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# Kondratiev and a new methodological agenda for economics

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## Abstract

This article addresses Kondratiev’s approach to the problems of economic dynamics, cycle and conjuncture in the context of a new methodological agenda which was formulated in the 1920’s in Europe and the USA by representatives of the “brilliant generation of economists,” mostly members of the econometric movement and its adherents among Russian economists. A distinguishing feature of this generation was that its representatives were striving to make economics an objective science penetrated by rigorous ways of thinking and based on a unification between the theoretical quantitative and the empirical quantitative approaches to the study of economic phenomena. This paper discusses Kondratiev’s project on the general theory of economic dynamics as an embodiment of that methodological agenda. It also highlights a free exchange of ideas between Kondratiev and economists from different countries as a breeding ground for the emergence of the project and a necessary condition for its implementation.

*Keywords:* Kondratiev, economic methodology, economic dynamics, statistical approach.

*JEL classification:* B1, B2, B3, B4.

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## 1. Introduction

Nikolai Dmitrievich Kondratiev (1892–1938), Russia’s most internationally renowned economist, who gained worldwide recognition mainly for his idea of large cycles (major cycles, long waves) in economic development, belonged to the generation whom he described regretfully thus: “The heavy chariot of history has passed our generation by” (Kondratiev, 1998, vol. 4, p. 299). He meant those who had received education before the October revolution (and in many cases not only in Russia but also abroad<sup>1</sup>) and had started their professional activi-

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<sup>1</sup> For instance, L. N. Yurovskii, Kondratiev’s colleague and one of the authors of the monetary reform of 1922, studied in the University of Munich. A. V. Chayanov, one of the most famous Russian agricultural economists, having graduated from Peter the Great Agricultural Academy, as the best graduate was sent abroad in 1912.

ties in the pre-revolution years and during the revolution. They were forced to make a difficult choice between emigrating and living in Russia under the new political regime. Those who decided in favor of the latter had to decide whether to stay in the profession or leave it. In the early 1920s, a good many well-educated economists were ready to work actively and, almost inevitably, were involved in solving politico-economic problems and implementing the ambitious plans of the Bolsheviks—such as reconstructing the economy, developing the state-regulation system, and, finally, building the planned economy, the plans for which were on such a grand scale that many of them were enthralled.<sup>2</sup> Those economists who were involved in research (whose research could not always be detached from their practical activities) inevitably faced the need to find a compromise between adherence to strict scientific objectivity—which, as they saw it, was the precondition for successfully resolving practical issues, on the one hand, and loyalty to the authorities as the precondition for continuing research work, on the other hand.

However, notwithstanding these and other circumstances, including those connected with the emigration of many talented economists and destruction of the pre-revolution system of the research activities, the 1920s became the “Golden Age” of economic science in Russia—a fact recognized by the majority of those who study the history of Russian economic thought (Campbell, 2012, p. 189). Unfortunately, the “Golden Age” was swept away by the “Dark Middle Age” of the twentieth century, when even the implementation of scientific objectivity could arouse suspicion.

Russian economists of the 1920s faced similar circumstances and shared similar attributes. These included the diversity and complexity of their problems, their background competence in applying modern methods to economic analysis, including statistical and mathematical ones, and also their interest in the work of foreign colleagues. It was then that Russian economic science made the tangible leap forward to embrace what used to be known as modern economic science. And in that progressive movement, which, alas, lasted only for a short time, Kondratiev certainly was one of the leaders, both as a researcher and an organizer of scientific research—first and foremost, as the Director of the Conjuncture Institute, and also as an economist who manifested his skill in the work of several government organizations and agencies, and as a researcher who was integrated into world economic science.

## 2. The new methodological agenda: Origins and interpretation

A meaningful point is that changes taking place in Russian economics of the 1920s, and which were preconditioned to a considerable extent by practical tasks and foreshadowed by previous developments in Russian economic science, were taking place simultaneously and in parallel with changes initiated by the representatives of the “brilliant generation of economists” from different countries (Louçã, 2012, p. 1). First and foremost, we are referring

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<sup>2</sup> As for economists of the 1920s, see, for instance, Jasny (1972). We would refer to just some well-known names—such as A. V. Chayanov, L. N. Yurovskii, E. E. Slutsky, V. A. Bazarov, N. P. Makarov, N. N. Sukhanov, A. L. Vainshtein, et al.

to the econometric movement and its leaders—such as I. Fisher, R. Frisch, Ch. Roos, J. Schumpeter, F. Divisia and others, who were united by the idea of transforming economics into a rigorous science that in terms of objectivity would not be inferior to natural science disciplines and would reconcile the theoretical-quantitative and the empirical-quantitative approaches and unify mathematics, statistics and economic theory (Schumpeter, 1934). Kondratiev belonged to that movement—not so much formally as he was one of the first figures to whom the movement’s founding-fathers addressed in 1930 (Announcement, 1930),<sup>3</sup> and in 1933 he was one of the first few elected fellows of the Econometric society (Fisher, 1933)—but rather on account of his contribution to shared ideas of what economic science should look like, and because of what he had already done (or just what he planned to do), especially in the field of cycles, and economic dynamics in general.

