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**KONDRATIEV LONG CYCLES:  
NEW INFORMATION ABOUT  
DISCUSSIONS IN THE USSR IN  
THE 1920S.**

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**KONDRATIEV LONG CYCLES:  
NEW INFORMATION ABOUT DISCUSSIONS  
IN THE USSR IN THE 1920S.**<sup>2</sup>

This article is devoted to a reconstruction of Soviet discussions on long cycles based on an analysis of a wide range of unknown and little-known historiographical sources and archival materials. The role of discussions in the formation of assessments and ideas about long cycles is shown. In particular, we show that the controversy between Kondratiev and *Gosplan* about the economic development of the USSR had a noticeable effect on the negative evaluation of Kondratiev's method for identifying long cycles. The study defines the relations between Kondratiev's views on the problem of long cycles and his probabilistic-statistical approach to the analysis of society.

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Keywords: long waves, Kondratiev cycles, capitalism's crisis, the Conjecture Institute, random events, criteria of refutation, teleological method, genetic method.

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## 1. Introduction

The history of long cycles is surprising in that the publication of an abridged translation of Kondratiev's article (1935) led to active discussions, which were based on extremely fragmented ideas about his scientific heritage. This publication made him, a prisoner at that time, famous in the international scientific community: soon afterwards major cycles<sup>3</sup> were named in his honor by Joseph Schumpeter (1939) but only much later in the 1980s were Kondratiev's major works reissued (1989; 1991; 2002) and later still translated into English (1998; 2004b). In light of this, researchers analyzed the heritage of this scientist and his colleagues in the search for answers to questions regarding the theory of long waves (Barnett 1998; 2002; Klein 1999; Maddison 2007; Owen 2009).

An investigation of discussions in the USSR about major cycles faces a number of problems. These difficulties can be of three types. The first is connected with the reconstruction of the views of Soviet scientists on the basis of various types of sources, many of which remain unknown or little-known to the scientific community. For example, in contemporary Western literature, researchers seem to ignore *Basic Problems of Economic Statics and Dynamics* (1991), written while Kondratiev was incarcerated and which remained unknown in his homeland for over half a century. It was in this work (about 20 author's sheets) that he proposed his own epistemological conception, without which it is problematic to understand his views on the problem of major cycles.

The second group of difficulties is the methodological order. Unfortunately, the literature on long waves is full of works retelling the ideas of Kondratiev and his contemporaries. An internalist approach, peculiar to methodology of the history of ideas, also cannot be fully satisfying. We should turn to the toolkit of intellectual history, which calls for an account of the social and political contexts when analyzing ideas.

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<sup>3</sup>The participants in the discussions in the USSR, in contrast to modern scientists, did not contemplate 'long waves' and 'major cycles' as synonyms (*Obsuzhdenie* 1989 [1928], p. 310). They believed the major cycle (long cycle) to be strictly periodic. The fact that Kondratiev chose 'cycle' rather than 'wave' is important.

The third type of difficulty is the textual form: the correct translation of key terms, citations, abbreviations when reissuing the works. Most of the questions arise regarding the sources and literature used in the research. Kondratiev and his contemporaries, analyzing a number of conceptions and ideas, often did not mention the names of their authors (as a rule, for political reasons). The sources mentioned in the manuscript (1991) written by Kondratiev while incarcerated are especially fragmented. This requires very careful work to reconstruct the sources of his research.

All these difficulties require us to question the established assessments of Kondratiev's scientific work, and the work of his colleagues and rivals. The Soviet discussions in the 1920s significantly affected the study of long cycles and we would like to focus on two very topical problems in this field of research, where this influence is most noticeable.

First, there is the contentious question of the methods of detecting long waves, in particular a pronounced skepticism about the validity of the method proposed by Kondratiev (Garvy 1943; Metz 1987; Goldstein 1988; Maddison 2007; Poletaev and Savel'eva 2009, etc). We usually deal with the work of researchers who, even if they recognize the existence of long waves, still tend to give preference to spectral analysis. It is not difficult to see that this view was to a certain extent formed under the influence of the works of Kondratiev's opponents. At first glance, the opinion of his critics in the mid-1920s inspires confidence. This discussion took place far before Kondratiev was accused of 'wrecking', however, the majority of the participants were famous Soviet economists and statisticians specializing in economic conjuncture. Modern empirical research realized that the methods "widely used in natural science are unsuitable for revealing quasiperiodic fluctuations in the social-economic processes", for the identification of which "approximate and obvious methods are used" (Glaz'ev 1990, 32-33). In this article we reinforce this point of view, though not on the basis of the analysis of Kondratiev's method about which the literature has said everything or almost everything. We justify the hypothesis that critics of the 1920s, having put forward

claims to this method, were very biased, therefore their arguments should be treated very carefully. It is necessary to find out why, in the mid-1920's, Soviet scientists were almost unanimously critical of Kondratiev's research.

