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When History Invades Science: To the 130th Anniversary of N. D. Kondratieff's Birth

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Abstract

The paper analyzes N. D. Kondratieff's approach to the construction of a general theory of dynamics, presented in his unfinished work 'Basic Problems of Economic Statics and Dynamics', written in prison. The approach proposed by Kondratieff is considered in the context of the discussion of this problem in the West, including in connection with the formulation of the most important methodological problems of economic science of the respective period.

Keywords: *N. D. Kondratieff, economic dynamics, statics, statistical and probabilistic approach, methodology of economic science.*

The jubilee of any scientist is always a cause to analyze his ideas and their significance in the history of science, and finally to remember him. When we are talking about a scientist belonging to a generation which, in his own words, fell under the chariot of history, the interference of historical processes takes on a tragic character. It is very symbolic that we celebrated the anniversary of the birth of the outstanding Russian economist N. D. Kondratieff in the year of the centenary of the two Russian revolutions (in 2017), which undoubtedly influenced world history and changed the fate of the country and the destinies of people, personal and professional. There is nothing surprising in this fact. Any revolution affects the destiny of a country and its citizens. New in this case is that the October Revolution and the following events have a major impact on the development of social science as a whole and especially economic science.

If we continue our chronological excursion, we will notice some more symbolic coincidences. It is 90 years since the sentencing of a group of economists in the case of the Peasants Labour Party – one of the most notorious trials of the early 1930s. And it is also 35 years since the rehabilitation of N. D. Kon-

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dratieff and his colleagues in this case, which took place on the wave of democratization in the late 1980s. The year 2023 marks 85 years since the scientist's execution and 60 years since his 'thawed' rehabilitation by the 1932 verdict. Indeed, not only the life, but also the death of the scientist is amazingly woven into the history of our country.

The personal fate of N. D. Kondratieff is certainly of great interest to the researcher but this paper aims at a part of his scientific heritage, considered in the context of the discussions that took place in the West. I will focus on the works written in the prison and devoted to the problem of economic dynamics. When assessing the scientific heritage of many of our scientists who became victims of repression, it is not easy to dismiss the thoughts of their tragic fate and analyze their ideas only in the context of the development of science. The past has also shown itself specifically in the formation of the scientist's image for several decades of oblivion. As for N. D. Kondratieff, some of his works became known abroad in the 1920s and were positively responded.[1] But even in the USSR the oblivion was not complete – the works of the scientist could be found in large scientific libraries, his name was mentioned even in encyclopedias, but in a critical way. The problem was that Kondratieff's ideas were unclaimed but this was a problem of domestic science itself and another symptom of interference (of political processes, ideology) in science.

Not surprisingly, the first positive mention of Kondratieff's concept of long waves in our press (and, what is more, in the party press) in 1984 (Menshikov 1984) was an event. A huge revelation for researchers was also the story of the scientist's daughter about the manuscript saved in the family, written in 1930–1932 in Butyrka prison and in the Lubyanka internal prison, as well as letters from the places of imprisonment. Kondratieff's full rehabilitation which took place in 1987 accelerated the process of returning his name and ideas to science, although preparations for the publication of his first collection *Problems of Economic Dynamics* (Kondratieff 1989) began even before taking the decision to rehabilitate him. This collection included not only the famous *Large Cycles of Conjuncture* together with the discussion of 1926, but also other works on the problem of economic dynamics. In 1991, an unfinished manu-

[1] Thus, in terms of the integration of Kondratieff's ideas into world science, it is difficult to overestimate the importance of J. Garvy's review paper (1943) or the fact that in 1934 J. Schumpeter associated Kondratieff's name with long waves of conjuncture (Schumpeter 1982: 49). The acquaintance of the world scientific community with the works of Nikolai Dmitriyevich began with the publication of his articles on the problems of dynamics and long waves, as well as works reflecting the activities of the Institute of Conjuncture (Kondratieff 1925a, 1925b). After a surge of interest in Kondratieff's works in the West in the 1920s, such publications were sporadic for several decades, and only in the 1970s reprints of the scientist's works appeared again, and in 1984 a translation of his famous 1926 report (Kondratieff 1926) on large conjuncture cycles was published as a separate book in the United States (*Idem* 1984).

