Nikolai Kondratiev and the Early Consensus and Dissensions about History and Statistics

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Nikolai Kondratiev (1892–1938) was one of the most, if not the most influential of the talented young Russian economists working in the first third of this century, and certainly the best known internationally at that time. His decisive contribution was the presentation of the hypothesis of the long waves in capitalist development—named by Joseph Schumpeter and known thereafter as "Kondratiev waves"—that for some time was 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 "Kondratiev problem," since the existence of long periods with impressively distinctive patterns of development was widely recognized, and (b) the relevance of the newly devel-

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oped statistical methods for checking and proving the existence of the long waves. Such consensus did not, however, extend to the explanation of these long phases or cycles.

The current article is an inquiry into the disputes, the syntheses available during the first half of the century, and the problems emerging from this challenging research, which are still crucial for any inquiry into the dynamics of capitalism. The first section indicates the direct inheritance of Kondratiev's research. The second section mentions the predecessors, the key features of Kondratiev's analysis, and the elements of the consensus and dissension it generated among contemporary authors.

1. Kondratiev, 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 Mikhail I. 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 age twenty-five, he was appointed Minister of Supply of the last Kerensky government, which 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 Conjuncture Institute in Moscow. As its first director, he managed to develop the institute, from just a couple of scientists at its beginning, into a large and respected center with fifty-one researchers in 1923.

In 1923, Kondratiev intervened in the "scissors crisis" debate (the growing divergence between prices of agricultural and industrial products), following the general opinion of his colleagues. In 1923–1925, he worked on a five-year plan for the development of Soviet agriculture. In 1924, after publishing a book presenting the first tentative version of his theory of the major cycles (Kondratiev 1922; see appendix 1), Kondratiev traveled 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 favored 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 economic policy lasted until 1925, declined in 1926, and was over by 1927 (Barnett 1995, 431). By that time, the NEP had been canceled after a political shift in the leadership of the Communist Party. Kondratiev was removed from the directorship of the institute in 1928 and arrested in July 1930, accused of being a member of an illegal and probably nonexistent "Peasants' Labor Party." As early as August 1930, Joseph Stalin wrote a letter to Vyacheslav Molotov asking for the execution of Kondratiev (437).

Condemned to eight years in prison, Kondratiev served his sentence, beginning in February 1932, at Souzdal, near Moscow. Although his health deteriorated and the conditions were bad, Kondratiev still managed to continue his research and had even decided to prepare five new books, as he mentioned in a letter to his wife. Some of these texts were indeed completed and were published in Russian in the early 1990s, with an English edition forthcoming (Kondratiev 1998).

He sent his last letter to his daughter, Elena Kondratieva, on 31 August 1938. Shortly afterward, on 17 September, he was subjected to a second trial, condemned, and executed by a firing squad. Kondratiev was forty-six years old at the time of his murder and was only rehabilitated almost fifty years later, 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 due to be drawn up, his name was immediately proposed: Ragnar Frisch wrote to Schumpeter on 7 October 1932 suggesting two Russians, Kondratiev and Eugen Slutsky.[1] Subsequently, Kondratiev—who was already in prison—became

1. Slutsky, who had also been involved with the Conjuncture Institute, did not become a member of the Econometric Society (ES), for reasons unknown. He was a friend of and regular correspondent with Frisch, the driving force behind the new association, and his 1927 paper (later published in *Econometrica*, 1937, under the auspices of Frisch) was widely circulated and attracted much attention. But there is no indication in their correspondence of the reason for Slutsky's failure to participate in the ES, although one can speculate that his fear of the political consequences of being associated with a foreign institution eventually decided the issue. Anyway, Slutsky survived the Stalinist purges. On the other hand, Kondratiev's

the sole Russian among the twenty-nine founding Fellows of the Econometric Society elected in August 1933,[2] along with Frisch, Wesley Mitchell, Schumpeter, John Maynard Keynes, François Divisia, Arthur Bowley, Luigi Amoroso, Irving Fisher, Henry Moore, Henry Schultz, Corrado Gini, Gottfried Haberler, Harold Hotelling, and others.

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.[3] Moreover, some of the most influential economists, statisticians, and 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, Jan Tinbergen, Arthur Spiethoff, Simon Kuznets, Mitchell, Schumpeter, Oskar Lange, Alvin Hansen, and many others.

Yet what they knew was not the whole text and in some cases was even a misrepresentation of Kondratiev's ideas. With the exceptions of Kuznets and George Garvy, who could read Russian and who knew the original contributions, the others read just the German or American translations of parts of the 1925 paper and missed out on both the 1926 paper and the debate that took place in the same year, not to mention other texts by Kondratiev on central methodological issues. Furthermore, they read inexact translations: as J. Escudier (1992, 244) has shown, the term *long cycles* was translated into German as *long waves*, whereas Kondratiev preferred to use *waves* for the analysis of variables, and to use *cycles* for his interpretation of global movement. Moreover, these terminological and conceptual mistakes were later reproduced in the derived translations, such as the American one. This

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inclusion is also an enigma, since he was at that time in jail. Either he accepted via his wife, or the founders of the society took his participation for granted from previous contacts with Kondratiev.