Secondly, the arguments about the factors of long waves are interrelated with the confusion regarding how Kondratiev explained them. Opinions were expressed that he allegedly explained major cycles by solar activity (Chaunu 1974) or “various exogenous shocks to the system, such as wars and gold discoveries” (Blaug 1986, 114), that he wrote about “the important role of the innovation process” (Fontvieille 1991, 238). Some researchers allege that Kondratiev never offered a theoretical explanation (Rostow 1975, 720; Perez 2010, 190; Hagemann 2014, 124). To understand his point of view it is not enough to establish what factors he considered important. It is necessary to reconstruct his ideas about the nature of regularities and about the specifics of cause-effect relationships in the social world. It would not be an exaggeration to consider that Kondratiev remains a researcher of long cycles, who paid the most attention to epistemology. Turning to his epistemological views, we are likely to be able to understand his vision of major cycles and find answers to the questions about their verification. It is important that in the current decade, which is the beginning of a new phase of the Kondratiev’s cycle, we are observing a corresponding, fundamental change in the economic and social-political life of society.

In order to understand the motivations of the discussions in the 1920s, and also reconstruct and analyze Kondratiev’s epistemological views, it is necessary to consider the Soviet discussions about major cycles.

## 2. *Kondratiev vs his first critics*

Kondratiev first proposed the existence of long cycles in 1922 in his monograph *The world economy and its conjunctures during and after the war* (2004b [1922]). He only presented the alleged dating of major cycles and determined the place that the crisis of 1920–1921 occupied in the cycles. Kondratiev argued that this crisis completed the rising wave (1896–1921) of the

third major cycle. He denied the view “that we deal with some specific exceptional crisis that is not an element to be interpreted within an economic theory” (ibid., 250). However, proposing this counter-thesis, he did not mention his opponents.

The majority of the first critics of Kondratiev were very skeptical of his views. His assessment of the crisis of 1920–1921 contradicted the notions of the inevitable and swift collapse of world capitalism – such ideas were especially typical for representatives and supporters of the left opposition. Therefore, Kondratiev’s position could not but draw the attention of well-known Marxists. Kon, Bronsky and Osinsky criticized Kondratiev’s ‘intention’ to prove the possibility of capitalism surviving the economic crisis and the eternal existence of major cycles (Kon 1923; Bronsky 1923; Osinsky 1923a).

Responding to his first critics, Kondratiev expressed bewilderment at how they had inferred the alleged premise of the eternal existence of capitalism and major cycles. Moreover, he noted, “we consider this great cycle of capitalist conjunctures as *probabilistic* [italics added] [...] because, first of all, we do not know the possible course of an irreversible socio-economic process” (1923, 73). According to Kondratiev, if capitalism collapsed, the downward motion of the major cycle would not take effect. However, the correctness of the law “cannot be made dependent on the constant nature of its detection” (ibid.). It is no coincidence that in his subsequent studies (1925, 65; 1989 [1928], p. 172). Kondratiev emphasized that his conclusions are valid only for the capitalistic economy; apparently, he feared accusations of his intent to prove the ‘eternity’ of major cycles.

In 1923–1925 Kondratiev’s monograph attracted the most attention from Osinsky (1923a; 1923b; 1924; 1925). Was his criticism based on some problems in his relationship with Kondratiev or a negative attitude to Kondratiev’s political activities in 1917–1918? Most likely, the appearance of the monograph in particular spoiled their relationship, since several months before its release Osinsky had asked Stalin not to exile Kondratiev, “The head of the whole administration

has been arrested [...] thereby a number of statistical works were frustrated [...] Kondratiev presents no political danger” (Kondratiev 2004a, 674).

### 3. *Kondratiev vs Trotsky*

At the end of 1923, Trotsky joined the discussions, which were strongly influenced by his ideas. He first proposed a counter-thesis that Kondratiev’s long-term fluctuations are not cycles: they can only be considered as long waves which are determined by random factors and “the external conditions in which capitalist development occurs” (1923, 9). Trotsky attributed to such external conditions: “the absorption by capitalism of new countries and continents, the discovery of new natural resources, and, in addition, significant factors of a ‘superstructural’ order, such as wars and revolutions” (ibid.). How can the cyclical nature of long-term fluctuations be justified? This question had been put before Kondratiev.

