

Springer Studies in the History of Economic Thought

Vladimir Avtonomov  
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# Russian and Western Economic Thought

Mutual Influences and Transfer of Ideas

 Springer

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Editors

# Russian and Western Economic Thought

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# Preface

The idea of this book originated in 2017 in Rome where a round table about European economic thought was organized by Antonio Magliulo. One of us (Vladimir) was invited to deliver a paper on relations between Russian and European economic thought. This was the beginning. Well, relations of Italian or Polish economic thought with European one are the relations between the part and the whole. The relations between Chinese economic thought and the European one are the relations between two very different entities. However, Russia as a Eurasian country with European culture and great historical and mental characteristics occupies a very special place. Its relations to Europe and the West on the whole are controversial and complex. Therefore, the parallel development of Russian and Western economic thought looks to be an object worth investigating.

The discussions on this exciting topic mounted in the proposal for a special issue of the *Russian Journal of Economics (RUJE)*. This plan was finally realized. The articles by Vladimir Avtonomov (general introduction and a paper on Storch and Chernyshevsky), Harald Hagemann (on Leontief), Natalia Makasheva (on Kondratiev), Francois Allisson (on Tugan-Baranovsky) and Denis Melnik (on Lenin) were published in *RUJE* 2021 Vol. 7, Issue 1, and served as the basis for the corresponding chapters of this volume. We express our sincere gratitude to the editors of *RUJE* for granting permission to include these articles. In the meantime, we started to think about expanding our scope and inviting experts from Russia and from the West to contribute to a wider book project. The response, with additional fourteen chapters, surpassed our expectations. It wasn't easy to foresee that such a big group of well-known and very busy scholars would agree to our plan and write papers on Russian economists who played an important role in the relations between Western and Russian economic thought. Moreover, they did the cross-refereeing of the chapters and helped their colleagues with valuable comments and corrections. We would like to thank all our authors for their involvement in the project, but, probably, Andrey Belykh, who reviewed several chapters, deserves a special gratitude.

The order in which the economists of Russian origin appear in this volume follows their dates of birth. There is one exception: Leonid Kantorovich was in fact older

than Evsey Domar, but we chose to finish our succession with him, because he was the only one whose biography was wholly connected with the Soviet Union.

Another special problem connected with Russia is the English spelling of Russian names. There don't exist any universal rules even in dictionaries and encyclopedias, so after long deliberation we decided to make our own list following established traditions and our taste.

Moscow, Russia  
Stuttgart, Germany

Vladimir Avtonomov  
Harald Hagemann

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# The Transfer of Economic Ideas Between Russia and the West: An Introduction



Vladimir Avtonomov

## 1 General Historical Relations of Russia and the West: A Brief Summary

The general relations between the West and Russia have been a fundamental question of self-identification for Russian society since long ago. We cannot touch this controversial issue but very superficially.<sup>1</sup> It reflects the general situation of Russia as a country with significant specificity determined by its history and geography. The opposition between Russia and Europe was based on ecclesiastical matters since the Kievan Russia adopted the Eastern Orthodox Christianity in the end of X century. Eastern Russian principalities, where Moscow gradually became the political center, were under Tartar rule between the middle of the XIII and the end of the XV century which further contributed to enlarging the distance from Western Europe. Since then, the process of interaction involved a sequence of modernizing reforms which involved imitation of Western practices (under Peter I, Alexander II, the Bolsheviks, Perestrojka) and following counter-reforms aiming at preserving and stabilizing the autocratic political regime. It could be stated that the general principle guiding the Russian position toward the West was the pragmatic borrowing and imitation of instrumental and technical knowledge and skills (mostly, but not exclusively for military purposes) combined with opposition to Western political and spiritual influence.

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<sup>1</sup> Among innumerable sources we can mention: Billington (1966), Hedlund (2003) and Pipes (1995).

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Russian, Soviet and Post-Soviet Russian rulers never believed in the interrelation of the technical and spiritual sides of Western civilization.

The main question can be stated as follows: do Europe and Russia take the same road of progress with Europe far ahead and Russia lagging behind, or do they belong to different civilizations with different trajectories<sup>2</sup> (possibly, with Russia having some advantages of the spiritual kind).

## 2 The Interrelations of Western and Russian Economic Thought as a Controversial Issue

In Russia, we can distinguish two “extreme” positions regarding the relation between Russian and European economic thought:

The first one considers Russian thought as permanently lagging European one, adopting and distorting European ideas. This position was expressed by Vladimir Svyatlovsky in the first history of Russian economic thought (Svyatlovsky 1923) and Jack Normano,<sup>3</sup> who published his history in the USA. (Normano 1945)

These authors analyzed foreign influences on Russian thought distinguishing phases of English, French, and German influence. The unilateral transfer of ideas between the West and Russia was possible because the ruling and intellectual elites of Russia traditionally spoke and understood foreign languages (mostly French and German) and often were educated in foreign (German) universities. This situation existed since the end of XVIII century till the 1920s.

The representatives of the second position insisted on specific features of the so-called Russian school of economic thought from Posochkov till Tugan-Baranovsky (even Lenin was sometimes included!) which were characterized by ethical approach and non-individualist methodology (Abalkin 2000). This specificity was considered as an important advantage of Russian thought in relation to Western one which kept economic and moral issues apart.

In fact, there also existed the third position which somehow combined the two mentioned above. In the post-war period, Soviet historians of economic thought mostly acknowledged the existence of a single economic science world but underlined the superiority of Russian thought whenever it seemed possible. This approach related to the “campaign against cosmopolitanism” conducted by Stalin in 1947–1953 and was directed at denouncing all Western influences and extolling Russian thinkers (a popular joke succinctly summarized this tendency: “Russia is the homeland of elephants”). We could notice this tendency in the three volumes of “History of Russian Economic Thought” edited by Pashkov (1955–1966).

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<sup>2</sup> The most ambitious attempt to establish this point of view was probably Danilevsky’s book where he included Russians with other Slavic people in a separate cultural-historic type which allegedly was going to be realized soon. See Danilevsky ([1869] 2016).

<sup>3</sup> Isaac Ilyich Levin (1887–1945) published his works under this name after his emigration from Russia.

We should specially mention two works in this field which appeared in the 1940s: the books by Blyumin (1940) and Shtein (1948). For both authors, Russian economic thought was the secondary source of inspiration: Blyumin was the best Russian historian and critic of Western marginalist theory, and Shtein was known as sinologist and historian of Chinese thought. Both were severely reprimanded for “objectivism” and lack of Marxism. Shtein even had to blame Blyumin for declaring Russian political economy of the first half of XIX century a “foreign science imported from the West” (Shtein 1948, p. 5). Unlike his predecessor, Shtein published his essays during the campaign against cosmopolitanism and had to unite Marxism and Russian nationalism. But under these severe circumstances, both authors managed to write the works based on genuine research which paved the way for later historians of Russian economic thought who had more freedom in their investigations. One of them was Anikin (1990) who underlined the fact that economic science could not be advanced in such countries as Russia with lagging economic development.

The general trend in Western histories of Russian economic thought was to underline Western influences which were received in Russia “in a peculiar distorted form” (Barnett 2004, p. 22). Among modern Western histories of Russian economic thought which correspond to this thesis (with which we agree completely), we can name Zweynert’s and Barnett’s books (Barnett 2004; Zweynert 2002). But, Western authors mostly have not dealt with the feedback influence of Russian economists on Western thought. There are several rare exclusions (Barnett 2011; Beckmann 2005; Chipman and Lenfant 2002; Janssen 2004).

Our position can be briefly described as follows (Avtonomov 2021). There is a certain scheme of interrelations between Russian and Western economists. In this scheme, economic ideas usually come to Russia from the West. Then, they are modified (often very significantly) according to specific Russian circumstances. After that, sometimes a certain feedback occurs—the modified ideas in their turn influence some Western economists of next generations. We must, however, underline that this type of interrelations was not predominant: the last stage (feedback) occurred not very frequently.

### **3 The Factors Causing Modification of Western Theories by Russian Economists**

Among these factors, we can mention moral and religious factors, the peasant question, the influence of Marxism, development of mathematics and statistics in Russia in 1890s–1920s, the unique experience of designing, and building a planned economy in the post-revolutionary Russia. The factors belonging to this heterogeneous set (certainly, it is not the whole list) are not independent of each other. For example, the first two ones are obviously connected, as far as the XIX century is concerned. The attention of Russian economists to peasant question related to the negative moral attitude of large circles of Russian society to serfdom. An important reason why

Marxism became particularly influential in Russia in the end of XIX and the beginning of XX century was the high degree of social tension in the country after non-consistent peasant reforms. Political sympathies of our heroes were predominantly with socialist movement of their times. Probably, only Brutzkus, who did not find his well-deserved place in our volume only because we could not find an author in time required, displayed the liberal position.

The last two factors may also have something in common. The approaches to building a planned economy were prepared by experience in mathematics and natural sciences among leading Bolsheviks like Bogdanov and Bazarov (see *Elizaveta Burina's* chapter). Dealing with the heroes of this volume, we shall very often encounter the combination of socialism and high-brow mathematics.

This long-time characteristic is also reflected in game theory modeling during the Cold War period which created a positive environment for game theory in the Soviet Union as well as in the West. A comparison of similarities and differences reveals that there was a lagged evolution of game theory in the Soviet Union, which focused more on operations research and issues of centralized planning. However, due to the works of Nikolay Vorob'ev, Leon A. Petrosyan, Elena B. Yanovskaya, and Olga N. Bondareva on dynamic stability, non-emptiness of the core, and many refinements, game theory in the Soviet Union was able to catch up to the Western level quickly (Hagemann et al. 2016). This has also been shown at the famous conference in Vilnius 1971 which fostered interaction between Soviet game theorists and Western colleagues such as Oskar Morgenstern.

### 3.1 *Moral/Religious Factors*

Following Zen'kovsky ([1948] 2001), Zweynert enumerated the following elements of Russian patriotic legacy important for shaping Russian economic thought (Zweynert 2002, pp. 31–35):

1. Essentialist-organic holism (unity of faith and thinking, unity of individual and society, unity of the state and the church).
2. Anthropocentrism—emphasis on social questions and moral norms.
3. Mystical realism which means the emphasis on spiritual, non-material world.

These factors refer to the orthodox worldview influencing Russian philosophy, and their relation to economic thought is indirect. We can also see some contradictions between them: emphasis on social questions is hardly compatible with the emphasis on non-material world. However, a public worldview (and even an individual one) is not necessarily consequent, and the above-mentioned features were present in Russian economic ideas which were often intertwined with religious and philosophical ones.

As an example of the influence of such factors, we can mention a man who is considered to be the first Russian economist, Ivan Pososhkov (1652–1726). Pososhkov lived under Peter the Great and wanted to influence the Czar's policy

by a confidential note (Pososhkov [1724] 2004). His views could be classified as mercantilist, and he advised the Czar how to find means for financing the army, the navy, and the building of St. Petersburg. Pososhkov was not acquainted with European economic thought of his time. Neither did he exert any influence on foreign economists. So, we cannot count him as an example of our pattern. But, his ideas were a pure case of the first factor of Russian specificity—a special stress on spiritual and religious factors. Pososhkov’s pamphlet had strong religious overtones.<sup>4</sup> Probably, he was the first Russian economist who mentioned the “veritable truth” as a non-tangible wealth (Shirokorad 2008, p. 28)—a recurrent topic in Russian economic thought though Pososhkov’s book was not known to Russian economists till much later times. Pososhkov was even in principle against competition on moral grounds. He insisted that the prices of goods should be kept uniform to avoid disorders connected with competition.

von Storch’s conception of inner goods (Storch 1815), which is dealt with in *Vladimir Avtonomov’s* chapter, could also be associated with these moral-spiritual trend in Russian economic thought.

Other much later examples of moral and religious bend of Russian economists can be found in the works of former Marxists Tugan-Baranovsky (see *Francois Allisson’s* chapter) and Bulgakov.<sup>5</sup> Tugan-Baranovsky believed that Marxism should be grounded on Kantian ethics (Tugan-Baranovsky 1909). He founded the labor theory of costs on the basis that only human efforts are counted by human beings. But, the most radical turn from Marxism to idealism and religious ethics was undertaken by Bulgakov (1903). It is strange that his “Philosophy of economy” (Bulgakov [1912] 2009) was considered a doctoral dissertation in political economy because it was a totally theological text setting a goal of personal and spiritual progress and fighting with “economism”. Bulgakov’s works were apparently too exotic and orthodox to influence Western thought.

Around the turn of the centuries, Leo Tolstoy became the leading moral authority in Russia, though the official Orthodox church condemned his ideas. His influence in the country was considered to be equal to the Czar’s. No surprise that among our heroes, we can find followers of the great writer and moralist (e.g., George Charasoff).

### 3.2 *The Peasant Question*

No wonder that among the main factors influencing Russian economic thought the special importance of the peasant question and slavery was one of the most long-lived. The serfdom in Russia was abolished much later than in other European countries, and the “peasant question” remained to be a burning issue for many decades. Nikolay Chernyshevsky’s works on Russian *obshina* apparently influenced Marx’s position on the possibility of reaching socialism without the preceding capitalist stage (see

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<sup>4</sup> Pososhkov also composed special pamphlets devoted to purely religious topics.

<sup>5</sup> See also Makasheva (2008).



Vladimir Avtonomov's chapter). But, this specificity apparently did not produce any feedback on Western economic thought probably apart from Chayanov (1989) who used marginalist logic (presumably of Auspitz and Lieben) to build a theory of the peasant's household (see *Carol Leonard's* chapter). The peasant question reappeared on stage after the October Revolution of 1917 when the leading Bolsheviks argued over the best way to build socialism in the agrarian country. The great industrialization debate between the "rightist" Bukharin supporting a gradualist policy with voluntary cooperation of peasants and the "leftist" Preobrazhensky arguing for the "primitive socialist accumulation" redistributing resources from agriculture to industry by means of centralized policy had a long-term influence on Russian economists. The "leftist opposition" was eliminated before the "rightist" one, but Preobrazhensky's position was eventually taken by Stalin in 1929 when he implemented the policy of forced collectivization.<sup>6</sup>

We can follow the influence of this debate in several chapters of our book. Several economists who stayed in Russia (among them Chayanov and Kondratiev) lost their lives presumably as supporters of Bukharin's line. But among Russian émigré economists, we can see a somewhat surprisingly big number of those who were influenced by the "hardliner" Preobrazhensky's arguments (see *John King's* chapter on Baran and *Mauro Boianovsky's* chapter on Domar).<sup>7</sup> The reasons for that seem to consist in a more schematic style of Preobrazhensky's reasoning which was compatible with economic modeling and inspired several models of development economics. On the contrary, Bukharin's position had a humanistic appeal and was closer to those economists who had sympathy with the peasants (Chayanov, Kondratiev). However, when centralized development strategies lost their attractiveness over the whole World (1960s–1980s), Chayanov was rediscovered in the West.<sup>8</sup>

### 3.3 *Marxism and Socialism in Russia*

Marx's influence on Russian economic thought was indeed enormous. The first volume of *Capital* was translated into Russian by Lopatin and Danielsson in 1872, and it was the first translation of this book in any foreign language. But even before that (in 1871), Nikolay Sieber (1844–1888) defended his master thesis at Kiev University which was partly devoted to the economic theory of Marx (as reflected in the first volume of *Capital*) (Sieber 1871). The thesis, where Sieber analyzed Marx's theory of value and capital as the continuation of Ricardian one, was sent by Danielsson to Marx and highly praised by the latter. Sieber's works played an important role in spreading Marx's political economy in Russia in 1870s and influenced different circles of Russian intellectuals (Raskov 2018; Shirokorad 2018). One of the reasons

<sup>6</sup> A very concise but precise summary of this controversy can be found in Barnett (2004, pp. 79–81).

<sup>7</sup> See also, Ellman (1987).

<sup>8</sup> On Chayanov see also Coleman and Taitlin (2008) and Schefold (2017).

of subsequent enthusiastic adoption of Marxism in Russia was its appearance as a strict scientific theory on the background of amateur discussions between Russian populists and liberals. “The fact that Marxism took root among the Russian intelligentsia was evidence of a further Europeanization of Russia and of her readiness to share to the end the destiny of Europe” (Berdyayev 1940, 1951, pp. 117 f.). In the debate on possibility and inevitability of capitalism in Russia, in which the “populists” (narodniki)—Danielsson, Vorontsov—insisted on its impossibility and were countered by Liberals and Marxists (Plekhanov, Struve, Lenin), who were convinced that capitalism was already there, both sides appealed to Marx for approval.

On the next stage, the dividing lines were drawn among Marxists themselves. The evolution of Marxism in Russia led not to pragmatic revisionism and reformism as in Western European countries but to radical bolshevism (Leninism) as one extreme and to idealism and even religious mysticism as the other (see above). The first direction certainly found followers in the West after the victory of October revolution. The works of Lenin as a researcher-economist which emerged during relatively short periods in his active political life are dealt with in *Denis Melnik's* chapter. The other line, the so-called legal Marxists is represented in our volume by Tugan-Baranovsky and Struve (see *Francois Allisson's* and *Nikolay Nenovsky's/Guenther Chaloupek's* chapters) who did not accept Marxism fully and completely and revised it in different directions. Their opposition to Marxist economics was to a large extent methodological and ethical. However, their influence on Western thought was connected mostly with Tugan-Baranovsky's theory of business cycles and not with methodological and ethical considerations. Another epistemological modification of Marxism along the positivist lines under the influence of Mach and Avenarius was undertaken by Alexander Bogdanov who became a kind of predecessor of the systems theory. Here, we can speak about our West-Russia-West scheme in the realm of philosophy rather than in economics. But, Bogdanov's tektology later inspired Vladimir Bazarov's economic analysis of the recovery process of the Soviet economy (see the chapter by *Elizaveta Burina*).

Russian mathematical economists: Dmitriev, Bortkiewicz, Charasoff contributed to the discussion of the so-called transformation problem dealing with the alleged contradiction between the First and the Third volume of Marx's *Capital*. Partly correcting and partly rescuing Marx's approach, they could be considered as forerunners of Sraffianism.

We would like to mention that important feedback from Russian to Western economic thought was caused by the fact that Marx treated the capitalist economic system as a transitory one and analyzed its weak points more deeply than other theorists. For example, we should underline the importance of Marx's attitude toward business cycles as an endemic and fundamental phenomenon of the capitalist economy which deserved a detailed treatment. It inspired the work of Tugan-Baranovsky (his influential contributions to business cycles research and exposition of theoretical foundations of Marxism are analyzed in *Francois Allisson's* chapter), Bounyatyan, and later the long cycles theory of Kondratiev and Pervushin.

Apart from being Marxists in theory, several Russian economists were active socialists in politics: Bolsheviks (Bogdanov, Bazarov), Mensheviks (Marschak), or

Social Revolutionaries (Kondratiev) who played an important role in political life in Russia after the 1917 revolution and/or were engaged in dealing with social problems in the countries of their emigration.

Other former Marxists participated in political activity not as socialists: Struve in the Provisional government and Tugan-Baranovsky as the Finance Minister of Ukraine.

Probably, only Baran carried on his work along Marxian lines and developed with Sweezy a new Marxian economic theory of XX century (*Monopoly Capital* 1966).

### 3.4 *Mathematics and Statistics in Russia*

One of the factors, which influenced Russian modification of economic thought and its following spreading in the West, was achievements of Russians in the fields of mathematics (especially, probability theory) and statistics (Belykh 2016; Barnett 2011, pp. 52–55). The reasons, why they occurred in the end of XIX—the beginning of the XX century need not occupy us here.<sup>9</sup> But, their impact on economic theory was undebatable. Some Russian economists took to the heart the logical-mathematical Lausanne tradition, while the Austrian version of Marginalism was relatively more popular in this country. The succession Dmitriev-Bortkiewicz-Charasoff (see the chapters by *Christian Gehrke* and *Heinz Kurz*) is very important from our point of view. Vladimir Dmitriev can be considered as the founder of Russian mathematical economics based on the Ricardian system, Marx's theory, Cournot's analysis, and Walrasian general equilibrium theory. Bortkiewicz, influenced by Dmitriev, carried this line of thinking further in Germany and served as a bridge between Russian and Western mathematical economists (cf. his collaboration with Walras and his activity in Berlin where Marschak and Leontieff were among his students). Not surprisingly, Bortkiewicz got acknowledgment in the West pretty soon, whereas Dmitriev did not until the 1960s. Only then, his work was recognized as a predecessor of Sraffianism and monopolistic competition theory of Chamberlin. We must mention that the influence of Dmitriev and Bortkiewicz was spread among leading Russian theoretical economists. For example, Chayanov opens his principal theoretical work with gratitude to Dmitriev, Zhelesnov, and Pervushin for their advice and especially to Bortkiewicz for indispensable mathematical guidance (Chayanov 1989, p. 70).

The interplay of Marxist and Walrasian approach could be really traced in the history of Russian economic-mathematical thought, but there were very important examples of non-Marxian mathematical economists. The most outstanding one, judging by his present popularity, was Slutsky who did not need any metaphysical foundations like Marxism or hedonism. Slutsky carried on the objectivized Paretian approach to marginal utility theory and developed it in a mathematical way (Slutsky

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<sup>9</sup> One of them could be the introduction of Latin and Greek enforced by reactionary Ministry of Education under Alexander II and Alexander III. Mathematics was also promoted for being abstract enough and so far from political issues.

equation) which retained sound economic sense. His second masterpiece on summation of random causes as the foundation of cyclical processes was grounded on the entirely different branch of mathematics which showed him as one of greatest polymaths in the history of economics (see *Jean-Sebasti en Lenfant's* chapter).

Statistics was developing in Russia at high speed after the Great Reforms which delegated health care, school education, and data gathering to local municipalities (“zemstvo”). In that context, one should not forget the external influence of the German Younger Historical School with its emphasis on gathering and processing statistical data (Zweynert 2002, p. 259). Russian professors, who got their education in Germany under followers of Schmoller, transmitted their knowledge to their Russian students (like Fomin and Levitsky lecturing to Kuznets in Kharkov—see *Moshe Syrquin's* chapter on Kuznets). This was not a general rule—Marschak's teacher of statistics in Kiev was Slutsky with no connections to Schmoller and his followers. However, this statistical background suited well for studies of business cycles and long-term economic growth.

Probably, the main form of Russian impact on Western mainstream economics was provided through the emigration of Russian mathematical economists to the West (Barnett 2008). Their typical life-trajectory often went from revolutionary and post-revolutionary Russia to Germany and then further to the USA. According to Frey and Pommerehne (1988, p. 103), who based their calculations on Blaug and Sturgis's *Who's Who in Economics* (1983), the Soviet Union lost twenty-four of its thirty-six most outstanding economists through emigration. Together with the  migr s from Nazi Germany and the successor states of the Danube monarchy, the USA, on the other hand, gained 161 outstanding economists through immigration, which accounts for about thirty percent of those economists born in the USA.

In Germany, which before World War I was dominated by the Younger Historical school, Russian influence contributed to the development of rigorous economic theory (Janssen 2004). Among Russian  migr  economists were experts in mathematical economics and statistics who got recognition in the West: Marschak, Kuznets, Leontieff, and others. They emigrated from Russia as young men and made their principal contributions in the West, but their education in Russian universities apparently was a good background. Marschak was the central figure in this process due both to his scientific works and impressing organizational activities. His work in the Econometrics Society and the Cowles Commission (see *Robert Dimand's* and *Harald Hagemann's* chapter) played the central role in reorienting economic theory and influencing the subsequent development of economics in the whole world. Nobel Prize winners Kuznets and Leontieff made lasting contributions to statistical methods of economic theory (see chapters by *Moshe Syrquin* and *Harald Hagemann*). The chapter by *Natalia Makasheva* shows us that Kondratiev could have also contributed to the new research agenda of economics put forward by the Econometric Society if his fate gave him a chance.

Important Russian economists, using mathematical and statistical tools, who stayed in Russia faced a gloomy fate. They were either executed under Stalin's regime (e.g., Kondratiev and Chayanov) or changed their main occupation for a less

dangerous one (Slutsky, who became a pure mathematician) (see chapters by *Natalia Makasheva, Carol Leonard, and Jean-Sebastien Lenfant*).

### 3.5 *Designing and Building a Planned Economy*

According to Colin Clark, “it is a disaster for the idea of Planning that Russia should have been the country where it has first been tried” (Barnett 2004, p. 1). However, it was the case, and Russia got the unique experience of designing and building a planned and centralized economy. Marx had left no concrete indications how to create such an economy, so everything had to be invented from scratch. Economists of other countries were interested in this experience. Here, we can name the economic growth model by Feldman created in 1928 for the general 15-year plan for national economy and based on Marx’s reproduction schemes. It was subsequently discovered by Domar and elaborated in works of Lowe and Dobb (see *Harald Hagemann’s* chapter on Feldman). Then, there are Bogdanov and Bazarov’s works which could have contributed to methodology of planning; Strumilin’s work on maximization of a social welfare function. Leontief also began his work on input–output models in the context of Soviet planning. The theory of linear programming, put forward by Kantorovich and highly appreciated in the West, was also a response to the needs of the Soviet planned economy. But speaking about these works, we should not forget that the majority of them were turned down by the ruling Soviet circles which were not always interested in welfare of the citizens and felt suspicion against any non-Marxist proposals. Some inventions were, however, realized. But, the Iron Curtain kept these ideas secret from Western economists who understandingly came across the works of their Soviet colleagues with great tardiness and acknowledged their achievements in retrospect. (See the chapter by *Michael Ellman* on Kantorovich and the chapter by *Harald Hagemann* on Feldman.)

Speaking about the general attitude of Russian émigré economists to the developments in Soviet economy, we have to state the certain sympathetic interest in majority of cases which often changed for criticism as far as the new previously hidden information about the Soviet Union spread over the world (the case of Kuznets). It is interesting to mention that in the famous debate on economic calculation under socialism, such Russian mathematical economists as Marschak were on the socialist side (see Hagemann 2019).

Some of our heroes developed in emigration theoretical models of Russian economic and social development (see the chapter by *Andrei Belykh* on Gerschenkron) or served as experts in Sovietology (Gerschenkron, Domar, Baran), others deliberately distanced themselves from their Russian past (Leontief).

## 4 Transmission Mechanisms and Mutual Influences

The list of factors determining Russian specificity is not constant and stable. Their importance changed with time. On the early stages (beginning and middle of the XIX century), the mental ethical factors predominated, toward the end of the XIX and the beginning of the XX century, when Russian economists reached a degree of professionalism, the Marxist influence and mathematical-statistical bend became more important, and the experience of designing and building a planned economy, naturally, came to the fore after October 1917.

The transmission mechanisms between economists of different countries consist of studying at foreign universities, reading literature in foreign languages, translations of foreign literature, and personal contacts (now, mostly, through conferences, but previously through emigration). The period of intensive connections between Russia and the West we are covering in this volume is not very lengthy and extends from the 1890s to 1920s. In these decades, Russian economists were educated mainly in domestic universities, but the knowledge of foreign languages: German, French, and English in order of frequency was widespread among Russian researchers so that they could not only read foreign texts but also write in foreign languages for Western journals and publishing houses. Tugan-Baranovsky, Struve, Bortkiewicz, Chayanov, Kondratiev, Slutsky, and others published their works abroad and so could perform the feedback from Russian economic thought to the Western one. These connections were strengthened by personal contacts during their scientific trips abroad. This situation existed till the end of the 1920s.<sup>10</sup> Since then, the direct connections with foreign colleagues were no longer possible, and contacts and mutual influences could be performed only through Russian émigré economists, mostly in Germany and the USA. The reception of “bourgeois” Western economists in the Soviet Union is a very special and interesting topic which cannot be dealt with here, but apparently in Soviet times, connections between Russian and Western scholars were negligible. Not only Western but also the leading Russian economists who are heroes of this book were eliminated from Soviet textbooks and reprimanded in critical articles as “bourgeois” “apologetic” authors. We must stress that in that time, émigré Russian economists were considered even more suspicious or dangerous than their colleagues who were born in Western countries. In this time, we can mention only several retrospective discoveries of Russian economists by Western scholars (Slutsky by Allen and Hicks, Feldman by Domar, Kantorovich by Koopmans, etc.) which attracted the attention of international economic community to the forgotten Russian scholars. This can hardly be called “influence”, but that was the way the great Russian economists who stayed in their homeland eventually entered the hall of fame of the history of economic thought. The authors of our volume give them full credit.

Translations of Western economic texts into Russian were a relatively well-developed industry.<sup>11</sup> We have already mentioned that Russian was the first language Marx’s *Capital* Vol. I was translated into. But, other directions of economic thought

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<sup>10</sup> See, for example, Seraphim (1925).

<sup>11</sup> See Latov Y. V. Thesis Vol. 1 (1993).

were also well represented. German Historical school (mostly Werner Sombart) and Austrian school (mostly Eugen von Boehm-Bawerk) were often translated and published by Russian publishers. However, among the founders of the Marginal revolution only Carl Menger was translated (both his theoretical and methodological volumes), whereas the classical texts of Walras and Jevons had to wait until the XXI century to be accessible to mass Russian audience. But, professional economists of the period indicated had no problems to read these works in original languages.

The difficulties arose with Western economists' access to Russian economic texts. They could get acquainted only with those written in foreign languages and published in Western journals. Sometimes, Russian authors living in Russia succeeded to get attention of the international economic community in this way (Tugan-Baranovsky, Chayanov, and, of course, Bortkiewicz), but in other cases, the situation was much worse (see, for example, Slutsky) and the respective texts had to wait many years before being "rediscovered".

We should also mention some touchy national issues. Our volume is about *Russian economic thought*. We have included economists who were born and studied in the Russian empire and the post-revolutionary Russia (since 1922 the Soviet Union). Some of our heroes were born in the parts of this country which belong now to Ukraine, Belarus, and Poland. A lot of them were Jews born in Ukrainian, Belorussian, and Polish towns or villages belonging to the so-called Pale of Settlement where Jewish residence was permitted. Restrictions for Jews concerning the place of their residence, acceptance to schools and universities, not to mention pogroms, made the young generation of Russian Jews adhere to Socialism and actively participate in the revolutionary movement. Some of them had good abilities in mathematics and got education in commercial and technical institutes. These factors contributed a lot to the formation of Russian Jewish economists who achieved international fame. Anyhow, Russian was the language all our heroes spoke and wrote. The degree of their Jewish or Ukrainian self-identification was different and sometimes very important for their personalities, but mostly not for their economic ideas.

We never planned our list of Russian economists known in the West to be exhaustive. But gradually, we realized that the interest of potential participants was great and our list became more and more impressive. Now, we do not feel at ease because we have missed several persons of importance like Bulgakov, Bilimovich, Bounyatyan, Vainshtein, Voitinskii, and many others. There are several reasons we did not include them in our list, but the main and, probably, sufficing reason is we have not found an author to write on them. So, we acknowledge that the blame is fully ours and hope that sometimes (may be, if a Russian edition of this volume will be realized), we will enhance the scope of our work.

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# West-Russia-West: Early Interaction in Economic Thought. Cases of Storch and Chernyshevsky



Vladimir Avtonomov

The interaction scheme “West-Russia-West” described in the Chap. 1 of this Volume and consisting of the adaptation of Western economic ideas by Russian economists and transmitting them to the West in a modified form is encountered mostly after 1890s when the Russian economic science reached the stage of maturity. Before that time Russian economic thought was largely embedded in a broad stream of non-specialized social ideas. This syncretism of Russian social thought was frequently mentioned in the literature. At that stage we can hardly find examples of Russian economists having an impact on their Western colleagues. However, we’ll try to analyse two possible sequences from that epoch: Smith–Storch–List and Haxthausen–Chernyshevsky–Marx.

## 1 Smith–Storch–List

Under Catherine II Smith was apparently considered as an important figure of European Enlightenment which came to Russia mostly through French thinkers. Catherine sent two young Russians Ivan Tretyakov (1735–1776) and Semyon Desnitsky (1740–1789) to Glasgow to attend Smith’s lectures. After 1789 when the French spiritual

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influence diminished, Smith's influence survived. In early years of Alexander's I reign which were marked by the rise of liberal ideas, Smith became popular in Russia mostly as a prophet of freedom, both political and economic. The "Wealth of Nations" was translated into Russian in 1802–1806 and published at the cost of the state! In 1803, Say's "Traité" was published and Smith's ideas in Say's rendering became accessible to Russian French-speaking public (nobility). Under Alexander I political economy became fashionable in universities, literary magazines and even in Czar's family where Storch taught two Royal Princesses and then the Great princes Nikolay and Mikhail. In 1804, political economy and statistics were included in University curriculums. It so happened that Smith's ideas were presented to the Russian public mostly by German professors. Germans were always considered by Russians to be exemplar foreigners. The very word "nemets" (Russian word for a German) literally means "dumb"—a person who can't speak Russian. A lot of Germans served in Russia as scientists, military men, doctors, craftsmen and civil servants of different ranks including the highest (Ministers, generals, etc.). Thus European ideas (including British ones) most often came to Russia through educated Germans. And more importantly still, from the beginning of nineteenth century a large proportion of Russian *intelligentsia* were educated in German universities.<sup>1</sup>

As is well known, Wilhelm Roscher even spoke about The "Russian German school", including among others Heinrich von Storch, Christian von Schlözer (1771–1831), Ludwig Heinrich von Jacob (1759–1827) and the longstanding Finance Minister Georg (Yegor) Cancrin (1774–1845). In fact, the list of German professors and statesmen active in Russia could be easily extended. Roscher's statement was certainly an exaggeration, because such a school, strictly speaking, didn't exist, but there was something important connected with the fact that main teachers of political economy in Russia were German. The first Russian textbook in political economy (1805–1807) was published in German, Russian and French variants by Schloezer, who had the first Chair of political economy in Moscow University. German economists had already adapted Smith's doctrine to the realities of less developed economies of German states and inherited a lot from German Cameralism containing detailed descriptions of good state policies (Staatswissenschaftskunst). German economists preferred an organic, not a mechanical image of society. The next stage was to adapt classical political economy to Russian circumstances, still more distant from English origins. German professors could easily point to the relativity of English classical school using the example of Russia.<sup>2</sup>

The most prominent representative of German Russian economists was Heinrich (Andrey) von Storch (1766–1835)—the first Russian economist, who was known in Western Europe, praised by MacCulloch, published (without the author's permission) and criticized by Say (Storch 1823). "Storch, who in the Russian literature is usually referred to as 'Andrei Karlovich', was a mediator between Western Europe and Russia" (Zweynert 2004, p. 525). In Western histories of economic thought he is considered as a German economist (Rentrup 1989, S. 3), whereas Russian histories

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<sup>1</sup> For more about the German influence on Russian economic thought see Shirokorad (2005).

<sup>2</sup> This issue was first mentioned by Roscher (1874, S. 791–792).

count Andrey Karlovich Storch as a Russian one. His whole career developed in Russia and his other works (excluding the rejoinder to Say's critique) are dealing exclusively with Russian topics.

His main work, "Cours d'économie politique" (Storch 1815) was written and published in French and Czar Alexander I paid the printing costs. In fact, French was the language in which he taught Great Princes. These lectures actually built the foundation of the Cours.<sup>3</sup> The Cours was divided into two parts. The first one was devoted to the theory of national (material) wealth. Here Storch was mainly following and partly citing Smith and Say,<sup>4</sup> but not in all issues. For instance, his theory of value was not labour-, but utility-based. But we will concentrate upon the second part devoted to the so-called theory of civilization.<sup>5</sup> This part contains Storch's main original input in theoretical economics—his theory of "internal" non-tangible goods, consisting of three groups: physical goods (health, skills), spiritual goods (knowledge, aesthetics), moral goods (morals, religion). Besides these principal internal goods, there are two auxiliary ones—security and leisure (Storch 1815, p. 11).<sup>6</sup> These goods can't be bought or sold, but the labour producing them can be bought and sold in form of services. This is Storch's original theory, we can't find any borrowings here. Smith and Say approached the topic we may now call human capital, but their treatment was rather materialistic: they stressed analogy between material resources and human skills. Schloezer went further and had a broader conception of non-material resources (Zweynert 2004, p. 530). But the highest point was reached by Storch. This theory became a remarkable feature of Russian version of classical economics in 1840s–1850s (Zweynert 2002, S. 71). Storch attempted to enrich political economy with the analysis of non-material goods and factors and directly opposed Smith's division of productive and nonproductive labour, which was based upon the distinction of material and non-material goods. Storch considered the labour creating internal goods as productive.<sup>7</sup> But most important was his emphasis on inner goods as a component of national prosperity. These goods can also be accumulated and transformed into capital like the material "richesses" (Storch 1815, p. 20). Storch's theory of inner goods is not confined to basic definitions, it is developed on a large scale and non-material side is never forgotten. The motives for providing services include besides material benefits also moral ones: want of esteem and honour, love, virtue and duty (Storch 1815, p. 39). For instance, a remuneration for services of members

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<sup>3</sup> Later it was translated into German and published by Rau in 1819. It was never translated into English. The Russian translation of the first volume was performed by Ivan Vernadsky in 1881.

<sup>4</sup> This practice caused the angry commentaries of Say. We can explain it by pedagogical goal of the Cours. Storch didn't write his own version where the classical texts didn't need improvements and corrections.

<sup>5</sup> The French term "civilisation" was translated in other languages as "social education" ("gesellige Bildung" in German, "образование" in Russian). But the original meaning of the term was much broader.

<sup>6</sup> The basic internal good is security. Probably one reason for this is that the addressed "audience" will by definition take care of national security.

<sup>7</sup> By the way, according to Storch, for producing inner goods we need not only the producer's but also consumer's labour (student's, patient's, spectator's, etc.).

of Russian Senate consists mostly of honour, because the modest pecuniary award could be only supplementary (Storch 1815, pp. 90–91).

Summing up, “what Storch’s theory of internal goods is dealing with is the connection between cultural and economic development” (Zweynert 2004, p. 532).

It is difficult to assert that Russian environment exerted an important influence on Storch’s theory of inner goods. Storch was born into a German family (though Riga already was the part of Russian empire at that time), studied at German universities (Jena and Heidelberg) and besides Smith’s doctrine, received a German cameralistic education.<sup>8</sup> Among German predecessors of his idea of internal goods we can probably name von Soden and Hufeland, but Storch didn’t mention them as such and didn’t seem to be influenced by them. On the contrary in the very beginning of the Part 2 devoted to civilization theory he is talking about beginning an entirely *new science*, for which a lot of material was gathered by the Old and New economists, but which requires coordination and systematization. The author is setting a goal of outlining the principal lines of this new science (Rentrup, S. 55).

Storch didn’t seem to find inspiration in specific Russian circumstances. In fact, in the foreword he underlined that in such a country as Russia political economy (the classical one, of course) can also be applied. He wanted to set right goals before future governors of the Empire—Great Princes. As Zweynert underlines, the theory of inner goods was an attempt to set directions along which Russia can catch up with more civilized Western European countries (Zweynert 2002, S. 106).<sup>9</sup> Probably the most important issue is the detrimental role of serfdom which is underlined in many places of the book in connection with different issues: fertility, safety, morals, etc. A special chapter deals with the influence of slavery upon civilization, and this influence is declared to be negative. It is handled on a theoretical level, and the examples given are mostly not Russian. But one can unmistakably feel what the author is trying to convey to his Royal students. For instance, Storch mentioned that a serf-peasant had a chance to be happy, but not a serf-industrial worker. This a clear allusion to Russian realities.

Another specific Russian detail worth mentioning is that Storch saw the favourable aspect of Russian reality in the predominance of agriculture in relation to industry. We should bear in mind that industry he saw was in an early stage of development characterized by long working day, children’s work, etc. and didn’t look attractive to contemporaries. A peasant, working in the open air, possessing various productive skills and not one primitive skill of the specialized industrial worker (and being personal free—let us not forget the previous point!) had many advantages including moral ones (Storch 1815, p. 163) in comparison with alienated (the Marx’s term is in place here) industrial worker (Storch 1815, pp. 110, 135). Storch’s slogan could have been “Every progress is reactionary if it destroys the Man” (a citation from Soviet poet Andrey Voznesensky (1933–2010)). He investigated, what happens to each internal good with the transition from agricultural stage to the industrial one, and found that

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<sup>8</sup> Georg Sartorius (whose work was also translated into Russian in 1796) could be mentioned among his forerunners (Zweynert 2002, S. 102).

<sup>9</sup> By the way, one of such important directions was to enhance the knowledge of foreign languages.

in some cases, especially health, skills and morals, the situation is going to get worse (Storch 1815, pp. 106–136, 161–184). On the other hand, aesthetics and knowledge grow in industrial era. Thus, the theory of internal goods led Storch to reconsidering the growth of civilization and stress the advantages of agricultural stage Russia was experiencing. Even in foreign trade, as Storch argued, industrial products tend to grow cheaper while the prices of agricultural products increase (he used the example of Ireland). Storch used, though not very often, examples of different countries, illustrating his theoretical statements. But Russia is mentioned less frequently, than, for instance, England, though in his other works Storch compiled and published a vast collection of facts and figures on Russia. Probably, more plausible is Zweynert's thesis that German states and Russia had much in common both economically and mentally with Russia lagging behind.

The influence of Storch upon Russian economists of next generations and their estimates of his work is profoundly analysed by Dmitriev (2005). Our task here is to consider a possible feedback from Storch to the Western thought. Schloezer and Storch are considered by some authors (among them, Roscher) as forerunners of the German Historical school (Zweynert 2002, S. 81). Their works are believed to contribute to transformation of Classicism into Historicism in Germany. A good illustration of this transformation is provided by Storch's description of subject matter of political economy in the end of introductory chapter of his *Course*: political economy is based on study of man and people. "It is necessary to research the human nature, state and fate of society in different places at different times, to consult the historians and travelers, not only laws and customs but also the way they are realized ..." (Storch 1823, p. 36). Storch explicitly doubts any universal economic laws which are independent from concrete historical phenomena (Rentrup 1989, S. 14). In Book 8 of the *Cours* he outlines a schema of subsequent historical stages which is so characteristic for List and Historical economists.<sup>10</sup> In fact, in order to produce "internal goods", a nation should achieve a stage, where the "external goods" (food, clothes, etc.) are already produced in sufficient quantities (Rentrup 1989, S. 57). The founder of German Historical school, Wilhelm Roscher considered the "German-Russian school as an antecedent of Historical school" (Rentrup 1989, S. 54), though he didn't accept Storch's theory of inner goods and criticized it for dissolution of economic issues in general considerations (Rentrup 1989, S. 128; Zweynert 2004, p. 538).

Zweynert discovered that Storch's list of internal goods literally coincides with the list of productive forces compiled by List in his "National System of Political Economy" (Zweynert 2002, S. 84). This indicates that List was influenced by Storch in the essential part of his doctrine. This doctrine also was not academical and was directed towards making German states catch up with world industrial leaders. But List and Storch had quite opposite political views. Storch was a consequent free-trader and supported a kind of gradual industrial development of Russian economy using agriculture as the starting point because of comparative advantage which Russia

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<sup>10</sup> He was, actually, not the first to undertake such an attempt. We can find such stages already by Turgot (Rentrup 1989, S. 23).

possessed in this sector (Rentrup 1989, S. 51).<sup>11</sup> In this respect as in many others Storch remained true to the classical school, the member of which he is considered by major historians of economic thought (Rentrup 1989, S. 54).

To our mind this can be considered as the first example of feedback influence of Russian economic thought on Western European one. Another Western economist who is believed to be directly influenced by Storch was a French liberal Charles Dunoyer (1786–1862). In fact, Dunoyer mentioned Storch in a footnote, where he acknowledged that Storch rightly criticized Say for mixing the labour, which produced non-material goods, with its products—the goods themselves. Storch did it 12 years before Dunoyer, but Dunoyer claimed that he didn't know about Storch's book before he came to the same conclusion. Dunoyer asserted that Storch did not make good use of this distinction (Dunoyer 1846, p. 226 f.2). So, if we give credit to these words, we can't speak about a direct influence of Storch on Dunoyer. However, Dunoyer devoted a lot of place and efforts to describe and analyse cultural factors of economic life. In Britain Storch was not well received because the British economists, at least since Ricardo, worked on a more abstract level, than Continental ones. The lasting impact of Storch's theory of internal goods (partly through Dunoyer) was achieved only in Russia (Zweynert 2004, p. 537).

One can discover economic ideas resembling Storch's internal goods by more recent authors. Something like Storch's conception of "division of knowledge" which is related to internal goods can be found by Hayek (Schumann 1997, S. 176). Among modern economic theories we can probably mention human capital theories, new growth theories, development economics. Some inner goods by Storch are produced by households which sounds like Kevin Lancaster's and Gary Becker's theories of consumption (Rentrup 1989, S. 108ff). But all these authors were not influenced by Storch directly, and some of them hardly knew about his existence. So, von Storch could have occupied a more impressive position in the history of economics than he has now.

## 2 Haxthausen–Chernyshevsky–Marx

August von Haxthausen was widely believed to be a discoverer of Russian obshina. His "Studien über die innern Zustände, das Volksleben und insbesondere die ländlichen Einrichtungen Russlands"<sup>12</sup> published in Germany in (1847–1852) after the 12 month-trip through different regions of Russia in 1843–1844, which was financed and supported by Nicholas I<sup>13</sup> was the first systematic and rather sympathetic description of this institution, which became the central issue of discussions

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<sup>11</sup> Probably, this was the reason why List didn't acknowledge Storch's influence on his theory (Zweynert 2004, p. 534).

<sup>12</sup> See Haxthausen (1972). This is an abbreviated one-volume English translation.

<sup>13</sup> An interesting point is that besides obtaining information on Russian agriculture and recommendations considering its possible reforming, Nicholas I also planned the publishing of Haxthausen's

in Russian social thought of subsequent decades. There are some debates about the priority of this discovery.<sup>14</sup> Haxthausen himself wrote that he came across this issue while describing organization of agriculture in Prussia in 1830 (this investigation was financed by the other monarch, Friedrich Wilhelm IV of Prussia), where he found some peculiarities in Prussian regions formerly inhabited by Slavs. In fact, Haxthausen was acquainted with Polish literature about the obshina before his trip to Russia (Svyatlovsky 1923, pp. 180–181). Certainly, he also could have had some preliminary talks with Russian Slavophiles in Moscow who discussed obshina. Goehrke wrote that Haxthausen discovered the obshina as the crucial institution of Russian social life without any influence of Slavophiles (Goehrke 1964). Among Russian historians there are different views on this issue. Shtein, of course, postulated the opposite sequence: Haxthausen borrowed the views of Russian Slavophiles which at that time were not reflected in any publication (Shtein 1948, p. 116). But several pages later he mentioned that Slavophiles originally treated obshina as an ethical problem and formulated a corresponding social theory only in 1847–1852 (Shtein 1948, p. 121)—that is after the publication of Haxthausen’s work. This debate, however, doesn’t change the main fact that this work was the first empirically founded scientific source upon which any further discussion could be based. Haxthausen was not a professional economist, but he certainly had a great practical experience of managing for 12 years his family estate in Westfalia where the abolition of serfdom took place just in this time-period. As for his research methods, they are characteristic for a representative of the Historical approach.<sup>15</sup> We should mention his friendship with Grimm brothers, one of whom (Jacob) taught him law in Göttingen along the lines of Historical school of law headed by their friend Karl von Savigny. Like his friends from the Romantic school Haxthausen collected folk songs and fairy-tales, and one of the courses he took in Göttingen was called “The man in his organic entirety”. His adherence to statistical method also points in the direction of Historicism.

Haxthausen saw in Russian obshina with its regular redistribution of land sites between families and communal organization of agricultural works a chance for Russia to escape proletarianization and social unrest. At the same time, he was “practical” enough to estimate the obshina as a factor retarding technological progress.

Haxthausen’s book was allowed to circulate in Russia in German and French versions but the translation into Russian was prohibited until 1870 (and even then several critical passages were omitted).<sup>16</sup>

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book as a certain antidote for European audience to the recent critical publication of Astolphe de Custine’s “La Russie en 1839”.

<sup>14</sup> This question is analyzed in detail by Zweynert. See Zweynert (2002, S. 202).

<sup>15</sup> The accepted tradition is to classify as main members of the old Historical school Wilhelm Roscher, Bruno Hildebrand and Karl Knies. I can agree with this as far as the academic economic theory is concerned. But, for example, the not purely academic List’s “National System of Political Economy” (1841) could be also called a manifestation of Historical approach which appeared earlier than Roscher’s works.

<sup>16</sup> The following passage is based on: Avdeeva (1998).



After the publication of his travel account Haxthausen was generally considered an expert on Russia and its agriculture. He never ceased to be engaged in Russian reforms in intensive collaboration with the Great Princess Helene of Russia, participated in the discussion and wrote several notes which were handed to Alexander II (Stoyanoff-Odoy 1991). In 1857 he published in “*Russky vestnik*” two articles on agricultural reforms connected with abolition of serfdom in Prussia and Austria to make the Russian public familiar with Western European experience. In the discussion Haxthausen’s position was closer to the “liberal camp”, he advocated the liberation of peasants with the half of their land free of charge. According to him, main actors in post-reform Russia should be obshinas and responsible noble landowners, but not Russian bureaucrats, on whom his opinion was not much more favourable than that of Marquis de Custine. How to avoid these middlemen under autocracy of Russian czars, remained unclear. In 1866 a new book in German was published by Haxthausen and his associates provided by the Great Princess Helene to give the European public a detailed account of Russian agricultural reforms (Haxthausen 1866). A special chapter of this book is devoted to Russian obshina which he recommends to preserve despite its technological backwardness because of its political importance and as a barrier to proletarianization of peasants.

Haxthausen’s book propelled general discussion on obshina with Nikolay Chernyshevsky as an important participant. The discussion started right after the publication of Haxthausen’s book, but Chernyshevsky joined it with some delay. He was not an economist but a philologist by education, a prolific and energetic journalist of radical views who became the main figure in influential “*Sovremennik*” magazine. He began to deal with economic problems only in 1857. But this very talented autodidact very rapidly became known as one of the leading Russian economists. His main economic work was the translation of John Stuart Mill’s “*Principles of Political Economy*” with extensive commentaries in which he tried to draw socialist conclusions from Mill’s theoretical work. The second direction of his thought was devoted to the problem of obshina. In the beginning his treatment of this problem was broadly ethical, a combination of Slavophile and socialist ideas: “A single man, becoming independent was left in a helpless state ... After the idea of the rights of a single personality an idea of union and brotherhood emerged ... In the agriculture it must be expressed in the transition of land in common use, in industry—in the transition of factories into common use of their workers ... let us not dare to touch the holy, salvatory custom, which we inherited from our past, let us not dare to encroach on obshina use of land...” (Chernyshevsky 1857a). In fact, Chernyshevsky combined materialism with a dose of Christian-orthodox ethics which couldn’t be a surprise taking into consideration his religious upbringing and education. Here we may refer to his “expressed anthropocentrism” and treatment of political economy as a normative science.

But in the polemics with the liberal Professor Ivan Vernadsky, the editor of “*Ekonomicheskyy ukazatel*” Chernyshevsky used the materialist economic discourse and applied to the authority of Haxthausen as an expert and impartial researcher. In 1857 he published a big article in “*Sovremennik*” (Chernyshevsky 1857b), exposing Haxthausen’s work of 1847 very extensively with his commentaries (these excerpts

were actually the first Russian translation of a considerable part of Haxthausen's first volume!), making some corrections, and praising the book in the whole as a source of reliable information. In this article Chernyshevsky proposed Russian obshina as a more favourable transitory stage to large-scale industrialized agriculture than small private farms of West-European peasants. In this article he didn't glorify the obshina for its own sake like in the notes cited above. According to him Russia was just lucky to be backward enough, so that this obsolete form of organization survived. Subsequently, when machines will be used in agriculture, obshina will promote the graduate transition to socialism in agriculture. In his commentaries Chernyshevsky referred to Haxthausen as a representative of a country with privately owned farms, who, nevertheless, as a practical man was impressed by the practicality of common ownership in Russia.

In another publication in "Sovremennik" (Chernyshevsky 1858) Chernyshevsky approached the obshina question from the philosophical standpoint using a kind of Hegelian dialectics. After the first nomadic stage with communal property the growth of population creates the need for intensification of agricultural production, capital investments and private property. But on the next stage the concentration of production will lead to the return to communal property. So Russia had a possibility to skip the second stage (Chernyshevsky 1858, p. 388).

Already in the end of 1858 Chernyshevsky began to doubt the protecting role of obshina, but continued to believe in it as a way to transition to socialism. After the reforms of 1861 he seemed to drop the slogan of obshina altogether and concentrated on the struggle against autocracy (Gurevich).

Working on the first edition of Capital Vol. I, Marx was very sceptical about Russian supporters of an obsolete institution of obshina, including Alexander Gercen (Herzen). He noticed that Gercen's views were influenced by Haxthausen, but apparently was not familiar with the latter's work at that time (Vada 2018). Marx shared the Young Hegelians' antipathy to the Romantic school to which Haxthausen belonged, and believed that Haxthausen was duped by the Russian authorities and the peasants trained by them (White 2019, p. 7). At that time Marx firmly believed that the remnants of primitive communal property could be found all over the Globe and were progressively eliminated by capitalist development. He was confident that the backward countries could make their way to socialism only with the help of the proletariat of advanced Western countries winning the socialist revolutions in their countries. But in 1870s his position was changing. Researches into the history of land-holding he was studying "suggested that peasant communal ownership was far more resilient than had previously been supposed" (Steedman Jones 2017, p. 570). These studies which were "part of a mainstream development in German and Anglo-Saxon culture" (Steedman Jones 2017, p. 578) were originally concentrated upon the German Mark, but the case of Russian "obshina" became especially important for Marx because of its political potential.

Marx learned Russian and urged Engels to do the same to read the non-translated works by Chernyshevsky on obshina (White 2019, p. 12) and V. Bervi-Flerovsky (1829–1918) on the plight of the working class in Russia. Marx first learnt about Chernyshevsky in 1867 from N. Serno-Solovyevich (Steedman Jones 2017, p. 579).

Very important was his contact in 1870 with German Lopatin (1845–1918)—a member of the First International and the first translator of “Capital” into a foreign language<sup>17</sup>—who was an admirer of Chernyshevsky’s works.<sup>18</sup> Marx was favourably impressed both by Chernyshevsky’s commentaries on Mill and his works on obshina. We don’t know precisely which Chernyshevsky’s article on obshina Marx read, but this reading obviously became the turning point in his attitude towards Russian obshina (Vada 2018)<sup>19</sup>. In Chernyshevsky’s socialist version the argument of Haxthausen about the viability of Russian commons seemed to Marx irresistible (Steedman Jones 2017, p. 579). Chernyshevsky’s use of Hegelian dialectics with common ownership of land first giving way to the private one and later coming back on a higher level of development may have also played a role. We can also assert that Marx closely followed Chernyshevsky in his interpretation of emancipation of Russian serfs (White 2019, p. 16).

The other reason which made the Russian obshina so attractive for Marx was the political one. In 1870s the situation in Western Europe didn’t look promising for a socialist revolution anymore. Russia, especially in the case of a possible defeat in Russian–Turkish war could explode and in the predominantly agrarian country the obshina could serve as a foundation of collective socialist production.<sup>20</sup> Accordingly, Marx changed the preface to the second edition of “Capital” (1873), inserted the praise for Chernyshevsky as “the great Russian scholar and critic” (Marx, p. 15) and eliminated the principal thesis that all the countries should undergo the process of primary accumulation of capital before they could aspire for socialist future. This change in Marx’s position was reflected in his letters, and especially in drafts of the letters to the editor of “Otechestvennye zapiski” (Marx 1878) and Vera Zassulich (Marx 1881). In the first draft of the letter to the editor Marx referred once again to “the great Russian scholar and critic Chernyshevsky” and repeated his thesis which was formulated in Chernyshevsky (1858). Marx wrote to Zassulich that the fact that peasant communes had disappeared in Western Europe didn’t mean that the same would take place in Russia (White 2019, p. 41). In the draft of the letter to Zassulich Marx thus came to refute his earlier beliefs and revised them insisting on the principle that no theory can be applied universally. It turned out that the influence of Chernyshevsky was the main reason why Marx changed his view on obshina and supported not Russian Marxists but the populists in their dispute (Steedman Jones 2017, pp. 580–581).

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<sup>17</sup> With the exception of Mikhail Bakunin who dropped this project in the beginning.

<sup>18</sup> At that time Chernyshevsky was already in exile in Siberia, where Lopatin somewhat later tried to set him free, dropping his work on translation of “Capital” which had to be completed by Danielson.

<sup>19</sup> Shtein tried to prove that Marx had a favorable opinion regarding Russian obshina (regarding it as an institution which mitigated poverty) even before his acquaintance with Chernyshevsky’s works (Shtein 1948, p. 228), but this argument doesn’t look persuasive. Marx could be hardly accused for having such a kind-hearted position in any political or theoretical question.

<sup>20</sup> This position is amply reflected in the Preface to the Russian edition of “The Communist Manifesto” in 1882: “If the Russian Revolution becomes the signal for a proletarian revolution in the West, so that both complement each other, the present Russian common ownership of land may serve as the starting point for a communist development” (Marx and Engels 2002, p. 196).

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The first examples of West-Russia-West connection, which we described above could be summarized in the following sequences.

- I. Classical political economy (Smith, Say)—Russia (Storch)—Historical economic thought (List).
- II. Historical economic thought (Haxthausen)—Russia (Chernyshevsky)—Marx (though not Marxism).

In the first sequence the Russian economist Storch transformed the Classical theory in a more spiritual version including internal goods, which was inherited by a Historical economist (List), though not an academic one. In the second sequence the Historical empirical approach to economic reality was used by the Russian thinker and activist Chernyshevsky to build a hypothesis about historical evolution of Russian obshina, which Marx, in his turn, found promising for a future revolutionary change of the existing order. The stories these sequences are telling us are different. All the members of the second sequence certainly didn't aim at improving economic theory. Social and political problems in the turbulent times were really important for them, though Haxthausen wanted to avoid turbulence, Marx—to increase it, and Chernyshevsky in 1857–58 was probably moving from the first to the second position.

The first sequence points in the direction of making economic theory less abstract, embracing an array of inner goods, classical economists abstracted from. But this was naturally done not for the theory alone but for the technical, moral and social progress of lagging countries Storch and List were promoting.

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# Tugan-Baranovsky and the West



François Allisson

## 1 Introduction

Western thought, unlike other branches of science and culture, has not taken into account the development of economic theory in Russia. One can boldly assert that, in the field of economic theory, M. I. [Tugan-Baranovsky] was the first to force European thinking to pay serious attention to its movement in Eastern Europe and in Russia. [...] Not only did he become on a level with the epoch and on a level with the scientific economic thinking of advanced countries, but he was also able to contribute to its progress and, by virtue of this, he, more than anyone else enabled Russian economic science to be placed on a par with that of Europe.

(Kondratiev [1923] 1998, p. 337)

Nowadays, the name of Tugan-Baranovsky is associated with one of the most famous Russian economists, renowned for his developments in the field of crises and cycles theories. But where does this odd last name come from? According to a family legend, the Tugans were an old Tatar family—that some hoped to go back directly to Genghis Khan—settled in Poland, and from at least the fifteenth century were serving in the cavalry for the Kingdom of Poland and the Grand Duchy of Lithuania. During the 1410 Battle of Gruenwald, Tugan-bek, the chief of the Tatar cavalry engaged against the Teutonic Prussian knights, fell in love with a Polish princess, Rosalia Baranovskaya. She agreed to marry him on the condition that he brought her back the head of a Teutonic general. No sooner said than done, according to the same family legend: the Tugan-Baranovskys were born. The Tatar and Polish noble origins were a pride in the family, but in the nineteenth century, they lost their nobility titles, following participations in the 1830 and 1863 uprisings in Poland,

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including by the grandfather of our economist. Mikhail Ivanovich was born in this Russian family in Solyonoe in the province of Kharkov, in the territory of today's Ukraine, as the son of Ivan Yakovlevich Tugan-Baranovsky, freshly converted to the orthodox faith, having changed his original name, Ibrahim Jakubovich. This story, told and documented by archival proofs by Mikhail Ivanovich Tugan-Baranovsky's own grandson<sup>1</sup> (the episode of the Teutonic head left no official document), gives a rather colourful meaning to the origins of Mikhail Ivanovich Tugan-Baranovsky. He is Russian, but as this story tells, Russia is only a summary of a much more complex story made of exchanges between cultures, religions, languages and geographical places.

This story was therefore told here as a useful reminder that when I will use the broad categories of *Russian* and *West* in this paper, it never targets a pure national idea, as the latter doesn't exist. Yet, having this caveat in mind, Nikolay Kondratiev, the father of long cycles theories, correctly asserted in the epigraph above, that Tugan-Baranovsky was one of the most influential *Russian* economists in the *West*. According to Sorvina, he is even "the first Russian economist with a world-known name" (Sorvina 2005). The Western economic literature is full of appraisals of Tugan-Baranovsky's achievements (most notably, but not only) in the theory of crises and cycles. From Werner Sombart calling Tugan-Baranovsky "the father of the new crises theory" (Sombart 1904, p. 113) to Keynes expressing his "strong sympathy with the school of writers – Tugan-Baranovski, Hull, Spiethoff and Schumpeter – of which Tugan-Baranovski was the first and most original" (Keynes 1930, vol. 2, p. 100), through Spiethoff, Kautsky, Bernstein, Lescure, Aftalion, Mitchell, Cassel, Robertson, Schumpeter, etc., the list is long and impressive.<sup>2</sup> Reconstructing a full list of those Western economists is not what will concern us in what follows.<sup>3</sup>

In what follows, the objective is to observe the circulation of ideas from the West to Russia, and back from Russia to the West, in the case of Tugan-Baranovsky. To this purpose, Sect. 2 looks at the Western impact on Tugan-Baranovsky, in particular on his education as an economist. Then, as a way to observe how these ideas are adapted and transformed in the Russian context, and how they travelled back in the West, two case studies are chosen. The first evolves around the publication of Tugan-Baranovsky's master dissertation on *Industrial crises in contemporary England, their causes and influences on national life* (1894), in Sect. 3. And the second dwells on his book *Theoretical foundations of Marxism* (1905a, b), in Sect. 4.

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<sup>1</sup> Dzhuchi Mikhailovich Tugan-Baranovsky (1948–2015) was a professor of history at the State University of Volgograd (Russia), specialist of the French Revolution, of Napoleon Bonaparte campaigns, and of his own family, including his grandfather Mikhail Ivanovich. See his biographical account of his grandfather (Tugan-Baranovsky 1997).

<sup>2</sup> Such references can be found for instance in Beckmann (2005) or Barnett (2001).

<sup>3</sup> A similar list could be built for Japan, where Tugan-Baranovsky's works were quite early translated and widely discussed. For a bibliographical starting point, see Kojima (1975).

## 2 The West as a Source of Inspiration

In order to study Tugan-Baranovsky's legacy, the scholar has at its disposal a whole range of secondary literature. First, there is a handful of very good quality dictionary and encyclopaedia entries (see, among others, Avtonomov and Makasheva 2016; Nove 2008; Crisp 1968), there are a few bibliographies (especially Amato 1980, 1981), and one can easily find outstanding work of synthesis (like Makasheva 2008; Bogomazov 2006; Howard and King 1990; Nove 1970). There are a lot of dissertations on him (among the earliest, Gringauz 1928; Gotz 1930; Kowal 1965), especially since the 1990s. The secondary literature on Tugan-Baranovsky is huge,<sup>4</sup> and in recent times, it has grown a lot, in the West and particularly in the Russian and Ukrainian literature.

But when one is interested to work in a contextual approach, then it becomes more difficult. There are no personal archives of Tugan-Baranovsky, because both his huge library—about 40,000 volumes—and his personal papers were lost in 1917 during the First World War (taken by the Germans?), when Tugan-Baranovsky moved from St. Petersburg to Kiev, to accept the position of Minister of Finance of the Ukrainian Central Rada. The researcher is then forced to collect scattered materials. A few researchers have dug and found nice archival pieces here and there, like Tatarnikova (1991), Shirokorad (1996) or Tugan-Baranovsky (1997), and we are much grateful for the publication of such collections as *Unknown Tugan-Baranovsky* (Shirokorad and Dmitriev 2008), which contains a lot of materials and especially letters found in the papers of Tugan-Baranovsky's contemporaries.

As Tugan-Baranovsky hasn't left us with an autobiography, many of what we know of his life course and contacts is given to us by his contemporaries, colleagues and especially students (like Kondratiev, Solncev, Klejnborst). It is therefore ordinary that many facts about his life are found contradictory in the various accounts that are published, and every study about Tugan-Baranovsky is necessary an impressionist piece of scholarship. For instance, according to his grandson, who had never met his grandfather, but who reminds the numerous stories told by his father, there were many books in Mikhail Ivanovich's library with dedicated autographs, from people like Werner Sombart, Eduard Bernstein, Karl Kautsky, Bernard Shaw, Georgy Plekhanov, etc. and by just reading the autographs, you could track Tugan-Baranovsky's travels in Germany. Unfortunately, these books are lost.<sup>5</sup>

To take a significant example, it is difficult to know how his interest to political economy was born. It is well known that, as a schoolboy in the second gymnasium of Kharkov, Tugan-Baranovsky read and enjoyed Kant and Dostoyevsky—a Western and a Russian author.<sup>6</sup> Tugan-Baranovsky was chiefly interested in natural

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<sup>4</sup> I have myself contributed to these Tugan-Baranovsky's studies: see for instance Allisson (2011, 2014, 2015, Chaps. 4–5).

<sup>5</sup> Fortunately, the reverse is not always true: Tugan-Baranovsky's books survived, with annotations, within Plekhanov's personal library, as exhibited online at the National Library of Russia in St. Petersburg (see <http://nlr.ru/exib/Lenin/len-baran.html>).

<sup>6</sup> Both authors will remain very important to him, and he will return to them. See Makasheva (2008).



sciences, and after he finished the gymnasium in 1883, he first enrolled at the natural science department of the faculty of physics and mathematics at Petersburg Imperial University. And when he was arrested and then expelled for his participation in a manifestation and in a group of students for anti-imperial agitation, and exiled in his native Kharkov province, he enrolled thanks to the active administrative help of his father at the University of Kharkov in the physico-mathematical faculty, where he graduated in 1888, again in natural science. But, almost simultaneously, he studied as an external student in the law faculty, where his interest for political economy raised. There he wrote a dissertation on “The causes of value” (1889).<sup>7</sup> But this part of his economic education, before he joined the University of Moscow to gain a *magister* degree in 1894, is not well known. We are aware that I. I. Yanzhul played an important role in the formation of Tugan-Baranovsky in Kharkov. But nothing is known, for instance, of a possible role of Tsekhanovetsky, then also professor of political economy at the University of Kharkov.<sup>8</sup>

Tugan-Baranovsky travelled a lot in his life. For his studies, he moved from the province of Kharkov to Petersburg, then was exiled back to Kharkov, then moved to Moscow, and eventually spent 6 months in London in 1892. From there, back to Moscow, then Petersburg starting from 1895, then exiled anew in 1901, in Lkhvica in the province of Poltava (nearby Kiev), in the region of his second wife and of his mother, and then back in 1905 in Petersburg. Finally, a third exile from Petersburg in 1917, the last and the first chosen: Kiev and around, for the last two years of his life. We also know that he travelled in France (where he met his first wife—on the Eiffel Tower!—Lidya Karlovna Davydova), in Italy (after the marriage with his second wife—Olga Fedorovna Rusinova), and we have a lot of proofs of his presence in Germany, especially for his editorial and publishing affairs. So, while he spent most of his time in the Russian empire, he travelled in Western Europe quite often, and at a time in his life, in 1904, he even thought of moving definitely to Germany, where he felt more understood.<sup>9</sup> As Tugan-Baranovsky lived only until 54 years old, it is unknown if he would have settled definitely in the Ukraine, or if he would have exiled himself once again, in Western Europe, as sometimes suggested in the literature.

Tugan-Baranovsky is known to handle several foreign languages. According to his quotation practices, and translation experience, he must have mastered German and English, and have a more passive understanding of French and Italian, at least. About English, the stay in London at the library of the British Museum during 6 months was formative. There, in spring and summer 1892, he gathered materials for his *magister* dissertation on *Industrial crises in contemporary England ...*

<sup>7</sup> So far, it is unknown to me if this work subsists somewhere. It is likely that it provided the basis of his first published work, “Study on the marginal utility of economic goods as the cause of their value” (Tugan-Baranovsky 1890), which introduced marginalism in Russia (see Allisson 2015, Chap. 3; Makasheva 2009).

<sup>8</sup> G. M. Tsekhanovetsky was also one of the teachers of N. I. Sieber two decades earlier in Kiev. On Tsekhanovetsky, see Allisson et al. (2020, pp. 301–302).

<sup>9</sup> As it is apparent from his letters to A. A. Kaufman, at the time he was assigned in Lkhvica, from 1901 to 1905 (in Shirokorad and Dmitriev 2008, pp. 48–112).

(1894), working on the English so-called Blue Books (official documents, often from the Parliament, containing official statistics), published works of parliamentary commissions, various statistics, that he will supplement with researches at two libraries in Petersburg (Tugan-Baranovsky 1894, p. ii). The idea to travel to England to gather materials in the library of the British Museum for a dissertation on political economy about the British economy shall not be taken as something extraordinary. It was even considered standard. To take just two examples, I. I. Janzhul wrote himself a dissertation (published in 1874) on the history of the system of English excises, with materials collected in the library of the British Museum and in the Royal Library of Munich, and A. A. Manuilov published in 1895 a dissertation on the rent of land in Ireland, with stays at the British Museum and in Ireland. What was becoming less typical at the time of Tugan-Baranovsky was the necessity to travel after the dissertation to complete the education abroad, as did many Russians with state-sponsored grants in the 1860s and 1870s.<sup>10</sup>

About German language, Tugan-Baranovsky arrived in 1904 at the conclusion that “In German, I now write quite satisfactorily”,<sup>11</sup> even if he recognized that “It is difficult, because to write in another language is not the same as writing in its own language”.<sup>12</sup> The injunctions to write in German seemed frequent at the time, as Ladislaus von Bortkiewicz, Russian economist and statistician established as an extraordinary professor in Berlin, told in 1904 his friend A. A. Chuprov, still but not for long settled in Russia:

You are right to notice that such works as Dmitriev’s *Essays* in Russia went unnoticed. But this will also apply to your notes in your Institute’s *Izvestia*. My advice: write on such topics [...] in German! [...] And no need to worry about the language. According to my observations, even Tugan-Baranovsky’s German style satisfies the vast majority of readers.

(Ladislaus von Bortkiewicz to A. A. Chuprov, in Sheynin 2005, letter 75)

While many of Tugan-Baranovsky’s works were translated into foreign languages during his lifetime (in German, in French, in English, in Spanish, in Czech, in Ukrainian), Tugan-Baranovsky supervised the French editions of his work, and often translated himself the German translations. Among Western languages, only in German, or almost, did Tugan-Baranovsky had an independent publishing activity. In fact, from the beginning of the twentieth century, his bilingual publications were almost always first published in German, and then translated by him into Russian. In Germany, he collaborated to such journals or encyclopaedias as Werner Sombart’s *Archiv für Sozialwissenschaft und Sozialpolitik*, Karl Kautsky’s *Die Neue Zeit*, Wilhelm Lexis’s *Handwörterbuch des Staatswissenschaften*, or Heinrich Braun’s *Archiv für soziale Gesetzgebung und Statistik* and *Annalen für sociale Politik und Gesetzgebung*.

<sup>10</sup> On the case of A. I. Chuprov, I. I. Janzhul, N. I. Sieber, etc., see Masoero (1995).

<sup>11</sup> Letter no 11 from Tugan-Baranovsky to A. A. Kaufman, in Shirokorad and Dmitriev (2008, p. 98).

<sup>12</sup> Letter no 12 from Tugan-Baranovsky to A. A. Kaufman, in Shirokorad and Dmitriev (2008, p. 98).

The fact that Western authors and ideas influenced him is apparent from his publishing records, where he devoted many studies to individual Western economists. For instance, he published two biographies for Pavlenkov's popular biographical series "lives of remarkable people": on Proudhon (1891) and on John Stuart Mill (1892). Between 1901 and 1902, he published a series of articles in the journal *Mir Bozhij* with dedicated articles on Smith, Malthus, Ricardo, Owen, Saint-Simon, Fourier, Sismondi, Rodbertus, Marx, the Austrian School, etc. that were collected in a successful book, *Essays in the newest history of political economy* (Tugan-Baranovsky 1903, in Russian) that went through several editions. In all, his readings of foreign economists in the original language ranged much more widely than the canons of classical political economy: he was up to date in the latest developments of the German historical schools, of the writings of the second generation of Austrian marginalists, of German and Austro-Marxists, of so-called later utopians (Pecqueur, Thompson), of German Neokantianism (Windelband, Rickert), of psycho-physics (Wundt, Weber-Fechner), etc.<sup>13</sup> His readings were nevertheless more German and English than French or Italian.

Eventually, Tugan-Baranovsky was involved in several translations projects: he supervised in 1896 the translation of Henry George's *Progress and Poverty* into Russian, he translated and introduced in 1897 some chosen texts of John Eliott Cairnes, he translated in 1900 Georges Blondel's *L'essor industriel et commercial du peuple allemand*, he prefaced the 1901 Russian translation of Werner Sombart's articles on the organization of labour, and in 1909 he published an authorized translation, with a preface, of Böhm-Bawerk's second edition of *Kapital und Kapitalzins*, and he prefaced the Russian translation of Karl Vorländer's texts on Kant and Marx. All this shows the significant implication of Tugan-Baranovsky in the importation of foreign, and especially Western thought. These influences are found back in his own works. His popular and award-winning textbook, *Foundations of political economy* (1909), several times re-edited, contains in his bibliographical supplements to each chapter a quite impressive range of influences (also with a subsection containing specifically Russian literature). All in all, what has been said in this section shows how much Tugan-Baranovsky owed to Western thought.

### 3 The Reception of *Industrial Crises in Contemporary England*

It is the normal fate of academic business that the intentions of an author in terms of audience are rarely met as expected. The success of Tugan-Baranovsky's books in the West is indisputable, but his intentions have scarcely been followed by his readers. Let me explain what I have in mind with the following two case studies. First, his book *Industrial crises in contemporary England ...* was written and intended for a

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<sup>13</sup> Comparatively, he devoted much less essays (necrologies and polemical texts excepted) to Russian thought; Dostoyevsky and Chernyshevsky being notable exceptions.

Russian audience, but the destiny of the book was to be mainly recognized abroad (see below). Eventually, the reputation of the work abroad played on its success in Russia. Second, his book *Theoretical foundations of Marxism*, published in both German and Russian languages, was intended first and foremost for the German audience, but it knew successive editions only in Russian (see next section).

The reception of Tugan-Baranovsky's book on crises in the West is contingent, for linguistic reasons, on the existence of editions that circulated in the West: mainly, during Tugan-Baranovsky's life, the German 1901 and the French 1913 editions. The absence of an English edition is also important in this story. It is therefore necessary to understand the history behind these various editions.

The book *Industrial crises in contemporary England, their causes and influences on national life*, published in Russian in 1894 is the result of almost four years of work in London and in St. Petersburg, to complete a *magister* dissertation at the University of Moscow. The book is organized in two parts. The first part (history of crises) deals with a lot of empirical material about the history of English crises in the nineteenth century and their social consequences (9 chapters). The second part (theory of crises) contains only two chapters. One about the "theory of markets", the other about the "theory of crises".

The first of these theoretical chapters ("theory of markets") contains Tugan-Baranovsky's explanation of the possibility of crises, caused by the capitalist anarchy in production, the tendency towards infinite accumulation of capital, and the key argument of disproportionality between sectors of production. It is in this chapter that Tugan-Baranovsky provided the bases of his path-breaking "revisionist" contributions to the field of Marxian economics, with his absolutely novel use of Marx's schemes of expanded reproduction, to which he added a third sector for luxury consumption goods, and which he used in subsequent publications—for criticizing the labour theory of value, the law of the tendency of the rate of profit to fall, and even to find a solution to the transformation of prices of production to labour value—gathered in his *Theoretical foundations of Marxism* (1905a, b).

