the parameters. In spite of his concern, Kondratiev was one of the authors who used the decomposition method most intensively: his debate with Trotsky and Bogdanov was precisely about these relations between trends and cycles as unitary processes or as separable sets of factors. In spite of the sophistication of the current techniques, the decomposition problem is still with us, and it will be further discussed in the next chapter.
3.6 The Contemporary Impact of Kondratiev's Writings

Given the subsequent history, the impact of Kondratiev's few articles published in English and German was not only very effective but also quite surprising. A large part of the economists involved at the core of the project for developing the new approach of econometrics (including Frisch, Tinbergen, and Schumpeter), and simultaneously some of the more distinguished economists involved in quantitative and historical research (Mitchell, Kuznets), took notice of Kondratiev's work and fully endorsed it or referred to it with varying degrees of enthusiasm.

Frisch visited the United States in the spring of 1927, and in April prepared a long manuscript, 'The Analysis of Statistical Time Series', which was widely circulated among American economists with the precious help of Mitchell, although it was never published. From the first pages, Frisch subscribed to Kondratiev's hypothesis of 30 to 50 years of 'long time movements around which the business cycle is fluctuating', forming a 'major cycle' (Frisch 1927: 4). The source of the reference was the 1926 German translation, but Frisch had also borrowed a manuscript by Kuznets (the book to be published in 1930), which included not only an account of the Russian debate but also statistical information giving credit to Kondratiev's theory. It is quite obvious that Schumpeter—later on a close friend of Frisch, and who also shared this idea—developed his approach autonomously from Frisch: the correspondence between them first began in August 1927, after the dissemination of the time series paper. It did not mention Kondratiev, whose hypothesis had already been publicly accepted (p.87)

by Schumpeter. Indeed, their adherence to the long wave hypothesis was simultaneous but independent. Schumpeter became the main Western defender of the theory of long cycles, and dedicated a large part of Business Cycles (1939) to it, although it is also obvious that he had read the 1926 German translation and had accepted its main idea since then.
As this is quite well known, Schumpeter's arguments in favour of Kondratiev will not be discussed in this chapter. But it is worth emphasizing Frisch's engagement in the same camp; although he did not discuss the hypothesis in detail in his scientific and mathematical texts, he did try to show that some of his models of cycles could generate long waves for certain ranges of parameters, and considered this to be an indication of the likelihood of the models. Moreover, Frisch insisted again and again on his interpretation of the depression of the 1930s and the dangers of war by means of the long wave argument, and in 1932 he gave a series of radio lectures in which the question was discussed. Later, in the pamphlet including these lectures and dedicated to the discussion of the conjuncture, Frisch illustrated his argument with a long series of wheat prices for 1201–1800 from a nineteenth-century book by D'Avenel (Figure 3.1).

Frisch looked at the years 1300–1800 in particular; he used a ten-year moving average, much as Kondratiev did, and detected large persistent movements, which he interpreted as indicating long cycles of prices for the whole history as described by the graph. Since this explanation was very effective for understanding the great ravages of the 1930s, at least as far as Frisch was concerned, he maintained it throughout his life.

(p.88) Tinbergen very soon, and also independently, defended the same hypothesis for quite similar reasons, since he had read de Wolff's book and reviewed it in 1929, noticing that a parallel line of investigation was being carried out in Russia: 'Research on long waves is still in an initial stage, and it is mainly in Moscow that valuable work has been done on this subject' (Tinbergen 1929: 29). Like Frisch, Tinbergen maintained the same interest all his life and continued to participate in conferences on this topic well into the 1980s. In 1987 he wrote a rather favourable preface to Kleinknecht's book on the issue (Kleinknecht 1987b).

In Wesley Mitchell's important book, first published in 1927, he acknowledged the work by van Gelderen, de Wolff, and Kondratiev (once again based on Kondratiev's 1926 German
As far as immediate reactions are concerned, Kuznets was the other important young researcher interested in Kondratiev's works at the time. As he was able to read Russian, Kuznets was the first to study Kondratiev's work in depth, namely his 1922 book, his 1925 paper (plus the German translation), and the Kondratiev–Oparin controversy (Kuznets 1930: 259 ff.). Furthermore, Kuznets was familiar with Trotsky, van Gelderen, and de Wolff's arguments, and included a synthesis of their contributions in his own book. His important 1940 survey of Schumpeter's *Business Cycles* indicated a much more critical attitude towards the long wave hypothesis, and it is well known that he developed an alternative account of long-term historical evolution. (For a more recent reappraisal, Solomou 1987.)