Kondratiev responded to Trotsky’s criticism in his report *The World Grain Market and Prospects for our Grain Exports* in 1923 but by this time Kondratiev had a hypothesis explaining the long cycles: “the major cycles of conjuncture [...] are caused by [...] a radical redistribution of accumulated and accumulating capital” (1993 [1923], 211). In fact, Kondratiev responded to Trotsky’s remarks in *The Major Economic Cycles* (1925). The results of his empirical study confirmed the hypothesis of the existence of major cycles and also denied that “long waves – as distinct from the intermediate ones which come from causes within the capitalistic system – are conditioned by casual, extra-economic circumstances and events, such as (1) changes in technique, (2) wars and revolutions, (3) the assimilation of new countries into the world economy, and (4) fluctuations in gold production” (1935 [1925], 112).

Recognizing the influence of these processes on economic dynamics, Kondratiev stressed that the opposite effect is also present. However, he revealed the rhythmic development in technology, the regularity of wars, revolutions, and gold production. Therefore, according to Kondratiev, we should not recognize the random nature of these phenomena (ibid., 115). However, in his article, in regards

to denying the randomness of these phenomena, he did not explain what he meant by this concept. Kondratiev still did not present a vision of the causes of major cycles. In other words, he rejected Trotsky's arguments, but still did not propose a substantiation in his thesis for the existence of major cycles.

On January 18, 1926, Trotsky and Kondratiev took part in a discussion at the Industrial Economic Council, whose materials (except for Trotsky's separately published report) remained unknown in the scientific community (*Mirovovoe* 1926). Kondratiev and Trotsky were invited to present forecasts for the next triennium. Against the backdrop of the rapid economic development in the United States and a number of European countries, ideas about capitalism's stabilization became widespread, and this required contemplation. At the discussion Kondratiev said, "In the near future, before the passing of the year 1927, we can expect the onset of [...] an industrial crisis. This will be the second post-war crisis in the United States" (*ibid.*, 64). Reinforcing his point of view, Kondratiev pointed out that the prices of the shares were "close to the culmination point, which is a sure harbinger of an upcoming crisis" (*ibid.*). This is the only published prediction of Kondratiev which directly related to the crisis of 1929. However, one should take into account the fact that well-known economists of the 1920s, like most analysts, "were devastatingly wrong about the 1929 crash and ensuing economic crisis" (Skousen 1993, 249).

In his speech, Trotsky continued to criticize the idea of major cycles. He stated that they were not confirmed for individual countries, in contrast to the cycles studied by Marx (1926, 190). In this instance, Trotsky had anticipated (or predetermined) one of the key questions of the subsequent disputes on Kondratiev's conception.

#### 4. *Kondratiev vs Gosplan*

A month later, at the Institute of Economics, Kondratiev's report was discussed on the initiative of Oparin (Kondratiev 1989, 471). In this work, Kondratiev linked long cycles with the mechanism "of accumulating and



dispersing sufficient capital for the new productive forces” (ibid., 400). In other words, he presented an explanation for major cycles by analogy with Marx’s explanation for medium-term cycles. It would be erroneous to believe that Kondratiev’s reference to Marx was required and expected from a Soviet scientist. However, the middle 1920s was a time of relative pluralism, in which Kondratiev declared his balanced attitude to Marxism, “We do not consider ourselves to be Marxists, but we consider Marx’s method as very valuable and scientifically productive” (1923, 64).

This report was translated rather late (Kondratiev 1998), which may explain why there is confusion about Kondratiev’s view of the causes of major cycles. The lateness of the translation of the discussion materials means it is possible to explain why Kondratiev was absolutely not associated with Marxism for most Western scholars, including a number of neo-Marxists. Perhaps this explains why one of the most famous critics of Marxism, Schumpeter, played a major role in popularizing of Kondratiev’s conception and why Schumpeter named the major cycles in honor of Kondratiev, but not Jacob van Gelderen and Salomon de Wolff, who were Marxists and who had previously argued for the existence of long cycles.