The second chapter ("theory of crises") contains his path-breaking explanation of the different phases of the capitalistic cycle, and of the recurrence and periodicity of crises with the use of various mechanisms, including the cyclical fluctuations of free loanable capital in the banking sector. The two chapters together form Tugan-Baranovsky's theory: crises occur for causes that lie in the theory of markets, and their periodicity is explained as part of a capitalistic cycle.<sup>14</sup>

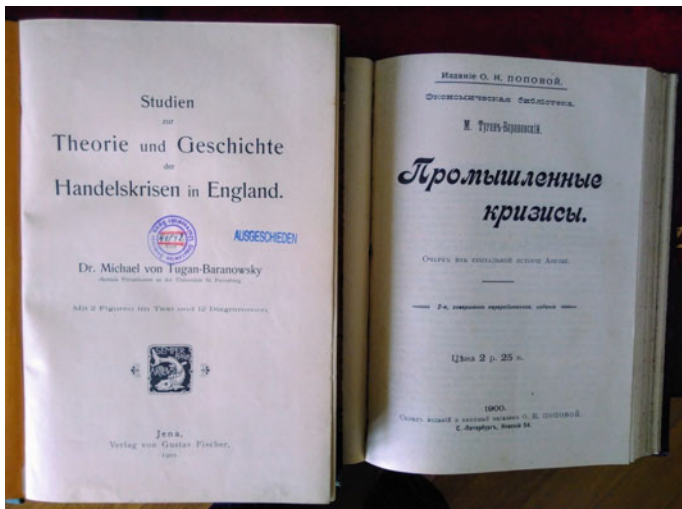
The second Russian edition appeared in 1900, under the title *Industrial crises* (Tugan-Baranovsky 1900a). It is still divided into two parts, but differently. The first part ("theory and history of crises") starts with the chapter on the theory of markets, then proceeds in four chapters to an updated history of crises, and eventually ends with the chapter on the theory of the periodicity of crises. The second part ("social importance of crises") now takes four full chapters and is much more developed. In all, about two-thirds of the book were new for the reader.

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<sup>14</sup> For a presentation of Tugan-Baranovsky's theory of crises, see Hagemann (1999, pp. 91–97). For the link between the theory of markets and the theory of crises, and the controversies it raised, see Besomi (2006).

The first German edition of the book appears in 1901. But before this publication, a long paper appeared in German, containing parts of the 1900 Russian and 1901 German editions on the social consequences of crises (Tugan-Baranovsky 1899a). The full German edition was published in Jena by Gustav Fischer, under the title *Studien zur Theorie und Geschichte der Handelskrisen in England* (Tugan-Baranovsky 1901). The edition had the same structure as the 1900 Russian edition, except for the inclusion of two new theoretical chapters at the end of the first part: one containing a critique of under-consumption theories of crises (Sismondi, Hobson, Dühring, Herkner), and another one on Marx's theory of crises, containing a critique of the theory of labour value and of the law of the tendency of the rate of profit to fall.<sup>15</sup>

Then arrived the 1913 French edition, entitled *Les crises industrielles en Angleterre* (Tugan-Baranovsky 1913), indicated as translated by Joseph Schapiro from the 2nd Russian edition, enlarged and improved by the author. It is in fact an almost third edition. The third Russian edition, is published under the title *Periodic industrial crises* (1914). Structurally, the 1913 and 1914 editions are almost identical, with three parts: the first part ("history of crises") retraces the history of crises until the end of the 1900 decade. The second part ("theory of crises") now contains 6 theoretical chapters in the Russian edition (1. Circulation of social capital, 2. Theories of markets of the classical school, 3. Theories of markets of Malthus-Sismondi and of the Marxian school, 4. Solution to the theory of markets, 5. Theory of crises, 6. Causes of the periodicity of crises), organized in three chapters in the French edition. Finally, the third part ("social consequences of crises") contains five chapters.



Title pages of the 1901 German and 1900 Russian editions

<sup>15</sup> This last chapter, on Marx's theory of crises, has been recently translated into English. See Tugan-Baranovsky (2000b).

It is necessary to add something on the absence of a full English translation of any edition,<sup>16</sup> as it impeded the early diffusion of his work, which went more indirectly in the Anglo-Saxon world, through the French rather than the German edition. In 1954, there has been a partial translation of some theoretical chapters of the third Russian edition: Chaps. 1, 5 and 6 of the second part mentioned above (Tugan-Baranovsky 1954), and in 2000, there has been a translation of two theoretical chapters from the German 1901 edition: on the theory of markets, and on Marx's theory of crises.<sup>17</sup> The English reader is still waiting for a complete edition, and the present author is actually even dreaming of a complete, multilingual variorum edition.

From the first to the third Russian editions, and even if not explicitly stated, there have always been three parts in this work: a historical part, a theoretical part, and a part on the social consequences of the crises. These three parts became explicitly autonomous in the structure of the book only in the last, 3rd Russian edition, and in the French edition. The historical part has been constantly updated to include new facts, but has also been rewritten at the margin. The part on the social consequences of the crises has been much updated for the 2nd Russian edition, and was only slightly touched afterwards. The theoretical part is what underwent the most important modifications. As mentioned above, it has always considered two aspects. For Tugan-Baranovsky, both the theory of markets and the theory of crises are necessary, and they are genetically linked. In the various editions, the theory of markets was improved in a defensive way: because of reactions against it, it has been clarified, and deepened with historical excursus in the theory. The theory of crises did not meet such resistance: it developed its way from an attempt to discover the reasons behind the periodicity of crises in the nineteenth century (and in the historical parts, to describe the various crises in their singularity) to an understanding of the crisis as a phase of a capitalistic cycle up to a full endogenous theory of the cycle as a feature of capitalism (and in the historical parts, it tended to concentrate a bit more on the common features between the crises).<sup>18</sup>

How took place the reception of Tugan-Baranovsky's book in the West? In a nutshell, the theory of crises attracted almost no attention in Russia, while it quickly started to disseminate in Germany, even before the German edition. As soon as 1895, there was already a mention to this theory in a book by Bergmann (1895, p. 438). This cannot be explained except by the existence of a proximity between the German and Russian academic worlds since a long time (Rieter et al. 2005). The Western economic academic world was lacking a proper theory to explain the recurrence and the periodicity of crises, and it found in Tugan-Baranovsky exactly what it needed, in terms of internal theoretical developments. Parts and parcels served as the

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<sup>16</sup> Apart from English, among the posthumous editions (not in the bibliography below), there has been a fourth (1923) and a fifth Russian edition (1997, reprinted in 2008), some German reeditions and reprints (1969, 2018), and notably two Japanese translations (in 1931 based on the French edition, in 1972 based on the German edition).

<sup>17</sup> See Tugan-Baranovsky (2000a, b) and the preface to their translations (Ramos-Martínez 2000).

<sup>18</sup> This evolution can be followed in parallel with the different entries on «Economic crises» in the *Brockhaus-Efron encyclopedic dictionary*, written by Tugan-Baranovsky in 1895, 1909 and 1915. See Allisson (2011).

basis of important further developments: in the German speaking world, with Bernstein, Spiethoff, Lederer, Hilferding, Sombart, Schumpeter; in France, with Lescure, Aftalion; in Sweden with Cassel and Wicksell, and later, more indirectly, in the Anglo-Saxon world, with Hawtrey, Keynes, Mitchell, Robertson, Kalecki, etc.<sup>19</sup>

As far as the theory of markets is concerned, it was either ignored or criticized in both Western and academic worlds. On the contrary, it was hotly debated in Russia, and more generally in the politically engaged Marxist circles. In the Preface to his 2nd edition, Tugan-Baranovsky complained: “This theory [of markets] did not draw any attention on it at first, but in recent years, it has sparked a very lively controversy” (Tugan-Baranovsky 1900a, p. i). His theory was either harshly criticized (for instance by Bukharin, Lenin, Luxemburg), or critically discussed (for instance by Kautsky, Bernstein, Hilferding, Bulgakov). The hardest criticisms gave birth to alternative developments, and in this sense, the book was powerfully influential, even in a negative sense.<sup>20</sup>

Clearly, for Tugan-Baranovsky, this must have been a surprise. For him, his book was studying England, only in so far as it contributed to the Russian debates on whether Russia was following the Western path. As a Westerniser himself, at least in this part of his life, he was convinced that Russia was undergoing the same patterns as England, only with a lag. In his Preface to the 1st Russian edition, Tugan-Baranovsky asserted that his book was intended to understand the Russian economy, through a circumvolution:

Russian economists are often reproached for taking topics outside of the Russian life for their work. One cannot disagree with the thought underlying these reproaches. Without any doubt, the task of Russian economic science consists mainly in studying the phenomena of the Russian economy. But very often, for the understanding of one’s own country, it is useful to turn oneself towards other countries, and in this regard, England is the most instructive.

(Tugan-Baranovsky 1894, p. i)

With his contribution to the history and theory of English crises, Tugan-Baranovsky intended to reach a Russian audience. And his creativity that produced the theory that was to earn him a world fame was only an unintentional by-product. It took time until the Russian public understood the message, and this had mainly to wait for the publication of his doctoral dissertation on *The Russian factory in the past and present* (1898, in Russian), where it became clear that the vision of recurring industrial crises was starting to take place in Russia as it did earlier in England. With *The Russian factory*, Tugan transformed his “Western” *Industrial crises* for a Russian reader. But the unintended fame of *Industrial crises* was already there in the West ...

<sup>19</sup> All this have already been documented with great care in the literature, notably by Beckmann (2005), Zweynert (2002, ch. 5.5.5 et 5.5.6), Reijnders (1998), etc.

<sup>20</sup> Again, this has also been carefully studied, by Milios and Sotiropoulos (2007), Beckmann (2005), Howard and King (1990), Amato (1984), etc.

#### 4 The Reception of *Theoretical Foundations of Marxism*

After his incursion in the theory of Marxism, already in 1894, Tugan-Baranovsky entered into serious controversies about Marxism. In a series of 6–7 articles in Russian, starting from “The fundamental error in Marx’s abstract theory of capitalism” (1899b) and “Labour value and profit. To my critics” (1900b), and ending in 1904 with a paper in German on “Der Zusammenbruch der kapitalistischen Wirtschaftsordnung im Lichte der nationalökonomischen Theorie” (1904), Tugan-Baranovsky constructed a whole criticism of Marxism: first by questioning the very notion of materialism and social classes, by criticizing the notion of surplus value and of the labour theory of value, and by offering a devastating knock to the falling rate of profit. In those developments, he introduced his own developments: Kant as an ethical viewpoint, the need to consider the psychological and subjective factors in history, the notion of absolute costs, his ideas about the productivity of labour, the distribution of income, etc.<sup>21</sup>

The arguments of these articles entered in Tugan-Baranovsky’s book, *Theoretical foundations of Marxism*, published in 1905 in both Russian and German. Instead of publishing it in Russian, and then translating it into German, as he did with his book on crises in 1900–1901, Tugan-Baranovsky reversed his thought. By the end of 1903, he remarked that “recently, my works have received more attention abroad than in Russia”, and therefore decided not to rush to publish his critical book on Marx in Russian, but waited to get a German edition as well.<sup>22</sup> He had the idea of going in Germany and get it first published into German, before coming back in Russia to get it published in Russian. In 1904, he repeated to his correspondent: “I want to release this [German edition of the] book because in the German literature my books are currently receiving much more attention and interest than in Russia.”<sup>23</sup> In November 1904, Bortkiewicz informs his colleague Chuprov about the presence of Tugan-Baranovsky in Germany:

Lately, many Russian guests have visited me; at the present time, Tugan-Baranovsky is here [in Berlin]. He is publishing his work on Marx in German.<sup>24</sup>

Eventually, the German book will be published at the beginning of 1905 in Germany, with a preface dated from November 13th, 1904. In Russian, the book will also be published in 1905, but a bit later, with a preface dated from February 20th, 1905. With his *Theoretical foundations of Marxism*, Tugan-Baranovsky changed his strategy: he was writing a book for a German audience, much more versed in Marxian

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<sup>21</sup> On the reception of the latter *Soziale Theorie der Verteilung* (published also in German and in Russian), see Pokidchenko (2019). More generally on Tugan-Baranovsky’s critique of Marx, see Howard and King (1990).

<sup>22</sup> Letter no 8 from Tugan-Baranovsky to A. A. Kaufman, 29 November 1903, in Shirokorad and Dmitriev (2008, p. 87).

<sup>23</sup> Letter no 9 from Tugan-Baranovsky to A. A. Kaufman, in Shirokorad and Dmitriev (2008, p. 89).

<sup>24</sup> Letter from Ladislaus von Bortkiewicz to A. A. Chuprov, Letter 77, 17.11.1904, in Sheynin (2005).



literature than the Russian public, only interested—so he thought—in sterile controversies. But a detailed study on the differences between both editions even reveals something more:

Interestingly, a comparative analysis of the Russian and German texts reveals a number of discrepancies in the presentation of the same issues, as well as the absence of some portions of texts in the German, and reversely, in the Russian editions. This is due to Tugan-Baranovsky's understanding of the difference in the level of preparedness of the Russian and German audiences for understanding the teachings of Marx: if in Russia the works of Marx were habitually interpreted in an orthodox way, and the critique of its propositions was excluded, in Germany, such critique was commonplace.

(Eremenko 2009, p. 178)

Tugan-Baranovsky adapted the book to both audiences, anticipating however a better understanding in Germany. The differences between the two books mainly concerned the philosophical part of the book, and, because of censorship, also political and revolutionary considerations. But was his overall appreciation correctly headed? As he himself recognized in his letters to Kaufman, Marxism was also becoming an interesting topic in Russia. A second Russian edition was already needed in 1905, prefaced on June 30th, 1905, and a third edition was released, with some modifications, in 1906, with a short preface dated on April, 12th, 1906. In contrast, no further German edition was needed. Of course, the book was discussed in Germany, and even appreciated by some (more by Bernstein than by Kautsky, for instance). But in Russia, even as a turnoff, it proved much more discussed.

It is somewhat ironic that the only economist who was going to bring this book to posterity, by emphasizing and developing its analyses of value, its analysis of the tendency of the rate of profit to fall and above all its use of reproduction schemes in the transformation of prices of production to labour value ... is Ladislaus von Bortkiewicz, the Russian economist in Germany, who had the choice between the two editions and was, perhaps in part for that, the ideal audience of this work.<sup>25</sup>

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Tugan-Baranovsky was without any doubt one of the greatest economists of his time. As far as his education was concerned, he was a real Westerniser. And his works did acquire an enduring fame in the West. But the relationships between his own intentions, in terms of audience, have often been contradicted by the facts. He was thinking of writing for the Russians, and his work became famous in the West. Then, when his relation with the Russian public became more difficult, he intended to write for the Western audience, but he was more welcomed in Russia. He was probably more in phase with the Russian public than he thought. But anyway, his mind was made for a universal radiance.

Personally, I find that where Tugan-Baranovsky showed himself at its best, both as a researcher and as a teacher, is in his textbook, *Fundamentals of political economy*

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<sup>25</sup> See Bortkiewicz (1906–1907), in which he also brings the attention to Dmitriev (1904) to the German audience.

(1909). There, he could offer his thoughts to one of his favorite audience—his students. There, he brought together all the influences, most from the West, in a very personal way, to which no public was perhaps prepared. I bet there is no coincidence that this book was never translated in the West, during Tugan-Baranovsky's own life. And this is unfortunate that this has not been done until now. Maybe the audience is still not ready, but it would eventually allow a comprehensive return to the West, of all that Tugan-Baranovsky borrowed from it.

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# The Circular Flow of Ideas: Vladimir K. Dmitriev



Christian Gehrke and Heinz D. Kurz

## 1 Introduction

Vladimir Karpovich Dmitriev (1868–1913) was a Russian mathematical economist who published three major essays on economic theory. Born on 24 November 1868 on a landed estate near Smolensk as the son of a well-known agronomist, Dmitriev attended the gymnasium in Tula and then studied political economy at the Law Faculty and medicine at the University of Moscow. He graduated and got married in 1896 and then became an excise controller in the provincial town of Von'kovitsy. Three years later he had to give up this post because he had contracted pulmonary tuberculosis, and chronic illness and notorious financial problems plagued him for the rest of his life. He died on 30 September 1913 in Gatchina, near St. Petersburg.

In 1898, Dmitriev published an essay (in Russian) on Ricardo's theory of value and distribution, followed in 1902 by two further articles on Cournot's theory of competition and on the theory of marginal utility. He had outlined those essays already in his student days and completed the first one on Ricardo's value theory in the year of his graduation, but problems with finding a publisher delayed its publication. In 1904, the three essays were published together (in Russian) in a book entitled *Economic essays. First series: Attempt at an organic synthesis of the labour theory of value and the theory of marginal utility*. A French translation of the collection was published in 1968 and an English translation in 1974; the three essays are now also available in Italian, Spanish and German. Dmitriev's further writings include

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a survey on statistical theory, a statistical survey on the problem of alcoholism in Russia, a book on alcohol consumption, and several survey articles and book reviews on economic theory.<sup>1</sup>

The present chapter informs about Dmitriev's contributions to economic analysis, major Western and Russian intellectual influences on his thinking, the reception of his work in East and West, and the impact of his findings on the further development of economic theory. It opens, in Sect. 2, with a summary account of Dmitriev's main works and then turns to major sources of inspiration for his remarkable contributions. In Sect. 3 the important influences on his work of economic theorists from the West are discussed, while Sect. 4 focuses attention on the impact the contemporary Russian economic discourse had on him. In Sect. 5, we summarize how Dmitriev's contributions were received in Russia in the early twentieth century and try to assess their importance for the attempts by Nikolay N. Shaposhnikov, Evgeny E. Slutsky, and Leonid N. Yurovsky to reconcile the theories of production costs and of marginal utility. We then turn to Dmitriev's rediscovery in the 1960s and discuss whether the origin of input–output methods and of planning models can be traced back to Dmitriev's equations for the determination of labour values, as has been contended in the literature. In the West, the early reception of Dmitriev's *Economic Essays* was so closely associated with the contributions of Ladislaus von Bortkiewicz ([1906–7] 1952, [1907] 1949), who elaborated on the latter's findings in the first essay and applied them to a critical discussion of Marx's theory of value and distribution, that an impact of Dmitriev's ideas in the first half of the twentieth century can be discerned only indirectly, via von Bortkiewicz. Our discussion in Sect. 6 therefore first concentrates on the importance of Dmitriev's contributions for the debates on Marx's theory of value and distribution that were instigated by von Bortkiewicz's papers. We then summarize briefly the re-assessment of Dmitriev's work against the background of the much-improved understanding of the classical approach to economic theory, its genuine significance and distinctive analytical features elaborated by Piero Sraffa's interpretive and reconstructive work. Section 7 contains some concluding remarks.

## 2 Dmitriev's *Economic Essays*

In his first essay of 1898, entitled “The theory of value of David Ricardo. An attempt at a rigorous analysis”, Dmitriev made important contributions to the development of the classical approach to economic theory and the clarification of its analytical structure. In particular, he demonstrated that (i) prices can be decomposed into wages

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<sup>1</sup> For a list of Dmitriev's writings, see Nuti (1974b: 30–31). In the following, all references to the *Economic essays* are to the English translation in Dmitriev (1974). With regard to Dmitriev's further contributions, which are available only in the original Russian language, we have relied on the summary accounts and critical discussions that are provided in German, French and English secondary sources, including in particular Allison (2015), Nuti (1974a, b), Schütte (2003), Seraphim (1925), and Zauberman (1967, 1969). Unless otherwise stated, with regard to these writings the English translations are ours.

and profits via a “reduction to dated quantities of labour”; (ii) relative prices are proportional to relative labour values only with zero profits or with “equal organic composition”, to use Marx’s term; (iii) Ricardo’s concept of an inverse relationship between the general rate of profits and the real wage rate, given the technical conditions of production, that is, the wage–profit relationship, can be given a precise analytical expression; (iv) the origin of profit stands in no special relationship with human labour; (v) the data of Ricardo’s approach (i.e. the real wage rate and the technical conditions of production) suffice to determine simultaneously relative prices and the general rate of profits.

- (i) *Reduction to dated quantities of labour.* Dmitriev investigated first how the total amount of labour expended in the production of a commodity can be ascertained. He considered a system with single production that can be represented, in matrix notation, as

$$\mathbf{v} = \mathbf{l} + \mathbf{A}\mathbf{v}, \quad (1)$$

where  $\mathbf{A}$  is the  $n \times n$  matrix of commodity inputs,  $\mathbf{l}$  is the  $n$ -vector of direct labour inputs, and  $\mathbf{v}$  is the  $n$ -vector of direct and indirect labour inputs. Then, replacing  $\mathbf{v}$  on the RHS of the equation repeatedly by the expression for  $\mathbf{v}$ , we get

$$\begin{aligned} \mathbf{v} &= \mathbf{l} + \mathbf{A}\mathbf{l} + \mathbf{A}^2\mathbf{l} + \cdots + \mathbf{A}^t\mathbf{l} + \cdots, \quad \text{or} \\ \mathbf{v} &= \mathbf{l}_0 + \mathbf{l}_1 + \mathbf{l}_2 + \cdots + \mathbf{l}_t + \cdots, \end{aligned} \quad (2)$$

where  $\mathbf{l}_t = \mathbf{A}^t\mathbf{l}$ . Dmitriev set out the simultaneous equation system (1) and concluded that since there are as many equations as unknowns,  $\mathbf{v}$  is determined, given  $\mathbf{A}$  and  $\mathbf{l}$ . He thus rejected the common misconception that a “historical regression” would be needed in order to ascertain the total labour contents of commodities: ‘[W]e can always find the total sum of the labour directly and indirectly expended on the production of any product *under present-day production conditions* (...) the fact that all capital under *present-day* conditions is itself produced with the assistance of other capital in no way hinders a precise solution of the problem’ (1974: 44).

However, in the following Dmitriev then assumed that the series of dated quantities of labour in (2) is *finite*, and thus implicitly adopted an “Austrian” perspective: Dmitriev’s representation of production processes in terms of finite series of dated quantities of labour corresponds to Austrian processes of the “flow input–point output” type, and presupposes the absence of “basic” commodities (Kurz and Salvadori 1995: 176–178).<sup>2</sup> It is within this framework that Dmitriev then confirmed the proposition, originally proposed by Smith

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<sup>2</sup> Sraffa (1960: 7–8) refers to commodities which enter directly or indirectly into the production of all commodities as “basic” commodities; otherwise they are called “non-basic” commodities. This distinction is related to, but not identical with, the distinction between “necessaries” and “luxuries” of the classical political economists.

and adopted also by Ricardo (but sharply criticized by Marx), that the price of every commodity can be *entirely* resolved into wages and profits in a *finite* number of steps. Following the classical authors, wages are supposed to be paid *ante factum*, that is, at the beginning of the uniform period of production, and prices are explained in terms of a reduction to a *finite* stream of dated quantities of labour, that is,

$$\begin{aligned} p &= (1+r)w[\mathbf{l} + (1+r)\mathbf{A}\mathbf{l} + (1+r)^2\mathbf{A}^2\mathbf{l} + \dots + (1+r)^k\mathbf{A}^k\mathbf{l}], \quad \text{or} \\ p &= (1+r)w[\mathbf{l}_0 + (1+r)\mathbf{l}_1 + (1+r)^2\mathbf{l}_2 + \dots + (1+r)^k\mathbf{l}_k]. \end{aligned} \quad (3)$$

- (ii) *Proportionality of prices and labour values.* Dmitriev next turned to the investigation of the properties of relative prices, which in his framework are given by

$$\frac{p_i}{p_j} = \frac{(1+r)w\mathbf{e}_i^T[\mathbf{l} + (1+r)\mathbf{A}\mathbf{l} + (1+r)^2\mathbf{A}^2\mathbf{l} + \dots + (1+r)^k\mathbf{A}^k\mathbf{l}]}{(1+r)w\mathbf{e}_j^T[\mathbf{l} + (1+r)\mathbf{A}\mathbf{l} + (1+r)^2\mathbf{A}^2\mathbf{l} + \dots + (1+r)^k\mathbf{A}^k\mathbf{l}]}, \quad (4)$$

or

$$\frac{p_i}{p_j} = \frac{l_{i0} + (1+r)l_{i1} + (1+r)^2l_{i2} + \dots + (1+r)^k l_{ik}}{l_{j0} + (1+r)l_{j1} + (1+r)^2l_{j2} + \dots + (1+r)^k l_{jk}}$$

where  $l_{it} = \mathbf{e}_i^T \mathbf{l}_t$ .

Dmitriev confirmed Ricardo's finding that relative prices are proportional to relative labour values in two special cases only: (i) when the series of dated quantities of labour are linearly dependent pairwise, i.e. when the commodities exhibit "identical organic composition"; and (ii) when the rate of profits is zero. In general, relative prices deviate from relative labour values.

- (iii) *Wage-profit relationship.* Dmitriev next turned to the analysis of the general rate of profits and of production prices. He praised Ricardo for having clearly specified the factors that determine the general rate of profits, that is, the real wage rate and the technical conditions of production in the industries producing wage goods or means of production used (directly or indirectly) in the production of wage goods: 'Ricardo's immortal contribution was his brilliant solution of this seemingly insoluble problem' (1974: 58). Dmitriev suggested that Ricardo had accomplished the solution of this problem because he recognized 'that there is one production equation by means of which we may determine the magnitude of  $r$  directly (i.e. without having recourse for assistance to the other equations)' (1974: 59). In Dmitriev's reading, Ricardo had adopted the simplifying assumption that the real wage basket consists only of a certain amount of corn, that is

$$w = p_c c, \quad (5)$$



where  $c$  is the amount and  $p_c$  the price of corn, respectively. In this case the rate of profits is determined from the price equation of the corn industry alone, since

$$p_c = (1+r)p_c c e_c^T [\mathbf{l} + (1+r)\mathbf{A}\mathbf{l} + (1+r)^2\mathbf{A}^2\mathbf{l} + \dots + (1+r)^k\mathbf{A}^k\mathbf{l}], \quad \text{or} \\ \frac{1}{c} = (1+r)e_c^T [\mathbf{l}_0 + (1+r)\mathbf{l}_1 + (1+r)^2\mathbf{l}_2 + \dots + (1+r)^k\mathbf{l}_k]. \quad (6)$$

With  $l_{ct} = e_c^T \mathbf{l}_t$ , it follows that  $r = f(l_{c0}, l_{c1}, l_{c2}, \dots, l_{ck}; c)$ . Dmitriev then generalized this result by considering the case of multiple wage goods and vindicated Ricardo's proposition—which had been disputed by Marx—that the general rate of profits is not affected by changes in the conditions of production of the 'non-basic' industries, i.e. industries which produce neither wage goods nor means of production used directly or indirectly in the wage goods industries.

- (iv) *The origin of profit stands in no special relationship with human labour.* Dmitriev next demonstrated that surplus value and profit on capital also exist if instead of human labour only the labour of working animals (or of self-reproducing machines) were used in production. In this case the rate of profits is determined simply by replacing in Eq. (6) the workers' subsistence requirements (in terms of "corn" or a given wage basket) by those of the working animals: 'Whether the potential energy incorporated in the production good  $c$  is released and used in production in the form of *human labour*, as happens at present, or by means of some other process (*not involving the participation of human labour*) is a matter of indifference' (1974: 63).
- (v) *Simultaneous determination of prices and distribution.* Finally, Dmitriev refuted Walras's contention ([1874] 1954, Lesson 40, § 368) that Ricardo's "cost of production" explanation of prices and income distribution is indeterminate, because the number of unknowns exceeds the number of equations in the classical approach to value and distribution. Dmitriev deserves the credit for having demonstrated that the data of Ricardo's approach, i.e. the real wage rate and the technical conditions of production, suffice to determine simultaneously relative prices and the general rate of profits.

In the remainder of his first essay, Dmitriev discussed the theory of competition, the problem of the choice of technique, the problem of fixed capital, and the theory of rent. Although he aimed at 'an organic synthesis' of classical and marginal utility theory, Dmitriev retained the fundamental asymmetry in the treatment of the distributive variables that characterizes the classical approach to economic analysis, misleadingly maintaining that the investigation of the conditions affecting the level of real wages 'falls outside the scope of political economy' (1974: 74). In classical political economy wages were determined in that part of economic theory that was concerned with economic growth and development, technical progress and population dynamics, whereas when determining the rate of profits and relative "natural" prices in a given economy at a given time they were taken as given.

In the second essay, Dmitriev critically examines the theory of competition of Cournot (1838) and makes some steps towards the development of a theory of competition with strategic interaction. The starting point of his argument is the observation that Cournot's analysis, in which each seller is supposed to seek a "temporary profit" (Cournot's *bénéfice momentané*) from expanding his individual output until in equilibrium price equals marginal costs, is based on the assumption that other sellers cannot react immediately by increasing their own market supplies. Rejecting this premise, Dmitriev argued that in competitive conditions rational producers will always hold inventories and/or excess capacities for strategic reasons, that is, as a threat to potential competitors:

Stocks of commodities in the struggle for sales play the same role as intensified armament of the Powers in *peace time*. Expenditure on such armament appears completely pointless since it does not yield any *apparent* result; its significance is purely negative and may be understood only if we consider one Power beginning to disarm when the others remain in their former position. Not only would the position of this Power deteriorate *in the event* of a disturbance to international peace, but the very fact of its disarmament would be an *incentive to the disturbance of the peace*. The other Powers for whom a favourable outcome of a struggle (...) with this state (...) would have been in doubt (...) could now count on victory should a struggle commence. Precisely the same thing would occur in the market if one or more of the competitors were to liquidate their 'dead' stocks: [For] the remaining competitors (...) it would seem advantageous (...) to disturb the existing equilibrium (by lowering the price existing in the market). (1974: 148)

Calling the costs incurred by producers from holding excess capacity or dead stocks "realization costs" and considering these as entering into competitive costs, in addition to the ordinary expenses of production, Dmitriev concludes that 'unrestricted free competition invariably tends to raise actual production costs above the essential level, i.e. above the lowest level possible for a given state of production technique' (1974: 95). He also points out that technical progress tends to raise the size of the stocks and/or spare capacities held by producers for strategic reasons and argues that this constitutes an important aspect in the explanation of economic fluctuations and of crises—a topic he intended to address in a further treatise which however did not materialize.

In the third essay, Dmitriev provided a detailed account of the genesis of marginal utility theory. This essay is mainly of interest from the perspective of the history of economic thought, because Dmitriev denied the occurrence of a major break in the development of economic theory in terms of a "marginal revolution" in the 1870s. He maintained that important contributions to marginal utility theory had been made already much earlier by economists such as Gossen, Senior, Rossi, Dupuit, and Molinari, and that, indeed, 'we find *all the information* needed for the construction of a *finished* theory of marginal utility in the work of such an "old" economist as Galiani' (1974: 182). He also argued that 'the Austrian School as such (Menger, Böhm-Bawerk, von Wieser, and others) added very little (unless much significance is given to the introduction of new terms) to what had been done before them *for the solution of the problem*' (1974: 181). Important contributions had been made only by economists who used the mathematical method, including first and foremost 'Walras (who may justifiably be regarded as the creator of marginal utility theory), Launhardt, Auspitz

and Lieben, and Jevons' (1974: 182). Dmitriev neatly summarized the contributions made by each of the mathematical economists mentioned, but showed no awareness that utility may not be cardinally measurable and did not contribute in a significant way to the further development of marginal utility theory.

Dmitriev explicitly intended to provide 'an organic synthesis of the labour theory of value and the theory of marginal utility', but the result of his efforts is not convincing. The main reason for this is that he did not clearly perceive the differences in the analytical structure of the classical and the marginalist (later dubbed 'neoclassical') approach to economic theory. He interpreted Ricardo's theory of value and distribution partly on the basis of the analytical structure that characterizes the classical approach, and partly against the background of the neoclassical approach: While he correctly perceived the *asymmetric* treatment of the distributive variables in the classical approach, he failed to recognize that the classical economists had not based their analyses of prices and income distribution on well-defined quantitative functional relationships between costs and quantities produced. In the analysis of prices and distribution those quantities were rather treated by them as *given* magnitudes, whose determination was considered to be the task of a theory of accumulation and growth, taking into account the specific factors prevailing in a given historical situation. Therefore, in the classical approach, unlike in the marginalist one, the influence of "demand" on prices is unrelated to the presence or absence of constant returns.

### 3 Western Sources and Influences

A search for the sources on which Dmitriev was drawing for his path-breaking contributions must start with the observation that he was remarkably well-read in the economics literature, in particular with regard to pure economic theory and mathematical economics.<sup>3</sup> Dmitriev strongly advocated the application of mathematical methods in economics, and accordingly mathematical economists loom large in his bibliography: the contributions of Auspitz and Lieben, Bernoulli, Cournot, Dupuit, Gossen, Jevons, Laplace, Launhardt, Pantaleoni, Thünen, and Walras are all discussed favourably—though not necessarily uncritically—by him. Dmitriev typically made bold statements about the merits (and demerits) of earlier contributions to the problem under consideration. He had an independent mind and did not shy away from strongly criticizing also the views of authors he held in high esteem otherwise. He was particularly critical of the theoretical contributions of the Austrian school, but

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<sup>3</sup> As Samuelson (1975: 494) rightly noted, there are many references to Italian, French, German and Austrian writers in Dmitriev's *Economic Essays*, but only few to English (not to speak of American) economists. Dmitriev had an intimate knowledge of the contributions to marginal utility theory (except Rau etc.) and had studied carefully the contributions of the major classical political economists. He was familiar also with the contributions of preclassical authors like Petty, Cantillon, Boisguilbert, Le Trosne, and Turgot. Conspicuously absent from Dmitriev's bibliography in the *Economic Essays* are Quesnay's *Tableau économique* and Marshall's *Principles*.

there are disparaging remarks also on Roscher, Knies, and Hildebrand,<sup>4</sup> and critical comments on particular aspects of the contributions of scholars highly esteemed by him, like Thünen and Walras.<sup>5</sup>

With an author as well-informed as Dmitriev it is difficult to relate the “origin” of his ideas to a small number of writings or to individual authors only. Nevertheless, in our reading the main authors to have importantly influenced Dmitriev’s thinking were Ricardo, Marx, Cournot, and Walras.

**Ricardo.** As we saw above, Dmitriev vindicated many of Ricardo’s propositions in the theory of value and distribution. He clearly regarded Ricardo’s exposition in the *Principles* as the most advanced statement of the classical approach to the theory of value and distribution—and saw little merit in Marx’s subsequent contributions to economic theory, which he did not perceive as the (preliminary) culmination point of the surplus approach tradition, but rather as a retrogression. In Dmitriev’s understanding, ‘the most important point in Ricardo’s theory’ is his determination of the general rate of profits, given the real wage and the technical conditions of production: ‘Ricardo’s immortal contribution was his brilliant solution of this seemingly insoluble problem’ (1974: 50, 58).

**Marx.** It would be quite wrong to suppose that because Dmitriev made almost no references to Marx’s writings<sup>6</sup> and refrained from discussing *explicitly*<sup>7</sup> the implications of his findings for Marx’s theoretical construction, the latter therefore had played no role in the development of his ideas. Dmitriev’s findings are obtained by eschewing Marx’s concept of “abstract labour” and by disregarding his distinction between “value form” and “price form”: Dmitriev thereby implicitly demonstrated the redundancy of what Bortkiewicz ([1906–7] 1952: 257) later called Marx’s “successivist” approach to the determination of prices and income distribution. By the same token, he also showed that Marx’s contention was mistaken that (abstract) human labour

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<sup>4</sup> See Dmitriev (1974: 190, n. 1 and 195, n. 2).

<sup>5</sup> See Dmitriev (1974: 50). Thünen is criticized for disregarding the inverse wage–profit relationship in his formulation of the maximization problem in the derivation of his “natural wage” (1974: 75–76), and Walras *inter alia* for wrongly associating market outcomes under free competition with the maximum satisfaction of wants (1974: 149 n).

<sup>6</sup> There is only a single explicit reference to vol. 1 of *Capital* (1974: 43); vols. 2 and 3 of *Capital* are not even included in Dmitriev’s bibliography. However, in the “Conclusion” of the *Economic essays* there is a brief reference also to vol. 2 of *Capital*, where Marx’s notion of “abnormal stocks” in Chap. 6 of vol. 2 is rejected, ‘since it seems to prejudice the question whether these stocks are in conformity with the *correct* calculus of entrepreneurs’ (1974: 217, n. 2).

<sup>7</sup> However, there are clearly *implicit* references to Marx, such as the following remark on Ricardo’s theory of rent: ‘In order to free the definition of the amount of “socially necessary” labour from the conditions of supply and demand, some advocates of this “developed” form of the labour theory of value attempt to *equat[e] the amount of socially necessary labour with the average amount* used in the production of a given commodity. To assert this is, however, to deny everything which Ricardo did to clarify the laws governing the value of those products, individual portions of which are produced with different production costs. Ricardo’s analysis leaves no doubt that the value of a commodity is determined by the quantity of labour expended on its production *not under average but under the most disadvantageous* conditions of its production’ (1974: 87).

alone constitutes a “common third” which makes possible the commensurability of commodities. Moreover, Dmitriev conclusively clarified that human labour assumes no special role in the explanation of the “origin” or “source” of profit:

Hypothetically conditions could exist where the profit rate actually prevailing in all industries (...) would be determined by production conditions (costs) of the subsistence of some domestic animal ... [and] conceivably a state of technology could exist where (...) the profit level is determined in a production process where *no “living” power is involved at all* and “reproduction” of goods (including machines) is effected by “inanimate” natural forces. Therefore, we can imagine a state of society where *wage labour is not used* in production, *but where “surplus value” will nevertheless arise, and where, consequently, there will be profit on capital.* (1974: 214)

Dmitriev could not possibly have known that Ricardo had also contemplated such a futuristic situation in which human (and all other kinds of) labour are not needed any longer: The reference is to a fully automated system of production in which machines and other products are produced by machines alone. In a letter to J. R. McCulloch of 30 June 1821 Ricardo wrote: ‘If machinery could do all the work that labour now does, there would be no demand for labour. Nobody would be entitled to consume anything who was not a capitalist, and who could not buy or hire a machine’ (1951–73, VIII: 399–400). Surplus value and profit on capital exist even though human labour is not involved at all in the production process.

It is necessary to stress, however, that Dmitriev was not justified in neglecting Marx’s theoretical contribution to the development of the surplus approach with regard to one important point. He failed to notice that when Marx criticized Ricardo (and Smith) for not having introduced the distinction between “constant” and “variable” capital, he was referring not only to its alleged relevance for “revealing” the origin of profits, but more importantly to the fact that with circular production relations commodity prices cannot be resolved *entirely* into wages and profits in a *finite* number of steps. The important analytical implication of this is, as Marx had also correctly pointed out, that there is a finite maximum rate of profits corresponding to (hypothetically) zero wages.

**Cournot.** In developing his novel theory of competition, Dmitriev harked back to the contribution of ‘the only author to give a complete theory of competition, (...) the great “forgotten” economist Augustin Cournot’ (1974: 95, 215). Dmitriev elaborated on Cournot’s contribution, which had been unduly neglected in Russia and in the West alike, by introducing strategic holdings of stocks and spare capacities and thus separating production and sales. Under Dmitriev’s assumptions, prices are not lowered to necessary production costs even in Cournot’s limiting case of unrestricted competition, so that constant production costs alone can no longer be considered to suffice for rendering prices independent of demand conditions on the basis of the marginalist approach. Clearly, Dmitriev greatly admired Cournot’s ‘immortal work’, which constituted the most important source of inspiration for his attempt ‘to construct a rigorously scientific theory of price determination under unlimited free competition’ in his second essay (1974: 95, 97).

*Walras*. In the third essay, Dmitriev refers to Walras as ‘the creator of marginal utility theory’, at least ‘in its developed form’ (1974: 182). The latter’s formulation of a system of equations for the determination of a general economic equilibrium clearly constituted a major source of inspiration for Dmitriev’s work. It should not be overlooked, however, that Dmitriev was critical of several aspects of Walras’s contribution. First, as already mentioned above, Dmitriev conclusively refuted the circularity argument Walras had raised against Ricardo’s theory of prices and income distribution.<sup>8</sup> Secondly, Dmitriev (1974: 149 n) rejected Walras’s claim that ‘production in a market ruled by free competition (...) will give the greatest satisfaction of wants’ ([1874] 1954: 255). Dmitriev’s argument is based on his theory of competition, in which the “wasting” of resources for advertising and stock holdings leads to the excess of prices above necessary production costs. Third, while in his third essay Dmitriev (1974: 204–205) set out, in a simplified form, Walras’s pure exchange equations of Lesson 21 of the *Elements* ([1874] 1954: 243–254),<sup>9</sup> he showed no interest in (and left uncommented) Walras’s theory of production, and he also entirely disregarded his theory of “capitalization”. Finally, and most importantly, Dmitriev considered it necessary, unlike Walras, to retain the asymmetric treatment of the distributive variables that characterizes the classical approach to economic theory. This becomes very clear also in his last publication, a review of a book by Solntsev (1911), where Dmitriev (1912) reiterated his view that the income shares of wages and profits are *not* determined by economic theory, but rather by ‘inductive sociology’. Whereas major marginalist authors, including Jevons and Walras, prided themselves with having overcome the indeterminateness of the theory of the classical authors, which was said to have had too few equations to determine all the unknowns, Dmitriev raised doubts about the possibility of providing a determinate solution to the problem of income distribution by means of economic theory alone.

Distinguishing between commodities produced in conditions of constant, decreasing, and increasing costs, Dmitriev stated in his “Conclusion”:

[T]o whichever of the three categories established by Ricardo a product belongs, its price cannot be determined independently of the conditions of demand, and therefore of consumption. This led us to an analysis of the ‘demand curve’ (barely touched upon in Ricardo’s writings). We drew the data for this analysis from the theoreticians of marginal utility, who have given a completely scientific solution to the problem of the relationship between the price of a product and the amount of the quantity offered (sold). (1974: 218)

This statement elicits the following remarks. First, Ricardo and the classical authors in general did not argue that “demand”, that is, the quantities of the different products needed and wanted, play no role in determining relative prices. Gross output levels clearly impacted on them, as becomes very clear in the theory of differential

<sup>8</sup> On this see also Kurz and Salvadori (2002).

<sup>9</sup> Strangely, in his comment on the Walrasian *exchange equations*, Dmitriev referred to Walras’s individuals as “producers” and to the commodities exchanged as “products” (rather than “initial endowments”). He noted: ‘[G]iven the vast specialisation of labour existing almost universally at the present time, the *producer* usually manufactures exclusively one *product*, and himself consumes only an insignificant proportion of his own product, so that when the volume of production is sufficiently large this quantity may be completely disregarded.’ (1974: 205; emphases added).

rent, for example. Secondly, the strange thing is that Dmitriev, while insisting that marginalist theory did not involve also the determination of the distribution of income, at the same time argued that it provided a complete and coherent theory of relative prices. However, as he had pointed out *inter alia* in his first essay, relative prices depend also on the distribution of income, that is, the level of the rate of profits and the corresponding levels of the wage rates. In other words, wherein consisted the step forward by the marginalist authors relative to the classical ones in regard to the coherence and completeness of the theory, if not by filling the lacuna the classicals had purportedly left?

In general, Dmitriev did not clearly perceive the existence of an alternative and distinctive classical approach to value and distribution that differs in terms of its analytical structure from the neoclassical or marginalist one. This is partly due to the fact that he did not fully understand the analytical structure of the classical approach: he correctly perceived the asymmetric treatment of the distributive variables, but did not see that in the analysis of prices and income distribution the classical authors took the quantities demanded as given. In addition, Dmitriev also had no clear understanding of the analytical structure of the neoclassical and in particular the Walrasian approach to economic theory—presumably, Dmitriev would not have accepted “given initial endowments” as a data, because he argued that all magnitudes that are economically determined must not be treated as exogenous data.

#### **4 Russian Influences on Dmitriev’s Ideas: The Role of the Economic Discourse in Russia**

While the main sources of Dmitriev’s work are undoubtedly contributions to the Western economic literature, he was of course also influenced by the theoretical traditions of his home country and the contemporary economic discourse in Russia. Here, the main influences on his thinking appear to have come from the following authors.

*Zhukovsky and Zalesky.* As Shukhov (1988: 11) noted, in his first essay Dmitriev ‘continued the analysis of the theory of value of the classical school begun by Zhukovsky’. Dmitriev himself indeed also stated that Zhukovsky ‘has quite correctly understood and explained in his book the importance of Ricardo’s theoretical conclusions’ (1974: 61). In his attempt to reformulate Ricardo’s theory of value in mathematical terms Zhukovsky (1871) had provided in particular ‘a model analysis of the theory of rent’, in which he showed that Ricardo’s theory does not depend on a historical progression from more to less fertile soils, but follows directly from the principle of a uniform profit rate in situations where cost conditions vary for different portions of the total quantity produced. However, according to Dmitriev his remarks on Ricardo’s theory of profits showed that he ‘failed to understand the very basis of Ricardo’s theory’: Zhukovsky had maintained that for the determination of the profit rate Ricardo had referred to ‘the ratio between the supply and demand’ for capital

(1974: 50–51). Neither Zhukovsky nor his follower Zalesky (1893), whose treatise on the classical theory of value Dmitriev likewise found commendable, had correctly grasped Ricardo’s ingenious solution to the problem of the determination of the rate of profits.

**Sieber.** In his article “Value theory (Survey of the literature in Russian)” Dmitriev (1908: 16) argued that the reception of marginal utility theory had been delayed for more than twenty years in Russia, and that Sieber must at least partly be held responsible for this ‘abnormal situation’. Through his “Marxian” interpretation of Smith and Ricardo, which saw in Marx’s theory of value and distribution the (preliminary) culmination point of the classical tradition, Sieber (1871, [1885] 1937) had significantly contributed to the predominance of Marx in the Russian economic discourse. On the other hand, with his reading of Smith and Ricardo as major representatives of a classical surplus approach tradition he had also initiated a return to the theoretical ideas of the British classical political economists, seriously impeding thereby the reception and acceptance of the marginal utility theory (1908: 16–17). In the *Economic essays* Sieber is hardly mentioned at all and Dmitriev seems to have made an effort to dissociate himself from the latter’s “Marxian” interpretation of Ricardo. He appears to have regarded his own analysis as a continuation of the line of research begun by Zhukovsky, rather than the one initiated by Sieber.

**Tugan-Baranovsky and Struve.** In his literature survey on value theory in Russia Dmitriev (1908) credited Tugan-Baranovsky (1890) with having induced the recognition and acceptance of marginal utility theory in Russia, followed by the treatises of Zalesky (1893) and various contributions by Struve, in particular Struve (1900). According to Dmitriev, no positive contributions to the development of the theory of value and distribution had been made by the orthodox Russian Marxists. The important contributions had rather been those studies in which Marx’s erroneous views had been exposed and discarded. In this regard, Dmitriev referred to Frank’s treatise (1900) for a neat summary of the criticisms of Marx’s theory, and also mentioned favourably the criticisms of Marx’s theory of value and distribution put forward by Buch, Struve, and Tugan-Baranovsky, that is, by the “Legal Marxists”. It is noteworthy also that Dmitriev characterized Böhm-Bawerk’s “Zum Abschluß des Marxschen Systems (On the Close of the Marxian System)” ([1896] 1949) as an ‘excellent’ work—an assessment definitely not shared by Ladislav von Bortkiewicz.<sup>10</sup> Dmitriev’s position vis-à-vis Marx’s theory thus might seem close to some basic tenets of the Legal Marxists, and there can be little doubt that he was strongly influenced by some of the ideas of Tugan-Baranovsky and Struve.<sup>11</sup> This also holds true for Tugan-Baranovsky’s and Struve’s rejection of Marx’s exploitation

<sup>10</sup> Clearly, Dmitriev’s verdict on Marx’s contribution to the development of economic theory, and also on Böhm-Bawerk’s Marx critique, differed sharply from that of his most important “follower”, Ladislav von Bortkiewicz. According to von Bortkiewicz ([1906–07] 1949), Marx’s value theory was useful in explaining the origin of profit, and Böhm-Bawerk’s Marx critique he considered to be marred with misunderstandings and inconsistencies.

<sup>11</sup> Zauberman speaks of an ‘intellectual kinship’ between Dmitriev and the two ‘*prominenti* of the school’, but rightly notes that ‘on the whole, of the two he was closer to Struve’ (1962: 440).



theory, based on the argument that surplus value can arise independently of human labour: according to Struve, “exploitation” simply consists in the appropriation of surplus value by non-producers, irrespective of whether this has been generated by animals, workers, or machines. This proposition, which he confirmed through his analysis in the first *Economic essay*,<sup>12</sup> Dmitriev had clearly taken up from the contemporary economic discourse in Russia.

It would be wrong, however, to associate Dmitriev too closely with the views and ideas of Tugan-Baranovsky. In his *Economic essays*, Dmitriev criticized Tugan-Baranovsky (1890) for having introduced marginal utility theory only in its Austrian variant, based on Menger’s *Grundsätze* and Wieser’s *Natural Value*, and for having adopted a linear or uni-directional, “Austrian” production model in his determination of labour values. He accused Tugan-Baranovsky of having based his argument on a ‘completely arbitrary assumption’, which ‘deprives the solution of the problem of the generality which is required’. He also could not accept, ‘either in form or in content, the “mathematical” solution of the problem which he [Tugan-Baranovsky] proposes at the end of the paper’, because it is based on a ‘completely unreal and arbitrary assumption’ and on ‘equat[ing] incommensurate quantities’ (1974: 45 n). And in his third essay Dmitriev in effect refuted the account of the development of the theory of marginal utility provided by Tugan-Baranovsky (1890). He quoted approvingly Tugan-Baranovsky’s statement that ‘the theory of marginal utility provides us with a fully scientific explanation of the fact (...) that the price of commodities is dependent on the quantity in which they are offered in the market’, but then went on to deny that ‘the honour of having solved this problem belongs entirely to the Austrian school headed by Menger’ (1974: 181). That Dmitriev did not think highly of Tugan-Baranovsky as an economic theorist, and in particular of his application of mathematics to economic problems, is shown also by his review (1909) of the latter’s *Principles of Political Economy* (1909), where he characterized Tugan-Baranovsky’s understanding of Thünen’s theory of wages as insufficient; his view that profit must be analysed independently of values and prices as plainly erroneous; and his formulation of the theory of production costs as inconsistent. More importantly, Dmitriev also rejected Tugan-Baranovsky’s attempted reconciliation of the labour theory of value and the theory of marginal utility: This formulation allows only for the determination of the efficient allocation of the labour of an isolated individual amongst alternative uses.<sup>13</sup> Finally, Tugan-Baranovsky’s priority claim with regard to the “synthetic theory of value” in his textbook Dmitriev considered ‘rather strange’, because similar ideas had been put forward already earlier, in particular by Struve and Zalesky.

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<sup>12</sup> Both Nuti (1974a, b: 18–19), in his Introduction to the English edition, and Denis (1974: 261–269), in his Postface to the French edition, deny that Dmitriev’s argument disproves Marx’s exploitation theory.

<sup>13</sup> For a similar criticism of Tugan-Baranovsky, see Bukharin ([1927] 1970: 169–171). In his review of the habilitation thesis of A. A. Manuilov, Dmitriev (1901) also criticized the author for having presented the marginal utility theory only in the ‘elementary, simplified (and partly distorted) form’ in which it can be found in the works of Menger and Böhm-Bawerk, since in this form it is applicable only to the problem of isolated exchange.

## 5 On the Reception of Dmitriev's Contributions in Russia

In Russia, the history of the reception of Dmitriev's *Economic essays* starts with a short, but highly favourable review by Chuprov ([1905] 1986), which prompted Ladislaus von Bortkiewicz to study carefully the book recommended by his friend. In addition, there was an equally favourable, but much more detailed and partly more critical review article by Shaposhnikov (1905), who tried to explain without technicalities the contents of Dmitriev's treatise to non-mathematical readers. Both agreed on the originality of Dmitriev's analysis in the first essay, but whereas Chuprov praised also the theory of competition in the second essay, Shaposhnikov in his review sought to disprove Dmitriev's argument that under competitive conditions prices are not lowered to necessary production costs because of "realization costs". Thereafter, Dmitriev's theory of competition seems to have been little noticed or discussed in Russia, the main emphasis being on his algebraic analysis of production prices in the first essay. Until the mid-1920s, Dmitriev's contributions were indeed appreciated mainly by a younger generation of mathematical economists and often discussed in the context of the development of some form or other of a "synthesis" between the classical theory of production costs and the marginal utility theory. Chuprov did not participate in these attempts at developing a synthesis, but his high regard of Dmitriev's *Essays* induced him to devote a collective study to the detailed examination of the methods and findings presented by Dmitriev (1911) in his statistical study on the problem of alcohol consumption in Russia. This study was published thanks to Struve, who also wrote a short obituary article after Dmitriev's death ([1913] 2003), in which he noted inter alia that the latter 'presents us with a logically and mathematically thought-out and tested Ricardo' ([1913] 2003: 165).

As Allisson (2015: 135) has noted, Shaposhnikov can be regarded 'as the finest observer of the recent developments in mathematical political economy in Russia' and he was indeed 'one of the very first to consider and appreciate the works of Dmitriev and Bortkiewicz'. In his very accurate assessment of Dmitriev's achievements in his obituary speech at the Chuprov Society, Shaposhnikov (1914) spelt out the implications of Dmitriev's findings for Marx's theoretical construction, which had been made explicit by von Bortkiewicz ([1906–7] 1952). Shaposhnikov had already stressed the originality of Dmitriev's contribution in his remarkable review article of the *Essays* in 1905, and after his graduation from the Law Faculty of the University of Moscow he started work on a dissertation that aimed at synthesizing the recent developments in mathematical economics in the theory of value and distribution. Formally a student of Tugan-Baranovsky, he seems to have had a rather difficult relationship with him,<sup>14</sup> and during his customary study period abroad, from 1906 to 1910, Shaposhnikov worked with von Bortkiewicz in Berlin. In his dissertation on *Theory of Value and Distribution* (1912), which was read by von Bortkiewicz in draft form, he set out his version of a reconciliation between the labour theory of value and the marginal utility theory.<sup>15</sup> Like Dmitriev, he approached the determination

<sup>14</sup> For an account of Shaposhnikov's life and writings, see Allisson (2015: 145–148).

<sup>15</sup> For a summary account, see Allisson (2015: 148–156).

of prices in terms of two systems of equations, for the demand side and the supply side, respectively. On the demand side, his analysis was based, like Dmitriev's, on Walras's exchange equations, whereas for the supply side he restated Dmitriev's equations in terms of the reduction of commodity prices to dated quantities of labour (1912: 41–46). However, because he rejected Dmitriev's theory of competition, he then argued that demand could affect prices only in the case of commodities that are subject to non-constant production costs, thus adopting a marginalist supply-and-demand approach to price determination. He was aware of the fact that on the basis of this approach '[m]any economists regard the contemporary theory of distribution as a sub-division of the theory of value. Wages, profits, and rents are nothing other than the prices of labour, capital, and land. The law of supply and demand determines these values' (Shaposhnikov 1912: 4; quoted in Allisson 2015: 148). However, Shaposhnikov did not follow this "recent trend", but rather sided with Dmitriev and von Bortkiewicz in retaining the asymmetric treatment of the distributive variables, arguing that 'the possibility of an abstract-deductive solution to the problem of distribution seems seriously doubtful' (1912: ii; quoted in Allisson 2015: 153).

Dmitriev's equations for the determination of labour values, as the sum of the amounts of direct and indirect labour embodied in commodities, were restated also by Slutsky ([1910] 2010: 352), who noted in a footnote, however, that he had arrived at them independently, without knowledge at the time of Dmitriev's earlier formulation.<sup>16</sup> From the summary accounts of Slutsky's remarkable master's dissertation provided by Barnett (2011: 207–215) and Allisson (2015: 141–143) it would seem that the main task that Slutsky had set himself was 'to unite the psychological direction with mathematics' (Slutsky 1910; quoted from Barnett 2011: 207). His formulation of the theory of production costs only played a very minor role in this endeavour and in any case would not seem to have derived its main inspiration from Dmitriev's *Economic Essays*. Finally, it should be mentioned that Dmitriev's price equations were restated also by Yurovsky, another mathematical economist and student of Tugan-Baranovsky who attempted to develop an "organic synthesis" in his *Essays on Price Theory* (1919: 100). However, as Barnett (1994) and Allisson (2015: 156–161) have shown, Yurovsky moved further away from Dmitriev's concerns by introducing elements of the Marshallian interpretation of classical economics, such as the distinction between short and long periods, into the analysis.

***Dmitriev's rediscovery in the Post-Stalinist era.*** In his home country, Dmitriev's work was largely forgotten after the 1920s<sup>17</sup> and his name only became better known again when Nemchinov referred to him as 'the first Russian mathematical economist' in 1959, and tribute was paid at the same time to Leontief for having supplied a mathematical interpretation of the overall balance of an economy 'by means of linkage equations of cost and output [of the kind] indicated in the past in the work

<sup>16</sup> See Allisson (2015: 142).

<sup>17</sup> However, in vol. 2 ("The Mathematical School") of Blyumin's *Subjective School and Bourgeois Political Economics* (1928) a whole chapter was devoted to an exposition of Dmitriev's analysis. We owe this reference to Allisson.

of Walras and of Dmitriev' (Nemchinov [1959] 1964: 18; see also 1962: 72; quoted from Nove and Zauberman 1961: 97). In addition, Nemchinov also suggested that Dmitriev's equations for calculating the total labour contents, further elaborated by Lubny-Gercik (1922), formed the basis for the development of the "chessboard balances" and for the advancement of Leontief's input–output methods.<sup>18</sup> In the early 1960s, both Nemchinov and Novozhilov referred to their own work as a continuation of a Russian tradition in mathematical economics that had been initiated by Vladimir K. Dmitriev, and they felt justified in referring to the fundamental input–output equation for the calculation of full input requirements as the 'well-known Dmitriev formula usually employed in the determination of full inputs' (Nemchinov 1961: 129; quoted in Zauberman 1967: 443). Shukhov (1988: 17) later even suggested that Dmitriev's remarks on the efficient use of resources in his review of Tugan-Baranovsky's textbook can be seen as already foreshadowing Novozhilov's later work on optimization techniques.

**Leontief and input–output analysis.** From Nemchinov's and Novozhilov's claims it was only a small step to the suggestion that the "true" origin of Leontief's input–output methods is to be found in Dmitriev's system of equations for the determination of labour values in the opening part of his *Economic essays*.<sup>19</sup> However, the reconstruction of Leontief's path to the development of input–output tables and techniques is a complicated and rather contentious issue, which it seems difficult to settle conclusively. According to Bjerkholt (2016: 26), Leontief might well have been influenced by his reading of Dmitriev's *Essays*, but there is no evidence that he knew them and Leontief himself had no memory of having studied them.<sup>20</sup> This does not exclude the possibility, of course, that the influence on Leontief's ideas was exerted indirectly, and in particular through the "chessboard balances" developed in Russia in the 1920s or through Bortkiewicz's use of Dmitriev's equations and the discussions with him.<sup>21</sup>

**Chayanov.** A related aspect concerns Dmitriev's influence on Alexander Chayanov and, through him, on the development of input–output analysis. Clear evidence exists for a connection between Dmitriev and Chayanov, who recalled in a letter of 1923 that in his early, immature work he had calculated the minimum living wage of the peasant as a constant quantity, although he had formulated already his own theory, from which a variable subsistence wage resulted. Due to a lack of empirical

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<sup>18</sup> See Shukhov (1988).

<sup>19</sup> See, e.g., Clark (1984). For Leontief's early contributions to the development of input–output tables, see Leontief (1925, 1928).

<sup>20</sup> According to Parys (2018: 7), Leontief owned a copy of Dmitriev's *Economic essays* in the original Russian language which however he had acquired only in 1968 in Russia.

<sup>21</sup> It is clear that the roots of Leontief's input–output techniques are to be found in the surplus approach tradition and the works of the classical political economists, including Quesnay and Marx (see Kurz and Salvadori [2000] 2003). Circular flow models in the classical tradition were also explored by Fritz Burchardt, Alfred Kähler, and Adolph Lowe in the late 1920s at the *Institute of World Economics* in Kiel, where Leontief worked from 1927 to 1931; on Leontief's German period see Hagemann (2021).

data and ‘contrary to my theory’ he was compelled, however, ‘to accept a theoretical correction, suggested to me by Dmitriev, who then was my only leader in my work’ (Bourgholzer 1999: 107). However, this statement by itself would seem to be too unspecific to support Nuti’s claim that Chayanov ‘developed Dmitriev’s scheme into an input–output table for agriculture’ (1974a: 11, 1987: 908). Belykh (1989), who investigated the Dmitriev–Chayanov–Leontief line more closely, concluded that Nuti’s attribution of a role to Chayanov in the development of input–output analysis is unfounded, because Chayanov’s labour “norms” ([1925] 1966) are unrelated to input–output coefficients, and his tables for agriculture are no more than modified accounting balances.

The recent re-assessment of Dmitriev’s work in Russia emphasized his role in the development of the classical surplus approach and in statistical analysis (see Klyukin 2008; Bogomazov and Melnik 2014; Belykh 2015).

## 6 The Reception of Dmitriev’s Writings in the West

An account of the reception of Dmitriev’s *Economic essays* in the West must start with reporting a chance not seized. In 1905, Chuprov was invited by Max Weber to contribute an essay on recent developments in the social sciences in Russia to the *Archiv für Sozialwissenschaft und Sozialpolitik*. Chuprov declined this invitation, but suggested Dmitriev’s *Economic essays* for review. In a further letter, Weber confirmed that a review of Dmitriev’s book would be highly welcome, but Chuprov, having reviewed the book already in Russia, declined the invitation (MWG II/4: 493, 495). German-speaking economists therefore only learned of Dmitriev’s *Economic essays* through Bortkiewicz’s footnote reference in his 1906/07 article. For Bortkiewicz, Dmitriev’s essay on Ricardo’s theory of value and distribution was a major source of inspiration and he openly acknowledged this, calling it a ‘remarkable work’, which ‘bears evidence of an exceptional theoretical talent and presents something really new’ ([1906–7] 1952: 20 n; see also Bortkiewicz 1931). Bortkiewicz applied Dmitriev’s formalization of Ricardo’s theory to the critical assessment of Marx’s analysis of the transformation problem, and thereby made the implications of Dmitriev’s findings for Marx’s theoretical construction explicit. In extending Dmitriev’s method of determining prices and the rate of profits to the discussion of Marx’s “law of the tendency of the rate of profits to fall” he overlooked, however, that Dmitriev had jettisoned circular production relations by basing his argument on an Austrian production model.

It is not known whether Dmitriev ever studied von Bortkiewicz’s articles. Shaposhnikov (1914: 8 n) reports to have had numerous discussions with both von Bortkiewicz and Dmitriev, so that it would seem rather unlikely that the latter never learned of the former’s contributions. In the West, the recognition of Dmitriev’s contributions was closely related to the reception of Bortkiewicz’s two articles on Marx’s theory of

value and distribution.<sup>22</sup> These, however, were properly appreciated only by a handful of specialists, and none of the authors who sought to elaborate on von Bortkiewicz's contributions in the German-language area, including von Charasoff (1909, 1910), Moszkowska (1929), and Grossmann (1929), appears to have followed up the reference to Dmitriev's *Economic essays*. The only author who may perhaps have done so was Peter (1933, 1934), who referred to the 'Dmitriev-Bortkiewicz system of equations', the 'Dmitriev-Bortkiewicz theory of the real wage', the 'Dmitriev-Bortkiewicz method' of determining the rate of profit (1933: 162, 164, 165), and even the 'mathematical formulation of the objective theory of prices of Dmitriev-Bortkiewicz' (1934: 41)—but also in his case a direct reference to Dmitriev's *Economic essays* is missing.

Since Sweezy (1942: 115–130) in his summary account and discussion of Bortkiewicz's articles on the transformation problem failed to mention the latter's acknowledgement of his indebtedness to Dmitriev's *Economic essays*, these did not benefit from the attention that Bortkiewicz's papers started to receive in the English-speaking world after the publication of Sweezy's 1942 book and his subsequent inclusion of an English translation of Bortkiewicz's 1907 paper in Sweezy (1949). It was therefore only in the early 1960s, when Alfred Zauberman became aware of Dmitriev through Nemchinov's reference to him, and started to publish several papers on the *Economic essays*,<sup>23</sup> that Western scholars first began to study his work. With the French translation by Zauberman in 1968, and the English translation, edited and introduced by Nuti, in 1974, Dmitriev's writings then first became more easily accessible also to Western scholars.<sup>24</sup> In the 1960s and 1970s, attention focused again almost exclusively on his value and price theory in the first essay, and it was then recognized that Dmitriev had anticipated some concepts and findings of Piero Sraffa's *Production of Commodities by Means of Commodities* (1960), and in several publications it was pointed out that Dmitriev's contribution in the first essay was firmly rooted in the classical surplus approach tradition.<sup>25</sup>

Dmitriev's second essay also remained unknown in the West, and accordingly it could make no impact on the development of (imperfect) competition theory in the first half of the twentieth century.<sup>26</sup> In the German-speaking countries, where Dmitriev's competition theory could perhaps have been expected to attract some

<sup>22</sup> For a more detailed account of the reception of von Bortkiewicz's two papers, see Desai (1988: 312–328), Howard and King (1989: 59–62, 1992: 227–245), and Gehrke and Kurz (2021).

<sup>23</sup> See Zauberman (1960, 1962, 1967, 1969) and Nove and Zauberman (1961).

<sup>24</sup> Nuti (1974a, b: 30) observed that the only known copy (in the original Russian language) of Dmitriev's *Economic essays* in the West was in the library of Sraffa. Apparently, Nuti was unaware of the fact that Leontief also owned a copy (see note 18 above). According to the recent catalogue of Sraffa's library by de Vivo (2014: xxxix, 138), Sraffa acquired this copy only in the 1960s.

<sup>25</sup> See, for instance, Skourtos (1985, 1986), Schefold (1986, 1992), Kurz and Salvadori (1995, 2002), and Schütte (2003).

<sup>26</sup> According to Samuelson, the development of imperfect competition theory was impeded for several decades because the English and Continental theorists first had to free themselves from the influence of Marshall and Edgeworth. With regard to Dmitriev's contribution to imperfect competition theory, Samuelson therefore suggested that his ignorance of Marshall's and Edgeworth's verdict on Cournot's theory of competition may have been fortuitous, because it saved him from becoming entangled in their rejection of this line of research.

attention, both because of Bortkiewicz's reference to the *Economic essays* and also because of Dmitriev's discussion of the contributions of the Austro-German scholars Auspitz and Lieben and Launhardt, no one seems to have taken any notice. But at the turn of the century the interest in pure economic theory, and even more so in mathematical economics, was at a very low ebb in the German-speaking countries. (This also explains why Launhardt's contributions were not properly appreciated before the mid-1930s.) Later on, when important contributions to imperfect competition theory were made by authors like von Stackelberg, Schneider, and others, the unknown Russian author apparently escaped these scholars' attention.

With the rediscovery of Dmitriev's *Essays* in the 1960s also his theory of competition was acclaimed as a major achievement by several commentators. Denis, in his post-face to the French edition, drew attention to the 'high quality' and 'rigorous execution' of Dmitriev's analysis (1968: 262, 267); Nuti called it a 'refreshing and highly relevant contribution to a field that had come to a standstill' (1974a: 7); and Samuelson praised Dmitriev's recognition of a tendency to collusion in the presence of excess capacities, his hint at the wasting of resources in competitive conditions ('an argument that Chamberlin would have liked'), and his 'modern and impressive' reference to cyclical aspects of oligopolies (1975: 494). Only Dobb (1974: 985–986) raised doubts about the plausibility of Dmitriev's argument, along lines very similar to those of Shaposhnikov (1905, 1912). According to Schütte (2003: 160–172), Dmitriev had indeed anticipated some elements of Chamberlin's approach to imperfect competition theory and demonstrated that a homogeneous oligopoly has a determinate solution that corresponds to an implicit collusion. Dmitriev correctly perceived the analogy between military and oligopolistic strategies and also provided the same explanation for implicit cooperative behaviour as modern game-theoretic approaches to oligopoly theory. In fact, by separating the producers' decisions on production levels from those on sales (by means of inventories or excess capacities), he introduced sunk costs and a two-step recursive solution procedure, in precisely the same way as this is done in sequential games for oligopolistic markets of the 1970s and 1980s.

## 7 Concluding Remarks

There can be no doubt that Dmitriev and his remarkable *Economic essays* deserve a prominent place in a volume on the West-East-West transfer of economic ideas. Dmitriev was an original and profound thinker, a knowledgeable historian of ideas with a firm and sober judgement, and a first-rank economic theorist. He contributed to the introduction and recognition of several concepts and ideas from the West in Russia, and by combining them with ideas developed by Russian authors and the application of mathematical analysis he was able to establish a number of important results in the theory of value and distribution. He confirmed several of Ricardo's findings by means of algebraic analysis and disposed of some erroneous ideas that had been introduced by some of his followers. He was unsuccessful,

however, in his attempted reconciliation of the classical theory of natural prices with the theory of marginal utility, which was clearly inspired by the contributions of Tugan-Baranovsky and Struve, but on which he further intended to elaborate by the introduction of the Walrasian system of simultaneous equations.

With regard to the retransfer of Dmitriev's work on Western economic ideas to the West, two different phases can be distinguished. In the first phase it was only through von Bortkiewicz's application of Dmitriev's findings to Marx's transformation problem that his work exerted some (rather limited) influence on the development of economic theory in the West. Apparently, in Dmitriev's case the international transfer of his findings was seriously impeded by a number of obstacles, amongst which the language barrier and the poor state of theoretical economics in the German-speaking countries at the beginning of the twentieth century played an important role. Dmitriev's work was properly appreciated in the West only from the late 1960s onwards, when his *Economic essays* had become more easily accessible also to Western scholars through French and English translations.

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# Ladislaus von Bortkiewicz: Traveller Between Worlds



Christian Gehrke and Heinz D. Kurz

## 1 Introduction

Ladislaus von Bortkiewicz (1868–1931) can be considered in many respects as a central figure in the West–East/East–West migration of ideas in economic theory at the turn of the twentieth century. As a law student at St. Petersburg University, he studied Léon Walras’s *Éléments* and started a correspondence with the Lausanne economist; later, he contributed to the dissemination of the Walrasian theory of general economic equilibrium by inducing a number of Russian mathematical economists, including A. A. Chuprov, N. N. Shaposhnikov and L. N. Yurovsky, to study it carefully (see Allisson, 2009). In 1906–07, as an associate professor of statistics at the University of Berlin, he applied some concepts and ideas of the Russian mathematical economist Dmitriev ([1904] 1974) to the critical assessment of Marx’s theory of value and distribution. Through Bortkiewicz’s article (1906–07), Dmitriev’s findings became known also to Western scholars interested in the development of the classical surplus approach. At the same time, Bortkiewicz demonstrated by means of his “correction of Marx’s fundamental theoretical construction” ([1907a] 1949) that Mikhail Tugan-Baranovsky (1905) had been wrong in concluding that Marx’s erroneous transformation procedure was non-rectifiable. In 1927–28, Bortkiewicz served on the examination board of Wassily Leontief’s doctoral dissertation (1928) at the University of Berlin. He thus also contributed to the East–West “remigration” of the economic circular flow idea, which Leontief seems to have picked up earlier in

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Soviet Russia from his study of Quesnay's *Tableau économique* and Marx's reproduction schemes, and perhaps also from the "chessboard balances" that were used in the development of planning methods in the early 1920s in the Soviet Union.

As the following essay will show, Bortkiewicz was a rigorous and uncompromising scrutinizer of theoretical concepts and ideas, whose encyclopaedic knowledge of economic theories allowed him to assume the role of a mediator between various theoretical approaches at a time when language barriers and differences in national discourses and economic traditions were still of great importance. Joseph A. Schumpeter's qualification (1954: 851) that Bortkiewicz was a "comma hunter" was clearly off the mark. We start, in Sect. 2, with a brief account of Bortkiewicz's life in order to make clear why he was in a special position that allowed him to foster the exchange of ideas between Russia and the West. In the next section, we then provide a summary account of Bortkiewicz's most important contributions to economic theory. Section 4 is concerned with the reception of Bortkiewicz's economic contributions in Russia and in the West. Section 5 concludes.

## 2 Bortkiewicz's Life and Academic Career

Ladislaus von Bortkiewicz was born in St. Petersburg on 7 August 1868 into a wealthy family of Polish descent. His father was a colonel and military instructor teaching artillery and mathematics; his mother made sure that her children learned German and French. Two of the three children also developed a strong interest in mathematics: Ladislaus and Helene. However, Ladislaus—unlike his younger sister<sup>1</sup>—never obtained a proper mathematical education apart from the usual courses at a humanistic gymnasium. He studied several subjects: law at the university of his hometown, where he graduated in 1890, political economy and statistics first in Strasbourg from 1891 to 1892, then under the supervision of the eminent German statistician Wilhelm Lexis in Göttingen in 1892. However, he had begun to study mathematics, statistics, and economics by himself already in his student days and even before his graduation had managed to publish two papers on population statistics (in Russian) and a review article (in French) on the second edition of Walras's *Eléments* (Bortkiewicz, 1890). In 1888, he not only began a correspondence with Léon Walras but also wrote a letter to the eminent German statistician Georg Friedrich Knapp, in which he suggested a reform of the methods used in estimating mortality. Knapp was sufficiently impressed to answer the letter, saying that he would be delighted to meet the author. Three years later, Bortkiewicz studied with Knapp at the University of Strasbourg, where the Austrian socialist Carl Grünberg was one of his fellow students. After Bortkiewicz had earned his doctorate with Wilhelm Lexis at the University in

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<sup>1</sup> Helene von Bortkiewicz (1870–1939) was one of the first female students of mathematics to attend "Higher women's courses" at the University of St. Petersburg. She subsequently studied mathematics with David Hilbert in Göttingen and upon her return to Russia published several mathematical papers in Russian journals. From 1910 to 1914 and from 1919 to 1931, she lived with her brother in Berlin (cf. Härdle & Vogt, 2015).

Göttingen with a statistical thesis on life expectancy in 1893 (see Borkiewicz, 1893), he returned to Strasbourg, where he began to lecture on statistics and actuarial science as a “Privatdozent” from 1895 to 1897. There he developed a close and life-long friendship with Aleksandr Aleksandrovich Chuprov, who wrote his dissertation with G. F. Knapp. In 1897, Bortkiewicz returned to St. Petersburg, from where he applied for a professorship in Russia. But when his attempts failed, he had to accept a post as a clerk in the Ministry of Transport in his hometown. Thanks to A. A. Chuprov’s father, Aleksandr Ivanovich Chuprov, a professor of economics and statistics at the University of Moscow (and author of the leading Russian textbook on economics at the time), he also taught statistics at the prestigious Aleksandrovsky Lyceum. In 1901, Bortkiewicz became an extraordinary professor of statistics at the Friedrich Wilhelm University (now renamed Humboldt University) in Berlin—a position he had been offered upon the recommendation of Wilhelm Lexis. He remained in this position until 1920, when he was appointed to the chair of statistics and political economy,<sup>2</sup> which he held until he passed away rather unexpectedly on 15 July 1931, aged 62. Bortkiewicz received several honours, including membership of the *Royal Swedish Academy of Sciences*, the *Royal Statistical Society*, the *American Statistical Association*, and the *International Statistical Institute*. He was a member of the *Gesellschaft für Soziologie*, a founding member of the *Deutsche Gesellschaft für Versicherungswesen* and a regular attendant of the meetings of the *Verein für Socialpolitik*. He was actively involved also in the foundation of the *Econometric Society*, of which he became a council member in 1930.

Bortkiewicz’s exceptional theoretical talent showed early on and could have secured him one of the most prestigious chairs in economics in Western Europe. When Léon Walras’s health deteriorated and caused him to retire from his chair in Lausanne in 1892, he asked Bortkiewicz whether he would become his successor, reflecting an enormous esteem for the 24-year-old. It was only after Bortkiewicz had told him that he had turned to statistics and was not interested<sup>3</sup> that Walras, upon Maffeo Pantaleoni’s advice, supported the appointment of Vilfredo Pareto. Due to the latter’s intervention, Pantaleoni was then preferred over Bortkiewicz when in 1896 a professorship at the University of Geneva had to be filled, although the two candidates had been ranked *ex aequo* by Walras, who however had also written “un vibrant panégyrique (a vibrant eulogy)” in favour of Bortkiewicz (see Bridel, 2008: 721). He was again passed over when in 1899 a replacement for Pareto in Lausanne had to be found—presumably due to his rather disparaging review of Pareto’s *Cours* (see Bortkiewicz, 1898a) in the preceding year.<sup>4</sup>

***Bortkiewicz’s relations with Russia and Russian economists.*** Except for the brief period from 1897 to 1901, when he taught statistics at the Aleksandrovsky Lyceum

<sup>2</sup> Bortkiewicz’s appointment to a chair did not involve an increase of his salary. This was part of a measure, purportedly intended to “democratize” faculties, by which *all* extraordinary professors became full professor *ad personam* (Sheynin 2001: 227).