In terms of its methodology, one of the goals of the econometric movement was to bridge the gap between theoretical and empirical approaches as well as between deductive and inductive methods. The problems inherent in such a methodological gap had been grasped by economists as early as in the era of J. S. Mill. It is worth noting that Mill had also posed the problem of the gap between statics and dynamics, but had not been able to advance towards a possible solution solving this.

The idea that statistical studies and pure theory are complementary can be found in W. S. Jevons’s “The theory of political economy”: “The deductive science of Economy must be verified and rendered useful by the purely empirical science of Statistics. Theory must be invested with reality and life of fact. But the difficulties of this union are immensely great” (Jevons, 1871/1879, p. 24). J. N. Keynes, too, called upon bridging the gap between the empirical and theoretical approaches: “If pure induction is inadequate, pure deduction is equally inadequate. It is a mistake that is too common, to set up these methods in mutual opposition, as if the employment of either of them excluded the employment of the other. It is, on the contrary, by their unprejudiced combination alone that any complete development of economic science is possible” (Keynes, 1890, p. 164).

The attempt to find a methodological compromise can be found in works by A. Marshall, who also expressed the opinion of most economists when they said that economic theory needed quantitative as well as qualitative analysis. The approximate date of the ending of the debate concerning the appropriate methods of economics may be set, therefore, in 1890 with the publication of Marshall’s “Principles of economics” and J. N. Keynes’s “Scope and method of political economy.” The same date, 1890, marks also the approximate beginning of an era of pronounced expansion of statistical activities (Persons, 1925, p. 180). Moreover, Marshall took a step towards the theory of dynamics when he

<sup>3</sup> We do not know whether Kondratiev received the written invitation to attend a founding meeting of the Econometric society, which was sent to him in November of 1930, when he was already in prison. Whatever the case, there is no evidence of his actual response thereto. However, it is known (Bjerkholt, 2017) that Slutsky declined the invitation of June 1930 by Frisch, Roos, and Fisher, containing the draft list of the invitees to join a founding meeting, which included names of Kondratiev and Slutsky. Slutsky explained that by the fact that his studies were focused more on mathematics rather than economics. Bjerkholt notes correctly that it is quite possible that Slutsky did so under the impression of arrests of his colleagues. In fact, Slutsky could have received this letter, which was sent him not earlier than on June 15, right after Kondratiev’s arrest (June 19).

introduced the time dimension into theory by considering market-period, short-period, and long-period equilibriums and “opened the road” for the facts, albeit at the expense of the reduced degree of generality. For this reason, Marshall’s theory is often referred to as synthetic (Avtonomov and Avtonomov, 2016), but it would hardly be correct to say that Marshall *did* resolve the problem of the methodological gap or put forward the theory of economic dynamics.

By the early 1920s, the old methodological dispute of the 19<sup>th</sup> century—the *Methodenstreit*—was consigned to the past. In lieu of the *true method* issue, some economists, especially those engaged in statistical studies, raised the question of combining various methods and research techniques. For instance, W. Mitchell wrote: “We do not speak of qualitative versus quantitative analysis. We do not seek to prove even that one type should predominate over the other. Instead of dogmatizing about the method at large, we are experimenting with methods in detail. In the measure of our proficiencies, we all practice both qualitative and quantitative analysis, shifting our emphasis according to the task we have in hand” (Mitchell, 1925, p. 1).

Many economists connected the hope for reconciliation between the theoretical and empirical approaches with advanced statistical methods, the application in economic studies of the correlation and regression analysis which earlier were being refined and applied actively in research of non-economic phenomena and processes—first and foremost, biological and demographic ones (K. Pearson, G. U. Yule, et al.). Moreover, by that time application of such methods began to be considered—especially by representatives of statistical science—as evidence of the scientific status of any discipline (Fisher, 1925), and some economists even started to appraise the state of economic science from such a point of view and to regard its evolution as the statistics-oriented movement, qualified by them as a great trend (Stigler, 1962; Ise, 1932).

In the 1920s, the idea that economics—just like any other discipline—must be logically strict and based on a solid empirical basis, found a considerable number of adherents in Russia. Prior to the revolution, Russia had some of the most internationally renowned schools of statistics, both descriptive (practical) and theoretical. The latter one, represented by such a prominent and internationally renowned scientist as A. A. Chuprov,<sup>4</sup> was closely connected with the Russian mathematical school (P. L. Chebyshev, A. A. Markov, A. M. Lyapunov). In the 1920s, when the role of statistics grew tangibly in the light of new politico-economic goals, Russian economists relied on this to underpin their work. They could also rely on the work of Russian economists who at the very beginning of the 20<sup>th</sup> century initiated the application of mathematics in economics—V. K. Dmitriev, M. I. Tugan-Baranovsky, N. A. Stolyarov, and on the work of those who somewhat later followed this path—A. V. Chayanov, E. E. Slutsky, N. N. Shaposhnikov, and some others.<sup>5</sup> However, the process of adoption of mathematics as the language of economics was neither easy nor straightforward.

<sup>4</sup> A. A. Chuprov (1874–1926), an honorary fellow of the Royal Statistical Society, a member of the International Statistical Institute, the Royal Economic Society, etc. left Russia just after the Revolution. He published his writings both in Russia and in other countries and was in correspondence and had personal contacts with many famous economists, statisticians and mathematicians, including J. M. Keynes, F. Edgeworth, L. Bortkewicz, K. Pearson, R. Fisher, O. Anderson, V. Romanovsky, N. S. Chetverikov.