script written in confinement was also published under the title *Basic Problems of Economic Statics and Dynamics*. The first publications were followed by others. A significant social and scientific event was the First International Conference dedicated to the 100th anniversary of the scientist's birth, which established The International N. D. Kondratieff Foundation.

Amid growing focus on the processes in the USSR in the 1980s, a wave of increased interest in Kondratieff spread abroad. In 1992, a collection of works devoted to the problems of large cycles was published in France (Kondratieff 1992), and a four-volume edition of his works was published in England in 1998, including the first foreign publication of the book *Basic Problems of Economic Statics and Dynamics* (Makasheva *et al.* 1998).

One can note that in terms of publication of the scientist's works, justice has been restored. However, reflecting on the development of science in general and on the specifics of the development of economic science in our country in particular, one cannot but recognize with regret how much has been irretrievably lost. In science, as in life, time is irreversible: the content of problems, analysis tools, conceptual apparatus tend to change; what was accepted by the scientific community several decades ago has become an integral part of science, and what is discovered with a huge delay, to a significant extent turns out to be a fact of history rather than of modern science. One cannot but agree with Kondratieff, who at a very difficult period of his life wrote that the most priceless thing in our difficult life is thought. I think he meant a free thought, openly expressed and, of course, heard.

An example of what was heard abroad is, of course, the concept of long waves. Kondratieff's name is primarily associated with it.[2] This theme prevails in modern domestic publications as well. However, the theory or, according to the scientist, the hypothesis of large cycles as such not only does not reflect the whole idea of the theory of dynamics, but also, due to its attractiveness and, I would say, intriguing character, in a sense even distracts from the essence of this idea.

The fact is that, reflecting on the theory of dynamics, Kondratieff posed a number of important and still relevant philosophical and methodological problems: the relationship between theoretical and empirical knowledge; individual and social, the role of time in economic science (related problems of dynamics, including cycles and trends), which go beyond the concept of long waves as such. He was preoccupied with the problem of the theory of economic dynamics. And in this he was certainly not alone. Against the background of the spread of the idea of the deductive character of economic theory and together with the approval of the equilibrium model in the spirit of L. Walras as its

[2] Fortunately, thanks to new translations, the works devoted to other issues started to appear (see, *e.g.*, Carry 1996).

model, some economists expressed doubts as to whether it was possible to construct a dynamic theory based on the initial assumptions of the equilibrium model, which became the core of neoclassics. Thus, at the same period when Kondratieff was reflecting upon the problem of dynamics, J. Schumpeter (1982) made an attempt to go beyond the boundaries of the equilibrium approach by revising the model of individual behaviour. In a broader context, what Schumpeter proposed can be interpreted as a desire to abandon the Newtonian view on economic order as being, which was fully embodied in equilibrium models, and replace it with an interpretation of order as becoming, which was compliant with shifts in the natural sciences.[3] A few decades later, Schumpeter's ideas were partly realized in evolutionary economic theory (see, *e.g.*, Nelson and Winter 2002).[4]

In Kondratieff's time, the problem of dynamics was inextricably linked with the problem of the cycle. One could even say that the latter was the starting point of the former. It is not by chance that practically all economists who addressed the problem of dynamics were engaged in the study of the cycle. The analysis of cyclical processes led to the realization of the discrepancy between the equilibrium approach, on which the abstract economic theory was based, and the view of the cycle as a deviation from equilibrium. The question is whether the cycle is a non-equilibrium process, and therefore its analysis involves the use of fundamentally different tools, or whether the equilibrium approach is universal. During the period in question, economists were more in favour of the former. Only half a century later a new trend emerged – new classics – which advocated the possibility of using equilibrium models in the study of the cycle. I mean the theories of equilibrium and real cycles.