<sup>3</sup> The main reason why Bortkiewicz turned down the offer was that he had hoped to succeed the statistician and economist Y. E. Yanson in his hometown (Baranzini & Allisson, 2015: 282).

<sup>4</sup> See Gattei (1982).

in St. Petersburg, Bortkiewicz spent his entire academic career at German universities. He nevertheless had close contacts with many Russian economists and statisticians also after he had settled for good in Berlin, and several young mathematical economists and talented statisticians from Russia regularly passed through Berlin in order to visit him and seek his advice. Bortkiewicz also frequently invited visitors, colleagues, and students to his apartment in Berlin-Halensee,<sup>5</sup> which he shared with his sister Helene. Because of his wide-ranging intellectual interests (which included mathematics, mathematical and theoretical statistics, economic theory, sociology, and actuarial science), his encyclopaedic knowledge of the Western and Russian scientific literature, and his close personal relations with leading statisticians, sociologists, and economic theorists in Russia and Western Europe, he was arguably one of the most important mediators of ideas between Russia and the West around the turn of the twentieth century.

Besides his close friendship with the mathematician, statistician, and economist A. A. Chuprov,<sup>6</sup> Bortkiewicz was in regular contact with a number of further prominent Russian statisticians and mathematicians, including A. Markov, A. A. Kaufman, and N. S. Chetverikov, as well as with O. Anderson, a former student and then assistant of Chuprov. The statistician M. Ptukha and the mathematical economist N. N. Shaposhnikov, who had both studied with him in Berlin from 1906 to 1910, also became regular correspondents and frequent visitors after their return to Russia. Another regular correspondent, with whom he exchanged articles and discussed issues in statistics and economic theory, was E. E. Slutsky (Allisson, 2015: 59). After WWI and the Russian revolution, Bortkiewicz attracted anew a number of excellent Russian students, among them Wassily Leontief and Jacob Marschak. In the mid-1920s, Ladislaus and Helene Bortkiewicz developed a friendship with the Russian couple Vladimir S. and Emma S. Woytinsky,<sup>7</sup> whose famous seven-volume work *Die Welt in Zahlen* (1924–1928) was published in Berlin under the editorship of Bortkiewicz.<sup>8</sup>

Efforts were made, both by himself and by others, for an appointment of Bortkiewicz to a chair in Russia, and it seems that he seriously contemplated a return to Russia in at least three different phases of his life: in 1897–1901, in 1905, and in 1912. In the period from 1897 to 1901, Bortkiewicz himself actively tried to obtain a chair in Russia, both in St. Petersburg and Moscow, as his correspondence

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<sup>5</sup> In the 1920s, the Halensee district had a large Russian émigré community.

<sup>6</sup> Chuprov obtained his degree in 1896 in mathematics and statistics and then stayed at several German universities to study social sciences, including Berlin, where he studied with Adolph Wagner, and Strasbourg, where he worked with G. F. Knapp. From 1902 to 1917, he taught statistics at the St. Petersburg Polytechnic Institute and after the October revolution emigrated to Western Europe (Sheynin, 2005).

<sup>7</sup> Details of their relationship with Bortkiewicz are given in the autobiographies of Woytinsky (1961: 452–3) and Woytinsky (1965: 108–110).

<sup>8</sup> Originally, the book series was supposed to be published in both German and Russian, but only two volumes were published in Russian in 1924 and 1925, and then the project was suspended.



with A. A. Chuprov shows.<sup>9</sup> However, the fact that he held no Russian degree and almost all his publications were in German was a major obstacle. In 1905, when A. A. Chuprov held the chair of statistics at the St. Petersburg Polytechnic Institute,<sup>10</sup> Bortkiewicz seriously considered accepting a job offer of an actuary at the Department of the State Savings Banks and of combining this with a chair of insurance at the Polytechnic Institute. His reasons for his decision to stay in Berlin he explained in a letter to Chuprov of 22 July 1905:

Here I do not feel myself badly at all. On the contrary, it is wonderful as far as the kind, the conditions, and the place of work are concerned. Only one circumstance is troublesome, although not really in earnest: the remuneration is comparatively small. ... As compared with the present, in Petersburg, if employed in the Ministry and being a professor, I will at once become rich. ... [But] we should bear in mind the possibility of an abrupt change of the general direction of the official policy, if not of the entire structure of political life. (Letter excerpts quoted from Sheynin, 2019: 75)

A further attempt for an appointment at the University of St. Petersburg was made in 1912, when his former student Ptukha informed Bortkiewicz in a series of letters about the situation at the Law faculty, where the chair of political economy was vacant and Tugan-Baranovsky was proposed as a candidate and likely to be elected. But then, an influential professor suddenly nominated Bortkiewicz (without the latter's consent) and because Bortkiewicz had no Russian scientific degree also proposed the conferment of a doctorate *honoris causa* to him. Many professors supported this suggestion, but to get it approved by the ministry would have required time and effort. Bortkiewicz therefore refused to participate in the ballot, and Tugan-Baranovsky was elected. Also in 1912, the statistician A. A. Kaufman took some measures for electing Bortkiewicz to correspondent membership of the St. Petersburg Academy of Sciences. Although in case of his election this would have required his return to St. Petersburg, Bortkiewicz did not object—but then it turned out that there were no vacancies.<sup>11</sup>

Bortkiewicz visited St. Petersburg for the last time in the summer of 1914 in order to attend the funeral of his father; thereafter, he never saw Russia again. At least until WWI, he retained strong connections with Russia, and with an appropriate offer he might possibly have returned to his hometown. The statistician P. D. Azarevich wrote in 1912: “Each time I see Bortkevich, I regret that we have let him go. He is a veritable man of science!”<sup>12</sup>

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<sup>9</sup> Bortkiewicz in this period belonged to the staff of the Directorate of the State Railroads in St. Petersburg—a fact he concealed from the administration of Strasbourg University, because it would have implied that he could not have retained his status as a “Privatdozent”.

<sup>10</sup> See Hårdle and Vogt (2015: 20).

<sup>11</sup> See Sheynin (2019: 66). Kaufman also attempted to publish a collection of some of Bortkiewicz's papers in Russian, but the project was suspended with the outbreak of WWI.

<sup>12</sup> Quoted from Sheynin (2019: 64). Bortkiewicz also maintained active contacts with Polish statisticians and economists and was sounded out on accepting a chair at the Universities of Warsaw and of Crakow. A curious fact, reported by Sheynin (2018: 49–50), is that Bortkiewicz was released from his teaching duties and employed as a “statistical assistant” in the civil administration of the General Governorate of Warsaw from November 1916 to January 1917, where he wrote a memorial on behalf of the Polish Government with reference to the life-insurance of mortgagers.

*Bortkiewicz's relations with German and Austrian economists.* In his obituary article, the statistician and economist Oskar Anderson observed:

Although von Bortkiewicz spent more than a half of his life as an instructor at two German universities, he remained to a certain extent foreign to them. To a much higher degree he was a Russian rather than a German professor and scholar. Scientifically, von Bortkiewicz was somewhat isolated; he won incomparably greater recognition abroad than in Germany itself where he had hardly any followers. (Anderson 1932: 246)

This statement needs some qualification. Although Bortkiewicz indeed had few (if any) “followers” in Germany, many leading social scientists in the German-speaking countries held him in high regard. A prominent case in point is Max Weber,<sup>13</sup> who variously expressed his esteem of Bortkiewicz and repeatedly recommended him for a professorship—and this in spite of the fact that Bortkiewicz had more than once severely criticized him (as well as also his brother, Alfred).<sup>14</sup> As Weber’s correspondence shows, Bortkiewicz was offered a professorship at the University of Basel in 1913, but he informed the appointment committee that he had no intention of leaving Berlin.<sup>15</sup> In 1918, Bortkiewicz was recommended by Max Weber and Knut Wicksell for one of the two vacant chairs formerly held by Philippovich and Wieser at the University of Vienna,<sup>16</sup> but in the end, Wieser’s chair was left unfilled (and O. Spann was newly appointed).<sup>17</sup> In 1919, Bortkiewicz was also proposed by Weber as “a candidate in the first line” when the chair of Mayr had to be filled at the University of Munich (MWG II/10.2: 729). Three years later, when Wieser finally retired, Bortkiewicz was again under consideration as Wieser’s successor and strongly promoted by Carl Grünberg but in the end rejected by the faculty.<sup>18</sup> Interestingly, Max Weber in 1909 also invited Bortkiewicz to contribute the entry on “Dogmen- und Methodengeschichte” (Economic Doctrine and Method)

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<sup>13</sup> Apart from Weber and, of course, Lexis and Knapp, one could also mention, for instance, Werner Sombart, Emil Lederer, Knut Wicksell, and Ferdinand Tönnies.

<sup>14</sup> In 1911, at a meeting of the *Verein für Socialpolitik*, Bortkiewicz harshly criticized the method adopted in a study on “Industrial workers”, which had been carried out under Max Weber’s guidance. Weber replied to Bortkiewicz’s intervention in the “concluding discussion” and then wrote him a letter, in which he defended the young author of the study but fully accepted Bortkiewicz’s criticism. For Bortkiewicz’s critique and Weber’s reaction, see Bortkiewicz (1911) and (MWG II/7: 282–3). See also Bortkiewicz’s severely critical review (1910) of Alfred Weber’s book on the location of industries.

<sup>15</sup> See MWG (II/8: 256).

<sup>16</sup> See Wicksell’s letter of 7 November 1919 to Bortkiewicz (in the Bortkiewicz archive, Uppsala) and Max Weber’s letter to Carl Grünberg (MWG II/10.1: 79–82 and 176–178).

<sup>17</sup> See Klausinger (2016: 215–7). In a letter to Bortkiewicz of 11 December 1919, Knut Wicksell expressed his disappointment about the decision in Vienna and added: “Among all the critics of Böhm-Bawerk you are one of the few who have put forward real reasons. This should be regarded as an achievement, not as a shortcoming—as I have also written to the appointment committee”. (Bortkiewicz archive, Uppsala).

<sup>18</sup> Spann and Wieser rejected Bortkiewicz by arguing that he was primarily a theoretical statistician and not an economist, and moreover a critic of the Austrian school (Klausinger, 2016: 145).

to his planned *Handbuch zur Politischen Ökonomie* (later renamed *Grundriß der Sozialökonomik*) (MWG II/11: 66–8), but the latter turned down the offer.<sup>19</sup>

Bortkiewicz thus spent almost his entire academic career at the University of Berlin, where he mainly taught courses on statistics but occasionally could lecture also on “general theoretical economics”, “theories of value”, “value and price theories”, “the socialist theory of the economy (presentation and critique)”, and “contested issues in theoretical economics”.<sup>20</sup> Although admittedly not a good teacher, he occasionally attracted some excellent students, such as Otto Neurath, for instance, who studied statistics with Bortkiewicz and became involved in debates on Marx when he studied in Berlin from 1904 to 1906. Moreover, in the period from 1926 to 1929, a group of mathematicians and mathematical economists around Bortkiewicz, which included Robert Remak and John von Neumann, discussed models of prices and income distribution with distinctively classical features.<sup>21</sup>

### 3 Bortkiewicz’s Work in Economic Theory

Bortkiewicz’s work is wide-ranging, covering mathematical statistics, actuarial science, economics, mathematics, and physics. He was well read in all major economic theories, including those of David Ricardo, Karl Marx, Léon Walras, Eugen von Böhm-Bawerk, and Vilfredo Pareto, and he was keen to identify the differences and similarities between them and whether and when there was progress in the discipline. For example, his observations on major authors, such as Ricardo, Marx, or Böhm-Bawerk, were typically embedded in more general discussions and assessments of the developments in economic analysis. Bortkiewicz’s main interest was the theory of value, capital, and income distribution. He admired David Ricardo, but also Marx and Walras. His analytic mind was acute and uncompromising. He did not allow sloppy arguments to pass unnoticed and therefore was feared as a “taskmaster” in the profession.

In the following, the main focus is on Bortkiewicz’s contributions to economics. We begin with a discussion of his juvenile “collaboration” with Léon Walras. Then we deal with his critique of Böhm-Bawerk’s theory of interest and his comparative assessment of the theories of value and distribution of David Ricardo and Karl Marx.

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<sup>19</sup> Bortkiewicz declined the offer on the ground that he was currently occupied with other work, whereupon Weber in a further letter invited him to write on some other topic that was more in line with his current field of research, assuring him that he would see to it that an appropriate title for his contribution would be found—but Bortkiewicz turned down also this offer (MWG II/11: 69).

<sup>20</sup> Upon his appointment in 1901, Bortkiewicz agreed to lecture also on “The economic conditions in Russia” and to conduct classes in collaboration with the “Russian Seminar”, but he never seems to have done so (Sheynin, 2020: 48). A full list of the courses he taught at the Friedrich Wilhelm University is available at [Wiwi.hu-berlin](http://Wiwi.hu-berlin) (2012), and some of his lecture notes are preserved in the Bortkiewicz Papers at the University of Uppsala.

<sup>21</sup> See Kurz and Salvadori (1995: 397–407).

### *Bortkiewicz and Walras*

The relationship between Bortkiewicz and Léon Walras was largely unknown until William Jaffé's edition of the correspondence of Walras (1965). It turned out that Bortkiewicz had written to Walras when he was barely 20 years of age and that the latter had found it rewarding to exchange altogether 56 letters with him, 31 of them written by Walras, in the period between 1887 and 1899.

Towards the end of the 1880s, the young student from St. Petersburg was complaining in his private correspondence that Russia remained "totally foreign to the recent advance in the theory of [the] science [of economics]". In Russia, this science was still "dominated by the preponderant influence of the German economic science", but Bortkiewicz was confident that in an upcoming reaction against historicism, the mathematical method would be introduced also in Russia. Bortkiewicz not only shared Walras's enthusiasm for the mathematical method but also assured him that the spirit of the Walrasian system "is inherent to my way of conceiving the economic world".<sup>22</sup>

As a reflection of their discussions and in an attempt to defend Walras against some criticisms put forward by Francis Y. Edgeworth, Bortkiewicz published a review article of the second edition of the *Éléments* (Bortkiewicz, 1890). Later followed a short review of *Etudes d'économie sociale* (Bortkiewicz, 1898b).<sup>23</sup> The main themes discussed in the "triangular debate" were essentially the following: (1) capital theory and especially the role of production costs in the determination of the prices of new capital goods; (2) the theory of exchange and the concept of competition; and (3) the concept of *tâtonnement*. Walras apparently tried to manipulate the young Russian on his behalf in his controversy with Edgeworth, but Bortkiewicz was far too clever to get tricked by him. Bortkiewicz questioned the validity of Walras's proof of the "theorem of maximum utility of newly produced capital goods proper", with regard to which Walras contended that the costs of production of the goods played no role. As later discussions demonstrated, Bortkiewicz was right in this regard, but he could not convince Walras.

In his review of the second edition of the *Éléments*, Edgeworth (1889: 435) maintained that Walras went "too far in the way of abstraction when he insists that the ideal *entrepreneur* should be regarded as 'making neither gain nor loss'" (original emphasis). This was in fact a criticism of too narrow a concept of competition Walras was said to have entertained. Bortkiewicz rushed to Walras's defence by arguing that only the ideal entrepreneur is compatible with Walras's concept of general equilibrium. Soon afterwards, however, he appears to have got doubts and was willing to admit that there is a difference between competition in industry and in commerce. Edgeworth had also objected to Walras's construction that "the equations of exchange are of a statical, not a dynamical, character. They define a position of equilibrium,

<sup>22</sup> See the letters of Bortkiewicz to Walras of 13 September 1891, in Jaffé (1965, II: 1024), and of 12/24.04.1888, in Jaffé (1965, II: 829).

<sup>23</sup> For summary accounts of the debate among the three authors, see Marchionatti (2007) and Bridel (2008).

but they afford no information as to the path by which that point is reached” (Edgeworth, 1889: 435). Walras, who had prided himself with having elaborated a general analysis of the gravitation to equilibrium, was mistaken. Bortkiewicz in his reply to Edgeworth, which was endorsed by Walras, defended the concept of a “realistic” process of *tâtonnement* but admitted that there might be several methods of arriving at the equations and that Walras’s assumption that there is no trade out of equilibrium might be replaced by one that reflects the actual “practices in markets”. The real problem under discussion was, of course, whether the equilibrium, if it existed, was stable or not. This was crucial, because if it happened to be unstable, what was the use of equilibrium theory?

Later in his economic writings, Bortkiewicz variously referred to Walras’s general equilibrium theory and he even suggested to incorporate the classical cost of production equations into a Walrasian system (see Bortkiewicz, 1921). In this way, he sought to integrate the objectivism of the classical authors and Marx, on the one hand, and the subjectivism of the marginalists, on the other, in a single theory. It can however be shown that Walras up until the fourth edition of the *Éléments* was (erroneously) of the opinion that his system reflected a long-period equilibrium, characterized by a uniform rate of net profits, and that the proposed equations satisfied the “law of cost of production” (see Kurz & Salvadori, 1995: 24–5, 439–41).

### ***Bortkiewicz on Böhm-Bawerk***

In his criticism of “the cardinal error” of Eugen von Böhm-Bawerk’s theory of capital and interest (profits), Bortkiewicz criticized the “Three Grounds” put forward by the Austrian in favour of a positive rate of interest: (1) the differences between wants and provision in different periods of time; (2) the systematic underestimation of future wants and the means available to satisfy them; and (3) the technical superiority of present compared with future goods of the same quality and quantity. Bortkiewicz focused attention on the third ground—according to Böhm-Bawerk (1889 [1902]: 286) the “main pillar” of his theory—which referred allegedly to a “purely objective factor” (Bortkiewicz, 1906: 945).

Bortkiewicz distinguished between three types of approaches to the theory of interest, only one of which met what he called the “touchstone” of the theory of profits:

I believe that this can be regarded as *the touchstone of such a theory*: whether it is able to show the *general cause of interest* also for the case in which not only *no technical progress*, of whichever type, takes place, but also *the length of the periods of production appears to be technically predetermined*, so that *no choice* is possible between different methods. (Bortkiewicz, 1906: 970–71; emphases added)

In other words, interest ought to be explained in conditions of a given system of production and neither in the context of a choice of technique problem nor as a fruit of technical progress. As Bortkiewicz made clear, two of the most popular theories of profits at the time did not meet these criteria: John Bates Clark’s marginal productivity theory of capital and Eugen von Böhm-Bawerk’s “Austrian” theory of capital and interest. The former explained profits in terms of the marginal productivity

of capital and thus did not start from a given technical system of production, and the latter presupposed a variable length of the average period of production and thus a dynamic element. Elsewhere Bortkiewicz expounded the implications of his postulate with regard to the theory of value:

Now my opinion is that in general the value of goods can only depend upon such *technical knowledge as is applied in practice*. But the value of goods remains unaffected by *knowledge, which, on whatever grounds, is not utilized*. The result thus obtained can be summed up in the following brief formula: *for [the determination of] the value of goods there come into consideration only actual methods of production, and not merely potential ones.* (Bortkiewicz, 1907b: 1299; emphasis added)

The third ground concerning the superiority of “more roundabout” processes of production Böhm-Bawerk had attempted to illustrate in the *Positive Theory of Capital* in terms of a numerical example. According to Bortkiewicz, the example was misleading because Böhm-Bawerk had given only an incomplete picture of the case under consideration. The example refers to production processes started in consecutive years. Alas, Böhm-Bawerk had assumed without any justification that all processes stop at the end of the process started first. If each process was instead taken to break off after the same number of years as the first one, we arrive at a uniformly staggered system of production. Now, the process started first is no longer superior to all other processes with regard to all future time periods, because after its truncation, the other processes still generate outputs, whereas the first one no longer does. Bortkiewicz (1906: 958) concluded that, “seen from a purely formal point of view, [Böhm-Bawerk] did not reason correctly. His argumentation, on which he puts the main weight, suffers from an internal mistake”.

Bortkiewicz was also critical of the other two grounds and particularly of Böhm-Bawerk’s argument in favour of a positive rate of time preference. He insisted that one ought to be “extremely cautious” with any sort of “psychological reasoning” and (as Friedrich von Wieser, Böhm-Bawerk’s brother-in-law, had argued before him) that it would have to be shown that a positive time preference exists independently of the phenomenon of interest, because if the latter is positive, the former must necessarily be positive too: a positive time preference would have to be shown to be the “*prius*” relative to the phenomenon of interest (Bortkiewicz, 1906: 948). He also attacked the view that a positive time preference follows from the fact that all future possessions are more or less uncertain. Since Böhm-Bawerk was concerned with explaining interest proper, that is, net interest as opposed to gross interest, which includes a risk premium designed to take account of the element of uncertainty just mentioned, myopic behaviour due to uncertainty can play no role in his argument. “Taken all together”, Bortkiewicz concluded, “the purely subjective foundation of Böhm-Bawerk’s doctrine turns out to be uncertain and precarious” (Bortkiewicz, 1906: 950). In a review of the fourth edition of Böhm-Bawerk’s *Capital and Interest* ([1889] 1924), edited and introduced by Friedrich von Wieser, Bortkiewicz (1925b) agreed with Wieser’s objection to Böhm-Bawerk’s agio theory but pointed out that it was inconsistent of Wieser to reject the agio theory and at the same time to endorse and praise Böhm-Bawerk’s critique of the socialist withholding theories, because this critique was based entirely on the agio theory.

### ***Bortkiewicz on Ricardo and Marx***

Which theory, if any, met the touchstone criteria? According to Bortkiewicz, it was the theory of the classical economists in the form David Ricardo gave it. Bortkiewicz drew the attention to this fact especially in his essay “Wertrechnung und Preisrechnung im Marxschen System” (“Value and price in the Marxian system”), published in three instalments in 1906–07 (only parts 2 and 3 have been translated into English; see Bortkiewicz, 1952).

The essay is actually as much about Ricardo as it is about Marx. In his essay, Bortkiewicz referred to the work of the Russian mathematical economist Vladimir K. Dmitriev, to which his attention had been drawn by A. A. Chuprov. Dmitriev in a paper published in Russian in 1898 had formalized Ricardo’s approach to the theory of value and distribution and had shown that the rate of profits and relative prices can be determined once “the technical conditions of production of commodities (including the commodity labour power) are given” (Bortkiewicz, 1906–07, II: 39).<sup>24</sup> Besides the system of production and the real wage rate (that is, the remuneration of the “commodity labour power”), no other data were needed.

### ***Prices and profits***

Bortkiewicz (1906–07) took Dmitriev’s formalization as the starting point of his own analysis and assumed unidirectional production processes of finite duration, that is, one-way avenues starting from what Ricardo had called “unassisted labour” via a number of intermediate products or capital goods to final outputs. In such “time-phased” production processes, prices of commodities can also be conceived in terms of what Sraffa (1960) called “dated quantities of labour”, with the dated wage bills paid at the consecutive stages of production properly discounted forward at the current rate of profits  $r$ .<sup>25</sup> Let  $l_{-1j}$  be the amount of labour expended during the last year before the completion of one unit of commodity  $j$ ,  $l_{-2j}$  the amount expended two years before,  $l_{-3j}$  three years before, and so on. If the process has been started  $T$  years ago, and if wages are paid at the beginning of each year (*ante factum*), where  $w$  is the real wage rate in terms of some commodity, which also serves as standard of value, then we get the following reduction to dated quantities of labour for commodity  $j$ :

$$p_j = (1 + r)wl_{-1j} + (1 + r)^2wl_{-2j} + (1 + r)^3wl_{-3j} + \dots + (1 + r)^T wl_{-Tj} \quad (j = 1, 2, \dots, n) \quad (1)$$

With a given  $w$  and a standard fixed as indicated, there are  $n$  equations to determine  $r$  and the remaining  $n - 1$  prices. Hence, Ricardo’s determination of the rate of profits was perfectly sound and was not marred by an insufficient number of independent

<sup>24</sup> See the chapter on Dmitriev in this volume.

<sup>25</sup> The method of the reduction to dated quantities of labour does not imply an historical regress to times past long ago and methods of production superseded. It rather refers to a purely logical procedure that depicts the co-existence of the various stages of production using today’s methods of production side by side in an evenly built-up production complex in a temporal perspective.

equations to ascertain the unknowns, as critics like William Stanley Jevons and Walras had contended. Bortkiewicz agreed with Knut Wicksell, who had defended Ricardo against these accusations.

Bortkiewicz then extended the framework to analyse (1) the problem of a choice of technique, (2) fixed capital, and (3) scarce natural resources that are non-exhaustible (land).

### *Choice of Technique*

As regards the first problem, with several alternative ways to produce a given commodity, we get as many reduction equations as there are technical alternatives. Obviously, and flukes apart, different methods of production do not support the same rate of profits  $r$ , given the real wage rate. Bortkiewicz corroborated Ricardo's finding that in competitive conditions the method will be chosen that minimizes unit costs. If the method that does so is at the same time a method employed directly or indirectly in the production of wage goods, its adoption will entail an increase in the general rate of profits. Otherwise, in the case of "luxuries", it will only lead to a reduction in the price of the commodity in the production of which the new method is used (and in the prices of commodities in whose production the commodity enters as a means of production).

This argument anticipates already why Bortkiewicz refuted Marx's attempt at explaining a falling tendency of the rate of profits in terms of technical progress. Technical progress implies that new methods of production become available, and for a given real wage rate, they will be adopted by cost-minimizing producers if and only if they allow to reduce costs of production, which however means that the general rate of profits will either rise or stay constant. This result became later known as the "Okishio theorem".

### *Fixed Capital*

Next Bortkiewicz turned to the case of fixed capital. Ricardo had defined fixed capital in the following way: "According as capital is rapidly perishable, and requires to be frequently reproduced, or is of slow consumption, it is classed under the heads of circulating, or of fixed capital" (Ricardo, 1951–73, I: 52). However, he did not deal in detail with the particular difficulties the presence of durable instruments of production involves in the theory of value and distribution. Without much ado, the highly successful stockjobber had rather assumed that the problem can be dealt with in terms of annuities.

Bortkiewicz (1906–07, II: 27–32) credited Ricardo with having integrated fixed capital in his theory of value and distribution in a satisfactory way. He then formalized Ricardo's approach, which implicitly dealt with the case of constant efficiency of a machine. Assume that a (new) machine can be used for  $n$  years and the price of the brand-new item is given by  $p_{m0}$ . At the end of the  $t$ -th year of its employment, its book value is  $p_{m,t}$ ,  $t = 1, 2, \dots, n$ , whereas at the end of its life, the price is taken to be nil. (This means implicitly that it has neither a scrap value nor incurs disposal costs.)



The difference between the prices of the machine in two consecutive years is equal to the machine's depreciation. Since the law of one price for the commodity produced holds, this implies that the yearly charge in terms of profits and depreciation—the annuity—must be constant across the entire life of the machine. Let  $z$  be the charge, then the following  $i$  equations hold true:

$$z = r p_{m,t} + p_{m,t} - p_{m,t+1} \quad (2)$$

for  $t = 0, 1, 2, \dots, n - 1$ . ( $p_{m,n} = 0$  by assumption) Multiplying the  $i$ -th equation by  $(1 + r)^{-i}$ ,  $i = 1, 2, \dots, n$ , and adding the equations, all terms on the RHS except  $p_{m0}$  cancel out and we get:

$$p_{m0} = \frac{z}{1 + r} + \frac{z}{(1 + r)^2} + \dots + \frac{z}{(1 + r)^n} \quad (3)$$

Solving the sum of this geometric series for the annual charge on the machine  $z$  gives:

$$z = p_{m0} \frac{r(1 + r)^n}{(1 + r)^n - 1}. \quad (4)$$

The constant annuity represents that component of the price of a commodity that is due to the use of the fixed capital item, as a share of the price of the brand-new durable instrument of production employed.

Bortkiewicz observed that compared to Ricardo's treatment of fixed capital, Marx's was inferior and presupposed not only constant efficiency of the durable instruments of production, but also  $r = 0$ , i.e. a profitless economy. In this case, the labour theory of value holds as a theory of relative prices and depreciation is linear, that is, it equals period after period the  $n$ th fraction of the value of the brand-new instrument until it has been entirely written off.

### ***The So-called Transformation Problem***

Probably best known among Bortkiewicz's writings is an essay in which he sought to correct Marx's attempt to "transform" labour values in prices of production in volume III of *Capital*, posthumously edited by Friedrich Engels in 1894 (see Bortkiewicz, [1907a] 1949). Marx had approached the problem in the following way. (We assume for simplicity only circulating capital.) He had stipulated that the "law of value" holds in the aggregate and that therefore the ratio of the sum total of surplus values produced in the various sectors of the economy,  $S = \sum_i s_i$ , and the sum total of constant and variable capitals,  $K = \sum_i (c_i + v_i)$ , gives the correct value of the general rate of profits in the system,  $\rho$ , that is:

$$r = S/K = \sum_i s_i / \sum_i (c_i + v_i) \quad (i = 1, 2, \dots, n). \quad (5)$$

He then used this rate to discount forward the values of constant and variable capitals in the different sectors in order to arrive at prices of production:

$$p_i = (1 + r)(c_i + v_i) \quad (i = 1, 2, \dots, n), \quad (6)$$

where  $p_i$  designates the production price of commodity  $i$  in whose production the same rate of profits is earned in competitive conditions as in all other sectors of the economy. Marx was aware that this procedure was contradictory, because it implied that inputs were sold and bought at labour values, whereas outputs were sold and bought at production prices. However, he appears to have assumed that transforming also the constant and variable capitals in production prices would not undermine the validity of his determination of the general rate of profits in labour value terms.

The fact that in conditions of free competition and the corresponding tendency towards a uniform rate of profits commodities cannot exchange at labour values had been noticed already by readers of volume I of *Capital*. It had prompted Engels when editing volume II in his foreword to defy economists to show “in which way an equal average rate of profit can and must come about, not only without a violation of the law of value, but on the very basis of it” ([1893] 1956): 19 Interestingly, one of the economists who accepted the challenge was Wilhelm Lexis (Bortkiewicz’s supervisor in 1892 in Göttingen). However, according to Engels, none of the people participating in the “prize essay competition”, including Lexis, succeeded in solving the riddle, and so the publication of volume III of *Capital*, which was announced as containing the solution, was eagerly awaited. Alas, the book did not keep the promise. While several people criticized the procedure Marx had suggested, very few were able to grasp its shortcoming, let alone suggest an algorithm that would solve the transformation problem for good, provided a solution existed. Probably, the most effective criticism of Marx came from Eugen von Böhm-Bawerk (1896), who, however, failed to see the slip in Marx’s argument and contented himself with pointing out a “fundamental contradiction” between values and prices in Marx.

In 1907, Bortkiewicz published a paper in which he achieved two aims simultaneously: He solved the transformation problem, but in doing so, he also showed that the magnitudes in the numerator and in the denominator of the expression giving the general rate of profits were generally affected by the transformation and could not be taken to be invariant with regard to it, as Marx had assumed. The implication of this was that the general rate of profits,  $r$ , was not equal to the one ascertained in labour value terms,  $\rho$ . Bortkiewicz argued that Marx’s “successivist” procedure—determining the general rate of profits first and only after this had been accomplished determining prices of production—was inadmissible in general and had to give way to a “simultaneous” determination of both the rate of profits and prices.

Following Tugan-Baranovsky (1905), Bortkiewicz established these results within a circular framework of production with three “departments”, department 1 producing means of production, department 2 wage goods, and department 3 luxury goods consumed by capitalists. He assumed a stationary economy, that is, simple reproduction, which implied that the sum total of wages (or variable capitals) is entirely

spent on wage goods and the sum total of profits (surplus value) entirely on luxury goods. He formulated the following system of simultaneous equations:

$$(c_1x + v_1y)(1 + r) = (c_1 + c_2 + c_3)x \quad (7)$$

$$(c_2x + v_2y)(1 + r) = (v_1 + v_2 + v_3)y \quad (8)$$

$$(c_3x + v_3y)(1 + r) = (s_1 + s_2 + s_3)z \quad (9)$$

In it, the  $c_i$ 's,  $v_i$ 's and  $s_i$ 's ( $i = 1, 2, 3$ ) are known labour value magnitudes, from which Marx had started his reasoning;  $x$ ,  $y$ , and  $z$  are price coefficients that may be interpreted as value-price transformation coefficients; and  $r$  is the general rate of profits. The system has three equations and four unknowns:  $x$ ,  $y$ ,  $z$ , and  $r$ . Fixing a standard of prices by setting  $x$ ,  $y$ , or  $z$  equal to unity (for example,  $z = 1$ , as Bortkiewicz did) allows one to determine the remaining unknowns, including the general rate of profits. It turns out that except in very special technological conditions (or with particular normalizations), none of Marx's invariance postulates holds: the sum of values is not equal to the sum of prices, the sum of surplus values is not equal to the sum of profits, and the sum of capital advances in labour value terms is not equal to the sum in price terms, and, most importantly,  $r \neq \rho$ . The "law of value" turned out not to provide the solid basis upon which the edifice of political economy could safely be erected, as Marx had thought. This does not mean that Bortkiewicz sided with the critics of Marx: these, he maintained, had thrown out the valuable along with the undesirable. Marx's idea of transforming labour values in prices of production did not imply chasing a will-o'-the-wisp. It can be done, but when it was done correctly, it necessitated important revisions of Marx's doctrine. Last but not least, while Marx had assumed that the (labour) values of commodities are known magnitudes, it is clear that in a circular (as opposed to a unidirectional) framework, they can only be ascertained by also solving a system of simultaneous equations. This system reflects a very special constellation of the sharing out of the product. To emphasize this fact, Sraffa in the early 1940s coined the term "Value theory of labour" (see Sraffa Papers D3/12/44: 3, D3/12/46: 24): values are proportional to labour quantities if and only if there are no profits (setting aside the exceedingly special case of uniform input proportions across all industries of the economy).

Bortkiewicz (1906–07: 56) traced Marx's ineptitude to provide correct solutions to the problems he raised back to the "meagreness of his mathematical abilities". He praised instead Ricardo for having provided sufficiently correct answers on a number of issues with regard to which Marx failed.

### **Rent Theory**

After he had been able to study Kautsky's edition of Marx's *Theorien über den Mehrwert* (1905–10), Bortkiewicz published two papers devoted to the treatment of land in the theory of value and distribution. In both papers, the attention focuses on

the theories of rent of the German economist Karl Rodbertus, on the one hand, and Marx, on the other, and on whether these theories involved any progress with respect to Ricardo's theory (Bortkiewicz, 1910–11; 1919). In volume III of *Capital* Marx had criticized Ricardo for having missed the concept of "absolute rent", that is, rent obtained by the proprietor of "marginal" land. Absolute rent emerges, because the competitive process is said to be imperfect and thus fails to channel surplus value produced in agriculture, which is taken to exhibit a lower organic composition of capital than manufacturing, away from it in an amount necessary to bring about a uniform rate of profit. Some of the non-redistributed surplus value is said to allow the proprietors of marginal land to pocket a rent. Marx located the deeper reason for Ricardo's inability to see this in his failure to distinguish between constant and variable capital. Ricardo is therefore also accused of having missed an important element at work in the transformation (or lack thereof) of (labour) values in prices of production.

While there is a correct element in Marx's criticism in so far as Ricardo had indeed tended to neglect the existence of non-wage capital when determining the rates of profit and rent (see Gehrke, 2012), two observations are apposite. First, as Bortkiewicz stressed, Ricardo did not advocate a "law of value" in the sense of Marx. Secondly, without free competition across all sectors of the economy, the results would differ from those obtained. There is nothing surprising here, as the classical theory of differential profit and wages rates, originating with Smith and further developed by Ricardo, shows (see *Works* I, Chap. 1, sec. II). As Bortkiewicz demonstrated, Ricardo's theory of rent emerges largely unscathed (see also Gehrke, 2012).

The question was close at hand: did Marx's theory involve only regress compared with Ricardo's? Bortkiewicz was not of this opinion, but credited Marx essentially with a single important achievement only: his explanation of the "source of profits". In the third instalment of his 1906–07 essay, Marx is said to have had the illuminating idea of building a scheme in which, while commodities exchange according to labour values, there is surplus value and thus profits. In this way, Marx was able to refute both the vulgar idea that profits are the result of raising prices above their values and the proposition that profits are a payment for the "productive services" of capital. Marx was able to show conclusively that profits reflect "unpaid labour", and thus exploitation, and imply a "deduction" from the produce of labour, as Adam Smith had already argued.

#### **4 The Reception of Bortkiewicz's Contributions in Russia and Germany**

The influence of Bortkiewicz's contributions on the contemporary economic discourse in Russia was rather limited. Apart from the language barrier and the

insufficient familiarity of the older generation of Russian economists with mathematical methods, the reception of his findings was also hindered by the fact that he refrained from participating actively in the Russian debates on Marx's economic theory. In addition, the Russian advocates of marginal utility theory often preferred Menger's approach over the Walrasian one. Bortkiewicz's economic contributions were therefore recognized and appreciated only by the younger generation of mathematical economists, some of which also took up his suggestion of attempting a "synthesis" of Ricardo and Walras.

His critique of Böhm-Bawerk's theory of profit (1907b; Bortkiewicz, 1906) seems not to have been much noticed in Russia, mainly because the latter itself, unlike Böhm-Bawerk's Marx critique and the marginal utility theory of the Austrian school, was not very influential in Russia (cf. Seraphim, 1925). The few Russian followers of Böhm-Bawerk's agio theory felt no need to respond to Bortkiewicz's critique,<sup>26</sup> and its more numerous critics did not take up his criticisms either. A noteworthy exception is Nikolai Bukharin, whose critique of Böhm-Bawerk's capital theory in *Economic Theory of the Leisure Class* ([1924] 1970) was based largely on Bortkiewicz's arguments in his "Kardinalfehler" article. Bukharin indeed referred approvingly to Bortkiewicz's criticisms of Böhm-Bawerk's first, second, and third ground for the overestimation of present goods ([1924] 1970: 130, 134, 141), quoting in full the relevant passages.<sup>27</sup>

As was the case also in Germany, the orthodox Marxists in Russia responded to Bortkiewicz's articles of 1906–07 and 1907 only with a considerable delay. The first Marxist response to Bortkiewicz's contribution on the transformation problem seems to have been an article by Sholom Dvolaitzky (1922), who argued that Bortkiewicz (and Tugan-Baranovsky) had wrongly identified Marx's "production spheres" of vol. III with the "departments" of the reproduction schemes in vol. II of *Capital*. Another leading Marxist theorist of the 1920s, Isaak Illich Rubin, in his *Essays on Marx's Theory of Value* ([1928] 1973: 102), merely inserted a spurious reference to the 1906–07 "Wertrechnung" article but failed to address Bortkiewicz's argument in his chapter on "Labour value and production price", where he based his defence of Marx's erroneous transformation procedure on Hilferding's contribution (Hilferding, [1904] 1949).

The Russian revisionists (or Legal Marxists), and in particular Mikhail Tugan-Baranovsky, whom Bortkiewicz had directly attacked in the final part of the first instalment of his "Wertrechnung und Preisrechnung" article,<sup>28</sup> and also, of course,

<sup>26</sup> Böhm-Bawerk himself sought to defend his capital theory against Bortkiewicz's criticisms in one of his "excurses" (cf. Böhm-Bawerk, 1924: 251–69; and Hennings, 1997: 252).

<sup>27</sup> On the other hand, Bukharin ([1924] 1970: 163–72) made no mention of the contributions of Bortkiewicz (1906–07, 1907a) in his critique of the Tugan-Baranovsky/Struve views on the theory of value. Interestingly, Bukharin attended the lectures of Shaposhnikov in 1909–10 at the University of Moscow (Allison, 2015: 147).

<sup>28</sup> See Bortkiewicz (1906–07, I: 41–50) for a critical discussion of Tugan-Baranovsky's contribution in the first instalment of the "Wertrechnung und Preisrechnung" article. Interestingly, Max Weber, in his capacity as the journal editor, in a letter to Bortkiewicz of 24 May 1906 informed him that "the part on Tugan", for reasons of space, might "have to go into the second instalment" (MWG II/11:

in his 1907 paper,<sup>29</sup> likewise failed to make any reply to Bortkiewicz's arguments. In his two articles, Bortkiewicz (1906–07, I; 1907a) had shown that Tugan-Baranovsky (1905: 170–88) had committed several elementary errors in his treatment of the “inverse transformation problem” and attempted disproof of Marx's “law of the tendency of the rate of profits to fall”. Bortkiewicz therefore distinguished sharply between Tugan-Baranovsky's correct inferences<sup>30</sup> and his invalid conclusions derived from “erroneous proofs”.<sup>31</sup> According to Bortkiewicz, Tugan-Baranovsky had in particular failed to provide a refutation of the Ricardo-Marx explanation of profit in terms of a “withholding theory” (1906–07, I: 50).

***Bortkiewicz's advocacy of a “synthesis” of Walras and Ricardo.*** According to Allisson, “Bortkiewicz contributed to some extent to the spread of Walras' works in Russia. Although he never published any of his works on Walras (or on economics) in Russian, he may have been a bridge between Walras and Russia through his personal contacts” (2015: 58–9). His main influence appears indeed to have been through his personal contacts with some major Russian economists: A. A. Chuprov and N. N. Shaposhnikov, as well as other young Russian economists, read Walras and Pareto on Bortkiewicz's advice, and they in turn advised their students to study the works of the Lausanne school (cf. Allisson, 2015: 59). It should be noted, however, that Bortkiewicz's admiration for Walras did not extend to Pareto: In his review of

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15). Had this been the case, the part on Tugan-Baranovsky would presumably have been included in the English translation of the final two instalments of Bortkiewicz's 1906–07 article—and thus would have become more easily accessible also to English-speaking readers.

<sup>29</sup> Bortkiewicz (1907a) did not simply provide “a more general demonstration” of Marx's erroneous transformation procedure than Tugan-Baranovsky, merely introducing an algebraic formulation instead of the latter's discussion (of the “inverse transformation problem”) in terms of arithmetical examples (1905: 170–88), as has sometimes been contended in the secondary literature. This reading overlooks that a major concern of Bortkiewicz's 1907 paper is the critique and refutation of Tugan-Baranovsky's erroneous views.

<sup>30</sup> “The laws of value and surplus value ... not only do not apply to the individual production sectors, ... but do not even determine the share of the total product which goes to the capitalist class as a whole. In this regard Tugan-Baranovsky is certainly right against Marx” (1906–07, I: 47). Similarly: “One will definitely have to agree with Tugan-Baranovsky that the law of the falling rate of profit ... is nothing but a “fallacious appearance”. (1906–07, I: 48).

<sup>31</sup> Bortkiewicz (1906–07, I: 48) pointed out that “where an increasing rate of profit emerges in Tugan-Baranovsky's examples in spite of a rising proportion of constant capital this goes hand in hand with an increase in the rate of surplus value”. In the 1907 article he noted that Tugan-Baranovsky's “two numerical examples with which he tries to refute the Marxian thesis are precisely characterized by the assumption that the organic composition of capital is equal in all three departments”. But in this case, of course, “the entire operation of converting values into prices is pointless” ([1907a] 1949: 220). Moreover, in one of Tugan-Baranovsky's examples (1905: 177), the rate of surplus value and the rate of profit both increase, while in the other example (1905: 180–181), both fall. Tugan-Baranovsky concluded from the fact that in the one case a growth in the share of constant capital accompanies a fall and in the other case a rise in the rate of profit, that the general rate of profit is entirely independent of the organic composition of the social capital, and that therefore, the Marxian theory of profit is false. Bortkiewicz commented: “As though such numerical examples could in any way impair the Marxian theory of the influence of the organic composition of the total social capital on the rate of profit! According to Marx, this influence makes itself felt in the indicated way only if the rate of surplus value remains unchanged”. ([1907a] 1949: 220–221).

Pareto's *Cours d'économie politique* (1896–97), he criticized the latter's "strongly pronounced liberal and individualistic *disposition* ..., which assumes the role of a *prius* in relation to his truly scientific opinions on which he has arrived by way of strict deduction" (1898a: 1214).

By discussing his work in the context of a historical reconstruction of the development of the "Russian synthesis" Bortkiewicz's advocacy of a "synthesis" of the objective and the subjective theory of value has been associated with that of Tugan-Baranovsky: According to Allisson, it formed part of a "research programme" to which Tugan-Baranovsky (1890) "certainly gave the impetus" (2015: 155). It should be noted, however, that Bortkiewicz never endorsed the specific form of a "synthesis" proposed by Tugan-Baranovsky (1890) and was very critical of Tugan-Baranovsky's treatment of Ricardo and Marx. It seems more likely that Bortkiewicz's advocacy of a possible reconciliation between the objective and subjective theory of value in the form of the Walrasian system of equations goes back to his student days. Why could Bortkiewicz (and Walras himself) think that the Walrasian system of equations is compatible with the classical theory of normal prices and a uniform rate of return on the supply prices of capital goods? A plausible answer is suggested by Petri (2016), who noted that Bortkiewicz (1890: 84–5) in his review of Walras's *Éléments* argued that the latter's theory of capitalization is concerned with determining *endogenously* the quantities manufactured of capital goods, while *these same quantities* are taken *as given* in Walras' theory of production. Bortkiewicz thus was (erroneously) of the opinion that the *given* initial endowments with capital goods can already be supposed to be those that must be *endogenously determined* in a long-period equilibrium, and he therefore could propose embedding the classical cost of production equations in the Walrasian system of equations ("Following the example of Walras ..." (Bortkiewicz, 1906–07, III: 478)).

The idea of a compatibility between the classical cost of production theory and the Walrasian general equilibrium theory was present already in Bortkiewicz's, 1890 review article on Walras. It reappeared in the 1906–7 paper, and then again in "Objektivismus und Subjektivismus in der Werttheorie" (1921). In this paper, which is a contribution to a "Festschrift" in honour of Knut Wicksell (in a special issue of the *Ekonomisk Tidskrift*), Bortkiewicz's main concern is with demonstrating that Böhm-Bawerk "acknowledged the objective value factor,<sup>32</sup> ... notwithstanding his strictly subjectivist attitude" (1921: 15).<sup>33</sup> Bortkiewicz also distanced himself from Marshall's reconciliation of objective and subjective elements in the determination of

<sup>32</sup> As an aside, we may note that Bortkiewicz also makes the dubious claim that Marx's theory is characterized by "a purely objectivist treatment of the problem of exchange value", because it makes no reference to the motives of actors, whereas in Ricardo's value theory, through the reference to "the striving of the entrepreneurs for the largest possible profit", a "subjective factor" has been introduced (1921: 3).

<sup>33</sup> Interestingly, in a letter to Wicksell of 18 December 1921, Bortkiewicz wrote: "I believe that if I had the privilege of seeing you more often, still more points of agreement between us would emerge, compared to which our somewhat different assessments of Böhm-Bawerk's capital interest theory count for little". (Bortkiewicz archive, Uppsala) As their correspondence shows, Bortkiewicz and Wicksell held each other in high regard and were connected by a warm personal friendship.

prices, because the latter had replaced the classical notion of cost with a subjectivist one and had invariably associated changes in demand with changes in preferences (or utilities), whereas for the classical economists, it had been clear that “demand can change also for reasons that have nothing to do with utility” (1921: 20).

N. N. Shaposhnikov, who spent the period from 1906 to 1910 as a post-graduate student in Berlin and also remained an informal student of Bortkiewicz after his return to Russia, attempted to provide the integration of classical production costs into the Walrasian system in his doctoral dissertation, *Theory of Value and Distribution* (1912), which he wrote officially under the guidance of Tugan-Baranovsky at St. Petersburg University (but with whom he had a rather difficult relationship). With regard to the classical part of the “synthesis”, he basically adopted the formulation of Dmitriev and Bortkiewicz of the production price equations, without adding any substantial new elements.<sup>34</sup> Bortkiewicz also corresponded with E. E. Slutsky, whose dissertation, *The theory of marginal utility* (1910), also contained an exposition of the theory of production costs using the same system of equations as Dmitriev for the determination of the direct and indirect labour contents.<sup>35</sup> It was thus mainly through his personal contacts and extensive correspondence with young Russian mathematical economists that Bortkiewicz contributed to the transfer of “Western” economic ideas to Russia.

It is surprising that Vladimir K. Dmitriev never commented on Bortkiewicz’s 1906–07 contributions in his later writings and in fact showed little interest in the debates over Marx’s theory of value and distribution more generally. Presumably he was aware at least of Bortkiewicz’s “Wertrechnung” article, because Shaposhnikov (1914: 8n) reported to have discussed with him the theory of value and distribution. Unlike Bortkiewicz, Dmitriev (1908) considered Böhm-Bawerk’s critique of Marx’s theoretical construction as well-taken.

After WWI and the Russian revolution, Bortkiewicz again attracted a number of students from Soviet Russia, among them Wassily Leontief and Jacob Marschak. Marschak emigrated to Berlin in 1919 and studied with Bortkiewicz for six months, before he moved on to work with Emil Lederer in Heidelberg. Leontief studied in Berlin from 1925 to 1927, and Bortkiewicz became one of the examiners, together with Werner Sombart, of his doctoral dissertation, entitled “Die Wirtschaft als Kreislauf” (1928). In the letter to the Dean accompanying his report on Leontief’s PhD thesis, Bortkiewicz wrote:

Although I find much that is objectionable in it, this dissertation is without any doubt acceptable. In developing his – in my opinion very doubtful – theoretical constructs the candidate received no guidance whatsoever from his academic teachers. He arrived at his present position quite independently, one might say, despite them. It is very likely that he will maintain this scientific point of view also in the future. (quoted from Hagemann, 2021: 77)

<sup>34</sup> On Shaposhnikov’s version of the “synthesis”, see Allisson (2015: 148–56).

<sup>35</sup> It is contained in Chap. 3 of part V, entitled “The theory of costs of production”. For a summary account of the contents of Slutsky’s dissertation, see Barnett (2011: 207–215) and Allisson (2015: 141–3). Slutsky explained in a footnote that he had “arrived independently at the same equation derived earlier by Dmitriev, without knowledge of it” (Allisson, 2015: 142).



From 1926 to 1929, a group of mathematicians and mathematical economists around Bortkiewicz at the University of Berlin discussed problems of economic theory and developed models with distinctively classical features. Among the members of this group were the mathematicians Robert Remak, who published two papers on economics (Remak, 1929, 1933) and John von Neumann ([1937] 1945).<sup>36</sup>

### **The Reception and Impact of Bortkiewicz's Papers on Ricardo and Marx in Germany**

Bortkiewicz's articles on Ricardo and Marx were clearly of great importance for "the diffusion and integration of the work [of Tugan-Baranovsky and Dmitriev] into the theoretical debates of the Western literature" (Allisson, 2015: 17)—albeit with a considerable time-lag. Bortkiewicz's two papers were in fact appreciated for a long time only by a handful of specialists, and none of them seems to have followed up his reference to Dmitriev's *Economic Essays*. The older generation of German-Austrian Marxists, including Kautsky, Hilferding and Bauer, attacked Tugan-Baranovsky's "revisionist" ideas but chose to ignore Bortkiewicz's contributions on the transformation problem. The first (and for a long time the only) author to comment on Bortkiewicz's simultaneous determination of production prices and the general rate of profit was the mathematician Georg von Charasoff (1909, 1910), who indeed appears to have arrived at similar findings, independently of Bortkiewicz, at about the same time.<sup>37</sup> Bortkiewicz's contributions were then discussed again only in the 1920s and 1930s, first by Kühne (1922), and then by Moszkowska (1929), Grossmann ([1930] 2017, [1932a] 2017), Peter (1933, 1934), Klimpt (1936), and Walter (1936). Kühne (1922) reformulated Bortkiewicz's model in order to rectify a perceived confusion between physical quantities and labour time units, but his argument was seriously flawed, as Bortkiewicz showed in his review (1924) of Kühne's treatise and his rejoinder (1925a) to the latter's response (1925). More interesting was the contribution of Natalie Moszkowska (1929), who accepted Bortkiewicz's findings but proposed to close the model by equalizing the sums of values and prices. As Moszkowska demonstrated, in this case, the equality between aggregate surplus value and aggregate profit (and between the rate of surplus value and the ratio of profits to wages) does not hold. She nevertheless maintained, much like Bortkiewicz, that the source of profit is to be found in surplus labour, and that Tugan-Baranovsky had been wrong to deny this. Henryk Grossmann, who is best known for his work on the breakdown theory (1929), attempted to refute Bortkiewicz's findings in two long manuscripts ([1930] 2017, [1932a] 2017), which however he decided to leave unpublished during his lifetime.<sup>38</sup> At the time, Grossmann only published a related paper

<sup>36</sup> On the contributions of Remak and von Neumann, see Kurz and Salvadori (1995: 397–407).

<sup>37</sup> See Gehrke (2015) and the chapter on Charasoff in this volume.

<sup>38</sup> Grossmann was Carl Grünberg's assistant at the *Institut für Sozialforschung* in Frankfurt. His first manuscript is entitled "Zum Abschluss des Streites um die Wert- und Preisrechnung im Marx'schen System (Eine Berichtigung des grundlegenden Fehlers bei Bortkiewicz, Rosa Luxemburg und Otto Bauer)" ["On the Close of the Dispute over Value and Price in the Marxian System (A Correction of the Fundamental Error in Bortkiewicz, Rosa Luxemburg and Otto Bauer)"]. It contains *inter*

on Marx's crisis theory ([1932b] 2018), in which he criticized Bauer and Luxemburg for having presented their crisis explanations in value rather than price terms but refrained from entering into a discussion of Bortkiewicz's correction of Marx's erroneous transformation procedure.

Hans Peter (1933, 1934) elaborated on Bortkiewicz's findings in the Ricardo-Marx articles in his habilitation thesis,<sup>39</sup> where he attempted a reconciliation of the objective and the subjective theories of value, arguing that the two seemingly incompatible price theories can be merged so as to form a coherent whole. In order to achieve this reconciliation, Peter (1933: 138, 155–6) proposed to substitute Bortkiewicz's price equations in terms of dated quantities of labour, to which he referred as the "Dmitriev-Bortkiewicz system of equations" (1933: 162), into the equations system in Lecture 20 in Walras's *Elements*, thus replacing Walras's equations expressing equality between the selling prices of the various products and their respective costs of production (cf. Walras, 1954: 240). According to Peter (1933: 64–5), Walras's original system of production equations in Lecture 20 merely allows to determine equilibrium prices and quantities for an economic system without profits ("auterge Wirtschaft"). By replacing Walras's with Bortkiewicz's price equations, an additional variable, the general rate of profits, is introduced into the equations system, which can now be interpreted as depicting a capitalist economic system ("allerge Wirtschaft"). Peter argued that the rate of profit can be treated either as an exogenous variable, or else it can be determined endogenously by treating the real wage rate as given (1933: 155, 157). However, according to Peter, by supplementing the set of data from which the Walrasian production model starts out, that is, endowments, preferences, and technologies (1933: 82), with a given real wage rate (1933: 157), the postulate of universal free competition has been suspended with regard to the labour market. Hans Peter appears to have been the only German author who took up Bortkiewicz's suggestion of attempting a synthesis of the classical theory of production prices with the Walrasian general equilibrium theory. However, Peter's work went almost completely unnoticed.

The same applies also to Werner Klimpt's doctoral dissertation, entitled (in German) "Mathematical investigations on reproduction and the rate of profit following L. von Bortkiewicz" (1936). Klimpt sought to elaborate on Bortkiewicz's solution to the transformation problem by deriving necessary and sufficient conditions for the value scheme under which the two Marxian invariance postulates hold

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*alia* a critical discussion of Bortkiewicz's solution to the transformation problem ([1930] 2017: 74–90). Grossmann claimed that Bortkiewicz's transformation procedure is erroneous for two reasons: First, because he wrongly identified Marx's "production spheres" of vol. III of *Capital* with the "departments" of the reproduction schemes in vol. II; and secondly, because the adopted numéraire is not an "invariable measure of value" produced with the average organic composition of capital ([1930] 2017: 84). The same two criticisms Grossmann put forward also in the second paper ([1932a] 2017: 115–9).

<sup>39</sup> Peter's habilitation thesis was submitted to the University of Tübingen in 1932, and subsequently published, in three parts, as *Grundprobleme der theoretischen Nationalökonomie* (1933, 1934, 1937). Interestingly, Peter had been a student at the University of Berlin in 1918, but at this time was mainly interested in mathematics and philosophy, so that he probably did not attend Bortkiewicz's lectures (Haller, 1960: 156).

simultaneously. He also maintained that Marx had endorsed the idea of a simultaneous determination of prices and the rate of profit. Finally, the Swiss sociologist Emil J. Walter sought to defend Marx against Bortkiewicz (1906–07; 1907a) and Moszkowska (1929), claiming that a transformation of values into prices respecting the two invariance conditions is possible. However, his demonstration required *different* price value transformation coefficients for the *same* commodities when used as inputs in different departments. It also led to a discrepancy between the rates of surplus value in the value scheme and the ratio of profits to wages in the price scheme, which he sought to rationalize by suggesting that the transformation process is associated with a displacement of workers and wage reductions. On the other hand, Walter (1936: 389–90) noticed that Bortkiewicz’s argument in the 1906–07 paper, unlike Marx’s, was based on an “Austrian” production model. He failed to notice, however, that this implied the non-existence of a maximum rate of profits.

As is well-known, after WWII, the summary account of Bortkiewicz’s contributions provided by Sweezy (1942: 115–125) and his inclusion of an English translation of Bortkiewicz’s, 1907 paper in his edition of Böhm-Bawerk’s Marx critique (1949) then gave rise to a rich literature on the “transformation problem” in the English-speaking world.<sup>40</sup> The publication of Piero Sraffa’s *Production of Commodities by Means of Commodities* (1960) increased even further the international interest in Bortkiewicz’s contributions and also led to the long overdue recognition of Dmitriev’s *Economic Essays* in the West.<sup>41</sup>

### ***Sraffa on von Bortkiewicz***

It is interesting to note that among Piero Sraffa’s unpublished papers there is a notebook of 1942–43 with extensive excerpts from, and critical comments on, three contributions of Bortkiewicz: his criticism of Böhm-Bawerk’s theory of capital and interest (1907b; Bortkiewicz, 1906) and his essay on “Value and price in the Marxian system” (Bortkiewicz, 1906–07). Sraffa in his comments on Bortkiewicz’s essay on the “cardinal error” in Böhm-Bawerk’s theory of interest approved of Bortkiewicz’s specification of the task of interest theory—a task Sraffa had in fact established independently of him and accomplished (with regard to single production) with his “second equations” relating to an economy with a surplus and given real (that is, subsistence) wages elaborated towards the end of 1927.

Sraffa held Bortkiewicz in high esteem because of his “dictum” concerning the criteria that the theory of value and distribution ought to satisfy, but he also accused him of having put forward misleading interpretations of Ricardo and Marx and of inconsistencies in the 1906–07 article. Sraffa’s main criticism concerned the fact that Bortkiewicz, who had considered Marx’s analysis as a “regression” from the state of the classical theory of value and distribution achieved by Ricardo, had not seen that Marx had indeed contributed a major analytical insight. He had shown that

<sup>40</sup> For summary accounts of these contributions, see Desai (1988) and Howard and King (1992, part IV). Later, also French, Spanish, and Italian translations of Bortkiewicz’s contributions were published.

<sup>41</sup> On the reception of Dmitriev’s work see Gehrke and Kurz (2022).

with circular production relations, which Ricardo had for simplicity set aside in his analysis of the wage-profit relationship, the rate of profits is always bounded from above (that is, there is a finite maximum rate of profits)—with important implications for the theory of distribution, capital accumulation, and technical change. As regards Bortkiewicz’s treatment of the “transformation problem”, Sraffa objected that while Bortkiewicz had assumed different organic compositions of capital between the three departments under consideration, he had implicitly assumed the same organic compositions of all industries *within* each department. (For an in-depth discussion of Sraffa’s comments on Bortkiewicz, see Gehrke & Kurz, 2006).

## 5 Concluding Remarks

The present essay has concentrated on the transfer of ideas between Russia and the West with regard to Bortkiewicz’s contributions to economic theory only. It needs to be stressed, therefore, that the situation was quite different in the field of statistics. In the late nineteenth and early twentieth century, Russian statisticians were not only “world class”: they were world leaders and pioneers. The theoretical statisticians of the Russian Empire dominated certain statistical fields (such as, e.g., probability theory) and many of them were well integrated into, and highly respected by, the international scientific community (Wheatcroft, 2020: 66). In economics, things were different. Here, Bortkiewicz played an important role as a mediator between Russia and the West that helped the Russian profession to get closer to the frontiers of knowledge and allow some of its brightest representatives to contribute in important ways to the further development of the subject.

As we have shown, Bortkiewicz brought Dmitriev’s findings on the classical theory of value and distribution to the attention of the German-speaking readers with his essays of 1906–07 and 1907, in which he further elaborated on the contribution of the “first Russian mathematical economist” and spelled out some of the implications for the assessment of Marx’s contribution to the development of the classical surplus approach to the theory of value and distribution. On the other hand, Bortkiewicz parted company with Dmitriev in the latter’s rejection of Marx’s explanation of profits in terms of the generation of surplus value exclusively by human labour. Bortkiewicz also contributed to making Tugan-Baranovsky’s analysis of the “inverse transformation problem” better known in the West, although he was very critical of large parts of Tugan-Baranovsky’s “ultra-revisionist” economic ideas. In Russia, Bortkiewicz seems to have had only a rather limited influence on the contemporary economic discourse with his critique of Böhm-Bawerk’s *agio* theory and defence of the classical “deduction theory” but contributed significantly to the spread of Walrasian general equilibrium theory among the younger generation of mathematical economists.

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# Peter B. Struve as Economist: Philosophical Foundations of Economics and Development Theory



Günther Chaloupek and Nikolay Nenovsky

## 1 Peter Struve as Intellectual Figure

No single designation appears satisfactory to characterise the personality of Peter Berggardovich Struve, born 1870 in Perm, died 1944 in Paris: political publicist, journalist, economist, social philosopher and politician—these roles were of particular relevance in certain phases of his life. The philosopher S. Frank—a disciple and close friend of Struve—said of Struve that he belonged to those people who are “once born” (Frank 1956, 209). But as a scholar of the social sciences and especially economics, Struve is almost forgotten today.

Struve is mentioned in histories of pre-revolutionary Russia. In the former communist countries, Struve enjoyed considerable prominence as a target of Lenin’s critique. In an *Ökonomenlexikon*, which appeared in the last year of the German Democratic Republic, there is entry on Struve, “Russian bourgeois politician, economist and philosopher, main representative of ‘legal Marxism’.” Struve gets credit for having refuted the reactionary critique of capitalism of the “populists” (*narodniks*) and also for providing the opportunity for Lenin to publish in a periodical of the legal Marxists. Later, however, Lenin exposed Struve’s “false methodical approach in his characterisation of the populists and in his studies on the development of capitalism in Russia” based on “reactionary bourgeois and vulgar-economic ideas” (Krause et al. 1989, 558f).

In the West, especially in Germany, Struve had acquired a reputation as a scholar of revisionist socialism before World War I, as a social philosopher, thereafter as

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a representative of economic theory in Russia. Struve's early publications<sup>1</sup> from 1892 to 1894 were predominantly in German (11 out of 19), most of them reported on issues of social policy in Russia, which appeared in the bi-weekly periodical *Socialpolitisches Centralblatt*, edited by Heinrich Braun in Berlin.<sup>2</sup> In 1896, in his Social-democratic period, Struve wrote several articles on Marxist topics for the *Neue Zeit*—the most prestigious journal of European socialism at that time, edited by Karl Kautsky. With his reputation as a Marxist scholar, in 1899, Struve was invited by the famous *Archiv für soziale Gesetzgebung und Statistik*<sup>3</sup> to write a review of Eduard Bernstein's recently published book *Die Voraussetzungen des Sozialismus und die Aufgaben der Sozialdemokratie*, and he presented his thoughts on capitalism and socialism in more general terms in another essay in the same journal. Struve's ambition to establish and maintain his presence as social scientist beyond his home country finds its expression in his essay on the philosophical foundations of economics in the newly founded journal *LOGOS* 1910/11.

In the period between the two World Wars, when Struve lived in exile, several publications refer to him as one of the main representatives of Russian economic theory. At the occasion of a Festschrift with contributions by Struve's colleagues and pupils, published 1925 in Prague, an article in the Slavonic review confirmed that "his academic standing in the first rank of Russian economists is more than assured" (Williams, 1925, 19). In the contribution about Russia to the survey *Die Wirtschaftstheorie der Gegenwart*, edited by Hans Mayer, Struve was called "the most important of the theoreticians who originated from the Marxist movement of the 1890s" (Gelesnoff, 1927, 165). Struve's writings (in Russian) on the theory of value and price, interest and income distribution are extensively covered in monographs on Russian theory of value and interest by Seraphim (1925) and Ischboldin (1971).

During his years in exile (1920–1944), Struve continued to publish articles on a large variety of topics in Russian, English, French and German (as well as a small part in Serbian and Bulgarian). His most substantial publications during this period on economic issues were still in German, which became his preferred language for scholarly articles. The majority of them dealt with methodological problems and appeared in the highly prestigious *Zeitschrift für Nationalökonomie*, the periodical of the Austrian economists' association, edited by Hans Mayer and Oskar Morgenstern.<sup>4</sup>

After World War II, as economist Struve was no longer remembered in the West (except in Ischboldin's book, 1971, 241–258).<sup>5</sup> The reasons for this are several. Economic theory in its proper meaning became increasingly identified with abstract tools of analysis. A large part of the literature, especially among scholars writing in

<sup>1</sup> A complete list of Struve's publications (excluding newspaper articles), whose number reaches almost 1.000, is provided by Pipes (1970, 1980).

<sup>2</sup> Struve communicated with most leading European scholars, see, for example, meetings with Simmel (Frank, 1956, 60).

<sup>3</sup> Later the name of the journal was changed into "*Archiv fuer Sozialwissenschaft und Sozialpolitik*".

<sup>4</sup> Morgenstern and Struve were invited and read lectures at the Statistical Institute in Sofia (in 1935 and 1936, respectively).

<sup>5</sup> Jossa (2020) appeared too late to be covered in the present contribution.

German, deals with issues of the theory of value in relation to price, which has become irrelevant to such an extent that today it is not easy to follow the arguments. Until the middle of the last century, there existed distinct national traditions of economic discourse, not least with respect to the relevance of “value” for price formation, but also due to the understanding of what constitutes the essence of economic “theory”. It was in this context that Struve had a certain reputation as representative of Russian economic thought. Struve’s most original contributions with a focus on institutions and evolution of the economy have thus fallen out of the scope of economic theory. Also, the interest of economists in a discussion about epistemological and methodological foundations of economic theory diminished.

Richard Pipes’ massive two-volume monograph on Struve is mainly concerned with Struve as a political figure. Doubtless an impressive achievement, its impact appears to have been confined to the circle of “sovietologists”. Only recently has interest in Struve’s work, specifically in economics, been revived thanks to the publications of Nenovsky and Penchev (2017, 2019), which focus on Struve’s writings in Russian and the contributions of Struve’s disciples in Bulgaria. Struve always wanted to address scholars without knowledge of Russian, so many of his most important articles are in German (or in both languages). It was essentially through those German (and French) publications that Struve was a respected participant in discourses among European economists before and after World War I. This paper makes an attempt at assessing Struve’s contributions to twentieth-century economics in the light of these publications and the achievements of his students.

Research on Struve’s economic views and ideas provides an opportunity not only to illustrate the thesis of the present as stated by V. Avtonomov:

The pattern suggests that experiencing strong influence from the West, leading Russian economists developed and modified Western economic theories, adapting them to specific Russian political, ideological and cultural circumstances. As a result, they exerted a certain influence over the next generations of Western economists. Among these circumstances the paper mentions moral and religious factors, the peasant question, the special influence of Marxism, the development of mathematics and statistics in Russia in the 1890s-1920s, and the unique experience of building a planned economy” (Avtonomov, 2021, 1),

but also, to enrich the above-mentioned transfer of ideas with a new, rarely analysed transfer channel, the line “West—Russia—European East/CEE” (which we can provisionally call “European periphery”). The European periphery is the intersection of Western and Russian influence, and the latter has been extremely strong in the past, especially among Slavic and agrarian peoples. The trajectory of Struve’s ideas illustrates the second point of Avtonomov’s thesis—the role of the agrarian question, the specificity of Russian Marxism, the role of statistics and empirical research and the moral factors. To these, we can add the natural and spatial factors, as well as the specific role of power and the state in the evolution of institutions in Russia.

## 2 Peter Struve and His Formation as Economist

No single description can characterise Struve's professional orientation. After graduating from the Gymnasium, he began to study zoology at the faculty of natural sciences of the St. Petersburg's University. But after a year, he lost interest in the subject of his first choice (Pipes, 1970, 65ff). Even before, his main interest had been in the social sciences and in philosophy, basically driven by political motives. It was reinforced by the experience of his stay in Germany and Switzerland during the summer of 1890, from where he brought with him a large collection of Social-democratic literature. Upon his return to the university, Struve became head and organiser of a students' circle whose purpose was the study of the writings of Karl Marx and other authors of "scientific socialism". In 1892, Struve went abroad again and enrolled as extraordinary student at the University of Graz, Austria, where he took courses in sociology (Ludwig Gumplowicz) and political economy (Richard Hildebrand), but soon found out that his expectations were not met. Nonetheless, it was in Graz where Struve "decided to become a professional economist" (ibidem, p. 78). However, this decision took many years to materialise.

Drawn into politics from an early age, Struve became what today is called a political publicist, at that time the only form of political activity in the authoritarian Russian monarchy, although severely impeded by censorship and exposed to risk of becoming a victim of the repression to which all opposition was subjected. As an economist (also as a philosopher), Struve originally was a self-taught ("autodidact"), who first learned his economics from *Das Kapital* and other writings of socialist literature. His economics developed in the context of Marxian social philosophy and German Neo-Kantian philosophy. He "turned to professional economics late in his life" at the age of 36 (Pipes, 1980, 119f), assuming his first teaching post in economics without any formal degree. The first volume of his book "Economy and Price" was published in 1913, for which he was conferred a master's degree in economics from the University of Moscow. Upon the completion of the second part of "Economy and Price" in 1917, Struve finally received his doctor's degree from the University of Kiev in May 1917 (under the tutorship of A. Bilimovic), two months after the first revolution (Pipes, 1980, 123).

Election to membership in the Academy of Sciences in May 1917 was an official recognition of Struve as scholarly economist, but it did not mark the beginning of his academic career. During World War I, Struve was in charge of the Special Council on Food Provision. Several of his disciples worked under Struve's leadership (e.g. S. Demostenov and N. Dolynski) to regulate the prices of consumer goods, which led to a number of publications, including a book in English (edited by P. Struve), "*Food Supply in Russia during the World War*", issued by Yale University Press. Having supported the republican constitutionalist government of Alexander Kerenski, Struve joined the anti-Bolshevist forces after the October revolution. After their defeat, he ultimately left Russia in 1920. Academic posts now provided the main basis of existence for Struve and his family, while he also continued his activities as political journalist in a variety of periodicals of the emigrant press.

In 1922, Struve was appointed professor at the newly founded “Russian Juridical Faculty” in Prague, which, however, had to close when the government withdrew its support. In 1928, he found a new post at the “Russian Institute” in Belgrade, where he pursued his studies in Russian economic and cultural history and in economic thought (Pipes, 1980, 395ff). An appointment to a chair at the Juridical Faculty of the University of Belgrade in 1934 ultimately did not materialise, when his inaugural lecture had been turned into a scandal by hostile interventions from parts of the audience.<sup>6</sup> The government backed down, but compensated Struve with lectures at the Faculty of Law in the Serbian city of Subotica. It was mostly during his years in Belgrade when Struve wrote and published his essays on methodological and epistemological problems of economics, most of them in German. He undertook several trips for research and lectures. In 1939, Struve received an Honorary LLD from the University of Sofia. When the German army occupied Yugoslavia in 1941, Struve was arrested and detained in Graz. Released after three months, he and his wife took their last residence in Paris, where their sons had moved before. Living in pitiable circumstances, Struve continued work on his long-term project of a history of Russia. He died in 1944.

### 3 Marxism and the Theory of Economic Development

If, in the last decades of the nineteenth century, Marx’s writings were intensely discussed in Germany’s non-socialist political and academic circles, they enjoyed even greater acceptance in Russia than anywhere else in the world. “Russians learned to view economic phenomena through Marxist categories” (Pipes, 1970, 46). Marx’s popularity was mainly due to his theory of long-term economic development, according to which capitalism was a necessary intermediate stage in the evolution from feudalism to socialism. It served as conceptual framework for the opposite views of the *Narodniks*, of the liberal proponents of bourgeois capitalism, and of revolutionary Social Democrats.

It was in this intellectual surrounding, where Struve also embraced Marxism in his search of a model for development of Russia’s backward economy, because it provided a plausible basis for a perspective of the country’s future path towards capitalism, and in parallel, towards constitutional democracy. Struve decidedly opposed the *Narodniks’* perspective of a Russian *Sonderweg* (special path) to some kind of socialism, thereby circumventing the stage of capitalism.

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<sup>6</sup> In the early 1920s, Struve had several proposals from the University of Sofia, but the appointment was not realised. However, a number of his students (N. Dolynski, S. Demostenov, O. Anderson, and F. Belmer) held academic positions in Bulgaria on his recommendation and recommendation from A. Chuprov.

One of Struve's first publications (in German) was a review of a book by N.F. Danielson<sup>7</sup> which was a forceful plea for such a Russian special path (Struve, 1893, English translation in Pipes, 1970, p. 90f). Struve argued that it was obvious that it was impossible to "reconcile somehow the utopian faith in a 'unique' economic development of Russia with the insight of Marx and Engels". His wholehearted support for capitalist industrialisation earned Struve the critique of eulogising the system of capitalism. Support for Struve came from Lenin, and even from Engels, who rejected the idea of bypassing capitalism on the path towards socialism (Pipes, 1970, p. 93ff). In 1894, at the age of 24, Struve published his first book, which quickly became popular, in which he systematically analysed and criticised the views of the *Narodniks*.

Struve's investigation in the development of "scientific", i.e. Marxian socialism (Struve, 1896–97), published in the leading theoretical periodical of German Social-Democrats *Die Neue Zeit*, edited by Karl Kautsky, is an exercise in the history of socialist theory. Citing extensively from the writings of early German socialists (Moses Hess, Karl Grün), Lorenz von Stein and Marx and Engels, Struve demonstrates how, during the 1840s, the essentials of scientific socialism developed in a process through which Marx and Engels emancipated themselves from the ideas of German philosophical socialism, to which they had originally adhered. The article contained an important political message for Russia, where current debates appeared to him as an example of "the overestimation of the ideology of the intellectual class" (p. 77) as a recurrent phenomenon typical for countries lagging behind in their economic development, which had to be firmly denounced (p. 81).

Despite such pleas for the "orthodox" Marxian view of economic development, Struve never accepted Marxism in its entirety. For philosophical reasons, he had always rejected the labour theory of value (see the next section). If he had "professed the most loyal adherence to Marxism" before 1899, at the same time he had entertained serious doubts about certain parts, which he felt the duty to keep to himself" in the interest of the social-democratic movement (Pipes, 1970, 221). In two articles in the *Archiv für soziale Gesetzgebung und Statistik* Struve (1899a, 1899b) elaborated, where his views on the theory of economic development differed from those of Marx, Engels and Karl Kautsky.

The first article (1899a) is a discussion of the main concepts of Marx's theory of "modern society": the developments of productive forces under capitalism (concentration of capital, "anarchy of competition"); increasing immiseration (*Verelendung*) and elimination of the middle classes; emergence of the revolutionary proletariat (p. 660). The process of development is characterised by a tendency of increasing contradictions between productive forces and forms of social institutions in the economy, between bourgeoisie and proletariat in society. Struve argues that this tendency must be subjected to empirical investigation (p. 664f). Struve agrees to the main proposition of historical materialism that changes of legal forms follow changes

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<sup>7</sup> Nikolai F. Danielson (1844–1918), alias Nikolai-on, Russian economist, publisher and translator of Marx's *Capital*. The other populist criticised by Struve was Vasily Vorontsov (1847–1918), under the pseudonym V.V.

in productive forces; however, the consequence is not a “movement towards blasting (*Sprengung*) of the social order, but there is a movement of adaptations of legal forms to socio-economic phenomena”. The concept of social revolution “is misleading”, a transformation of the social order is conceivable only as a continuous long-term process (p. 673).