During the late 1930s interest in Kondratiev's work apparently began to fade, and no new contributions were added to the research, with the major exception of Schumpeter's *Business Cycles* (1939). At the same time, other researchers into business cycles, such as Haberler, distanced themselves from any claim about long waves. In spite of this, and basing himself on Spiethoff and Schumpeter, Haberler accepted that each long cycle had a historical physiognomy of its own and that a general theory was admissible, although he doubted if anyone could show the existence of regular factors generating the fluctuations (Haberler 1937: 308).

Another distinguished researcher, Alvin Hansen, acknowledged and quoted from the 1935 US translation of the 1926 German translation, and compared Kondratiev's arguments to those of Spiethoff, Schumpeter, Mitchell, and von Wantrup. He found that the regularity of the three long waves was comparable to that of the shorter business cycles: ‘as high a degree of periodicity has prevailed for these three waves as any which we find for the major business cycles’ (Hansen 1941: 29). It should be added that (p.89)
later Hansen took a much more ‘agnostic and even very sceptical position’ on the same issue (Hansen 1951: 56).

It is obvious that by that time—the end of the 1930s and the beginning of the 1940s—Schumpeter had become the main proponent of the thesis, or at least the person most involved in its defence, since both Frisch and Tinbergen were isolated in Europe and surrounded by war, and in any case had decided not to devote their professional attention to this issue. In 1942 Frickey published a book that included an important argument against trend decomposition and suggested a not entirely specified alternative genetic method. Frickey argued that the secular trend should be assessed as ‘a problem in historical description’, not as ‘a problem in mathematical curve fitting’, and he demonstrated that the fit of different functions could imply arbitrarily created cycles and therefore spurious conclusions (Frickey 1942: 8). His conclusions from US data were presented as compatible with Kondratiev's hypothesis (pp. 231 n., 232, 340).

The long paper by George Garvy (1943) presented Kondratiev's main theoretical arguments, methods, and statistical evidence, and compared these with those of his opponents, concluding with Garvy's own view. Garvy's main criticism concerned the lack of explanation, in Kondratiev's assessment, for the lower turning point, and therefore the lack of any theoretical basis for the claim that there is a ‘rhythmical movement of long duration of the economic system as a whole’ (Garvy 1943: 208). Furthermore, he argued that there was no explanation for the trend—insisting that Kondratiev recognized his inability to show that the trend corresponded to the real economic evolution—and that the acceptance of Cournot’s distinction between supposedly independent entities as trends and cycles forced Kondratiev to look upon ‘the economic processes as a sum of the actions of independent forces’ (Garvy 1943: 210). Discarding the long waves hypothesis, Garvy nevertheless argued that the enigma was relevant, since successive stages with differential growth rates could be detected in economic history, and actual dynamics should account for them (pp. 219–20).

In the 1940s, another researcher taught the Kondratiev thesis at the London School of Economics: W. W. Rostow (1948: 9, 29, 45) based his lectures on Schumpeter and in particular on the 1935 translation of Kondratiev's paper. Others, such as the
very young Richard Goodwin, learnt the thesis from Schumpeter and later spread it to others. At the same time, Fellner took a somewhat more sceptical position. In 1949 he had prepared a manuscript, which was discussed with Schumpeter, entitled ‘On the Waves (p.90) of Different Lengths with Particular Reference to the Long Waves’ (letter from Fellner to Schumpeter, 26 March 1949, Harvard University). Fellner was probably under the spell of Schumpeter's argument, but his own contribution exhibits some doubts about the nature of the interrelation between the ‘process’ and the ‘external factors’, suggesting for instance that in the future the innovations accounting for a next Kondratiev wave could be exclusively or predominantly generated in the military sector, therefore being ‘external’.

Later, Fellner took up the issue again, presenting Kondratiev's statistical methods (Fellner 1956: 38) and inspecting a certain number of empirical series (pp. 40–1); his conclusions indicated the acceptance of long rhythms, but as irregular features of development. As a consequence, ‘we prefer not to assert the existence of long cycles of fifty years’ (p.42), since ‘the so-called long cycles in general economic activity are merely alternations between intermediate trends of greater and of lesser steepness’ (p. 49).