2. The difficulty or impossibility of corresponding with Kondratiev nevertheless implied that his name was sometimes referred to (e.g., in the September 1934 list of the Fellows) and sometimes omitted (e.g., from the October 1933 list), while sometimes there was a reference to the fact that he was a member "if living" (Schumpeter Archive).

3. Appendix 1 shows 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 discussed the methodology of historical research in 1915. In 1918 he published a critique of Bolshevik economic policy; several papers on agricultural economics and planning were published in the twenties.

version of Kondratiev's 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 secondhand German translation, did not include the theoretical part, and was limited to the presentation of the statistical method and empirical laws (reproduced in Kondratiev [1925] 1984; see 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, and those written by Kondratiev while in jail were only made available in Russian in the 1990s.

As a consequence, Kondratiev waves have long since been discussed by authors who did not know the most important of Kondratiev's texts: J. van Duijn (1983) only refers to the papers published in German in 1926 and 1927 and to the American translation of 1935; Alfred Kleinknecht (1987) to the same German versions of the papers; and Andrew Tylecote (1992) to the 1979 translation. In general, until the 1990s non-Russian-speaking authors knew only these versions. In fact, a complete English edition of Kondratiev's 1925 "Major Economic Cycles" was only published in 1979, and his 1925 "Long Wave Debate" was published only in 1984. Not until 1992 was a collection of the main papers of the 1926 debate at the Conjuncture Institute published in French; the English translation only appeared in 1998.

In spite of this, what may be considered surprising is the resilience of the research program 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 it was also reactivated later on, namely, just before and just after the thirty "golden years" of postwar expansion in the industrial economies. And, more recently, in the entirely new framework of complexity theory, some authors 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.

2. Predecessors, Contemporaries, and Waves of Debates

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 in those cases where he completely ignored their writings. The second part presents the "Kondratiev hypothesis," in keeping with its original formulation and the ensuing debate. The third part indicates some of the immediate reactions to his work and outlines the parallel interpretations that the same enigma suggested in the 1930s and 1940s.

A. The Predecessors: The Early Consensus about the Organic Nature of Major Fluctuations in Economic Activity

Three authors belong to the first wave of predecessors: Hyde Clarke, W. S. Jevons, and Karl Marx. Clarke was completely ignored by Kondratiev. In 1847 Clarke published a paper in the *British Railway Register* and a short pamphlet (Louçã and Reijnders 1998; see also Black 1992), but he owed his fame mostly to the fact that Jevons (1884, 129) pointed to him as the creator of the hypothesis of a long cycle in economic activity, and not to his own original contribution, which still remains largely ignored. Indeed, Clarke argued that the 1847 crisis of scarcity 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 George MacKenzie (Klein 1997, 113–15). Jevons accepted these ideas.

Marx and Friedrich 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 ([1858] 1988, 222) 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 he also considered other longer periods. He quoted Scrope at some length, who, after describing the five- to ten-year period for the construction of productive tools and fixed capital, wrote: "The capital spent on buildings, for example, factories, shops, . . . seems not to cir-

culate. 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 twenty- or fifty-year rotation" (Scrope, quoted in Marx [1885] 1977, 2:163; my 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 (1990, 420 n), 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. This is not accurate. In fact, Engels just discussed the 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 the 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] 1977, 3:489 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.

Yet, in an appendix to the 1886 American edition of his early book on the British working class, Engels went so far as to describe different historical periods—1825–1842, 1842–1868, and so forth—which fit 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 the 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 prod-

> ucts become 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 revolutionise the structure of capital throughout the entire periphery of major areas of production. (Marx, quoted in Menshikov 1987, 69)

Nevertheless, this passage is mainly a descriptive account, and it is well known that this long rhythm was not even considered when Marx formulated his law of the tendency of the rate of profit to fall, nor the countertendencies 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 for the realization of profit. And although some of the countertendencies explicitly dealt with technical change (changes 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 periods—indeed, it would have been difficult for him to do so, since he was writing in the first years of the second wave, the first one to have a really international character. From this point of view, Marx cannot be considered a direct predecessor of the long wave research, even if his 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: they all witnessed frequent 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 (1899, 429) detected a period of forty-five years in the maturation of new methods of production; and Parvus ([1901] 1972), Tugan-Baranowsky (1901, 52–53), Knut Wicksell, and Vilfredo Pareto used the same calendar for describing the long periods. Thus, the first essential element of the early consensus was the recog-

nition 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 and periods marked by depression was indisputable, although their explanation was not. Several authors followed Marx's insights and endeavored 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 ([1901] 1972, 12, 16, 19, 20, 26) argued that there are periods when capitalist production "jumps": long periods of "*Sturm und Drang*," that is, of capital expansion, followed by contractions. The undulatory movement of capital accumulation corresponds to the irregular development of the world market, or to the "laws of capitalist oscillation" (27).

Parvus acknowledged that Marx and Engels had only dealt 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 (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, intuitively noting the possibility of periods of general expansion that were longer than the business cycle upswing, but no theory or general historical vision was presented to account for this. As a consequence, Duijn's (1983, 61) claim that long waves should be called "Parvus cycles" is clearly exaggerated. As previously said about the early authors who noted the change of tide from the dominance of expansion to the dominance of depression in the long fluctuations, Parvus also 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 the statistical identification of the long movements. J. Van Gelderen's 1913 article was the single most important

contribution to the research before Kondratiev's work.[4] 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 Werner Sombart who noticed the periods of expansion and contraction but considered that these were simple coincidences (Van Gelderen [1913] 1997, 45–46). Van Gelderen then analyzed 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" (14).