Struve rejects the “laws of dialectics”, of which the theory of collapse of capitalism due to the exacerbation of its internal contradictions is an application, as an ontology which he considers seriously mistaken epistemologically (p. 683). Struve argued that neither of the two main tendencies, which in Marx’s view would make such a collapse inevitable, i.e. progressive immiseration of workers and increasing impossibility of realisation of the rising surplus value, have materialised in reality. For Struve, it appears as serious inconsistency to expect that a pauperised and culturally degraded working class would be able to take over the role of reorganising society according to the ideals of socialism (p. 662). If pauperisation had been a reality before 1848, in Struve’s view, the successful struggle of workers for improvement of their living conditions demonstrated “real gains of economic and political power of the working class within the capitalist system” (p. 690f).

Struve doubts that “development towards socialism can be realistically conceived in detail”, because he thinks that “there are narrow limits for a scientific theory for the development of socialism”. “Socialism is realistic only to the extent that emerges from changes originating from the presently existing economy” (p. 698).

For Struve, the theory of final breakdown of capitalism is a “rationalist superstition”, which was nonetheless instrumental to the self-confidence of the working class and the formation of Social democracy as a political movement. This explains the violent reactions against Eduard Bernstein’s book *Voraussetzungen des Sozialismus und die Aufgaben der Sozialdemokratie*, which Struve considers “a notable symptom of reconstruction of social-democratic ideology” (p. 701). To keep socialism as a goal of the social-democratic movement, it must be conceived of as a “practical-political” ideal (p. 699), whereas “orthodox pseudo-science” has to be abandoned. As a political movement, socialism combines science and utopia. The utopian element is necessary because the future of society cannot be understood as pre-determined, but as fundamentally uncertain. “Anybody, who feels as socialist, will value its utopian and revolutionary elements as dearly as the realistic ones. Only that kind of utopianism is untruthful which pretends to be science” (p. 703f).

The second article is a 16-page review of Bernstein’s above-mentioned book and of Karl Kautsky’s rebuttal *Bernstein und das sozialdemokratische Programm*. As regards the fundamentals, also in most details, Struve sides with Bernstein, but, as it appears to be an omnipresent habit of his mind, Bernstein also gets his due share of rebuke. Bernstein is criticised for his eclecticism.

If Kautsky, against Bernstein, tries to salvage the immiseration theory by a new interpretation, which substitutes “relative immiseration”, i.e. a declining wage share, for increasing absolute poverty, Struve holds against Kautsky that he has not offered any empirical proof for his contention (Struve, 1899b, 732). Against Kautsky’s concentration on the conquest of the state as precondition for the final showdown of capitalism, Struve pleads for continuous action of the working class in the economic



as well as in the political sphere. Bernstein rightly emphasises the importance of economic organisations of workers, such as co-operatives, as piecemeal steps towards socialism, and rightly discards the “idea of establishing a socialist economic order” by political act in one stroke. “Social democracy sets itself the goal of ‘social revolution’ and fights for this goal through democratic-socialist reforms” (p. 736). It should not be viewed as disgrace if Bernstein, in his critique of Marxist orthodoxy, has borrowed from “bourgeois” science. Rather, it should help “that the phrase of ‘bourgeois science’ loses its grip on minds” (p. 738).

#### 4 The Impact of the Dispute with the Narodnik’s on Struve’s Economic Thinking

There is no doubt that early disputes with the Narodniks over the economic development of Russia (set out most fully in his 1894 book), and the specifics of Russian collectivism and Marxism, led Struve gradually to rethink Marxism in general, but also to realise his ambitions to construct a specific theoretical system. Struve’s interpretation of economic, and in particular Russian, development is most fully set out in his posthumously published book on the economic history of Russia (Struve, 1952).

First, as a critical reaction to the Narodniks, Struve rejects the claims for the existence of a specifically Russian traditional society (*commune/mir*) based on equality and the absence of social differentiation. He emphasises the crucial importance of capital for the efficient use of labour even at the earliest periods of Russian history. Above that, he denies that part of Marx’s stage theory according to which by necessity socialism follows capitalism. In his first book (1894), Struve’s Marxist interpretations of the development of capitalism in Russia were based largely on the work of N. Sieber and F. Engels. Struve does not accept the “concessions” that Marx makes to Russian *Narodniks* (see Glovelli, 2014; Avtonomov, 2021).

As a second step, this led Struve to a radical reformulation of the problem of distribution and its justice, which had been a central concern of the Narodniks. For Struve, the only correct approach to distribution could be the scientific, positivist approach (not the moral, ethical or dogmatic one). Specifically, this can be done through empirical and statistical analysis. This analysis, in turn, can only be done on the basis of concrete monetary prices and accounting. Logically, Struve is led to a complete rejection of the existence of value and any substance of it (for instance, labour in Marx, etc.). As will be shown in the following paragraphs, Struve places exchange and the market at the centre of every economic problem.<sup>8</sup> Undoubtedly, communication with his colleague, the great Russian statistician A. Chuprov (1874–1926), also contributed to the construction of the statistically based theory.

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<sup>8</sup> If Struve rejects Marxism in general, there are also moments which he interprets positively, in common with some of Rubin’s positions, e.g. on the role of money, on commodity fetishism, money, etc. (see Nenovsky, 2019).

The third point that we think is relevant is that of the principles of dualism. The latter can be seen as a reaction to Russian economic history, in which the state and power occupy an important and special place (e.g. the agrarian reform of 1861 initiated by the Tsar). According to Struve, both the overall economic and social history and each individual institution, whether economic or not, can be interpreted as the struggle and interaction of two elements—the spontaneous (heterogeneous) and the central-volitional (autogenetic). The former gives rise to the economic form “system/coordination”, the latter to the economic form “unity/subordination”. From these positions, Struve, and subsequently his students and followers analysed Russian history and different institutions (such as money, law, state, language, financial system, etc.). Struve’s introduction of dualism as a basic category in many ways anticipates trends in economic science, such as institutional and evolutionary theory, French monetary institutionalism, and also a number of basic postulates of Ordoliberalism (the state as a unity of order and freedom) and of systems theory and economic cybernetics (the economic system as coordination and subordination, as an information system, etc.).

The controversy with the populists and the rejection of Marxism quickly led Struve to the great question of the philosophical foundations of economic thinking, and hence of economic theories.

## 5 Struve’s Fundamental Concept of Economic Reasoning

In an essay published in LOGOS, Struve (1910/11) and later in the book Struve (1913) gives an exposition of what he considers as *grundlegende Momente im nationalökonomischen Denken*—basic elements of economic thinking.<sup>9</sup> He starts from the observation that in the second half of the nineteenth century, theoretical economics bears the imprint of socialism, directly or even more often, indirectly. To him, this appears paradoxical since in reality a liberal economic system prevails. Struve’s discussion of the questions involved focuses on three basic dichotomies: universalism/singularism, realism/nominalism, and rational/irrational.

In the universalist view, society and the socio-economic process are conceived of as a totality, whereas singularism has the individual as underlying perspective. Socialism is fundamentally universalist, based on the a priori perception of the whole economy as a unity, represented by concepts such as total product and its distribution, social classes as collective entities (Struve, 1910/11, 348). Equally fundamental for socialism as an idea, in Struve’s view, is the belief in the possibility of “complete rationalisation of the economic process”, while he considers the final goal of the

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<sup>9</sup> Struve published his first article on this subject in Russian in 1908 (Struve 1913, p. xix–xx). According to G. Gloveli, Struve was influenced by James Bonar, but mostly by the sociology of Lester Frank Ward, whose dichotomy “genetic/teleological”, Struve turned into a dichotomy “heterogenic /autogenic”. Gloveli shows how Ward has a lasting influence on the concepts of M. Tugan-Baranovsky, as well as on the types of planning defined later by V. Bazarov (Gloveli, 2014).

socialist movement less important. By “rationalisation” Struve means the “purposeful control/ordering of economic relations through the will of a central subject” (p. 345f).

Socialism’s opposite pole, economic liberalism, is singularist in its belief in the “free play of individual wills” resulting in “natural harmony”, which amounts to a “spontaneous rationalisation” of the economic process. In this sense, liberalism is “singularist rationalism”. But this is true only for “practical” liberalism, as it underlies the existing economic system, whereas theoretical economic liberalism as represented by Adam Smith is “more universalist than singularist in its method”. Struve thinks that the theoretical economic doctrine of liberalism represents a form of universalist thinking. If it conceives of the economy as a whole, it does not explicitly presume a central subject, but the latter “is always somehow (tacitly) imputed” (p. 346f).

The clandestine universalist approach inherent in theoretical economic liberalism has, in Struve’s view, prepared the ground for the triumph of socialism which dominates the debate at the ideological level. On the other hand, Struve diagnoses a crisis of socialism due to increasing doubts concerning the possibility of complete rationalisation of the economic process, which is beginning “to be recognised as cardinal error by the ‘mature’ parts of mankind” (p. 345).

The antithesis universalism—singularism has a parallel at the level of logic: realism and nominalism. Struve does not give definitions of these concepts, but he obviously refers to the medieval dispute about general concepts (*universalia*), whether they are something real or just names of things (*nomina*). It is universalist concepts in realist understanding which, according to Struve, present the greatest threat to the freedom of men, if they serve as basis of a political ideology. Logical realism of Hegel’s great metaphysical systems served as “fertile ground for the universalist constructs of Rodbertus, Marx and Lassalle” (p. 350). Struve warns that recent advances of Edmund Husserl’s “critical realism” might give new support to universalist/realist constructions of thought, especially socialism.

But this does not mean that Struve completely rejects universalism as such, because he admits that general concepts may be fruitfully applied in the social sciences, if they are “subject to critical examination. Most likely, the result of such an examination would be the uselessness of these concepts” (p. 352). Struve gives an example, which shows the productive application of such a general concept by Marx in his theory of history.

In its emergent formative phase, liberalism in its critique of the pre-modern traditional worldview relied on the law of nature as determinant of social and political relations. As a consequence, the worldview of “singularist rationalisation” was thought of being entirely consistent with the law of nature (p. 354) Yet, under the aspect of the dichotomy rational/irrational, Struve argues that the concept of law of nature cannot be used for the explanation of the functioning of economy and society. From the perspective of a “critical-empiricist approach”, he emphasises “the fundamental, immanent dualism of the socio-economic process”. By this, he means that only a limited part of it is determined by the forces of nature, including technology. Otherwise, the “irrational moment” prevails, wherever the will of men enters into the process—it is the specifically “human” element that is irrational. As “domain

of the irrational” the sphere of consumption should be given greater attention by economics (p. 356f). For Struve, money is a prominent example of the impossibility of complete rationalisation of the economic process, as demonstrated by G. F. Knapp’s state theory of money, which aimed at achieving complete control over this instrument. Struve’s was the first attempt to present a theory of the evolution of money through the prism of the fundamental dichotomy (universalism—nominalism) in the first volume of *Economy and Price* (1913) and later on his disciple S. Demostenov devoted to this task several large-scale studies (see Demostenov, 1937, 1945).

## 6 Parallel Concepts of Universalism: Karl Pribram and Othmar Spann

It seems somewhat strange that the ancient concepts of universalism and its antithesis—nominalism—were rediscovered more or less simultaneously between 1910/12 by three economists: besides Struve, the Austrian social scientists Karl Pribram and Othmar Spann. Judging by the dates of publications, Struve was first, followed by Spann in 1911 and Pribram (1912).<sup>10</sup> In their search for philosophical/epistemological foundations of economic thinking, they embraced a concept that did not play any noticeable role in the contemporary philosophical discourse, in which their use of the concept remained unnoticed, perhaps with the exception of Spann, who was, nonetheless, hardly taken serious as a philosopher. Pribram concurred with Struve in his rejection of “universalist” elements in economic thinking, whereas Spann developed an encompassing concept of economy and society in his version of a universalist philosophy.

The term universalism is generally associated with the medieval theologian William of Occam (1287–1347), who maintained that abstract concepts—universals—are the creations of the human mind—mere symbols (*nomina*, nominalism), which represent a multiplicity of individual objects—and have no extra-mental existence.<sup>11</sup> The opposite position, that universals have an extra-mental existence, like Plato’s eternal ideas as true essence of actual individual phenomena, hardly appears in nineteenth-century philosophy. The debate shifts to the question about the possibility of an “ontology” based on general concepts. In their critique of universalist thinking, Struve and Pribram refer to medieval universalism and to (Hegelian) ontology.

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<sup>10</sup> Struve notes that he was the first to formulate these concepts and notes the differences with Pribram’s approach (Struve 1913, p. xix-xx).

<sup>11</sup> In his tract, *Summa logicae* I 15.

## 6.1 *Pribram's Anti-Universalist Liberalism*

Pribram (1877–1973; on Pribram see Chaloupek, 2014, 2019) shares Struve's liberal political worldview, which motivates the critical attitude of both thinkers towards the use of universalist concepts in politics and in the social sciences. Like Struve, Pribram rejects political ideologies based on universalist realism. In his book on the "Origins of the individualistic social philosophy" (1912), he investigates the formation of modern economic thinking from the Middle Ages and the early modern period until Adam Smith's *Wealth of Nations*, with its change in the fundamental pattern of thought from universalistic thinking (realism) of medieval theology to individualistic nominalism. Man-centred nominalism leads to an increasingly empirical epistemology with experience, which is essentially individual, as source of true knowledge. The individualistic approach became the basis of political liberalism and of Adam Smith's economic liberalism. Pribram treats "collectivism" as a corresponding concept to universalism. At this point, it is important to note that Struve's use of the term universalism also implies collectivism in social thinking, whereas Pribram makes a distinction between the two. Pribram does not offer a philosophically convincing argument for such a correspondence. On the contrary, he gives examples of the use of collectivist universalist concepts by individualistic thinkers, when they insinuate "harmony of the world as a whole" (Pribram, 1912, 17), which reminds of Struve's above-mentioned critical remark against Smith.

Still, the triumph of individualism/nominalism was not complete. While individualism came to dominate economic and political thinking in Western Europe and in the New World in the nineteenth and twentieth century, in Germany "the power of thinking in terms of the collective has never been fully overcome" (ibidem, p. 102). As successors in political universalism, Pribram identified the authoritarian doctrine of pre-revolutionary Russia and the concept of German nationalism (Pribram, 1917/18, 184f). Later, Pribram added "dialectic reasoning" (Marxism in its various forms) as a separate pattern of economic thinking rooted in a distinct epistemological approach. Pribram refers to Engels' "dialectical materialism" (in his "*Anti-Dühring*") with its inevitable laws derived from nature and permanent struggle between classes, which are considered real collective entities, as driving force of historical development (Pribram, 1949, 39; Pribram, 1983).

## 6.2 *Spann's Version of Universalism*

The Austrian economist and social philosopher Othmar Spann (1878–1950) conceived of his *Ganzheitslehre* ("doctrine of totality", holism) as a philosophy as well as a political ideology, offering the model of a social order based on the concept of universalism. Whereas individualism, its opposite, portrays "society as sum of independent individuals, like a pile of stones, of which each has a self-contained, finished existence, connected only in a superficial, mechanical way", for universalism

“the interrelationship between individuals must be thought of as an own entity, which is the super-individual and primary, while the individual is its secondary derivative”. The basic error of individualism is that it thinks of the individual as autarchic, “mentally completed before it enters into social relations”. Universalism views society as a “spiritual entity”, as “reverberation of (individual) minds”, and “only in the spiritual community of many the individual is constituted and formed as mental person of its own”. (Spann, 1928, 24f).

With respect to the social order, Spann proclaims the primacy of the super-individual totality of society over its individual members, who are assigned their place and rank in society according to a structure that emerges from their function to the collective institutions, which themselves form a hierarchical order. As regards the economy, price formation is not the result of free competition, but determined by the functional performance of production units within the corporatist structure of the economy (*ibidem*, p. 165). Spann presented a detailed concept of social order in his book *Der wahre Staat* (1921). In such a “true state” society is organised in a multitude of corporate bodies, whose rank in the social structure is determined by the value of its contribution to the whole. The highest rank is occupied by the “leaders of the state”, conceived as a self-supplementing elite whose members are relieved of the degrading political fights that characterised electoral parliamentary democracy, which Spann despises, while the bulk of executive tasks is the responsibility of the de-politicised corporations.

Spann’s ideas for a social and economic order are based, on the one hand, on Plato’s Republic, and on the other hand, on romantic German philosophy and Catholic social doctrine of the nineteenth century.<sup>12</sup> In his characteristic presumptuousness, Spann claims to create a new ontological philosophy, but his sociological universalism, based on the unfounded logical priority of the whole over the part, eventually results in “pure social mysticism” (Wirkus, 1996, 173), presented in obscure, cranky terminology permeated by newly invented words.

Struve was critical of Spann’s theory, who, according to him, does not see the organic unity of society, and presents it mechanically. In a conversation with S. Frank, Struve resents that Spann has never heard of a science called “ecology”, where a forest is seen as an organic unity rather than as a mechanical group of trees (Frank, 1956).<sup>13</sup>

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<sup>12</sup> For brief surveys, see Wirkus (1996), p. 166ff, and Kampits (1984), p. 177f.

<sup>13</sup> Struve’s disciple in Bulgaria, N. Dolynski, criticised Spann’s universalism and holism from Struve’s positions:

“We can imagine such a conclusion, which is unacceptable for understanding the essence of the economy, only as the sublime expression of the idea of universalism of the whole which, in its effort to underscore the interrelation between the elements of the whole, simply results in refuting the only real thing in the social totality—the living individual. And the latter is exactly the most real thing, as he is in the centre of the economy, because depending on whether he exists or not, we can say that there is or there isn’t an economic life. Othmar Spann’s constructs, that end with a funeral march for the economic theory, are at the same time the farewell prayer for the social science as a whole” (Dolynski, 1930, 38–39).

### 6.3 *Disentangling the Issues*

The dispute between Struve and Pribram on the side of liberal individualist nominalism and Spann's concept of collectivist universalism with its authoritarian affinities concerns central questions of epistemological foundations of the social sciences. Several distinct issues are at stake which should be treated separately as well as in their interconnections:

- (i) The Whole (the totality) versus the Part;
- (ii) Abstract concepts ("universals"): can they be assumed to have a separate existence besides human minds ("real");
- (iii) The meaning of "rationality".

As concepts of pure logic, Whole and Part mutually presuppose each other, without any implication of priority of one over the other. Hence, from the viewpoint of pure logic, no claim can be made about priority if the logical concepts are applied in the analysis of society. As regards the relationship between man/the individual and world/the totality, the shortcut of Spann's *Ganzheitslehre* to establish priority for the whole, which serves as basis for his proposal for the "true" social order, appears ill-conceived and unconvincing, to say the least. But the question in itself has been a central problem of philosophy from the ancient Greeks to the present.

Singularism (Pribram's individualism) views society as a multitude of individuals. In economics, individual agents interact with each other, while the result of these interactions is determined solely by given original characteristic properties of the agents. This serves as basis for the liberal claim of priority of the individual (part) over society as a whole. This claim is both normative and factual—empirical: normative with respect to what is considered an ideal social order. In an empirical sense, absolute priority of the individual can be assumed only if no repercussions from the results of interactions are allowed on the characteristic properties of individual agents. Modern economics has "solved" this problem by the a priori-assumption of "methodological individualism". But this is not sufficient to claim priority of the Part in an explanation of the functioning of the economy.

Priority of the individual perspective leads Struve to reject concepts which perceive of the economy as a whole ("total product"). He also questions the legitimacy of general abstract concepts, e.g. "value". He criticises Marx's labour theory of value which refers to labour as a real substance which has a life independently of the human mind. He argues that the social sciences, and economics in particular, are prone to resort to this kind of universalist realism in social thinking, and warns against such tendencies, asking for critical examination case by case (Struve, 1910/11, 352f).

At the same time, Struve argues that universalist concepts should not be rejected a priori, they should not be done away with altogether. They may be productively applied to analyse historical developments and social phenomena. As examples he mentions Marx's analysis of the antagonism between the interests of the individual and the interests of society as a whole in the emergent capitalist economy

(p. 353f), and also Marx's analysis of "fetishist commodity production" in the capitalist economy (p. 356).

What Struve had in mind when he concluded that "universalist moments are super-individual" (p. 354), is a form of social thinking based on general abstract concepts (universals) of non-realist nature, i.e. for which no existence outside the human mind is made (as opposed to, Plato's "eternal ideas"). This is what Hegel, whom Struve misinterpreted, thought of, when he postulated a "transcendental subject", suggesting that there is a basic structure of self-consciousness outside the individual subject in the form of some "objective" and/or "absolute spirit". In more modern social science language, this implies that the individuals' perceptions of the world (and of society and economy) develop in parallel with a self-sustaining system of (super-individual) perspectives, without any priority of origination. In this context of his discussion, Struve states that "every super-individual is irrational in the above-mentioned sense". But this is unconvincing, since any "system of super-individual perspective" must be based on certain fundamental principles. Still, on this issue, Struve's view of the matter is more differentiated than Pribram's, who indiscriminately lumps together both kinds of "universalist realism".

By "immanent dualism of the socio-economic process", Struve means that only a limited part of it is determined by material conditions, knowledge of which permits "rational action" of individual agents whose behaviour can be rationally explained. Otherwise, the "irrational moment" prevails, wherever the will of men enters into the process—it is the specifically "human" element that is irrational. What Struve means here seems to be the following. Rational action pursues ends within the given conditions by means which agents consider best adapted to the ends, i.e. based upon the best available knowledge, whereas the choice of ends is "irrational", not subject to logical reasoning. But this does not mean that ends cannot be empirically analysed, and can therefore not be the object of (rational) scientific investigation, as Struve suggests in a shortcut.

Also, Struve's claim that rationality of a system by necessity requires its subjection to the "will of a central subject" is untenable. Economics since Adam Smith has portrayed the economy as a decentralised self-governing system which meets essential ends of individual agents, and it is simply not correct to say that it is based on a hidden supposition. Viewing the economy as a whole, does not imply the existence of a central governing subject—as Struve and Pribram suggest.

In his essay published in LOGOS Struve—as can be often observed in his writings—tried to achieve too much at one stroke. In his search for epistemological foundations of economics, he discussed the relevant issues in a wider philosophical context than his contemporaries Pribram and Spann. Overall, his treatment of the subject appears more differentiated, while at the same time less conclusive with respect to results.



## 7 Struve on Price, Value and Money

In his book “Economy and Price” (in Russian *Khoziaistvo i tsena*, 1913/16), Struve distinguishes two principal types of social formations, depending on their specific ways of interaction between their basic elements: a dualism of (i) “system” and (ii) “unity”. The first form is based on a horizontal interaction between individual independent elements. Coordination is the result of their spontaneous interaction (catalactics). In the second form, coordination is achieved through vertical theological links of subordination (hierarchy), power and ethical norms. Following this dualism, Struve distinguishes three types of economies “a set of autarchic economies, standing side by side and not interacting” ; “a system of interacting economies—economic order”, and “a subjective teleological ‘unity economy’”. According to Struve, of relevance are the last two types, namely “system of economies” and “unity economy”, as it is they that are manifested in economic reality.

With respect to methodology, Struve adopts the position of empirical positivism, according to which the starting point of each analysis is the “actual”, the facts. The “actual” is the subject matter of the analysis, which in turn determines the method. Prices and money occupy the central place in economic reality. The concept of “value” has no independent analytical meaning. Value has neither substance (labour, etc.) nor can it be derived from psychological dependencies (marginal utility, etc.). Only price is a fact.

In a “system of interacting economies”, there is free formation of prices (*вольные цены*), while “value” could only be conceived of as derived from prices, thus being a simple average of the prices observed “by eye” (“*глазомерно*”).<sup>14</sup> In the “unity economy”, prices are indicated by the authorities, or else are derivatives of some ethical norm (decreed prices, *указные цены*). In both systems, measurement and calculation, made possible by money and its function as price measure, are indispensable. Measurement is associated with the medium of exchange function, dominating the first form of coordination, and with the means of payment function, which is leading in the second form of coordination.

According to Struve, only in interacting economies can we speak of economic order, i.e. of economic activity in the true meaning of the word. The core here is exchange and prices (“who says exchange—says prices”, see also Struve, 2007 [1924]). “A good that has no price is not an economic good” (similar to C. Menger). Struve is critical of the different concepts of production, whether subjective or objective. In the system of interacting economies, the economies and the economic actors are differentiated by legal form and in accounting terms. Through price movement and calculation, the effectiveness of individual economies and economic agents is measured and compared. According to Struve himself:

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<sup>14</sup> For this, Struve was criticised by his disciple S. Kon (1925), as well as by Chuprov (1925), who believe that value has a right to theoretical (nomographic) and independent existence, defined as a mathematical expectation of prices. It can be argued, however, that Struve’s simple average approach corresponds to his approach of realism and is based on actual exchange behaviour (actors do not have a priori exchange access to probabilities).

The concepts of I. Economic good; II. Price  $\equiv$  value; III. Money, cannot do without each other. Their logical order is established as we have described in the previous sentence. But this order could be reversed ... because all these three concepts are just different aspects of an essentially unified phenomenon. [...] Without the idea of measuring, measurability, comparability, which is the essence of money, we cannot consider economic concepts such as price-value and good. And vice versa: money, as the specific phenomenon of a universal tool of exchange and payment is only the embodiment of the idea of measurability of the goods in the turnover and its price. (Struve, 1916, p. 68).

The empirical approach, opposite to the metaphysical one, maintains that the equality between commodities and goods is generated by and only by the process of exchange. There is, and there can be, no common substance, no equality prior to exchange, that is nothing exogenous to the exchange. It is obvious that from this point of view value cannot influence prices. In general, only the psychological process of evaluation precedes price formation. As regards value, it is the result of prices. (Struve, 1922, 185).

Since change of prices is a permanent process, Struve denies the existence of equilibrium, be it static or dynamic. According to him, equilibrium is a special case of movement. Struve talks about “mobile” statistical equilibrium (today we can call it “non-stationary”). According to Struve, the traditional understanding of equilibrium is related to the wrong theories of value.<sup>15</sup>

In the spirit of his strict empiricism, Struve places special emphasis on accounting, for which prices serve as information base. Accounting reflects the performance of all economic actors and all types of activities, thus also becoming the basis for political economy. Accounting (book keeping) goes hand in hand with the legal framework of economic activity, which sets the boundaries of “individual” economies and gives clarity about their contractual interaction (economies are “legal atoms”, Struve 1916, 59, “economies are accounting and legal entities”, p. 5). In general, accounting is a manifestation of the basic principle of economic activity:

The activity of every economic subject is aimed at obtaining more for less, at realising positive value differences. In the field of non-exchange natural economy, this process can exclusively proceed in the form of subjective evaluations. In the field of the exchange-based economy this process becomes objective as regards price formation. (Struve, 1916, 22)

The above-mentioned dualism manifests itself in the field of accounting in two different types of records: (i) records at actual prices and (ii) evaluation records, linked with the duality of prices (“decreed” and “free”), and also the duality of law (private and public) (Struve, 1916, Ch. IV). Accounting also provides the only solution to the—in Struve’s view—“so-called” problem of income distribution.

Political economy cannot provide more information about profit than an accounting report ... Only the statistical processing of initial data and precise accounting can answer a whole series of questions raised by political economy which it has tried to solve “deductively”. The problem of accounting for the incomes of individual economies can be solved in this way

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<sup>15</sup> Against Struve, A. Bilimovic defended the concept of equilibrium, as well as the theoretical existence of the category “value”. For Struve’s reply, 2007 [1923, 1924] and the publications of A. Dmitriev [especially 2013].

Bilimovic later repeated his critique in a comment to Struve’s German article of 1936 (see the following section).

only. The actual essence of the problem of distribution can be explained only by accounting. (Struve, 1916, 86)

There is no such thing as a nation's total income or product. National income is a naturalist fiction (Struve, 1916, Chaps. 3 and 5). Struve criticises the "theory of imputation" (Wieser and Clark), Marx's "theory of exploitation", or the "social theory of distribution", advocated by M. Tugan Baranovsky. According to the Struve, incomes (grouped into three categories: direct, indirect and derivative) are only a manifestation of prices. There are no specific "substantial", subjective, psychological or ethical determinant factors behind them. It is purely a matter of calculation.

A similar interpretation is given to "capital". Struve conceives of capital not as a physical good, as Böhm-Bawerk does, but as a sum of money. It is a capitalised income, and income and profit represent only price differences that occur in space and over time. Only in the model of interacting economies ("system"), "capital accounting" takes place, while in a centrally planned economy capital is completely ruled out.

The dualism re-appears in Struve's theoretical considerations of money. As a central economic institution, money is both "heterogenic"—money as a natural phenomenon, emerging as result of a spontaneous social process; and "autogenic"—money as a rational, wilful act of authority (Struve, 1913, 67–84).

Actually, what is money – an autogenic or heterogenic phenomenon? ...Sticking to the facts, we can apply the basic dualism of the economic process to the phenomenon of money. The "natural" moment, the heterogenic moment, prevails in money over a long period of time, but the rational, autogenic moment is also there. The task and history of development, the theory of money boils down to the fact that the effect of the two moments mentioned above must be examined and assessed in the actual phenomenon of money. [...] Since from the viewpoint of basic dualism the "theory of money" is just a special case and a special issue of "the theory of price" then the same problem arises here in this significantly broader field [author's note: on dualism]. (Struve, 1913, 79)

As for the functions of money, apart from measure, money, according to Struve, are above all "a means of payment" and "a medium of exchange". These functions are equally represented and exist equally in monetary history. The medium of exchange function reflects heterogeneity, spontaneity, characteristic of the model of interacting economies, the means of payment function represents autogenicity, characteristic of the system of teleological economic unity.

In different periods and types of economy and society either one or the other function of money predominates. For example, in the time of feudalism, the means of payment dominates, which is analysed in the "feudal theory of money". At several instances, it appears that Struve gives priority historically/ genetically (but not systematically) to the means of payment function:

Money grows from two roots: exchange/interchange (a bilateral act) and payment (a unilateral act). But in as far as money has a state public and legal character, in as far as it is the object of regulation and is an autogenic phenomenon, it emerges from the order which regulates payments. (Struve, 1913, 317)

As regards the selection of the specific type of good which becomes money, Struve points to the role of imitation which brings him closer to C. Menger's approach:

While these objects serve as individual decorations or distinctive signs, they are not yet able to perform this function [the authors: a means of payment and a medium of exchange]. But the development of needs is determined by two tendencies: (1) an aspiration for separation and (2) a striving for levelling. The decoration arises individually, and then it becomes universal through imitation. This distinctive sign gradually obtains a general assessment, a certain fluidity (*'hodkost'*). This is how the objects of decoration initially appeared in the role of money". (Struve, 1916, 161f)

The duality of monetary institution is the reason why Struve accepts the two leading monetary theories of his time, namely that of C. Menger and that of G. Knapp, as equally valid. Menger's approach describes spontaneity, heterogeneity and the function of exchange, and Knapp's approach—rationality, autogeneity and the medium of payment function. Knapp's theory shows the ongoing process of rationalisation of monetary relations. And, this has a direct impact on monetary policy:

The state – and this has been manifested throughout the history of money circulation in the world – is not omnipotent but it is not powerless either as regards money. [...] "The idealisation" or "nominalisation" of the monetary constraint is reduced to a simple order; this is the problem of the rational mastering of the complicated overlapping of phenomena in which the heterogenic element plays a significant role. Ignoring this element, "exceeding" the economic power of the state, immediately results in a collapse of monetary policy: the management of money circulation has become a monetary anarchy. (Struve, 1913, 321)

Although noted in Russian and Soviet literature, Struve's monetary theory has never been the subject of a special analysis.<sup>16</sup>

## 8 Essays on Central Concepts of Economics: *Gleichgewicht*, *"Wirtschaft"*

In the 1930s, when Struve held a chair at the University of Belgrade, he published a series of articles in German in the *Zeitschrift für Nationalökonomie*; and in the Publications of the Statistical Institute for Economic Research University of Sofia. Struve mentions that some of these essays are based on his book *Economy and Price* (1913/16), of which he planned a German edition which never materialised.

The meaning of "economy" is central subject of two of Struve's essays. "Economy" should not be identified with provision of goods for the satisfaction of needs. What Struve calls "primary economy" is not economy in the proper sense (which Struve called "secondary economy"), because for the latter the valuation of goods in terms of prices expressed in units of money is essential (Struve 1922, 505f). This is not the case in an administered economy "with unified central natural accounting" which inevitably requires not only central regulation of production but in addition "restriction and gagging of consumption" (p. 507).

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<sup>16</sup> Most seriously and thoroughly, it was used by S. Demostenov in his work on monetary theory and history of the theories of money (Demostenov, 1937, 1942, 1945). For more, see Nenovsky (2019) and Nenovsky and Penchev (2017).

From this understanding of economy as secondary economy, Struve criticises Friedrich Wieser's concept of a "simple economy" based "on the idealising assumption that the subject is a single person" (Wieser, 1927/1914, 9). Rational decisions in the simple economy are made by analogy with decision making in the "social economy". Hence, Wieser's "deduction of social economy from the simple economy is untenable. Only the exchange relationship (between a multitude of economic units) constitutes the economical, even in its simplest form" (Struve, 1938, 8).

Struve's discussion of the use of the equilibrium concept in economics may be seen as most typical for his rather unsystematic theoretical endeavours, also because this article provoked critical responses (Bilimovic 1936; Conrad, 1937). Struve's principal argument follows from his denial of "universal concepts", which he now seems to reject without exception. Struve questions the adoption of equilibrium, which is a concept of the natural sciences, by economics, where equilibrium is turned into an "independent power above (empirical) phenomena", a "mythological concept" (Struve, 1936a, 485f). Besides that, he has a variety of objections to equilibrium in economics: the static character of the concept, which would entail the disappearance of a market once equilibrium has been established (p. 511ff); Stackelberg's markets without equilibrium in case of monopoly on both sides (p. 506), counterfactual assumption of equalisation of production costs (p. 525f), rejection of the use of idealised models as approximation of reality, etc. As alternative concept, Struve proposes that economics, in order to find causal relations between variables, "must think stochastically and work on the basis of statistics" (p. 522). In Struve's view, metaphorical use of the equilibrium concept is legitimate only for the accounting system. Business accounts are based on the assumption of "reversibility", i.e. that all real assets of a company can be properly valued in terms of money prices. Accounts are an expression of equilibrium of "nature" (physical assets) and monetary assets. Depending on the realism of the initial assumption, one can speak of equilibrium, or its disturbance (p. 529f). Also, Struve thinks that company accounts could be an important source of empirical economic research. Even for economic theory "accounting, as 'spontaneous' elementary economic doctrine, can provide building blocks for a strictly empirical economic theory" (Struve, 1938, 7). Therefore, Struve regrets the separation of business economics from political economy.

Struve's article on the problem of business cycles is another example for his tendency to subject various approaches to his critical examination, with negative results: due to the complex nature of the modern economy, "there can be no abstract-deductive general theory of the business cycle. Therefore, the limitations for predictions are extremely narrow" (1937b, p. 14).

In a comment on Struve's equilibrium-article Alexander Bilimovic (1876–1963), professor at the University of Ljubljana, denies that equilibrium serves as a "mythological concept" in economics. Far from being a pre-conceived idea, it refers to "a tendency of the market, resulting automatically from subjective and objective moments under certain circumstances, thus determining the economic process" (Bilimovic, 1936, 220). Stationary equilibrium, in contrast to Struve's view, is not a motionless state. Rather, it means the equality of the quantities continuously offered and bought (p. 222). Bilimovic also argues that Struve confuses equality of two

different goods in the act of exchange with equality of supply and demand of the same good in market equilibrium. In the same vein, Conrad (1937), on the basis of his alternative theory of the market process, argued that in a (static) state of “persistence” (his terminology) exchange does not stop, as markets clear through continuous supplies and purchases (p. 22). Also, Bilimovic questions the applicability of equilibrium for understanding the nature of company accounts, in which only money values appear on both sides of the balance sheet, including purely monetary assets without physical content on the asset side (Bilimovic, 1936, 228).

## 9 “Ideal Types” Versus Positivism

The problems Struve had discussed in his essay of 1910/11 occupied a central place in his writings during the years of exile. His critical discussions of fundamental concepts of economics inevitably touch upon the question about the relationship between positivistic empiricism and abstract-analytical theory. This became the central subject of his essay on foundations for the perception of objects in economic theory and its methodological consequences (Struve, 1936b).

Any science investigates the relationships between variables. Unlike mathematics and philosophy, economics is concerned with objects represented by general concepts through which real phenomena are identified. (Struve, 1936b, 8f).

If, by necessity, any science must be based on general concepts (universalia), this does not pre-empt that “false and empty verbal concepts” may come to dominate thinking about social phenomena. This threat can be countered not through general methodological–epistemological discussion, but only through case by case-investigations in the social sciences which confront theoretical concepts with reality (p. 10).

Relationships between phenomena are derived from inductive observation of real processes; they cannot be established by logical deduction as in mathematics. Therefore, Struve emphasises the empirical character of the social sciences. Induction may take two forms: through “theoretical description”, which arrives at its conclusions by establishing causal relationships in verbal form; and “probabilistic” (stochastic and statistical) induction.

As theoretical inductive variant, Struve discusses Max Weber’s approach of “ideal types” at several occasions. He is highly critical of Weber’s use of the concept, which comprises such diverse phenomena as broad historical syntheses, e.g. capitalism, mercantilism, Christianity, as well as general concepts of abstract economic theory, e.g. exchange, price, value, capital. Thereby, Weber “overstretched” his concept of ideal type, “rendering it useless in its universality”. (Struve 1922, 503). If this excessive use had been criticised even by Weber’s followers, who tried to save the concept of ideal type as attempt to establish models of causal relationships gained

by “exaggerating” certain features of interrelations,<sup>17</sup> Struve is not convinced by this apology. In his view, thinking in terms of ideal types implies “to operate with concepts to which reality does not correspond . . . arbitrary constructs and empty abstractions, which do not originate from experience” philosopher Ferdinand (Struve, 1936b, 19).

Instead, Struve pleads for a rather strictly positivistic approach in economics, in which causality of a relationship is replaced by statistically established “necessity”, not identical with causality. Economics must start from exact observation and appropriate summing up of data as “statistical collectives” which comprise a multitude of non-uniform singular observations. Likewise, relationships between empirically established magnitudes do not have a definite exactness, but hold only within certain limits. Moreover, the nature of the established relationships is “a stochastic connectedness of random variables”. Here, “random” is not meant in an absolute sense, but that variables can have different values with certain probabilities.<sup>18</sup>

From all this, it appears that Struve wholeheartedly embraced the positivistic approach to economics—and yet, there remains an ambivalence which is characteristic for his thinking and his personality. In an article on the German sociologist and social philosopher Ferdinand Tönnies (1855–1936). Struve refers to his sociological conception based on the dualism of “unity” and “system” (Struve, 1937a, 57). In Tönnies’ most important book *Gemeinschaft und Gesellschaft* (first published 1887), *Gemeinschaft* (“community”), which is positively connotated, stands for unity, whereas *Gesellschaft* (“society”) stands for system, representing some form of decay. Struve criticises Tönnies for his assessment, which in Struve’s view has contributed to prepare the ground for the advancement of National Socialism, notwithstanding the fact that Tönnies was a sympathiser of the Social-democrats (p. 60). Tönnies book marks a distinct deviation from the prevailing tendency in sociological thought from a “voluntaristic-rationalistic/subjectivistic” concept of society towards a “voluntaristic objectivistic current” as initiated by Hegel and Lorenz von Stein (p. 58). The method by which Struve analyses and criticises Tönnies’ book is exactly the one which he has rejected as insufficient because of its reliance on purely theoretical-methodological reasoning.

## 10 Conclusions, Struve’s Heritage

Undoubtedly, Struve is a vibrant intellectual figure, a “once born” social scientist who connects Russia and the West. He was an important actor on the political theatre

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<sup>17</sup> In this context, Struve quotes an essay by Weber’s disciple Schelting of 1934. He might as well have quoted Sombart (1930, 258f), who proposed the term “rational schemes/*rationale Schemata*” as alternative.

<sup>18</sup> Struve’s references for his approach to the fundamentals of economics are, among others, the Russian statistician Alexander Chuprov and the Austrian mathematician Richard von Mises (1883–1950, brother of the economist Ludwig von Mises) and his book *Wahrscheinlichkeit, Statistik und Wahrheit* (1928).

in pre-revolutionary Russia. In his lifetime, he also enjoyed considerable reputation as economist. His works were known to the Western reader, especially among German-speaking scholars. He not only integrated the achievements of Western economic thought into Russian economic theory, but his methodological and analytical approaches later on influenced an entire generation of disciples and followers. The formation of Struve's ideas and their subsequent diffusion not only illustrate the basis of V. Avtonomov's thesis about the refraction of Western ideas through the Russian reality and their reverse influence on the West, but also add new moments. One of them is the influence of economic thought in CEE. In addition, Struve's complexity and multifaceted interests make him an example of a fruitful synthesis of different scientific traditions.

Struve's main achievements are in the philosophy of economic thought. His main interest was focused on epistemological foundations and methodology. This clearly reflects his descendancy from the style of economic thinking prevailing in Germany in the nineteenth and early twentieth century with its principal concerns in conceptual foundations, systematising of concepts, ideological interrelations, and on history, whereas work on the abstract-analytical apparatus was rather neglected (Schumpeter, 1927, 1ff). Struve's economic writings have since fallen into oblivion, when the latter increasingly came to dominate economics in the twentieth century. Struve's disregard for analytical theory comes to the fore in his treatment of central phenomena, e.g. price and money, where he shows little interest in explaining the formation of prices, or changes in the value of money. In his view, common sense models could serve as basis for statistical verification to which he assigned a prominent place in what he considered economic theory.

It is this strong focus on "facts" which explains Struve's turn towards positivism, and his critical attitude towards various currents of economics which operate with analytical models. However, he always remained ambivalent between strict factual positivism and the grand general approaches which aim at getting economy and society into view in its totality. This becomes clear from his continued use of general "universal" concepts. In this respect, Struve's basic "dualism" between System and Unity is a remarkable achievement, as it anticipates modern systems theory. With respect to economic policy, Struve could be seen as one of the forerunners of Ordoliberalism and institutional economics.

The vast majority of Struve's followers emigrated and did not forget to pay a tribute to their teacher, as demonstrated by the *Festschrift* (1925) published in Prague. If most of his followers, who stayed in Soviet Russia, could not openly demonstrate their intellectual sources, Struve's influence can be found in a number of publications (e.g. L. Yurovskiy, M. Bertantzky, V. Bazarov, B. Livshits, I. Trakhtenberg and even S. Strumilin).

Struve has a strong influence on economic thought on the European periphery, and thus on the overall development of economic thought in Europe. Today, the interest in the common European cultural tradition is strong<sup>19</sup> and the study of the spread of Struve's ideas proves useful. There is no doubt that Struve's influence in Bulgaria

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<sup>19</sup> See Magliulo (2019).



is felt the strongest. Through his two prominent followers, S. Demostenov and N. Dolynski, he became extremely popular between the two world wars.<sup>20</sup> Today, Struve is present in Bulgaria through his disciple Demostenov, whose textbook on political economy, as well as his publications on the theory of money, on history of monetary theories, and on banking, are still popular. During communism, Demostenov became the main target of attacks by Marxists in Bulgaria. Interestingly, some of Struve's Bulgarian students lived to see the fall of communism and shared recollections of him. Followers of Struve can be found in other countries, especially Yugoslavia; e.g. the prominent Russian economist A. Bilimovic, who worked in Ljubljana and who, notwithstanding his disputes with Struve, shared a number of his ideas.

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<sup>20</sup> Among Bulgarian followers stands out the prominent philosopher Dimitar Mihalchev (1880–1967), who actively communicated with Struve since 1911. In the *Festschrift* (1925), Mihalchev published an article entitled “*Social reality as a starting point and subject of historical science*”, in which he applied Struve's methodology of realism and dualism. Mihalchev's correspondence, as well as his autobiographical notes “*My Philosophical Development*”, reveal Struve's influence (Mihalchev, 1996).

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# Bazarov, Bogdanov, and the West



Elizaveta Burina

## 1 Introduction

Intellectual heritage of Alexander Bogdanov (1873–1928), a philosopher, a politician, an economist, and a medical doctor, has not yet been fully appreciated, neither by Russian, nor by Western scholars: Very few papers make reference to Bogdanov.<sup>1</sup> Even less is written on Vladimir Bazarov (1874–1939), Bogdanov’s close friend and colleague, a Gosplan<sup>2</sup> economist, a philosopher, and a translator. Bogdanov and Bazarov<sup>3</sup> studied in the same high school in Tula, where they became friends. Then, in 1892, both of them entered the Moscow State University to pursue a bachelor’s degree in chemistry, at the department of natural sciences and mathematics. The two never received their diplomas as they were expelled back to Tula for engaging in revolutionary activity and Marxist propaganda. Afterwards, they were, in their own ways, popularizing Marx’ economic thought among the Russian workers and proposing the principles of economics for the “New World”,<sup>4</sup> as Bogdanov used to refer to the Soviet Union economic organization. Up until the end of 1910s, they

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<sup>1</sup> In Russian, see the works of Gloveli (2017, 2018, 2020). He is one of the major contributors to the investigation and re-appreciation of Bogdanov’s intellectual legacy in Russia, as well as in the West.

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<sup>2</sup> The State Planning Commission of the Soviet Union, formed in 1921 as the agency responsible for central economic planning in the Soviet Union. Gosplan functioned until the dissolution of the Soviet Union in 1991.

<sup>3</sup> “Bogdanov” was actually a *nom de plume* of Alexander Malinovsky that he adopted to publish Marxist and revolutionary articles, as well as “Bazarov” was a *nom de plume* of Vladimir Rudnev.

<sup>4</sup> “New World” was a collection of articles that Bogdanov published in 1905.

worked together on different projects: editing revolutionary newspapers and journals, publishing collective volumes on philosophy, and publishing translations.

But, let us consider their story since the very beginning, tracing the Western ideas that shaped Bogdanov's and Bazarov's economic and philosophical views. Marxism was, undoubtedly, a cross-cutting theme for their works in economics. Studying at the Moscow State University, Bogdanov and Bazarov became fascinated by Marx' doctrine, as many educated young people did. At the same time, they suggested to adapt Marxism to *the* Russian reality of that time. For them, Marxism was a scientific doctrine that lacked, however, a coherent system for the philosophy of science. To "complete" Marxism, Bogdanov, followed by Bazarov, chose the empiricist philosophy of science, elaborated independently by two German-speaking scientists: physicist Ernst Mach and philosopher Richard Avenarius.

In general, Bogdanov's work can be divided into four distinctive fields: the development of empiricism-based philosophy of science, Marxist propaganda, social economic policy, and medicine. Bazarov worked mainly on philosophy and economics of social planification. The traveling of ideas from the West to Russia and back in Bazarov's and Bogdanov's case is observable in all the fields, but we will focus here on the philosophy of science and economics.<sup>5</sup> The two authors are considered together here because their works were complementary: Bogdanov elaborated the philosophical and epistemological framework (for which he borrowed the ideas of Mach and Avenarius) that was later used by Bazarov in his economic investigations.

The first goal of this chapter is to figure why did Bogdanov and Bazarov opt for framework and how did they use it. That is, the "West-Russia" part of our story, focusing on the period from the end of the 1880s to the end of 1920s. The second objective is to bring out the "Russia-West" part. For both of them, we are observing the type of knowledge circulation that Keim (2014) conceptualized as *reception*<sup>6</sup>: "A scholar takes up theories, methods or concepts from elsewhere and relates them to his or her work" (Keim 2014, p. 93). In our case, "elsewhere" is the West, namely German-speaking countries. Keim distinguished four modes of reception: acknowledgment, acceptance, modification, and rejection (*ibid.*, p. 97). What factors enabled the reception process? What mode of reception was it and why? The first section of this chapter is an attempt to answer these questions. The second section addresses

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<sup>5</sup> Marxist propaganda, conducted by Bogdanov and Bazarov, deserves, however, a mentioning in this chapter. In their first exile in Tula they organized "workers' circles", regular meetings with the workers of Tula factories, to teach them the basics of Marxism. Later with the help of Skvortsov-Stepanov and Bazarov, Bogdanov processed the material used for his lectures at the workers' circles meetings into *A Short Course of Economic Science* (Bogdanov 1899), a manual for workers first published in 1897. The latest, 10th edition, was published in 1920. The book was a success both in Russian society, and in the West, it was translated into Polish (1905), Estonian (1907), English (1923). Also, Bogdanov was quite famous among the Italian communists because he organized the Communist Party Schools on Capri (in 1909) and in Bologna (in 1910, 1911), see Scherrer (1978) for the detailed description of the School's organization.

<sup>6</sup> Keim (2014) distinguished three types of knowledge circulation: reception, exchange, and negotiation.

the same questions for the “Russia-West” side of the knowledge circulation in case of Bogdanov’s and Bazarov’s ideas.

## 2 West-Russia. The Search for the Methodological Objectivity

### 2.1 Context

Both Bogdanov and Bazarov had a good command of foreign languages (German, French, English, and even Latin and ancient Greek) since high school, that was a “classical gymnasium”, i.e., a high school with the main emphasis on learning ancient languages and theoretical mathematics. In their early works, we find numerous references to Western authors. In his own texts in Russian, Bazarov would sometimes insert phrases and quotes in German to better express his idea (Bazarov 1904, p. 276). Together with Ivan Skvortsov-Stepanov,<sup>7</sup> Bazarov prepared the translation of *Das Kapital*, edited by Bogdanov. First, they published the translation of the second volume (in 1907), then the third in 1908, and finally, the first volume in 1909. Their translation was the most complete (previous translations, edited by Nikolai Danielson and by Peter Struve, lacked the translations of certain quotes and footnotes). Even before the publication of this fundamental translation, Bazarov was earning his living from translations of texts in German.<sup>8</sup> Bogdanov was actively involved in editing of the translations.

Thus, the first factor that facilitated the reception of the philosophy of science proposed by Mach and Avenarius was the absence of language barriers. The Russian intellectuals of that time were well acquainted with the German-speaking cultures and, thus, with the social context in which the texts were written. They traveled to Germany and Switzerland to study abroad and often chose these two countries as their emigration destination. Bazarov, for example, went to Berlin in 1900 to study philosophy; Lunacharsky, who actively worked with Bazarov and Bogdanov during the first decade of the twentieth century, studied in Züerich in 1895–1896.

Let us now examine the content-related factor. What exactly attracted Bogdanov and Bazarov? In the following section, we shall focus only on Bogdanov, since he was the original “importer” of the ideas of Mach and Avenarius. He edited the first Russian translation of Mach’s *Analysis of Sensations*, published in 1906, wrote a preface to it (Bogdanov [1906b] 2005, pp. 31–38), and published numerous articles for philosophical and revolutionary journals and newspapers,<sup>9</sup> taking up Mach’s philosophy

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<sup>7</sup> Skvortsov-Stepanov was a Russian Bolshevik revolutionary. He met Bazarov and Bogdanov in exile in Tula in 1896.

<sup>8</sup> For example, in 1903 Bazarov (together with Skvortsov-Stepanov) published the translation of Harald Høffding’s *Philosophy of Religion*; in 1904 they published the translation of Werner Sombart’s *Modern Capitalism*.

<sup>9</sup> Together with a Russian writer Maxim Gorky, Bogdanov himself edited several journals.

of science and sharing his perception of it. Noticing the similarity between Mach's ideas and those of Avenarius, Bogdanov was often considering them together. The results of Bogdanov's work in the philosophy of science are summarized in two books: *Empiriomonism. Articles in Philosophy* (1904–1906) and *Universal Organizational Science. Tektology* (1913, 1917). We shall focus our following analysis on these two works as they mark the two main stages of the development of his philosophical ideas.

Young Bogdanov was searching for a suitable philosophy of science that would be in line with Marxism. He testified that before getting acquainted with the historical materialism of Marx, he “was mainly engaged in the natural sciences and was an ardent supporter of the worldview that can be designated as ‘materialism of natural scientists’” (Bogdanov 1906a, p. III). That “materialism of natural scientists”, or, as Bogdanov named it “old materialism”, referred to a picture of the world, built entirely from one material—from “matter” as an object of physical sciences. To Bogdanov, that worldview lacked the epistemological component to be a solid philosophical foundation for Marxism, and he started his search for it, that resulted in the adoption of the positivist philosophy of Ernst Mach and Richard Avenarius (known as empiriocriticism<sup>10</sup>). Further investigation of this reception process requires us to give a brief description of their views and some historical context of the emergence of those views.

## 2.2 *Empiriocriticism of Mach and Avenarius*

Ernst Mach (1838–1916) was a physicist by training. In 1860, he received his doctorate in physics at the University of Vienna with the thesis on electrical charge and induction (Blackmore 1972, p. 14). He continued working at the physics department of the University of Vienna giving lectures in physics, his main interest always remaining experimental physics. At the same time, already by 1860, he had gotten acquainted with several books in philosophy (ibid., p. 26), had read and analyzed Ernst Weber's and Gustav Fechner's works on the measurement of mental response to physical stimulus, and had set up several experiments to “prove” Fechner's law.<sup>11</sup>

Mach's fascination with the experimental physics and the physiology of sense organs determined his philosophical and epistemological views. For him, knowledge was sensational and experiments were the only source of evidence. He believed that the goal of philosophy was to unite the special sciences into one integrated coherent

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<sup>10</sup> The term “empiriocriticism” was suggested by Avenarius and refers to a positivist empirical philosophy of science. In Russian tradition this philosophical current is also called “the second positivism” (the first positivism refers to that of Auguste Comte, John S. Mill and Herbert Spencer; the third—to Vienna Circle philosophy). This classification is taken from the Soviet Dictionary of Philosophy (Rosental and Yudin 1963).

<sup>11</sup> Fechner's law is formulated as follows: “The magnitude of a sensation is proportional to the logarithm of its stimulus” (Blackmore 1972, p. 20). Mach tried to prove it for the perception of time, which resulted in the rejection of the law.

whole.<sup>12</sup> Mach expressed his main findings in philosophy and epistemology in his book *Beiträge zur Analyse der Empfindungen* (Contributions to the Analysis of the Sensations) (Mach 1886), which was edited and republished under the new title *Die Analyse der Empfindungen und das Verhältnis des Physischen zum Psychischen* (The Analysis of Sensations and the Relation of the Physical to the Psychical) several times (in 1900, 1901, 1902, 1906). It was the last, fifth, edition that Bogdanov used to prepare the Russian translation.

Mach took a stand against metaphysics, and he also claimed that the Kantian “thing-in-itself”, that is, anything behind appearance, is unknowable (Mach [1906] 1959, p. 6). Thus, “things-in-themselves” could not be subjects of any science. In general, Mach defended epistemological phenomenalism: The only knowable things are phenomena, which were, for Mach, appearances, sensations,<sup>13</sup> feelings. These phenomena (relatively constant) and their manifold inter-relations were the subjects of science.

To study these inter-relations, Mach suggested to “replace the conception of cause by the mathematical conception of function” (ibid., p. 89). He believed that since the phenomena (complexes of sensations) are all inter-related in different constantly changing ways, it was not possible to fit these inter-relations in a cause-effect framework, neither was it logically necessary. Mathematical functions served him as an appropriate scientific tool for the study of phenomena dependence. The main scientific method proposed by Mach was the principle of the economy of thought, or *Denkökonomie*. He called upon the scientists to search for the universal concepts and laws, formulated in the mathematical language as general formulas (instead of reporting the numbers for each individual event or experiment), since the universal form economized memory and mental efforts. According to Mach, the main task of philosophy was the coordination and organization of the special sciences into a whole, a unified science with positivist methodology and general laws, written in mathematical language. In that complete scientific concept of the world, Mach believed it to be possible to reason by analogy.

Mach’s philosophical views had roots in physics. They were, however, closely aligned to the ideas of a philosopher by training, Richard Avenarius (1843–1896). In Mach’s own words, the affinity between the views of Avenarius and his own was “as great as can possibly be imagined where two writers have undergone a different process of development” (ibid., pp. 46–47). For the major part of his career, Avenarius was a professor of the philosophy of science in Zürich, where he stayed until his death in 1896 (Carstanjen 1897, p. 29). The term “empiriocriticism” emerged from the title of Avenarius’ *magnum opus Critique of Pure Experience* (Avenarius

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<sup>12</sup> From Mach’s inaugural speech in the University of Vienna: “As the blood in nourishing the body separates into countless capillaries, only to be collected again and to meet in the heart, so in the science of the future all the rills of knowledge will be gathered more and more into a common and undivided stream” (Mach 1895, p. 162).

<sup>13</sup> Mach ([1906] 1959) tells the story of how he, at the age of 15, realized something that was decisive for his whole view: “on a bright summer day in the open air the world with my ego suddenly appeared to me as one coherent mass of sensations, only more strongly coherent in the ego” (Mach [1906] 1959, p. 30).



[1888] 2018).<sup>14</sup> This term characterizes his epistemological theory that in many ways coincided with Mach's philosophy. It was also built upon an empirical basis: all the knowledge originated from the pure experience. What did Avenarius consider as pure experience?

First, he stated a fundamental empiriocriticist assumption that once an individual faced any element of his environment, he claimed of his experience, forming a statement. In this empiriocriticist view, anything is considered pure experience if it has been stated as experienced by an individual (Carstanjen 1897, p. 451). Then, Avenarius introduced two forms of pure experience: synthetic and analytical. The synthetic definition of pure experience implied that the elements of the environment were the only prerequisites for it (Avenarius [1888] 2018, p. 4). In other words, what individual experienced and what he formed a statement about was caused by something from his surrounding (e.g., he saw a red bird, he smelled flowers, he got hit by a car). Analytical concept of pure experience did not require the environment to be the underlying premise of the experience statement. Human's beliefs, for example, could also lead to a certain experience.<sup>15</sup>

Thus, for both Avenarius and Mach, the relation between mental and physical phenomena became the central research issue. Both of them assigned the crucial role to the analysis of sensations in the physiological sense. Their theories of knowledge relied on biological processes in human beings.<sup>16</sup> Both of them emphasized the necessity of the economy of thought principle. The two men never met in person (Mach [1906] 1959, p. 49) and never worked together, although empiriocriticism is now described as a philosophy, elaborated by both Mach and Avenarius.

### 2.3 Reception by Bogdanov

In his works, Bogdanov admitted that he had indeed chosen Avenarius' *Critique of Pure Experience* as "a starting point for the further work" (Bogdanov 1906a, p. XX) and explained why he was attracted to the empiriocriticism in general. To him, it was "the strictest of the existing forms of positivism" (ibid.). Positivism attracted Bogdanov as he was rejecting metaphysics in general and idealism in particular. He also explained his interest in Mach in his preface to the translation of the *Analysis of Sensation*: "in our turbulent time, in our blood-soaked country, what he [Mach] teaches is especially dear: the calm relentlessness of thought, the strict objectivity of

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<sup>14</sup> 2018 edition used further in this paper is the English translation of the third original German edition, dating back to 1921.

<sup>15</sup> If a child states that she has seen angels, then the angels are an experience for this child (Carstanjen 1897, p. 451). But this sort of individual's experience does not come from the environment, it comes from individual's belief that the angels wander among us.

<sup>16</sup> Mach's theory of knowledge is referred to as "biological" by some scholars (Čapek 1968). Also, Mach supported Darwinist approach and integrated the conceptions of struggle for existence, of development, and of selection into his philosophy of science (Mach [1906] 1959, p. 50).

the methodology, the merciless analysis of everything taken on faith, the merciless extermination of all idols of thoughts” (Bogdanov [1906b] 2005, p. 37).

Bogdanov’s preface could be a curious case-study for someone who would venture to do a comparative sociology of prefaces, as bequeathed to us by Pierre Bourdieu in his “Conditions sociales de la circulation internationale des idées” (Bourdieu 2002, p. 6). The preface was titled as “What should a Russian reader look for in Ernst Mach?” and answered precisely that question, placing Mach’s ideas in the revolutionary context of post-1905 Russia, the context in which Mach himself was not imagining his doctrine. However, by placing Mach’s ideas into Russian context, Bogdanov actually tried to prevent the misunderstanding arising from the general tendency that Bourdieu warned us about: ideas travel without the context of their emergence (ibid.). Bogdanov stated that even though Mach was not a social democrat and not a revolutionary, he was not a “bourgeois” author (in the sense hostile to proletarian culture), therefore could not be understood as one, i.e., could not be considered as idealist.<sup>17</sup>

What did Bogdanov actually use to develop his own doctrine of empiriomonism? He claimed that the only thing that he borrowed from Mach was “the idea of the neutrality of the elements of experience in relation to the ‘physical’ and ‘mental’, and the dependence of these characteristics only on the connection of experience” (Bogdanov 1906a, p. XLI). However, in *Empiriomonism* and later in *Tektology*, we find many more elements that constituted Mach’s philosophy of science: the necessity for the unity of science, the empiricist epistemology, the economy of thought principle. What he actually modified for his own philosophy was the dual character of experience, claimed by Avenarius. That is where the part “monism” comes from in the name of Bogdanov’s book. How did he overcome the challenge, posed by the existence of two sorts of experience, according to Avenarius (recall the synthetic and analytical concepts of pure experience<sup>18</sup>), and created a concept of monist experience?

Bogdanov claimed that pure experience, i.e., the only legitimate source of knowledge, could not be personal (in Avenarius’ terms, analytical). It could only be collective. Thus, the collectively or socially recognized experience<sup>19</sup> was the only objective source of knowledge. “If we forget about this supreme criterion of objectivity, then systematic hallucinations could form an objective world, and sane people could hardly agree with this” (Bogdanov 1906a, p. 37).

Bogdanov also fits Mach’s categories of “physical” and “mental” into his vision: “the antithesis of the ‘physical’ and ‘mental’ series of experiences is reduced to

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<sup>17</sup> The mere thought of understanding Mach as an idealist does not make much sense, however, Bogdanov had to emphasize it since Plekhanov, the main ideologist of the Bolsheviks, accused Mach of idealism and solipsism.

<sup>18</sup> Bogdanov explains that for Avenarius “it was not a dualism of reality, but a dualism of the cognition process, and Avenarius believes that ‘diese Dūalitaet ist kein Dualismus’ (this duality is not dualism). But this point of view is difficult to accept as correct, no matter how reassuring it might sound” (Bogdanov 1906a, pp. 21–22).

<sup>19</sup> Collective experience, in Bogdanov’s view, meant the experience of the whole humanity, therefore a collective hallucination of a group of people could still not be considered as collective experience.

the difference between the socially organized and the individually organized experience” (ibid., p. 41). He believed that Marxism, enhanced by empiriomonism, was set to become the ideological basis for the “new world”. In their correspondence in 1909, Bogdanov and Maxim Gorky discussed the means of the popularization of this ideological basis (Spiridonova et al. 2010, pp. 47, 48).

Later, Bogdanov took empiriomonism to the next level, creating *tektology*,<sup>20</sup> a universal organizational science, as a scientific basis for the systematization of the socially organized (collective) experience, “scattered” over specific branches of knowledge. Bogdanov viewed the world as consistent of different systems: social, natural, technical, ideological (e.g., societies, human bodies, machines, doctrines). His *tektology* was a monist science of organization, and its objects were different systems or complexes. It studied every **system** from the **organizational** point of view, that is, analyzing the **inter-relations** among its **elements** and its inter-relations with other systems. The key to understanding *tektology* as a theoretical framework lays in Bogdanov’s definition of “organization”, the definition that connects the concepts in bald: The organized system is one which is practically greater than the sum of its inter-related elements.<sup>21</sup> These elements were “merely parts into which the object under study is required to be decomposed according to research goals” (Bogdanov [1913] 1996, p. 75).

With *tektology*, Bogdanov proclaimed universalization at two levels: (1) in the structure of all systems (no matter how big or small, no matter social or natural) and (2) in the method of studying these systems. At the first level, Bogdanov distinguished two most general universal organizational principles: the formative principle of ingression and the regulative principle of world selection. Ingression allows systems to combine, join together. Bogdanov stated that if two systems lack common elements (e.g., two people, two pieces of different materials, two ideas), they could be joined with the help of some “intermediate” complexes (language for different people, a translator in case they speak different languages, glue in between the pieces).<sup>22</sup>

Selection principle clearly came to *tektology* from biology. Bogdanov elaborated it to distinguish two types of it: conservative (treats only preservation and non-preservation of systems) and progressive (treats the growth and development of systems) (see Bogdanov [1913] 1996, pp. 175–200). Basing his theoretical analysis on ingression and selection principles, Bogdanov presented further mechanisms that he believed to be innate to all organized systems.

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<sup>20</sup> “We shall call this universal organizational science ‘Tektology’. The literal translation of this word from the Greek is ‘the theory of construction’. Construction is the most general and suitable synonym for the modern concept of ‘organization’”. (Bogdanov [1913] 1996, p. 63).

<sup>21</sup> “... in reality the organized whole turned out to be practically greater than the sum of its parts, not because new activities were created within it out of nothing, but because its available activities were combined more successfully than the opposing resistances”. (Bogdanov [1913] 1996, p. 71).

<sup>22</sup> The opposite *tektological* principle was *disingression*: separation of some elements from the complex, leading to system’s short-term or long-term imbalances. *Disingression* usually facilitates the transition of the system to a new state.

One of these universal mechanisms is dynamic equilibrium<sup>23</sup> and the law of Le Chatelier, associated with it. Bogdanov stated that this law (formulated as “if a system of equilibrium is subject to an influence, which alters some of the conditions of its equilibrium, it produces processes directed so as to counter-act this change” (ibid., p. 261)) could be applied not only to the chemical and physical systems, but to all the systems, including economic. About three decades later, Samuelson (1947) formally introduced Le Chatelier principle into economics, namely into demand theory. Samuelson first demonstrated this principle on the example of the demand for an output: “the demand for labor by a firm will be more inelastic if you hold the price of its land constant than if you hold the quantity of its land constant (and this whether land and labor are complementary rather than substituting” (Samuelson 1960, p. 368). Later, Samuelson himself and other economists suggested ways to extend the use of Le Chatelier principle, including applying it to explain the logic of multipliers in macroeconomics (see Samuelson 1972; Milgrom and Roberts 1996; Milgrom 2006; Koebel and Laisney 2010).

Another universal tektological principle, particularly important for economic science, was “the law of the leasts”: “the stability of the whole depends on the least relative resistance of all of its parts at any moment of time” (ibid., p. 219). Originally developed in biology,<sup>24</sup> the law of the leasts (or the law of the minimum) has had several applications to economics. In general, it means that the growth is limited by the weakest element of the system (e.g., the least developed industry or the most scarce input). Bogdanov himself illustrated the law of the minimum with an economic example: “an extension of the economic whole depends on those of its parts that lag farthest behind”. For instance, if the inputs quantities could be increased by 2, 4, 6, 9%, etc., the overall expansion would only be successful up to the limit of 2% (ibid., p. 303). However, this could only be the case for the Leontieff production function specification, which is the actual example of the law of the leasts in economics. At the same time Bogdanov’s application of the law of the leasts to economic growth was not very convincing, since the inference will not be correct given other production function (especially in case of inputs being perfect substitutes<sup>25</sup>).

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<sup>23</sup> “Dynamic equilibrium” refers here to the physical meaning of the term: dynamic equilibrium occurs when two reversible or opposite processes are balanced. Bogdanov explained: “Tektology must consider any case of the conservation of forms as the result of their dynamic equilibrium, and any occasion of dynamic equilibrium - as the practical relative equality of the two processes of assimilation and de-assimilation” (Bogdanov [1913] 1996, p. 190). On the application of Bogdanov’s tektological equilibrium theory to economics see Belykh (1990). This article was published in English.

<sup>24</sup> The law was originally proposed by a German biologist Carl Sprengel and then popularized by Justus von Liebig. It states that the growth of a plant is determined not by the overall number of resources available, but by the resource that happens to be deficient.

<sup>25</sup> See the presentation of some relevant critique of the law of the minimum in its application to agricultural economics in Lok (1963). In the lecture at the First congress on the Scientific Organization of Labour Bogdanov suggested that “the elements that may be mutually replaced, for example, different kinds of fuel, must be considered as one and the same item” (Bogdanov [1913] 1996, p. 304). This could fix the problem of the perfect substitutes only.

Universalization at the second level, i.e., in the method of tektology as a science, relied on the principle of the economy of thought, clearly a Machian principle. Bogdanov considered it to be the essential principle of the knowledge organization in science. He meant tektology to study organization of things, people, and ideas, in other words to unite all the specialized sciences. The way to accomplish the unity of science was close to the Vienna Circle research program. In Bogdanov's own words, "the aim of tektology is to systematize *organizational experience*; this science is clearly *empirical* and should draw its conclusions by way of *induction*" (Bogdanov [1913] 1996, p. 85). He also searched for a unified language for tektology and found it to be mathematical language as the most neutral: "mathematics abstracts from all the particular properties of the elements hidden behind its schemata. This is achieved by mathematics with the help of indifferent symbols, like numbers or letters" (ibid., pp. 86–87). In general, Bogdanov's methodology was coherent with the methodology that Mach proposed for the philosophy of science:

The methods of tektology, as is seen, combine the abstract symbolism of mathematics and the experimental character of the natural sciences. Furthermore, the very formulation of its problems, the very treatment of organizedness by tektology, as has been elucidated, should stick to the social historical viewpoint. And whatever the subject matter, or the content, of tektology, it embraces the whole world of experience. So tektology is really a universal science by its methods and its content. (ibid., p. 95)

Thus, Bogdanov believed in the existence of universal mechanisms, applicable to all the systems (social, natural, etc.), and in the existence of the universal method of cognition of those mechanisms. From Marx, he took the idea that the task of philosophy was not only to interpret the world, but also to change it, and from Mach, he borrowed the economy of thought principle and the empiriocriticist principle that both the physical and psychical domains have the same aspects of experience. Thus, the influence of the philosophy of science of Mach and Avenarius, or empiriocriticism, on Bogdanov's views is quite strong and easily observable both in *Empiriomonism* and in *Tektology*. The main reason why Bogdanov chose this framework was its empirical character that would help, in his view, to build a truly Marxist philosophy of science. He never got to finish his tektology project and actually implement the above presented ideas to build the sound theoretical framework, however, he was one of the pioneers of the unity of science movement.

The question left to answer is: what kind of reception was it? Certainly, it was not a mere acknowledgment, as Bogdanov borrowed the empiriocriticist framework to build empiriomonism and tektology. At the same time, it was not pure acceptance since Bogdanov did not just apply the whole theoretical framework. He made his changes, the most crucial of which was the re-definition of the source of knowledge: For him, it was only collective experience, and not the experience of individuals.<sup>26</sup> Therefore, the form of reception that we observe here is modification.

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<sup>26</sup> Another point that did not satisfy Bogdanov in empiriocriticism was the lack of proactive appeal and "contemplative" philosophy of being (see Boll 1981 for more detail).

## 2.4 Reception by Bazarov

So far, we have left Bazarov out of scope. He and Bogdanov were in constant communication during the time of Bogdanov's work on *Empiriomonism* and *Tektology*, and Bazarov was well aware of his friend's ideas. Bazarov himself was inspired by empiriocriticism and contributed to its popularization in his philosophical articles. In 1910, he published a collection of articles where he presented the discussions in the philosophy of Marxism that took place among the Russian intellectuals (Bazarov 1910). Bazarov defended Bogdanov's position that empiriocriticism of Mach and Avenarius was a suitable framework for Marxist philosophy of science.

Unlike Bogdanov, who was a theoretician and a philosopher, trying to elaborate a scientific concept of the world, Bazarov was a practitioner. He worked on the socialist monetary theory, on business cycles modeling and on social planification. His reception of empiriocriticism is better characterized as acceptance since he borrowed independent methodological elements (mainly from Mach) and applied them to his economic investigations. For his works in economics, he adopted the analogical reasoning method, advocated by Mach. Also, he analyzed the economy mathematically, trying to find the general equations for the economic phenomena, and for that, Bazarov was using differential calculus. Finally, as well as Mach, he believed that "the ideal of the universal monistic science is, undoubtedly, the supreme goal of the cognitive work of the humanity" (Bazarov [1927] 2014, vol. 2, p. 37).

The historical context is very important for the analysis of Bazarov's work. He started working at Gosplan in 1921, the first year of the New Economic Policy (NEP). The economy that he got to observe and plan was not completely socialist just, yet, as the NEP allowed for some important elements of the free market (small-scale agriculture and retail industries). At the same time, it was the economy, completely destroyed by the wars and revolution. One of Bazarov's main theoretical objectives was the mathematical description of the recovery rates. The model that he suggested was an oversimplified ad hoc model, and it is not the model itself that is interesting, but the methods that Bazarov used to build it.

Following the theory of dynamic equilibrium, elaborated by Bogdanov in *Tektology*, he represented the recovery process of the Soviet economy in the beginning of the 1920s as a shift from one level of the development of productive forces to another, higher level. Within this framework, he built a mathematical model, describing the pace of the recovery process, employing the equation of an autocatalytic reaction from chemistry. According to him, "the unity of the method is quite legitimate where qualitatively different phenomena have identical organizational ties, where materially different processes are formally the same, have the same structure" (ibid., p. 91). For example, it was necessary to determine what kind of reaction (multi-molecular or mono-molecular) would be structurally identical to the absorption of commodities by the market, and "many other questions touching on the inter-relationships among the elements", and "only after an exhaustive analysis has given a satisfactory result can we apply a formula to the study of the speed of

market processes that depicts the dynamic regularity of a specific kind of chemical reaction” (Bazarov [1928] 2019).

Bazarov viewed his model as an example of Bogdanov’s idea<sup>27</sup> that natural (here: chemical) and social (here: economic) processes were governed by the same mechanisms and as an illustration of the possibility of the unity of science. It was a model designed to predict the pace of the socialist economy recovery, yet it was based on the market mechanisms of supply and demand and was positive, rather than normative.

At Gosplan, Bazarov worked on the general methodology of the socialist planning. He suggested to formally distinguish two approaches to socialist planning: “genetic” and “teleological”.<sup>28</sup> In Bazarov’s definition, genetic planning was based on the extrapolation of existing trends, and teleological planning was based on the directives from the authorities (Bazarov 1924). The first approach meant sticking to the natural development of the economy, and the second required the Soviet government to come up with quantitative objectives (in “material” terms, i.e., units of goods and services) for all the industries. This distinction, most likely, had its roots in the distinction between spontaneous-inertial genetic and consciously directed teleological social processes, introduced by the American positivist sociologist Lester F. Ward. Georgy Gloveli pointed out that it was Mikhail Tugan-Baranovsky (followed by Nikolai Kondratiev) who was the first to “import” this distinction from American sociology to his economic theories (Gloveli 2014, p. 27). In his theoretical and practical suggestions for “genetic” and “teleological” planning, Bazarov made use both of Ward’s sociology and of Bogdanov’s tektological principles.

Bazarov suggested to plan the development of the state industries teleologically and the agriculture sector genetically. He believed that only the synthesis of the two approaches could provide the best methodology for the long-term planning (Bazarov 1926). In Bazarov’s view, only this synthesis of positive (genetic) and normative (teleological) approaches could ensure the stability of the “dynamic equilibrium system” of the Soviet economy (during the NEP) by reconciling its socialist and free market elements (*ibid.*). This approach is clearly tektological. Moreover, he directly applied tektological concepts, for example, the law of the minimum. He believed that the speed of the recovery was limited by certain weak links of the economic system chain (e.g., the lack of effective demand for the consumer goods).

The majority of his Gosplan colleagues split into two groups, for instance, Vladimir Groman gave a priority to the genetic approach, and Stanislav Strumilin—to teleology. The latter, who called himself a “communist planner” (Gloveli 2020, p. 76), put forward the development of a system of quantitative parameters, summarized in numerical series of a directive plan. In the end of 1920s, this teleological approach was approved by the Party as the only right methodology not contradicting communist ideology, and Bazarov was unduly criticized for being a firm believer in the primacy of genetic approach.

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<sup>27</sup> Bazarov wrote about the methodology of this chemistry-based model in his article, written as a memorial to Bogdanov’s life and work to support Bogdanov’s ideas (Bazarov [1928] 2019).

<sup>28</sup> According to Belykh (2011).