The period of N. D. Kondratieff's high creative activity was one of the most fruitful in the development of economic science in the 20th century, not only in the West, although primarily there, but also in our country up to a certain moment. In the history of Western economic science, it is usually characterized as the end of the A. Marshall era and, according to J. Shackle's famous statement, can be referred to as 'the years of high theory' (Shackle 1967). Methodological discussions of the 1920s – the 1930s reflected the objective duality of economic science. On the one hand, there was the aspiration of economists to the strict logic of abstract deductive science, which is based on the

[3] Of course, F. Hayek was one of the most vivid and consistent critics of the idea of order as a state (being). In the 1930s and 1940s arguing against the theory of market socialism, in fact he raised a more general problem – the dominance of a mechanistic, 'equilibrium' worldview in economic science, the most consistent implementation of which was the theory of general economic equilibrium (Hayek 2011).

[4] Of course, there are fundamental differences between Schumpeter and modern evolutionists. For Schumpeter, *e.g.,* the evolutionary principle was a way of explaining the nature of capitalism. While for others it was rather a way of describing the functioning of a complex system of interacting subjects under conditions of uncertainty.

model of the rational human (methodological individualism) and poses analytical problems. On the other hand, there was a tendency to make science practically significant, which contributed to the development of empirical research, which in that period was mainly focused on working with aggregate values (methodological holism).

As already noted, the theory of general economic equilibrium was perceived by economists as a model of abstract theory. It represented the economy as a set of isolated rational economic agents optimizing their goal functions under certain specific conditions. This theory aimed at studying the state in which all participants are satisfied with the achieved result and, therefore, there are no internal reasons to change it. The abstract nature of this theory provided significant opportunities for formal analysis and at the same time made it a target of criticism. It is easy to reproach it with detachment from reality and lack of attention to important problems that had no place in the perfect world of L. Walras. Indeed, the theory of equilibrium developed mainly under the influence of its internal logic. At the same time there was a change in the understanding of science itself. In the mid-1930s, in his famous essay, L. Robbins defined it as a field of knowledge centered on the optimal use of limited resources (Robbins 1993). In this approach, economic theory was no longer seen as a model of reality, albeit a very simplified one, but as a tool for analyzing certain problems from a certain point of view, which allowed to remove the question of realistic assumptions.

The proponents of the empirical approach who had strong and obvious arguments in favor of their point of view opposed this trend. The accumulated data base and developed tools allowed to give answers to practical questions and thus to resist the claims of abstract theory to the status of the only scientific one. The opponents of the pure theory allowed the penetration of other sciences into economics, which pure theorists sought to avoid. As noted by Th. Veblen, the goal was to create a ‘culturological’ economic theory based on anthropology, social psychology and sociology (Veblen 1919: 149). Although the dream of American institutionalists was not realized, W. Mitchell's (1913) works on the problems of cycles (so close in spirit to Kondratieff), in which ‘economic history, theory and statistics were uniquely combined’, became the highest achievement of the institutional school (Rima 1981: 62). It was the theory of cycles that became the arena of struggle between two opposing methodological positions. It is not by chance that this very problem largely determined the vector of N. D. Kondratieff's theoretical research.