Let us turn to the discussion materials. It is believed that this discussion was mostly of a scientific nature: Kondratiev's critics focused their attention on the theoretical and methodological aspects of his research. As noted above, their ideas influenced the emergence of a negative assessment of the method proposed by Kondratiev for identifying long cycles. These discussion materials were quoted many times, especially in modern Russian historiography (Belyanova 1991; Poletaev 1991; Grinin and Korotaev 2017). Therefore, we would like to focus only on Bogdanov’s<sup>4</sup> speech because he intended to overcome the one-sidedness of the discussion, “Almost all the comrades who spoke referred only to the statistical form of the rapporteur's presentation, leaving its theoretical component without criticism” (*Obsuzhdenie* 1989 [1928], 300-301). Actually, Bogdanov had developed Trotsky’s thesis about the random nature of long waves, “by

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<sup>4</sup>We have no record of V.E. Bogdanov. He should not be confused with Alexander Bogdanov.

randomness we mean [the accidental] intersection of two causal series” (ibid., 304). What matters for subsequent consideration is that in this instance Bogdanov meant the intersection of “on the one hand, the internal dynamics of the capitalist mode of production and, on the other, the state of the extra-capitalist environment” (ibid.).

It would be wrong, however, to think that the interest of Kondratiev’s critics in this instance was purely scientific. First of all, we should pay attention to the appearance of Bazhanov’s review in the newspaper *Pravda* (1925). In this review, he criticized the first issue of the journal *Questions of Conjunction*, where Kondratiev’s article was published (1925). By this time, criticism from Trotsky sounded less authoritative, but the appearance of Bazhanov’s review could affect the formation of a collective assessment. After his defection, Bazhanov wrote in his memoirs very flattering words about Kondratiev. He also noted that “the Gosplan communist cell, with no Bazhanov to hinder it, took on Kondratiev” (1990, p. 139). In the mid 1920’s most of Kondratiev’s opponents were employees of *Gosplan*<sup>5</sup>: Bazarov, Falkner, Oparin, Osinsky, Pervushin, Slutskii. Most of the critical articles were published in the journal *Planned Economy* (Bazarov 1926; Oparin 1926a; 1926b; Pervushin 1926; Suhanov 1926; Trotsky 1926; Svetlov 1929), which was published by the same institution. Why did *Gosplan* pay close attention to Kondratiev’s conception of major cycles? Was it just a coincidence?

In answering this question, we turn to the polemic between Kondratiev and *Gosplan* about the development plan for the national economy. Unfortunately, his study of socio-economic dynamics is usually considered without regard to this issue, and vice versa. In our opinion, there was a connection between Kondratiev’s conception of major cycles and his planned conception, which was criticized by *Gosplan*. The essence of this discussion (see Jasny 1972, 167-171; Barnett 1998, 143-146) was the choice of a teleological method (based on setting up desired targets regardless of the possibility of their achievement) or a genetic method (based on the extrapolation of spontaneous tendencies). The first approach was

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<sup>5</sup>The State Planning Agency.

taken by *Gosplan*, while the second approach was taken by Kondratiev and his colleagues in *NKFin*<sup>6</sup> and *NKZem*<sup>7</sup>.

In this instance Kondratiev's model of economic cycles, as shown by Vincent Barnett (1998, 72-73), was one of the tools for analyzing Soviet export prospects. Kondratiev's study of economic cycles was not devoid of practical significance and was not of a purely theoretical nature. We see that *Gosplan* criticized both Kondratiev's planned conception and his conception of major cycles. It seems that it was important for *Gosplan* to criticize not only the planned method based on the extrapolation of spontaneous tendencies, but also the prediction methods proposed by Kondratiev.

Within the structure of *Gosplan* was the Conjunctural Council, which carried out similar tasks to the Conjuncture Institute, which was under *NKFin*. It was possible to find a document which indicated that the most famous opponent of the major cycles conception, Oparin, proposed liquidating the Conjuncture Institute, of which Kondratiev was the director. Furthermore, Oparin believed that, "the Conjunctural Council should take the initiative to establish a commission", which would deal with analysis of the conjuncture (*RGAE, f. 769, op. 1, d. 21, l. 49*).

As we can see, Kondratiev had a complicated relationship with *Gosplan* and we can assume that they were very biased about his ideas.

##### 5. *Kondrat'evshchina vs Soviet Agricultural Scientists*

At the end of 1927, the journal *Bolshevik* published Zinoviev's article in which, for the first time, Kondratiev was accused of wrecking. In the following year, he was dismissed from his post as director of the Conjuncture Institute. Against the backdrop of these events, a number of articles emerged, whose authors 'defeated' his conception of long cycles (Gercenshtein 1928; Granovsky 1929; Eventov 1929a; 1929b).

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<sup>6</sup>The People's Commissariat of Finance.