### 3 Russia-West: Scattered Evidence

The reaction on Bogdanov's and Bazarov's works among their Russian contemporaries was important for the further diffusion of their ideas to the West. By the end of the first decade of the twentieth century, there were two camps of Marxists in Russia: "... a heated debate is raging among Russian Marxists on the question of whether Marxist philosophy originates just in the empirical world, or does it postulate a certain reality beyond all possible experience" (Bazarov 1910, p. 69).

The first camp (those who believed that the experience was the source of knowledge) was "led" by Bogdanov and the second (those admitted the existence of an abstract reality beyond any human experience) by Plekhanov. It so happened that Lenin took Plekhanov's side in that philosophical controversy.<sup>29</sup> In his work *Materialism and Empiriocriticism* (1909), written as a critique of the empiriocriticist ideas, he called Bogdanov and his supporters "Machists",<sup>30</sup> the word quickly became a pejorative and made the further philosophical endeavors of those related to the "Machist" group very difficult.

The analysis of Lenin's critique is outside of the scope of this chapter. I shall only quote Bogdanov's reaction to it that he shared with Gorky in their private correspondence:

I've read Lenin's book. It's something wonderful. He knows everything! But he messed everything up. Impudence and ignorance are at maximum. He stands for the absolute truth and the eternal truths; that is a truly Berdyaev's<sup>31</sup> position. Speaking about "things", every now and then he switches to the hated "Machism" and sometimes even to empiriomonism; but, of course, does not notice it. The book is too complicated for a layman to read; but for a specialist, the ignorance of the author is too obvious. (Spiridonova et al. 2010, p. 60)<sup>32</sup>

What is curious, however, is that the Western scholars of that time got acquainted with Bogdanov's and Bazarov's version of Mach's and Avenarius' ideas through Lenin's book which was translated into English and German in 1927 and quickly became very popular.<sup>33</sup> Before the publication of Bogdanov's biography in 1966 (in German), he was mostly known by the Western authors as "the leader of the first leftist opposition among the Bolshevik ranks and an advocate of a philosophical heresy, suppressed by Lenin in his famous work *Materialism and Empiriocriticism*"

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<sup>29</sup> For a detailed analysis of the philosophy of different Bogdanov's supporters and their dispute with Plekhanov see Kelly (1981). The reason why Lenin decided to side with Plekhanov could be not purely philosophical. Many authors point to the possible political tension between Lenin and Bogdanov. See, for example, Grille (1966), Biggart (2019). Bogdanov was Lenin's serious political rival, and by writing *Materialism and Empiriocriticism*, Lenin made Bogdanov look like a traitor of true Marxism in the eyes of all those who did not care to understand Bogdanov's approach.

<sup>30</sup> Here: the followers of Ernst Mach.

<sup>31</sup> Nikolai Berdyaev was a Russian orthodox Christian philosopher, who has always been openly defending idealism. As a result, in 1922 he was forced to immigrate from Russia.

<sup>32</sup> Gorky replied, agreeing that "Lenin's reasoning is religious indeed" (Spiridonova et al. 2010, p. 63).

<sup>33</sup> While the complete translation of *Empiriomonism* was published only in 2019 (Bogdanov [1904–1906] 2019)!



(Haupt and Weill 1967, p. 1360). Though, the Western intellectuals who studied both empiriocriticism and the official philosophy of the Communist Party pointed at the inconsistency of Lenin's critique. Indeed, Philipp Frank, a Vienna Circle member interested in Soviet science and philosophy,<sup>34</sup> was well aware of Bogdanov's and Bazarov's ideas. In 1935, he wrote that in the USSR, "the various forms of neopositivism and logical empiricism are all branded with the label 'Machism' and, as such, are sharply condemned. It was perhaps an ominous event for the history of philosophy in the U.S.S.R. that Lenin set forth his philosophic views in a book directed against the Russian followers of Mach and Avenarius - the book *Materialism and Empiriocriticism*" (Frank [1935] 1955, p. 198). In Frank's opinion, "Lenin took issue with Machism because it is in many respects related to diamat,<sup>35</sup> and he considered it especially suitable for him to bring out his own teachings very sharply by means of a polemic against it" (ibid.).

Other than that, the Western reaction on Bogdanov's and Bazarov's works can be roughly divided into the following categories:

- (1) Translations. *Tektology* was first translated into German in 1926 and published by Kniga editing house in Berlin. George Gorelik published its first English translation in 1980. Even though that translation was not complete, its publishing greatly contributed to the popularization of Bogdanov's ideas in the West. Gorelik also published articles, providing the analysis of Bogdanov's tektology appeared around 1970s, see Gorelik (1975, 1980, 1983). Ilmari Suslioto also provided such an analysis in his book *The Origins and Development of Systems Thinking in the Soviet Union* (Suslioto 1982). The first English translation of *Empiriomonism* was published only in 2019 in one of the volumes of the great project "Alexander Bogdanov Library",<sup>36</sup> edited by John Biggart, Evgeni Pavlov, and David Rowley. This library is the most complete collection of English translations of Bogdanov's works. It also unifies the relevant contemporary research on Bogdanov. Bazarov's main theoretical work, *Capitalist Cycles and the Recovery Process of the USSR Economy*, is still waiting to be translated. Among his works that were translated and published<sup>37</sup> is his monograph *Productive Labor and Labor that Generates Value*<sup>38</sup> and four of his articles in the methodology of the social planning (published in Spulber 1964).
- (2) Historical interest in their personalities. Biographies and narrative articles fall into this category. In 1966, Grille published Bogdanov's biography in German

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<sup>34</sup> Frank's lecture on his "Travel Impressions on the Scientific World Conception in Russia" was the first official lecture of the Verein Ernst Mach. In November 1928, "an impressive audience of 200 was reported to the police directorate" (Stadler 2015, p. 146).

<sup>35</sup> Dialectical materialism.

<sup>36</sup> <https://bogdanovlibrary.org/>—Project of the Historical Materialism Series at Brill.

<sup>37</sup> There are also some online translations. For example, Francis King's English translation of Bazarov's "What is Needed for Socialism" <https://web.archive.org/web/20110610151146/http://www.uea.ac.uk/his/webcours/russia/documents/chto-nuzhno.shtml>.

<sup>38</sup> Independently published by David G. Rowley.

(Grille 1966). Another biography was recently published in the Historical Materialism Book Series by White (2018). Bazarov's first intellectual biography was written by a Western researcher, Francis King. In 1994, Francis King defended his Ph.D. dissertation on Bazarov. This dissertation was particularly valuable because it contained the first almost complete list of Bazarov's works and his detailed biography (King 1994). King also wrote a short chapter "Bazarov and the West" in a recently published collection of Bazarov's works, edited by Andrei Belykh (King (2014). He noted that the reaction from the West to Bazarov's legacy so far is just expressed in short descriptive articles and encyclopedic notes.<sup>39</sup> Vincent Barnett, a well-known historian of Russian economic thought, in his Ph.D. dissertation also mentioned that Bazarov was "somewhat known, but the detail of his economic theory remains uninvestigated by Western scholars" (Barnett 1992, p. 4). This is still true, however, Bazarov is quite popular in the West due to his achievements in the development of the socialist planning methodology. An analysis of his economic theories of planification was first presented in Erlich (1960). Later, Bazarov was mentioned in historical and economic works on the Soviet economics<sup>40</sup> (see Despres 1980; Tartarin 1980).

In case of Bogdanov, the corpus of historical secondary literature is vast: The works of the historians, notably from the University of East Anglia, led by John Biggart, in collaboration with Georgy Gloveli, provide solid ground and guidance for further research (see especially Biggart et al. 1998).

- (3) Analytical papers on the Russian reception of empiriocriticism and polemics among the Russian Marxists. See Kelly (1981, 1990), Boll (1981), Williams (1986). These authors thoroughly investigated the philosophical disputes between Plekhanov's and Bogdanov's supporters, paying special attention to Bogdanov's and Bazarov's modifications to empiriocriticism. This topic is also covered in a book *Beyond Marx and Mach* by Jensen (1978), devoted to Bogdanov's philosophical views.

All these examples of reception can only be characterized as acknowledgment. The works of Bogdanov and Bazarov sparked interest mainly in historians of political economy. It is odd especially in the case of Bogdanov's tektology that was the forerunner of the modern systems theory and cybernetics. Systems theory is an interdisciplinary study of systems as they relate to one another within a larger, more complex system. The key concept of systems theory, regardless of which discipline it is being applied to, is that the whole is greater than the sum of its parts. It is a popular opinion that the systems theory originated in the 1930s and the first call for it came from Ludwig von Bertalanffy's research, who in 1968 published *General System Theory: Foundations, Development, Applications* (Bertalanffy 1968). Bello

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<sup>39</sup> King mentioned that Bazarov should be translated into Spanish, taking into account the importance of economic planning in the Latin America developing countries.

<sup>40</sup> Only in the general catalogue of the theses, defended in the Paris 1 University from 1970 to 1989, there are about 20 works, mentioning Bazarov and even investigating his theories. These Ph.D. dissertations were supervised mainly by Marie Lavigne and Michel Lesage.

(1985) demonstrated that Tektology and the general system theory were similar in approach and major concepts. Bello admitted that “in the Western and in the Eastern worlds, Ludwig von Bertalanffy is considered the ‘father’ of General Systems Theory” (Bello 1985, p. 111), however, tektology was first published around fifteen years before Bertalanffy published his first sketches of the general systems theory. Soviet scientist Takhtajan claimed that Bertalanffy’s concept was a repetition of Bogdanov’s ideas in tektology and saw a clear influence of the latter on the former. Nevertheless, Bertalanffy never mentioned Bogdanov’s works even though he knew about the German translation (Takhtajan 1989, p. 350).

There were probably several reasons for neglecting *Tektology* in the West. First of all, its suppression in the Soviet Union prevented the emergence of secondary literature, providing neutral syncretical analysis of Bogdanov’s theory. Second reason was the relatively late English translation. At the same time, Bogdanov used complex terminology from natural sciences, which made it difficult for his *Tektology* to gain popularity. As for Bazarov, the lack of works by Western authors is simply explained by the absence of a translation of the *Capitalist Cycles*.

## 4 Concluding Remarks

Bogdanov’s reception of the Western ideas is an example of reception-modification. Adapting empiriocriticism to the Russian Marxist context, Bogdanov suggested a solution to what he considered a problem: dual character of experience that was a source of knowledge in Avenarius. Bazarov’s reception of the same ideas is better characterized as reception-acceptance.

What they were looking for and what they have found in empiriocriticism was the objectivity of method. Departing from empiriocriticism of Mach and Avenarius, passing by the development of empiriomonism, Bogdanov arrived to tektology. Even though he has not yet been widely recognized as a forerunner for the systems theory, the recent increase in interest in Bogdanov’s legacy is certainly contributing to making his ideas more known to Western, as well as Russian, audience. After all, by trying to destroy Bogdanov as a political rival, Lenin made him, as well as Bazarov, curious figures to philosophers and economists of nowadays.

The reception of Bogdanov’s and Bazarov’s ideas by the Western researchers was mainly reception-acknowledgment. Overall, if we only focus on economic science, we can trace the travel of concepts from the empiriocriticist philosophy of science developed by Mach and Avenarius to Bogdanov’s tektology (with its further applications to economics), from there to Bazarov’s economic theories and back to the West through the works of the historians of economic thought and economists-sovietologists, investigating Soviet theories of socialist planning.

Interestingly, Bogdanov’s philosophy of science and Bazarov’s methods of economic analysis had the same roots as the logical positivism of the Vienna Circle. Vienna Circle members accepted only positivist and empiricist methodology as truly scientific, they were looking to create a neutral language of science, common for the

scientists all over the world, verification by induction, and unified methodology for all disciplines. Many of them found the basis for their research program, known as logical positivism, in Ernst Mach's empiricist philosophy of science,<sup>41</sup> originated in his investigations in physics. The main difference was, probably, the aspiration of Bogdanov and Bazarov to combine empiriocriticism with Marxism, "to ally socialism and science by means of Mach's philosophy" (Blackmore 1972, p. 240). However, Vienna Circle members did not refer to Bogdanov or Bazarov. The absence of translations to English had been and still is one of the main obstacles for the circulation of ideas, especially in Bazarov's case. This means, there is some work to be done!

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<sup>41</sup> Vienna circle members first organized their educational activities under the name "Ernst Mach Society".

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# Lenin's Development Economics: An Outline



Denis Melnik

## 1 Introduction

The amount of literature dedicated to Lenin is vast. The critical review of that literature would require an extensive study of its own. The history of the Lenin studies, in both the Soviet world and outside of it, made his activities to be one of the most documented among the historical actors of his time. Yet that history tells more about the political trends of the twentieth century than about Lenin himself. The author of one rather recent study distinguished “three Lenins”: “the living Lenin, who created Bolshevism, led the Bolsheviks to victory in 1917”; “the posthumous Lenin as Stalin fashioned him”; Lenin as perceived by the perestroika reformers: “a Marxist idealist, an anti-Stalin and fundamentally a democrat.” “Both posthumous figures were carved out of the original; both had some claim to represent him; neither fully captured him” (Gooding 2002, vii). This list of “Lenins” is by no means exhaustive. But the main challenge has always been posed by “the living Lenin” made elusive by the accumulated amount of conflicting interpretations, each claiming to convey “the original.”

All activities of Lenin usually were subject to scrutiny in terms of his actions as the leader of Bolshevism and to evaluation in terms of the attitude to the course (real, potential, or desired) of the history of Soviet Russia. As a result, Lenin the towering figure of the political history of the past century has supplanted Lenin the intellectual. Lenin is somewhat lost for the intellectual history (and perhaps the Soviet hagiography contributed to this at least as much as anti-Bolshevik studies). In general, the widespread approach crossing the partisan lines assumed that Lenin was a great politician, but not a theorist.

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Arguably, the most part of attention to Lenin's intellectual legacy in the twentieth century was due to the struggles among currents in Marxism. After the crucial period of 1956–68, those can be boiled down to the confrontation between the Soviet and anti-Soviet approaches.<sup>1</sup> An essential point of that confrontation can be presented as the question: How much Marx was there in Lenin? One author indicated in the 1970s, that Soviet historiography acknowledged the impact of Russian revolutionary tradition, but “the prevailing interpretation holds that Lenin's political ideas were mainly inspired by Marx.” On the contrary, Western authors tended to emphasise the influence of Russian radicalism (Theen 1973, 72). The same author holds that “Bolshevism ... is best understood as an amalgamation of the economic and historical materialism of the Marxist doctrine and the voluntarism of the Russian revolutionary tradition, as expressed in Lenin's personality” (ibid., 71). Thus, the intellectually sophisticated evolution of Marxism in the Western social democracy was contraposed to the reflections of the Western ideas in well-intentioned but erratic movements of a (or, perhaps, the) Russian soul.

In recent decades, the shift of attention away from the political context of the twentieth century, the departure from the ideological framework of the Cold War and the weakening of the premises of the Eurocentric approach have made it possible to relieve the grip of East–West dualism in the Lenin studies. “Though Lenin did not begin his revolutionary career as a Marxist, this does not mean that his Marxism was perfunctory, that it was mere gloss on a pursuit of power by any means. Once converted, Lenin never questioned the basic principles of Marxism as he had originally understood them. ... Marxism was the rock on which he founded his life—without it there could have been no Leninism” (Gooding 2002, 34). It is admitted that Lenin's political project “was driven by a highly optimistic, indeed romantic, scenario of inspiring class leadership that had strong roots in European Social Democracy” (Lih 2011, 16).

“... Lenin did not make a single practical decision in his whole life which was not the rational and logical outcome of his theoretical standpoint” (Lukacs [1924] 2009, 41). Already at the earliest stage of his activity as a public intellectual Lenin demonstrated commitment to provide the firm theoretical foundations for his political activity. He was not original in his approach to application of Marx's theory to the issues of economic development among other “orthodox Marxists” of the 1890s. Nor did he make the most advanced analytical contributions in the field (suffice it to refer to Mikhail Tugan-Baranovsky's theory of business cycles). Yet, Lenin succeeded in elaborating his own line that deserves consideration in terms of intellectual history not only because it was Lenin's, but on its own merits.

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<sup>1</sup> “Anti-Soviet” does not necessarily stand for anti-Communist. Lenin the reformer and architect of NEP outlined above as “Lenin number three” usually got positive acclaim in the accounts opposite to the “actually existing socialism” both in and outside of the Soviet world, being considered as the alternative to Stalin. See, e.g., Lewin ([1968] 2005).



Still, the issue of Lenin's consistency as a Marxist theorist remains. The reception of Marxism in Russia required its interpretation if only because Russia was a relatively backward economy, while Marx's theory assumed the situation of a fully developed capitalist mode of production. Lenin embraced the message of the inevitability of capitalist development in Russia and elsewhere and staunchly defended it from attempts to outline some particular "Russian road" to development (cf. Wada 1983). The question for Lenin was never whether the peculiar institutions of backwardness could prevent the expansion of capitalism "by the book". He got the firm (dogmatic or otherwise) grasp of Marxism to assume the superstructure may change the basis. The question was how to apply the principles of the political economy of the developed economic system to a developing one (cf. Milios 1999). The paper presents an interpretation of this Lenin's endeavor at the early stage of his intellectual track.

## 2 Lenin's Early Development Studies in Context

Radicalization of young Vladimir Ulyanov presumably began in 1887, when he was 17, following the trial and hanging of his older brother, Alexander, involved into a plot to kill Russian czar Alexander III. In August of the same year, Lenin began law studies at the Kazan University only to be excluded and sent in exile to the family estate in the Kazan region due to the participation at the student unrest in December. There he spent almost a year digging into the literature available at the home library, mostly Russian journals of the 1860s–80s. It was in this period that he might encounter the tradition of Russian radicalism as expressed with "the Aesopian language" in the censored publications.

"In October 1888, restrictions were loosened enough to allow Vladimir and his family to move back to Kazan. Here, the properly Marxist stage of Vladimir's evolution began, as he participated in illegal Social Democratic reading circles and began to cut his teeth on Marx's *Capital*. Here began the love affair with the writings of Marx and Engels that continued all his life" (Lih 2011, 33). By that time, it should be noted, not only the first but also the second volume of *Capital* had been available in Russian (Russian editions appeared in 1872 and 1885 respectively), though Lenin might use German editions as well. Characteristically, his first analytical intervention into Marx's theory (Lenin [1893] 1937)<sup>2</sup> was focused on the elaboration of Marx's reproduction schemes from Ch. XXI of the 2nd volume.<sup>3</sup> In December 1894, in the letter to his sister Maria in Moscow, Lenin mentioned that he had requested

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<sup>2</sup> To facilitate chronological placing, here and thereafter in references to Lenin's works the date in square brackets indicates the year(s) of writing; the following date in brackets—the year of the first publication. All references to Lenin are based on the last Soviet editions of his collected works published in Moscow: Russian (PSS) and English (LCW).

<sup>3</sup> In January 1896, shortly after being detained in prison in St. Petersburg, Lenin wrote to his sister Anna requesting some publications to study: "[I] am busy with Tugan-Baranovsky; he has published a sound piece of research but his diagrams, those at the end, for instance, are so confused that I must confess I do not understand them; I shall have to get Volume II of *Capital*." (Lenin to A.I. Ulyanova

some unspecified acquaintance about getting the third volume of *Capital* (which was published by Engels in German earlier that year) and asked her to remind of the request: “I should like to know because it is not easy to get that book” (Lenin to M.I. Ulyanova [1894] 1929).<sup>4</sup>

Lenin’s earliest contributions date back to 1893, when he was 23.<sup>5</sup> His latest contributions—the notes he dictated being already ill and unable to write by himself—were made in 1923. Several periods of Lenin’s literary activity can be distinguished within those thirty years. Obviously, the primary concerns of Lenin after the 1917 revolution kept his attention away from theoretical issues. But at no time between the early 1890s and 1917 did he pursue any abstract cause. Since the earliest contributions, he made the application of Marxist theory to the pertinent issues of the day his main political weapon.

In the early 1890s, Lenin entered the debates between the Russian Narodniks, the socialist approach whose representatives advocated a non-capitalist path of Russia’s development toward agrarian socialism, and the “orthodox Marxists,” the group of young university intellectuals mainly from St. Petersburg, who argued that Russia could not circumvent the capitalist stage of development and, in fact, had already entered it contrary to Narodniks’ claims. Lenin obviously sought to uphold the Marxist cause. But neither did he initiate the debates, nor was the major figure there. The leading intellectuals on the Marxist side were Peter Struve, Tugan-Baranovsky, Sergei Bulgakov. Lenin’s designated enemies at that battle were the Narodniks. Yet, the implicit (and interconnected) targets of a “visiting Marxist”<sup>6</sup> who arrived at the imperial capital in the fall 1893 was: to make a name of himself among the refined St. Petersburg intellectuals; to lay the foundation for his political project through establishing the connections with the “circles” (groups) of radical intelligentsia and workers. The first target was fulfilled with “an oeuvre covering more than 1,600 printed pages” (Milios 1999, 2)—the corpus of writings constituting the first three

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[1896] 1924). Evidently Lenin mentioned the first edition of the study of business cycles (Tugan-Baranovsky 1894). The cited English translation in LCW by Progress Publishers is astonishingly incorrect. In the Russian original of the letter Lenin wrote about “schemes” (cf. PSS, vol. 55, 21). Tugan-Baranovsky’s (1894) edition indeed contained 12 “diagrams” as is indicated on the cover—charts and data on cyclical fluctuations. But there are also elaborations on Marx’s reproduction schemes derived from Ch. XXI of the 2nd volume. Lenin presumably might refer to Scheme 2 “Accumulation of capital under the growing number of workers” (p. 411); Scheme 3 “Accumulation of capital under the constant number of workers and given techniques” (pp. 419–420); Scheme 4 “Accumulation of capital under the progressive state of techniques” (pp. 423–424).

<sup>4</sup> Russian edition of the third volume would be published in 1896 by Nikolay Danielson, who held extensive correspondence with Marx and Engels and edited also the first two volumes of *Capital*. During the 1890s, Danielson, under his penname Nik.—on, was the subject of Lenin’s recurrent harsh criticism as a proponent of the Narodnik approach.

<sup>5</sup> Earlier, in the late 1889 or 1890, he translated from German the *Manifesto*; the manuscript of translation had some limited circulation among Lenin’s acquaintances in Samara before being destroyed. (PSS, vol. 1, 567).

<sup>6</sup> That is how Nadezhda Krupskaya defined Lenin recollecting his paper presentation (Lenin [1893] 1937) at a meeting of one radical group (quoted in: PSS, vol. 1, 574, note 17). In the English edition, the phrase was tactfully changed by editors into “our new Marxist friend” (LCW, vol. 1, 516, note 17).

volumes of LCW. Realization of the second led to Lenin's imprisonment in December 1895 that lasted until February 1897 and to the subsequent three years he spent in the Siberian exile.

The sources Lenin used for the earliest contribution comprised the works of Marx and Engels available at that period, Russian economic literature and statistical data, few foreign (mainly German) sources. In line with his first steps as a radical, the main reference point remained the contemporary Russian journals (LCW, vol. 1, 603–616). In May–September 1895, Lenin made his first trip abroad, visiting Austria-Hungary, Switzerland, France, Germany. Lenin met Georgy Plekhanov, Paul Lafargue, Wilhelm Liebknecht, other Russian and European socialists,<sup>7</sup> got access to European literature and libraries, and had an opportunity to practice his language skills.<sup>8</sup> During more than four years of imprisonment and exile Lenin obviously was restricted in access to sources, though with help of his family members he managed to get an ample supply of literature; occasionally he was able also to work in libraries. In prison and exile, he managed to start and to complete his “magnum opus” of the period—and in fact his most extensive monographic contribution—*Development of Capitalism in Russia*—based on a wide array of sources.

In January 1900, Lenin's exile ended and he left Siberia. Later that year he left Russia. The first period of his literary activity, when he underlaid the foundation for his claim to political leadership, was over. Except for nearly two years in 1905–1907, when he illegally stayed in St. Petersburg and then in Finland during the “First Russian Revolution,” Lenin's emigration in Europe lasted until 1917. Lenin fought many factional battles and continued pursuing his political project. “Bolshevism, as a distinct current in Russian Social Democracy, arose in the years 1904–14. During those years, Bolshevism was a Russian answer to Russian problems” (Lih 2011, 84). The outbreak of the First World War caught Lenin at the foot of the Tatra Mountains (since 1912 he lived in nearby Krakow), in present-day Poland, then Austria-Hungary. In the early August 1914, as a Russian national, he was put in prison and spent 11 days there before being released and allowed to leave for Bern, Switzerland due to the intercession of Polish and Austrian socialists, most notably the leader of Austrian Social Democrats Victor Adler. “The Krakow Lenin was a Russian

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<sup>7</sup> In Geneva Lenin visited his friends from the Samara period (they got acquainted in 1890–91): Apollon Schucht and his family, who emigrated by that time from Russia. Lenin was a godfather to their daughter Anna (1893–1963) (see *Biograficheskaya ...* 1970, 52, 54, 80, 101). Apollon Schucht (1860–1933) never played any significant political role. However, two sisters of Lenin's goddaughter—yet-to-be-born Yulia (1896–1980) and Tatiana (1887–1942) would play an important part in life of Antonio Gramsci.

<sup>8</sup> Lenin wrote to his mother from Salzburg: “This is my second day of travel abroad and I am practising the language; I have discovered that I am weak at this and have the greatest difficulty in understanding the Germans—or rather, *I don't understand them at all ...* I am not discouraged and continue distorting the German language with some zeal.” (Lenin to M.A. Ulyanova [1895a] 1929). In August, he wrote to her from Berlin: “The only bad thing is the language—I understand far less conversational German than French. The pronunciation of the Germans is so unlike what I am accustomed to that I do not even understand public speeches, although in France I understood practically everything in such speeches from the very outset.” (Lenin to M.A. Ulyanova [1895b] 1929).

Social Democrat with opinions about European and global issues. The Bern Lenin was a European Social Democrat of Russian origin.” (Ibid., 123–124). Reacting to the war and breakup of socialist internationalism, Lenin returned to the issues of capitalist development.

### 3 Two Scenarios for Russia’s Economic Development

If the “orthodox Marxists” indeed won over the Narodniks in the 1890s, then this could be because they began to control Marxist narrative in Russia. In doing so, they almost erased from subsequent historiography the fact that for nearly two decades before they entered the scene in the early 1890s, it was up to Narodnik authors (most notably Nikolai Danielson, who maintained correspondence with Marx and Engels and edited three volumes of *Capital* in Russian) to propel and apply Marxist approach in Russia. By the end of the decade, young Marxist intellectuals (Lenin included) succeeded in representing the opponents as “homegrown” (reactionary, utopian, etc.) weirdos out of touch with “the modern theory” from the West. This way they got the monopoly over Marxist narrative in Russia (almost immediately starting to struggle between themselves).<sup>9</sup>

The debates, however, cannot be reduced to rhetorical issues. One of the major paradoxes of Marx’s theory consisted in a contradiction between the description of capitalism as the most productive and dynamic social system ever existed and the forecast of its imminent breakdown. This inadvertently led to interpretations of Marx’s vision that conveyed either the progressive or the breakdown scenario for capitalist development. At the end of the nineteenth century, Narodniks focused on the economic difficulties generated by capitalism (general impoverishment and disproportions due to the competition-driven expansion). Their opponents found themselves in rather an awkward role of the promoters of capitalist development.

Lenin did not leave the challenge unnoticed. “Recognition of the progressiveness of [capitalist development] is quite compatible ... with the full recognition of the negative and dark sides of capitalism, with the full recognition of the profound and all-round social contradictions which are inevitably inherent in capitalism, and which reveal the historically transient character of this economic regime.” (Lenin 1899b, 596). Hence followed Lenin’s assault on Narodniks who allegedly failed to grasp the real contradictions and problems of the contemporary Russian capitalism just because they outright deny its potential for growth. Skillful as it was, this rhetorical dodge could not resolve the analytical issue of combining the elements in Marx’s approach that accounted for the dynamic nature of capitalism and for its imminent breakdown into a comprehensive model of Russia’s economic development.

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<sup>9</sup> The publications of the two alternative editions of Marx’s *Capital*—by Peter Struve on the “liberal” side and by Bazarov and Skvortsov-Stepanov on the Bolshevik side—can be seen as a continuation of the fight over the right to control the Marxist narrative. Danielson’s editions turned into bibliographical rarity (as well as Struve’s during the Soviet period). On Russia editions of *Capital* see Vasina (2019).

After all, it was Marx himself who (in one of the most read chapters of *Capital*) provided a vision of the initial stage in capitalist development—the original (primitive) accumulation—that forcefully demonstrated that the expropriation of small producers and growing misery were the preconditions for the launch to capitalist accumulation and the subsequent rise of the large-scale industrial production. It was precisely what was happening in Russia, according to Narodnik economists. A conclusion out of that account was that the Russian economic situation at that stage of development, “the shortage of markets,” would inevitably prevent any attempt at capitalist expansion. In spotlight of Narodniks, there were Russian peculiarities, the set of institutions and factors that could allow for overcoming the capitalist impasse. But there was another, even more troubling conclusion. On the same premises, one could argue that it was the peculiarity of the situation in the major Western powers, their struggle for territorial expansion as revealed in the division of a larger part of the world by the end of the nineteenth century, which allowed them a unique leverage to counter the tendency to “general glut” inherent in any capitalist expansion.

To exorcise the ghost of a permanent original accumulation, Lenin attempted to undermine the key thesis of the Narodnik approach: on the lack of market outlets for the products of large-scale capitalist industry engendered by its own growth:

The question of the market is entirely eliminated, because the market is nothing other than the expression of [the] division of labour and commodity production. [...] Once social economy is based on the division of labour and the commodity form of the product, technical progress must inevitably lead to the strengthening and deepening of capitalism. (Lenin [1893] 1937, 108)

Furthermore, there was another important dimension in this line of criticism. Just as “the Russian road” was by no means peculiar as compared to the general trends of economic development, so too was its representation in the Narodnik economics. According to Lenin, Narodniks just reproduced formulas of the utopian petit-bourgeois reaction against the rise of capitalism as exemplified by Sismondi, without his insights and inspiration though (Lenin 1897).

Yet, Lenin evidently was wary that the sheer demonstration of incompatibility between the premises of Marxism and the Narodnik theory of economic development was not enough. The latter argued not for the “impossibility” of capitalism, but for its “impossibility” in Russia. To reformulate in terms of the later structuralist approach, this meant the possibility of emerging the persistent duality in an economic structure of a developing nation: an outward-oriented “capitalist” sector versus an inward-oriented sector of small “commodity production.”<sup>10</sup> The shortage-of-market thesis

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<sup>10</sup> Russian debates on economic development of the 1890s tackled the issue of the duality in economic structure. Narodniks distinguished two heterogeneous sectors: “capitalist”—large-scale industrial production (which, they argued, was constrained by a lack of substantial market outlets abroad and heavily relied upon government support at home); non-capitalist “people’s production”—small-scale communal (co-operative) production of agricultural and manufactured commodities and supply of services oriented towards consumption and local markets. Lenin, as well as other Marxist participants of the debates, admitted the heterogeneity claiming, however, that it was the result of different paces in capitalist development across various regions and sectors, hence co-existence of advanced capitalist enterprises and pre-capitalist forms of production. Lenin’s

employed by the Narodniks economics indicated not only at the restrictions on selling the products of capitalist industry (*the realization problem*), but at a deficient nature of the accumulation process in a backward economy (*the structural problem*).

Once combined in the framework of the Narodnik approach, the trend to constant disproportions between capitalist production and national income and the persistent structural heterogeneity of a developing economic system called for enhancing the potential of “people’s production” (or, to apply a later term, moral economy<sup>11</sup>) in agriculture and handicrafts, instead of pump-priming the large-scale industrial production. But the same combination in the framework of the Marxist approach meant the obstacle for its application to the situation of a developing (or backward) economy.

Lenin clearly perceived the issue when handling a review of Ch. XXI of the 2nd vol. of *Capital* by a fellow Marxist, German Krasin.<sup>12</sup> In his response, Lenin first tackled the realization problem. For this, he elaborated on Marx’s realization schemes, with perhaps main analytical outcome being the notorious “law” on the greater rate of growth in production of capital goods in comparison with that of consumption goods. Redressed as “the law of the priority of heavy industry” it would gain an enormous significance in the context of Soviet industrialization. Lenin himself, however, did not put much emphasis on it:

The whole meaning and significance of this law of the more rapid growth of means of production lies in the one fact that the replacement of hand by machine labour—in general the technical progress that accompanies machine industry—calls for the intense development of the production of coal and iron, those ‘real means of production as means of production’.  
(Lenin [1893] 1937, 105)

Thus, the “law” just expressed the labor-saving character of technical progress under capitalism. As such it explained both the general (or, rather, secular) trend to impoverishment in the course of the capitalist development and the counterbalancing effect of the growth of the markets for capital goods that accounted for the capitalist dynamics (interrupted from time to time by crises due to the disproportionate growth of individual sectors) in a shorter run. This, in turn, was but a manifestation for the basic contradiction of capitalism.

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own contribution to the approach of Russian Marxists consisted in shifting the focus of attention from the pre-capitalist to early capitalist forms: he argued that the bulk of Russian peasantry had already entered the transformation from holders of common lands into small proprietors (proto-bourgeoisie).

<sup>11</sup> In Lenin’s account, “the absurd idea of a mythical ‘people’s system’” (Lenin 1894, 321).

<sup>12</sup> German Krasin was a younger brother to future influential Bolshevik and the Soviet People’s Commissar for Foreign Trade Leonid Krasin. Both brothers participated in a socialist group, where Lenin made his paper presentation (Lenin [1893] 1937), but at that time the elder brother was in exile. After the Revolution (and, it seems, long before it) the younger brother quitted political activity and engaged into engineering, architecture, urban planning (in the Soviet period he would become a Corresponding Member of the Soviet Academy of Architecture). For whatever reason, the editors the Vol. 1 of Lenin’s Collected Works indicated “G.B. Krasin” as the author of the review criticized by Lenin without providing any biographical information on him. This perhaps led to a widespread confusion in attributing the review on Marx’s reproduction schemes to his famous brother L.B. Krasin as is stated, e.g., in the entry on Leonid Krasin in the authoritative *Big Russian Encyclopedia*, see Lukovtseva (2016).

The “law” provides an insight into Lenin’s understanding of what should be the truly Marxism, which he had got from his intense studies of Marx since 1888 and fiercely defended thereafter. But in the context of the Russian debates of the 1890s, it was not particularly original (being undoubtedly stimulated by recent publication of the 2nd volume of *Capital*). After all, Lenin’s was a re-elaboration of what he perceived as an erroneous elaboration presented in Krasin’s review. Lenin closely followed Marx, even though there is a room for debates on the correctness of his interpretation.<sup>13</sup>

The major point of Lenin’s attention was the structural problem. His general conclusion was that the realization problem could be dropped off from the analysis: “The limits of the development of the market, in capitalist society, are set by the limits of the specialisation of social labour. But this specialisation, by its very nature is as infinite as technical developments” (Lenin [1893] 1937, 100). However, “... the explanation of how capitalism develops in general does not in the least help to clear up the question of the ‘possibility’ (and necessity) of the development of capitalism in Russia” (ibid., 89). “The miserable remnants of the natural economy of indigent peasants” indeed posed the methodological problem for application of Marxist theory, based on assumption of “general and exclusive domination of capitalist production” (ibid., 79). Krasin’s approach evidently made Lenin aware of the fact that attempts in interpretation of the Marxist theory in the framework of a backward economic system could hit a breach in the fortress of orthodoxy.

It seems that in Krasin the ghost of the “original accumulation” was let into the process of capitalist accumulation proper.<sup>14</sup> At any rate, he obviously reacted to the Narodnik approach to economic development and, in Lenin’s view, made an inexcusable concession to it. Krasin distinguished:

two essentially different features in the accumulation of capital: (1) the development of capitalist production in breadth, when it takes hold of already existing fields of labour, ousting natural economy and expanding at the latter’s expense; and (2) the development of capitalist production in depth. ... when it expands independently of natural economy, i.e., under the general and exclusive domination of the capitalist mode of production. (quoted in: ibid., 89)

Krasin seemed to admit that the shortage-of-markets thesis was relevant for the first stage, when non-capitalist, external sectors were used as outlets for outward capitalist expansion, while criticizing “the current view” for its neglect of the stage of inward development, when the accumulated potential of the capitalist industry allowed for the growth on its own basis, “independently of any countries with direct

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<sup>13</sup> Cf., e.g. Zarembka (2003). The claim to keeping the orthodoxy on the analytical level, though, was a political asset. For example, it would allow Lenin to assume the self-imposed part of a referee in a theoretical discord between two of the most authoritative representatives of the Russian Marxism of the 1890s (Lenin 1899a).

<sup>14</sup> This hypothesis cannot be proved factually though, as the only available insight into Krasin’s review is provided by selective quotations of Lenin. Another eyewitness who left the written evidence of reading Krasin’s manuscript with Lenin’s notes on it, Nadezhda Krupskaya, mostly confined herself to a comparison of both authors’ handwriting (unfavorable to Krasin). The text itself was most likely lost, see Krupskaya (1933, 11–12).

producers, i.e., independently of so-called foreign markets” (quoted in: *ibid.*, 91). Lenin unequivocally identified “the current view” with the Narodnik approach (*ibid.*). In Krasin’s own words, “if we adhere consistently to the view under examination [i.e., the Narodnik approach], then we must conclude that it is not possible for the development of the capitalist mode of production to become universal” (*ibid.*, 92). If so, capitalist development did require a sort of big push or “implementation” (cf. *ibid.*, 107–108) to expand the sphere of capitalist accumulation outside the enclaves relying “upon consumption in ‘foreign’ markets” (quoted in: *ibid.*, 91).

Krasin put “foreign” in inverted commas as the term meant not only external to the confines of a national economy, but also external to the capitalist sector within a national economy. This was a widely perceived usage of the term in the Russian debates on markets, which indicates that at that time the notion of duality of the national economic system implicitly was present in the debates, even if it was somewhat blurred with the matter of international economic relations. In the framework of the 1890s debates, both external outlets (the non-capitalist domestic sectors and the foreign markets) were regarded as reservoirs for the capital accumulation by the national large-scale industry. Lenin criticized that view. He pointed out that there was no heterogeneity of markets:

... nobody in our literature has yet prophesied the ruin of our handicraft industry because of the disappearance of ‘markets,’ although the handicraft industry produces values totalling over a thousand million rubles and supplies the very same impoverished ‘people’. (*ibid.*, 102)

In this sense, there were no “foreign” markets within the country once the commodity production started to unleash its transformative power. But the same logic suggested that there were no “foreign” markets for the capitalist system on the global scale:

The capitalist enterprise ... inevitably outgrows the bounds of the village community, the local market, the region, and then the state. Since the isolation and seclusion of the states have already been broken down by commodity circulation, the natural trend of every capitalist industry brings it to the necessity of ‘seeking a foreign market.’ [...] This necessity demonstrates the progressive historical work of capitalism, which destroys the age-old isolation and seclusion of systems of economy (and, consequently, the narrowness of intellectual and political life), and which links all countries of the world into a single economic whole. (Lenin 1899b, 66–67)

On the other hand,

the ‘home market’ for capitalism is created by developing capitalism itself, which deepens the social division of labour and resolves the direct producers into capitalists and workers. The degree of the development of the home market is the degree of development of capitalism in the country. (*ibid.*, 69)

Therefore, the issue related to availability of markets for developing capitalist production seemed to be closed. Lenin rejected the idea of non-capitalist sectors serving as the source of surplus value indispensable for accumulation in the capitalist sector. He repudiated Krasin for his readiness (“if only in part”) to allow that:



capitalism did indeed (?), in its infancy, develop in this very easy (sic!?) way (very easy because here existing branches of labour are involved) and is partly developing in the same direction even now (??), since there are still remnants of natural economy in the world, and since the population is growing. (quoted in: Lenin [1893] 1937, 92; notes in parentheses are Lenin's)

Instead, Lenin advanced the universal scheme of economic development, with the stage of achieved technical level as the decisive factor (having in mind the endogenous nature of technical progress):

... it is wrong to divide the development of capitalism into development in breadth and in depth; the entire development proceeds on account of division of labour; there is no 'essential' difference between the two features. Actually, however, the difference between them boils down to different stages of technical progress. In the lower stages of the development of capitalist technique—simple co-operation and manufacture—the production of means of production as means of production does not yet exist: it emerges and attains enormous development only at the higher stage—large-scale machine industry. (ibid., 105 fn.)

This was, according to Lenin, the truly Marxist approach to economic development as confirmed both by Marx's theory and the "facts of life" related to the development of capitalism in Russia. However, precisely those "facts of life" soon induced Lenin to transform it. In 1893, he rejected Krasin's division between the inward ("in depth") and outward ("in breadth") capitalist development. Just few years later, in the *Development of Capitalism in Russia* (written in prison and exile in 1896–99), he surprisingly re-introduced it:

the process of the formation of a market for capitalism has two aspects, namely, the development of capitalism in depth, i.e., the further growth of capitalist agriculture and industry in the given, definite and enclosed territory—and the development of capitalism in breadth, i.e., the extension of the sphere of the capitalist domination to new territory. (Lenin 1899b, 594–595).

Krasin made the distinction in terms of the dual economy. Lenin saw in it the misleading indication to the boundary between the sectors of pre-capitalist and capitalist economy that could eventually prevent the expansion of capitalist production from its original enclaves due to diminishing opportunities to extract profits from "foreign" markets (both the international and the "home market" impoverished by the destruction of the natural economy). Instead, Lenin argued, the natural economy was the cradle for the growth of capitalism. Rather than being somehow "implemented" into the natural economy in form of enclaves, capitalism was engendered by the commodity production transforming the natural economy from within, much like a virus.

However, a closer look into the "fact of life" evidently induced Lenin to move further and to allow the structural problem in, only that the resurged distinction between the principles of inward and outward development got another meaning. Now it indicated not the structural heterogeneity within the boundaries of one economic system but rather the heterogeneity caused by colonialism, forceful imposition of capitalism over the pre-capitalist economic system.<sup>15</sup> Lenin was keen to

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<sup>15</sup> Characteristically, the distinction was introduced in the chapter "The significance of the border regions. Home or foreign market?"

emphasize that it was not simply the question of territorial expansion and hastened to distinguish the political conquest and the economic “conquest” (ibid., 593). Nevertheless, this way—through the addition of colonialism into the original scheme of economic development—the structural problem paved its way into Lenin’s reasoning:

The development of capitalism in depth in the old, long-inhabited territories is retarded because of the colonisation of the outer regions. The solution of the contradictions inherent in, and produced by, capitalism is temporarily postponed because of the fact that capitalism can easily develop in breadth. [...] If Russian capitalism had possessed no range for expansion beyond the bounds of the territory already occupied at the beginning of the post-Reform period, [the] contradiction between capitalist large-scale industry and the archaic institutions in rural life ... would have had to lead quickly to the complete abolition of these institutions, to the complete clearing of the path for agricultural capitalism in Russia. [...] It goes without saying that such a deceleration of the growth of capitalism is equivalent to preparing its even greater extension in the near future. (ibid., 595 fn.)

Without abandoning the original stance on the realization problem, Lenin admitted (albeit implicitly and with reservations) that the “market” was not simply a function of the division of labor and that the availability of “foreign markets” could influence the pace of economic development. However, to prove that this should not be the obstacle preventing capitalist expansion, Lenin explained the strive of capitalist enterprises to move outside the confines of the “home market” by the same disproportions in growth between economic sectors that caused economic crises: “With the unevenness of development inherent in capitalism, one branch of production outstrips the others and strives to transcend the bounds of the old field of economic relations” (ibid., 591).

Thus, the notion of unevenness came to represent the dynamic dimension of the structural disproportions. Rather than exogenously set structural heterogeneity, unevenness was both the result of capitalist development conforming to the theoretical framework of Marxism and a plausible *economic* explanation to the “facts of life” that indicated the persistent divergence in the levels of development across sectors and regions on the national and international levels.

## 4 From Russian to Global Capitalism

At first glance, Lenin himself did a lot to preclude any serious attention to his economic analysis. As other “orthodox Marxists,” Lenin tackled the “notorious question of markets.” But it is striking that in doing so Lenin avoided big issues and big names of the time. While his fellows in application of “the modern theory” to Russia adopted the latest trends, exploring Kantian philosophy, the marginalist critique of Marxism and so on, Lenin dug into *zemstvo* statistics and delivered lengthy, cameralist-style explorations on that. Instead of focusing his critique, for instance, on

Böhm-Bawerk, he pounced on Sismondi.<sup>16</sup> It is hardly an exaggeration to suppose that if not for Lenin's attention, the names of Vladimir Postnikov (Lenin [1893] 1923), German Krasin (Lenin [1893] 1937), Boris Efrusi (Lenin 1897) were absent in the histories of economic thought.

But behind this was a tenacious persistence in achieving goals and an impressive ability to focus on the problem. Lenin possessed *the* theory (or was possessed by it). Lenin's theorizing was always built around the fights with specific designated enemies, only that the concrete enemies changed during his activity. He hardly found it necessary to waste time and efforts on clashes with representatives of the "vulgar bourgeois political economy." His true enemies were always inside Marxism and the socialist movement. At the early stage of his activity, Lenin's target was to crush the opponents to the interpretation of Marxism in Russia which he believed was right. In Postnikov he found the interpretation of agricultural statistics that ran contrary to the mainstream and allowed to set the methodological foundations for his own empirical critique of the Narodnik economics; in Krasin—the warn to fellow Marxists of the danger of wrong interpretations of Marx's theory; in Efrusi—the opportunity to turn a "Romanticist" study in the history of economic thought into the theoretical critique of the Narodnik economics.

Criticizing the opponents, Lenin made his own contribution to the development studies. He rejected the existence of economic obstacles to the expansion of capitalist production. Fighting Narodniks, he insisted that political and institutional "peculiarities" were but temporary impediments to the spread of the virus of commodity production. There is no place for a modernization theory in Lenin. He conceived "industrialization as a transition from one (the underdeveloped) capitalist form to another (the developed). In contrast, most contemporary approaches conceive the initial stages of industrialization as a transition from pre-capitalism to capitalism" (Milios 1999, 17).

Such an approach is open to criticism as an institutionally blind. "Like other Marxists before him, Lenin believed that peasants were a transient social element, soon to disappear before the onslaught of commercial capitalism" (Kingston-Mann 1983, 3). This may lead to the claim that Lenin "ignored" the Russia peasant commune, the *obshchina*, and to the reintroduction of the old claim dated back to the controversy over Marx's reception of Russian debates on *obshchina*: "Marx was correct to have taken the Russian commune seriously, quite a bit more seriously than did Lenin in St. Petersburg in 1893" (Zarembka 2003, 286). The influence of Russian approaches to the issue on Marx is treated elsewhere in this book (see Chap. "Russia-West-Russia: Georg von Charasoff, the "Humane Economy", and the Critique of Marx's Theory of History"). Suffice it to say here that while Lenin was not open to admitting the perseverance of pre-capitalist forms as an essential structural phenomena of Russia's economy, he did not ignore *obshchina*. He tried to dismiss the Narodnik interpretation of its economic and social meaning.

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<sup>16</sup> Lenin characteristically ignored two big articles of Efrusi with the outline of the theories of capital and profit in Rodbertus, Marx, and the Austrian School, which undoubtedly reflected the context defined by Böhm's critique of Marx following the publication of the third volume of *Capital*.

Zarembka's claim also suggests that there is an objective and unbiased snapshot of the situation in Russian agriculture that Lenin "ignored" for whatever reason, while other authors did not. In fact, the "facts of life" used by Lenin were the same as those available to his opponents: sketches of peasant life in the stories of the observers, in literature, and, above all, *zemstvo* statistics. Regarding the latter, Lenin himself hinted to the problem (Lenin 1914). *Zemstvo* statistics was not a solid database collected according to certain established principles. The local statisticians in Russia were often driven in their activity by the movements of soul, not by precise methodology.<sup>17</sup> Lenin understood well that to make use of those data, some theoretical interpretation was needed. Surely, his was an interpretation, and as such it can and should be the subject to critical analysis regarding his methods, etc. (see, e.g., Kotz and Seneta 1990). This interpretation can be, then, confronted with other interpretations (which may be critical or supplementary to it). But Lenin's interpretation (as well as anyone else's) cannot be plausibly dismissed by stating that it had failed to grasp "the true" picture of Russian agriculture supposedly conveyed by a myriad of heterogeneous data. Otherwise, the long due critical reconsideration of the 1890s debates among the Russian radicals boils down to a simple rewrite of the alleged result ("the Marxists won the Narodniks"), rejecting Lenin's analysis as not conforming to Narodnik's vision.

The claim that Russian agriculture had already become capitalist was arguably the most controversial element of Lenin's development studies. But it conformed to his general message: the economic development of Russia was subject to the principles of capitalist accumulation, and the presence of non-capitalist sectors could not alter that path.

Not only Lenin virtually ignored the notion of "original (primitive) capitalist accumulation" that would make its way into the Soviet debates on industrialization. As is shown above, there are reasons to suggest that he rejected it as an analytical tool and based the analysis of development on the notion of capitalist accumulation proper. If this interpretation is correct, Lenin's approach stands in contrast with the big push strategies widespread in the post-WW2 development economics.

Considering the role of market outlets for the capitalist industry, Lenin initially rejected the realization problem and dismissed the structural problem with Krasin's distinction between the principles of inward and outward development. He repeated that the theoretical analysis of the impact of international market was only an unnecessary complication able to add no new insight as compared with the case of closed economy. As his studies progressed, however, Lenin made a concession to recognizing the impact of "foreign" market on a less abstract level. It was conceived as required by the uneven character of capitalist development, with several advanced sectors or enterprises exceeding the average of the national economy in their growth.

Thus, by the end of the early period of Lenin's development studies (and his economic studies in general), he came to the notion of unevenness, which allowed to combine two essentially different frameworks of analysis—the one prescribed by the dynamic scenario that Russian Marxists of the 1890s followed in their clashes

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<sup>17</sup> On the history of the Russian agrarian studies at that period as a narrative see: Kotsonis (1999).

with Narodnik economists; and that suggested by Marx's predictions on the end of capitalism. In application to the specific Russian case, the notion gave Lenin a tool to explain the heterogeneity of the Russian economy not as a structural impediment to capitalist development, but as its direct result. The notion would re-emerge in the context of WWI.

Lenin was always well-adjusted to major cataclysms, and he definitely saw the war as an opportunity rather than a shock or a tragedy. The real shock for him came from what he regarded as another major betrayal within European social democracy after the rise of "revisionism" in the late 1890s: in the first weeks and months of the war, virtually all prominent socialists (to whom Karl Kautsky was the epitome for Lenin) abandoned pre-war internationalism and came to support, one way or another, their national governments. Lenin's stance on historical materialism called for an explanation for Kautsky's "apostasy" on the same grounds as Eduard Bernstein's about two decades earlier.

*Imperialism* is by far the best known piece of Lenin's studies of this period (and, perhaps, of the entire corpus of his economic studies). At the very beginning, Lenin (1917, 195) made explicit references to two major sources of influence: Hobson (1902) and Hilferding ([1910] 1981). There he also pointed to his primary political target: "the ex-Marxist" Kautsky (ibid.), whose concept of "ultra-imperialism" was the subject of Lenin's recurrent harsh critique. Subsequent commentators (even sympathetic to Lenin) usually agreed that *Imperialism* lacked an analytical originality.

It should be noted that *Imperialism* was never intended as a thorough theoretical study and bears the subtitle "A popular outline." However, the task that Lenin set was non-trivial. He did not (and could not) rely upon Hobson and Hilferding in refutation of Kautsky, because their analysis was compatible with Kautsky's conclusions, not Lenin's. Both authors indicated at a major institutional transformation of capitalism that had ceased to function on the basis of free competition and entered the stage of regulation by big entities: monopolies, banks, governments. From there, neatly followed the possibility of an evolutionary or at least relatively peaceful path to socialization, without much need to resort to the revolutionary class struggle. Actually, Lenin (1917, 205) himself embraced the thesis on the historical transformation of capitalism: "Competition becomes transformed into monopoly." With that came "the change from the old type of capitalism, in which free competition predominated, to the new capitalism, in which monopoly reigns ..." (ibid., 218). In Marxian framework, that meant that capitalism had lost its dynamic element and was bound to meet its "natural" end to be facilitated by economic and social reforms.

Lenin had to support "orthodox" Marxist prediction on inevitability of a revolutionary breakdown of capitalism starting with "unorthodox" premise that the free-competition capitalism analyzed by Marx had gone. The idea that capitalism lost its economic potential to expand in the West should have revived in Lenin his early issue with Narodniks over impossibility of capitalism in Russia. Not surprisingly, he once again employed the notion of unevenness:

the uneven and spasmodic development of individual, enterprises, individual branches of industry and individual countries is inevitable under the capitalist system [...] As long as capitalism remains what it is, surplus capital will be utilised not for the purpose of raising the standard of living of the masses in a given country, for this would mean a decline in profits for the capitalists, but for the purpose of increasing profits by exporting capital abroad to the backward countries. (ibid., 241)

On a level of national economy, "... uneven development of individual branches of industry is, therefore, the final cause of crises and capitalism's expansionist tendencies" (Grossman 1932, 31). Already in his works on capitalist development in the 1890s, Lenin pointed to these tendencies as the primary force that pushed capitals toward the search for foreign markets. During the war, the breakup of global economic relations induced many contemporary observers to herald the rise of the closed military economy, including fellow Bolsheviks like Nikolai Bukharin.<sup>18</sup> Lenin (1917, 300) remained unimpressed with this intellectual trend. He continued to stress the dynamic potential of capitalism as a global system:

it would be a mistake to believe that [the] tendency to decay precludes the rapid growth of capitalism. [...] On the whole, capitalism is growing far more rapidly than before; but this growth is not only becoming more and more uneven in general, its unevenness also manifests itself, in particular, in the decay of the countries which are richest in capital (Britain).

Lenin's dynamic vision of capitalism persisted through the revolution. Already in 1919, criticizing Bukharin's proposals on the new party program, Lenin (1919a, 168) stated:

it is one thing to understand and another to act upon this understanding. Comrade Bukharin's concreteness is a bookish description of finance capitalism. In reality we have heterogeneous phenomena to deal with. In every agricultural gubernia there is free competition side by side with monopoly industry. Nowhere in the world has monopoly capitalism existed in a whole series of branches without free competition, nor will it exist. ... If Marx said of manufacture that it was a superstructure on mass small production, imperialism and finance capitalism are a superstructure on the old capitalism. If its top is destroyed, the old capitalism is exposed. To maintain that there is such a thing as integral imperialism without the old capitalism is merely making the wish father to the thought.

Debating the same issue, he reiterated:

It is characteristic of all countries that capitalism is still developing in a great many places. This is true of the whole of Asia, of all countries which are advancing towards bourgeois democracy; it is true of a number of parts of Russia [...] even in Russia, capitalist commodity production is alive, operating, developing and giving rise to a bourgeoisie, in the same way as it does in every capitalist society. (Lenin 1919b, 189)

The "ultimate stage" allowed for the revolutionary breakdown of capitalist system, but its dynamic economic core remained in place. This called for being ever-vigilant and ever-ready to crash the potential for capitalist development by all means of the established proletarian dictatorship.

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<sup>18</sup> Bukharin's ([1915-17] 1929) study on imperialism had preceded Lenin's.

## 5 Conclusions

As well as many “Lenins,” intellectual histories of the twentieth century convey many “Marxes,” not to mention “Marxisms.” Following his political project, Lenin eventually carved one of the most influential interpretations of Marx’s theoretical legacy: Marxism-Leninism.<sup>19</sup> It had its origins in the discursive practices—the rhetoric aimed at ridiculing, denigrating, and suppression of opponents—of the Russian radical intelligentsia, which Lenin did not invent.<sup>20</sup> This was perhaps the main impact of Russian radicalism in his background, rather than the alleged propensity to the acts of terror.<sup>21</sup> Strive to produce, to propagate, to control the discourse (in his case, by means of the “written word”) as the principle tool to achieve political goals made Lenin quite a “post-modern” politician.

But “the living Lenin” was also quite “modern” in his approach to the theory. He himself intentionally did not invent a discourse or forged “fake news.” He believed in the power of the theory over minds and sought to promote the rational knowledge to raise the consciousness of people. That theory—Marxism—is the clue to approaching “the living Lenin,” as it guided his writings, his actions, and undoubtedly his thoughts. But to approach that Lenin, a consensus is required as to what is Marxism. Too often in the Lenin studies, this was the Marxism (or “the real” Marx for that matter) that should have been—the one suitable to a certain context, rather than a Marxism Lenin himself had in mind. There are two different, though by no means self-excluding, questions: Who was “the living Lenin”?; What is “the real” Marxism? They are important each. Perhaps the main problem of the Lenin studies was to mix them up.

This paper assumes that to approach “the living Lenin” it is necessary to understand the theory he was guided by. The main methodological obstacle for this, as it seems, consists in the fact that Lenin himself created that theory. By the 1890s, he most probably had got the revolutionary vision but did not have any ready-made theory at hand to apply it for the case of Russia. Instead, he had picked up some elements, which he fused and on which he elaborated in the course of his political

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<sup>19</sup> In this regard, there is another “notorious question” of the Soviet historiography which I do not enter here: “how much Lenin was there in Stalin?”

<sup>20</sup> Lev Tikhomirov, a prominent Narodnik in the 1870s–80s who eventually abandoned the movement and turned to monarchism, indicated that “in protesting against the rigid official dogma of the regime and its censorship, the radicals had themselves created an intellectual despotism and a censorship—directed against all those who did not wish to pay tribute to the gods of revolution” (Tidmarsh 1961, 48). It is debatable whether this pattern can be applied exclusively to Russian radicalism, though. Marx was not immune to that practice either. Russian landlord and author Pavel Annenkov, who met Marx in 1846, was appalled by the mode of debates led by him, which was quite opposite to the gentlemanly rules of debates adopted among the Russian intellectuals of his circle then (cf. Tvardovskaya and Itenberg 2009, 12–14).

<sup>21</sup> In fact, in the pre-revolutionary period, Lenin rejected terrorism as a futile political tool. Terrorist activity was associated more with some groups within the Socialist Revolutionaries (the *Esers*), the political wing of the Narodniks. Bolsheviks indeed implemented “the state of terror” soon after the takeover. But the search for their theoretical inspirations in that arguably should be started with the interpretation of Marx’s *Civil War in France* that framed their reading of the histories of French revolutions.

battles. The context of the 1890s channeled his early theoretical activity to application of Marx's theory of capitalist accumulation to contemporary Russia's situation. He rejected the approach that the early (transitory) stage of capitalist development required external sources to pump-prime the accumulation process inside the capitalist sector. Lenin ascribed this approach to the Narodniks and to the "Economic Romanticism" traced back to the early nineteenth century. The foundations to that approach were found by him in the reaction of small proprietors endangered by the rise of the large-scale capitalist industry.

At the same time, Narodnik economists found some validation to their arguments on lack of resources for capitalist accumulation in Russia in Marx's depiction of the effects of capitalist accumulation in Britain and elsewhere. Furthermore, Lenin evidently perceived that Marx's outline of the original (primitive) accumulation was not incompatible with the claim on peculiarity of the development principles at a distinctive transitory stage between the state of economic backwardness and the launch to the modern ("capitalist") economic growth. Lenin, instead, insisted on the universality of the development pattern "natural economy—commodity production—capitalist production." As I tried to show above, to provide an analytical substantiation to this thesis Lenin implicitly distinguished *the realization problem*—the availability of market outlets affecting the pace and the very possibility of the steady growth of the capitalist sector; and *the structural problem*—the possibility for the heterogeneity in internal economic system and the situation on the global market to alter the development path of a backward economy. Initially, he rejected both. In the course of his studies, presumably under the influence of empirical data, Lenin, while maintaining the initial stance on the first, made some concession to the second. But in Lenin, the coexistence of heterogeneous economic sectors and regions on the national and international levels—was not the original (primitive) obstacle to be overcome in the course of subsequent capitalist development. Rather, it was the effect of unevenness in capitalist development.

The earliest period of Lenin's intellectual activity is remarkable as it was focused almost entirely on the matters of economic theory. Lenin obviously never lost the economic part of Marxist doctrine from his sight. But it was not until 1915–16 when he again began focusing his attention on those matters—and, again, in the field of economic development. In his studies of imperialism, Lenin extended (with some transformation) the framework he set up for the case of Russia to the case of the global economy.

In 1893–99, Lenin followed the dynamic scenario of capitalist development assuming limitless economic potential for the expansion of the capitalist sector in Russia. In 1915–16, he explored the "ultimate" stage in capitalist development on the global scale, which was outlined by Kautsky, Hilferding, other Western socialists. Even if at that period Lenin came closer to the breakdown scenario, he never fully embraced it. Using the notion of unevenness first employed by him in the late 1890s, he tried to combine the two scenarios. In both periods of his development studies, Lenin admitted only the political breakdown of capitalism and called for catching each and every social expression of economic contradictions inherent to capitalism to smash its state apparatus. After the revolution, his vision of the dynamic capitalist



potential suggested the perseverance of the virus of commodity production despite the political takeover (specifically, in the agricultural sector) and its highly contaminating nature capable of transforming the socialist system from within. This was by no means the only element in substantiation of the proletarian dictatorship. But this was an influential factor behind the subsequent rise of Soviet development model integrated into Soviet political system.

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# Russia-West-Russia: Georg von Charasoff, the “Humane Economy”, and the Critique of Marx’s Theory of History



Christian Gehrke

*I therefore already in advance do not expect too much success from my attempt to develop Marx’s, or more generally, the classical economic theory in a positive direction and to round it off. A purely destructive “critical” work could certainly reckon on more recognition, and this the more so, the fewer original ideas it contained and the closer it fell in line with the already familiar critical works.*  
(Charasoff 1910: vii)

## 1 Introduction

It is not immediately obvious how the remarkable scholar whose ideas will be discussed in this essay could possibly be introduced so as to fit neatly into a volume entitled “Russian and Western Economic Thought: Mutual Influences and Transfer of Ideas”. Georgy Artemyevich Kharazov, who was born in 1877 in Tiflis, the Georgian capital, regarded himself as a “Russified Armenian”—not as a Russian. Moreover, he was not an economist but a self-taught private scholar with an academic background in mathematics and physics. Still more importantly, he can neither be said to have significantly influenced the economic discourse in Russia by the import of Western ideas nor to have contributed to the development of economic theory in the West by the transfer of economic ideas deriving from Russian traditions. This is so because the innovative concepts and original ideas he presented in *Karl Marx über die menschliche und kapitalistische Wirtschaft* (1909) and *Das System des*

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*Marxismus: Darstellung und Kritik* (1910)<sup>1</sup> were not recognized as such and consequently made no impact on contemporary developments. Charasoff's contributions to Marx's economic theory and its critique in fact had to wait until the early 1980s in order to be properly appreciated.

The present chapter informs about Charasoff's economic work, about some important elements in its formation, and about its reception in the West and in Russia. It opens with a brief account of his life and then turns to the novel concepts and analytical tools that he developed for the determination of relative prices and the general rate of profits. Next, his unorthodox interpretation of the role of the labour theory of value in Marx's theoretical construction is explained and related to the contemporary debates on Marx's economic theory in Germany and Russia. By recalling the extraordinary story of his life, by reconstructing some of the intellectual sources and formative elements of his work in economic theory, and by further situating the latter in the contemporary economic discourse in Russia and the West, the chapter seeks to provide an explanation for the *neglect of original economic ideas* and consequently also for their *non-transfer* across national borders.

## 2 From Russia to the West: Charasoff's Life and Some Formative Elements of His Work

Georgy Artemyevich Kharazov was born on 24 June 1877 in Tiflis, Georgia, into a wealthy family of Armenian origin. His father was a count and state councillor, who owned factories and landed estates in Armenia and Georgia. Georgij, who appears to have been the only son, attended the classical gymnasium in Tiflis from 1886 to 1890, but after his father's early death was transferred to a boarding school in Odessa. He returned to his hometown in 1893, where he passed the final exam as an external pupil in the following year. He then enrolled at the University of Moscow as a student of medicine, but was expelled during the students' protests of 1896 and forced to leave the Russian Empire in order to continue his studies. Kharazov went to Germany, where he enrolled at the University of Heidelberg, changing his name to Georg von Charasoff and his study fields to mathematics and physics. He finished his studies in February 1902 with a doctoral dissertation in mathematics, entitled "Arithmetische Untersuchungen über Irreduktibilität" (1902). It was a slightly revised version of a paper that had won him a prize essay competition in the previous year. The University of Heidelberg had an excellent reputation, in particular in law, medicine, and the natural sciences, and attracted considerable numbers of students from Russia, often with an aristocratic background and sympathetic to some form or

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<sup>1</sup> In the preface to his second book, Charasoff informed his readers that he intended to supplement the two books with a third one, which was to bear the title "Die Probleme der Produktion und der Verteilung" {The problems of production and distribution} and which was to contain "a thorough criticism of the subjective theory of value" (Charasoff 1910: xiv, n.). Alas, this book was never published.

other of socialist/revolutionary ideas. In Heidelberg, Charasoff temporarily shared the same address as the social revolutionaries Abram Gots, Vladimir Zenzinov and Ilya Fondaminsky, and he developed friendships with the brothers Vadim and Michail Reissner,<sup>2</sup> and in particular with Otto Buek, a student of chemistry, mathematics, and philosophy from St. Petersburg. With Buek he continued to meet and correspond also after the latter had moved to Marburg in 1899, where he wrote his doctoral dissertation under the neo-Kantian philosophers Hermann Cohen and Paul Natorp. In Marburg, Buek actively participated in a neo-Kantian socialist-anarchist group that had formed around Cohen and Natorp and to which also Robert Michels and Kurt Eisner belonged. At the time, all three sympathized with syndicalism and anarchism, drawing inspiration in particular from Tolstoy's works.<sup>3</sup> In 1904, Buek moved to Berlin, where he led a bohemian life and earned his living as an editor, journalist, and translator. Buek was well connected in Berlin's intellectual, artistic, and literary circles. Several of his friends were left-wing political activists or anarchists, but he was close also to the pacifist Georg Friedrich Nicolai and for a time used to meet regularly with Albert Einstein in order to discuss philosophical problems with him and to play music together. In 1914, Buek was one of the four signees of Nicolai's anti-war pamphlet "Aufruf an die Europäer!", together with Albert Einstein and Friedrich Wilhelm Förster. In 1933, upon the rise of Nazism, he emigrated to Paris. He died, impoverished, and isolated, in a home for the elderly near Paris in 1966.

In 1905 and 1906, Buek visited Charasoff in Zurich, where the latter had meanwhile settled down, *en route* to a stay in the famous *Monte verità* commune in Ascona, where he attended an international gathering of anarchists and free thinkers. In the summer of 1907, the two of them again spent some time together in a luxurious sanatorium at Lake Constance. The friendship with Buek was important for Charasoff not only because he could discuss his ideas on Marx's economic theory with him, but also because Buek translated his manuscripts (from Russian into German) and helped him with securing a publishing contract for his German books.<sup>4</sup> Moreover, Buek was later responsible also for the (unauthorized) re-publication of excerpts from his books in the literary-political journals *Die Aktion* and *Der Gegner* in 1918 and 1920/21, when Charasoff had already returned to his hometown and become a

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<sup>2</sup> Michail Reissner studied law at the University of Heidelberg in 1897–98. He later became a professor at the law faculty of Petrograd University in 1917, and was involved in the drafting of the first constitutional law of the Soviet Union. In the 1920s, as a member of the Soviet Ministry of Sciences and Education, he was responsible for the foundation of the "Communist Academy" in Moscow, which became a centre for Marxist social sciences. He was also a founding member of the Russian Psychoanalytical Society in Moscow, where Georg Charasoff delivered two invited lectures in the 1920s.

<sup>3</sup> Buek's article on "Leo Tolstoy" (1905) was one of the first examples for the application of neo-Kantian ideas to specifically Russian themes. Otto Buek can therefore be considered as one of the founders of "Russian neo-Kantianism" (Dmitrieva 2016). On Buek's role in the development of an anarchist-socialist variant of neo-Kantianism, merging the neo-Kantian ideas of the "Marburg school" with Tolstoy's pacifism and anti-modernism; see Hanke (1993), Sieg (1994), and Dmitrieva (2007).

<sup>4</sup> The publisher of Charasoff's books, Hans Bondy, belonged to Otto Buek's circle of friends in Berlin, which included many (now) well-known artists, writers, publishers, and intellectuals.

major contributor to the extraordinary flowering of literary and artistic life that had sprung up in Tiflis during the pre- and post-revolutionary turmoil in the period from 1917 to 1921.<sup>5</sup>

After the completion of his dissertation, as already mentioned, Charasoff had moved to Zurich, where he led the life of an independent private scholar. On the income from his inherited wealth he could live comfortably with his first wife, Marie von Charasoff, neé Seldovic, and his three children, born in 1900, 1902, and 1903,<sup>6</sup> and freely pursue his multifarious intellectual interests. He persisted in these pursuits also after his wife's early death, which seems to have occurred around 1904/05. In the beginning, and at least until mid-1905, he continued to work on problems in pure mathematics. This can be inferred from some manuscripts he sent to David Hilbert in 1904, and from the fact that he enrolled for courses of mathematics at the University of Zurich in the summer term 1905.<sup>7</sup> Another major concern of his, until at least 1907 if not beyond, was a serious study of Tolstoy's writings and ideas. This emerges from a number of letters that he wrote, between 1902 and 1908, to Vladimir Chertkov, the major representative of the Tolstoyan movement in the West, who was then living in exile in England. The extant correspondence<sup>8</sup> opens with a letter of 1902 to Anna Chertkova, in which Charasoff informs her about an article on the exposition of the Marxian system that he intends to contribute to issue no. 4 of *Svobodnoe Slovo*, the magazine published by Chertkov in Britain from 1901 to 1905. In the same letter, he announced a further submission, as a separate article for a subsequent issue, on the critique of the Marxian system. But Chertkov, to whom the first article was passed on by his wife, advised him to further elaborate on it and proposed to send in the meantime some reviews or rebuttals of German publications on Tolstoy's ideas. In 1903, Charasoff indeed prepared some critical reviews of some German and French publications on Tolstoy (including one by Vladimir Posse)—which Chertkov, however, seems not to have published. In one of his letters of 1903, Charasoff then referred to a "book on political economy", which is said to be "almost finished", but when Chertkov invited him to send the manuscript for possible publication (in several instalments) in his magazine, Charasoff repeatedly postponed the delivery. The last extant letter from this period is a Christmas card from December 1903, with no further mention of this project. As his correspondence with Chertkov shows, Charasoff pursued the idea of a publication on the Marxian system and its critique

<sup>5</sup> Franz Pfemfert, the editor of *Die Aktion*, was a friend of Otto Buek; for the excerpts from Charasoff's books published in these journals, see Charasoff (1918a, b, c, d, e, 1920, 1921).

<sup>6</sup> Alexander (b. 1900 in Strasbourg) was the illegitimate child of Marie Seldovic's sister, Anna Helena Seldovic, and apparently was adopted by von Charasoff. Arthur (b. 1902 in Zurich) and Helene (b. 1903 in Zurich) were legitimate children of Georg and Marie von Charasoff.

<sup>7</sup> Charasoff enrolled for the courses of Professor Burkhardt, who at this very time was one of the examiners of Albert Einstein's doctoral dissertation.

<sup>8</sup> Charasoff's letters to Chertkov and his wife are preserved in the Chertkov Papers at the Russian State Archive of Literature and Art (RGALI) in Moscow. Charasoff seems to have met Chertkova earlier and to have corresponded with her before. He also must have received an invitation to visit the couple in England, because in his first letter Charasoff declined this invitation on the ground that he had to edit the proofs of his doctoral thesis, which was about to be printed.

already immediately after he had completed his doctorate in Heidelberg, and his original plan appears to have been for a publication in Russian.

Before we discuss the renewal of his correspondence with Chertkov in July 1907, a further intellectual fascination of Charasoff in this period deserves to be mentioned: he apparently immersed himself quite deeply also into the study of the works of Sigmund Freud and C. G. Jung, and in the summer term 1907, he also attended lectures on psychoanalysis by Prof. Eugen Bleuler at the University of Zurich.

Charasoff's preoccupation with Marxism and his intensive study of Tolstoy's ideas may well have been related to a fascination with anarchism, but (unlike Buek's) it involved no renunciation of a bourgeois lifestyle. During his residence in Zurich, Dr. Charasoff was known as "a very rich man",<sup>9</sup> who lived with his family in fairly generous rented apartments, always close to the University library and the "Russian quarters". As in Heidelberg, he was certainly in touch with other Russians in Zurich as well, but very little is known about his personal contacts. At the turn of the twentieth century, large numbers of students from the Russian empire had come to Switzerland, not least because it was one of the first countries to admit also female students. The Russian community in Zurich had a rather mixed (and partly overlapping) composition: it consisted primarily of students and academics, but there was also a considerable Jewish group (often engaged in commercial activities), as well as some exiled political activists of the various fractions of the Russian revolutionary movement. Charasoff seems not to have been affiliated with any particular political group, but in a letter to Chertkov of 1907 (and in his 1909 book), he reports to have lectured on Marx's economic theory and its critique to an audience of some forty listeners over a course of three years. In 1904, Charasoff is also reported to have made a critical intervention at a lecture delivered by Georgy Plekhanov in Zurich on "Scientific socialism and religion".<sup>10</sup> He probably knew personally also Pavel Axelrod, the exiled leader of the Russian Social Democrats: from 1912 to 1915, they lived in the same neighbourhood and could hardly have avoided meeting each other on the street that leads up to their apartments. We also know that during a cure treatment at Lake Constance, in the summer of 1907, he met Vera Figner and Leonid Shishko, the prominent *Narodnaya Volya* activists, as well as Lidiya Petrovna Kochetkova, a member of the Russian Socialist-Revolutionary Party.

Some more information can be provided on Charasoff's studies of economic theory and the gestation of his books. In the Foreword of his second book he noted that he had developed the concepts of "production equations" and "production series" from his intensive study of Marx's writings long before he had first read the contributions of Menger, Böhm-Bawerk, and Walras around 1904/05 (1910: xiv). If this is true, he must have developed the central concepts of his two books already during his student years in Heidelberg. On the actual composition of his two books, however, he appears to have embarked only in the summer or autumn of 1907, after he had changed his original publication plans. The latter seem to have consisted of two closely connected, but separate projects for the presentation of his ideas. There was,

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<sup>9</sup> See "Vormundschaftsakten Kinder Charasoff" (Stadtarchiv Zürich).

<sup>10</sup> See Plekhanov (1976: 56–63).

on the one hand, a plan to publish an article on the critique of Marx's economic theory in the theoretical journal of the German Social Democratic Party, *Die Neue Zeit*. However, Charasoff's article, translated and submitted on behalf of his friend by Otto Buek to the editor, Karl Kautsky, in January 1907, met with a desk rejection. Neither the manuscript and submission letter nor Kautsky's rejection letter are extant, but a letter of Charasoff to Kautsky of 7 February 1907 has been preserved in the Kautsky Papers.<sup>11</sup> In this letter, Charasoff informed Kautsky that he did not consider the reasons which the latter had given for his rejection convincing and that he was keen to read a more detailed criticism of his views.<sup>12</sup> Since the manuscript seems to be lost, it is unclear which concepts and ideas from his later books were already contained in it. The article would in any case have been published—had it been accepted by Kautsky—almost simultaneously with Ladislaus von Bortkiewicz's famous two papers on Marx's economic theory.<sup>13</sup> On the other hand, Charasoff also pursued the idea of publishing a book on political economy in Russia(n) with the help of Vladimir Chertkov. Shortly after he had received Kautsky's rejection letter, Charasoff in July 1907 renewed his earlier correspondence with Chertkov and returned to the issue of the book again, on which he had worked "for almost four or five years", and which (again) is said to be "about to be finished". He acknowledged Chertkov's influence behind the project and asked for his help in finding a publisher in Russia. In his next letter, Charasoff admitted that the book would be "too scholarly" for a general public. At one instance though, being obviously in a good mood due to the perceived success of the lectures he gave along the lines of the book, he is quite positive and adds that it can be popular and would require just a bit of elementary algebra to be digested. Charasoff thus originally intended a book publication in Russia, with the support of Chertkov, who (again) invited him to send his manuscript. After repeated postponements, Charasoff eventually sent a draft version of the first four chapters (according to the indication in the file's cover, in December 1907, but the letter itself is undated), mentioning that the other parts were needed for further elaboration and that "a friend of mine" (no doubt a reference to Otto Buek) is currently preparing a German translation. Interestingly, Chertkov in his reply proposed that the chapters may be shown to Leo Tolstoy—but whether this actually came about is not clear. In his cover letter, Charasoff chose to highlight the main points behind his project. According to him, the aim was to provide nothing less than a new basis for political economy, a basis which he considered to be compatible, and indeed to be fully in line, with Tolstoy's moral teaching.<sup>14</sup> In 1908, Charasoff continued to work on

<sup>11</sup> See Kautsky Papers (Correspondence D VII 66, International Institute of Social History, Amsterdam).