<sup>5</sup> For a detailed history of mathematical economics in Russia see Belykh (2017).

The contributions of the first generation marginalists had passed almost unnoticed by Russian economists, while representatives of the second generation (mainly the adherents of the Austrian school) were seen first and foremost as opponents of Marx's labor theory of value and therefore their basic ideas and methods were rejected by most Russian economists who had fallen under the influence of Marx's doctrine.<sup>6</sup> And the work of Dmitriev, who adopted marginalism and used mathematics to conduct an organic synthesis of the labor theory of value and the theory of marginal utility and obtained some original results, as well as the works of Stolyarov who proved the theorem formulated by Tugan-Baranovsky on the proportionality of marginal utilities to labor values, and by some other authors,<sup>7</sup> did not noticeably advance the process of mathematization. Most Russian economists remained committed to the non-rigorous way of thinking and non-mathematical methods of reasoning.

By the early 1920s, not only in Russia, but also in the West, economics was veering far from the standards of rigor and objectivity established by natural sciences. This might seem strange, because it could be expected that after the marginalist revolution the phase of the "narrative" economics was over and a resolute step made towards its quantification and mathematization. But this did not happen. In the early twentieth century it became clear that hopes that had been nourished in the last three decades of the nineteenth century, to make economics the "exact science," "social mechanics," "physique sociale," or "mechanics of utility" (Moore, 1914, p. 84–85) had faded away. Indeed, not only in the USA, where the historical school and institutionalism dominated absolutely (Crum, 1925), but also in Europe, where the trend towards the mathematization of economics was deeply rooted, the application of mathematical methods was viewed with considerable scepticism. Even those economists who had a good command of mathematics, such as Marshall, Wicksell, or Pigou, tried to dispense with the minimal number of formal instruments and mathematical symbols (Niehans, 1990, p. 159–163).<sup>8</sup>

Such a situation could have been explained by the fact that the audience lacked proper training in mathematics, or that universities had been without departments of economics for quite some time and as a rule, chairs of political economy were established in faculties of law. However, it is evident that there was a problem with marginalist economic theory as such, as its original hypotheses were too abstract, the basic notions were non-quantifiable, and the applied equilibrium approach represented "a disguised form of the classical form of *ceteris paribus*, the method of static state" (Moore, 1914, p. 86). Moore also criticized Marshall's method for being "limited to functions of one variable" (Moore, 1929, p. 93). It is therefore not surprising that applying this theory to analyze the rapidly changing economic reality sowed strong doubts among many economists.

Demand for the theory which could deal with economic change was satisfied partly by the historical school and institutionalism, which did not make claims

<sup>6</sup> On the perception of Austrian school by Russian economists see Avtonomov and Makasheva (2018).

<sup>7</sup> On value-price problem debates in Russia see Allisson (2015).

<sup>8</sup> F. Mirowski, having analyzed publications in the *Revue d'économie politique*, *Quarterly Journal of Economics*, *Journal of Political Economy*, and *Economic Journal*, draw the conclusion that in the period from 1887 through to 1924, "Journals rarely devote more than 5 percent of their pages to mathematical discourse, and in no journal does the proportion of mathematical pages venture beyond one standard deviation of zero" (Mirowski, 1991, p. 150).

for creating a general and strict theory, and partly by the economic-cycle studies, which, however, “have never been integrated in the body of the deductive theory” (Kuznets, 1930a, p. 427). Some economists—for instance, J. S. Mill, J. B. Clark and others—put forward “the postulate” of a theory of dynamics, but this remained “postulate” (Grossman, 1941/1977, p. 69). It is worth noting that adherence to statics and an equilibrium approach was not the result of an ideological commitment to the idea of an invisible hand or free competition, but rather a recognition of the complexity of a dynamic approach in terms of mathematics.

So, although economic science in Russia and the West, especially in Europe, developed along different trajectories, conditioned by specific features of the economic development of countries and national schools traditions in economic thought, by the 1920s there appeared to be a shared demand for a new methodological agenda. Such an agenda was not supposed merely to assert an accepted correct method as sought by participants of the famous *Methodenstreit*, who contraposed the empiric and theoretical methods, but recognized the possibility and need to combine different methods as well as suggested the alliance among mathematics, statistics, and economic theory.

In the 1920s, a notable numbers of Russian economists connected the application of statistical and mathematical methods with the opportunity to make economics an objective and useful discipline and to depart from mixing—what was traditional for Russian economic thought—the analytical and social components of economic discourse. Kondratiev certainly shared the aforementioned point of view. He realized the need to have a new methodological program, although its outline was probably not entirely clear to him. All his activities provided evidence of his striving for economics as a true scientific discipline, framing the “battles” between proponents of different schools in the fields of mathematics, statistics and logic, rather than politics and ideology. We may say that he proceeded to implement such a methodological program through his (mostly empirical) studies of cycles and conjuncture.

Kondratiev’s name is associated in the West, and now in Russia, first and foremost with the concept of “long cycles” (major cycles, long waves).<sup>9</sup> In the 1920s, just a few of Kondratiev’s works were available for Western researchers: in 1926, the German translation of “The major economic cycles” (1925); in 1925, the *Quarterly Journal of Economics* published the abridged translation of “The static and dynamic view of economics” (1924); in 1927, the partial version of the *Problems of Forecasting* (1926) was published in German, and in 1928, the abridged German version of the “Dynamics of industrial and agricultural prices” (1928) was published—the last publication during Kondratiev’s lifetime. This led Louçã to write that “the impact of Kondratiev’s few articles published in English and German was not only effective, but also quite surprising” (Louçã 1999, p. 192), and, one can add, extending beyond the boundaries of economic research (see, for example, Chapin, 1925).