Consequently, periods of expansion from 1850 until 1873, of depression from 1873 until 1895, and of expansion after 1896 were detected. The "springtide" 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 and the consequent increase in the demand for metals, and, as far as the expansion of 1850–1873 was concerned, the emigration to America (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 (20).

Van Gelderen then undertook a systematic study of four types of causal factors, whose presence was discussed in several time series (22–37):

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)" (40); the emergence of "electrical engineering" was particularly stressed;
2. the expansion of transport systems, especially to colonies;

4. Ernest Mandel (1982, 86–87) argued that neither Kondratiev nor Schumpeter nor Dupriez matched the depth and scope of Van Gelderen's arguments. This is an exaggeration, 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. Mandel presented his own theory of long waves in other contributions (see, in particular, Mandel 1980).

3. the evolution of the trade turnover through the expansion of the capitalist system to new areas, such as the industrialization of the United States, Russia, and the east Asian 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 "springtide" was considered to be the expansion in aggregate demand caused by the increase in production. The faster growth rate 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 later writers and only translated into English in 1996) and of the author himself (he committed suicide in 1940 when the Nazi invasion of the Netherlands was imminent [Reijnders 1990, 54 n]) cries out for justice in this regard. The recent publication of his essay ([1913] 1997), as well as the forthcoming publication of the works of Clarke, Sam De Wolff, and Parvus (Louçã and Reijnders 1999), is a step in that direction.

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 (1911, 220), claiming to apply the "scientific procedures" of physics, described the economic system as a pendulum. Its dynamics were consequently studied as a form of energy propagation accounting for the waves, which were generated by exogenous factors (222). Pietri-Tonelli considered the interplay of economic and political factors 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, Pietri-Tonelli conducted an extensive investigation into the symptoms of the long fluctuations, namely, time series of prices, theater 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.

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

Pareto (1916) explained the long waves in the economy by the social conflict inside the elite (the ruling class) between entrepreneurs (speculators) and *rentiers* (traditional capitalists): the alternating domination explained the successive periods of daring expansion and timid contraction. At the same time, A. Aftalion (1913, 1–7), Marcel Lenoir (1913, 148–49), and J. Lescure (1912, 452–90) detected and discussed these long movements.

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 Conjuncture Institute organized a debate about Kondratiev's 1926 paper, Spektator referred to Parvus, and S. A. Falkner criticized Kondratiev for not acknowledging the works of De Wolff, Bresciani-Turroni, and Pietri-Tonelli. In his reply, Kondratiev (1992, 244, 250, 289) indicated that, after the preparation of the 1926 draft, he had read Bresciani-Turroni, but not the others. No one yet referred to Van Gelderen. Later on, 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 De Wolff (1924, appearing for the first time in English in Louçã and Reijnders 1998). 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 (1825–1849, ebb tide, 1850–1873, springtide, 1873–1895, ebb tide, 1895 and afterward, springtide or *Sturm und Drang*) and used sophisticated descriptive statistical methods following Van Gelderen.

These authors are important predecessors: they indicate a broad con-

sensus 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 the fact that they all reached the same conclusions independently of each other. Such consensus established the main methodological 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 showed), 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, as well as the statistical treatment of the series in order to detect and prove regularity and recurrence.

B. 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 because they 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 has been the most precise and complete source of reference in this debate—and still a rare reference in English, together with Richard Day's 1981 book and a few other contributions[5]—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 also his work has been discussed for at least five or six decades on the basis of incomplete and incoherent versions of the original writings. The following pages provide a short review of that work, briefly outlining the main theses (see also Day 1981; Kleinknecht 1987; Solomou 1988; and Reijnders 1990), while some of the analytical contributions will be examined in the third section.

The 1922 book and the 1923 Kondratiev-Trotsky debate In 1922, Kondratiev published a book formulating in passing the long-cycle hypoth-

5. A major new contribution on these topics appeared recently (Barnett 1998).

esis 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 a paper prepared as an answer to his critics, Kondratiev (1923, 524) emphasized that the "major cycles of the conjuncture were only considered as probable." This was interpreted by some as implying a simple mechanical recurrence so that, after World War I and the severe depression of the recent postwar years, a longer period of recovery would necessarily occur. As a consequence, the author was sharply attacked by some critics (e.g., Ossinski), who accused him of a procapitalist attitude. This particular controversy is irrelevant and will not be considered here.

On the basis of his previous work on epistemology and the analytical representation of history, Kondratiev argued that irreversible and reversible processes coexisted,[6] although "the evolution of the economy as a whole is an irreversible process" (496), comparable to that of an organism. Although declaring himself a non-Marxist, Kondratiev insisted that he was precisely following Marx's understanding of the genetic process of capitalism, in keeping with the analyses of major cycles by Lescure, Aftalion, Leon Trotsky, Anton 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 (see also Klein, this issue). 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 (1923, 7–12) referred to two concepts of equilibrium: (1) the "secular equilibrium," that is, 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 dis-

6. The 1992 French translation of Kondratiev's works is used here.

proportion. 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.[7] 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—that is, 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, 1851–1873, 1873–1894, 1894–1913, and so on (15). In order to illustrate his argument, Trotsky used a table published earlier in January by the *Times*, describing political, ideological, and economic evolution over more than one hundred years (see Klein, this issue).