Speaking about the struggle between ‘empiricists’ and ‘theorists’, one should bear in mind that, since empirical research in that period was connected with the collection and analysis of aggregate data and with attempts to establish statistical dependencies between macroeconomic indicators, the problem of meaningful interpretation of the obtained statistical regularities arose. This

problem became especially acute after the works of J. Keynes, which presented a macroeconomic situation – involuntary unemployment, poorly explained on the basis of accepted ideas about rational behaviour. The need to explain this phenomenon based on the model of rational behaviour caused the problem known today as the problem of correlation between micro and macro approaches, or the problem of macro-to-micro reduction.[5]

Another 'eternal' problem directly related to Kondratieff is the problem of statics and dynamics. The question that was raised in the discussions of the 1920s – the 1930s was the following: Is it possible to construct a deductive theory of dynamics, can it be done on the basis of hypotheses of rational behaviour and using the equilibrium approach? It was suggested that this was fundamentally impossible due to the specific nature of the initial assumptions of the theory of equilibrium (see, *e.g.*, Northrop 1941). Thus, for the theory of dynamics J. Schumpeter was ready to radically revise the foundations of equilibrium theory – the model of behaviour of the economic subject (Schumpeter 1982). Kondratieff may have a special place in the context of this discussion. He proposed to change not the model of individual behaviour (although he suggested some modifications), but to use a different toolset – statistical and probabilistic.

N. D. Kondratieff formulated his main task as follows: to create the foundations of the theory of socio-economic dynamics, an integral part of which is the theory of cycles including large cycles. This task was explicitly outlined in the early 1930s, when the scientist was already in prison, but in the 1920s he used some of his then still unclearly formulated ideas in the study of cycles. The importance of the problem of cycles was determined for him, first of all, by the fact that cycles represented regularly recurring events, which can only be the subject of science (Kondratieff 1991).[6]

Already in the very first work devoted to cycles *World Economy During and After the War* (1922)[7] Kondratieff defines his methodological position. He writes about the economy as a system of relationships and connections of elements and at the same time mentions about different approaches to its analy-

[5] As history shows, there are two principal ways to resolve this contradiction: either to try to give microeconomic justification to macroeconomic dependencies, *i.e.* to recognize the legitimacy of the reduction problem and try to find a way to obtain macroeconomic dependencies from microeconomic ones, or to deny macroeconomic dependencies in any content and consider them as statistical artefacts. The representatives of the Austrian school uncompromisingly defended the latter point of view – the principle of methodological individualism. But they were 'spoilt' by certain circumstances, *e.g.*, money, the functioning of which cannot be explained without going beyond the limits of methodological individualism.

[6] See Kondratieff's article 'Towards a Question on the Concepts of Economic Statics, Dynamics and Conjuncture' (Kondratieff 1989: 48–90).

[7] This work of a specifically empirical nature focused directly on one cycle, which was interrupted by war and culminated by crisis of 1920–1921, which extremely alarmed economists in terms of the prospects of the capitalist economy (*Idem* 2002: 40–340).

sis – dynamic and static points of view. In fact, he identifies the theory of conjuncture[8] with the theory of cycles and calls it an integral part of the theory of dynamics. He argues that dynamic theory as a theory that establishes regularities of changes in the elements of economic life in their relationship, *is possible in principle*,[9] and its practical significance and scientific validity are determined by the quality of the forecast that can be made on its basis. Subsequent works, including the famous report 'Large Cycles of Conjuncture', in many respects represent the development and substantive content of these methodological guidelines and at the same time are the starting point of his theory of social economy, the contours of which a few years later will be outlined in *Basic Problems of Economic Statics and Dynamics*.

Despite many inconsistent problem statements and a variety of approaches to their solution, in that period two cross-cutting themes stood out in the field of cycles research. One of them – methodological – concerned the problem of correlation between empirical and theoretical approaches to the problem of cycles. It was a question of whether an abstract theory of the cycle, developed in a deductive way on the basis of hypotheses concerning individual behaviour and derived mainly introspectively, was possible, or whether, due to the specifics of the subject, only a theory arising from the analysis of actual data, *i.e.* inductively, was admissible. Another problem related to the first one – theoretical – was to find out the driving force of the cycle, and it also contained a methodological component – the question of the principal possibility of the existence and definition of this single cause. The proponents of the abstract theory adhered more strictly to the idea of the existence of a single cause (which, of course, did not exclude disagreements about it), while the 'empiricists' recognized the action of several forces, realized partly independently.