<sup>9</sup> In the Soviet period the situation was different: Kondratiev, if he was ever mentioned, was referred to first and foremost as an agrarian economist, alien to Marxism, a neo-populist, and critic of the industrialization policy (see, e. g., Figurovskaya, 1975). The idea of major cycles was not perceived so acutely, although it was addressed in the political context. It was only in the late 1980s–early 1990s that the first works started to appear, in which various facets of Kondratiev’s heritage were discussed and which were free from ideological bias (Piyasheva, 1988; Belyanova and Komlev, 1989; Makasheva, 1989; Abalkin, 1992).

The first two of the aforementioned works contained “the hypothesis of the long waves in capitalist development—named by Schumpeter and known thereafter as ‘Kondratiev waves’—that for some time was an important topic in the research agenda of economics” (Louçã 1999, p. 169), and today is a part of the scientific economic discourse (Campbell 2012, p. 189). It should be emphasized that Kondratiev did not pretend to build the major-cycles *theory*. He just set forth the hypothesis, and moreover, as evidenced by his letters from Suzdal political prison, he saw the theory of major cycles, as well as that of business cycles, only as elements of the general theory of dynamics; its development was seen by him as a task of paramount importance (Kondratiev, 2004). Meanwhile, he certainly understood that the study of cycles had its own significance as well.<sup>10</sup>

### 3. The methodological agenda and the problem of cycle

The choice, made by Kondratiev regarding the subject of research,<sup>11</sup> as well as the selection of his works to be published abroad, reflect the general trend in the economic studies of that period—the intense interest of economists in the problem of cycles, which had stirred as early as before World War I. In the pre-war years, the two lines of research had been outlined—the empirical and the theoretical ones; the gap between those became most evident in the 1920s.

Those who may be referred to as adherents of the theoretical stream include L. Mises and F. Hayek, J. Schumpeter, D. Robertson, A. Pigou, R. Hawtrey, G. Myrdal, and others. These economists saw the cycle theory first and foremost as an abstract deductive theory. In such case, the most difficult problem was how to incorporate the phenomenon of cycle into the general theory of economic equilibrium because the former, according to a German economist A. Löwe, were in “obvious contradiction” with the latter (Hayek, 1933, p. 33). That contradiction was manifest in the fact that the equilibrium approach happened to be productive only in cases when the external factors were considered as the cause of cycle.

Hayek, who was one of the most consistent adherents of the deductive method and equilibrium approach in the study of cycle, denied the need and possibility of statistical verification of the theory as built by the deductive method from the hypotheses of rationality. In particular, he wrote: “A priori we cannot expect from statistics anything more than the stimulus provided by the indication of new problems” (Hayek, 1933, p. 31). “Statistics can never prove or disprove a theoretical explanation, they can only present problems or offer fields for theoretical research” (Hayek, 1933, p. 232). At the same time, as early as the 1940s, Hayek, still an adherent of the equilibrium approach, criticized the equilibrium models of neoclassical economics for determinism and static approach. He also demanded to revise the meaning of some its basic concepts—for instance, competition, and to reject the neoclassical equilibrium models as consonant with the idea of a centrally planned economy, the latter being a great fallacy (Hayek, 1948).

<sup>10</sup> At the same time, it is necessary to take into account that Kondratiev’s activities were not limited by his research of economic cycles, he also took part in the discussion on the methodology and principles of planning and forecasting, current economic problems, paid special attention to the condition and development of agriculture, etc.

<sup>11</sup> The choice was made in 1922 by publication of the “World economy and its conjuncture during and after the war” (Kondratieff, 2004).



Kondratiev turned to the methodological issues connected with the problem of dynamics in 1924 in his article “The concepts of economic statics and dynamics.” He presented the critical analysis of then existing ideas of the relation between statics and dynamics, and outlined his immediate task as conducting the conjuncture research, having described the method of the research as “concrete empirical” and, in particular, *statistical* (Kondratiev, 1998, vol. 1, p. 23).<sup>12</sup> A noteworthy point is that, as early as 1922, Kondratiev referred to W. Mitchell, A. Aftalion, J. Lescure and others as representatives of the empirical approach in business cycle research (Kondratieff, 2004), which started to develop very actively in the 1920s (Andvig, 1981, p. 699). Actively engaged in empirical research, Kondratiev did not remove from the agenda the task of elaborating the theory of dynamics, which would be quantified and verified, i.e. “turned to facts.” We can say that from the very beginning Kondratiev’s research program was quite in tune with that of the econometric movement.<sup>13</sup>

While studying business cycles, the adherents of the empirical stream (Mitchell, 1913; Moore, 1914; Fisher, 1911) not only recognized the importance of statistics, but offered the special articulation of its purpose and meaning. The latter was not so much to verify theory as “to establish more precisely the facts concerning cyclical fluctuations in particular economic processes [...] Statistical analysis affords the surest means of determining the relation among, and the relative importance of, the numerous factors stressed by business-cycle theories. In turn, rational hypotheses are the best guides of statistical research, and theoretical significance is the ultimate test of statistical results” (Mitchell, 1927, p. 189–190).