<sup>12</sup> For a more detailed discussion of the Charasoff-Kautsky correspondence, see Mori (2007).

<sup>13</sup> See Bortkiewicz (1906–07, 1907). Bortkiewicz's contributions are discussed by Gehrke and Kurz (2022b, this volume). For English translations of the two papers, see Bortkiewicz ([1907] 1949; [1906–7] 1952).

<sup>14</sup> A more detailed account of the contents of Charasoff's "Chertkov manuscript" and a comparison with the contents of his German books of 1909 and 1910 is beyond the scope of this paper. It is planned to provide such an account, as well as additional information on the Charasoff–Chertkov correspondence, in a joint project of the author with Denis Melnik (HSE Moscow).



his economic manuscripts and eventually must have decided to prepare a publication in German only, to divide the material into three books (of which, however, in the end only the first two materialized), and to abstain from referring explicitly to Tolstoy.<sup>15</sup> The Preface of the first book, *Karl Marx über die menschliche und kapitalistische Wirtschaft*, was completed on “12 October 1908” (Charasoff 1909: page not numbered), and the book was published in February 1909.

For the writing of the second book, Charasoff moved to Clarens at Lake Geneva in February 1909, together with his three children and his future second wife, Marie Kriegshaber, a young Russian medical student with a recent doctorate in gynaecology from the University of Zurich. In August 1909, the Charasoff–Kriegshaber family then moved on to Lausanne, where the couple got married in October. In February 1909, shortly after his arrival in Clarens, Charasoff sent a copy of his first book to Karl Kautsky, together with the manuscript of an article that he submitted for publication in *Die Neue Zeit*. Only one week later, he sent another letter, in reply to Kautsky’s desk rejection, where he noted that although he was “no Marxist in the conventional sense of the term” he was “not ready to concede so easily ... that my construction should be fundamentally wrong”, and that he was awaiting “with great interest” Kautsky’s explanation for the rejection.<sup>16</sup>

The main ideas of the 1910 book had certainly been worked out before, but its actual composition and, in particular, the writing of its final part, where Charasoff repeatedly refers to the recently published book of Boudin ([1907] 1909), was clearly carried out only in 1909 in Clarens. There, Charasoff certainly met with other Russians. In the nineteenth and early twentieth century, this part of the “Swiss Riviera” was extremely popular with Russian artists and intellectuals, such as Tolstoy, Tchaikovsky, or Nabokov, and the hotels and restaurants in the Montreux area were continuously filled with (wealthy) Russian guests. Moreover, Charasoff may well have been in contact also with Russian revolutionaries during his stay in Clarens. His rented apartment was within walking distance of the famous Russian library of Nicolai Rubakin, which attracted many exiled social revolutionaries, who temporarily took residence in the nearby village of Baugy-sur-Clarens. The Foreword, and probably also the book, was finished on “24 December 1909” in Lausanne (1910: xxvii). Three months later, Marie von Charasoff gave birth to a son. The book was published at about the same time, and in September 1910 the family returned to Zurich.

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<sup>15</sup> Charasoff’s extant correspondence with Chertkov, who in July 1908 left the Tolstoy colony in England and returned to Russia, breaks off without providing any information on the reasons for the collapse of the Russian book project.

<sup>16</sup> See Kautsky Papers (Correspondence D VII 67–8, International Institute of Social History, Amsterdam).

### 3 Charasoff's Innovative Concepts and Original Findings

Charasoff was one of the first economic theorists to recognize that prices of production and the general rate of profits can be determined by the eigenvector and eigenvalue of the (augmented) input coefficients matrix, respectively. He not only anticipated most of the arguments that were proposed later in the discussion of Marx's "transformation problem", but also noted the duality properties of the price and quantity system, a finding that is usually associated with the seminal paper of von Neumann ([1937] 1945–6). Moreover, in the course of his investigation he defined and made use of the concepts of a "production series" {Produktionsreihe}, "original capital" {Urkapital}, and "basic products" {Grundprodukte}, thus anticipating Sraffa (1960) with regard to the related concepts of a "reduction series to dated quantities of labour", the standard commodity, and the basics/non-basics distinction. In addition, Charasoff also anticipated the so-called Fundamental Marxian theorem of Morishima (1973) and the theorem of the rising rate of profit from the introduction of technical progress, that is, the so-called Okishio theorem, which is generally attributed to Okishio (1961), but is already to be found in Sraffa (1960). Although Charasoff's analytical argument was undoubtedly based on mathematical reasoning, he chose to present it in non-mathematical form, using only simple arithmetical examples in order to illustrate his findings. From a mathematical point of view, it is remarkable that Charasoff failed to make use of (and in spite of a number of shared mathematical interests apparently was unfamiliar with) the recently developed theorems of Perron and Frobenius on eigenvalues and eigenvectors of positive and non-negative matrices (Parys 2014). However, according to Mori (2013), Charasoff in some of his argument in fact anticipated the solution method of the so-called von Mises iteration, which was introduced and further developed by the mathematician Richard von Mises in the 1920s.

Since Charasoff's findings on the determination of production prices and the rate of profits partly resemble results obtained already a decade earlier by Dmitriev ([1898] 1974), it deserves to be noted that there is no indication that Charasoff had read Dmitriev's 1898 essay on Ricardo's theory of value, although he refers to the famous paper by Ladislaus von Bortkiewicz of 1906–07 (which contains a reference to Dmitriev). At any rate, Charasoff submitted his first economic manuscript to Kautsky (which is not extant, but which seems to have contained his main findings with regard to these problems) already in January 1907, so that he can be supposed to have arrived at his findings *before* he was able to study Bortkiewicz's papers.

As opposed to Bortkiewicz (1906/07), who followed Dmitriev in adopting an Austrian production model,<sup>17</sup> Charasoff developed his main argument within the framework of an interdependent system of production, which exhibits all the properties of the later input–output model and is fully specified in terms of the amounts of material inputs and labour needed per unit of output. The central concept of

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<sup>17</sup> An "Austrian" production model exhibits a finite number of production stages and presupposes the non-existence of basic commodities. See the chapter on Dmitriev in this volume (Gehrke and Kurz (2022a, this volume).

Charasoff's analysis is that of a "production series" (*Produktionsreihe*): it consists of a sequence, starting with an arbitrary (semi-positive) net output vector (where net output is defined exclusive of wage goods), followed by the vector of the means of production and the means of subsistence in the support of workers needed to produce this net output vector, then the vector of the means of production and the means of subsistence needed to produce the previous vector of inputs, and so on. Charasoff called the first input vector "capital of the first degree" (*Kapital erster Ordnung*), the second input vector "capital of the second degree" (*Kapital zweiter Ordnung*), and so on. This series "has the remarkable property that each element of it is both the product of the following and the capital of the preceding element. Its investigation is indispensable to the study of all the theoretical questions in political economy" (Charasoff 1910: 120).<sup>18</sup>

Let  $y$  denote the  $n$ -dimensional vector of net outputs and  $A$  the  $n \times n$  matrix of augmented input coefficients, i.e. each coefficient represents the sum of the respective material and wage good inputs per unit of output (in reckoning the wages as capital advances Charasoff followed the classical economists and Marx). Then the "production series" is given by

$$y^T, y^T A, y^T A^2, \dots, y^T A^k, \dots, y^T A^\infty$$

With circular production relations this series is infinite. Tracing it backwards, first all commodities that are luxury goods disappear from view; next, all commodities that are specific means of production in the production of luxuries; then, the specific means of production needed to produce those means of production, etc.. On the implicit assumption (which Charasoff omitted to make explicit) that none of the commodities mentioned enters into its own production, "it is clear that from a certain finite point onwards no further exclusions have to be made, and all the remaining elements of the series of production will always be made up of the self-same means of production, which in the final instance are indispensable in the production of all the different products and which therefore will be called *basic products* {*Grundprodukte*}". Charasoff added: "The whole problem of price boils down ... to the determination of the prices of these basic products. Once they are known, the prices of the means of production used in the production of luxuries, and finally also the prices of the latter, can be derived." (1910: 120–121).

The mathematician then pointed out that a further property of the "production series" deserves to be stressed. The "capital of the second degree" ( $y^T A^2$ ) is obtained

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<sup>18</sup> In the Preface of his second book Charasoff emphasized: "With regard to the theory of the production equations and the production series developed by me I would like to point out that in this regard priority claims could be made for the contributions of K. Menger, Böhm Bawerk, Walras and others. I nevertheless regard the entire theory of production stages as my own, since I developed it entirely on my own already several years ago (when I had not yet any knowledge at all of the similar theories of the founders of the subjective method)" (1910: xiv). It needs to be stressed that Charasoff, unlike Menger and Böhm-Bawerk, developed his analysis on the basis of an interdependent production system with circular production relations and thus with an *infinite* number of "production stages".

by multiplying the “capital of the first degree” ( $\mathbf{y}^T \mathbf{A}$ ) by  $\mathbf{A}$ : “Yet since the physical composition of a sum of capitals is obviously always a medium between the physical compositions of the summands, it follows that the capitals of the second degree deviate from one another to a smaller extent than is the case with the capitals of the first degree” (1910: 123; original emphasis). Accordingly, the further backwards one goes in the series, the more equal the compositions of the capitals become, i.e. capitals of a sufficiently high degree “may practically be seen as different quantities of one and the same capital: the *original* or *primary capital* {*Urkapital*}”. As Charasoff pointed out, this finding is of the utmost importance for determining the general rate of profits, because it can be shown that “this original type, to which all capitals of lower degree converge, possesses the property of growing in the course of the process of production without any qualitative change, and that *the rate of its growth gives the general rate of profit*” (1910: 124; emphasis added). The rate of profit can thus be ascertained in terms of relating to one another two quantities of the same composite commodity: the “original capital”.

Let  $\mathbf{u}$  designate the  $n$ -dimensional vector of an elementary unit of the “original capital”,  $\mathbf{u} \geq 0$ , then  $\mathbf{u}^T \mathbf{A}$  is the capital corresponding to  $\mathbf{u}^T$ , and we have

$$\mathbf{u}^T = (1 + r)\mathbf{u}^T \mathbf{A}$$

with  $r$  as the general rate of profit. Von Charasoff emphasized: “The original capital expresses the idea of a surplus-value yielding, growing capital in its purest form, and the rate of its growth appears in fact as the general capitalist profit rate” (1910: 112). These considerations also provide the key to a solution of the problem of price. For, if the various capitals can be conceived “as different amounts of the self-same capital ...”, then prices must be proportional to the dimensions of these, and the problem of price thus finds its solution in this law-like relationship” (1910: 123). Let  $\mathbf{p}$  designate the  $n$ -dimensional vector of prices,  $\mathbf{p} \geq 0$ , then we have the following system of price equations:

$$\mathbf{p} = (1 + r)\mathbf{A}\mathbf{p}$$

Thus, while  $\mathbf{u}$  equals the left-hand eigenvector of  $\mathbf{A}$ ,  $\mathbf{p}$  is equal to its right-hand eigenvector; and  $1/(1 + r)$  is the dominant eigenvalue of the augmented input matrix  $\mathbf{A}$ . The solution to the price problem can therefore be cast in a form in which “the notion of labour is almost entirely by-passed” (1910: 112). Given the technical conditions of production and the real wage rate, the general rate of profits and the prices of commodities can be determined without having recourse to labour values.

However, Charasoff by no means concluded from this finding that the labour theory of value is redundant and can be dispensed with. In his understanding, the main task of political economy consists in providing a theory of capitalist development, with technical progress as a main driving force. The classical economists, that is, Smith, Ricardo and Marx, are credited with having put forward such a theory and with having forged instruments for investigating this problem. Charasoff motivated their emphasis on labour values in terms of the fact that human progress must involve

a (partial) liberation of mankind from the struggle for the procurement of its material needs. For Smith, Ricardo, and Marx, the historical progression of the human race must therefore inevitably involve technical change that is associated with a saving of human labour time:

This is the fundamental law underlying the classical law of value. By abandoning this objective basis of the determination of value, the modern subjective theorists have relinquished the only possible explanation of technical progress, and therefore they are without any theory of the technical development of the capitalist economy. (Charasoff 1910: xv–xvi)

#### 4 Situating Charasoff's Contributions in the Contemporary Economic Discourse

Charasoff clearly intended his books as an intervention to the then ongoing debates on Marx's economic theory in Germany. These concerned mainly the relationship between volumes I and III of *Capital* or the so-called transformation problem (of values into prices of production and of surplus value into profit); Marx's law of the tendency of the rate of profits to fall in vol. III of *Capital*; and Marx's theory of history and the breakdown of capitalism (including his crises theory). Charasoff was interested mainly in the latter two sets of problems, and he regarded the correct solution of the "transformation problem", though of course indispensable for a proper treatment of those problems, to be only of limited interest in itself.<sup>19</sup>

The main works he cited, and commented on in more detail (apart from Marx's *Capital*, of course),<sup>20</sup> are Kautsky's *Karl Marx's ökonomische Lehren* ([1887] 1908), Tugan-Baranovsky's *Theoretische Grundlagen des Marxismus* (1905), and, in the final part of his 1910 book, Boudin's *Das theoretische System von Karl Marx* ([1907] 1909).<sup>21</sup> Kautsky and Boudin served as Charasoff's scapegoats for presenting, and then attacking, the position of the orthodox Marxists, while Tugan-Baranovsky is introduced as an "important political economist and former Marxist", who sees in Marx's law of value "only the disguised demand for the whole product of labour", and who has turned his back on Marx for the wrong reasons (1909: 26, emphasis added). By arguing that prices are determined merely "by custom and tradition" without any

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<sup>19</sup> See his remarks in Charasoff (1909: 65–67), where he relegates the demonstration of his method of price determination to an appendix.

<sup>20</sup> There are brief mentions also of other contemporary authors and their views on specific points, including Oppenheimer, Brentano, Eckstein, Hilferding, Lexis, Liefmann, Schmidt, and Sombart, but none of them plays an important role.

<sup>21</sup> Boudin (Boudianoff), was a Russian-born lawyer and Communist activist based in New York City. The German edition of his *The Theoretical System of Karl Marx in the Light of Recent Criticism* (1907), translated by Luise Kautsky, was published in spring 1909. By attacking Boudin, Charasoff clearly meant to criticize also the views of Karl Kautsky, who had written a favourable foreword for the German translation.

recourse to values, and that the origin of profit is sufficiently explained by the existence of a physical net output, Tugan-Baranovsky had shown his incomprehension of the role of the labour theory of value in Marx's theoretical construction<sup>22</sup>:

As regards profit, Tugan argues that its existence can be explained without any surplus value theory by considering that in the production process the mass of iron, corn, coal and so on that can be commanded by society is enlarged and thus a surplus is generated. As one can see, Tugan-Baranovsky firstly confuses the surplus product, that is, quantities of material goods, with the profit, a pure number. Secondly, he seems to be convinced that the surplus product must always exist, even when the economy is guided by wrong economic ideas: He assumes that prices are independent of values and simply determined by "custom and tradition". ... What fruits can such a critique of Marx bear? Only unripe ones certainly, if one does not even comprehend what Marx wanted to tell us with his theory of surplus value. (1909: 27)

What is it, then, that Marx "wanted to tell us with his theory of surplus value"? In Charasoff's understanding, the labour theory of value, while not needed for the determination of normal prices and the general rate of profits, is nevertheless indispensable for a proper understanding of the functioning of the capitalist economic system. In his reading, the role of the labour theory of value in Marx's theoretical construction is not primarily that of providing an explanation for the "origin" or "source" of profit, as countless readers of *Das Kapital* (including von Bortkiewicz) have maintained. Its main purpose in Marx's theoretical system is rather that of showing that a capitalist economy, as opposed to a "humane" one, achieves only imperfectly the ultimate purpose of the development of the human race, which consists in the saving of (human) labour time as a precondition for the attainment of the realm of freedom. The reason for this is that capitalist producers are interested only in the saving of the labour time for which they have to incur costs, that is, the variable capital (or "paid part" of the working day) and the constant capital (or "dead" labour), but not for the surplus labour time (or "unpaid part" of the working day). According to Charasoff, the labour theory of value is indispensable for the analysis of the development of the productive forces in economic systems that are subject to capital accumulation and technological change. In this connection, it is important to note that in Charasoff's understanding the "law of value" assumes a different role in the writings of the classical economists, Smith and Ricardo in particular, and in those of Marx:

The classics understood the law of value to mean that commodities exchange at their values, and they saw in it an unailing means for measuring and saving the entire labour expended in production. Marx pointed out that this is not so, that the labour embodied in commodities is indeed fully reckoned with, but that the not yet objectified living labour not appearing in value form eludes the value calculation. ... For the classical economists, the law of value was synonymous with the saving of the entire human labour time, but not so for Marx. If the classics said that the law of value rules on the market, they meant to assert by this the rationality of the capitalist market. Marx, on the contrary, understands by the law of value this peculiarity of the capitalist market, that not the labour itself, but the commodities, the

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<sup>22</sup> The final chapter of the 1909 book, entitled "Karl Marx to his bourgeois critics", is written in the form of an imaginary speech by Marx to an audience made up of capitalists. There, Charasoff puts into Marx's mouth the statement that Tugan-Baranovsky "merely poses as my disciple" and that his work is marred with "grave misunderstandings" (1909: 90–91).

labour time incorporated in them *alone*, is evaluated, whereas labour which does not appear in value form eludes the capitalist economy and assumes the form, not of expenses, but of profit. (1909: 30–31)

This unorthodox interpretation of the *analytical role* of Marx's use of the labour theory of value also explains Charasoff's at first sight rather perplexing attitude to the contributions of von Bortkiewicz (1906/07, 1907). He fully approved of von Bortkiewicz's "correction of Marx's fundamental theoretical construction" in terms of a simultaneous determination of prices and the rate of profits without any recourse to values—a solution, which he had indeed confirmed also by means of his own analysis. But he did not share von Bortkiewicz's assessment of Marx's theoretical achievements vis-à-vis those of the classical economists and in particular did not agree with his argument that the only point where Marx had improved on Ricardo's analysis was in showing the "origin" of profit in terms of a "withholding theory" (1909: 57 n.). Charasoff apparently regarded von Bortkiewicz as a "bourgeois" critic of Marx and even associated him with the critique of Marx's doctrines by members of the Austrian school:

Already twenty years ago the representatives of the economic science in the persons of Mr Zuckerkandl and Mr v. Böhm-Bawerk uttered the peculiar idea that Marx had presented his theory deliberately in an unclear and dialectically sharpened form, and just recently this judgement has been repeated anew and held against Marx by a University Professor from Berlin,<sup>23</sup> who maintained that Marx had taken a shine in the role of a Mephisto and meant to provoke and disconcert the learned world with captiously constructed sophisms. Can the critics reveal more clearly their distress and their inability to understand a thinker correctly than by advancing the reproach against Marx that he published his writings only in order to hide his thoughts and to fool his fellow men? (1909: ii)

Karl Kautsky, although his best-selling "primer" (1886) is recommended as "certainly one of the best and most popular expositions" of Marx's economics (Charasoff 1909: Preface), is also chided for having misrepresented Marx's views on the law of value and on the development of the productive forces under capitalism: According to Charasoff Kautsky wrongly attributed to Marx the view that capitalism develops the productive forces and the technology perfectly (1909: 39).

Charasoff's books contain no references to Russian sources, and no explicit references to neo-Kantianism and Tolstoyanism, or to the contemporary debates among German socialists on Tolstoy's pacifistic and anti-modernist ideas.<sup>24</sup> It is therefore difficult to reach clarity on Charasoff's stance on neo-Kantianism and Tolstoyanism and its importance for his work. If we associate Hermann Cohen's neo-Kantianism with the view that Kant's categorical imperative implies that humans must be seen as ends in themselves, rather than as means to an end,<sup>25</sup> then Charasoff's reference to a

<sup>23</sup> This is unmistakably a reference to a passage in Bortkiewicz (1906/07, I: 4).

<sup>24</sup> See Hanke (1993: 117–167) on the debates on Tolstoy's teachings among German socialists and Marxists. Kautsky, in his *Ethik und materialistische Geschichtsauffassung* (1906: 43) had referred to Eisner's neo-Kantian reinterpretation of Marxian ideas as "Gefühlssozialismus" {sentimental socialism}. The assessments of Tolstoy's novels and teachings among German socialists reached from "revolutionary" (Eisner) to "reactionary" (Kautsky).

<sup>25</sup> On Cohen's neo-Kantianism, see Hanke (1993: 126–128).

“humane economy” would seem to draw inspiration from both neo-Kantianism and from Tolstoy’s moral teachings. In addition, we can also relate Charasoff’s reading of Tolstoy<sup>26</sup> to Otto Buek’s understanding of the great Russian thinker (Buek 1905).<sup>27</sup> Buek’s essay, written against the background of the Russian revolution of 1905, combined Cohen’s Kant interpretation with a reading of Tolstoy’s plea for non-antagonism as a truly revolutionary ideal. In Buek’s reading, Tolstoy appears as a revolutionary anarchist, for whom man, as a free and self-determined being, is at the centre of his teaching: “Man *is to be* free, or he is *not* to be at all!” (1905: 541). The guarantor of freedom is reason, which “is not an extrinsic and imposed reason, but intrinsic, spontaneous, autonomous reason. Therefore, the freedom of man is his own law, the law of his being, which can be given to him by no one else than by himself, if he wants to be himself. Freedom is self-determination—is autonomy” (1905: 541). Tolstoy himself, by his own life, had put this ideal into practice. For Buek, however, it is an “irony of history” that Tolstoy, the “boldest advocate and partisan of the autonomy of ethical reason”, sought to ground this autonomy “in heteronomy, in the external determination through the bible and the new testament” (1905: 576). In Buek’s view, Tolstoy had committed a grave error by attempting to ground his teachings in Christian ethics (or rather, he showed through his own life that self-determination is possible and no external moral institutions are needed). Buek was aware of the fact, of course, that Tolstoy’s teaching of non-antagonism had been regarded by theoreticians of the social revolution, such as Kautsky and Lenin, as “reactionary”. Buek disagreed with this view: Tolstoy’s principle of non-antagonism is “the revolutionary principle *par excellence*, without any curtailments and concessions, but not of course as a romantic coup or a flirtation with bloody conspirator fantasies, but as *permanent revolution*, ... as the essence of man itself, revolution as method” (1905: 542). It would perhaps not seem too far-fetched to maintain that Charasoff agreed with these views of his close friend and that his notion of the “humane economy”, which he used in the title of his 1909 book, is related to the neo-Kantian ideas of the Marburg school and to Otto Buek’s reading of Tolstoy.

For Tugan-Baranovsky’s “ethical rationalization” of the theory of value and surplus value, Charasoff had nothing but scorn and contempt. However, he acknowledged Tugan-Baranovsky’s partly correct criticisms of Marx’s analysis of the tendency of the rate of profits to fall and of the crisis theory, and he agreed also with

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<sup>26</sup> Charasoff outlined his views on Tolstoy in a letter to Robert Michels of 1914. There he noted that Tolstoy is often wrongly regarded as a thinker in the tradition of Jean-Jacques Rousseau, based on the rather superficial observation that both have advanced a critique of modern science and culture. In Charasoff’s reading, Tolstoy should rather be seen as an *antipode* of Rousseau: While the latter obliges the individual to defend the constitution, Tolstoy identified the progress of the human race directly with the progression of the individual towards the renunciation of violence and governance. Tolstoy, Charasoff maintains, substitutes for the Roman law as the foundation of society “Christian love”, by which he means not a sentimental feeling, but social relations of a kind in which society has renounced all demands for force and violence on the part of the individuals.

<sup>27</sup> On Buek’s contribution to the “anthropological turn” in Russian neo-Kantianism, see Dmitrieva (2010: 89–90).



his rejection of crises explanations that refer to general overproduction (1909: 82–83). Charasoff indeed showed that the profit rate could fall only if diminishing returns from the limited availability of natural resources (land in particular) à la Ricardo set in, or if real wages rose. Marx's law of the tendency of the rate of profits to fall, he concluded,

is no law at all ..., but a *plain error*. ... According to the principles of the capitalist economy, the profit rate can *never* fall. In order to arrive at a falling rate of profit, we must presume a new method of production that promises to the capitalist who applies it a lower rate of profit already in advance. (1910: 184, 192)

The disproof of Marx's law of the tendency of the rate of profits to fall invalidated not only his crises theory but more importantly, Charasoff maintained, his theory of history:<sup>28</sup> Marx's proposition that the fall of the rate of profit must inevitably bring about the breakdown of capitalism and "naturally" lead to socialism had been shown to be wrong:

With the falsification of the law of the falling rate of profit Marxism, insofar as it intends only to be a scientific doctrine and to predict the natural demise of capitalism independently of the human will and of a rational objective, has lost its most secure foundation and cannot be sustained anymore. (1910: 196)

In Charasoff's understanding, the materialist conception of history was an attempt to derive historical developments from purely objective, (quasi-)natural laws. With his dialectical materialism and his claim to have developed an economic theory of "scientific socialism" (as opposed to a "utopian socialism") Marx "wanted to turn political economy into a natural science" (1910: 326). But in this he was mistaken, Charasoff objected, because in political economy "nature and all its laws turn out to be not only matter, but material that can be further transformed by human will, and here the determinism of natural science is never completely applicable" (1910: 325).

Charasoff's critique of the materialist conception of history should not be misinterpreted as a rejection of Marx's economic theory and his entire theoretical system. He advanced the slogan: "Forward, going beyond Marx" (1910: 252), and from his disproof of the law of the falling rate of profit he by no means concluded that Marxism was all washed up. His conclusion was rather that, contrary to Marx's prediction, "socialism cannot emerge from the play of natural forces of its own volition, it must be brought about, must be *produced*" (1910: 324). A "revolution" was needed, but for Charasoff this was to consist not so much in the "socialization of the means of production" by turning the latter into state property, but rather in the development of a quest for autonomy on the part of the workers and an understanding of the necessity of surplus labour for reproduction and accumulation purposes and for the development of technology: "Every man, and consequently also the present worker, must under certain conditions of his existence, feel the need to operate the production process autonomously." (1910: 326).

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<sup>28</sup> For the conceptual and analytical problems involved in the attempt to scrutinize Marx's theory of history, see Cohen ([1978] 2000).

In the final chapters of *Das System des Marxismus*, Charasoff criticized several statements in Boudin's book *Das theoretische System von Karl Marx* ([1907] 1909). He commented inter alia also on Boudin's statement that the classical economists had been guilty of circular reasoning, explaining prices from prices (1910: 290). Charasoff pointed out that Vilfredo Pareto had "characterized quite well the inadequacy of the classical production cost theories" (1910: 290) when he observed: "Par exemple, le coût de production du charbon de terre dépend du prix des machines, et le coût de production des machines dépend du prix du charbon, par conséquent, le coût de production du charbon dépend du prix de même charbon" (Pareto 1909: 241). Pareto's further observations Charasoff summarized succinctly as follows:

In the determination of prices it all boils down, according to Pareto, to the formulation of the production equations, where it turns out that the unknowns cannot be ascertained separately and each on its own, but only *simultaneously*. But since most authors, due to insufficient mathematical knowledge, were unable to solve a system of simultaneous equations, they invented the vague notion of *value* in order to forcibly break up the spell of the enchanted circle in which they were caught up. (Charasoff 1910: 290–291, note)

Charasoff approved of Pareto's statement, and then turned it against Boudin and Marx—but not in the sense of accusing the latter of having resorted to the labour theory of value as an inadequate *Hilfskonstruktion*. Unlike Pareto (and von Bortkiewicz), who argued that Marx and the classical political economists, had they known how to solve simultaneous equations systems, could have dispensed with the labour theory of value, Charasoff rather pointed out that with circular production relations not only the prices but also the labour values of commodities cannot be supposed to be known—they too first need to be determined from solving a simultaneous equations system:

But is not the same true also with Marx? Does not Marx always assume the value of the means of production to be given, but without ever showing anywhere how this value is to be determined? In Marx, the notion of a "reproduction base" is missing, and without this notion no value can be presumed to be known. (1910: 290)

In the Index of *Das System des Marxismus*, one also finds an entry on "Walras" (1910: 376). The reference is to page 121, where however there is no mention of Walras. However, it is precisely on this page that the system of simultaneous equations for the determination of the "original capital" is set out. Charasoff's reference to Walras is the more remarkable in view of his statement in the Preface, according to which he had developed the concepts of "production equations" and "production series" independently of the modern subjectivist theorists, based solely on his thorough reading of Marx's writings (1910: xiv). If this is true, Charasoff's page reference in the index would seem to imply that although Walras's equations in the *Elements* had not been a source of inspiration for him, he nevertheless clearly recognized, and acknowledged, the similarities between his own equations and Walras' formulation. It needs to be stressed, however, that Charasoff—unlike Dmitriev ([1904] 1974) and von Bortkiewicz (1906/07, 1921)—emphatically rejected the idea of integrating the classical production cost equations into the Walrasian system of equations, attempting thereby a "synthesis" of the objective and the subjective (or marginal utility) theories of value.

## 5 Contemporary Reactions

Karl Kautsky, the leading economic theorist of the German Social Democrats and editor of *Die Neue Zeit*, did not consider it necessary to react to Charasoff's books in print. The Austro-Marxist Otto Bauer briefly reviewed Charasoff's first book in the May issue 1909 of *Der Kampf*. According to Bauer, Charasoff had rightly pointed out that a central element of Marx's theoretical system is the proposition that capitalism fails to develop the productivity of labour to the highest possible degree, "because the introduction of labour-saving production methods is hindered by the fact that the capitalist only pays for necessary labour, but not for surplus labour" (1908/09a: 380). However, "this correct idea is presented by Charasoff in the clumsiest possible way", so that Marx's important proposition "is distorted by his unfortunate style of presentation to the point of making it appear ridiculous" (1908/09a: 380, 381). Bauer's rather superficial review makes no mention at all of Charasoff's analysis of prices and distribution. This prompted the author to send a reply to Bauer, which the latter refused to publish. He merely provided a summary account of Charasoff's letter in a single paragraph of the July issue of *Der Kampf*, in which he reported that "Charasoff complains about the fact that my review did not discuss his solution of the contradiction between the first and the third volume of *Capital* and his analysis of the relationship between the law of the falling rate of profit and the crisis theory" (1908/09b: 480). Charasoff, understandably, was disappointed, because he rightly felt that Bauer had failed to engage with the parts of his book which contained his most important findings.

In the following year, Bauer also reviewed *Das System des Marxismus* in the March 1910 issue of *Der Kampf*. There he conceded that Marx's transformation algorithm was "incomplete", because Marx had "refrained from showing how the formation of the prices of production must then in turn modify the rate of profit". But this "gap" cannot be filled, he argued, "by simply setting the prices of the basic products {Grundprodukte} equal to their values, and by thus falling back into the errors of the physiocrats" (1910/11: 237). Bauer's objection clearly missed the point of Charasoff's argument, which was to show the incompatibility of Marx's two invariance postulates ("sum of values = sum of prices" and "total surplus value = total profits"). Bauer also failed to understand Charasoff's proof of the proposition that the general rate of profits is determined by the production conditions in the industries producing basic commodities alone.<sup>29</sup>

The 1910 book was reviewed at some length also by Conrad Schmidt in *Sozialistische Monatshefte*, the revisionist counterpart of Kautsky's *Die neue Zeit*. Schmidt opened his review with a complaint about Charasoff's "tricky sophistry" {verzwickte Rabulistik}, which "demands very hard work from the serious reader" (1910: 850). He then devoted the remainder of his review article to a lengthy defence of his own position on the labour theory of value. According to Schmidt, the labour theory of value must be jettisoned in order to avoid the errors and contradictions into which one is inevitably led by a further adherence to it. Schmidt also contended, without

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<sup>29</sup> For a more detailed discussion of Bauer's review, see Mori (2007).

providing a proof, that all the important Marxian ideas can be derived by means of an analysis in terms of production prices only. There is no real discussion of Charasoff's work in Schmidt's review article.

A further "review" of Charasoff's 1909 book appeared in *Vorwärts*, the Party newspaper of the German Social Democrats, in the section "Literarische Rundschau". All that the reviewer, Gustav Eckstein, had to say on Charasoff's work is contained in the following passage:

If one wanted to note all the nonsense which is in this book, one would have to transcribe it; if one wanted to set it right, one would have to expound the entire economic system of Karl Marx. There is hardly any notion in Marx's theory which Mr. Charasoff has not misunderstood, hardly any doctrine which he has not distorted. (Eckstein 1909)

No substantial grounds are provided for this judgement.<sup>30</sup> Apart from Bauer, Schmidt, and Eckstein no other authors from the camp of the orthodox Marxists or the Revisionists felt compelled to react to Charasoff's books. These were ignored also by the "bourgeois" economists in Germany and Austria. The one contemporary economic theorist who could perhaps have recognized and appreciated Charasoff's important findings, Ladislaus von Bortkiewicz, apparently remained unaware of his books (which is rather surprising in view of the fact that around 1909/10 von Bortkiewicz was engaged in studying Kautsky's edition of Marx's *Theories of Surplus Value* for his articles on Marx's rent theory; he thus could be expected to be interested in the latest literature on Marx).

Even more disappointing were the reactions Charasoff received from Russian economists on his contributions: there were none! Tugan-Baranovsky, if he had noticed Charasoff's severe criticism of his views at all, chose to keep mum. The only contemporary Russian author who referred to Charasoff's books was Nikolai Bukharin in his *Economic Theory of the Leisure Class* ([1927] 1970), but his book, though completed already before the Great War, was published only much later.<sup>31</sup>

Charasoff was rather disappointed by the reception of his work, and in the following years made some further attempts to get some recognition for his findings. Upon his return to Zurich, he in October 1910 enrolled as a student of political economy, apparently with the intention of obtaining a degree. He pursued this goal for two years, taking classes, writing exams, and participating in seminars, but then suddenly terminated his studies after his second wife had poisoned herself with cyanide in 1912.<sup>32</sup> His enrolment appears to have been an attempt to get in contact

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<sup>30</sup> For completeness, it should be mentioned that there was also a short review of Charasoff's book of 1909 authored by Moride (1909), a young French economist who had just finished a doctoral dissertation on Marx and physiocracy.

<sup>31</sup> Bukharin's book manuscript was completed and first published in German in 1913/14, but the first Russian edition appeared only in 1919, the first German translation of the Russian edition in 1926, and the first American edition in 1927.

<sup>32</sup> This is reported in Vormundschaftsakten Kinder Charasoff (Stadtarchiv Zürich). No further information is provided on whether her death was caused by an accident (as a doctor of medicine she may well have worked with poisonous substances) or she had deliberately killed herself (and if so, why).

with academic economists and perhaps even to become one himself. A further attempt to get some recognition for his work he made in 1914, when he was invited by Robert Michels to contribute an entry on “Tolstoy” to the latter’s projected *Handwörterbuch der Soziologie*. In a letter to Michels, he offered to write in addition also an entry on “Marxism”, but with the outbreak of WWI Michels’ editorial project was suspended.

Apart from the fact that contemporary economists often lacked the necessary mathematical training for a proper understanding of his work (Mori 2007), there were also other reasons for the total neglect of his original ideas.<sup>33</sup> First, Charasoff’s choice of the publisher was not very conducive for ensuring a large readership, because the Hans Bondy Verlag existed only from 1908 to 1913 and the books had a very low print run. Secondly, Charasoff lacked any connections, both in academia and in Socialist/Marxist/Anarchist circles, in the German-speaking countries. His only contacts seem to have been those mediated by Otto Buek, who provided him not only with the link to Robert Michels, but in all likelihood was also responsible for the re-publication of extracts from Charasoff’s books in the literary-political journals *Die Aktion* and *Der Gegner* in 1918 and 1920/21. These extracts, however, did not include those parts of his books in which his innovative ideas are to be found.

## 6 Charasoff’s Return to Russia and His Lectures on Political Economy

In spring 1915, Charasoff travelled to Tiflis in order to take care of some financial business, leaving his four children behind in Zurich under the guardianship of Dr. Max Husmann, a Russian-born friend of the family, whom he had earlier supported in the establishment of a private school. His departure from Zurich may not have been unrelated, however, also to the fact that he was involved in a court procedure, because he had dismissed a housemaid after she had rejected his sexually motivated advances. When Dr. Husmann had no news from Charasoff for several months, he disbanded the family household and placed the four children with various Russian families in Zurich. For several years, he supported the children from his own pocket, but in spring 1919 he asked the Zurich municipality to take over the guardianship of the four children. In the following months, their legal custodian then arranged for their “home transport”, and by January 1920 all four children were “back home” in Tiflis (where they had never been before).

In the period from 1915 to 1921, Charasoff participated very actively in the literary and artistic activities of the futurist movement that had sprung up in Tiflis, and

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<sup>33</sup> See Gehrke (2015a, b).

appears to have spent his time and energy mainly with writing, discussing, and interpreting novel forms of poetry.<sup>34</sup> In 1918/19, his (landed) property<sup>35</sup> was seized by the new (Menshevik) government, and in spite of teaching mathematics at the newly founded University he (and, from 1920, also his two younger children who lived with him)<sup>36</sup> suffered from poverty and deprivation. He nevertheless persisted in his literary activities and assumed the role of a leader of a group of poets until the high time of cultural life in Tiflis abruptly came to an end with the invasion of the Red Army and break-up of the Democratic Republic of Georgia in 1921. Like many artists and writers, Charasoff decided to move to Baku in Azerbaijan, where shortly afterwards he began to teach mathematics, physics, and an introductory course on theoretical political economy at the newly founded Polytechnical University.<sup>37</sup> In 1924, he authorized the publication (in Russian) of a transcript of his lectures from students' notes, revised, and corrected by himself (Kharazov 1924).<sup>38</sup> The book, which is extremely rare and seems not to have circulated widely, consists of some 250 pages and is divided into three parts, each consisting of eight chapters, which are entitled, respectively: "Part 1: The individual economy", "Part 2: Theory of capitalism", and "Part 3: World economy". Charasoff's *Introduction to theoretical political economy* is a rather wide-ranging, disorganized, and challenging book, because its contents go far beyond those of a typical introductory course on political economy. It contains implicit and explicit references to a wide range of scientific fields, including classic Russian and Western literature, philosophy, history, sociology, mathematics, physics, chemistry, psychoanalysis, and, of course, political economy. In Part 1, on "Individual economy", Charasoff introduced his students to the objectivist and the subjectivist theories of value and distribution, with remarks on Ricardo's differential rent theory, Senior's abstinence theory, Böhm-Bawerk's agio theory, and Jevons's labour supply curve based on individual labour-leisure decisions. Part 2, "Theory of

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<sup>34</sup> Charasoff published several "transrational" or "zaum" poems and also provided psychoanalytical interpretations of Russian poetry, including an influential Freudian interpretation of Tatiana's dream in Pushkin's *Eugene Onegin* (Kharazov 1919a, b). On the literary scene in post-revolutionary Tiflis and Charasoff's activities, see Marzaduri (1982), Margarotto et al. (1982), Janecek (1996), Nikolskaia (1998, 2000), and Ram (2004).

<sup>35</sup> He still owned a factory which however had been shut down.

<sup>36</sup> The two elder children, Alexander and Arthur, left Tiflis and found employment in Azerbaijan.

<sup>37</sup> Interestingly, Charasoff also referred to his earlier contributions to political economy in an auto-graph letter from 1921, dedicated to the actor Nikolay Khodotov, at that time his neighbour in Tiflis. On the front page of this letter is a typed list of Charasoff's books, supplemented with the hand-written quotation (in German): "Regierte Recht, so läget Ihr vor mir im Staube jetzt: Denn Ich bin Euer König." (Schiller: Maria Stuart.) ["Ruling right, you'd lie before me now in the dust: for I am your king." (Schiller: Maria Stuart.)]. On the back page is a poem that he wrote (in Russian) to the memory of the recently deceased Peter Kropotkin, under the title "I won't resurrect".

<sup>38</sup> In the preface of the 1924 book, Charasoff notes that he corrected the students' notes of his lectures and "poked out" on his typewriter a compilation of text passages that had been taken from his German books, from a "Summary of Political Economy" published two years ago by a local publishing house, and from some unpublished manuscripts. No copy could be found of the "locally printed" "Summary of Political Economy" of 1922.

capitalism”, covers the social economy, capital, capital accumulation, labour organization, division of labour, and the distinction between the “capitalist” and the “humane” economy. It is introduced with some remarks on the ideas of Kant, Plato, and Tolstoy on the importance of science, education, and passive resistance. The third part, on “World economy”, contains remarks on the relation between the individual and the state, Friedrich List’s ideas on national economic development, Marx’s theory of exploitation, the distinction between production prices and values, a critique of the breakdown theory, and the relationship between Marxian and Aristotelian philosophical ideas.

The Preface, authored by Charasoff himself, is of some interest, because in it he refers to his German books and their reception. It opens with the statement (and citation): “*Moi, je ne suis pas marxiste. K. Marx*” (Kharazov 1924: I). The dominant theme in Charasoff’s Preface is his relationship to Marx, to Marxism, and to various Marxian economists. He approaches this theme by first discussing Bukharin’s references to him in (the German edition of) *The Economic Theory of the Leisure Class* ([1927] 1970). Charasoff notes that Bukharin had referred to him as a “benevolent critic” of Marx,<sup>39</sup> whereas the “bourgeois economist” Karl Diehl, in his entry on “Marxism”, had called him a “Marxist”. Charasoff then characterizes himself as someone who has thoroughly studied Marx and holds him in the highest esteem, but this does not prevent him from criticizing Marx’s erroneous views. In *The Economic Theory of the Leisure Class*, Bukharin ([1927] 1970: 56) argued that the Austrian school, with its emphasis on the individual’s consumption choices in a static framework, is ill suited for tackling problems of social dynamics, and he refers approvingly to Charasoff’s statement that the representatives of the Austrian school “are incapable of even formulating, to say nothing of solving, such fundamental questions as the evolution of technique in a capitalist society, the origin of capitalist profit etc.” (Charasoff 1910: 19). Bukharin ([1927] 1970: 127) also quoted approvingly Charasoff’s criticism (1910: xxii) of Böhm-Bawerk’s explanation of profit in terms of the overestimation of present goods. However, he had failed to engage with the issue that Charasoff considered to be of central importance in Marx’s theoretical system:

In this connection it is of course important to me to discuss the competent polemics of Bukharin, in particular because in his great work on historical materialism<sup>40</sup> he has remained silent on a topic that greatly interests me, namely on the reduction of the working day as the indispensable precondition of the realm of freedom. (Kharazov 1924: 4)

In spite of Bukharin’s approving remarks on some of his statements on the Austrian school, Charasoff was rather disappointed that the latter, like all his other commentators, had not engaged with his fundamental critique of Marx’s materialist conception

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<sup>39</sup> Charasoff refers to a footnote in the German edition of Bukharin’s *Politische Ökonomie des Rentners*, which corresponds to the following note in the American edition: “Even the “benevolent” critics fail to understand this; cf. George Charasoff, op. cit., pp. 260, 26” (Bukharin [1927] 1970: n 41).

<sup>40</sup> The reference to Bukharin’s *Historical Materialism* ([1921] 1926) shows that Charasoff also after 1915 continued to study the recent literature on Marx.

of history. To the best of my knowledge, Bukharin made no response to Charasoff's remarks—and probably never saw a copy of his “Baku lecture notes” of 1924.

In 1925, Charasoff appears to have moved to Moscow, where he gave two invited lectures on the psychoanalytical interpretation of literary works at the newly founded Russian Psychoanalytical Society. In the same year, he also published a paper on the refutation of Einstein's relativity theory, which drew some approving comments from the group of “mechanist physicists”. According to a notice in *Izvestia*, Professor Kharazov died on 5 March 1931 near Zaporizhzhia, during a visit of the energy institute Dnieprostroy, which was overseeing the construction of a dam and a hydroelectric power station at the Dnieper.

## 7 On the Further Reception of Charasoff's Contributions in the West

Excerpts from Charasoff's two books, comprising altogether seven full chapters, were published in 1918 and 1920 in the literary-political journals *Die Aktion* and *Der Gegner*. However, these re-publications probably did not contribute to making his original findings better known among economists, because these texts appeared in non-scientific outlets and did not include the relevant parts of his books containing his novel concepts and analytical findings. Charasoff seems not to have been informed about these re-publications. He probably remained unaware also of some critical comments on parts of his books that were published in Germany in the 1920s and 1930s. He certainly missed the comments by Peter (1933, 1934) and Klimpt (1936), but it is very likely that he remained unaware also of the critical discussions of his contributions by Moszkowska (1929) and Grossmann (1929, [1932/33] 2019b).

Moszkowska<sup>41</sup> discussed Charasoff's analysis of the law of the falling rate of profit and his critique of Marx's crisis theory in her book *Das Marx'sche System. Ein Beitrag zu dessen Ausbau* (1929). However, she made no attempt to provide a serious discussion of Charasoff's determination of prices and the rate of profit. She merely remarked that his calculations are “even more complicated than Tugan-Baranovsky's”, and that “he claims to be able to examine the exchange relations without any relation to Marx's theory” (1929: 31). She then concluded with the observation that, “although our ways are different, the mathematical results coincide” (1929: 31). In her analysis of Marx's law of the falling rate of profit, Moszkowska returned to Tugan-Baranovsky's formulation of the problem and also followed him in constructing a numerical example in value terms. Her remarks on Charasoff's concept of “original capital” (*Urkapital*) show that she failed to understand its analytical role

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<sup>41</sup> Natalie Moszkowska, of Polish descent, came to Zurich in 1908 in order to study political economy. She wrote her doctoral dissertation (published in 1917 but completed already in 1914) on workers' savings banks in the Polish coal and steel industry under Heinrich Sieveking's supervision. It seems very likely that she and Charasoff met in Sieveking's seminars at the University of Zurich. On Moszkowska's life and work, see Howard and King (2000).



(1929: 111), and with regard to his discussion of Marx's crisis theory, Moszkowska criticized Charasoff for his neglect of the disproportionality aspect (1929: 139–141).

The originality of Charasoff's ideas was not recognized also by Grossmann, who discussed his critique of Marx's crisis theory against the background of his own "breakdown theory" in his *Accumulation and Breakdown of Capitalism* ([1929] 1992), and also, much along the same lines, in two further articles ([1929] 2019a; [1932/33] 2019b). Grossmann argued that Charasoff had omitted to take into account the repercussions of a rising organic composition of capital on the pace of accumulation and disregarded the resulting demand problems ([1932/33] 2019b: 381). In his critical discussion and attempted refutation of earlier contributions to the so-called transformation problem, Grossmann (2017) did not consider it necessary to discuss also Charasoff's contribution and concentrated only on the contributions of Bauer, von Bortkiewicz, and Moskowska.

In his habilitation thesis,<sup>42</sup> Hans Peter sought to reconcile the objective and the subjective theories of value and distribution by integrating the "Dmitriev-Bortkiewicz" production price equations into the Walrasian system of equations. Despite his mathematical training, Peter also did not notice Charasoff's original concepts and findings and confined his comments to Charasoff's objections to Marx's law of the falling rate of profit, which he claimed did not go beyond those of Tugan-Baranovsky<sup>43</sup>:

*Tugan and Charasoff object to Marx's statement that the rate of profit must fall when the productivity increases. It is on the contrary with a decrease in productivity that the fall of the rate of profit is associated. With increasing productivity, the rate of profit must rather rise. – It suffices for us to note that both indeed show with their critique the weak point in Marx's theory. The examples on which they base their argument are not chosen deftly and cannot provide a sufficient foundation for the general justification of a positive statement. What they show is only that a parallel movement of productivity and the rate of profit is possible, but not that it is necessary. (1934: 110)*

Charasoff's original ideas were not recognized as such also by the mathematician and economist Werner Klimpt,<sup>44</sup> who was one of the few authors who discussed Charasoff's concept of "original capital" {*Urkapital*}. But also Klimpt (1936: 119), "despite his mathematical background, ... rejected Charasoff's device as highly peculiar, complicated, and confused. Apparently, neither Klimpt nor his doctoral referees Emil Julius Gumbel and Emil Lederer understood Charasoff's innovations" (Parys 2014: 994).

Charasoff's path-breaking work also received no attention in the wake of the discussions on Marx's transformation problem in the 1940s and 1950s, after the essence of von Bortkiewicz's 1906/07 and 1907 contributions had been made available to English-speaking readers by Sweezy (1942). However, as Mori (2007) has

<sup>42</sup> Peter's thesis was submitted at the University of Tübingen in 1928 and then published in three parts as *Grundprobleme der theoretischen Nationalökonomie* (1933, 1934, 1937).

<sup>43</sup> Peter refers to Tugan-Baranovsky (1901, 1905) and Charasoff (1909, 1910).

<sup>44</sup> Klimpt had studied mathematics and economics in Berlin and Heidelberg in the 1920s. He submitted his doctoral dissertation, published subsequently as Klimpt (1936), at the University of Heidelberg in 1931.

noted, a rather direct line of development can be said to exist, *from a purely analytical point of view*, from Charasoff's contributions to the work of the Japanese scholars Shibata (1933) and Okishio (1961). As Mori shows, there are some striking parallels, with regard to the methods of proof and the findings, between Charasoff's contributions and theirs—but the two Japanese Marx scholars were of course also unaware of his work. Moreover, Shibata's contribution shared with Charasoff's the same fate: it also went completely unnoticed until many years later, both in Japan and in the English-speaking world.

## 8 The (Re-)Discovery of Charasoff's Contributions in the 1980s

It remains for us to note that in the 1980s the scientific reputation of Charasoff suddenly started to rise, thanks to the (re-)discovery of his economic contributions by Egidi and Gilibert (1984, 1989). In the following years, many other authors studied and appraised Charasoff's work, including Duffner and Huth ([1987] 2013), Kurz (1989), Kurz and Salvadori (1993, 1995, 2000), Stamatidis (1999), Marchionatti and Fiorini (2000), Mori (2007, 2011), Parys (2014, 2018), and Marchionatti (2019). Excerpts from his books have been published by Marchionatti (1998) and also, in an English translation, by Egidi and Gilibert (1989).<sup>45</sup> It is now widely recognized that Charasoff developed some of the tools and analytical devices that were later rediscovered, independently of him, by von Neumann, Okishio, and Sraffa. His concept of "*Grundprodukte*" bears a close resemblance to Sraffa's "basic commodities", his notion of "*Reproduktionsbasis*" is related to Sraffa's concept of a "subsystem", labour values are computed via an infinite dated labour series, the duality properties of the price and quantity system, emphasized by von Neumann, are clearly perceived by him, and the essential elements of the "Okishio theorem" are already clearly stated by him.

More recently, Charasoff's pioneering work has been recognized and appraised also by scholars in Russia, most notably by Klyukin (2007, 2008), who has placed his contributions in the context of the economic circular flow analyses in the tradition of Dmitriev and von Bortkiewicz, and has also (re)claimed their "Russian heritage".

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<sup>45</sup> Independently of Charasoff, a number of similar findings, including a proof of the duality properties of linear economic systems, have been presented in papers published between 1911 and 1913 by Father Maurice Potron, a French Jesuit and trained mathematician. For Potron's contributions, see Bidard and Erreygers (2010).

## 9 Concluding Remarks

Georg von Charasoff was an intellectual of great versatility with a sound academic background in physics and mathematics, who could draw on a thorough study of the major Western philosophical and economic works, and who systematically acquainted himself also with the latest developments in several other scientific and artistic fields. In his work on Marx's economic theory he was able to combine a lively literary writing style with a rigorous mathematical analysis, and to come up with important new insights and findings.

Prior to the publication of his two books, Charasoff had studied Marx's economic works and those of the classical political economists intensively for many years. These studies had led him to embark on a publication project already around 1902—a project that at least up until 1907/08 was still closely linked to a discussion and elaboration of Tolstoy's philosophical ideas and intended to result in a book publication in Russia(n). It was apparently only in 1907/08, when Chertkov's support for a Russian publication turned out to be insufficient and an article he had submitted to Kautsky's *Die Neue Zeit* had been rejected, that Charasoff abandoned the idea of a publication in Russia and resorted to the plan of publishing his ideas in the form of books in German, helped in the translation by Otto Buek. His friendship with Buek and his earlier allegiance with Tolstoyanism (if such it was) might suggest that he endorsed some neo-Kantian variant of ethical Marxism, but of this there are no signs in his books. On the contrary, he distanced himself emphatically from the attempts at relating Marx's economic theories to ethical considerations, and in the dispute with the Revisionists, led by Bernstein in Germany and Tugan-Baranovsky and Struve in Russia, he sided with the orthodox Marxists.

With his books, Charasoff sought to contribute to the then ongoing debates among German, Russian, and German-Russian Marxists—but the German orthodox Marxists (Bauer, Kautsky, etc.) were unable to recognize his innovations, the German revisionists (in the person of Schmidt) were interested merely in defending their own position, and Tugan-Baranovsky and other Russian economists and Marxists, with the notable exception of Bukharin, ignored him.

A number of reasons can be given for the limited recognition that his work received in the German-speaking countries. First, Charasoff had no academic position and no connections to economic theorists that were able to appreciate his work. Secondly, he used mathematics but not a mathematical method of exposition, so that the few contemporary mathematical economists were not drawn to study his books.<sup>46</sup> Third, his unfortunate choice of the publisher and his rather polemical writing style were not conducive to attracting many readers to his books.

Even more obstacles were standing in the way of a proper recognition of his work in Russia. Among the exiled Russian revolutionaries in Germany and Switzerland, Charasoff apparently was not well connected, and he made no efforts to participate in the Russian economic discourse once he had abandoned his earlier publication

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<sup>46</sup> The underlying mathematical reasoning and proofs had to be reconstructed; see Egidi and Gilbert (1984, 1989), Duffner and Huth ([1987] 2013), Mori (2011, 2016), and Parys (2014).

plans with Chertkov. Moreover, he also lacked connections to academic economists in Russia and in particular to the mathematical economists at the Universities of Moscow and St. Petersburg. When he later published the lecture notes of his course at the University of Baku, both the wide-ranging character and disorganized structure of the book as well as the changed intellectual and political climate stood in the way of attaining some attention for his contributions.

Finally, some role for his failure to obtain the recognition for his innovative economic ideas that the latter undoubtedly deserved must certainly be accorded also to Georg von Charasoff's idiosyncratic, self-centred, and difficult personality.

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<sup>47</sup> The location of the archive sources used in this paper is as follows: the "Vormundschaftsakten Kinder Charasoff" are held at the Stadtarchiv Zürich, the Kautsky Papers at the International Institute of Social History (Amsterdam), the Chertkov Papers at the Russian State Archive of Literature and Art (*RGALI*, Moscow), and the Roberto Michels Papers at the Fondazione Luigi Einaudi (Torino).

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# Eugen (Evgeny Evgenievich) Slutsky



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## 1 On Slutsky’s “Influence”

Slutsky’s name is famous to any economist; it is associated to several concepts or tools in microeconomics or economic statistics, most notably, to the “Slutsky equation” or “decomposition” of the effect of a price change upon demand into an income and a substitution effect (“à la Slutsky”, by way of contrast with a decomposition “à la Hicks–Allen”) and the “Slutsky–Yule effect” to denote a random-based correlation in the study of time series, notably of economic cycles. Those two contributions (Slutsky 1937b, 1952), and some others to the theory of statistics (Seneta 2001; Bru 2003), have turned out to be enough to make Slutsky part of the history of microeconomics, econometrics, macroeconomics and statistics.<sup>1</sup> Slutsky’s influence through these two articles also stems from the fact that they are the only two articles on economic subjects that were translated into English in the twentieth century. “On the theory of the budget of the consumer” originally published in Italian (Slutsky 1915), was published in English only in 1952—though an English translation circulated privately around 1933–34—and was reissued in 2012 in its original outlet, *Giornale degli economisti*. As for the 1927 article, “The Summation of Random Causes as the Source of Cyclic Processes” (Slutsky 1927a), it contained an abstract in English and circulated among Russian-speaking economists in the West, and an amended version was published in English in *Econometrica* in 1937 (Slutsky 1937b).

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<sup>1</sup>The “Slutsky theorem” (also Cramér’s theorem), based on the notions of convergence in distribution and convergence in probability, extends some properties of algebraic operations on convergent sequences of real numbers to sequences of random variables (Slutsky 1925).

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It is to be noted that the ideas and tools introduced by Slutsky in 1915/1952 and 1927/1937 have been accommodated to various kinds of problems and various theoretical settings, manifesting that they are much more than simply a historical landmark to the development of the neoclassical paradigm or any specific theory. Their lasting importance is an invitation to consider Slutsky's classics as well as his other published writings linked to the field of economics as a whole (including his as yet untranslated 1910 master thesis, *Theory of marginal utility*, Slutsky 2010). They are testament for Slutsky's strong sense of reflexivity on the foundations of economics. This deeper dimension of Slutsky's thought is already detectable in the two papers that have made his fame, though it was not fully highlighted. In what sense, then, can we say that Slutsky has been influential in the Western world? Certainly not as someone who provided a self-sustained agenda through a set of organized and cumulative works, but rather as someone whose investigations in economics provided both new tools and new methodological questions about the fundamental data of economics, tools that nurtured both macroeconomists and microeconomists throughout twentieth century.

In a nutshell, Slutsky could be described as a mathematical statistician and probability theorist with a strong interest in political economy, who inquired on the possibility to build a theory of economic systems on the basis of general behavioral principles. These principles in turn are analyzed through a theory of human action (praxeology) and a non-deterministic view of the world (probabilistic thinking).<sup>2</sup> His abilities in mathematics and statistics led him to contribute to the search of the best ways to organize and rationalize empirical data, be there behavioral (price-quantity) or aggregated time series data.

Until recently, to non-Russian-speaking scholars, Slutsky's known contributions, aside from the 1915/1952 and 1927/1937 articles, were mainly his contributions to statistics and mathematics published in French (Slutsky 1927b, 1928a, b, 1929a, b, 1938), Italian (Slutsky 1934, 1937a), German (Slutsky 1925, 1926a, b), English (Slutsky 1913a). Among these articles, the article on praxeology (Slutsky 1926a) was published in a Russian journal. Recent interest in Slutsky's work on the part of historians of economics has made available in English several important articles, notably the two articles edited by Chipman in 2004 (the article on praxeology translated by Wittich and the article on Böhm-Bawerk translated by Rosko and Chipman) (Slutsky 2004a, b), also an article on Sir William Petty (Slutsky 1914, 2005). Also, we owe to Sheynin (2010) a translation of several works on statistics and mathematics and of his 1912 booklet on correlation. In addition, Sheynin also translated two economics-related articles about monetary issues (Slutsky 1923a, b) and an article on the foundations of probabilities (Slutsky 1922) and several other pieces of interest (autobiographical notes, biographical notes and tributes, correspondences).<sup>3</sup> Barnett

<sup>2</sup> Barnett (2011, 185) aptly stresses that Slutsky's place within the neoclassical tradition is that of an outlier and that "Slutsky had sought to introduce stochastic concepts into the very heart of the basic elements of economic understanding".

<sup>3</sup> Curiously enough Barnett (2011, acknowledgments) announces a translation of Slutsky (1923b) which turns out to be merely a step by step presentation of it. He also provides a translation

(2011, 196–201) has reissued the substantial five pages English summary of the 1927 article, which was enough to make Slutsky's approach known in the West, and he provides us with a translation of the table of contents of Slutsky's master dissertation, *The theory of marginal utility (Teoriya Predel'noi poleznosti)* (Slutsky 2010).

The present essay is organized as follows. Section 2 provides biographical elements about Slutsky, pointing out the political circumstances that prevented him from devoting himself fully to economics. Section 3 presents the 1915/1952 article. Section 4 analyzes how Slutsky's theory of demand became the backbone of neo-classical demand theory. Sections 5 and 6, respectively, present the 1927/1937 article and how Slutsky's discovery came to be central within the theory of cycles in twentieth century. Section 7 comes back on Slutsky's lesser-known articles and attempts to bridge Slutsky's thoughts together.

## 2 A Short Bio of Slutsky: Economics Under Political Pressure

Eugen (Evgeny Evgenievich) Slutsky (1880–1948) was born in April 1880 in the governorate of Yaroslavl (a hundred miles northeast from Moscow).<sup>4</sup> His family originated from Ukraine and returned there in 1889. Showing an early interest for physics and mathematics, he joined the faculty of mathematics and physics of the University of Kiev in 1899. A spirited, “impetuous” and “boyish” character, Slutsky was involved in the students protests and meetings that were recurrent in Ukraine and Russia at the turn of the twentieth century. After being expelled twice from the University of Kiev, and being furthermore barred from any other Russian higher education institution, Slutsky studied mechanical engineering at the Institute of Technology in Munich (then, *Königlich Bayerische Technische Hochschule München*) for three academic years (1902–1905). During those years, his earlier interest for economics strengthened through reading Ricardo and Marx (Slutsky 1939a, 19–20) and getting acquainted with the marginalist school of economics. He was then able to come back to Russia and Ukraine after the revolutionary events of 1905. There, he went to the Law faculty of the university of Kiev, whose curricula offered courses in political economy. In 1906, he married Yulia Nikolaevna Volodkevich and retreated from revolutionary activities. As he recalls this time span that decided of his professional commitments: “I ... discovered that my visual memory was very weak. Therefore, ..., I could not become a good mechanical engineer. And by the same reason, I very badly memorized people by sight and mistook one person for another

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of the probability article titled “On the Question of the Logical Foundations of the Calculus of Probabilities”, but Sheynin's translation seems more recommendable since it is based on a revised version published in 1925, after Slutsky had “improved some formulations making them more intelligible” (Sheynin 2010, 41).

<sup>4</sup> This short bio borrows mainly from Barnett (2011) and from first hand and second hand sources (Chetverikov 1959; Seneta 2001) and two short autobiographical notes (Slutsky 1939a, 1942).

one even if having met them several times so that I was unable to be a political figure either". (Slutsky 1939a, 227). During those hectic years, he worked on his own on the application of mathematics to economics. The output of this immersion into mathematical economics within the context of the Marxist's criticism of bourgeois' economics and the raging debates between agrarian socialists (populists) and advocates of a proletarian revolution (Allisson 2014, 2015) was a master diploma dissertation devoted to pure theoretical issues, *Theory of Marginal Utility* (1910) for which he received a gold medal in 1911. This dissertation is testament that Slutsky's interests went beyond economics and mathematics to include also philosophy and psychology, delving deep into the foundations of economics.

Slutsky felt that his own individuality found its full expression through mathematical inquiry and its application to various subjects, notably economics. Between 1909 and 1915, he became a member of several Kievian scientific societies (Society of Economists, Mathematical Society, Sociological Society) and full member of the Society for the Development of Social Sciences at Moscow University (Chetverikov 1959, 252). Being barred once more from the university of Kiev, Slutsky eventually obtained a Master of Political Economy and Statistics from Moscow University in 1917. Due to his revolutionary tendencies, Slutsky was not able to get a position at the university of Kiev. In 1912, he accepted his father-in-law proposal to teach in his school at Saint-Petersburg and was later hired—probably under Chuprov's recommendation (Sheynin 1993, 250)—to teach mathematics and statistics at Kiev Commercial Institute (Campbell 2012).

By that year, Slutsky had gained recognition as a statistician. Indeed, toward 1911–1912, he had been discovering theoretical statistics through a book by A. V. Leontovich introducing Gaussian and Pearsonian statistics (Seneta 2009, 120–121), and this would be the starting point for his lifelong passionate involvement in mathematics, statistics, and probability theory. In the field of statistics, Galton's innovations (coefficient of correlation, coefficient of regression, principle of regression toward the mean) and Pearson's work did foster numerous works trying to establish connections between social, biological, natural, and economic phenomena. Slutsky deepened his interest in such matters and endeavored to publish a synthesis on the contributions of the English school of statistics (or biometric school), which came out in 1912 as *The Theory of Correlation and Elements of the Doctrine of the Curves of Distribution* (Slutsky 1912). The booklet is conceived of as a manual exposing Pearson's theory of correlation, together with some critical assessments of it and additional personal results. From that moment on, Slutsky would become part of the international community of statisticians, and his methods would be discussed by Pearson and Fisher (Pearson 1916; Fisher 1922). In Russia, it ensured Slutsky's reputation as a mathematician and statistician—though his stance was not in line with some features of the Russian school of probability.<sup>5</sup> Also, the booklet shows Slutsky's interest in applying statistics to social sciences, warning researchers from hasty conclusions regarding the proper influence of some external factors on economic phenomena by

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<sup>5</sup> The gist of the heated debate within mathematical circles is the status of the Law of Large Numbers as a mathematical statement and its relationship with the notion of probability (Seneta 1994).

use of partial correlation measures (to avoid attributing to one factor the causality on another, such as rain and temperature on the yield of a crop) (Barnett 2011, 28).

In 1913, Slutsky contributed in the *Journal of the Royal Statistical Society* with an article on the theory of regression, providing a criterion to find the most probable regression curve in a family of curves from a given type (“On the Criterion of Goodness of Fit of the Regression Lines and on the Best Method of Fitting them to the Data”) (Slutsky 1913a). It aroused comments by Yule, Pearson, and Fisher and was recognized by Deming (1934, 372) as “an invention in curve fitting”. It contained an analysis of correlation between lagged variables (the price of rye in Samara at  $t$  and its price one month earlier). During those years, Slutsky engaged in correspondence with mathematicians, statisticians, and economists in the West (Pearson, Bortkiewicz, Marschak, and later, Frisch).<sup>6</sup>

Apart from his position at the Kiev Commercial Institute—where he moved to the teaching of political economy which “[he] considered [his] main speciality” (Slutsky 1939a, 227)—Slutsky held various short-term appointments, teaching on various subjects (mathematics, probability, history of economics, and history of socialist ideas) at the Ukrainian Cooperative Institute (1917) and at the Kiev Institute of National Economy (1919) (Barnett 2011, 55), also working as statistician at the Kiev Statistical Bureau. During his years at the Commercial Institute (1913–1926), Slutsky’s interests in political economy led to a handful of articles on various subjects (apart from the 1915 article) that did not circulate in the Western world, except maybe through private offprints. They can be read also as pedagogical material in relation with Slutsky’s involvements. “The essence of Cooperation and its Forms” (1913b) is devoted to the analysis of the motivations driving cooperatives and their history in Russia (Barnett 2004). This interest for cooperatives is in line with a huge interest in the organization of cooperatives and in the cooperative movement in the wake of the search for alternatives to capitalistic concentration in private organizations. It was also a time of development of cooperatives in Russia (especially purchasing cooperatives and credit cooperatives) (?). As such, Slutsky’s contribution cannot claim originality (Barnett 2011). To Slutsky, cooperation implies a sense of co-decision and equality within the organization (*Solidarism* in France) and can be applied to any important function in the economy (credit, consumption, production, distribution). It owes more to Utopian socialism and other non-Marxist theories about the way to address the social question than to a Marxist–Leninist theory of economic development, it promotes equality of its members and the interests of other stakeholders, as well as not-for-profit activities.

In 1914, Slutsky published an article on Sir William Petty (Slutsky 1914) which can be read as an introduction for students to the economic views of a pre-classical economist with whom Slutsky could identify as a polymath searching for a synthesis

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<sup>6</sup> Jacob Marschak attended Slutsky’s lectures at Kiev Commercial Institute in 1915–1916. There is as yet no evidence that Slutsky ever corresponded with Moore and that his analysis on regression would have had any direct influence on Moore’s *Forecasting the Yield and Price of Cotton* (Moore 1917). According to Barnett (2011, 36) the fact that Moore calculated the correlation between fluctuations in the prices of New York cotton futures and the prices of spot cotton on exchanges in the South must have been induced through Pearson.

between various founding elements of value and raising statistical analysis to a standard of economic practice. We shall postpone a more thorough presentation of this article until the last section. Be it enough to mention that Slutsky's presentation shows a synthetic view of Petty's contributions and invites readers to take it as a fundamental reading to uplift their formation to economic thinking.

The outburst of the Bolshevik revolution (October 1917) put more and more the intellectual elites under spotlight, to reach its acme in the 1930s during the Stalinist era. It is quite certain that, after his deliberate excursion at the heart of the marginalist theory of value, Slutsky had to retreat to more ideologically immune research. To accompany the transition toward a planned economic system in the 1920s Soviet Russia, Slutsky contributed through his mathematical skills, only to deal tangentially with economic issues: He felt that the foundations of probability were weak and that only an axiomatic approach was acceptable, in which statistical tools would play a central role. Most of his research then centered on refining the Law of Large Numbers to account for a non-frequentist approach to probabilities, a view in which probabilities are defined as time-dependent stochastic processes. This approach relied on various authors (David Hilbert, Emile Borel), who aimed at connecting probability theory with set theory and functions. Slutsky expressed his views in this direction in "On the Question of the Logical foundations of the Calculus of Probabilities" (Slutsky 1922).

During this period (1917–1926), Slutsky made four excursions in the field of economics. In 1923, he published two connected articles on monetary issues (Slutsky 1923a, b), more precisely on State's currency emission. Then, in 1926, he published a very abstract reflection on the foundations of economics, a pioneer contribution to praxeology, "On the Formal Praxeological Foundations of Economics" (Slutsky 1926a, 2004b) (hereafter "Praxeology"). Last, a fourth article went to completion in 1926, only to be published in 1927 in German (Slutsky 1927c). It is devoted to a critical assessment of Böhm-Bawerk's theory of value, "A critique of Böhm-Bawerk's concept of value and his theory of the measurability of value" (Slutsky 1927c, 2004a) (hereafter, "Critique", see also Barnett 2011, 82sq.). The two papers on currency emission are motivated by the historical context of Soviet financial situation and the need to stabilize the monetary system. Praxeology is a highly abstract representation of economic activities and potentialities based on various concepts dealing with structural relationships between agents and their own representations. Critique bears on Slutsky's earlier account of the marginalist theory of value and capital. We shall postpone the analysis of these papers until the last section.

Year 1926 opens up a new period in Slutsky's life (1926–1930), away from teaching activities and from Kiev. In 1925, as Slutsky's reputation as a statistical theorist with an interest for economic issues was well established, he was invited by Nikolai Kondratiev to join the Conjunction Institute of the People's Commissariat of Finance in Moscow. This opportunity to leave Kiev came in conveniently to Slutsky, since Ukrainian authorities had summoned him to deliver all his lectures in the Ukrainian language, which he did not master and did not want to master. Slutsky moved to Moscow to become one of the three main consultants at the Conjunction Institute. The Moscow Conjunction Institute had been established in 1920, at times of severe

depression in Soviet Russia. The Institute was involved in supporting the NEP put forth by Lenin, providing forecasting on economic fluctuations and developing various production indices and price indices (both complementing and competing with Gosplan) (Klein 1999). At the Moscow Conjecture Institute, Slutsky was in charge with basic economic processes. It was in this environment of interest for fluctuations (Franco et al. 2022) that he prepared his 1927 paper on moving summations of random series as a cause of cycles. During his stay at MCI, all other published research by Slutsky focused either on statistical topics (regression and correlation theory) or on the theory of probability (stochastic limit theorems and the law of large numbers). Slutsky's research was certainly above the standard of mathematical technics used to produce reports and conjunctural analysis: He was following his own path of research.

In 1928, Slutsky attended an international congress of mathematicians in Bologna during which he presented a contribution "Sur les fonctions éventuelles compactes" (published as Slutsky 1929b), which revolves again on the issue of understanding new phenomena that cannot be simply built on the basis of past phenomena. During this famous conference, Slutsky was at the center of a mathematical quarrel with mathematician Cantelli about the priority of the strong law of large numbers (in the case of independent repetitions of a Bernouilli trial) (Seneta 1992; Bru 2003).

By the end of the 1920s, political pressures on intellectuals increased. In 1928, Kondratiev was dismissed and the MCI was transferred from the Ministry of Finance to the Central Statistical Office and was then officially closed after Stalin's arrival to full power in 1930. Kondratiev and several other members of the Institute were arrested, and rational scientific debate vanished from most disciplines in the USSR. Slutsky was not bothered, not having been involved in any compromising debate and journal (Barnett 2011, 99sq). However, he was wise enough in later occasions to avoid connecting his own research on random processes with economic topics, now working essentially on probability theory. Slutsky then moved to work at the Central Institute of Meteorology. There his research focused on the study of solar activity and other geophysics' topics. Notably, he was able to contribute to work on the periodicity of sunspot, a theme he had also discussed with Vainshtein while at the Conjecture Institute. Apart from this aspect of his post-1930 work, Slutsky's contributions dealt with the conditions for applying statistical tools to the study of geophysical phenomena, since the conditions for assuming independence of events in a series were not met and geophysical processes were likely to be non-stationary.

In 1935, Slutsky turned back to pure mathematics and statistical theory at the Mathematical Institute of Moscow, and in 1939, he held a chair of mathematical statistics at Moscow university. Among other works, he studied correlations of related series for a limited number of trials. He obtained conditions for measurability of random functions in 1937 (Slutsky 1937a, also in Russian as Slutsky 1939b). During this period, he was assigned to monitor a Commission on the Application in Industry of Mathematical Statistics. The goal of this commission was to increase the performance of Soviet industry through application of statistical techniques. During the war, Slutsky engaged in completing a statistical handbook, *Tables for the Calculation of the Incomplete  $\Gamma$ -Function, and the  $\chi^2$  Probability Function*. Unfortunately,

he died March 1948 of a lately diagnosed lung cancer before completing the book, which was published posthumously (Slutsky 1950).

### 3 Slutsky's 1915 Contribution to the Theory of the Consumer

Slutsky's name is foremost known to any student in economics for the Slutsky equation, which introduced to economists the decomposition of the total effect of a price change upon demand into an income effect and a substitution effect. The "Fundamental Equation of Value Theory" (Hicks 1939, 309), as Hicks would later call it, was put forth in "Sulla teoria del bilancio del consumatore", an article published in 1915 in the *Giornale degli economisti* (Slutsky 1915). To grasp the importance and originality of this contribution, three points need to be addressed. First, I shall present the content and structure of the article and discuss Slutsky's motivation to submit it to this journal. In the next section, I discuss how Slutsky's contribution came to be incorporated into the modern presentation of the theory of value as developed independently from Slutsky by Hicks and Allen (1934a, b) and in Hicks' *Value and Capital* (Hicks 1939). I also account for the spectrum of influences of Slutsky's 1915 article in the second half of the twentieth century, once its widespread dissemination was made possible through publication of an English translation in 1952 (republished as Slutsky 2012).

Slutsky's interest for utility and demand theory is a centerpiece of his master thesis *Theory of marginal utility* which contains already important developments on utility theory and Pareto's ordinalism. In this work, Slutsky shows a great familiarity with the thoughts of Menger, Jevons, Pareto, Marshall, Seligman, Edgeworth, Auspitz and Lieben, Cournot, Böhm-Bawerk, Wieser (with a specific critical focus on the Austrian school). The first chapters are dedicated to a discussion of the views of psychologists—Wundt, Ehrenfels, Brentano—on will, emotions, pleasure and displeasure, and the meaning of decision. The master thesis also contains theoretical investigations on various ways to model utility, with a focus on additive utility functions. Hence, the 1915 article can be read as a direct continuation of the subject where it had been left in 1910, going beyond the Paretian analysis of the price-demand relationship and providing new results both in the case of a generalized utility function and in the case of an additive utility function.

Budget is divided into 13 sections. Section 1 takes as a starting point the opposition between the hedonist school and the "modern" (positivist) school. Members of the hedonist school take as a grounding principle the idea that each good, as a provider of pleasure, is esteemed by individual consumers through the law of diminishing marginal utility. Therefore, the hedonist school takes psychological laws as its basis and remains dependent upon unsettled (endless) disputes about measurability of psychological values. On the contrary, the "modern" school relies on another conception of utility, an index function of utility first introduced by Pareto, which



is “completely strict and abstract” (Slutsky 2012, 174), formal and independent “of all psychological and philosophical hypotheses” (Slutsky 2012, 174). This positivist view on utility—a word not used by Slutsky—assumes that utility can be recovered “by empirica data” (ibid.). Slutsky notes that, as shown by Pareto, such a function cannot be determined uniquely, thus leading, as Slutsky interprets it—to a separation between psychological analysis and economics. Going beyond Pareto’s stance, who did not recognize in full the no-bridge between utility—Pareto’s ophelimity—and an index utility function, Sect. 1 ends on a teaser, that “we shall see later how it is possible to arrive at another, better defined concept” (ibid.).