This distinction had a political intention, namely, to preserve by conscious social decisions the possibility of ruptures imposed by antisystemic forces. In this framework, exogeneity once more emphasized the creative role of strategy and social design. As Day (1981, 89) 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 'internalising' Trotsky's external conditions, he produced an ultra-deterministic theory of history that few Marxists could contemplate."

The contradistinction was very sharp, since Kondratiev instead considered an "irreversible" movement, one that could not be changed by any sort of events 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.

7. In his report to the June-July 1921 Third Conference of the Komintern, Trotsky (1921, 226) 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 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, Kondratiev (1992, 521–22) quite candidly quoted Eugen Varga's position and Trotsky's speech at the Third Conference of the Komintern, in which they acknowledged the decisive change in the international conjuncture.[8] In these remarks about the change of the conjuncture Kondratiev saw something else, such as a more general statement about the possible evolution of a new long-term expansionary wave immediately after the depression years without requiring a new change in political conditions. Furthermore, in 1923 the setting was already completely different: the economic situation had once more deteriorated, and the revival of the German left led the KPD and the Russian CP leaders to prepare for a major confrontation and to hope for victory, not only in Germany, but also in other central European countries, such as Hungary. At that time, the most intense debate was quite different from the 1921 one and did not involve the German "left wing" any more.[9] Instead, it involved Trotsky and, in the opposite corner, Nikolay Bukharin, who argued that a process of stabilization dominated the evolution of the world economy and, namely, prevented a revolution in Germany (Day 1981, 87). Bukharin's concept of "moving equilibrium" was thus seen as a major theme for discussion, and Kondratiev's argument was easily interpreted as an endorsement that eventually provoked the reaction of the opposite side.[10] But I rest my case

8. In fact, by 1921, the Komintern was already engaged in a heated discussion opposing Trotsky and Lenin to Grigory Zinovyev, Béla Kun, and the leaders of the German KPD (Communist Party of Germany), who argued that the conditions for a central European revolution had been met and that the moment was ripe for an "offensive," given the maturity and the catastrophic nature of the crisis of capitalism. The Russian leaders, on the other hand, argued that the defeat of the 1918 German revolution, with the assassinations of Rosa Luxembourg and Karl Liebknecht, the new relationship between forces, and the change in the economic conjuncture, prevented any short-term revolutionary uprising. The events that followed, such as the failure of the 1921 "March Action" in Germany and the consequent defeat of the KPD apparently confirmed the point of view of Lenin and his followers. In this framework, the quotations used by Kondratiev were clearly misinterpreted by him and taken out of context.

9. The 1921 contenders no longer belonged to the leadership of the KPD, since Heinrich Brandler had replaced Arkadi Maslow and Ruth Fisher.

10. Kondratiev seems to have missed most of these implications. In 1923, he simply stated his surprise, since he considered Trotsky's 1921 position to be an anticipation of his own stance (Kondratiev 1923, 521). In 1926, in the debate at the institute about his major paper on long cycles, Kondratiev (1992, 285) commented again on Trotsky's criticisms (and V. E. Bogdanov's, from a similar standpoint), saying that they were non-Marxist and idealist, as if

here, since the intricacy of Russian politics in the 1920s is not the theme of this article.

Anyway, this controversy is very telling, since it involves at least four important topics: (1) the legitimacy of the formal analogy, in both methods and theory, between the business cycles and the longer movements, which was implicitly supported by Kondratiev and explicitly criticized by Trotsky; (2) the evaluation of the conjuncture, namely, 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.

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 Conjuncture Institute was more important from the statistical and methodological point of view, although it merely redefined the earlier questions about broader interpretation issues.

The 1924, 1925, and 1926 papers and the Kondratiev-Oparin debate

Kondratiev's 1924 paper on statics and dynamics raised the debate to a new level of argument. Statics was defined as describing the "essence" of phenomena and, as a consequence, equilibrium became the organizing concept: "The concept of equilibrium between the interdependent elements of reality is the most typical" (Kondratiev 1992, 2). Yet these definitions were paradoxical; the Aristotelian *essence* was supposed to be captured by statics, but reality is dynamic, since there are changes in time: "Economic reality is dynamic in its very essence" (2, 7). Kondratiev did not accept that the static *essence* was real, or at least that it represented the whole reality, and furthermore used the concept of *essence* in two distinct ways. Nor was such confusion solved by Kondratiev's references to certain authors interested in dynamics: the Historical School, Marx, Schumpeter, and Cassel.[11]

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they expected some indeterminate exogenous and mysterious forces to account for the turning-points. Indeed, Kondratiev's close proximity to Bukharin's argument was just coincidental, and there was no political solidarity between them—some years later, in 1928, after having been expelled from the troika in power, Bukharin denounced Kondratiev's stance on industrial politics (Barnett 1995, 431).

11. In 1924, Kondratiev (1924, 11) criticized Schumpeter's confusion, in his 1911 book, between the static mode of analysis and the claim that the processes he described were sta-

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*. 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 movements: (1) irreversible processes, which have a direction, for example, the growth of population and the volume of production, the models of enlarged reproduction (17); and (2) reversible processes, which may change direction, for example, interest rate, prices, employment (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 *substratum*, although he recognized that this did not have a convenient analogue in economics (14–15).