As in the famous debate about the method of the late 19th century, in the 1920s the representatives of the Austrian school such as L. von Mises and F. von Hayek remained the leaders of the theoretical approach. In this issue they were joined by J. Schumpeter, W. Jevons, C. Wicksell, A. Löwe, and partly A. Marshall. The place of the historical school was taken by scientists belonging to different directions, primarily American institutionalists W. Mitchell and J. M. Clark, as well as French statistician C. Juglar, German economist F. Lutz and others. J. Gobson, A. Spiethoff, A. Aftalion, R. Hawtrey, K. Wicksell, F. Hayek, J. Schumpeter can be attributed to the number of the supporters of the monistic approach to the question of the causes of the cycle, although

[8] The definition of conjuncture will be given later, in 1924, in the article 'Towards a Question on the Concepts of Economic Statics, Dynamics and Conjuncture' (*Idem* 1989: 48–90).

[9] Such a statement in itself is not obvious at all. The question of the fundamental possibility of the theory of dynamics as a great methodological and philosophical problem was only realized by scientists in that period. The above-mentioned article by F. Northrop contains an interesting view on this problem.

with some reservations. Among the supporters of the pluralistic approach one should mention J. Lescure, G. Cassel, M. I. Tugan-Baranovsky, W. Mitchell, F. Taussig, J. B. Clark and others. The reasons for the cycle included institutional factors, innovations, peculiarities of the production process (first of all, the high degree of roundaboutness of production), the specifics of market interaction under conditions of uncertainty, the existence of money and the peculiarities of the monetary system, *etc.*

The main arguments of the proponents of the empirical approach were as follows. Abstract theory is primarily a theory of equilibrium, and cyclical fluctuations are deviations from equilibrium, hence the contradiction between tools and phenomenon.[10] Abstract theory, due to the limited number of simple hypotheses on which it is based, overlooks many substantive aspects of the phenomena, which forces its proponents to consider additional exogenous factors and/or adjust the basic assumptions of the initial model. The conclusions of the abstract theory, obtained on the basis of hypotheses of individual behaviour, do not allow explaining changes in aggregated statistical indicators, which characterize the conjuncture. Finally, in the current stage of knowledge it is impossible to identify the main cause of the cycle.

It should be emphasized that the proponents of the empirical approach did not deny the importance and relevance of the theory, they only insisted on its difference from the one associated with the word 'theory' in modern economic science. Characterizing W. Mitchell's position, J. Schumpeter wrote,

> The theory of the economic process itself was to remain a theory, but it was to become a theory built from the results of detailed observation of actual behaviour and – since he did not exclude in principle either introspection or psychological interpretation inspired by introspection – motivation (Schumpeter 1997: 246).

Of course, the adherents of apriorism and deductive approach had their own arguments. In particular, they argued that no empirical research can be independent of the scientist's ideas about the nature of the phenomenon under study and its mechanism, *i.e.* from the accepted theory. Empirical research itself can reveal only statistical dependencies which are of limited value for establishing cause-and-effect relations[11]; empirical research is important for illus-

[10] How seriously this criticism was taken by the proponents of abstract theory can be judged at least by the fact that F. Hayek, trying to remove this collision, turned to money 'for help' – this unique phenomenon, which, strictly speaking, does not fit into the theory of equilibrium, facilitating the explanation of cyclical fluctuations (Hayek 1933). A response to this position was also J. Schumpeter's rejection of the equilibrium model and the introduction of the concept of innovative behaviour into the theory. Fifty years later, the modern proponents of pure theory, the new classics, took a different path: they recognized the cycle as an equilibrium phenomenon and thus brought the phenomenon and the tools of its study into compliance (see, *e.g.*, Snowdon *et al.* 1994).