Section 2 presents the mathematics of the index utility function and repeats the main tenets of the Paretian view on the empirics of utility. Utility as an index of utility expresses mathematically through a real-valued function, the fact that the more a combination of goods is desired, the greater the *value* attributed to this combination. It is fundamentally a relativistic value, whose meaning is dependent upon the value attributed to a combination *as compared* to another. Assuming that agents are utility maximizers—by definition, searching for the most desired combination—a budget is in a state of stable equilibrium over a time span if no alternative budget affordable has strictly greater utility. It is the task of the mathematical economist to establish “stability” conditions. The fundamental goal of the economist, then, is to determine practically the utility function from the empirical data, “to face and solve the problem of the determination of the utility function by means that are practicably attainable, such as the variations of demand as a function of income and of prices” (ibid., 175). Slutsky announces that the solution to the problem is connected “with that of the possibility of an agreement between the formal and the psychological aspects of the problem of utility” (ibid., 175). Commenting on the main assumptions made on the utility function, he notes that the problem of non-derivability or variability through time can be addressed by considering groups instead of individuals, “applying statistical methods in the investigation” (ibid.).

Let  $U$  the index of utility being represented as a real-valued function  $\Phi$  of the set of quantities of goods  $\Phi(x_1, \dots, x_n)$ . Each marginal utility is assumed to be positive ( $u_i > 0$ ), and the law of decreasing marginal utility (Gossen’s law) is replaced with a distinction between satiating goods and non-satiating goods ( $u_{ii} < 0$  and  $u_{ii} > 0$ ).

Sections 3–9 present the stability conditions for the budget of the consumer (i.e., for the way the consumer uniquely allocates his/her income in between the set of goods) and then the expression of the variation in demand as a consequence of the variation in income and price. Building upon Pareto’s mathematical treatment in the *Manuel d’économie politique* (Pareto 1909), Slutsky’s aim is to obtain a more operational result. This is based on deriving first the variations of the individual’s demand as a function of income (Sect. 6), introducing the distinction between “relatively indispensable” ( $\delta x_i / \delta r > 0$ ) and “relatively dispensable” ( $\delta x_i / \delta r < 0$ ) goods (where  $r$  stands for income)<sup>7</sup> (Slutsky 2012, 183). In Sect. 7, Slutsky establishes the variation in demand as a function of the variations in price and connects it with the previous result, thus obtaining the general formula

<sup>7</sup> In Slutsky’s article, income is represented by  $s$ .

$$\frac{\delta x_i}{\delta p_j} = u' \frac{M_{ij}}{M} - x_j \frac{\delta x_i}{\delta r}$$

where  $x_i$  is the quantity demanded of good  $i$ ,  $p_j$  is the price of good  $j$ , and  $u'$  is the marginal utility of income, while  $M$  is the determinant of the matrix of second-order partial derivatives  $u_{ij}$  of the utility function bordered by the prices, and  $M_{ij}$  is the minor of  $M$  with respect to  $u_{ij}$ . This is the fundamental equation of value, as it decomposes the reaction of a consumer to a price variation as the sum of a substitution term and an income term (Slutsky 2012, Eq.46 and 47, 185). The demand for a relatively dispensable good can be abnormal in certain cases.

Slutsky also derives a property (the “law of reversibility”) of the consumer behavior (Sect. 9), namely of symmetry of “residual variation”:

$$k_{ij} = \frac{\delta x_j}{\delta p_i} + x_i \frac{\delta x_j}{\delta r} = \frac{\delta x_i}{\delta p_j} + x_j \frac{\delta x_i}{\delta r} = k_{ji}$$

The residual variability  $k_{ij}$  corresponds to the change  $dx_j$  in the quantity demanded of good  $j$  resulting from a price change  $dp_i$  accompanied by a compensating adjustment in money income such as to make possible “the purchase of the same quantities of all the goods that had formerly been bought” (Slutsky 2012, 186). In other words, the residual variability is a compensated variation of demand associated to a compensated variation of price. To Slutsky, the symmetry property  $k_{ij} = k_{ji}$  (Slutsky 2012, Eq.55, 188) is of utmost importance, since it is a quantitatively defined relation between observable quantities. It can be (and should be) confronted with data on individual budgets:

Empirical confirmation is highly desirable, inasmuch as it would demonstrate the correspondence to the truth, or at least the plausibility, of the hypothesis that the increments of utility do not depend upon the mode of variation [ $u_{ij} = u_{ji}$ ]. (ibid., 188)

From this later quotation, it seems that Slutsky endorses Pareto’s interpretation of the symmetry relation as an expression of some assumed psychological law about the effect of the order of incremental variations of consumption upon utility (Chipman and Lenfant 2002). However, he is not quite explicit about this, and it would certainly need to be confronted with his former remark about the statistical nature (possibly over several individuals) of the mathematical relationships.

Sections 10 and 11 derive specific relationships when utility is assumed to be additively separable. The rationale for discussing the case is linked to its historical importance in the development of the marginalist theory of value. Slutsky starts from discussing the assumptions on  $u_{ii}$ . Gossen’s law of satiation would imply that all  $u_{ii}$  are negative. If one discards this assumption, then the right way to address the problem is to consider the case of a system of goods, some of them exhibiting non-satiation. In the case of a system of goods for which the individual’s preferences can be represented with one non-satiating good ( $u_{jj} > 0, u_{ii} < 0$  for all  $i \neq j$ ), Slutsky discusses a result obtained by Ricci (1904), that the demand for the non-satiating good could be increasing. He shows that the conditions obtained by Ricci for increasing demand

are incompatible with the “stability conditions” for the consumer (i.e., second-order sufficient conditions for a constrained maximum), hence “cannot occur in reality” (ibid., 191). Actually, for a system such that one good is non-satiating, and all other goods are satiating, either all goods will be normal, or there is a possibility only for a satiating good to be inferior and to exhibit  $\partial x_i / \partial p_i > 0$ .<sup>8</sup>

To some extent, Slutsky’s attitude toward the theory of utility is ambiguous. The point is that although Slutsky regards the hedonist view as intrinsically wrong (as will be seen in his criticism of Böhm-Bawerk), he takes as a scientific issue the question of whether some assumptions about the utility functions (additivity, signs of the partial second derivatives) would mirror some behavioral properties in the price-quantity space. Beyond pushing forward Pareto’s analytical results, Slutsky’s main motivation is driven by the search for empirically meaningful definitions of the relationships between goods and consumers. A running idea in the article is to provide criteria (by means of equalities or inequalities) expressed as relationships between *observable relationships* or *empirically measurable data*. Accordingly, the Slutsky equation is based on a notion of compensation that is observable under specific conditions. This empirical meaningfulness of the theory of the consumer is at stake in the two final sections of the article (Sects. 11 and 12) where Slutsky aims at discussing precisely the relationships between utility and individual behavior, answering the heated question at the core of the marginalist theory of value of whether there are any empirical counterparts to the signs of the second derivatives of the utility function  $u_{ii}$  and  $u_{ij}$ . As can be expected, since those properties are assumed independently from any system of signs in the system of goods, the answer is negative.<sup>9</sup> Formally, Slutsky expresses  $u_{ii}$  and  $u_{ij}$  as two functions of (i) the marginal utility of income, (ii) various determinants implying only observables ( $\partial x_i / \partial r$ ,  $k_{ij}$ ), and (iii) a term  $\theta$  which it is *impossible* to express “as a function of empirical data” (Slutsky 2012, 194–196).

This outcome of the empirical theory of (index) utility applied to consumer’s behavior opens to an “irreconcilable conflict” (ibid.) with the traditional hedonistic-psychological theory. Values (and signs) attributed to an individual’s utility function can be arbitrary, without any consequence for his observable conduct. To Slutsky—and undoubtedly so to any reflexive reader—such a consequence is but perplexing, “because, even though attaching great importance to the absolute logical independence of the methods of economic science from those of psychology, we could not ignore the existence of a very complicated interdependence between the *facts* studied by the two sciences” (Slutsky 2012, 197). In retrospect, the only way out of this dead end would consist in establishing new psychological facts, involving at least some cognitive elements. Slutsky’s attempt at finding out a bridge from the data of con-

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<sup>8</sup> Slutsky’s contribution to the additive utility case is discussed at length in ?.

<sup>9</sup> Fundamentally, only assumptions pertaining to the whole system of goods are likely to exhibit meaningful empirical properties independent of any arbitrary increasing transformations of the utility function. The additive utility assumption plus one increasing marginal utility is precisely an example of this, as is Chipman’s case of Auspitz–Lieben–Edgeworth–Pareto complementarity between all pairs of goods (Chipman 1977) (see also ?).

sciousness to an empirical utility function is soon abandoned. This way of solving the problem is much too fragile to stand as solution.<sup>10</sup>

Section 13 draws the lessons from the full development of the theory of index utility functions. The whole apparatus of categories and assumptions based on  $u_{ij}$  (including the Paretian distinction between complementary and competitive goods) is deemed useless “if one remains loyal to the formal definition of utility, for it is impossible to deduce from the facts of conduct the character (i.e., the sign) of the second derivatives of utility” (Slutsky 2012, 198), which then must remain disconnected from any belief in some sort of “*internal evidence*” about the consciousness of motives by which we are guided. Here, Slutsky, though he embraces the formalist view on utility, is unable to discard the grounding assumption of some psychological parallelism between empirical behavior and some inner value of things to the consumer. He would make one step forward in his critical analysis of Böhm-Bawerk. In any event, Slutsky does not consider that precise and complete empirical data could conceivably be obtained to solve this question, except perhaps by way of an experiment. Thus, the future of the theory of utility depends on the development of experimental investigation.

## 4 Reception and Importance of the 1915 Article

It is not exaggerated to claim that Slutsky’s 1915 article is a landmark in microeconomics. On the one hand, it completes the Paretian revolution, almost exhausting its meaning to a point that does not seem to have been fully acknowledged. On the other hand, it provides several tools that can be implemented in demand analysis, in accordance with the positivist and empirical orientation that would become prominent in the 1930s. More than that, Slutsky’s decomposition has been adapted in various theoretical settings throughout twentieth century, an indication of its methodological and epistemological power for modern economics. As Stigler (? , 382) put it: “The beauty and power of the essay are unique”.

To chart the fate of Slutsky’s 1915 contribution, one needs to figure out the state of demand analysis after Pareto’s breakthrough. The mathematical theory of utility and demand was then a subject of interest only to a small community of searchers scattered throughout the Western world (United States, France, Switzerland, Germany, Austria, Italy, and England) and with only few connections with each others. It is only during the 1920s that stronger connections would develop through correspondence and travels, up to the creation of the Econometric Society in 1931. By that year, the political landscape had changed in Russia, preventing Slutsky to be

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<sup>10</sup> In a nutshell Slutsky (2012, 197–198) starting from the idea that any consumer would be able to identify for himself all the situations when any two goods  $\alpha$  and  $\beta$  are independent, taking as data the list of those pairs of independent goods, Slutsky conceives of recovering from that list a complete set of equalities for  $\theta$ . Fundamentally, if ever the different empirical values obtained for  $\theta$  were to be the same, one would at best obtain a provisional utility function, likely to be discarded by any new empirical result contradicting the equalities.

one of its founding members (Bjerkholt 2017). One characteristic of this theoretical agenda is to let aside the fundamental issue of the foundations of utility, assuming that a utility function (in the ordinal meaning of the term) does exist and that individuals' behavior on markets derive from its maximization. In that way, reflection on the nature of rational behavior and its links with utility and observable behavior was ignored. It only became an issue in the 1930s, once researchers engaged in the search for an axiomatic representation of rational behavior and discussed the famous integrability conditions—i.e., conditions on a set of observable price–quantity behaviors to deduce an economically meaningful generating utility function.<sup>11</sup>

The fact that the potential of Slutsky's article went unnoticed in published work until 1933 and that tribute would be paid to its contribution only after similar results had been obtained by Hicks and Allen (1934a, b) has triggered the interest of historians of economic thought (Chipman and Lenfant 2002; Bjerkholt 2014). The rationale for Slutsky's endeavor is still a matter of speculation. According to Barnett (2011, 40 sq), Slutsky's contribution could be linked with the interests of several economists in Russia who dealt with the analysis of peasants budget at a microeconomic level, in search for methodologically sounder foundations for such studies, within a war context when natural experimental conditions of price and income variations were operating. Though nothing in the article justifies this circumstantial connection, nonetheless Slutsky puts emphasis on the formulation of empirical relationships that could be confronted with actual observable behaviors, and it is reasonable to think that he conceived of the compensated demand and the income–demand relations as relations to be implemented in empirical studies. However, Slutsky's interest for the marginalist theory of value and the relationships between individual psychology and consumer behavior is the main topic of his Master thesis (Slutsky 2010), which contains already a discussion of Jevons, Menger, Pareto and the contributions of German–Austrian schools of psychology (Wundt, Ehrenfels, Ebbinghaus, Brentano).

Many causes have been put forward to account for the fact that Slutsky's article did not catch attention for at least fifteen years. “Sulla teoria del bilancio del consumatore” was published in Italian in *Giornale degli Economisti* during war times.<sup>12</sup> It was at the vanguard in terms of mathematical formalism, and quite at odds with the Marshallian style of thought then dominant (Johnson 1913; Edgeworth 1915): Indeed, Slutsky does not make any simplificatory assumption (such as constancy of marginal utility of income); he does not use any diagram or figure to illustrate his thoughts in a two-good case; he does not consider the concept of indifference curves.

On the basis of published information, Dominedò (1933) was the first to grasp the importance of Slutsky (1915) for the theory of value and demand. Then, it was discovered (through Dominedò) by Schultz and by Hicks and Allen. Actually, Dominedò may have known about Slutsky's article through (Ricci 1932), who acknowledged

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<sup>11</sup> Notable contributions to both issues in the 1930s are due to Frisch, Allen, Alt, Georgescu-Roegen and Samuelson (Chipman and Lenfant 2002).

<sup>12</sup> The fact that Slutsky chose *Giornale degli economisti* as an outlet is quite understandable: he was completing Pareto's theory on utility published in the same journal and he commented on previous work by Ricci in the same journal. Also, *Giornale degli economisti* was in those years the journal most open to publishing mathematical economics.

Slutsky's criticism of his account of the additive utility case (Ricci 1904). Clearly, Ricci did not capture the importance of Slutsky for the theory of substitutes and complements since he did not mention him in his article on the subject in *Econometrica* (Ricci 1933). The same could be said about Rosenstein (1933), who mentioned Slutsky in relation with the consciousness of economic conduct.

The first author who grasped the importance of Budget is Schultz. The discovery of the relevance of Slutsky's paper takes place within a context of growing interplay between pure economics and statistical analysis, which developed during the 1920s in the USA. During this span of time, Lehfelddt, Holbrook Working, Moore, Schultz, Frisch, Gilboy, and others would confront methods to implement statistical methods (Pearson) into the theory of supply and demand functions (Morgan (1990), chaps. 5 and 6). By the beginning of the 1930s, statistical studies of demand came into endless debates about time trends and the best method to obtain the "true" demand curve. Under constant marginal utility of money, Schultz had obtained symmetry conditions on cross demands for which statistical tests were disappointing (Schultz 1933). He was thus looking for more severe theoretical properties in demand theory that could be used as a guiding principle in statistical studies of demand. We know that Schultz and Friedman (then Schultz's assistant) came across Slutsky's contribution around 1933–1934 and took benefit of his analysis in "Interrelations of Demand, Price, and Income" (Schultz 1935) (see Chipman and Lenfant 2002, 563sq).<sup>13</sup> Slutsky's article was providing the much wanted additional constraint at the individual level, offering as a second bird a new definition of complementarity (Schultz 1935, 481), which he favored over Hicks and Allen's definition (Schultz 1935, 447). Schultz praises Slutsky's operational spirit. The Slutsky equation contains a concept of compensated demand that can be observed empirically under some specific circumstances. Assume that an individual is subjected to changes in prices and income in such a way as to be able to observe his behavior when his new income allows him to buy exactly the same basket as before the price-income change, this would permit to identify a compensated price change under Slutsky's definition of it (but not a Hicksian compensated demand, which is based on the constancy of individual utility). However, Schultz identified that Slutsky symmetry condition is theoretically valid only at the individual level and not preserved at the aggregate level, while statistical data deal with aggregates. Consequently, statistical investigation on Slutsky symmetry did not improve on Hotelling (1932) simpler conditions.

In February 1934, the first part of Hicks and Allen's article appeared in *Economica* (Hicks and Allen 1934a). It contained a decomposition of the elasticity of demand for a good into a term involving the income elasticity of demand (i.e., an income effect) and an elasticity of complementarity (i.e., a substitution effect in modern terminology). Shortly after, Allen (1936) would acknowledge the pioneering work of Slutsky and Hicks (1937, 13). Their attention was driven to Budget through "various

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<sup>13</sup> Bjerkholt (2014) has documented in detail Schultz trip to Europe in 1933–1934, his contacts with Dominedò, Hicks, Allen, Robbins, Rosenstein-Rodan, and the fact that an ever-recurring subject of discussion was the treatment of complementary and substitute goods in demand theory.

references” (Allen 1950, 210) to it after they had come independently to similar results.

As a complement to published testimonies and recollections, we learn from Bjerkholt (2014) that Slutsky was not able to disseminate his article as he would have wished to do, only receiving the offprints of the 1915 paper in 1926. Besides, his attempt to inform Frisch (September 1926) about the content of the article was not met with success. After the publication of *Summation* (Slutsky 1927a), Slutsky’s fame increased abroad, and this too may have contributed to making his previous works more visible.<sup>14</sup>

Precisely, the interpretation of Slutsky symmetry condition has been a subject of heated debates in the 1935–1950 period, as it was recast within the issue of integrability of demand. Some authors like Allen rejected the symmetry condition, while others took it as an established theoretical truth of the theory of rational behavior. Empirical rejection could indicate an irrational behavior. The subject was closed (except for particular cases) by Samuelson, once the axiomatic setting of the theory of revealed preferences was put to an end with the Strong Axiom of Revealed Preferences (Samuelson 1950).

Slutsky’s equation and Slutsky symmetry conditions have become a cornerstone in microeconomics and have been applied to various fields of study beyond demand theory. Instances are the generalization of Slutsky equation when prices enter the utility function (Kalman 1968), when choice involves risky assets (Sandmo 1969) or contingent commodities (with no effect on expected return) (Fischer 1972) work-leisure trade-off (Gilbert and Pfouts 1958), optimal taxation (Diamond and Mirrlees 1971). Application of Slutsky decomposition for intertemporal choice or demand for durables turns out to be more difficult without specific assumption on the utility function.

Slutsky’s symmetry conditions and Slutsky equation have been central also in the development of aggregate demand systems in econometrics. Notably, the use of Slutsky symmetry conditions at the aggregate level assumes a representative agent or specific aggregation conditions.

An interesting test for the power of Slutsky’s theory of demand is whether it would still be an inspiration after the 1970s and 1980s, once the hopes to build demand systems on simple empirical features and rationality assumptions had vanished (Deaton and Muellbauer 1980). The principle of analyzing separately income effects and substitution effects at the individual or aggregate levels has proved fruitful, even in a framework disconnected from utility maximization arguments. For instance, Hildenbrand’s (2014) attempt at providing a socio-economic rationale for the market law of demand bears on the principle of identifying conditions on the aggregate income effect that are empirically supported. In technical terms, given the

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<sup>14</sup> Bjerkholt (2014) also documents that references to Slutsky appeared here and there. Thus, a brief summary was published in *Rivista Internazionale di Scienze Sociali e Discipline Ausiliarie* in 1915 and it was listed in the December 1915 issue of the *Economic Journal* (also in Murray 1915). In our view, this is confirming the fact that Slutsky’s paper could not attract attention as long as its potential for statistical demand studies based on utility foundations was not grasped and put to the fore by Schultz.

negative semi-definiteness of the average Slutsky matrix (matrix representing the compensated substitution terms), it is enough to assume that the dispersion of aggregate demand increases with income to obtain the Law of Demand. In other words, even though Hildenbrand operates a major shift as regards the principle of individual utility maximization, he nonetheless builds his whole argument on a decomposition between an income and a price effect that originates in Slutsky's Budget (see Lewbel 1994 for a discussion).

Finally, one may wonder what will remain of the Slutsky equation demand properties within the behavioral paradigm. This is open to question. It has been upheld (Gabaix 2014) that measures of deviations from the Slutsky symmetry and negative semi-definiteness conditions in a model of bounded rationality might serve to measure "behavioral inattention". This view certainly demands very much in terms of individual rationality and ignores the lot of experimental results that oppose such an instrumental use of Slutsky (1952). It remains that Slutsky's exact stance as regards the proper role and nature of empirical or experimental data in demand theory and his contribution to the empiricist turn in economics needs a careful interpretation (Hands 2010).

## 5 Slutsky's 1927/1937 Contribution to the Theory of Random Fluctuations

The other influential article by Slutsky appeared in 1927 in Russian in an outlet of the Moscow Conjunction Institute (*Voprosy kon'yunktury*) and was titled "The Summation of Random Causes as the Source of Cyclic Processes" (Slutsky 1927a). The goal of the article is to highlight that—and examine how—random series, under the effect of some lagged and weighted composition rule, can generate correlated series of values that exhibit non-erratic wave movements. This time, Slutsky's article did not escape notice. The translation of Slutsky's article into English was soon undertaken and its publishing announced in the first issue of *Econometrica*, to appear only in 1937 (Slutsky 1937b).<sup>15</sup> Between 1927 and 1937, the detailed summary that accompanied the 1927 article circulated among mathematical economists. "Summation" incorporated "a number of important results obtained after 1927" (*ibid.*, editor's note, 105fn), while some tables were not reproduced. In this section, I present the content of the article. The goal of Summation is to challenge a common view on economic cycles that bears, one way or another, on superpositions of regularities in economic or natural events to account for economic cycles. According to this approach of cycles, exemplified by Moore (1923), random components are being assumed only to explain slight deviations from pure sinusoidal fluctuations (or harmonic fluctuations). Turning its back from this approach, Slutsky proposes to inquire whether the two main features of cycles—"the undulatory character of the processes and the approximate

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<sup>15</sup> The spelling of Slutsky's name in the *Econometrica* article is "Slutzky". We have used the usual spelling.



regularity of the waves” (Slutsky 1937b, 107)—could be generated from a different starting point, assuming solely random causes and their combinations. This is the theory of chance waves. The novelty of the method is vindicated by Slutsky:

The method of the work is a combination of induction and deduction. It was possible to investigate by the deductive method only a few aspects of the problem. Generally speaking, the theory of chance waves is almost entirely a matter of the future. For the sake of this future theory one cannot be too lavish with experiments: it is experiment that shows us totally unexpected facts, thus pointing out problems which otherwise would hardly fall within the field of the investigator. (Slutsky 1937b, 107)

The fundamental statistical idea of the article is this. Consider an “incoherent” series of values representing a phenomena, that is, a series whose values are disconnected from each other, each value being uncorrelated to its previous and upcoming value in the series. If, on the basis of such a series, one considers the construction of other series through various processes of summation (and so on possibly with resulting series), then one may isolate in the derived series patterns of behavior typical of cycles. To develop an analysis of this mathematical-statistical phenomenon, Slutsky argues in five steps. The first step consists in defining what makes a coherent series as against an incoherent (random) one. The second step consists in building various coherent series from incoherent series that shall serve as test material for further analysis, similarities, and difference between those derived series being taken as the data of inductive reasoning. In this respect, Slutsky has been identified as a pioneer of simulation in economics (Orcutt 1960). The third step consists in establishing the undulatory character of the derived series, pointing out their properties of graduality and fluency. The fourth step deals with the regularity of the waves, an issue that “offers considerably greater difficulties” (Slutsky 1937b, 118), involving Fourier’s harmonic analysis and leading to identify a succession of “regimes” of cycles. The last step is a discussion of the pseudo-periodic character of the cycles. A mathematical appendix on random waves completes the whole. In the following, I come back on each step.

To Slutsky, a fundamental starting point—which shall be related with his views on the foundations of probability—is a distinction between two kinds of chance series. They can be either coherent or incoherent. A coherent series is characterized by the fact that, within the finite number of terms that make this series, one can identify correlations. The most important of these correlations is the one linked with the distance between terms. The coefficient of correlation, for such coherent series, can be expressed as a function of the distance between the terms correlated (the *correlational function* of the series). Even though such coherent series might exhibit other patterns, Slutsky limits his analysis to the case when the coefficient of correlation is determined solely by the distance between terms in the series. ( $r_t$ ) figuring the coefficient of correlation of any term with the  $t$ th term following, we have  $(r_0) = 1$  and  $(r_t) = (r_{-t})$ .<sup>16</sup>

<sup>16</sup> Note that Slutsky’s notions of incoherent and coherent series are defined for finite series, and are probably conceived of as two relevant notions for empirical work: “I venture to propose this name [incoherent series] because it seems to me that it truly expresses what is intended, namely, the

As a starting point for manipulations and inductive reasoning, Slutsky takes two experimental series obtained from the drawing of numbers for a national lottery, from which he extracts three incoherent basic series.<sup>17</sup> As for coherent series, Slutsky views them as the offspring of incoherent ones, due to “an especially prominent role ... played in nature by the process of *moving summation* with weights of one kind or another” (Slutsky 1937b, 108). Of course, coherent series once generated can in turn generate other coherent series through the same process of moving summation.

Consider the value of an event variable  $y$  at moment  $t$  as the “consequence” of several values taken through time by another causal variable  $x$  such that each consequence  $y$  is obtained through a linear combination of a finite incoherent series of  $n$  past values of  $x$ , with weights  $A_0, A_1, A_2, \dots, A_{n-1}$ . We get:  $y_i = A_0x_i + A_1x_{i-1} + \dots + A_{n-1}x_{i-n+1}$ . The same rule applied at various moment in time provides us with a series  $(y_i, y_{i-1}, y_{i-2}, \dots, y_{i-n})$ . Two adjacent values in the  $y$  series share a set of  $(n - 1)$  common causes, and the system of weights appears in a lagged way. This in itself is sufficient to make the terms of the derived series (or “consequences”) correlated to a certain extent with one another, “even though the series of causes are incoherent” (Slutsky 1937b, 109).

Using basic series, Slutsky then builds various derived series. One is obtained through ten-item simple moving summation. Another one is obtained by iterating the same process with the derived series. Still another one through iterating a two-item moving summation twelve times and a last one using specific weights based on the values of a Gaussian curve at given intervals. All these transformations are intended to provide a rich material—a set of models—upon which inductive reasoning can be implemented. “We could not be satisfied by a smaller number of models because it was necessary to observe their various properties and to have illustrations for the elucidation of the different aspects of the problem”. (Slutsky 1937b, 111–113). It is by selecting various such ways of composing incoherent series that one gets “an inductive proof ... that the summation of random causes may be the source of cyclic, or undulatory processes” (ibid., 114).

Then comes the task of assigning properties of graduality and fluency to those waves. The principle of graduality states that within a series of correlated items variations from minimal to maximal values is gradual in the sense that small difference between values of neighboring items is more probable than between more distant ones, thus do not look like irregular zigzags. Fluency concerns the property of the series of first difference of adjacent values of a series. In the case of a simple moving summation, the series made of adjacent differences  $y_i - y_{i-1}$  contains uncorrelated elements (except for  $\Delta y_i$  and  $\Delta y_{i+n}$ ); thus in that case, we have graduality without fluency.

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existence of some connection between the elements of parts of a thing (for example, of a series), but not a connection between this thing as a whole and another.” (Slutsky 1937b, 108, fn7).

<sup>17</sup> The first basic series is obtained through selecting only the last digit of the original series and the second one through replacing even numbers with 0 and odd numbers with 1. The second method was applied to another original series to obtain the third basic series.

The more derived series are produced through iterative processes of moving summations of random causes, the more they exhibit fluency of the undulatory process. Later in the article, Slutsky establishes also a tendency toward a sinusoidal form of series derived from random causes (i.e., tendency for second differences of the series  $\Delta^2 y_i = (y_{i+2} - y_{i+1} - (y_{i+1} - y_i))$  to be strongly negatively correlated with  $y_{i+1}$ ).

That first result being established, the next step is to demonstrate “*the approximate regularity of the waves*” (Slutsky 1937b, 118). This is, as Slutsky himself admits, a difficult task that he confronts first in an inductive way. Starting from the various experimental models built, Slutsky explains that a statistical treatment of the data in order to discard ripples from waves leads to identify relevant local maxima and minima, thus describing a number of half-waves of various length.<sup>18</sup> It is then possible to calculate the frequency distribution of these waves. Waves identified with Model II exhibit about the same coefficient of variation as empirical waves observed for 12 countries.<sup>19</sup> Coefficients of variations and average lengths of waves are smaller for the other models. Here, Slutsky argues against Mitchell’s attempt a denying regular waves. The apparent absence of periodicity is the result of a “tendency to stick to a purely descriptive point of view” (Slutsky 1937b, 119) and to ignore “imperfection of a visual impression” (Slutsky 1937b, 121). Crude evaluations of empirical results must be checked against alternative constructions of data: “Those investigators of economic life are right who believe in their acumen and instinct and subscribe to at least an approximate correctness in the concept of periodicity of business cycles” (Slutsky 1937b, 119). Thus by confronting data from series of Models II and III with sums of the first five harmonics of Fourier series, Slutsky comes to the conclusion that a small number of harmonics are enough to obtain a significant correspondence in terms of periodicity of waves (at least for a subset of values corresponding to six periods): “This hardly can be considered to be a chance occurrence; the explanation of such an effect must be found in the mechanism of the connection of the random values”. (Slutsky 1937b, 122). Beyond periodicity, Slutsky identifies regimes of cycles, that is, sequences of periodic waves with specific parameters. Transitions between regimes occur “sometimes rather gradually, sometimes more or less abruptly, around certain critical points” (Slutsky 1937b, 123). That way, Slutsky points out that non-stationarity is a component of the analysis of cycles generated by random events.

In the last section of the paper, Slutsky inquires about the pseudo-periodic character of series. He does this through a physical metaphor echoing Wicksell’s “rocking horse”. Consider a system with damped oscillations constantly under random shocks that feed the system with energy. What would the system look like through time? Slutsky’s answer is that if one were to consider a very long period of observation of the terms of series, the movement of the system would be reduced ultimately to a chance function, i.e., the result of “a particular instance of the summation of random causes” (Slutsky 1937b, 132). Within a given finite time span—whose length

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<sup>18</sup> This idea is supported by Husserl in *Ideas Pertaining to a Pure Phenomenology and to a Phenomenological Philosophy* titled “Deskriptive und exacte Wissenschaften”.

<sup>19</sup> Empirical data on business cycles are taken from Mitchell (1926, 32–33).

depends on the value of random shock—the process is well described as a periodic sinusoidal movement.

Slutsky's contribution is remarkable in several respects. In a nutshell, the running thesis is that the moving weighted average of identical random series generates series of consequences that are correlated with each others in such a way as generating cycles. This thesis is scrutinized through a sophisticated method of inductive reasoning implying simulation and deductive reasoning based on statistical and probability theory. As Klein notes (1999, 153, 158), Slutsky's analysis is anchored in a formalist thinking characteristic of Soviet mathematicians; it is also disconnected from materialistic causes of cycles and from any theoretical attempt at linking cycles with crises of capitalist economies. Notably, the notion of a regime change is not associated with a specific situation of crisis. As in ? (who is mentioned by Slutsky at the beginning of Summation), random phenomena are important as data generating processes. Slutsky's contribution leads to a kind of small-scale explanations, in which random causes are enough to exhibit approximate regularities over some period of time, without for all that pushing the statistician into upholding a strict forever regularity of cycles, quite opposite to what Moore (1923) had done in his *Generating economic cycles*, where he advanced a strict eight-year periodic business cycle. This way, Slutsky is not committed to providing a unique causal explanation, putting instead the principle that various regimes of cycles are succeeding one another.

## 6 Reception and Importance of the 1927/1937 Article

The fate of Slutsky's 1927 article is quite different from that of the 1915 article. As a contribution to economic cycles, it has been quickly associated to Yule's name in the "Slutsky–Yule effect". First of all, the publication contained a substantial summary that was enough to draw attention, and the publications of the Moscow Conjecture Institute were focused enough to have a watchful readership. Besides, it is known that Slutsky sent an offprint to Frisch and that it would be influential on his own theory of cycles (Bjerkholt 2007; Holta 2014, 456–457). After 1937, as the Cowles structural econometrics program and Keynesian macroeconomic modeling went under growing pressure, summation became a subject of renewed interest (Dimand and Veloce 2007; Dimand 2020), especially within the New Classical Macroeconomics movement of the 1970s–1980s. Throughout, summation has been interpreted in various ways, with only rare accounts for its methodological scope. However, its richness and absence of involvement within a specific way of theorizing about cycles or thinking about exogenous versus endogenous variables made it a prominent reference during the twentieth century (Dal Pont Legrand and Hagemann 2019).

## 6.1 *Summation Between 1927 and 1937: “Spurious Correlations” Versus “Inverted Inference”*

The early reception and discussion of Slutsky’s analysis of cycles is well documented, though archival work may still bring relevant information (Barnett 2006, 2011; Bjerkholt 2014, 2017). On the one hand, Frisch (1933) would exploit Slutsky’s random shocks only to provide exogenous impulses to his own dynamic device, warning economists that Slutsky’s analysis by itself is unsound as a foundation for economic theorizing. On the other hand, Kuznets would highlight the methodological importance of Slutsky’s contribution as a potential cause for cyclic movements. The central question that is implicit in these two approaches is: If random causes can generate cycles that look almost exactly like cycles observed in economic activity, should we consider that cycles are of necessity the result of random causes and abandon any causal economic interpretation beyond random causes or should we confine its use to a minimum set of exogenous shocks providing the proper amount of randomness to describe the facts of business fluctuations? Summation does not offer any definitive statement in favor of one interpretation over the other—though it contains enough to discard extreme interpretations—and this explains its openness to various uses and interpretations over the last century.

The first mention of Slutsky (1927a) in a published outlet seems to have been Mitchell (1927, 478) (“a most interesting analysis”) at the very end of an addenda chapter to a book ready for publication, *Business Cycles: The Problem and Its Setting*, where he notes Slutsky’s disagreement with him as regards the hypothesis of periodicity of cycles. Then, Kuznets (1929) noted that Slutsky’s analysis of random cycles provided as a self-contained source of cyclic movements (Kuznets 1929, 258). To Kuznets (1929, 258), “These results were in no way accidental. It is not only ‘possible’ that a summation of a random series will yield cycles, but also quite certain that this will be the case”. Kuznets’ interpretation makes random causes an actual source for genuine cycles, something that must be accounted for one way or another. In economic life, events may exert an influence for a while, and only for a while, on other relevant economic facts, thus giving them a shape well represented by moving summations.<sup>20</sup> Having studied the effect of various kinds of moving averages, Kuznets concludes by wondering whether such robust results obtained by Slutsky

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<sup>20</sup> “In the moving average two considerations are of importance: (1) the period taken; (2) the weights assigned to the single values. The period taken determines the time limit within which a certain event is to be counted as an effective cause. It is clear that the longer the period of the average, the longer this extension of an influence of either an exceptional item or an exceptional cluster of items. As a result the cyclical swings will tend to be of longer duration. On the other hand, the exceptional occurrences are combined with a large number of other less exceptional items, and therefore the amplitude is likely to be mild. By the same reasoning a moving average of shorter duration is likely to give a cycle that is shorter on the average and more violent in the amplitude of its fluctuations.” (Kuznets 1929, 269). Kuznets’s article presents Slutsky’s results in a pedagogic way, stressing the most important features for an economic theory of cycles. He probably contributed to making Slutsky’s ideas known to a wide readership.

may be taken as the rationale for a realistic-genetic interpretation of cycles. This is the “inverted inference” thesis:

can one ... say that ... cyclical oscillations may be conceived primarily as results of summation of random causes, and that the characteristics of some of these cyclical oscillations can best be grasped as a result of some peculiarities of the underlying random events or of the process of cumulation? (Kuznets 1929, 273–274)

Kuznets does not uphold such a radical stance; however, he intends to account for its consequences on economic theorizing:

Indeed, if one can explain how in certain processes of economic life, the response to stimuli is cumulative, then the whole discussion of the cause of business cycles becomes supererogation. If the business economy runs at a certain high or low plateau level, and the conditions of business behavior are in such shape as to cumulate favorable or unfavorable random events, then we are bound to have a cyclical up or down swing sooner or later. It is to be seen that the so-called institutional explanations of business cycles deal mainly with the economic forces that make for cumulation, with forces that explain why a given random event is not immediately canceled by an opposite reaction but allowed to exert its influence for some time to come, an economic counterpart of the statistical mechanism of a moving average. (Kuznets 1929, 274–275)

Also, Kuznets notes that differences between the features of two fluctuating series (e.g., index of production vs. shares at NYSE) may be ascribed to differences in the way past events are likely to operate through time as relevant and highly weighted causes. In other words, Kuznets, without fully endorsing an exclusively random-based rationalizing of cycles, points out that Slutsky’s results lead to reallocate the use of causality in the economic theory of cycles. Cycles are now seen as the outcome of the various ways by which economic agents and the institutional framework of an economy account for past events, be there purely random or derived from random causes. Kuznets also highlights the role of frequency distribution of random events (skewness). A critical point, not addressed by Kuznets, is to settle to what extent those behavioral features should be linked with individuals’ maximizing behavior and should be compatible with a deterministic (dynamic) system representing an economy.

Kuznets interpretation is often set against Frisch’s idea that moving average summation of random distributions leads to spurious correlations.<sup>21</sup> Two contributions by Frisch witness for his complex relationship with Slutsky’s results. Frisch (1931, 78) focused on the “Slutsky effect” as essentially the effect of applying a linear operator upon a random variable as a source of cyclical fluctuations. In other words, what Frisch did is to confine the scope of summation to a methodological paper that any econometrician or mathematical economist should take into account for understanding the undesirable effect of random causes that may blur the understanding and mere elucidation of genuine empirical cycles. Thus, random cycles are seen as a

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<sup>21</sup> In private discussion with Slutsky, Frisch reacted to the 1927 paper. In 1931, Schultz also asked Slutsky a copy of the article in order to initiate a translation (Bjerkholt 2014).

source of “spurious cycles” and should be eliminated as noisy fluctuations in order to focus on the interpretation of the deterministic component of cycles.<sup>22</sup>

Frisch’s attempt is then to devise various linear operators to be applied on time series in order to reveal the main relevant components of the series, that is, the various genuine cycles lying behind and a trend component as well. Actually, it is a matter of theory how many components will be searched for, and it will be deduced from the modeling of the main relationship at work to describe the working of a capitalist economy (a complete description being out of reach and incompatible with quantification of economic phenomena). Thus, through manipulations of time series data, it is expected both to smooth the effect of erratic shocks and to reveal genuine cyclic components of the time series (amplifying some and discarding some insignificant ones). Now, the problem that arises is simple: applying linear operators onto the series will import into it spurious fluctuations. To remove them, Frisch claims to have studied the laws of those “spurious cycles” (in terms of period and relative amplitude with genuine ones). Frisch’s interpretation of Slutsky is linked with his methodology and the way he articulates the study of data and theory. Given a time series, the first task is to identify different components in the series, then to inquire about the theoretical explanation of components, meaning by that to provide a rational explanation of the phenomenon identified in time series by way of a modeling strategy borrowed from physical systems (Frisch 1931, 74).

The relationship between theory and manipulation of data would be somewhat different in Frisch’s next venture into the theory of cycles. In his groundbreaking “Propagation Problems and Impulse Problems in Dynamic Economics” (Frisch 1933) launched classic distinctions in modern economic thought regarding dynamic and statics, exogenous and endogenous variables, and provided the first macro-dynamic model engendering fluctuations characteristic of cycles, through causal macroeconomic relationships involving few variables (net capital stock, consumption time), to build capital goods, and *encaisses désirées* as a factor influencing consumption. As compared with the 1931 article, Frisch is accomplishing a step forward in providing a broad “mechanistic” theory of a dynamic capitalist economy. This time, the treatment of time series data is left aside and what comes first is the understanding of how the dynamic system is likely to exhibit permanent fluctuations. Indeed, the dynamic linear propagation model could not account for persisting movements of swings of regular amplitude, and something more was needed to prevent the dampening of oscillations. To solve the “impulse problem”, Frisch assumed that random shocks (technological shocks) could change the value of parameters of the simultaneous equation system and trigger regular perturbations fostering the fluctuations in the system. From this purely theoretical standpoint, Slutsky’s effect is ignored and random shocks are assumed to be just sufficient in order to provide the necessary impulses whose influences over the system are entirely captured by the propagation

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<sup>22</sup> Actually, Kuznets’ contention already arose a reaction by Souter (? 72), who held the view that the economic nature of the operations leading to the cumulative reactions to random shocks needs to be addressed. Thus, the static general equilibrium model would remain an adequate paradigm to the understanding of economic dynamics.

structure. The seminal idea for this random causal mechanism is attributed to Yule and Slutsky. The study of the “*mechanism* by which such irregular fluctuations may be transformed into cycles” was undertaken “independently of each other by Eugen Slutsky and G. Udny Yule” (Frisch 1933, 198):

Slutsky studied experimentally the series obtained by performing iterated differences and summations on random drawings ... Yule only used second-order differences, but tried to interpret the random impulses concretely as shocks hitting an oscillating pendulum. By the experimental numerical work done by these authors, particularly by Slutsky, it was definitely established that some sort of swings will be produced by the accumulation of erratic influences, but the exact and general law telling us what *sort* of cycles that a given kind of accumulation will create was not discovered ... But still the main problem remained, both with regard to the mechanism by which the *time shapes* of the resulting curves are determined and with regard to the concrete economic interpretation. (Frisch 1933, 199)

In so doing, certainly Frisch reinterprets summation (Bjerkholt 2007) more than he elucidates it (Barnett 2006). He is however forced to admit this time that as a causal structure for swings, random shocks need some further analysis (this is sketched in a discussion about Schumpeterian innovations). To Frisch, the explanatory mechanism for cycles needs to be complemented to account for endogenous explanations, connected with some long term tendencies of capitalist systems to influence the path of dynamical systems (such as Schumpeterian innovations). Thus, Frisch lays the foundations for a development of structural econometric program in which random causes offer useful but still under-theorized complement to the cyclical behavior of capitalist economies (Dal Pont Legrand and Hagemann 2019). He would soon abandon his research in this direction due to flaws inherent in the linear dynamic model (Chen 2010; Louçã 2004).

## 6.2 From 1937 to Lucas

Frisch’s propagation-impulse modeling became a standard way of doing research on business cycles and has been interpreted as superior to other attempts in terms of realism (obtaining irregular cycles) and subjecting random causes and their cumulative effects to an explanatory scheme. Opposing views by Mitchell (1927) or Burns and Mitchell (1946) were famously criticized by Koopmans (1947) as “Measurement Without Theory”. What counted first was the articulation between theoretical structure and data handling as developed at the Cowles commission. As far as business cycles are part of the research program developed at Cowles, they are defined as models with dynamic properties (Dimand 2020, 565). Frisch and Slutsky as read through Frisch’s glasses appear as two basic references in Haavelmo (1938). In a structural dynamic system, each structural relation or relations obtained from their combination (confluent relationships) are not exact laws and “*must be taken as laws in the statistical sense, i.e., as average laws*” (ibid., 204). Klein (1999, 161) notes that “Slutsky’s moving average of random disturbances became the key building



block in the articulation and modeling of stationary stochastic processes. Isolating a stationary process is like constructing an inertial frame of reference”.

In the second half of twentieth century, Slutsky’s contribution would still be regularly mentioned as a methodological source for thinking about cycles. However, focus on growth and structural macroeconomic modeling leads to a disinterest in random shocks as a rationale for cycles (Duarte and Hoover 2012).

Along the Burns and Mitchell tradition, Adelman and Adelman (1959) offer a critical assessment of the Klein–Goldberger model of the American economy (Klein and Goldberger 1955). They provide a test aimed at distinguishing between economic series generated by the Klein–Goldberger model and pure random series. The question is whether the KG model is supported to model the dynamics of a modern industrial country. This is done first by constructing the values of exogenous variables in the model through a linear trend. A simulation of the model with those values leads to a smooth linear system. The next step assumes that cyclical movement originate from random shocks on this exact trend, in accordance with Slutsky and Frisch (Adelman and Adelman 1959, 606, fn19). To the authors, the necessity of adding random shocks to obtain cyclical movements comes from the impossibility to model a purely micro-founded model whose behavioral equations would strictly proceed from a heterogeneous set of individuals. Ignoring heterogeneity leads to inexact behavioral assumptions at the macroeconomic level. Imposing some type of random shocks in the model leads to oscillations of periods and amplitudes similar to those observed empirically. The cyclical fluctuations obtained through stochastic impulses are similar to those described by the NBER as characterizing the US economy. However, one should not conclude “that the type of perturbations actually responsible for the observed cyclical behavior are exogenous to economic theory in general” (Adelman and Adelman 1959, 621).<sup>23</sup>

Even though Slutsky’s contribution had been confined to a provider of random shocks, issues about serial correlation and the role of random causes in the analysis of propagation mechanism were still relevant in the Cowles program of structural economics in the 1970s (Heckman 2000).<sup>24</sup>

The revitalization of Summation as a methodological and even foundational reference took place in the 1980s, when it became a reference in the rational expectation revolution in macroeconomics, through the works of Lucas and the rational expectations revolution on the one hand and real business cycle theorists on the other hand.

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<sup>23</sup> Adelman (1960) complements this article with a methodological contribution involving the comparison of the Klein–Goldberger model plus random shocks versus an artificial model made of accounting equations. To Adelman, it is expected that the approximation obtained with a structural model such as the Klein–Goldberger is much better than the one obtained with a model with minimal economic content.

<sup>24</sup> Some aside comments on Slutsky’s discovery deserve mention. In a book titled *L’ère logique* (1969) Bureau, a man trained as agricultural engineer who is notably remembered as an enthusiastic promoter of jazz in France through the association *Hot Club de France*, interpreted Slutsky’s random cycles as an expression of a “microclimate of chance events”. In 1976, the book *Hasard et Prévision* (?) by Zajdenweber contained a comprehensive comment on Slutsky’s discovery.

Actually, among pioneers of cycle analysis, Slutsky is probably the most consensual reference for new classical macroeconomists.

Interestingly, the somewhat instrumental use of Slutsky and Yule work would evolve in the context of the rational expectations revolution. In his seminal article, Muth (1961, section 5) confronts the usual empirical implications of a cobweb-type model with that obtained through rational expectations plus random shocks. “From a purely theoretical standpoint, there are good reasons for assuming rationality. ... The only real test, however, is whether theories involving rationality explain observed phenomena any better than alternative theories.” (Muth 1961, 330). An empirically confirmed implication of the Cobweb model is the quasi-periodic fluctuations in prices of a number of commodities. However, the same pattern could be obtained through “a dynamic system forced by random shocks” (*ibid.*, 333). As one can understand it, the fact of random shocks blurs the interpretation of dynamic systems, a view that leads us back to Frisch (1931): “Slutsky and Yule first showed that moving average processes can lead to very regular cycles. A comparison of empirical cycle periods with the properties of the solution of a system of differential or difference equations can therefore be misleading whenever random shocks are present”. (Muth 1961, 133).<sup>25</sup> However, the rational expectation assumption leads to associate closely unexpected events with their exclusive counterpart (the difference between expected prices and actual prices).

Lucas’ (1980) epistemology of modeling stresses the critical importance of random shocks. Against structural modeling, Lucas advocates that an economic model must be erected as an artificial construct, based only on some behavioral and informational assumptions (rational behavior, rational expectations, structurally efficient information). Lucas’ view echoes Adelman’s idea of testing a model against another through its differential performance when subjected to random shocks: “not all well-articulated models will be equally useful. ... we need to test them as useful imitation of reality by subjecting them to shocks for which we are fairly certain how actual economies, or parts of economies, would react” (Lucas 1980, 696–697). The contribution of Slutsky (1937b) (and that of ?) are interpreted as independent advances in statistical and economic theory that promoted “the idea that one might describe an economy as a system of stochastically disturbed difference equations, the parameters of which could be estimated from actual time series” (Lucas 1980, 701).<sup>26</sup> In this respect, Lucas promotes a unified modeling of random shocks and propagation mechanism that does more justice to the ideas of Slutsky than did Frisch (1933), a view also set forth by Lines (1990) about Lucas’ (1975) equilibrium business cycle model in which erratic monetary-fiscal shocks undergo lagged information processing.

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<sup>25</sup> Here, Muth follows Haavelmo (1940) who alerted economists about the risk of introducing into the theoretical structure things borrowed from empirical data, that are merely the effect of random causes.

<sup>26</sup> A statement by Lucas echoes also Slutsky’s ideas that within a long time span, the apparent regularity of cycles is blurred and becomes a series of random events “If the Depression continues, in some respects, to defy explanation by existing economic analysis (as I believe it does), perhaps it is gradually succumbing to the Law of Large Numbers” (Lucas 1980, 706).

The importance of Slutsky has been underlined also within the group of real business cycle theorists. Hartley et al. (1997) show how RBC theory mixes layers of methodological underpinnings and strategies and complete recasting of the rules for assessing them. They also point out the weaknesses in their attempts at incorporating dead economists on their side. RBC theorists after Kydland (1982) have promulgated several new principles tied together to erect a new view on cycles, now viewed essentially as optimal paths of reactions of agents to present shocks (interpreted as temporary or permanent) in the economy. The group of new classical macroeconomists promoting RBC puts to the fore new methodological principles and a new language (rational expectations, microeconomic foundations based on a representative optimizing agent credo, calibration, perfectly competitive economies with efficient, clearing markets, and random shocks—essentially technological—as the necessary complement to the intertemporal coordinated decision plan of Crusoe’s economies). According to RBC, the usual business cycle models linked to the structural econometrics and Keynesian tradition are mistaken in their attempt at separating trends from cycles, at building business cycles as a necessary complement to growth theory.

Prescott (1986) and Kydland et al. (1990) depart radically from the standard view of the 1950s and 1960s that cycles are the outcome of endogenous phenomena around a trend, as exemplified in Goodwin (1967). Instead, they advocate real shocks are constantly operating on the economic system, triggering optimal reactions from individuals and permanent consequences on the economy with no tendency to come back to a previous trend, whose values are inferred from neoclassical growth theory. The whole visualization of cycles is discarded. This new interpretation of business fluctuations is often associated with a come back to Frisch and Slutsky.

Another aspect of Slutsky’s influence on RBC theorists is linked with the methods used to detrend data. Since Solow’s growth model is used to define the steady state path of macroeconomic variables (consumption, investment, output), it is necessary to produce these data. However, nothing justifies a constant trend at all times over a period; hence, the trend and the deviations from trend at each time should be estimated jointly. In practice, RBC theorists apply a technic of filtering data that relies on some a priori specification of the trend. These filtering technics (notably, the Hodrick–Prescott filter) can induce artificial fluctuations or correlations between series that are not present in the original data, which are interpreted as spurious correlations. In this matter, RBC theorists fail to adopt the same rigorous treatment to data that Slutsky had done in his simulations (induction on the basis of a rich set of simulations). Hence, the whole stage of detrending appears as a source of creation of artificial correlations and fluctuations around an artificially created trend, thus casting a doubt on the whole interpretation of facts (the steady state) against whose competitive theories might be erected (Hartley et al. 1997).

In “Understanding Real Business Cycles”, Plosser (1989) imagines Crusoe’s reactions to productivity shocks (temporary or persistent), the channels by which intertemporal substitution leads to reasonably expected correlations between time series of consumption, investment, labor time, output ... Productivity shocks could as well be preference shocks. To Plosser, it is common to think of the business cycle

as separate from growth and to characterize the cycle as deviation from a smooth deterministic trend that proxies for growth, due to productivity shocks that follow a random walk. “While rarely explicitly recognized, tests of these business cycle theories are actually joint tests of the model for growth (the trend) and the model for the cycle” (5). What is retained from Slutsky, then. It is mainly an output of the summation of random causes, the fundamental fact that they generate fluctuations and not cycles: “My own preference is to use the term ‘fluctuations’ since ‘business cycle’ frequently carries the connotation that there is true periodicity present in economic activity. Virtually all of modern macroeconomics dismisses the view that there are actual periodic cycles in economic activity. Instead it follows the important work of Slutsky (1937) and interprets the ups and downs in economic activity as the accumulation of random events or a stochastic process” (Plosser 1989, 54).

Real business cycle theorists do not engage in the understanding of the propagation process as did Lucas. Yet, Slutsky is providing the rationale for understanding the claim that random shocks can generate various kinds of reactions on the part of economic agents that eventually exhibit regularities in co-movements of some fundamental macroeconomic variables and “fluctuations” rather than “cycles” (Prescott 1986). The identification of Slutsky as an autonomous reference (kept apart from Frisch) is confirmed in Kydland et al. (1990):

An entirely different way [from that of Frisch] of generating cycles is suggested by the statistical work of Eugen Slutsky (1937). Slutsky shows that cycles resembling business fluctuations can be generated as the sum of random causes—that is, by a stable, low-order, stochastic difference equation with large positive real roots. (Kydland et al. 1990, 6)

The recognition of Slutsky’s summation as a mold for thinking about random causes of economic fluctuations in the real business cycles tradition is now well established (Chatterjee et al. 2000; Ramey 2016).

Further advances in the assessment of lasting influence of summation need to address more directly Slutsky’s inference from his own simulations with random series. This is done in passing by Blanchard and Watson (1986) who attribute to Frisch and Slutsky the use of the propagation-impulse framework. Both are identified with the idea that small random shocks transmitting energy to the propagation system, while other authors would focus on ceiling and floor assumptions (e.g., Hicks 1949, see also Eckstein and Sinai 2007). The article discusses the characteristics of shocks that provide the best emulation of cycles and concludes that large infrequent shocks provide a better description of cycles than do frequent small shocks as exemplified by Slutsky and reinstated by Lucas (1977). They conclude that economy is best described through a mixture of infrequent large shocks and more frequent small shocks of various nature (monetary, supply side, demand side, fiscal). The influence of Slutsky’s methodology as a representation of the causes of regular fluctuations in the economy has thus been revived since the 1980s. Within the context of a general increase in strong shocks on economic agents (financial crises, extreme meteorological events, pandemics), the interest for deepening the understanding of various probability distributions of events, notably the consequences of skewness, is well in tune with the spirit Slutsky’s 1927/1937 article.

## 7 Slutsky's Other Contributions: Recharting Economic Ontology

Slutsky's two famous and long-lasting contributions to economics deserve recognition owing to the methodological questions they address to economics and economists. Budget opens to the question of the proper balance between observable behavior and rationality assumptions in consumer theory. Summation leads to address the issue of how economic systems transform random events into structured economic behaviors through layers of intermediate phenomena involving human behaviors and institutional settings supporting them. It is probable that those two contributions will remain to some extent open systems for interpretation. Nevertheless, Slutsky's lesser-known contributions to economics may shed some light and help build a bigger picture of his views on economics.

Three articles deserve peculiar attention. The first one is a short presentation of Petty's thoughts (Slutsky 1914). The second one is a critical assessment of Böhm-Bawerk's theory of value (Slutsky 1927c). The third one is a programmatic article on the praxeological foundations of economics (Slutsky 1926a).

"Sir William Petty" appeared in 1914. The content of this short article was originally presented on November 1913 at the Society of Economists in Kiev and published first in the student's bulletin of the Kiev Commercial Institute. It presents the ideas of a "wonderful economist" to readers together with a translation into Ukrainian of a selection of Petty's writings covering various subjects (Barnett 2011, 130). Under the Marxist doctrine, Petty was seen as a bourgeois economist. Slutsky wanted to let know to readers that Petty's work could be read as a pre-classical theory of value, whose richness and originality might be a source of inspiration for thinking about economics. After an exalted presentation of Petty's life and brilliancy, Slutsky presents his political-economic views, which he deems of "greatest interest to us" (Slutsky 1914, 132). In Petty, Slutsky views the thinker who rejects metaphysical thinking in favor of "numbers, weights and measures". Slutsky links Petty's appeal to natural price with that of just price and market price, "and the very concept of justice in market affairs is discussed as the result of mutual voluntary evaluation of both parties acting with normal good conscience" (ibid., 133). And Slutsky goes on:

Thus we should represent this as being that the concept of a natural price is a free price, forming outside the actions of whatever power there might be, in only one mutual action between sellers and buyers freely competing with each other. (ibid., 133)

Slutsky criticizes Marx for presenting Petty simply as a predecessor of the labor theory of value. Writing that "labour is the father and active beginning of wealth, as the land is its mother" (from *Treatise on taxes and contributions*, 1662) cannot be taken as a formulation of the labor theory of value. To Petty, at best, Slutsky goes on, "labour is not the substance of value but merely a factor that defines the exchange proportion" that is a relative price (extrinsic value in Slutsky's wording). The notion of natural price is altered by several factors affecting agent's behavior (habits and principles, monetary influences, providing a "reasonably defined empirical theory of value formation, which should not on any account be confused with Marx's point of

view, which considers the actual substance of value to be in the quantity of expended (socially necessary) labour” (ibid., 135–136).

Slutsky is manifestly in proximity with Petty’s endeavor to search for empirically meaningful measures or comparative measures (quantity of food produced by land without added work; equivalence between types of food). Though he rejects several regulations, “[Petty] is not blinded by the phantom of automatic harmony” (ibid., 140). At some points in the article, Slutsky makes interesting digressions, exposing his opinion on some important matter, notably as regards intertemporal valuation, introducing a distinction between immediate feeling and a rationally constructed view on value. If I exchange a durable good A for a disposable good B, once B has been consumed, I am forced to consider the future utility accruing from the abandoned good A as “lower than its future significance”. Thus, Slutsky points out already an argument that will be developed in his critique of Böhm-Bawerk, an essential distinction between “utility” evaluation and a more general evaluation of “significance”.

We cannot escape our capability to look ahead into the future and be concerned about it, and this creates disharmony in our evaluation of economic goods: *the economic evaluation of the significance of a particular good does not exhaust our attitude to the good*. Were this not so, then there would be no place for the concept of exploitation, since in any and every exchange one would choose that which had the greater subjective value for him, rather than that which he gave away. (emphasis added, ibid., 141)

As a concluding remark to this presentation, Slutsky underlines that the economist cannot dispense from being engaged in the philosophical foundations of his own approach:

To bring the study of political economy to this state of wholeness, to link all its sources with all the cultural, scientific and philosophical foundation in which it developed, is the study of the history of science’s only worthy final aim. (ibid., 142)

The theme of value and the tension between immediate evaluation and rational evaluation as part of a whole come back as the central issue in “A Critique of Böhm-Bawerk’s concept of value and his theory of the measurability of value” (Slutsky 2004a). It is presented as the groundwork for a more ambitious project of a positive theory of value measurement that never appeared (Slutsky 2004a, 357). This masterful criticism echoes some questions raised in Budget as regards individuals’ capacity of valuing objects of choice. In this article, Slutsky provides a straightforward criticism of Böhm-Bawerk’s theory of value in *Kapital und Kapitalzins* (Böhm-Bawerk 1891) and indirectly a “final clarification of the received marginal utility theory ... [which] has not ceased to exert a hampering effect ... on the continual advancement of the science” (Slutsky 2004a, 357).

Slutsky focuses first on the issue of utility measurement and the theory of action. Slutsky puts to the fore the internal inconsistencies in Böhm-Bawerk’s statements about utility, desire, judgment of intensity and duration of pleasures and pain as psychic acts, summation of intensities of sensations, and our presumed ability to judge in advance the feelings associated to the consumption of a good. To Slutsky, in any act of choice involving several units of goods, the assumption that “several pleasures

could constitute in a direct manner, an immediate discernible unit of pleasure” goes against psychological knowledge that “if desire is a quantity, then it is a quantity of intensity. For this reason, it has no parts that could be contained in the unit of a whole in any intuitive way” (Slutsky 2004a, 359). In other words, the imagined summation of pieces of intensities when considering a lot made of several goods or units of the same good is contradicting Böhm-Bawerk’s own assumptions, a criticism that had been already addressed to Böhm-Bawerk by Čuhel (1907). More broadly, Slutsky rejects Böhm-Bawerk’s idea that value of a thing is essentially reduced to a measurement of *experienced* feelings that contradicts all his arguments in which desires and imagination about pleasures are used as equivalent. The second step in Slutsky’s criticism deals with the idea of the mind as a calculating machine in practice. “The fact that people economize rationally, weighing benefits and detriments” (ibid., 360), is no proof that calculation is processed by comparing intensities of sensations. Besides, the fact of measurement, if accepted is no justification for the logical possibility of measurement; the fact of choice is no proof that this kind of measurement has been operated, to the exclusion of all other motivational theories. Besides, it ignores the debates in the field of psychology over the possibility of measuring sensations, not to mention the meaning of measurement itself (ibid., 361). The third criticism addresses the incapacity of Böhm-Bawerk to take distance with a hedonist terminology and way of thinking and to confront with alternatives that involve other dimensions than pleasure and pain. Slutsky is there a fierce opponent to the “alchemy of hedonism [that] supposes itself capable of transforming all of these things: honor, duty, passion and who knows what else into sheer sensations of pleasure and pain” (ibid., 362).<sup>27</sup> Because he does not consider the variety in the circumstances and objects of choices and the impossibility to avoid to use imagination, “Böhm-Bawerk ... cannot do justice to the actual phenomenon of value in the primary and genuine sense of the term”. (ibid., 363). To this, Slutsky opposes the view that value proceeds from the recognition of primary goods having primary values, the economic value of economic goods being derived from their ability to provide these primary values in a given situation: “*desire, honor, duty, and so forth are customarily primary goods, and ... their values are primary values; ... the choice between primary goods is determined by the comparison between their values; ... economic value is derived from the primary, the realization of which depends upon one’s disposal over the corresponding economic good in a given situation*” (ibid., 363). Fundamentally, Slutsky’s analysis leads to flush out the fundamental flaw of hedonism:

it consists not solely in denying primary character to all goods other than one’s own pleasure and pain, but *even more importantly, in conferring a motivational meaning on pleasure as*

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<sup>27</sup> To Slutsky, inner calculations do not lead to unequivocal decisions: “Let us assume that someone must choose between his own pleasure and that of his wife or his brother. Let the second pleasure, according to his imagination, be, for example, greater than his own. Is it not then clear that the direction of his choice still is not by any means unequivocally prescribed thereby. A man can sacrifice a greater pleasure for a smaller one to his wife, yet not forgo his smallest pleasure for a larger one to his brother”. (Slutsky 2004a, 362).

*such, whose value, however, as a phenomenon sui generis is misapprehended and left out of play.* (ibid., 364)

The analysis of intertemporal choice leads to highlight the intrinsic contradictions of Böhm-Bawerk and to put pleasure in its right place with other primary values. The fact of being able to identify a future good as offering a greater value than a present one is contradictory with the fact of attributing the choice of the present good to a lack of will, since Böhm-Bawerk builds his theory of choice on the assumed ability to choose the greatest value. The only way out of this contradiction, Slutsky notes, is to conceive pleasure as something to be valued for itself (and not as a value by itself). Consequently, the fact of choice of a present good in the case of a choice between a present and a future good “means nothing other than I regard the value of the deferred realization as a smaller one in comparison with that value standing immediately before me. ... The essence of value cannot consist in the sensation of pleasure. *Pleasure is not value, but rather merely a possible bearer of value* and if, as is usual, it is such a bearer, *it is then a good*” (Slutsky 2004a, 366).

Eventually, in his incapacity of giving recognition to the meaning of primary goods (including pleasure) as the motivational support for desire and action, Böhm-Bawerk theory “proves itself to be evidently, a nominalist or even (ontologically) nihilist theory, since it overlooks and negates precisely the very phenomenon whose elucidation *should be its true task*” (ibid., 369).

The Böhm-Bawerk article stands as an informative complement to budget, offering new avenues to think at the kind of articulation between psychology and economics, in which goods are not seen as the direct bearer of utility and claiming for a refined theory of rational behavior. It also points to the implicit necessity to provide an ontology of economics.

What can be said about the influence of Böhm-Bawerk article in the West? On the basis of published articles and books, one is tempted to say that this article had little to no recognition in Western literature. To my knowledge, one of the first mention is in an article by Bagiotti commemorating the centenary of Gossen’s book, where he advances that Slutsky’s critical account of Böhm-Bawerk “is still ignored by its admirers” (Bagiotti 1955, 249). The next step in getting the Böhm-Bawerk article out of oblivion is Emil Kauder’s famous *A History of Marginal Utility* where Slutsky’s article is briefly presented (Kauder 1965, 129–130).

The third important contribution to economic thought to be commented in this section is “An Enquiry into the Formal Praxeological Foundations of Economics” (hereafter Praxeology). The article appeared in German in a Ukrainian journal. Praxeology is an attempt at building a formal-ontological building block to think of any kind of domain of knowledge involving the description of a system of elements linked together through parameters and dependence between past, present, and future states. The most general presentation of the concepts involved in this system (state parameters, possibility sets, optimality, directive forces, time processes, components of a state) constitutes a *formal physics*. When adapted to a field of thought by way of mapping an axiomatic and conceptual structure onto it, one obtains subdivisions of this formal physics. If this axiomatic structure involves “actions performed by



conscious beings” (373), one gets a *formal praxeology*. Still another narrowing of the conceptual apparatus—dealing with the administration of some objects of power called “assets”—leads to a “*formal economics*” (373). Praxeology can be read as a first attempt at establishing a balance between empiricism and a *prioristic* elements in economics, between determinateness and randomness, between systemic constraints and individuals’ expectations about their power over objects in the present and the future. In this respect, it connects several topics that are central in all the other contributions to economics by Slutsky. Slutsky’s goal in Praxeology is to provide us with a “formal-ontological” approach which, if successful, would be observable “as categorical forms in the corresponding substantive phenomena” (Slutsky 2004b, 372). As he puts it, the substantive content or the essence of economic structures “cannot be construed as consisting entirely of logical building blocks; human activity certainly cannot be decomposed without residual into quantitative relationships among the specific components of a quasi-mechanical system. Yet, on the other hand, reality is not by its very essence a seamless entity that defies any analysis” (Slutsky 2004b, 372).

An important point deals with the role of value or consciousness in the formal-ontological structure. To some extent, the description of the state of a system “without at all employing the concept of value or any categories of consciousness serves to demonstrate that certain empiricist trends of contemporary thought cannot be denied a relative validity” (*ibid.*, 372). In this respect, the influence of Russel and Husserl on Slutsky’s economic thought is fully acknowledged at several points in the essay, as it was already for Husserl in Critique and would be again in Summation. Once expectations about future possible states of a system enter stage, praxeology appears as the “foundation for a widely ramified deductive theory, ... the boundless richness of [which] ... represents a ‘definite multiplicity’ in the Husserlian sense” (Slutsky 2004b, 380).<sup>28</sup>

We can only give a partial account of the Praxeology article, which is extremely dense and contains a set of fundamental definitions of the categories of a formal praxeology and then of the basic concepts of a formal economics. A system is made of a set of partly interdependent variables defined by means of definite values (parameters). The whole set of conceivable parameters defines a state space. Any subset of values of the parameters in a state is a component. Some parameters—*determining circumstances*—determine the possibility set, while others determine the state that will be selected: they represent the *directive force* of the system that determines the *optimal point* that results from the operations of the directive force (*selection*) through a series of transformations of the state of the system (*effect*). The presentation of the series of *effects* that can take place through time is the *perspective* of the system.

At the center of the conceptual apparatus pertaining to agent’s behaviors is the concept of power. It is tempting and legitimate to interpret those elements within the narrow framework of a consumer budget share and a producer plan of produc-

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<sup>28</sup> Chipman (2004) insists on this “surprising influence” of Husserl and points out the implicit references to Husserl in his editor’s comments to Slutsky’s articles.

tion within a perfectly competitive environment. However, Slutsky warns the reader that various interpretative systems could be implemented on these praxeological categories. The objects on which agent's perform elementary operations could be economic goods or other agents, they could be rights, and even the kind of property on things is not strictly defined. The kind of decision taken may be parametric, but other kinds of interactions can be conceived of.

To Slutsky, a system can exhibit *passive* changes due to external circumstances, but only temporarily, that may induce some inertia into a system until the directive force becomes again the only driving force. As long as one considers the evolution of states from past to present, the optimal selection and temporal path depends on past history and initial values of the parameters. Once future values are taken into account as perspectives (involving views of human beings about the future), then the formal model is a *purposive process*, "a model that permits modification by various kinds of randomization effects" (Slutsky 2004b, 373). The fact of introducing purposive processes, hence a consciousness about views on the future, establishes a watershed between the use of this formal apparatus for mechanics and for economics. The temporal evolution of a system is characterized by the definition of components that could possibly be part of the system at various points in time. If two situations differ only by one component and each situation is stable, then the component is said *irrelevant per se*. A past component may be relevant at some later point in time and a future component too (retrospective or prospective relevance).

A formal physics becomes a formal praxeology once agents (conscious human beings) are considered in the system as the vectors of the purposiveness of the system, whose actions are effectuated upon various objects of power. Praxeological concepts deal with the definition of agents. Agents are the directive forces in a purposive system, and the changes of the system under their purposive action are *activities* (Slutsky 2004b, 375). The elementary *existential operations* performed on objects by agents are *creation, maintenance, and destruction* and transformation (*substitution*).

Slutsky has a complete terminology to describe operations of exchange:

The capacity of performing existential operations on an object we call power. The agent in question shall be called a *power wielder*, the object on which he operates the *object of power*. By capacity we mean here the objective possibility. If it is not absolute, the agent does not have an *absolute* but only a *relative power*, that is, a larger or smaller probability of being able to execute the operation in question. Any existential operation executed on power itself, considered an operation on this object of power, we call *power operation*. The operations of creation, maintenance or destruction of power are the elementary power operations; we shall call them *income earning, saving and spending*, and the corresponding objects are *incomes, savings, and expenditures*. The substitution of power over one object by power over another object is also a power operation, which we call *change*. If the power of one agent is replaced by the power of another agent, we talk of *power transfer*. If the change is effectuated by means of two interacting operations of power transfer, we shall speak of *exchange*. (Slutsky 2004b, 375)

Agents are seen as performing operations on objects and on powers, that is disposing of things in order to gain incomes, to spend or to save or to exchange (economic activities). The whole set of objects that can come under power of agent's disposition in the present and future are assets, and economic activity is the *administration of*

*assets*. The assets owned by an agent are his endowments. An agent and his assets are an economic unit. All the concepts presented here are orientating interpretation but are not necessarily synonymous with the usual concepts of economics, the point is that to Slutsky “no thorough understanding of economic phenomena can ever be attained without a closer analysis of the formal praxeological underpinnings of economics, and in particular without a closer analysis of the complex structures formed by power relationships as elements” (Slutsky 2004b, 376). Objects of power can be primary or derived objects. Agents may have a disposition potential over a power in the future. A disposition potential over a future disposable object is part of the endowment of an agent. Expectations about the future disposability of an object (*mere expectations*) are also part of the endowment. In the final analysis, endowments are characterized in their most general form as composed of disposition potentials and mere expectations about disposition potentials.

Then Slutsky considers changes occurring to the praxeological structure when randomness is added into this determinate structure. The most difficult question arises as to whether randomness in a component of a system still leads to a unique selection from the purposive process. Slutsky introduces into his system considerations about the probabilities of carrying out an existential operation of various disjunctive objects of power or probabilities over disjunctive outcomes. (The analysis would be even more complex if choices were treated as random.) The last section of the article deals with potential inconsistencies within an economic system, due to an ineffective effect of future expectations on present states. A specific praxeological device, taking the form of a complementary system of representation of agents’ activities, allows to think of various situations and how the fulfillment or non-fulfillment of expectations will lead to *corrective actions*.

The directive force is premier in fostering changes in any state-system, but the system itself is liable to passive transformations. However, in economics, it is usual to go beyond the mechanistic approach of a deterministic system through implementing in it a random component and the idea that agents within the system hold perspectives and induce purposive processes, implemented through “actions of beings endowed with consciousness” (373).

Slutsky’s contribution to praxeology can be put in perspective with his other contributions to economics. Consciousness and randomization are two recurrent issues and are witness for the importance he attributes both to a probabilistic representation of economic activities on the one hand and for the necessity to integrate agents own evolving representations in the understanding of economic choices. The importance of expectations and of views on the future thus appears in Praxeology and in Böhm-Bawerk as a possible nexus to understand how individual agents’ transform new data into new views about the future and update their plans. Unfortunately, on the basis of published archives, we cannot do more than guess in what directions Slutsky could have develop his thoughts.

In his praxeological framework, Slutsky elaborates on the notion of assets, viewed exclusively from the perspective of power on primary objects (individuals being endowed with some power on those primary objects) and of mere expectations about future possibilities to derive powers from the use of those primary objects in the

present and in the future. A full formal praxeological interpretation of economics needs to account for the gap between real components of a system (and of its future) and perceived components (presumed or expected in the future). Through time, agents would consider how their expectations are fulfilled and enter into corrective actions. Eventually, the whole presentation does not make reference to such concepts as value, capital, labor, prices, property (a concept necessarily linked with power). It seems that in this article, Slutsky aimed at clarifying his stance as regards empiricism, that is, the idea that economic concepts should derive from common sense or introspection, an attitude he had not discarded completely in his 1915 article when imagining introspective experiments as regards utility variations. As he declared to Frisch in a letter dated July 9, 1926, “Even though I always highly value the role of empirical experience and especially experiments in theoretical economics, I could not now subscribe to certain utterances in my earlier work, since, as I may believe, I have now arrived at a clearer insight into the relationship between the empirical and a priori elements of our knowledge”. (cited in Chipman 2004, 349). This confession, however, should not be taken as a plea for *a priorism* in economics, since Slutsky does not engage with the issue of rational behavior and lets it open as to the various ways to use the praxeological system in economics. Though individuals’ actions are grounded in past experience, they are also to a high point motivated by representations about their potentialities and expectations. For that reason, the world and its possibilities for the future cannot be packed within a deterministic and naturalistic framework, and the empirical description and analysis of future events cannot rest upon a frequentist or empirical knowledge of probabilities. Slutsky’s praxeology was referred to by Mises (1933, 16, 2002) as a formal praxeological system “patterned after the science of logic” different from the experienced-based approach he favors (see Barnett 2011, 74). Oskar Lange would also identify Slutsky as an independent founder of praxeology with Kotarbiński (Lange 1971). Hence, Slutsky is probably the first author to explicitly connect praxeology and economics.<sup>29</sup>

## 8 Conclusion

It is beyond the purpose of this essay to speculate more about what could have been a unified view of political economy according to Slutsky, and the proper role of theory and empirical work would have played in it. Beyond doubt, through only two articles, Slutsky has been among the most inspiring economists in the twentieth century. It is hoped that further research on Slutsky’s archives will add new insights on his thoughts.

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<sup>29</sup> On Slutsky and the origins of praxeology, see Chipman (2004) and Gasparski (1996). To Lange (1971, 1–2) “The Austrian economist Ludwig Mises also used the same term, although he erroneously identified praxeology with political economy and misconceived its foundations”, and praxeology in economics is definitely associated with the principle of economic rationality which, when applied to magnitudes or quantities, takes the form of the principle of greatest efficiency or the principle of economy of means.

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# Feldman and the Strategy for Economic Growth



Harald Hagemann

## 1 Introduction

Grigory Alexandrovich Feldman was born 27 August 1884 in Rostov-on-Don. He was trained as an electrical engineer and finished two courses of higher education, one in Germany and one in Moscow in 1912. Thereafter, he worked as an electrical engineer in St. Petersburg/Petrograd and Moscow. In February 1923, he joined Gosplan, the State General Planning Commission, which had been established by a decree of the Council of People's Commissars in February 1921 and two months later had begun its work on socialist planning at the beginning of the period of New Economic Policy NEP 1921–28.<sup>1</sup> Feldman, who was fluent in three foreign languages, worked in Gosplan's department of world economic conditions. His main focus was on the long-run analysis of the US economy from 1850 to the 1920s and on the German economy, particularly during World War I and thereafter. Feldman's studies and periodic surveys were published regularly in Gosplan's journal *Planovoe khoziaistvo* from 1927 to 1930.<sup>2</sup>

In January 1931, Feldman was forced to leave Gosplan. Thereafter, he worked in the Department of Technical Constructions in the Planning Academy from 1931 to 34, on the planning of the Northern Sea Route in 1935–37, and in engineering jobs

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<sup>1</sup> On the Soviet Industrialization Debate, see, for example, Erlich (1960), Spulber (1964a) and Ellman (2014).