This impressive list of scientists, including some of the major figures from several decisive research traditions in the first third of the century—neoclassical economics, econometrics, quantitative economics, heterodox approaches, evolutionary economics—clearly proves that Kondratiev was not alone in recognizing major structural changes and patterns of evolution in the history of industrial capitalism. The approximate dating of these processes was generally agreed upon by these researchers, although they disagreed as to the explanation and the epistemological and analytical solution to the difficulties felt by Kondratiev. And this is probably why there was such an impressive early consensus about the Kondratiev hypothesis, as will be further discussed in Chapter 4. There had been major economic and social changes, and Kondratiev provided a framework with which to date, interpret, and discuss these changes.

Nevertheless, there was no agreement on the causes or even on the nature of these periods of change. Indeed, these same
difficulties are still felt by our contemporaries, since the
puzzle these scientists tried to solve is still on the agenda, and
important methodological insights can be gained from these
earlier controversies.

3.7 Conclusions: Evolving Economics

Kondratiev's research was one of the first major quantified
inquiries into economic history. It established a general
consensus on the approximate dating of the initial long-term
economic movements, and for a certain time it became a
paradigm of the explanation of changes in capitalist
development. It was one of the first applied statistical
researches in economics, and it endured as a reference point
for future research, although the controversy surrounding this
effort illuminated some of its shortcomings and incoherences.
Two conclusions are therefore in order.

(p.91) The first concerns the importance, depth, and scope of
Kondratiev's endeavours. His work—in spite of its naivety and
simplicity—should be read by economic historians,
macroeconomists, and statisticians, since it clearly presents
part of the conundrum of the application of mechanical
statistical methods to real, concrete, and live history. Indeed,
Kondratiev's paper on forecasting (1926b) is one of the
masterpieces in the early literature on statistics and history. It
is a powerful survey of the contemporary authors in
economics, mathematics, physics, and philosophy, and deals
in detail with the problem of the relation between reversible
and irreversible processes.

Kondratiev adopted a cautious stance on recurrence and
causality: there is no more than a slight chance of repetition of
exactly the same causal environment, so ceteris paribus
conditions are not met in economic history—each event is
unique. But, according to Kondratiev, there is also a stable
causal structure, which accounts for a certain regularity of
phenomena. Of course, this implied that the explanation of the
complex whole is a priority for any inquiry in social sciences:
‘We must emphasize in particular that each given whole is not
the simple summation of its components and cannot be
understood from the peculiarities of these elements as such.
Each totality represents something new, something peculiar,
which cannot be reduced to the elementary phenomena unless
by default’ (1926b: 63).
Although Kondratiev dismissed the possibility of a precise forecast, since the initial conditions are not known, and the causal structure and its regularity are only approximately understood, induction was presented as the sole method capable of increasing the level of understanding of historical data. ‘Historico-comparative’ and ‘statistical’ methods were therefore the two available forms of induction, and both were to be used in that quest (1926b: 74). Moreover, they should be combined, since no definitive conclusion is possible from statistics itself:

The statistical method is no other than the method of knowledge acquisition [induction], which meets a series of difficulties whenever it is applied, that prevent it from strictly and exactly revealing the real relations and regularities. The difficulties do not just arise from the complexity of reality, but also from the quality of materials, the impossibility of disposing of the quantity of necessary elementary events and, finally, of our subjective errors. (Kondratiev 1926b: 77)

The combination of methods is therefore one of the central inheritances of Kondratiev’s research, even if he clearly preferred the certainties that derived from quite arbitrary statistical demonstrations. Our argument is that this feature was part of the reason for the success of his writings, since at the time it was generally admitted not only that capitalism was characterized by different patterns of growth, but also that history and statistics were each blind without the other.

Kondratiev assessed economic history as part of societal evolution, used the available analytical and statistical tools, and discussed their epistemological foundations. The original consensus obtained among his contemporaries demonstrated that long periods with distinctive characteristics were an imposing feature of industrial capitalism for so many of them, and the disagreements about his own explanation highlighted some of the limits of the methods and theories being used at that time. This was indeed Kondratiev’s decisive contribution, and what makes him worthy of our attention: he convincingly argued that history is part of economics, and economic methods are necessarily analytical and historical.

Notes:
Part of this chapter is based on previous work of one of us published in *History of Political Economy* (Louçã 1999). We thank the editors of that journal for the due authorization to use that material.