So, while Hayek believed that statistics would point out the phenomena to be theoretically studied, but would not evaluate theory, Mitchell, the leader of the empirical stream, assumed that theory would confirm results which were previously statistically obtained. In relation to the study of cycles and crises, this was formulated by H. Moore: “The development of the theory of crises illustrated the attempt to establish deductively results which have at first been reached empirically” (Moore, 1908, p. 31).

In mid 1920s, Kondratiev focused his efforts mainly on the statistical analysis of conjuncture and was interested in the work of foreign economists in this field, and above all in the work of Mitchell, the head of the NBER (founded in the same year—1920, as the Conjuncture Institute was established). The conformity of the two institutes’ research agendas and the similarity of Mitchell’s and Kondratiev’s approaches to the study of cycle predetermined the two scholars’ interest in one another, which manifested itself at personal meetings in the USA during Kondratiev’s business trip. As noted by Barnett, impressed by his meetings with Kondratiev, Mitchell agreed to publish his article in the *Voprosy Koniunktury*

<sup>12</sup> Started prior to the World War I, the rapid growth in the studies of cycles and crises continued in the 1920s and received additional impetus in the 1930s. Regrettably, after 1930 Kondratiev was deprived of an opportunity to follow new publications to a proper extent. For instance, he did not make references to Hayek’s publications at all, and only mentioned L. Mises and G. Myrdal in his letters from Suzdal (Kondratiev, 2004). Meanwhile, he “had time” to make a reference to Pigou’s “Industrial fluctuations” (1927), Cassel’s “Theoretische Sozialökonomie” (1921), Schumpeter’s “Theorie der wirtschaftlichen Entwicklung” (1911) in his last published work “Dynamics of industrial and agricultural prices” (1928).

<sup>13</sup> There is a large body of literature on econometrics and its history. Let us mention just a few: Qin (1997), Spanos (2006), Pesaran (2004), Louçã (2012), Morgan (1990).

[*Issues of Conjuncture*] journal published by the Conjuncture Institute, and mentioned major cycles in his book “Business cycles,” on which he was working at that time, although he considered Kondratiev’s hypothesis of major cycles to be quite dubious (Barnett, 1998, pp. 94–96).

While in the USA, Kondratiev also had meetings with S. Kuznets, then a young colleague of Mitchell, and maintained contacts with him afterwards. Kondratiev participated in the annual meeting of the American Economic Association in Chicago on December 30, 1924, attended by leading American economists, and “gave a summary [...] of the collapse and subsequent stabilization of the Russian currency” (AEA, 1925, p. 84); finally, he was admitted to membership of the Association. In general, we may say that, owing to his personal contacts, Kondratiev was able not only to attract American economists to the idea of major cycles and to works by Russian economists, but also to envision possible cooperation between American and Russian researchers.

#### 4. A difficult road to the “methodological alliance”

The new methodological agenda, which promulgated the reconciliation between the theoretical and empirical approaches, the alliance of mathematics, statistics and economic theory, as well as the movement towards the theory of economic dynamics, raised such questions as to what kind of deductive theory could be seen as the embodiment of this “alliance.” The attempt to adapt pure theory to requirements of the methodological agenda was made by Moore in “Synthetic economics,” which, as he wrote, “comprises both the rational and empirical branches of economic science” (Moore, 1929, p. 151). Moore developed the general equilibrium model, which was presented by the system of simultaneous equations of demand and supply, with the variables—unlike those of the Walrasian model, depending on time and representing deviations from empirically defined trend values. The form of functions was defined by the statistically drawn price elasticity of demand and supply. Moore’s system of equations described the sequence of the economy equilibrium states in relation to the trend.

In the review of Moore’s book, M. Ezekiel wrote that more than any other American economists, Moore contributed to the introduction of statistical methods into economics and to the connection between economic theory and facts of economic reality (Ezekiel, 1930, p. 663), but that Moore did not finally solve the problem of reconciling theoretical and empirical approaches, nor that of building the basis of dynamic theory. Certainly, the fact that this model did not imply that demand functions were directly derived from the utility optimization, made them less connected with the exogenous characteristics of individual behavior. But this was attained by means of rather arbitrary assumptions of the price elasticity: the constant, the linear, or quadratic functions of price, which substantially limited the analysis of the market interaction process. Besides, the dynamic features of the model variables were expressed through the relation to the empirically defined trend, the latter being theoretically unexplainable (Ezekiel, 1930, p. 678). With this, neither the equilibrium approach nor the principle of methodological individualism had doubts cast on them, while exactly they, in the view of Kuznets, prevented from building the theory of dy-

namics: “as long as economics will remain a strictly unified system based upon the concept of equilibrium, and continue to reduce the social phenomenon to units of rigidly defined individual behavior, its analytic part will remain of little use to any system of dynamic economics” (Kuznets, 1930a, p. 435). Kondratiev, who was familiar with Moore’s work, also did not see it as containing a solution to the problem. He stood closer to Kuznets’ position—at least, to his view on the trend as a theoretical problem.