Earlier in his 1923 paper, he considered that some processes, such as the evolution of technology, changes in needs and in the organization of firms, and changes in population and in volumes of production, were subject to both types of movements—irreversible and reversible (Kondratiev 1992, 493). Later on, in his 1926 paper, he considered that the more complex cases, such as technological change and innovations, wars, and revolutions, should be described both as consequences and as part of reversible processes.[12] Of course, both positions are incoherent, since if the irreversible increase in the volumes of production is separated from cyclical movements, there is no point in considering innovations—which affect the volume of production—in the

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tic by nature. He anticipated Frisch's critique, which was later accepted by Schumpeter himself. Apparently, Schumpeter never read Kondratiev's argument on this topic, nor did Frisch. Incidentally, the 1924 paper proves that Kondratiev had an impressive knowledge of the literature on macroeconomic cycles: Jevons, Léon Walras, Pareto, Clark, Alfred Marshall, Wicksell, Clement Juglar, Tugan-Baranowsky, Spiethoff, Lescure, Aftalion, Mitchell, and Schumpeter were all quoted. The paper also marks the first debate with D. I. Oparin.

12. Gaston Imbert (1959, 270–72) interpreted Kondratiev's theory as including a set of "intermediary causes," namely, technological changes, wars and revolutions, the exploitation of new regions, and changes in gold production and agricultural production. This is not entirely true: as far as long cycles are concerned, these variables are strictly endogenously determined in Kondratiev's model. He did not accept any notion of cumulative or hierarchical causation, perhaps because of his controversial bias and statistical necessities, since Kondratiev otherwise would be forced to abandon the statistically defined concepts of trend and cycle.

framework of reversible movements, and even less so than their mere consequences.

On the other hand, if Kondratiev insisted on his 1923 position, no trend-cycle decomposition would have been possible; with this later and completely self-contained model this became possible. It is therefore obvious that Kondratiev's precise arguments about the distinction and the boundary between reversible and irreversible movements were defined in function of the technical requirements of his statistical procedures, and that he accordingly changed his views in 1924.

In 1925 and 1926, Kondratiev presented long empirical studies and defined his theoretical approach. The thesis has four main epistemological and theoretical characteristics. First, Kondratiev (1926a, 111) argued that crises are "organically" a part of the capitalist mode of production, as Marx and Juglar considered. This was an important argument in favor of rejecting simple exogenous causality,[13] 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 (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 Kondratiev stressed in a debate with S. A. Pervushin (Barnett 1996, 1021).

Second, Kondratiev considered that this organic, holistic, and nonatomistic 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 natural sciences (1992, 83). In other words, unlike neoclassicals, for whom rationality is typically associated with the pattern of a representative agent, Kondratiev saw in it the creation of variation.

Third, for Kondratiev (1992, 159) such variation was still compatible with equilibrium. The system always tends toward 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." So, impulses were conceived of as disequilibrium

13. In fact, Kondratiev later used these concepts in yet another context, when arguing that planning should be "genetically" and not "teleologically" engineered (Abalkin 1992, 10). This disguised terminology reveals that Kondratiev opposed voluntarism in the definition of the plans.

processes, caused by "radical changes in the conditions of production" through infrastructural investment in essential capital goods (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 the changes in the system itself were not dealt with.

Fourth, Kondratiev considered two types of laws: (1) causal laws, which should be necessary and universal conditions; and (2) empirical laws, which were noncausally interpretable, based on correlation and statistical analysis, and corresponding to what later became known as "stylized facts" (73, 141).[14]

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 movements 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. 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. The contradiction is very clear in Kondratiev's analogy with Marshall's "third-order equilibrium," in which the conditions for the production of capital goods vary. Obviously, such an analogy is untenable unless these conditions are causally significant, and not mere effects of the equilibrating process itself. In short, in this account, equilibrium is constantly mentioned, but its functions vary in accordance with the needs of the researcher; it is a reference point, but so irrelevant that it needs

14. While in prison, Kondratiev prepared and posted to his wife the plan for a five-volume work that would include discussions on statics, dynamics, methods for the study of social sciences, long waves, and other matters. Some of these papers by Kondratiev were published in Russian in the 1990s (part of them will be in the forthcoming Kondratiev 1998). According to the description by L. Abalkin (1992), Kondratiev considered two types of laws: (1) laws of correlation (the empirical laws of 1926), and (2) "laws of changing occurrences," which would indicate the uniformity of changes in time (e.g., the entropy law and the law of the falling rate of profit).

no explanation and comprises no evolution. Just as in orthodox economics, equilibrium is a cliché for Kondratiev.