[11] As A. Pigou wrote, 'the absence of statistical correlation between a given series of change and industrial fluctuations does not by itself disprove, and its presence does not prove, that these changes are causes of the fluctuations' (Pigou 1929: 37).

trating and making predictions, and only through prediction the theory confirms its practical significance, but the value of the prediction depends on the reliability of the theoretical structure on which it is based (Hayek 1933: 35–39). Finally, they also argued that hopes for significant advances in theory as a result of increased empirical research are the result of misconceptions about the relationship between them (Löwe 1926). In the end, the final decision on the importance of a phenomenon in explaining the cyclical process cannot be given to mathematicians and statisticians, because it is the knowledge obtained theoretically that matters (Altschul 1926: 85).

In his works on the problems of cycles, Kondratieff is certainly a supporter of the empirical approach in its broad interpretation in the spirit of Mitchell. Defining the field of his research as the theory of dynamics, he formulated the task of the latter as follows: the establishment of regularities between the elements of the economic system in their changes in real time that these elements are characterized by quantitative indicators and that regularities are manifested primarily through statistical dependencies. Finally, the practical significance of the theory is determined by the possibilities it opens up for forecasting in the sense presented in his articles 'The Problem of Foresight' (Kondratieff 2002: 509–566) and 'Plan and Foresight' (*Ibid.*: 567–614).

As mentioned above, N. D. Kondratieff considered the theory of conjuncture or the theory of cyclical processes as part of the general theory of dynamics, which was originally the subject of his research. Since, according to his formulation, the conjuncture at a given moment of time is nothing but 'the direction and degree of change in the set of elements of national economic life in comparison with the previous moment' (Kondratieff 1989: 70), one should define a set of relevant elements, collect and analyze statistical data. It was on the basis of data analysis that he developed the concept of large cycles in full accordance with the empirical approach. This concept was not a theory in the classical sense of the word, *i.e.* a logical scheme built on the basis of behavioural hypotheses, it was only an explanatory hypothesis arising from the analysis of facts. This was quite consistent with the views of W. Mitchell and other representatives of the empirical trend.

In his works on cycles and conjuncture, Kondratieff was a supporter not only of the inductive-empirical method, but also of the pluralistic approach to analyzing the causes of long-term cyclical fluctuations. When explaining the mechanism of the cycle, he pointed to innovations, the interconnectedness of industries, the specifics of the distribution system, the peculiarities of the banking system, the existing relations in agriculture, *etc.* One can say that he followed the same path as M. Tugan-Baranovsky, A. Spitgoff, J. Schumpeter, G. Cassel, but at the same time he borrowed some elements of the monetary theories of cycles of C. Juglar, G. Sidgwick, R. Hawtrey and others. Of course, some criticisms against these scientists apply to his concepts as well. But while

acknowledging this criticism, it should be borne in mind that the scientist saw his main task, at least at the initial stage of his research, not in creating an exhaustive theory or model reproducing the mechanism of the cycle, but in studying the actual movement of the conjuncture and in defining the range of indicators characterizing the conjuncture, and filling them with statistical content.

N. D. Kondratieff's merit in this field consists not so much in explaining how the impulse coming from innovations spreads in the economy and leads to the emergence of a cumulative process, and what circumstances cause its stoppage and how the conditions for a new upswing are formed (not much has been done here), but rather in formulating the hypothesis of large cycles and its empirical substantiation. This concept, to the extent in which it was developed, represents an empirical illustration of one of the sections of his future theory of dynamics. And here one should mention the work *Basic Problems of Economic Statics and Dynamics*, in which the scientist planned to present the methodological basis of a new theory of economic dynamics, based on the statistical and probabilistic worldview and allowing reconciling theoretical and empirical approaches, static and dynamic points of view, methodological individualism and holism.