Kuznets (1930b) started his modification of the pure theory from denial of the methodological individualism principle and suggested addressing the market demand and supply functions. He also discarded the important premises, which are not put explicitly, of the equal and high rates of reaction of variables to external disturbances, while these premises ultimately make it possible to restore the equilibrium (see, for instance, Rosenstein-Rodan, 1934). Having discarded these premises, Kuznets admitted that the adjustment process was elongated in time and that therefore the irreversible process of interaction might begin, which will be influenced and overlapped by new disturbances. Under some conditions random shocks to economy may result, as shown by Slutsky (1927), in apparently cyclic process, while the overlapping processes might produce the moving trend (Kuznets, 1930b, p. 409–410).

We do not know whether Kondratiev was familiar with this and another relevant article of Kuznets’s which was published the same year, but it is known that he was well aware of the “Secular movements in production and prices” (Kuznets, 1930c) and even the two later published works (Kuznets, 1933, 1935). We would note that in the “Secular movements,” referred to by R. Hawtrey as “product of latter-day empirism” (Hawtrey, 1931, p. 586), Kuznets found that “the progress of any industry over a long period (several generations)” was described by the logistic curve. The analogous curve was mathematically obtained by Kondratiev as reflecting the law governing the trends of the capital and population in the model of economic dynamics, built by him in 1934. Unfortunately, we can make a judgment on this model only by its concise reproduction in his letter to his wife dated September 5, 1934.

We are not aware to what extent Kondratiev relied on Kuznets’ work, but in any case we may say that Kondratiev and Kuznets shared the interest in the problem of trend. The issue of trend was raised by Kondratiev in the course of discussion on the major-cycles problem, and at that time Kondratiev used to determine the shape of the curve and its characteristics empirically. While in Suzdal political prison, he theoretically derived trend and used it in a model of growing economy, parameters of which are to be determined empirically. Thereby, Kondratiev made a noticeable step towards reconciling theoretical and empirical approaches and building the theory of economic dynamics. Most of his works written between 1921 and 1928 may be considered as the preliminary stage of building such theory.

## **5. The general theory of dynamics in the context of the new methodological agenda**

One can make assumptions about the logical structure of the general theory of dynamics as well as Kondratiev’s view of true economic science by relying on

the quite limited number of materials, such as: the articles on statics and dynamics published before he was arrested; the unfinished (rather, interrupted) book “Basic problems of economic statics and dynamics,” written in 1930–1931;<sup>14</sup> the macroeconomic model as drawn from his book on the trend, unfortunately lost,<sup>15</sup> and letters to his wife, written from the Suzdal political prison in 1932—1936.<sup>16</sup> These letters serve as evidence of his intensive research done in prison at least until 1936, and of his striving to be aware of the new publications issued in Russia and abroad, as well as of his correspondence with leading foreign economists—such as Kuznets, Mitchell, I. Fisher, and E. Wagemann.<sup>17</sup>

In these letters, Kondratiev mentioned over 270 books, articles and booklets, to which he referred by memory, as well as those ones that he asked to send to him. These works can be divided into several groups: statistical studies of the long-term trends in the dynamics of prices, national income and wealth (W. I. King, G. M. Malhall, R. Pupin), capital (R. Giffen), population and employment (Moore, P. E. Levasseur),<sup>18</sup> works on the theory of cycles and crises (Mitchell, J. B. Clark, A. F. Burns, C. T. Schmidt, A. Spiethoff, Fisher, et al.) as well as on the problems of statics and dynamics (Moore, Mitchell, E. H. Vogel, Kuznets, et al.); works by the leaders of marginalism (L. Walras, W. S. Jevons, R. Auspitz, K. Wicksell, et al.); books and textbooks on mathematics, statistics, and the theory of probability by Russian and foreign authors.

As for the plan regarding the general theory of dynamics, we come to know it from the letter to his wife of November 7, 1934: “As soon as I have finished this book, I shall start a book on large fluctuations, whose plan and contents are already completely clear to me. Then I shall write a book on short cycles and crises. After that I shall return to the introductory general methodological part which I handed over to you in draft. Finally, I shall finish everything with the fifth book on the synthetic theory of socio-economic genetics or development” (Kondratiev, 1998, vol. 4, p. 304).

The first book mentioned in the above fragment is the work on the trend, which Kondratiev was writing in Suzdal—first, periodically and in parallel with studying mathematics and statistics, and then systematically. The same letter informs us about the titles of the first four chapters in the given book: Chapter I.

<sup>14</sup> Usually this work is referred to as the *Butyrskaya rukopis* (manuscript) (see, for instance, Klyukin, 2011), although it is difficult to say whether all the text, albeit unfinished, was written in the Butyrskaya prison, where Kondratiev was from April of 1931 through to February of 1932, or whether a part of it was written when he was kept in Lubyanka (from the arrest date through to April of 1931), where he also was doing his research work (Kondratiev, 2004, p. 719).

<sup>15</sup> Letter of September 5, 1934 (Kondratiev, 2004, p. 405–408).

<sup>16</sup> Letters to various addressees, written in the earlier period (from 1916 through to 1924), letters to his wife written in 1937 and 1938—in the period, when Kondratiev stopped doing research because of his health condition and the more severe imprisonment conditions, as well as various materials connected with Kondratiev’s relations with the authorities, including the materials regarding the supposed expulsion of Kondratiev from the USSR in 1922, his calls to various officials in the period of imprisonment, verdicts, etc.—all these are of great historical interest as evidence of that epoch.