Many authors centered their criticism of Kondratiev on the imprecision and vagueness of his causal explanation for the long cycle. This explanation 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: "The long cycles may be considered as a rupture and then as the reestablishment of long-term economic equilibrium. Their main cause is the mechanism of accumulating and dispersing of a sufficient capital for the new productive forces" (Kondratiev 1992, 167). The necessary conditions for the upswing are in this account: a large volume of capital accumulation, obtained by a process of accumulation taking place faster than investment; the concentration of capital; and the availability of capital for strategic decisions (159). My argument is that this debate was indeed relevant, yet 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 information.[15]

In this study, just as Van Gelderen had done fifteen years before, Kondratiev (1926a, 140) identified some major transformations in productive forces, such as the new industrial revolution that was driving

15. Kondratiev (1992) identified four empirical laws: (1) some years before the beginning of a new long cycle, important changes occur in technological innovation, monetary circulation, and the role played by new countries (138); these changes could occur as much as twenty years before (141); (2) the class struggle, including wars and revolutions, is more intense in the upswings; (3) agricultural depressions are more intense in the downswings; and (4) the downswings of the shorter cycles are more intense in the downswings of the long cycle, and the reverse is also true (140). Van Gelderen ([1913] 1997, 49) had already formulated this last "empirical law."

the transition from the second to the third long wave, based on the chemical, electrical, and motor industries. 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 1810–1817, and the downswing from 1810–1817 until 1844–1851; the upswing of the next wave from 1844–1851 until 1870–1875, and the downswing from then until 1890–1895; the upswing of the third wave from 1891–1896 until 1914–1920, and the downswing from 1914–1920 onward (see the comparison between Kondratiev's dating scheme and those of his forerunners and contemporaries in appendix 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 essential part of the research.

The crucial discussion of these ideas was centered upon the paper presented 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 counterreport, and in 1928 a pamphlet was published including Kondratiev's and Oparin's contributions and the minutes of the seminars. The 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 proved 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 "perspectivistic distortion." This topic will be discussed in more detail in the third section.

Eventov and V. E. 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, 246–50; Garvy 1943, 210), therefore implying that decomposition was not justified. N. 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.

The 1928 paper and the overall vision of the problem The 1928 paper on the dynamics of industrial and agricultural prices is a central piece in Kondratiev's (1928b) argument; it is the most complete of his texts and probably one that represents his greatest efforts at preparatory discussion with colleagues.[16] It presented an overview of the long-cycles debate and classified the main categories of theories: (1) authors noticing but not explaining the long cycles (Aftalion, Spiethoff, Parvus, De Wolff, Van Gelderen, Sombart); (2) authors explaining the cycle as the result of random impacts (Kautsky, Cassel, Pietri-Tonelli); (3) authors explaining the cycle as fluctuations of credit (Bresciani-Turroni); and (4) authors explaining the cycle through fluctuations in interest rates or accumulation (Wicksell), among other views (424 n). This classification is arbitrary and wrong in several instances, but it does at least indicate how Kondratiev read the contemporary authors as well as his own efforts to present an overall synthesis.

Two elements in that synthesis were already present in the 1925 and 1926 papers, but were discussed in greater detail in 1928. The first is the concept of the two dimensions of economic dynamics: the irreversible processes of development and the reversible fluctuations occurring in these trends, defined as the decisive features of the conjuncture. Once again, Kondratiev interpreted Marshall and argued that the irreversible trend corresponded to the "dynamic equilibrium" and that the reversible changes were part of this same phenomenon, but that one was forced for statistical reasons to study these facts as cycles that were totally independent of the overall irreversible dynamics. This fitted in quite well with both the available techniques and the general

16. The author thanked Slutsky, W. Persons, Hansen, and others for comments. The impressive list of quoted authors indicates the great scope of the argument: Sombart, Emile Durkheim, Georg Simmel, Aftalion, Schumpeter, Mitchell, Spiethoff, Cassel, Robertson, Lescure, Tugan-Baranowsky, Keynes, Emil Lederer, Fisher, Adam Smith, David Ricardo, J. S. Mill, Frank Taussig, Persons, Bowley, and François Simiand were quoted, among others.

consensus of the epoch, although it did not correspond to the organic and genetic approach Kondratiev was simultaneously arguing for.

The second 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. Consequently, Kondratiev (1928a, 425) established 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."

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 lifeless world. The next section presents some of the reactions by Kondratiev's contemporaries, and the following one discusses his methods and conclusions.

C. 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. The majority of the economists involved at the core of the project for developing the new approach of econometrics (Frisch, Tinbergen, 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 he 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 (1927, 4) subscribed to Kondratiev's hypothesis of thirty- to fifty-year-long "time movements around which the business cycle is fluctuating," forming a "major cycle." The source of the reference was the 1926 German translation of "The Major Economic Cycles," 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 one who also shared this conjecture about the existence of long waves—developed his approach autonomously from Frisch: they first began corresponding in August 1927, after the dissemination of the time series paper. Their correspondence did not mention Kondratiev, whose hypothesis Schumpeter had already publicly accepted. Indeed, Schumpeter's and Frisch's 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 favor of Kondratiev will not be discussed in this article. But it is worthwhile emphasizing Frisch's engagement in the same camp, although he did not discuss the hypothesis in detail in his scientific or mathematical texts. Yet, Frisch did try to prove 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, he 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 Georges D'Avenel (1894; see figure 1).