<sup>7</sup>The People's Commissariat of Agriculture.

The situation was aggravated following Stalin's speech (1949, 141–172), in which he supported the proponents of the teleological method, at the conference of the Agrarian Marxists on December 7, 1929, and after Kondratiev's arrest in 1930. Shortly afterwards the campaign against *Kondrat'evshchina* began (Vajsberg 1930; Targul'yan 1930; Kurov 1930; Shklovsky 1931; Sadovsky 1931, etc.). A key role in this was played by the Agrarian Institute of the Communist Academy.

The derogatory term “*Kondrat'evshchina*” was meant for the wrecking of agriculture by Kondratiev and his real and perceived supporters. However, the critics in this instance condemned Kondratiev's ideas not only about Soviet agriculture and the planning of the national economy, but also about economic cycles. In particular, it was important for them to show that Kondratiev's ideas did not have any relationship with Marxism. Accordingly, the critics had tried to show that his conception of major cycles was created under the influence of his teacher Tugan-Baranovsky, who was considered a bourgeois economist. This assessment of Kondratiev's scientific creativity formed during his persecution. However, this assessment had a very significant influence on the development of his heritage research. Therefore, we agree with Korotaev and Grinin that in modern historiography Tugan-Baranovsky's role is overrated from the point of view of the structure of Kondratiev's theory (2017, 87-96).

Another ‘obvious’ piece of evidence for the inconsistency of Kondratiev's ideas was his appeal to the theory of equilibrium. Although Stalin did not publicly criticize Kondratiev at the conference of the Agrarian Marxists, Stalin (1949, 141–172) noted the “unscientific theory” of Soviet economists, in particular the theory of equilibrium, which was developed in the works of Bogdanov<sup>8</sup> and Bukharin. This explains why the critics of “*Kondrat'evshchina*” condemned virtually all of Kondratiev's statements containing the term “equilibrium”, including his statement on Marshall's theory of equilibrium (Targul'yan 1930, p. 9-10; Kurov 1930, 26; Shklovsky 1931, 94-95).

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<sup>8</sup>Alexander Bogdanov (1873–1928) was a Russian philosopher, science fiction writer and revolutionary.

## 6. *Kondratiev's Manuscript: Rethinking Discussions*

Kondratiev, as mentioned, worked in the field of social science methodology during the period of his imprisonment. His manuscript can aid our understanding of his theoretical sources and the influence of his teachers on the formation of his scientific worldview. Kondratiev challenged some ideas but elaborated on the others of a particular scientist and scientific school. This is true with regard to the influence of various currents of Positivism (1991, 135-136, 161, 194, 276, etc.), Marxism (*ibid.*, 14, 21, 29, 68, 78, 99-101, etc.), and Neo-Kantianism (*ibid.*, 95, 125-126, 133-135, 178, etc.). For example, Kondratiev, while accepting some of Marx's ideas, noted that there was no strict distinction between evaluative (economic-political) and theoretical judgments in his works, like most economics classics (*ibid.*, 272). For Kondratiev, this distinction was extremely important, although he said that science was created by people who were inclined to express value judgments, which constantly penetrated into science. However, Kondratiev insisted that "a scientist trying to justify norms and [...] values, in essence, wants to create a scientific morality, a scientific religion, a scientific art, etc." (*ibid.*, 260). In general, we can conclude that his methodology in the social sciences was extremely eclectic. For this purpose, the scientist proposed a social analytical approach, based on statistical methodology. Kondratiev's approach is fairly defined as probabilistic statistical (Abalkin 1992, 9).

Kondratiev's interest in epistemological issues did not arise accidentally in the last years of his life. Most likely, the discussions around his conception led to his realization that even a large amount of evidence in favor of a theory makes its statements very likely to be true, but still not firmly reliable, therefore it was not by chance that in his manuscript Kondratiev addressed the issue of truth criteria. In particular, he proposed testing the predictions of a theory (1991, 141), and in this respect Kondratiev noted that forecasting rests on the existence of regularity and a causal relationship between phenomena (*ibid.*, 522, 533). Kondratiev's research in this area is very important for understanding his view on the nature of long cycles.

Firstly, being influenced by Machism (*ibid.*, 160), Kondratiev recognized the existence of bilateral causal relationships. In the socio-economic reality a reverse effect was very often present: one phenomenon can be a consequence of another and vice versa (*ibid.*). In this regard, the logic of the interconnection is clarified between major economic cycles and the dynamics of wars and social conflicts. Thus, on the rising wave, “wars originate [...] in the heightened economic struggle for markets and raw materials, and that social shocks happen most easily under the pressure of new economic forces” (1935, 113). At a certain point, however, wars and social upheaval are (additional) reasons for increasing the cost of capital, and accordingly, for the phenomena of declining waves.