<sup>17</sup> We do not know to what extent the foreign scholars being addressed by Kondratiev’s wife were aware of his situation. The fact that in December 1930 he became a member of the Econometric Society and in 1933—a fellow, can be interpreted in two ways: either foreign economists were not aware of his imprisonment, or they were aware of it, but wanted to support him by such honors. In our view, the latter is more probable.

<sup>18</sup> For instance, in the letter of February 9, 1933 Kondratiev asked his wife to address Mitchell with the request to send several volumes from the 13-volume series of books on trend issued under Mitchell’s leadership.

“The basic problems of the theory of economic dynamics”; Chapter II. “Trend, or the problem of the theory of economic dynamics”; Chapter III. “The state of study of trend in the theory of socio-economics”; Chapter IV. “Stochastic analysis of a time series and the problem of trend.”

In the letter of May 29, 1935 Kondratiev writes that the book is not yet finished and that he has to write the chapter “Abstract theory of trend,” and then 2 or 3 chapters of the empirical content, as well as that he is continuing to work on the chapter on the “Stochastic analysis of a time series and determination of the form of the trend” “devoted to a theoretical, probabilistic substantiation of techniques for establishing the trend from empirical data after the general form of the trend has been deduced” (Kondratiev, 1998, vol. 4, p. 309). The latter part of the phrase is related to the work, the concise result of which was the model as described in the letter of September 5, 1934.

Describing the plan for the general theory of dynamics, Kondratiev writes that the last, fifth book will be focused on the *synthetic* theory of socio-economic *genetics* or development. Here, the two terms are not quite clear—such as “synthetic” and “genetics,” the meaning of which may be only a subject for speculation. It seems most probable that Kondratiev, like Moore, understood the “synthetic theory” as the theory containing propositions drawn by deductive reasoning and subjected to verification, or drawn empirically but explained theoretically. As far as the term “genetics” is concerned, it might signify the endogenous nature of socio-economic change under consideration. However, in order to reach this final stage of the project, it was necessary to resolve a good deal of various problems, ranging from those of philosophy and methodology to the statistical and mathematical ones.

Like many of his colleagues in the West, Kondratiev preferred to start building the theory of dynamics with statics as the theory of equilibrium. This allows us to assume that Kondratiev did accept the analogy between economics and mechanics, but was well aware of the limitations of statics. He wrote that “in studying equilibrium, statics cannot and does not study the class of economic phenomena whose economic essence amounts to rejection of equilibrium or a violation of it or what are a consequence of the absence of equilibrium. These include, for example, the phenomena of crises, industrial profits, etc.” (Kondratiev 1998, vol. 2, p. 229). However, like some other economists, he admitted that within the framework of the static theory it would be possible to *identify* the problems, which are beyond the scope of equilibrium analysis. Otherwise, it would not be possible to start building the theory of dynamics from statics.

Kondratiev realized the need for “reconciliation” between the static and dynamic theories: “The concepts of statics and dynamics can only supplement one another if they relate to the same object of cognition, i.e. they form part of the same science and, consequently, are either both general or both particular concepts” (Kondratiev, 1998, vol. 2, p. 199). Thus the general theory must have the same degree of generality as the theory of statics and not be connected with specific events and facts.

So, how could it be made possible to provide the generality of notions related to the theory of dynamics? For the adherents of methodological individualism, the answer is clear: it is necessary to turn to the behavior of individuals and to start building the theory of dynamics on this basis. However, the theories based on methodological individualism were static. Kondratiev associated the exit from

such deadlock with the statistic-probabilistic approach to the analysis of social phenomena in general and economic ones in particular.

According to Kondratiev, the basic concept of the economic theory is the market system, the elements of which are demand, supply and prices, which are interconnected, their interconnection being expressed by the law asserting the “functional-causal dependence” between these elements under certain conditions. This law represents the generalization of the empirical data in the abstract form, while the cause, which preconditioned the existence of the dependence, is to be found in the mass phenomena, such as the changes in the subjective valuations of commodities<sup>19</sup> and activities of the great numbers of individuals who constitute a population. This can be seen as the first “brick” of the new theory. The second brick appears in the form of a claim that the equilibrium price is the statistical characteristic of the set of prices, at which transactions are carried out.

Meanwhile, market agents do not possess the perfect knowledge (as opposed to Walrasian model), the number of transactions is large, and the latter may be referred to as stochastic events. In such a case, the equilibrium price “is most closely characterized by its approximation to empirical mode, i.e., the price which occurs most frequently” (Kondratiev, 1998, vol. 2, p. 376). Certainly, all these discourses are related to a particular market and to the static equilibrium.<sup>20</sup> Regrettably, Kondratiev did not resolve, and even did not raise many questions, which arise inevitably in discussion of the market-price mechanisms under the assumption that individuals do not have perfect knowledge of the market state. Such questions may include: so called “decision dualism” (several decades after Kondratiev this problem was discussed within the non-Walrasian equilibrium approach); the impact of the rare but significant deviations which entails cumulative effects; and, finally, the different velocities of the adjustment processes (addressed by Kuznets), etc. The last, but by no means least, question is how, in the process of the equilibrium price setting and/or as a result of interaction among such processes, which take place in different markets, the forces may appear, which cause the cyclical deviations from the trend. These questions, like many other ones, remained unanswered.