Frisch looked in particular at the years 1300–1800, 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.[17] Since this explanation

17. 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

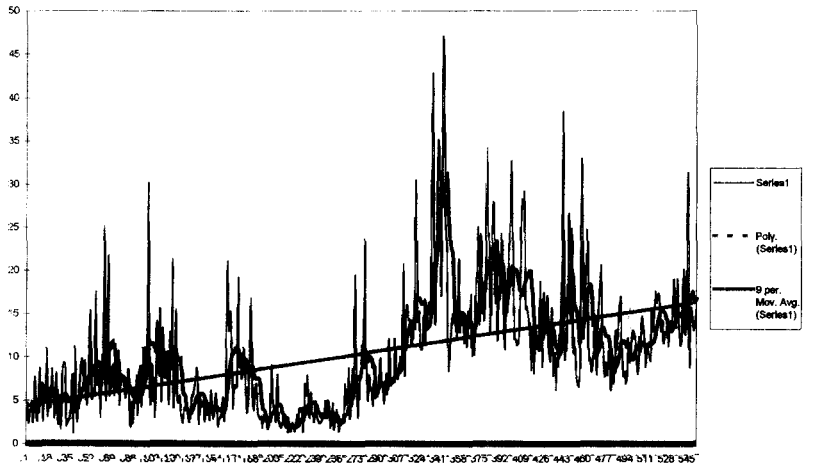

Figure 1 Author's reconstruction of Frisch's (1932) representation of D'Avenel's series for the price of wheat (1200–1800)

proved very effective for understanding the great ravages of the 1930s, at least as far as Frisch concluded, he maintained it throughout his life.

Tinbergen (1929, 858) very soon, and also independently, defended the same hypothesis for quite similar reasons, since he had read De Wolff's book and surveyed it in 1929, noticing that a parallel line of investigation was being carried 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" (my translation).[18] 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, Tinbergen wrote a rather favorable preface to Kleinknecht's book on the issue.

In his important 1927 book, Mitchell acknowledged the work by Van Gelderen, De Wolff, and Kondratiev (once again based on the 1926 German translation; Mitchell [1927] 1956, 227), and commented on their contributions, although the theme of the book was the business cycles. In a later work, Arthur Burns and Mitchell (1946, 431–40) again discussed "the most celebrated of the long cycle theories," "the

best doubtful. But this did not prevent Frisch's acceptance and profound belief in this interpretation.

18. I thank Marcel Boumans, Amsterdam University, for this reference.

daring hypothesis that long waves in the wholesale prices are an organic part of a long cycle characteristic of capitalism."

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 ([1930] 1967, 259–63) 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. Furthermore, Kuznets knew Trotsky's, Van Gelderen's, 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 toward the long-wave hypothesis, and it is well known that Kuznets (1940) developed an alternative account of long-term historical evolution (for more recent reappraisals, see Adelman 1965 or Solomou 1988). Lange (1941) criticized this approach and defended Schumpeter, despite his own doubts about the long-wave theory.

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 1939 book. At the same time, other researchers into business cycles (e.g., Haberler) distanced themselves from any claim about long waves. In spite of this, and basing himself on Spiethoff and Schumpeter, Haberler (1937, 308) 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 prove the existence of regular factors generating the fluctuations.

Another distinguished researcher, Hansen, acknowledged and quoted from the 1935 American translation of the 1926 German translation, and compared Kondratiev's arguments to those of Spiethoff, Schumpeter, Mitchell, and S. von Ciriacy-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 might be added that later Hansen (1951, 56) took a much more "agnostic and even very sceptical position" on the same issue.

It is obvious that by that time, the late 1930s and the early 1940s, Schumpeter had become the main proponent of the thesis, or at least the person most involved in its defense, since both Frisch and Tinbergen were isolated in Europe and surrounded by war, and decided not to

devote their professional attention to this issue. Frederick Mills was one of the economists Schumpeter took pains to convince: in a letter dated 12 April 1940, he kindly thanked Schumpeter for an evening spent discussing the hypothesis, "certainly an intriguing one and a useful one" (Schumpeter Archive, Box HUG[FP] 4. 21), but did not seem to be convinced. In 1942, Edwin Frickey published a book that included an important argument against trend decomposition and suggested a not entirely specified alternative genetic method. Frickey (1942, 8) argued that the secular trend should be assessed as "a problem in historical description," not as "a problem in mathematical curve fitting," and demonstrated that the fit of different functions could imply arbitrarily created cycles and therefore spurious conclusions. His conclusions from U.S. data were presented as compatible with Kondratiev's hypothesis (231 n, 232, 340).

The long paper by Garvy (1943) presented Kondratiev's main theoretical arguments, methods, and statistical evidence, and compared these with those of his opponents, concluding with the author'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" (208). Furthermore, he argued that there was no explanation for the trend—insisting that Kondratiev recognized his inability to prove that the trend corresponded to the real economic evolution—and that the acceptance of A. A. 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" (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 (219–20).[19]

In the 1940s, another researcher taught the Kondratiev thesis at the London School of Economics: W. W. Rostow (1948, 9, 29, 45), based on Schumpeter and the 1935 translation of "The Major Economic Cycles." Others, such as the very young Richard Goodwin, learned it from Schumpeter and later spread it to their colleagues. At the same