Secondly, Kondratiev denied the view that random events arise “as a result of the intersection of two or more causal series” (1991, 167). In this regard, according to Kondratiev, all events can be regarded as random, since “each of them arises from the intersection of several causal series” (*ibid.*, 169). This was the answer to the above criticism of Bogdanov (*Obsuzhdenie* 1989 [1928], 304). Preceding the definition of “random events”, Kondratiev addressed the interpretation of “regular events”. He noted that although every event is individual and unique, “many events are repeated, at least in their primary signs [...] Such events can be called regular” (1991, 171). According to Kondratiev, regular events, and not all irregular events, are not random. Among the latter there are such events, the causes of which can be established with a certain degree of precision. On the other hand, there is another group of irregular events “the causes of which cannot be determined on account of the current state of scientific knowledge and tools” (*ibid.*, 173). Kondratiev suggested naming them “random events”. With that background, it is clear why, after having established the rhythms of inventions, wars and revolutions, he concluded that the nature of these phenomena should not be viewed as random (1935, 115).

Thirdly, in his manuscript, Kondratiev substantiated his claim of the conditional and probabilistic nature of any regularity of society, which he considered an aggregate of a large number of people (1991, 53). Regularity is,

therefore, a consequence of numerous human acts. In other words, it “is a consequence the law of large numbers” (ibid., 220). Hence the probabilistic nature of the regularity of society. Kondratiev also emphasized that the law shows the connection of events with reference to only certain conditions. “But are these conditions provided for, [...] will they disappear, and when will they arise again? The formula of the law [...] cannot provide any answer for any of these questions” (ibid., 222).

This understanding of the nature of the law must be taken into account when verifying the conceptions of Kondratiev. As observed by Rummyantseva, modern critics try to “test the theory of long waves on the basis of [...] a recalculation of the indicators”, which demonstrated major cycles in Kondratiev’s works (2003, 9). In this regard, “the excuse to talk about the absence of long waves in the period of industrialization is the fact that the long wave pattern is not manifested everywhere” (ibid.). In fact, Kondratiev admitted that major cycles cannot be found in all of the indicators. In his famous article, he noted that he did not find major cycles “in French cotton consumption; in the wool and sugar production in the United States” (1935, 109). Unfortunately, Kondratiev did not cite sources or provide empirical data. However, it has been possible to identify previously unknown rough drafts, which reflected Kondratiev’s work with some statistical data. In particular, it was possible to find the time series of wool production in the USA in 1863–1902 (*RGAE, f. 769, op. 1, d. 8, l. 2*). Our analysis of the data exhibits only a pronounced 20-year cycle. However, such narrow time frames do not allow conclusions about long cycles to be drawn.

It is useful to recall Karl Popper’s words, the “criteria of refutation have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted” (2002 [1963], 49). Scientific theory, first and foremost, must imply the possibility of being disproven. This issue is much more difficult when we are dealing with probabilistic statements. However, Kondratiev did not mention any instance where he could recognize a refutation of his conception. If we accept that long cycles cannot be manifested everywhere and

at all times, then how, in principle, can we refute the conception (or rather the conceptions) of long cycles? From a logical point of view, Kondratiev's opponents not only need to reject the arguments, but also need to prove a counter-thesis. This task remains unresolved from a methodological point of view.

### 7. Concluding Comments

Social conditions have a significant influence on the development of long wave theory. We see that the controversy between Kondratiev and *Gosplan* about the economic development of the USSR had a noticeable effect on the formation of the negative evaluation of Kondratiev's method for identifying long cycles. Moreover, the priorities in the study of long waves were in fact determined by those circumstances that led to Kondratiev's scientific heritage remaining fragmentarily studied. Until now, at the center of attention has been questions of the statistical estimation of long waves, their theoretical and empirical substantiation. However, Kondratiev never turned to the analysis of the time series, which his opponents focused on. He did not recheck his calculations using spectral analysis, methodology which was developed at his institute (Weinstein 1925; Ignat'ev 1926). In the last years of his scientific activity Kondratiev turned to the consideration of epistemological questions (1926; 1991), which were directly or indirectly related to his conception of long cycles. Unfortunately, these questions remain on the periphery of modern research on long waves.

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