As a supporter of positivism,[12] like many representatives of his generation, N. D. Kondratieff proceeded from the unity of sciences and in the conditions of rapid development of natural sciences saw in their methods an object for borrowing by social sciences. One should note that we are talking about the so-called classical science, which includes, for example, physics of the prequantum period. What can be the worldview of a researcher of social phenomena, who strives for accuracy and objectivity, comparable to those characteristic of the natural sciences? What can be the core that unites both branches of science? Kondratieff's answer is simple and generally anticipated: a statistical and probabilistic worldview, as well as statistics and its methods, which are quite suitable for studying the phenomena of both physical and social world. It should be emphasized that for Kondratieff the statistical approach is not just a technical tool of analysis, but a means to perceive reality, which determines the nature of science.

Kondratieff considered the concept of aggregate, which he borrowed from A. A. Chuprov, as the central concept within this worldview. The aggregate as a statistical phenomenon implies such a large number of interconnected elements that enforces the law of large numbers. As a result, the aggregate appears as something that is not reducible to a simple sum of elements, but forms a system of interrelationships, generating specific phenomena. Kondratieff applies

[12] It should be noted that in this case the adherence to positivism meant, first of all, the conviction in the necessity of a strict distinction between the economic and social aspects of phenomena, which were traditionally mixed in Russian thought.

the concept of aggregate first to society and then to the economy. He understands the economy as an aggregate determined by the specifics of individuals' activity, which is fixed by the economic principle – the desire to satisfy needs in conditions of limited resources.

Thus, the individual remains the initial element of the economy, and its social nature is based on the fact that the economy is an aggregate. This means that people act in accordance with their individual preferences and make their choices by entering into relationships with others, influencing them and thereby changing the conditions of their own activities. Due to the extensive and complex nature of interaction within the aggregate, the properties we observe appear smoothed, averaged and irreducible to individual characteristics. In fact, we are talking about the law of heteronomy of goals known in philosophy and associated with the name of W. Wundt and the principle of non-intentionality of results no less famous in economics and associated with A. Smith's name. In Kondratieff's approach, the term 'invisible hand' is used for a description of a spontaneous and uncontrollable process of transformation of individual actions into a social result.

Thus, the statistical approach allows Kondratieff to retain the individual as the initial element of the economy and at the same time to focus on analyzing the economy as a social phenomenon. In this case, 'sociality' does not have any class, cultural or ethical meaning, but is a result of the interaction of a large number of elements of the aggregate, *i.e.* economic subjects. This approach assumes that economic science studies the social nature of economic activity, and its task (which Kondratieff formulates not without the influence of neo-Kantians[13]) is to establish a system of unambiguous cause-and-effect relations, *i.e.* regularities, which are the result of the law of large numbers and manifest themselves to the extent at that this law operates (Kondratieff 1991: 219).

It is logical that when discussing the problems of cognition, N. D. Kondratieff pays much attention to the question of the nature of causality. He answers it rather in the spirit of determinism characteristic of the 19th century, considering causality as having more an epistemological, rather than ontological, nature. From his point of view, it is the limitation of human capacities and the current knowledge that makes inevitable the 'shortening' of the considered 'tree' of causal relations, and the procedure of 'shortening' refers not only to space, but also to time.

The problem of a short time horizon is partly solved when repetitive, uniform, regular events are studied. That is why the scientist turns first of all to cyclic processes and sets the task to determine the laws defining the course of

[13] A comprehensive analysis of the philosophical and sociological foundations of N. D. Kondratieff's position is given in Y. N. Davydov's article 'N. D. Kondratieff and Probabilistic-Statistical Philosophy of Social Sciences' (Kondratieff 1991: 453–523).

uniform phenomena in time. But the specificity of social phenomena is that from the point of view of an observer they proceed too slowly (action and result are separated in space and time), and at any given moment the number of actions and acting subjects is quite small in comparison with what the representatives of natural sciences deal with. As a result, the law of large numbers in relation to social phenomena does not give the same results as in natural sciences, and the established regularities are inevitably probabilistic.