(1) Slutsky, who had also been involved with the Institute of Conjuncture, did not become a member of the Econometric Society (ES). He was a friend and a regular correspondent with Frisch, the driving force behind the new association, and his 1927 paper (later published in *Econometrica* in 1937, under the auspices of Frisch) was widely circulated and attracted much attention. When invited to participate in the new association, Slutsky declined, arguing that he was a statistician more than an economist, although one can speculate that his fear of the political consequences of being associated with a foreign institution was decisive. At any rate, Slutsky survived the Stalinist purges. On the other hand, the inclusion of Kondratiev is also an enigma, since he was at that time in gaol. Either he accepted via his wife, or the founders of the Society took his participation for granted from previous contacts with him.

(2) The difficulty or impossibility of corresponding with Kondratiev nevertheless implied that his name was sometimes referred to (September 1934 list of the Fellows) and sometimes omitted (October 1933 list), while at times there was a reference to the fact that he was a member ‘if living’ (lists included in the Schumpeter Archive, Harvard University).

(3) Kondratiev's first works discuss the methodology of historical research (1915). In 1918 he published a critique of Bolshevik economic policy; several papers on agricultural economics and planning were also published in the 1920s.

(4) ‘Parvus’ was indeed the alias of Alexandre Helphand, an active member of the Social Democratic International in the beginning of the century. As he always signed his papers with his pseudonym, we follow the same procedure and take ‘Parvus’ as the reference of the author of the texts.

(5) Mandel argued that neither Kondratiev nor Schumpeter nor Dupriez matched the depth and scope of van Gelderen's theory of long-term fluctuations (Mandel 1975: 52). This is an overstatement, since Kondratiev developed a larger body of
empirical work and more sophisticated theoretical explanations, although one might comment that he did not use the most suitable methods or provide general explanations.

(6) The French translation of Kondratiev's works, edited by Fontvieille, is followed here.

(7) In his report to the June-July 1921 Third Conference of the Comintern, Trotsky wrote: ‘Capitalism thus possesses a dynamic equilibrium, one that is always in the process of either disruption or restoration. But at the same time this equilibrium has a great power of resistance’ (Trotsky 1921: 226).

(8) Kondratiev identified four empirical laws. (1) Some years before the beginning of a new long cycle, important changes occur in technological innovation, monetary circulation, the role played by new countries (1928a: 138); these changes could occur as much as 20 years before (p.141). (2) The class struggle, including wars and revolutions, is more intense in the upswings. (3) Agricultural depressions are more intense in the downswings. (4) The downswings of the shorter cycles are more intense in the downswings of the long cycle, and the reverse is also true (pp. 140 ff.). Van Gelderen had already formulated this last ‘empirical law’ (van Gelderen 1913: 49—see Table 8.13 below).

(9) Nevertheless, Kondratiev was fully aware of the arbitrariness of detrending. He said so in 1925: “The term “evolutionary,” or non-reversible process applies to those changes which, in the absence of extraneous (non-economic) disturbing causes, develop a certain definite direction and therefore are not subject to repetition or reversion. As an example, one may point to the permanent tendency of population to grow, of the total volume of production to increase, and the like. It is clear that this conception of non-reversible processes is similar to that of a secular trend. Yet I am of the opinion that the current conception of secular trend is only technical and statistical; it is not economic. A certain secular trend, represented, for instance, by a straight line, will fit a certain period of production. But if we consider the same period only as part of a greater one, another secular trend may appear, represented, say, in the form of a parabola. This indicates that the conception of secular trend does not always give to economists an exact idea of the character of the
economic dynamic process. And for that reason it is not used in the present article’ (Kondratiev, 1925: 579–80). Nevertheless, he widely used it in the articles that followed.

(10) There are some severe shortcomings in this story, since D'Avenel’s series is merely an average of eclectic local observations and the meaning and coherence of the series itself is at best doubtful. But this did not prevent Frisch's acceptance and profound belief in this interpretation.

(11) Frederick Mills was one of the economists Schumpeter took pains to convince: in a letter dated 12 April 1940, Mills kindly thanked Schumpeter for an evening spent discussing the hypothesis, ‘certainly an intriguing one and a useful one’ (Box HUG(FP) 4.21, Schumpeter Archive, Harvard University); but he did not seem to be convinced.

(12) This is quite comparable with Maddison's position on the same subject (Maddison 1991: 95 f., 105 ff.).

(13) The text included references to, and quotations from, not only Clark, Bowley, Babson, Jevons, Tugan-Baranowsky, Beveridge, Denis, Schmoller, Cournot, List, Marshall, Mill, Kautsky, Engels, Marx, and Pareto, but also Person, Strouvé, Durkheim, Mach, Poincaré, Meyerson, Comte, Simmel, Laplace, Boltzmann, Planck, and the Portuguese Teófilo Braga.

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