The manuscript was given to Kondratiev’s wife, and he never returned to it after that.<sup>21</sup> We may just make assumptions as to why Kondratiev decided to set aside the methodological part and attend to the macroeconomic dynamics and especially to the problem of trend. Probably, this happened under the influence of the growing interest, in the West, in the problems of economic dynamics in general and that of trend in particular; the latter has been fueled not least by applying the mathematical and statistical methods in economic theory.

It took over two years for Kondratiev to resolve theoretically the problem of trend. In March 1934, he quite proudly writes that he has arrived at rather unexpected and quite pessimistic conclusions related to the regularity of economic development, and that these conclusions, when published, may cause the “even stronger assault” than

<sup>19</sup> While recognizing the significance of the marginal utility, Kondratiev considers it as insufficient for explanation of the price as a social phenomenon.

<sup>20</sup> In this case, Kondratiev makes a direct reference to Marshall (Kondratiev, 1998, vol. 2, p. 261).

<sup>21</sup> We do not know for sure, when and where this manuscript was handed over to his wife: prior to Kondratiev’s dispatch to Suzdal in February 1932, or earlier—for instance, before his relocation from the Interior Prison (Lubyanka) to Butyrskaya prison in April 1931.

his other works did (Kondratiev, 2004, p. 328).<sup>22</sup> Those results made it possible to build the small-size macroeconomic model of the national economy which defined trend values of the most important economic variables, while the model as such may be applied for forecasting the long-term dynamics.<sup>23</sup>

This model was innovative in many aspects, and according to some experts, its creation forestalled the appearance of similar models in the West at least for 20 years; in particular, he applied the Cobb-Douglas production function with Hicks-neutral technical progress (Belyanova and Komlev, 1989, p. 33–35). It is most probable that Kondratiev arrived at the function independently of both C. Cobb and P. Douglas, as well as J. Hicks, whose names he did not mention at all—unlike the works by Wicksell, in which the idea of the production function was present but not developed.

At the next stage, according to Kondratiev's plans, it was necessary to undertake the stochastic analysis of the time series as being related to the trend. It appears most probable that in other works, too, which would have been focused on cycles, he planned to follow the same logic: the theoretical deductive method had to be combined with the statistical, or statistical-probabilistic approach. Within the framework of that approach, the crucial notion was that of *population*, which was the major concept in A. A. Chuprov's<sup>24</sup> version of the "theoretical ground of statistics," which deeply impressed Kondratiev (Davydov, 1991, p. 456). Application of the concept of population to the phenomena of social life and, above all, to economics, changed the role of statistics and the theory of probability: they became not only and not so much tools for applied analysis, but means for understanding the very essence of real phenomena, including such a basic phenomenon as the market price. More than that, owing to the statistical-probabilistic approach, the logical connection was set (certainly, to some extent) between the problem-focused studies of the entire economy and the analysis of the price as a result of market interaction between economic agents constituting a population. As a result, we can get some idea of the logical structure of the whole project, although we may only try to make guesses on the lines and the logic of his reasoning.

Kondratiev certainly attempted to apply formal mathematical and up-to-date statistical methods in economic research. His efforts corresponded to his perception of economic science as being close in terms of the methods and tools to natural science disciplines dealing with measurable values and verifying theories empirically, even in terms of the forecast reliability. We may say that Kondratiev was working according to the logic of the methodological agenda—i.e., the program, which was promulgated by the "brilliant generation" of economists, mathematicians, and statisticians in the West. At the same time, his vision of modern economic science was probably somewhat different from what it became in the end. In some sense, we may say that a pioneering and promising approach passed away together with Kondratiev.

<sup>22</sup> We may just suggest that he meant that the system of equations described dynamic variation of cumulative variables in the economic system (such as capital and/or population).

<sup>23</sup> The short version of this model was presented in the letter of September 5, 1934. (Kondratiev, 2004, pp. 405–410).

<sup>24</sup> Kondratiev makes references not only to works by Chuprov (first and foremost to his "Ocherki po istorii statistiki" ["Essays on the history of statistics"]. St. Petersburg, 1910), but also to the work by E. Romanovsky, K. Pearson, V. Bortkiewicz, P. L. Chebyshev, S. N. Bershtein, and other scientists, who worked in the theory of probability and statistics.

## 6. Conclusion

M. Blaug wrote: “The development of economic thought has not taken the form of a linear progression toward present truths while it has progressed, many have been the detours imposed by the exigencies of time and place” (Blaug, 1990, p. 7). Mentioning the exigencies, Blaug most probably did not mean the political repressions or extermination of scientists, and mentioning the detours, he hardly meant the purpose-oriented destruction of the achievements that were previously accumulated by the national science. The situation was made especially dramatic in Russia ever since the early 1930s when the victims of the struggle against free-thinking were not only people, but economic science—national economics was foredoomed to subordination to ideology and politics, closeness and autarchy, and economic science in general suffered losses in the diversity of ideas and approaches. At the same time, as shown by Kondratiev’s case in point, the scientist’s thought can remain free even when he is not free physically. It would not be an overstatement to say that even while imprisoned, Kondratiev continued to belong to the global academic community of economists, and that the unique page in the history of a fruitful mutual exchange of economic ideas between Russia and the West is associated with the name of Kondratiev.

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