This fundamental conclusion determines a lot, in particular, ideas about the relationship between theory and facts, about the possibilities of practical use of theory, primarily for forecasting purposes, *etc.* This is the first methodological result of Kondratieff's application of the statistical-probabilistic approach. The second was to be the theory of economic dynamics.

N. D. Kondratieff rightly believed that none of the schools he knew had a dynamic theory: in the case of neoclassical one it was due to the specifics of the basic assumptions that give its theory a static character, as for the historical school it did not offer any theory, replacing it with a historical description. In addition, referring to the fact that science can only study regular events, he had a negative attitude towards J. Schumpeter's attempt to create a dynamic theory, the central element of which was the innovative activity of the entrepreneur. Of course, Kondratieff tried to show that his approach opened up the prospect of analyzing not only statics but also dynamics.

As for statics and application of the statistical and probabilistic approach to the analysis of equilibrium, by his own admission, he could not obtain anything fundamentally new in comparison with the traditional approach, although, as it seems to me, the possibility of transactions at non-equilibrium prices, which Kondratieff allowed, opened the way to the analysis of non-equilibrium processes. The study of a sequence of equilibria of different levels (similar to A. Marshall's equilibria of different order) seemed more interesting to him. It turned out that when moving to a higher level, *i.e.* when the number of elements whose values are assumed to be set decreases, the time factor becomes more and more significant, in other words, the element of randomness, irregularity, uniqueness becomes more and more important. What remains unclear is to what extent the use of the statistical-probabilistic approach would allow to cope with the task of revealing regularities of regular events in time. A proof of the value of this approach could be the theory of economic dynamics. Unfortunately, Kondratieff's line of reasoning was interrupted at the most interesting place. It is known that the scientist gave his unfinished manuscript to his wife before being sent to Suzdal prison in early 1932. He never returned to this manuscript after that, although at least during the first two years of his stay in Suzdal he continued to work.

In 1932–1934, Kondratieff's attention was occupied with the issue of trend. The result of this work was a large book. As far as one can judge from the scientist's letters to his wife, he analyzed the trend from the standpoint of the statistical and probabilistic approach. In his letter of 29 May 1934 he noted that the last of the written chapters 'Stochastic Analysis of Time Series and Determination of the Form of Trend' was devoted to

> theoretical and probabilistic justification of methods of establishing a trend based on empirical data after the general form has been deductively derived... This chapter is mainly mathematical, theoretical and probabilistic in nature and was very difficult. The next chapter entitled 'Abstract Theory of Trend' will also be difficult. After that I will write two or three chapters with empirical content and the work will be finished. But the end of it is not near yet, although now it has already more than 30 printed pages. The further I write it, the more I am troubled by the thought that I am doing it only for myself (Kondratieff 2004: 517).

The last assumption of the scientist turned out to be prophetic. A brief description (in a letter dated 5 September, 1934) of a one-sector model of economic growth, which was rather similar to the neoclassical models like Solow's model that appeared two decades later, survived. But even here Kondratieff was original, in particular, he used a logistic curve of changes in exogenous variables. Obviously, this model presented a deductively determined trend. But, judging by the letters, by that time a chapter on the theoretical-probabilistic definition of the trend had already been written, that is, the part of the work which is directly related to the statistical-probabilistic approach. But we do not know what theory of trends Kondratieff created, how this theory related to his unfinished methodological work, or how he further developed the original idea. Whether the path that N. D. Kondratieff was going to follow could lead to the development of a research paradigm that would at least partially resolve the fundamental methodological problems of economic science is a question that we are unlikely to ever get an answer to. The scientist continued to work until the end of 1936, but we know nothing about the results he obtained. Not only was thought – what the scientist considered most precious – but he himself was destroyed. Today, when we turn to the history of domestic economic science, we see that the chariot of history has rolled not only through Kondratieff's generation, as well as through other generations, but also through economic science, having interrupted the normal process of growth of scientific knowledge.

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