19. This is quite comparable with Angus Maddison's (1991, 95, 105) position on the same subject.

time, William Fellner took a somewhat more skeptical position. In 1949, he prepared a manuscript, which was discussed with Schumpeter, titled "On the Waves of Different Lengths with Particular Reference to the Long Waves" (Fellner 1949). He 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, in his 1956 book, Fellner took up the issue again, presenting Kondratiev's statistical methods (38) and inspecting a certain number of empirical series (40–41); his conclusions indicated the acceptance of long rhythms, but as irregular features of development. Consequently, "we prefer not to assert the existence of long cycles of fifty years" (42), since "the so-called long cycles in general economic activity are merely alternations between intermediate trends of greater and of lesser steepness" (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, and 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 dating of these processes was generally agreed upon by these researchers, although they disagreed as to the explanation and the epistemological and analytical solutions to Kondratiev's difficulties. And this is probably why there was such an impressive early consensus about the Kondratiev hypothesis: just as the early writers had known and acknowledged the impact of the Industrial Revolution, of the "hungry forties," of the Victorian boom, and of the "great depression" of the 1890s, the next generation had known the Belle Epoque, the periods of war, and the 1929 crisis. 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. Conclusion

As argued in this article, Kondratiev's research was one of the first major quantified inquiries into economic history. It established a general consensus on the dating of the long cycles, 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.

The first concerns the importance, depth, and scope of Kondratiev's endeavors. His work—in spite of its naïveté 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 (1926b) paper on forecasting is one of the masterpieces in the early literature about statistics and history. It is a powerful survey of the contemporary authors in economics, mathematics, physics, and philosophy,[20] 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 a more stable causal structure which accounts for a certain regularity of phenomena. Of course, this implied that the explanation of the complex whole is the 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 the

20. The text included references to, and quotations from, not only Clark, Bowley, Roger Babson, Jevons, Tugan-Baranowsky, William Beveridge, Denis, Gustav von Schmoller, Cournot, Friedrich List, Marshall, Mill, Kautsky, Engels, Marx, and Pareto, but also Person, Pyotr Strouvé, Durkheim, Ernst Mach, Henri Poincaré, Emile Meyerson, Auguste Comte, Simmel, Pierre Simon de Laplace, Ludwig Boltzmann, Max Plank, and the Portuguese Teófilo Braga.

author 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 (74). Moreover, they should be combined, since no definitive conclusion is possible from statistics alone: "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" (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. My 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 blind without each other. The reasons for the abandonment of this perspective are quite another story (Louçã 1997).

Kondratiev assessed economic history as part of societal evolution, used the available analytical and statistical tools, and discussed their epistemological foundations. The original consensus reached among his contemporaries proved that long periods of 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 in use at that time. This was indeed Kondratiev's decisive contribution, and what makes him worthy of our attention: history is part of economics, and economic methods are analytical and historical. Building on these insightful contributions, the challenging task presented by the economic appraisal of the major historical movements is still part of our future agenda.

Appendix 1 Main works by Kondratiev

Date	Publication	Notes
1922	"Report to the Third All-Russian Congress: Changes in World and Russian Agriculture and the Main Goals of our Agricultural Policy"	State edition, Vologda. In Kondratiev 1998.
1923	"Some Controversial Questions Concerning the World Economy and Crisis (Reply to Our Critics)"	Originally published in *Socialititcheskoie Khoziaistvo* 4–5:50–80. In Kondratiev 1992.
1924	"On the Notion of Economic Statics, Dynamics, and Fluctuations"	Originally published in *Socialititcheskoie Khoziaistvo*; one section was published as "The Static and Dynamic Views of Economics" (Kondratiev 1925). In Kondratiev 1992.
1925	"The Major Economic Cycles"	Originally published in *Voprosy Konjunktury* 1.1:28–79; German translation in *Archiv fur Soziawissenschaft und Sozial-politik* 56 (1926): 573–609; partial English translation in *Review of Economic Statistics* 18 (1935): 105–15; complete English translation (Kondratiev 1979 and 1984).
1926a	"About the Question of the Major Cycles of the Conjuncture"	
1926b	"Problems of Forecasting"	Originally published in *Voprosy Konjunktury* 2.1:1–42; German translation in *Annalen der Betriebswirtschaft* 1–2 (1927): 41–64, 221–52. In Kondratiev 1992 and 1998.
1928a	"Dynamics of Industrial and Agricultural Prices (Contribution to the Theory of Relative Dynamics and Conjuncture)"	Originally published in *Voprosy Konjunktury* 4.1:1–85; abridged German version in *Archiv fur Sozialwissenschaft und Sozialpolitik* 60 (1928): 1–85. In Kondratiev 1992.
1928b	*The Major Cycles of the Conjuncture*	Originally published in Russian with the 1926 papers and debate with Oparin as *Economitcheskaia Jizn*. In Kondratiev 1992 and 1998.
1934	"Main Problems of Economic Statics and Dynamics"	In Kondratiev 1998.

Appendix 2 Dating of long waves

	First LW		Second LW		Third LW	
Author/period	Upswing	Downswing	Upswing	Downswing	Upswing	Downswing
Engels		1825–1842	1842–1868	1868		
Tonelli			1852–1873	1873–1897	1897–1913	
Bresciani-Turroni			1852–1873	1873–1897	1897–1913	
Van Gelderen			1850–1870	1870–1895	1895	
De Wolff		1825–1849	1850–1873	1873–1895	1895	
Trotsky	1781	1851	1851–1873	1873–1894	1894–1913	1913
Kondratiev	1780–1790	1810–1817	1844–1845	1870–1875	1891–1896	1920
	1810–1817	1844–1845	1870–1875	1891–1896	1914–1920	

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