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<sup>2</sup> See also Fel'dman (1929a, b). *Planovoe khoziaistvo* is available online: <https://istmat.info/node/33576>.

in various cities of the Soviet Union since 1943.<sup>3</sup> Presumably, he was imprisoned or in labour camps for several years after 1937. Although he had finished a book on capitalist reproduction and growth in 1933, due to the political circumstances, Feldman was prevented from publishing anything on economics after 1930. Feldman died in Moscow in 1958.

Feldman's most outstanding contribution was his model of economic growth which he presented to the Gosplan committee for long-term planning in 1928. It was published in two essays "On the Theory of Growth Rates of National Income", I and II, in *Planovoie khoziaistvo* in November and December 1928 at the end of the NEP period. Feldman's Soviet model of growth, which is based on Marx's schemes of reproduction and had been "rediscovered" by Domar (1957), will be discussed in Sect. 2.

With the emergence of modern growth theory and the decolonization process starting with the independence of India in August 1947, Marx's two-sectoral growth model and Feldman's emphasis on the strategic role of the capital goods sector attracted growth theorists and economic planners in developing countries alike. India's second Five-Year Plan from 1956 to 1961 developed by Prasant Chandra Mahalanobis (1893–1972) was based on a similar two-sectoral model previously conceived by Feldman (Mahalanobis 1953) to develop the best strategy of industrial development for India. Adolph Lowe (1893–1995) had worked since the 1920s on a modification of Marx's reproduction schemes with similar intentions which he elaborated into a structural model of production (Lowe 1952, 1955), also forming the basis of his traverse analysis (Lowe 1976). Maurice Dobb (1900–1976), a long-time observer of Russian economic development since the 1917 Revolution, like Lowe, extended the two-sectoral model (Dobb 1960, 1967a, b) but kept the core idea of the strategic role of the machines tool sector. In the subsequent literature, conceptions such as the Feldman–Mahalanobis model<sup>4</sup> or the Marx–Feldman–Dobb model (Chng 1980; Erlich 1978; Hagemann and Jeck 1984) came up. Section 3 deals with the main characteristics and results of the Lowe–Dobb model which is an elaboration of the Marx–Feldman–Mahalanobis model making the model more flexible and gaining some additional insights by extending it from two to three sectors.<sup>5</sup> Some reflections and assessments will conclude in the final Sect. 4.

## 2 Feldman's Soviet Model of Economic Growth

Feldman elaborated his growth model against the background that the Soviet Union in the 1920s attempted to plan industrial development to catch up with the most

<sup>3</sup> See Vainshtein and Khanin (1968).

<sup>4</sup> "Feldman alone appears to be the true predecessor of Mahalanobis as is evident from Domar's discussion (1957)". Chakravarty (1987b, p. 276).

<sup>5</sup> On a comparison of the approaches of Lowe and Mahalanobis, see also the contributions in the special double centenary issue of *Économie Appliquée* 47(2), 1994.

advanced capitalist economies in the West, above all the USA, as quickly as possible. He had already tried to compare long-run growth in the USA from 1850 to 1925 with growth projections of the economy of the USSR between 1926/27 and 1940/41 in his first major paper (Feldman 1927). In December 1925, the XIV. Communist Party Congress had proclaimed Industrialization oriented towards economic independence as the major task. In October 1928, the first Five-Year Plan for the period 1928–1932 had been launched, which was formally ratified as late as April 1929, i.e. after Feldman's essays had been published. It proclaimed as its objective an unrealistic increase in the output of capital goods in a dimension that the fixed capital stock of the Russian economy would double within five years. Three decades later, Leontief (1960) dated the decline of economics in Soviet Russia back to the inauguration of the first Five-Year Plan.

Feldman's explicit aim was the long-run maximization of consumption, as he stated at the beginning: "... The primary objective of this work is to determine the potential volume of consumption of the masses, and its rate of growth as a function of the structure of the economy (Feldman [1928] 1964, p. 174). However, whereas in the long run, the rate of growth of consumption (and the rate of growth of investment) tends towards the rate of growth of national income, this does not hold for temporary adjustment processes. In particular a phase of accelerated growth "demands industrialization, heavy industry, machine building, electrification ..." (ibid., p. 194). This priority of heavy industries implies that the rate of growth of consumption is lagging in the short and medium run; in the Feldman model only relatively, in Stalinist reality some years later even absolutely (as, for example, during the Holodomor in the Ukraine).

Feldman's main concern was the *long run*. However, as pointed out by Domar<sup>6</sup> (1957, p. 246), "[h]e did not specify whether the variable to be maximized should be consumption at a point of time, or its rate of growth, or its integral over time; whether consumption should be discounted or not". Feldman did not address time preference and the discount problem to compare future with present consumption.<sup>7</sup> Perhaps the issue of individual time preferences was too sensitive, and the decision on the time horizon and the discount rate was left to the decisions of the planning authorities.

Feldman took the Marxian schemes of reproduction as his starting point and "stated outright that the increase of the rate of growth of production depends on the increase of the capital of sector A [producers' goods sector] as compared with the increase of the capital of sector B (consumers' goods sector)" (Feldman [1928] 1964, pp. 174–5). He modified the Marxian model in a way that sector A included all activities that increased the capacity to produce output, whereas all activities that were

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<sup>6</sup> Domar was not aware of Feldman's study when he formulated his own growth models, as explained by Mauro Boianovsky in his contribution to this volume. He was informed by Gregory Grossman on Feldman's work. Since Domar discusses the Feldman model in greater mathematical detail, I restrict myself in this section to commenting some essential elements and results.

<sup>7</sup> In 1928, the concept of discount rate was not yet adopted by Soviet economists. See Grossman (1953).

merely sustaining output at the current level were concentrated in sector B. Consequently, investment priority for the capital goods sector results as a pre-condition for attaining a higher growth rate. Whereas Feldman's conceptualization theoretically is attractive and convincing, it is much more difficult to apply empirically, as already shown in the subsequent debate in the Soviet Union which followed immediately.

Feldman made several major assumptions in his growth model. An important one is that he assumed a *closed economy*. In a country lagging in the industrialization process, it implies, at least in the starting phase, that the domestic capacity to produce capital goods is a major bottleneck for a quick and successful catching-up process. Structural incapacity to supply enough capital goods will prevent a rise in the saving ratio from being fully transformed into the desired level of investment. But it must be considered that a one-sided preoccupation with this "Feldman constraint" on the investment capacity side may bring the "Preobrazhensky constraint" on the consumption side into play. If the initial capacity of the capital goods industry would be just sufficient to replace the worn-out machines, growth could only take place at the expense of a temporary reduction in the output of consumer goods which might be impossible for subsistence reasons. In this case, a vicious circle of simple reproduction would emerge which is excluded by assumption from Feldman.

In the Feldman model, capital is the only scarce factor of production. He did not consider *labour* as a factor limiting growth. "[I]t is assumed that labour is available in any quantity and composition" (*ibid.*, p. 188). As in the Mahalanobis model for India, there are assumed to exist "unlimited supplies of labour", a stylized fact of underdeveloped economies with an overwhelming share of agriculture and high population growth which became prominent as the Lewis model (1954) in the early decades of development economics. This makes the capacity to produce capital goods the key to economic growth.

Feldman's growth model is one with *fixed coefficients*. Capital goods are non-shiftable; i.e. they cannot be transferred from one sector to the other. This inflexibility reduces the direct empirical applicability to a real economy.<sup>8</sup> The share of overall investment allocated to sector A is the key variable of the model. With full capacity utilization in a closed economy, an accelerated rate of growth could be reached only by a larger proportion of current investment entering the production of capital goods in sector A. In the long run, it would also raise national income and the output of consumers goods, at the expense of growth of consumption output in the transformation phase. Avoiding a too negative effect on the consumption of the masses is probably a reason why Feldman puts recurrent emphasis on raising the efficiency of the utilization of capital. Empirical data show that in the decade after 1928 the rate of technical progress in the capital goods-producing sector(s) has been much above the average in the Soviet economy.

Domar criticizes Feldman for his incomplete and defective treatment of the complex depreciation-replacement problem. "Feldman was evidently not aware that in a growing economy depreciation exceeds replacement and took their identity for

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<sup>8</sup> In a similar way, Weitzman (1971: 513) argues: "Clearly the Feldman model exaggerates the significance of capital ossification".

granted” (Domar 1957, p. 240). He abstracts from this problem in his further discussion of the Feldman model by assuming a permanent lifetime of capital goods. With given capital coefficients, the decisive variable for economic planners, is the share of overall investment effected in sector A which can vary from

$$0 \leq \gamma \leq 1.$$

In case of  $\gamma = 1$  the maximum rate of growth could be reached in the short run with consumption frozen at the initial level. In the opposite, extreme case of  $\gamma = 0$  consumption would increase in the short run but at the expense of growth in the long run. There exists a trade-off over time. Whereas a higher  $\gamma$  will lead to a higher rate of growth of consumption in the long run, a lower  $\gamma$  will result in a slower rate later. Feldman put all his focus on long-term growth and was less absorbed by the problems of the day.

In war times, all economies, whether market-oriented or planned, face a strong reallocation of resources towards heavy industries. Whereas the defence rationale was not the prime interest of Feldman in 1928, military and security aspects of the capital stock and investment were always important for Soviet planners and definitely for Stalin. They served as an additional justification for increasing  $\gamma$  and were enhanced after the Nazis’ rise to power in 1933. Domar (1957, p.238) specifies that in 1928 the share of investment in gross national output in the Soviet Union was 23.2% which adds up to 25.7% including armament expenditures and increased to 32.2% in 1940. With Hitler’s attack on the Soviet Union and the subsequent Cold War period, it increased substantially.

Kontorovich (2013) heavily criticizes the standard Western interpretation of Feldman’s model and the strategy for priority of heavy industries in the allocation of investment as a means for accelerating growth, and the ideas developed by Preobrazhensky<sup>9</sup> to provide the resources for the increase of investment from the agricultural surplus product created by the peasantry, as grossly wrong and assesses it a “myth”. According to him, the final victory of socialism, the strive for independence from capitalist countries, and the military aspect to strengthen defence were the decisive motives. Whereas the majority Western view downgrades defence, for Kontorovich military motivation was decisive for the priority of heavy industries. From the beginning of Soviet planning, the military sector emerged as the dominant part of the economy and became the most successful one (Kontorovich, 2019). Under the influence of General Tukhachevsky (1893–1937), a modern military planning complex was designed already in 1927 which demanded huge investments in heavy industry and defence aiming at the future production of weapons.<sup>10</sup> Since the mid-1930s, when threats emanating from Nazi Germany and militarist Japan on both

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<sup>9</sup> For a rediscovery of Evgeny Alexeyevich Preobrazhensky (1886–1937) as a leading Marxist economist and the author of primitive socialist accumulation in the West see Erlich (1950). See also Preobrazhensky (1979).

<sup>10</sup> For greater details, see Samuelson (2000).

sides became real ones, the pace of war production was accelerated far beyond what had been envisaged when the first Five-Year Plan had been shaped.

It is true that military power was a main driving force for Stalin and Soviet planners to promote forced industrialization. However, “[t]hey were their own economists” (Leontief 1960: 262). On the other side, the (Marx-) Feldman model has no explicit reference to military expenditures, and explicitly aimed at maximization of consumption in the long run. In that important sense, Feldman had little influence on Soviet policy because that was primarily determined by military factors.

However, it would be a gross misinterpretation to reduce Western interest in the Feldman model to the fashions and career motives of social scientists in the West and “the reluctance of academic economists to specialize in defence economics” (Kontorovich 2013, p.26). Feldman’s pioneering study explores the structural conditions for a successful catching-up process of an initially backward economy, respectively, the necessary restructuring in a more advanced economy which wants to grow faster. It is therefore of more general interest, as will become clear in the next section.

### 3 Structural Analysis of a Growing Economy: The Three-Sectoral Lowe–Dobb Model

As Feldman emphasized at the outset of his theory of growth rates of national income, his focus is on the structure of production. In his famous essay on Feldman’s Soviet model of growth, Domar (1957, p. 227 n. 8) referred to the fact that similar schemes had been suggested by Burchardt (1931–32), Nurkse (1935) and Lowe (1952, 1955). Adolph Lowe (1893–1995) had found the basis of his later works in Marx’s schemes of reproduction which he considered “the only comprehensive macro-economic model of the industrial process of production established before Keynes” (Lowe 1952: 141). He regarded Marx’s schemes to be especially suited to the study of real capital formation provided that three defects are corrected (see Lowe 1955, p. 586). The first correction consists in adding appropriate stock variables because the equations in Marx’s schemes make sense only if understood as describing flows. Secondly, the two sectors of Marx’s analysis must be disaggregated into vertical stages so that the scheme can be applied also to working capital goods as goods in process.<sup>11</sup> Finally, and most important, Lowe considered it necessary to extend the two-sectoral Marxian model to a *three*-sectoral scheme, through the splitting up of the key sector I of Marx’s reproduction model, in which capital goods are produced, into two subsectors. This subdivision of the equipment goods group is relevant for investigating the structural conditions for steady growth and, even more, for addressing questions of traverse analysis when the implications of this rather simple extension

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<sup>11</sup> In a similar way, Weitzman (1971: 512) argues in his discussion of the Feldman model: “Each department is best thought of as vertically integrated in all the intermediate materials it uses back to and including agriculture and the extraction of raw materials from the earth”. See also Dobb (1967a, b, pp. 116–7).



towards a more “realistic” representation of industrial structure become fully visible. Lowe had already pointed out the need for a tripartite scheme through the splitting up of the capital goods sector in Marx’s growth model, into one sector producing the equipment for the replacement and expansion of both capital goods sectors and the other sector producing the equipment needed to produce consumer goods, as early as 1926.<sup>12</sup>

At that time, Lowe was the director of the research department on business cycles at the Kiel Institute of World Economics where Burchardt was his closest collaborator. Fritz Burchardt, after his emigration from Nazi Germany to England in 1935, Frank Burchardt (1902–58) was the first economist who set out to compare, contrast and combine the schemes of the stationary circular flow in Böhm-Bawerk and Marx, and thus undertook the first synthesis of the vertical integration and the interindustry approach, i.e. the two important ways of disaggregating the production structure of the economy. Though for the most part concerned with the stationary aspects of a scheme of reproduction, Burchardt (1931–32) left no doubt about its relevance as an instrument for dynamic analysis. The seminal character of Burchardt’s work was recognized immediately in the Anglo-Saxon literature (Nurkse 1935). Lowe (1976) applied it in his pioneering traverse analysis, which relied on a model of production with three sectors and four stages within each sector that synthesized classical circularity and Austrian sequentiality. Domar had spent some time at the Oxford University Institute of Statistics in 1953 while Burchardt was Director (see Domar 1957, p. 195).

In Lowe’s division of the economy into three vertically integrated sectors, the subdivision of the equipment goods sectors is relevant for investigating the structural conditions for steady growth and, even more, to address questions of “traverse analysis”. In the analysis of transition processes, there is, in contrast to the traditional steady state or “golden age” analysis, no assumption of the proportionate growth of all sectors, so that the problem of structural change is moved to the centre of the stage. The decisive problem that the economy confronts upon departing from a steady growth path is the inadequacy of the old capital stock. The necessary adjustment process both requires time and cost and faces difficulties which arise from disproportions between sectors. During the Soviet industrialization debate, Fel’dman formalized the notion that investment priority for the capital goods sector was a precondition for attaining a higher rate of growth. In Lowe’s growth analysis, the “machine tools sector” also plays a key role and altering the proportion of total investment allocated to this sector has great direct and indirect consequences for growth.<sup>13</sup>

The main lesson to be learned from the Lowe–Fel’dman models is that the capacity of the machine tools sector is the decisive constraint which limits the rate of growth in a closed economy. Even though Lowe’s intentions were in many respects the same as Fel’dman’s, who wanted to include all activities that increase the capacity

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<sup>12</sup> See Lowe ([1926:190] 1997: 265) and Lowe (1952, n. 11).

<sup>13</sup> For a more detailed analysis of Lowe’s structural theory of economic growth and his traverse analysis, see Hagemann (1990) respectively Gehrke and Hagemann (1996).

in an economy to produce output in one sector, there exists one important difference. No ex-post transfer of machines between the two departments is possible in the Fel'dman model; there is complete rigidity as in most other two-sector models with a fixed-coefficient technique. On the other hand, an essential characteristic of the three-sectoral Lowe model is that it combines transferability with specificity. Accordingly, an ex-post transfer of machines between the two sectors of the equipment goods group during the traverse is possible. Dobb (1960, pp. 48–103) made extensive use of Lowe's (1955) version of the three-department scheme not to analyse the process of traverse but to discuss the question of the choice of techniques under planned development. Maurice Dobb's version of the Marx–Feldman model has been examined by Erlich<sup>14</sup> (1978) and Chng (1980).

The basic structure of the Lowe model can be described as follows:

*Sector 1* produces primary equipment goods or machine tools which are directly used for production of the secondary equipment goods in sector 2 and indirectly to produce consumption goods in sector 3. Sector 1 is the only one capable not only of producing machines for other sectors but also for itself, i.e. a self-reproducible sector. That puts sector 1 into a key position for any industrial system and a process of growth, especially during a “traverse” with its structural change. In Sraffa's terminology, sector 1 represents the “*basic system*”.

*Sector 2* represents the link between the machine tools sector 1 and sector 3, producing consumer goods. Sector 2 uses the same type of equipment goods as sector 1 does so that shiftability of parts of the capital stock between these two sectors is possible. On the other hand, sector 2 produces the secondary equipment goods which are used as inputs only in sector 3 producing consumer goods which means that the capital stock in the latter is not transferable.

*Sector 3* provides the consumer goods for the labourers working in the three sectors. In this industrial system, equipment and labour remain in a strict relation of complementarity. The production of primary equipment goods or machines is the bottleneck which any process of rapid expansion must overcome. The technical methods of production will be represented by a matrix of interindustry coefficients, denoted by  $A$ , and by a column vector of direct labour coefficients, denoted by  $l$ :

$$A = \begin{bmatrix} a_{11} & 0 & 0 \\ a_{12} & 0 & 0 \\ 0 & a_{23} & 0 \end{bmatrix} \quad l = \begin{bmatrix} l_1 \\ l_2 \\ l_3 \end{bmatrix}$$

with  $a_{ji} = F_{ji}/O_i$  and  $l_i = L_i/O_i$ , where  $L_i$  and  $F_{ij}$  denote the inputs of total labour and fixed capital goods of type  $j$  in the production of  $O_i$  goods in sector  $i$ .

Lowe's model of industrial production has two characteristic features. First, there exists a definite hierarchy of sectors, from 1 via 2 to 3, or, in popular terminology,

<sup>14</sup> Alexander Erlich's important investigation *The Soviet Industrialization Debate, 1924–1928* (Erlich 1960) is the revised version of his PhD thesis “The Soviet Industrialization Controversy” which he submitted in January 1953 to the New School for Social Research where Adolph Lowe was in his advisory committee.

machines  $\rightarrow$  tractors  $\rightarrow$  corn, which implies a unique intertemporal complementarity when adjustment processes with structural reportioning are taking place in historical time. This gives the Lowe model a touch of “Austrian flavour”, even if we abstract from the stage aspect and concentrate on the interindustry aspect of the model as expressed in the **A** matrix and the **I** vector. Secondly, the abandonment of the single capital good assumption leads to a special mixture of flexibility, arising from the dual utilization of machines which therefore can be transferred between sectors 1 and 2, and rigidity because the equipment goods of the second type are exclusively used in the production of consumption goods in sector 3.

Since the Cambridge debate on the theory of capital, we know that the sectoral machine-labour ratios  $q_i = a_{ji}/l_i$  are of crucial importance. Whereas the technologically given ratios  $q_1$  and  $q_2$  can be directly compared, problems arise with  $q_3$  because a different type of capital good is used as an input in the production of consumption goods. But there is a way out of this physical dimension dilemma if we only multiply  $q_3$  with  $l_2/l_1$  thus giving us the indirect machine-labour ratio  $q_3^*$  which can directly be compared with  $q_1$  and  $q_2$ . For example,  $q_3^* > q_1$  means that the economy indirectly uses more machines per unit of labour in the production of corn than in the production of machines where they are directly used as inputs. The consideration of the price and quantity systems of the economy shows the crucial role these machine-labour ratios play.<sup>15</sup>

In growth equilibrium, the composition of investment is the same as the composition of the capital stock. Furthermore, one can see *that the economy with the higher growth rate*

1. has a greater stock per capita with primary equipment goods or machine tools which are used in sector 1, has a smaller stock per capita with secondary equipment goods, and a greater relation between stocks of primary and secondary equipment goods,
2. uses a higher percentage of its primary equipment goods in the key sector 1 than in sector 2, uses a higher percentage of its labour force in the production of primary equipment goods, also, in relation to the production of secondary equipment goods, and uses a lower percentage of its labour force in the production of consumer goods.
3. The composition of production is such that the economy with the higher growth rate produces relatively more equipment goods than consumer goods and relatively more equipment goods of the primary type. The contradiction between consumption and accumulation manifests itself in the proportion between the two sectors producing means of production.

But some open questions remain, which all have to do with the specific Lowe-sector 2 which operates as the bridge between the basic sector 1 and the consumer goods sector 3. The reason for this indeterminateness is evident. Sector 2 participates with lower percentages in a bigger cake. The higher the growth rate, the higher is the

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<sup>15</sup> For a detailed mathematical analysis of the quantity equations, and also of the price equations, in Lowe’s three-sectoral model, see Hagemann and Jeck (1984).

weight of sector 1 and the lower the weight of sector 3—no doubt about that. But the weight of sector 2, using inputs produced in sector 1 and producing equipment goods used in sector 3, is influenced by both these factors with no unique result.

Feldman's result that, given the capital-output ratio, a higher rate of growth requires a higher share of the capital stock and of the new investment in the producer or capital goods sector 1 (*A* in Feldman) thus is confirmed. Furthermore, the disaggregation of the capital stock into two sectors 1 and 2 yields some additional insights. The shiftability of machines between sectors 1 and 2 adds flexibility. In particular, a shift of primary equipment goods (in Lowe's terminology) or machines from sector 2 to sector 1 allows for an accelerated growth in the early phase of a transformation process, at the expense of a relative reduction in the output capacity of consumer goods. On the other hand, such a shift can reduce the adjustment time required for a permanent increase in consumption via a later shift of machines in the opposite direction from sector 1 to 2, and thereby indirectly an increase in production capacity in sector 3.

## 4 Concluding Remarks

Feldman's growth model is a good example of interaction between Russian and non-Russian economists and their ideas. With the rise of modern growth theory initiated by Harrod and Domar and the emergence of development economics as a new subdiscipline, there also emerged a growing interest in Feldman's pioneering study of 1928. This was enhanced by Domar's essay on the Soviet model of growth (1957) and is reflected in English (1964) and German (1969) translations of Feldman's articles.

Investment priority for the capital goods sector as a precondition for attaining a higher rate of growth is the decisive result of the Marx–Feldman model as well as of similar ones by Mahalanobis, Lowe, and Dobb, as it was the case during the Soviet industrialization debate in the late 1920s. The existing capital stock (and in more sophisticated analyses the state of technology embodied in it) was identified as the main bottleneck to rapid economic growth by socialist planners. Constraints on the labour supply side played no role. Yet, in modern economies, human capital is important for growth. Existing skill structures and the resource- and time-consuming process of transforming existing and evolving new skills, as technology advances, can impose a similar constraint as a given stock of capital.

Experience with economic planning in the Soviet Union since the early 1930s, and decades later in Mao's China, demonstrated the limitations of a techno-structural approach which excludes motives, behaviour, and expectations of economic actors. India's experience in the 1950s and 1960s was not so much better, although an important difference consisted in the fact that it was a much more open economy. Here, the strategic role of the capital goods sector was also stressed by economists discussing the growth and planning problems (Mathur, 1965). India, which lacked a

self-sufficient machine tools sector, as an open economy could speed up her catching-up process by foreign trade. The Feldman constraint would be absolutely binding only if the domestic output of machine tools could not be supplemented with imports. However, the import of capital goods from the technologically more advanced nations may imply two dangerous developments: the problem of technological dependence, to which Merhav (1969) has called attention, and the danger of a debt trap, if the import of machinery must be largely financed by credit because of protectionism in industrialized nations which do not open their markets for agricultural products from underdeveloped countries so that these could finance their imports of capital goods with the proceeds from their exports. Examination of the structural conditions for catching up and steady growth processes must therefore be extended to the economy's external balance.

In contrast to Feldman who assumed a closed economy in his growth model, the importance of foreign trade was not neglected in the first Five-Year Plan. The plan was partly based on the import of modern technology (mainly from the USA) in exchange for primary products (mostly grain) and some credits. Key projects of that plan, such as the Stalingrad Tractor works, were partially designed by US engineers and partially built under their supervision. The Magnitogorsk Iron and Steel plant, the largest steel company in the Soviet Union which contributed to the victory over Nazi Germany by manufacturing masses of defence products, was modelled on a US plant in Gary, Indiana, with US engineers playing an important initial role in its construction.<sup>16</sup> The Soviet First Five-Year Plan thus included major elements of an "import led growth". Similarly, China's rapid development after 1978 was an example of "strategic integration" into the world economy (Ellman 2014, pp. 356–358). Hence, closed economy models, such as Feldman's, are inadequate to fully understand some important aspects of Soviet growth in the early 1930s and Chinese growth in the 1980s.

Despite all similarities in the structural analysis of real capital formation between the Lowe model and the Russian Indian models, which have their common root in Marx's schemes of reproduction, one important difference should not be overlooked. Many of the Marxian-inspired structuralist approaches, from Feldman to Dobb and the Indian school of planning, suffer from being permeated by material determinism. While the quantitative relations that are required between the sectors are emphasized, the behavioural factors which are bound to prevail in any institutional set-up very often are neglected (as later also emphasized in the important work of Kornai (1980) who identified a main cause of the shortage of goods in the lack of incentives of producers in planned economies which were sellers' markets). One almost "natural" consequence of this neglect is the fact that empirical predictions drawn from Feldman-type models are much too optimistic.<sup>17</sup>

Lowe's analysis, on the other hand, is considerably richer than a rather technocratic analysis. The derivation of possible transition paths based on structural requirements is only a necessary first step. Structural analysis has to be supplemented by "motor or

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<sup>16</sup> I thank Michael Ellman for providing me with these details.

<sup>17</sup> See, for example, Domar (1957) and Ellman (1987).

force analysis”, which studies the micro-behaviour and motivations that induce suitable reactions on the side of the individual agents to set the economy on an adjustment path in agreement with the macro-goals. It is this force analysis that raises economics above the level of a mere social engineering and has a special significance not only for market or mixed economies but also for planned economies. In his retrospective assessment of the Indian experience of development planning, Chakravarty (1987a) highlights the deficiencies of a purely techno-structural planning and points out that the force analysis of Lowe’s political economics had been proved pertinent.

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# Chayanov: The Reception of an Early Soviet Agricultural Economist



Carol Scott Leonard

A leading non-Marxist agricultural economist of early Soviet Russia, Director of the Moscow Institute for Agricultural Economics, Alexander Chayanov (1888–1937) produced exhaustively researched analytic work on the peasant economy, which had a lasting impact in two fields, rural sociology and economic anthropology. He dominated rural studies in Russia from the late imperial period through the mid-1920s. In the late 1920s, Chayanov and his colleagues at the Institute, founders of the Organization-Production School (*organizatsionno-proizvodstvennaia shkola*), failed to dissuade the Soviet government from a rapid course of industrialization and collectivization, destroying peasant farming, and the institute was closed as the “new course” was implemented. He was arrested, imprisoned, sent into exile, executed in 1937, and his works were not available in Russia until his rehabilitation in 1987.<sup>1</sup> His microanalysis on family farming then experienced a revival in late communist Russia, when it became relevant in the restoration of the rural market economy. Outside of Russia, Chayanov was widely read beginning in the late 1960s with the first English translation and continuing through the late 1980s and 1990s, after the republication of the translation in an edition by Teodor Shanin of *The Theory of Peasant Economy* (1986). Chayanov’s writings provide rich data-based framework for considering the peasantry as a distinct community within the larger economy with production incentives that are rooted in local custom and non-market exchange. A social agronomist, A. V. Chayanov was cast by Stalin as a leader of a (fictional) oppositional party, but his importance lies not on the sidelines in a historical opposition but in the continued influence of his works as the foundation of modern peasant studies.

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<sup>1</sup> Shanin (2009, p. 85) corrects his date of death as 1937, not 1939; Kerblay (1986, n.5) had been informed incorrectly by Vainshtein, that Chayanov was executed in 1939.



This chapter highlights waves in European and Russian interest in Chayanov's views on the Russian peasantry. It is divided into three parts. The first is academic background and his school's debates with the Agrarian Marxists. The second summarizes his analytic contribution. The last focuses on the interrelationship between his remarkable revival from the 1960s and shifts in the intellectual and political contexts in Russia and abroad.

## 1 Biography

### 1.1 Background

Chayanov, a well-published graduate of the Moscow Agricultural Academy, had considerable influence before the 1917 revolution.<sup>2</sup> He traveled in 1908 to Lombardy to study the irrigation system and to Belgium for a closer understanding of the organization of cooperatives (Kerblay 1986, p. xxvi, n. 7). His debt to Belgian influence is easily identified in his bibliography from early works on agricultural cooperation in Italy and cooperative insurance in Belgium and Russia (Chayanov 1986, pp. 289–80). At the age of 25 in 1913 with thirteen published papers, he was appointed assistant professor at the Moscow Agricultural Institute of Petrovskoe Razumovskoe (now Timiryazev Agricultural Academy). From 1919 until 1930, he led a research institute created from his own seminar at the Institute of Agricultural Economy in the new Soviet Timiriazev Academy of Agricultural Sciences (previously the Petrovskii Academy).<sup>3</sup>

At his institute, which dominated rural studies through the mid-1920s, he was the youngest researcher at the Organization-Production School. His colleagues were distinguished in Russia and internationally published scholars and practitioners,<sup>4</sup> and they worked in policy organizations while conducting agricultural research and giving seminars.<sup>5</sup> Some were statisticians from the pre-revolutionary era, part of the sweeping effort beginning in the 1880s in the *zemstva* (provincial and district assemblies) to collect village-level data. By the end of the 1920s, over 4000 volumes

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<sup>2</sup> Bourgholtzer (1999, p. 14 and 49, n. 3) observes that biographical materials about Chayanov are limited. For example, a lengthy stay in England and Berlin (April 1922 to October 1923) is known only through archived letters, later published by Vasily Chayanov (1998) and from the record of his interrogation in 1930 by the chief of the Secret Department of the OGPU, Yakov Agranov.

<sup>3</sup> Scientific Research Institute for the Agricultural Economy and Agricultural Policy.

<sup>4</sup> See also Chelintsev (1919) and Makarov (1920) from this group. Colleagues include V. S. Nemchinov, who is credited with introducing mathematical methods into Soviet economics, and mathematical economists A. L. Vainshtein and N. D. Kondratiev, who was director of the Conjuncture Institute in the Timiriazev Academy (Barnett 1995, p. 413).

<sup>5</sup> Much of the work of his school draws on budget surveys of peasants carried out from the 1880s by *zemstva* statisticians; he also continued work on surveys in the 1920s, from 1924, he had surveys done in Penza, Volokolamsk, and other guberniias, including where beets were grown, in 1925, and in Yaroslavl guberniya in 1927.

existed on the Russian peasantry.<sup>6</sup> On the basis of these surveys and from reading and travel abroad, Chayanov wrote his main works, published throughout the 1920s; Bourgholtzer (1999) writes that very little is known about his experiences abroad. His project for a book on the theory of peasant economy was planned for a long stay in England, where he had his entire library shipped to finish it. From there, he went to Berlin (April 1922 through October 1923) (p. 13). He was close to the exile community in Berlin, and in 1923, he seriously considered emigration and transferring his work to Lithuania or Estonia (p. 14). From there, however, after a stay in Heidelberg as well as Berlin, he returned to Moscow.

## 1.2 Policy Debates

In the early years of the New Economic Policy (NEP), at his seminars, Chayanov debated the state of agriculture, the impact of capitalism in rural areas, and socialist reform. The opponents in these debates were largely Marxist, for whom positions opened up in 1925 at their own Institute at the Communist Academy, where they were to create a “Marxist Science” of the rural sector (Solomon 1975, p. 562). Within a few years, the Agrarian Marxists rivaled the Organization-Production School in prominence. Debates continued through the late 1920s, and within the bounds of socialist theory and peasant production, his point of view was tolerated and his research and publications continued. These debates are the focus of exhaustive research by Mark Harrison, who published an important series of articles (1975, 1977, 1979) and of a book by Solomon (2019). They show how contested the policies were in this period and how rich was their empirical base. Shanin (1986, p. 9) underscores that behind these policy debates, from 1917 on, was an urgent seemingly unresolvable dilemma in crises from the domestic threat: “...of a postrevolutionary state intertwined with a socialist city-bound party facing a massive peasantry organized in rejuvenated peasant communes, in which 85% of the country’s population held more than 95% of its arable land.”

Chayanov and his school supported the Bolshevik government’s effort to conciliate the peasants in bringing an end to War Communism, a period of forced grain requisitions during the Civil War. Under the New Economic Policy (NEP), private trade flourished. The government aimed to use market incentives to build socialist relations in the rural economy while encouraging “primitive socialist accumulation.” The state controlled heavy industry, mining, banking, transport and foreign trade, and it nationalized the land, on which the bulk of the population were small producers. Chayanov wrote extensively with an agronomist’s expertise about extension services and cooperative organization for improvement of rural access to technology and finance. He also shared with the framers of NEP a longer-run vision for a socialist rural economy. He was not a Marxist, but nor was he a liberal economist. In his view,

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<sup>6</sup> See Thorner (1986, p. xii) and Solomon (1975, p. 554, n. 1), who covers the 1920s controversy over the nature of rural transformation, closely tracked from *zemstvo* compilations.

peasants' accounts could not be fit into existing models of household incentives in a market economy.

In debates, briefly, the Organization-Production School and Marxists disagreed on the degree of market penetration that might influence peasants' behavior. The Marxists were led by the distinguished director of the Agrarian Institute of the Communist Academy, L. N. Kritsman (1890–1937), editor of its journal *Na Agrarnom Fronte* (*On the Agricultural Front*). Harrison (1977) summarizes their rejection of Chayanov's clear advocacy of a further dominant economic role in the countryside for peasants and his denial that growing class conflict of a capitalist nature was evident in the countryside. The Marxists posited that in rural, as in industrial sectors, revolutionary potential among the "rural proletariat" was driven by class differentiation, considerable evidence for which could be found in Lenin's writing, as well as their own (p. 324):

Russian Marxism saw the Russian peasantry as a formation which had never existed in itself, but only through its subjection to the outside world...With the Emancipation of 1861 it entered upon...a period of slow and painful decomposition... by capitalist industrialisation. Changes in the larger social order brought about the emergence of a rural petty bourgeoisie and a class of rural labourers still largely—but not entirely—attached to the land. By 1914 the peasantry faced a choice: the continued development of large-scale capitalist agriculture, or siding with the Russian working class in a socialist revolution, ultimately to be completed by the socialisation of agriculture.

Chayanov's opposing view was that peasants were not differentiated by class but by scale of farming, vertical differentiation. Larger or smaller landholdings on now state-owned lands, he observed, were influenced by demographic, or internal, factors in the household and by crop requirements. His own data, depending on the region, did not consistently bear out his assessment of the peasants' calculus (Shanin 1986, p. 2). There was considerable complexity in the structure of peasant households and clear evidence of their extensive participation in markets. Chayanov understood this. "Economic systems exist side by side and make for very complicated conglomerations" (1986, p. 27).<sup>7</sup> He argued, like Karl Polanyi (1886–1964), that the social sciences must develop "a wider frame of reference to which the market itself is referable" (Özveren 2005, p. 774).

After 1927, despite the elimination at the highest levels of the Communist Party of the "leftist opposition", or those who sought rapid industrialization, the pace of planned transformation was still an urgent and unanswered question. However, sympathy for the peasantry during these years when peasants held grain back from the market to government observers touched on treason.<sup>8</sup> Chayanov remained roughly aligned with Bolshevik government policy, now critical of the NEP, in his 1927 book *Osnovnye idei i formy organizatsii sel'skokhozyaistvennoi kooperatsii* [The basic ideas and forms of agricultural cooperation], where he accepted the need for more rapid transformation and the formal goals of the collectivization program, although

<sup>7</sup> Quoted in Özveren 2005, p. 774.

<sup>8</sup> Erlich (2013, pp. 189–92), provides a chronology of industrialization debates and shifts in party decisions from 1921 through the autumn of 1929, when the collectivization drive began.

he favored “self-collectivization” or “cooperative collectivisation” in 1929 (Shanin 2009, p. 88) and warned against exclusive support for large-scale farms and managers who lacked knowledge of local conditions of farming and who were dependent on the state bureaucracy (see chapters by Boyanovsky and King on the fierce debates over government policy and the elimination of the Left and then the Right Opposition). Chayanov was removed from his post. In an ongoing crisis of grain collections, and amidst further political purges, his institute was closed, and in 1930, he was among those arrested as opponents to rapid industrialization; he was charged with belonging to a “neopopulist” front.<sup>9</sup> To summarize, as an economist and sociologist, his views were distant from the cultural orientation of the populists: although he disagreed with Agrarian Marxists’ on conditions of rural capitalism in the late 1920s, he supported modernization and mechanization, along with rural socialism. The only contributing documentation to the charge of “neopopulism” in his interrogation was a work of fiction he wrote in 1920 under the pseudonym Ivan Kremnev, a utopian vision of a future without the Bolsheviks, where peasant agriculture was dominant and state efforts to destroy the family and impose large-scale farming would fail (Bourgholtzer 1999, p. 36).

## 2 The Theory of Peasant Economy

Chayanov’s academic and policy writings show his consistent adherence to his marginalist model of peasant behavior, which he derived from agricultural economics, sociology, and Austrian marginalist theory. He considered himself a social agronomist, however, and not a liberal economist, and he presented an alternative analysis to the neoclassical model of entrepreneurial behavior in describing the peasant household in rural Russia.

The peasantry, he argued, was insulated from market forces because peasants did not use hired labor. Therefore, wages and net profits could not be estimated in an account of a household’s net income. Annual income, in his view, was driven not by profits but by the labor-consumer balance. Because that balance changed over the lifetime of a household, with births, marriages, and deaths, households would adjust the amount of land they needed to rent, buy or sell, and adjust the intensity of their labor effort so that in the long run their household consumption needs were met. As additional labor was required, households could rely on their neighbors’ assistance. He explained the pressure on labor’s contribution when children were

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<sup>9</sup> Russian populists [*narodniki*] formed a political movement in the 1860s and 1870s focused on peasants and the village commune. During and after the revolutionary era, many joined the radical Socialist Revolutionary party, some of whose members supported democratic socialism and thus opposed the Bolsheviks after they seized power; some participated in the Civil War on the side of the Whites. Shanin (2009, pp. 94–5) underscores how miscast the “neopopulism” label as applied to Chayanov, since he did not share their substantive political views, “e.g., their belief in the exclusive virtues of the Russian peasant commune.” Cf Harrison (1975, p. 390), who uses the term Neopopulist, applied to Chayanov by his critic, L. N. Litoshenko.

young, requiring more hours of work, through the time when grown children departed to separate households, when the household consumption needs were met by less labor input. The aim of the household was a satisfactory level of consumption over time without excessive drudgery. Thorner (1986, p. xvi) summarizes Chayanov's model: "Each family strikes a rough balance of equilibrium between the degree of satisfaction of family needs and the degree of drudgery of labor."

This, of course, was famously modeled by Chayanov (1966)

Without an accounting system based on wages, that is, substituting in place of wages the peasants' subjective utility, Chayanov understood that the exact value of labor expended could not be measured. Although rent, interest, and the cost of capital entered into the budget, wages did not. For this reason, the farm could not be assessed with a market-based model, since a key factor of production could not be monetized. The anthropologists Durrenberger and Tannenbaum (2002, p. 140) cite Chayanov to illustrate his argument:

...households would not accept the threshing machine although it made sense in bookkeeping terms; they paid rent higher than the profit of the land; and they grew labor-intensive crops that were less profitable than other crops (Chayanov 1966, pp. 38-41).

Chayanov argued that peasants' method of accounting was subjective but rational, its own economic "system" (Durrenberger and Tannenbaum 2002, p. 140). Due to demography and circumstances, peasants were not mainly motivated by profits, he argued, but by a satisfactory locally homogeneous standard of consumption at the intersection of the two utility curves.

Because of this, he argued, the peasantry co-exists with other systems. By contrast with Marxist understanding, it is not likely to disappear as capitalism advances to higher stages of economic development. For example, a peasant economy co-existed with and survived after the feudal stage of production disappeared. Chayanov understood the system as a fixture in larger systems, small-scale family labor in a world dominated by trading and finance capital (Lehmann 1982, pp. 143-4).

To support his arguments on the labor-consumer balance, he assessed values for factors of production (excluding labor) in the peasant economy from the size of holdings, qualities of soil, crops growth, livestock, manure, location, land, and interest rates on capital loans by current prices, feasibility of crafts and trades, availability of alternative work, and relative density of population (Thorner 1986, p. xvii). Thorner (p. xvii) summarizes the model: "...the equilibrium of production and consumption evaluations or, more accurately, a desire to maintain a constant level of well-being." Chayanov expressed this graphically (1986, p. 219-20), depicting,

the basis on which capital renewal and accumulation takes place in the family farm as an equilibrium between subjective evaluations of different on farm phenomena. I have used demand satisfaction, marginal expenditure of labor effort, equilibrium graphs, and displacement of curves in conventional terms, not subject to precise methods.

He acknowledged his debt to his Marxist opponents and to the "many services to economics" of the Austrian school (p. xvii). He insisted, however, that there could not be only one system at work in a national economy where there was a peasantry, who maintained a subjective evaluation of the utility of objects.

Despite the large empirical database of village surveys accessible in the Soviet and post-Soviet periods, Russian historians remain divided over whether Lenin's or Chayanov's description of the Russian rural sector more accurately reflects country-wide dynamics of peasant production in the late nineteenth and early twentieth century. Despite decades of quantitative research, in other words, there is still a lively controversy over the level of well-being in the countryside before the revolution. One reason, and here Marxists and those opposed agree, is that given the diversity of regional economic conditions, country-wide data cannot not be seen as representative of one or the other model (Leonard 1989, 507–514). Chayanov's model was not designed to apply to all peasants. His work addressed peasants in an agrarian country with low population density, such as Russia, but also other less developed countries.<sup>10</sup>

By contrast with complexity and vagueness in the model, his policy guidelines were clear. He advocated eliminating backwardness and encouraging modernization, while treating peasant households with care. Local authorities, in his view, should be responsible for household-focused agricultural extension along with providing cooperative infrastructure to introduce new technologies and spread knowledge. He advocated vertical integration, where large and small farms cooperated and exchanged knowledge and supplies, not horizontal integration (forced collectivization), which would ignore local knowledge and skills and impose routines at the expense of household decision-making (Kerblay 1986, pp. lii–liii).

### 3 Chayanov's Reception

#### 3.1 *In Russia: The 1920s*

In his treatment of a peasant economy as a system in regions and countries where population density was low, excluding by definition more advanced urbanized economies as well as many less developed ones, he acknowledges that his theory is not universally applicable. His life work was about Russia. This is paradoxical, as anthropologist Tannenbaum observes (1984, p. 29), “his goal was the development of a general theory of peasant household economics,” applicable to all societies in a formalist attempt to develop universal concepts (Schneider 1974, pp. 2–14; cited in Hedican 2009, p. 428). To the Bolshevik government of the early 1920s, however, his understanding of the potential persistence of the peasantry as a separate system under socialism and communism was contentious but tolerable, since his expertise was needed.

Indeed, in his advocacy of a large role for the state, Chayanov's views were compatible with the early Soviet Bolshevik regime. Before World War I, he and

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<sup>10</sup> Chayanov argued that some 90 percent of rural households at the turn of the century in Russia could be described by his model (Thorner 1986, p. xiii).

his colleagues worked in the countryside during the Stolypin reforms. They became convinced, Solomon (1975, p. 563) writes:

... that the redistribution of land would not itself alleviate rural backwardness. What was needed was the thoroughgoing reorganization of agriculture—the intensification of production, the introduction of complex machinery, and the relegation of certain farm tasks to the rural cooperatives.

In the 1920s, he clarified his views of the state's role. Kerblay (1986, pp. xliii-xliv) describes Chayanov's vision of socialism:

...the single will (that of the state), as a natural economy governed by the requirement of satisfying society's needs with the available resources...whose rationality...is evaluated at the macroeconomic level by determining the best use of the labor force for the increase of national income.

"One cannot help but notice," Kerblay goes on, that in some respects, this point of view was similar to that found in Bukharin's more radical theses, *The Economics of the Transition Period* (1920).

Chayanov's positive reception by the Bolsheviks was not only due to his experience with and knowledge of the peasant economy but also due to his expertise on cooperatives, which were a force in rural advancement before and after the Russian Revolution. In 1921 in Russia, the cooperatives were key to trade and distribution in Russia, and Chayanov was a major figure in the cooperative movement. With the economist, Sergei Prokopovich, he organized a department for the study of cooperatives at the people's Shanyavskii University. He helped found the all-Russian Center for cooperative committees in 1915, later the all-Russian Council of cooperative congresses in March 1917. He personally organized a successful cooperative venture in Russia, the Linen Center, and helped it develop an export trade. At this time, there were some 10 to 20 million members of more than 50,000 cooperatives (Sobolev et al. 2018, pp. 68–9). In the view of Chayanov, cooperatives were laying a foundation for the economic and cultural development of the Russian countryside. The Bolshevik government placed cooperative organizations under state control during the Civil War, and distribution replaced exchange, but under the NEP cooperation was revived, along with agricultural partnerships, although their function was narrowed only to supply grain to the cities (Sobolev et al. 2018, p. 69). Chayanov's expertise on cooperatives gave him a secure place in policy until 1929–1930.

### 3.2 *Abroad: World War I and After*

As in Russia, in Belgium, France, Spain, England, and the USA, where cooperative bank societies and unions of bank societies appeared, Chayanov was recognized in a larger movement (Morales Gutierrez and Carlos 2005, p. 48). After World War I, cooperatives were a key element of economic restructuring. They financed investments for the improvement of farms, and they were used widely in the more advanced farm sector. In colonial rule, perceptions of the state's moral and financial obligation

to farms were enhanced by bankruptcies, and the idea of development institutions gained strength in colonial development planning (Morales Gutierrez and Carlos 2005). As development policy matured, colonial policies became “a cultural, not just an economic, project,” and the language of aid in policy publications dichotomized the “modern” and the “traditional” and the “West” and the “rest.” often invoked to justify development interventions” (Kothari 2005, p. 49).<sup>11</sup>

In colonial policy, the role of the state was paramount. In this regard, Soviet collectivization of the Russian countryside entirely overshadowed interest abroad in Chayanov, and in Russia, his works were banned. In 1966, Kerblay (1986, p. xxv) writes: “...Chayanov is a name virtually forgotten today both in the USSR and in the West.”<sup>12</sup> Lack of interest in close analysis of peasant households in general can be seen in the development policy environment of the 1950s and even early 1960s.

Development theory in the 1950s continued the colonial era theorizing of economies divided into a modern and traditional sector—the latter seen as socio-logically and technologically backward.<sup>13</sup> As in Soviet model, the aim was to shift the terms of trade against the traditional sector, squeezing out labor from agriculture to encourage industrialization without causing inflation. A “leading sector approach” was developed by Lewis (1955) and revised by Ranis and Fei (1966). It follows from the concept of surplus labor from disequilibrium theory, which models the savings constraint on industrialization (Kaldor, 1977). Policy that encourages the extraction of labor resources will encourage rural savings and contribute to industrialization by lowering food costs and adding to foreign exchange by commodity exports.

Technological development in agriculture was also given prominence by aid agencies for a “green revolution,” oriented to new seeds, better farm management, measured amounts of fertilizers, pesticides, and water. These programs constituted Ruttan’s (2002) “High-Payoff Input Model.” Technological improvements were labor-intensive, land saving, scale neutral, and efficient in the use of costly capital resources to meet the needs of developing countries. For many reasons, these efforts tended to be insufficient, although there certainly were some countries and regions in which the green revolution produced striking results. The primary drawback was that the new technologies required appropriate institutional settings. Agrarian communities sometimes did not absorb technological innovation. For example, where plots were small and scale-neutral technologies had been imposed, the technologies were ill-adapted to local custom. High-payoff models left large farms dominant, with technological innovation held back on small subsistence farms.

World governments and economists closely followed reported successes of Soviet industrialization policy, despite its sacrifices. The role of the state was underscored in the overcoming of relative backwardness in developing countries. A book review by a leading economist of the Soviet Union of the translation of Chayanov (Millar

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<sup>11</sup> On colonial sociology see Dirks (1992).

<sup>12</sup> But see Kerblay (1986, p. xxv, n. 2) for references to Chayanov’s works (by A.L. Vainshtein and N. A. Savitskii), and western acknowledgements of his contributions (Werner Sombart, Alexander Gerschenkron, C. von Dietze, J. H. Boeke, and M. M. Postan).

<sup>13</sup> This draws on (Leonard 2010).



1968 p. 645) shows the impact of that first translation on the field of Soviet research. He argued that “the book challenged virtually universal understandings of the appropriateness of Stalin’s industrialization, which at that time was closely followed as a model for the developing world.”

### 3.3 *Abroad: The 1960s–1980s*

In the late 1960s and 1970s, the policy environment changed significantly. Soviet industrialization came to be criticized more widely by development specialists, many of whom were less optimistic about the role of the state. China’s introduction of the incentive-oriented Household Responsibility System in 1979 (Lin 1988) was an example of the global shift in policy to place emphasis in reform on peasant initiative and livelihood. Public choice literature and mainstream economics rejected industrial policy to force goods from the agricultural sector, and many economists came to challenge the state’s potential role in economic development (Gregory 1991).

The shift in views was accompanied by a revival of Chayanov, although his influence had spread to some countries already in the 1930s.<sup>14</sup> Shanin writes in his introduction to the republication of the English translation of *The Theory of Peasant Economy* (1986, p. 2) that Chayanov was extensively read and used by “analysts of different persuasions, countries, and academic disciplines.” There was a decisive impact in India, where dominant Marxist economists emphasized the evident elites in the rural population as signs of considerable social and economic differentiation, leading to village-level conflict, and they documented the uneven distribution and cumulative impact of commercialization on social change. Dewey (2019), however, emphasizes that there was a continuing challenge to this “nationalist-Marxist orthodoxy,” and that Chayanov’s model is the alternative analysis, one that “no one can ignore” (p. 495). Charlesworth (1979, p. 65–66) shows that the challenge to Marxism began when Chayanov’s concepts were directly imported into India in the stratification debate by Kessinger (1975) and Dewey (1976).

The revival of Chayanov as an alternative to the Marxist interpretation of Russian economic history can be seen in Evsey Domar’s 1968 review of *The Theory of Peasant Economy* (1966) in the *American Economic Review*, praised because of its welcome attention to the peasantry (see also the chapter by Boyanovsky in this book).

I would say that Chayanov’s main talent lay not in creating new economic theories, but in observing Russian peasants. This he did with a keen eye and much common sense. For instance, his treatment of the peasant family as a dynamic institution which changes its size and structure over time, is excellent; these changes seem to account for a good part of the differences in income and wealth among peasants, rather than the process of social differentiation stressed by Lenin (Domar 1968, p. 634).

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<sup>14</sup> Chayanov had influence in the 1930s on Dutch and Japanese social scientists (Chibnik 1984, p. 335).

### 3.3.1 Economic Anthropology

Chayanov's theoretical and methodological approach became a rich resource in economic anthropology. Eric Wolf (1966, pp. 14–15) made reference to Chayanov's work on the peasant economy in his book on peasant wars, and this stimulated considerable interest (Hedican 2009, p. 420). In 1974, the ethnographer Marshal Sahlins (1974) developed from Chayanov a norm of behavior for comparison across households.<sup>15</sup> He called his calculus "Chayanov's rule": "the greater the relative working capacity of the household, the less its members work" (cited in Smith 1979, p. 477–8). This was criticized since it failed to consider the trade-off in Chayanov between disutility of effort and utility of income (Sahlins 1974, p. 87; cf Tannenbaum 1984; Durrenberger 1984, p. 140). However, the labor-consumer balance proved useful, and others afterward argued that when different levels of output were actually observed among households, this could indeed be related to the balance (Hedican 2009, p. 421) with the reservation that the data were too often used to fit the theory (Tannenbaum 1984, p. 935).

Controversy over "Chayanov's rule" stimulated discussion, and in 1984, an edited volume was published that was in some ways a Chayanovian *festschrift*" (Hedican 2009, p. 425). Anthropologists theorized how to distinguish between the labor-consumer balance and the impact of the social and political system and ritual on community production. Evans (1974, p. 269) argues, for example, that "the differences between the theoretically expected ideal [i.e., the consumer-producer ratio] is a measure of the impact of the social system on the economic system." He attempted to use mathematical approaches to assess the degree of divergence of the empirical production pattern from the Chayanov model with suggestions about the nature of the differences. Chibnik (1984) analyzed twelve data sets across stratified (in Switzerland and Iowa) and unstratified societies finding that stratification strongly affects intercommunity variation, but that even in stratified communities, "household composition considerably influences family farm production" (p. 339). Along with social organization, political structures, ritual, and ideology, the consumer-producer ratio became part of the standard analysis (Durrenberger and Tannenbaum 2002, p. 143).<sup>16</sup> More generally, anthropologists have been open to Chayanov's approach since Geertz (1983, p. 56), because he sees things "from the native's point of view," sees culture as "a shared system of intersubjective symbols and meanings" (Hedican 2009, p. 430).

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<sup>15</sup> His examples were very small samples (a dozen or so cases) of Melanesian ethnographers' field work mainly in Tonga and New Guinea, where survey data had originally been collected for other purposes and were not longitudinal.

<sup>16</sup> "...households in the best positions to produce more, having less urgent utility curves, do not overproduce. Shan operate only in terms of the single constraint of the equilibrium between drudgery and utility (Durrenberger and Tannenbaum, p. 143).

### 3.3.2 Sociology: Peasant Studies

Harrison (1979, p. 87) notes the rise of Chayanov's influence in peasant studies in the 1960s, when development theorists rapidly expanded their empirical base from historical surveys as well as developing country studies:

In the mid-sixties some of Chayanov's most important works were republished in the West and translated into English. Within a few years, reference to Chayanov became obligatory for any serious work on peasant farming....Thus, ten years ago, the Chayanov tradition was dominant in many areas of agrarian economics and sociology.

According to Harrison, the reasons for the revival of his ideas were the following: the Green Revolution along with "new development strategies for agriculture in South and South-East Asia, based predominantly input-intensification," and the turn of attention to small-scale farming and new technologies. To Harrison, the third way in which Chayanov was viewed as important in this era was that he and his colleagues in the Academy were not only researchers with policy roles but also practitioners and organizers, who had worked on technical assistance projects and extension.

As Harrison shows, Chayanov had a critical role in peasant studies despite disagreement with Marxist approaches. Two former editors, Bernstein and Byres of the *Journal of Peasant Studies*, review Chayanov's influence in the introduction to their then new *Journal of Agrarian Change* (2001). They note that the translation of Chayanov (1966) generated considerable research on the experience of colonial rule in less developed countries of Asia and Africa. Peasant studies emerged in an environment of interest in pre-capitalist peasant societies, and, as in economic anthropology, bottom-up perception and multi-disciplinarity became important tools of understanding communities in development.

### 3.3.3 Political Science

At the close of the Vietnam war era, in peasant studies, where political science was integrated with anthropology, a major work by James Scott, *The Moral Economy of the Peasant* (1976), introduced a Chayanovian perspective (peasant forms of economic rationality) in research on collective action and agrarian institutions<sup>17</sup> in poor, largely rural countries. Reviewing the era, decades ago, when the peasant was widely understood as a major historical protagonist (Edelman 2005, p. 333) writes:

Scott's *The Moral Economy of the Peasant* came toward the end of a wave of foundational books in peasant studies, both building on such works and insistently injecting a new cultural and even psychological dimension in political science....

This was sometimes lacking, for example, in Wolf (1966) and Barrington Moore's *The Social Origins of Dictatorship and Democracy* (1966). Like Chayanov, Scott emphasized the importance to the peasant family of a subjectively unacceptable level of drudgery, leading the peasant family to stop working, and this demonstrated the

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<sup>17</sup> Defined as norms and routines in Bardhan (1989) in rural development as norms and routines.

relevance of the subsistence economy, where peasants do not accumulate surplus. Neither Scott nor Chayanov exclude peasants' pursuit of accumulation where the living standard is rising (p. 334). Scott's work was received critically by Samuel Popkin in *The Rational Peasant* (1979), which led to a large still ongoing debate among historians about peasant transformations as markets expand.

### 3.4 *Management Studies*

EU policy relevant research on ecological economics and sustainable agriculture has raised interest in small farming: labor-intensive agriculture, adaptive farm structure, appropriate technologies, and small-scale rural support services. Policies under frequent review include those in former communist states where former farm structures led to special programs for energy conservation and small farm development. The final report on High Nature Value—Farming profitability refers to Chayanov's concepts as still "very relevant" in current conditions (EIP-AGRI Focus Group Final Report 2016, n. 3, p. 8). Chayanov's work on peasant cooperatives has similarly served as a model for rural management theory. It drew attention in modeling digital consumer cooperatives (Glushchenko et al. 2021, p. 43). Sanches De Puerta (1994, p. 15) writes about how important Chayanov was in the 1990s in stimulating locally interactive extension and in countries moving away from the US style extension which have relevance mainly to advanced economies with fully marketized farming.

### 3.5 *Development Theory*

Upon republication in the 1980s, one reviewer remarked that Chayanov's *The Theory of Peasant Economy* is "...an intellectual safari to a world that no longer exists in the Soviet Union but may still be found in many parts of the Third World, where family subsistence agriculture is still pursued" (Copp 1989, p. 626). In the 1980s and 1990s, interest grew in still lagging economies after years of development aid; attention focused on structural constraints and village learning.

By the 1990s, more critics of development theory rejected the highly complex economic price-based models of constraints on growth composed by economic theorists, including rates of return on investment across the economy, income and price elasticities of demand for food, and the impact of appropriately advanced technologies. The data required for such models could not realistically be found in developing countries (Sah and Stiglitz 1992). There was also increased scholarship on disadvantaged peasant communities. Marxist sociologists and economists focused on peasants, "structurally marginalized and excluded from the benefit of modernity" (Shanin 1986, p. 21). Conceptual criticism was also aimed at capitalist countries, which had not secured general welfare (Shanin 1986, p. 21, 24).

Finally, by the 1990s, there was substantial evidence of the success of China's reform, mentioned above, the Household Responsibility System (HRS), which made the household the residual claimant on the land. Between 1978 and 1989 agricultural output (in constant prices) nearly doubled in gross value of output (De Brauw et al. 2002, p. 26). In China, as in Korea, there was research and extension for improved seed and fertilizers, services, including credit and marketing agencies and cooperatives, and rural governmental resources for building feeder roads and market information infrastructure. Household incentive-oriented reform has not been a success in all regions of countries, where it was attempted, but in some, farm incomes rose. Food became cheaper, more diverse, and better in quality.

### 3.6 Conclusion: Chayanov in Russia from the 1990s

In his introduction to his translated 2020 edition of Chayanov's *Main Ideas and Methods of Social Agronomy*, part 1 (1918) Alexander Nikulin writes that Chayanov was a social philosopher as well as activist and psychologist. After the end of communism, Chayanov's life and work generated interest in restoring household farming as the Russian government dismantled collective farming Alexander Nikonov, President of the Soviet Academy of Agricultural Sciences (VASKhNIL), began the preparation for republication of Chayanov's works, and he made it an urgent priority. Shanin recalls (2009, p. 83) his own invitation to speak about Chayanov in Russia, the swell of interest: "...about the man whom they now came to accept as their most talented colleague, the name of whom was spoken for generations in whispers and whose actual works were unknown to most of them." He was "iconized," he writes, "rather than utilized."

In the critical environment after Gorbachev's agricultural reform program within Perestroika, and into the 1990s, there was a consensus, after years of scarcity, about the need for progress in agriculture and for the reform to focus on peasant farming. Although in this period, there was considerable interest in Chayanov's works, his lasting influence in Russia lay not in policy, but in interdisciplinary research. There is still a significant readership of Chayanov in Russia, ensuring his continuing, if long delayed, recognition as one of the most original and powerful thinkers from the 1920s.

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# N. D. Kondratiev and a New Methodological Agenda for Economics



Natalia Makasheva

Nikolai Dmitrievich Kondratiev (1892–1938),<sup>1</sup> a tragic figure in the history of economic thought and Russia’s most internationally renowned economist, who gained the worldwide recognition mainly for his idea of large cycles (major cycles, long waves) in economic development, belonged to the generation described by him with regret as: “The heavy chariot of history has passed our generation by”

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<sup>1</sup> He was born on March 4(17) 1892 in a peasant family in the village Galuevskaya, Kineshemky district, Kostroma province (now Vichuga district, Ivanovskaya oblast). Thanks to his abilities, striving for knowledge and persistence he entered and in 1915 graduated from the St. Petersburg (Petrograd) university brilliantly. He was retained at the chair of political economy and statistics to prepare for professorship. During his university years such renowned scientists as M. I. Tugan-Baranovsky, L. I. Petrazhitsky, A. S. Lappo-Danilevsky, M. M. Kovalevsky profoundly influenced Kondratiev’s academic activity and his world view. Engaged in research work Kondratiev was also involved in political life: in his early years, peasant and socialist sympathies prompted him to join the Socialist Revolutionary Party (SRP). In 1917 he actively took part in the work on agrarian reform and food supply, participated in the work of various committees and organisations (for example, the Central Land Committee). On the eve of the October Revolution, Kondratiev was appointed Deputy Minister of Food Supply in the last Provisional Government. In November 1917 he took part in the All-Russian Congress of Food Supply and was elected a member of the Constituent Assembly representing Kostroma province on the list of the SRP. After the Bolsheviks came to power Kondratiev, as a member of the overthrown government, was arrested, but soon released. It was his first, but not the last, arrest by the Soviet authorities. In 1922 he was arrested as a member of the Union of Regeneration of Russia and the former member of the SRP and spent several months in jail, where he continued to work for the People’s Commissariat of Agriculture (PCA). His name was on the list of persons to be expelled from the country, but the decision to expel him was suspended at the request of the PCA. In early 1918 Kondratiev moved to Moscow. He began teaching at the Shanyavsky University, working in various organizations related to agriculture and cooperation. In 1919 his scientific interests led him to the Petrovskaya Agricultural Academy. In 1920, within the framework of the Academy a small research division was established—the Conjunction Institute (first within the PCA, and since 1923 as a part of the People’s Commissariat of Finance), which was headed by Kondratiev until 1928.

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(Kondratiev 1998, vol. 4, 299). He meant those who had received education before the October revolution (and in many cases not only in Russia but also abroad<sup>2</sup>) and had started their professional activities 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 to 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>3</sup> Those economists who were involved in research (whose research far not always could 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 part, 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 emigration of many talented economists and destruction of the pre-revolution system of the research activities, the 1920s became the “Golden Age” of the economic science in Russia—a fact recognized by the majority of those who study the history of the Russian economic thought (Campbell 2012, 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.

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The Institute very soon became a leading center for market and conjuncture research. This period (1920–1928) was very fruitful for Kondratiev: while doing scientific research, he successfully run the Institute, worked for various government economic agencies, actively participated in discussions on the path of industrialization, basic principles of planning, prospects of industrial and agricultural production, and he was also involved in preparing the first five-year plan of the national economy. Kondratiev opposed Stalin’s plan for accelerated industrialization, and this was the main reason, albeit not the only one, for his arrest in June 1930. Along with a number of renowned economists (Yurovsky, Chayanov and others), Kondratiev was accused of membership of the so-called Labour Peasant Party aimed at “to overthrow of Soviet power and establish a bourgeois democratic republic.” Recognized as the most important member of this “criminal group” he was sentenced (in January 1932) to eight years in prison, Suzdal political prison being determined as the place of his imprisonment. While in jail Kondratiev did his best to continue scientific research. In 1938 he was accused of anti-Soviet activities and sentenced to death, shot on 17 September and buried in a mass grave at the infamous Butovsky polygon near Moscow. N. D. Kondratiev was fully rehabilitated in 1987.

<sup>2</sup> For instance, L. N. Yurovsky-Kondratiev’s colleague and one of the authors of the monetary reform of 1922 studied at 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 to continue his education.

<sup>3</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. Yurovsky, E. E. Slutsky, V. A. Bazarov, N. P. Makarov, N. N. Sukhanov, A. L. Vainshtein, et al.

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.

## 1 The New Methodological Agenda: Origins and Interpretation

A meaningful point is that the changes taking place in the Russian economics of the 1920s, and which 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, 1). First and foremost, we are referring 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 transform economics into a rigorous science that in terms of objectivity would not be inferior to natural sciences and would reconcile the theoretical-quantitative and the empirical-quantitative approaches and unify mathematics, statistics and economic theory (Frisch 1932, 1933; Schumpeter 1933). 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>4</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 planned to do), especially in the field of cycles and crises, and economic dynamics in general.

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<sup>4</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 that in response to the letter 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 declined the invitation, explaining that by the fact that his studies were focused more on mathematics rather than economics (Bjerkholt 2017). 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).

In terms of the methodology, one of the goals of the econometric movement was to bridge the gap between the theoretical and empirical approaches as well as between the deductive and inductive methods. The problems inherent in such 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 toward a possible solution solving this.

The idea that the statistical studies and the 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 Statistics. Theory must be invested with reality and life of fact. But the difficulties of this union are immensely great" (Jevons 1879, 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 contrary, by their unprejudiced combination alone that any complete development of economic science is possible" (Keynes 1890, 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 dispute 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 "The 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, 180). Moreover, Marshall took a step toward the theory of dynamics when he introduced the time dimension into the theory by considering marketday-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 be hardly 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 nineteenth century—the *Methodenstreit*—was consigned to the past. In lieu of the *true method* issue, some economists, especially those engaged in the 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 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, 1).

Many economists connected the hope for reconciliation between the theoretical and empirical approaches with advanced statistical methods, application in the 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, such as 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 the statistical science—as an evidence of scientific status of any discipline (Fisher 1925), and some economists even started to appraise the state of the economic science from such point of view and to regard its evolution as the statistics-oriented movement, qualified by them as a great trend (Ise 1932; Stigler 1962).

In the 1920s, the idea that economics—just like any other discipline—must be logically strict and based on the 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>5</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 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 twentieth century initiated the application of mathematics in economics—V. K. Dmitriev, V.F. Arnold, 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>6</sup> However, the process of adoption of mathematics as the language of economics was neither easy nor straightforward.

The contribution 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 the Marx's labor theory of value, and therefore, their basic ideas and methods were rejected by most of Russian economists who had fallen under the influence of Marx's doctrine.<sup>7</sup> And the work of Dmitriev, who adopted marginalism and used the 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>8</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.

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<sup>5</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. Bortkiewicz, K. Person, R. Fisher, O. Anderson, V. Romanovsky, N. S. Chetverikov.

<sup>6</sup> For a detailed history of mathematical economics in Russia see (Belykh 2017).

<sup>7</sup> On perception of Austrian school by Russian economists see (Avtonomov and Makasheva 2018).

<sup>8</sup> On value-price problem debates in Russia see (Allisson 2015).

By the early 1920s, not only in Russia, but also in the West countries, 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 the resolute step made toward its quantification and mathematization. But this did not happen. In the early twentieth century, it became clear that the hopes that had been nourished in the last three decades of the nineteenth century, to make the economics the “exact science,” “social mechanics,” “physique sociale,” “mechanics of utility” (Moore 1914, 84–85) had faded away. Indeed, not only in the USA, where the historical school and institutionalism dominated absolutely (Crum 1925; Dorfman 1955), but also well in Europe, where the trend toward 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, Wickcell, or Pigou, tried to dispense with the minimal number of formal instruments and mathematical symbols (Niehans 1990, 159–163).<sup>9</sup>

Such a situation could have been explained by the facts that the audience lacked proper training in mathematics, or that the 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, 86). Moore also criticized Marshall’s method for being “limited to functions of one variable” (Moore 1929, 93). It is therefore not surprising that applying this theory to analysis of the rapidly changing economic reality sowed strong doubts among many economists.

Demand for the theory which could deal with economic changes was satisfied partly by the historical school and institutionalism, which did not make claims 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, 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 as “postulate” (Grossman 1977, 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 the 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

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<sup>9</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, 150).

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 number of Russian economists connected 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 the 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 the true scientific discipline, framing the “battles” between the 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>10</sup> In the 1920s, just a few of Kondratiev’s works were available for Western researchers: In 1926, 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, 192), and, one can add, extending beyond the boundaries of the field of economic research (see, for example, Chapin 1925).

The first two of the aforementioned works by Kondratiev 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, 169), and today is a part of the scientific economic discourse (Campbell 2012, 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

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<sup>10</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 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).

importance (Kondratiev 2004). Meanwhile, he certainly understood that the study of cycles had its own significance as well.<sup>11</sup>

## 2 The Methodological Agenda and the Problem of Cycle

The choice, made by Kondratiev regarding the subject of research,<sup>12</sup> as well as selection of his works to be published abroad, reflects 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 the World War I. In the pre-war years, the two lines of research had been outlined—the empirical and the theoretical one; 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 first, according to A. Löwe, were in the “obvious contradiction” with the latter (Hayek 1933, 33). That contradiction was manifest in the fact that the equilibrium approach happened to be productive only in the case 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 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, 31). “Statistics can never prove or disprove a theoretical explanation, they can only present problems or offer fields for theoretical research” (Hayek 1933, 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).

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 to conduct the conjuncture research,

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<sup>11</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 discussion of the methodology and principles of planning and forecasting, current economic problems, paid special attention to the condition and development of agriculture, etc.

<sup>12</sup> The choice was made in 1922 by publication of the “Mirovaya ekonomika i eye kon’yunktura vo vremya i posle voiny” (Kondratieff 2004).



having described his method of the research as “*a concrete empirical method and, in particular, statistical method,*” but alongside this method he also assigned “an important role to an abstract method” (Kondratiev 1998, vol. 1, 23).<sup>13</sup> A noteworthy point is that, as early as in 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, 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 econometric movement.<sup>14</sup>

While studying business cycles, the adherents of the empirical stream (Fisher 1911; Mitchell 1913; Moore 1914) 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, 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, 31).

In mid-1920s, Kondratiev focused his efforts mainly on the statistical analysis of conjuncture and was interested in the works of foreign economists in this field, and above all in the works of Mitchell, the head of 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.<sup>15</sup> As noted by Barnett, under the impression of the meetings with

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<sup>13</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 the 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” (2nd ed., 1926) in his last published work “Dynamics of industrial and agricultural prices” (1928).

<sup>14</sup> There is a large body of literature on econometrics and its history. Let us mention just a few: (Quo 1997; Spanos 2006; Pesaran 2004; Louçã 2012; Morgan 1990).

<sup>15</sup> In 1924 Kondratiev was missioned abroad by the People’s Commissariat of Agriculture to study agricultural production and markets which were of interest to Soviet authorities in terms of Russian

Kondratiev, Mitchell agreed to publish his article in the “Voprosy Kon’yunktury” (“Issues of Conjunction”) journal published by the Conjunction Institute, as well as 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, 94–96).

While in the USA, Kondratiev also had meetings with S. Kuznets, then a young colleague of Mitchell, and maintained contacts with him afterward. Kondratiev participated in the annual meeting of the American Economic Association in Chicago on December 30, 1924, attended by the leading American economists, and “gave a summary ...of the collapse and subsequent stabilization of the Russian currency” (Round Table Conferences 1925, 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 the American economists’ attention to the idea of major cycles and to the works by Russian economists, but also to envision possible cooperation between American and Russian researchers.

### 3 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 toward the theory of economic dynamics raised such questions as what kind of deductive theory could become “a member of such alliance” and what theory could be seen as its embodiment. The attempt to adapt the 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, 151). Moore offered the general equilibrium model with the variables—unlike those of the Walrasian model, depending on time and representing deviations from the 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 economist, Moore contributed to the introduction of the statistical methods into economics and to the connection between economic theory and facts of economic reality (Ezekiel 1930, 663), but that Moore did not finally solve the problem of reconciling theoretical and empirical approaches, nor that of building the bases of

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export prospects. During this four-month trip he visited Germany, England, Canada and the US where he met with many renowned economists and discussed a wide range of issues far beyond those of agriculture and agricultural markets. In the US he also visited his old friend Pitirim Sorokin, who was expelled from Russia in 1922, and became a famous sociologist, one of the founders of this science in the United States. According to Kondratiev’s daughter Elena, Sorokin told her father to stay in America. We are unlikely to ever know the true reason for his failure to follow Sorokin’s advice.

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, 678). With this, neither the equilibrium approach nor the principle of methodological individualism had doubts cast on them, while exactly they, in view of Kuznets, prevented from building the theory of dynamics: “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 rigidity defined individual behavior, its analytic part will remain of little use to any system of dynamic economics” (Kuznets 1930a, 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 the Kuznets’s position—at least, to his view on the trend as a theoretical problem.

Kuznets started his modification of the pure theory from denial of the methodological individualism principle and suggested addressing the market demand and supply functions (Kuznets 1930b). He also discarded the important, albeit usually put implicitly, premises 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 the random shocks to economy may result, as shown by Slutsky, in apparently cyclic process (Slutsky 1927), while the overlapping processes might produce the moving trend (Kuznets 1930b, 409–410).

We do not know whether Kondratiev was familiar with this and other relevant article of Kuznets which were published in 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, 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 as 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 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 in the period from 1921 through to 1928 may be considered as the preliminary stage of building such theory.

#### 4 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 on Kondratiev's view of the true economic science relying on the quite limited number of materials, such as: the articles on statics and dynamics, published before he was been arrested; the unfinished (rather, interrupted) book "Basic Problems of Economic Statics and Dynamics," written in 1930–1931<sup>16</sup>; the macroeconomic model as drawn from his book on the trend, unfortunately lost,<sup>17</sup> and the letters to his wife, written from the Suzdal political prison in 1932–1936.<sup>18</sup> These letters serve the evidence of his intensive research done in prison at least until 1936,<sup>19</sup> and of his striving to be aware of the new publications issued in Russia and abroad, as well as of his correspondence contacts with the leading foreign economists—such as W. Mitchell, I. Fisher, E. Wagemann, and S. Kuznets.<sup>20</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 his wife to send

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<sup>16</sup> Usually this work is referred to as the *Butyrskaya rukopis'* (manuscript) (see, for instance, Kluykin 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, 719).

<sup>17</sup> Letter of September 5, 1934 (Kondratiev 2004, 405–408).

<sup>18</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 evidences of that epoch.

<sup>19</sup> The letter of December 30, 1936 contains his last request to send him a scientific paper (Keynes's "A Treatise on Money"). There is reason to believe that later he was no longer systematically engaged in scientific research, both due to the tightening of the regime of imprisonment and due to the deterioration of health.

<sup>20</sup> We do not know as to what extent the foreign scientists being addressed by Kondratiev's wife were aware of his situation. The fact that in December of 1930 he became a member of the Econometric Society and in 1933—a fellow, can be interpreted dually: 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 option is more probable.

to him. These works can be divided in several groups: statistical studies of the long-term trends in the dynamics of prices, national income and wealth (W. I. King, G. M. Mulhall, R. Pupin), capital (R. Giffen), population and employment (H. L. Moore, P. E. Levasseur)<sup>21</sup>; works on the theory of cycles and crises (W. C. Mitchell, J. B. Clark, A. F. Burns, C. T. Schmidt, A. Spiethoff, I. Fisher, et al.) as well as on the problems of statics and dynamics by (H. L. Moore, W. C. Mitchell, E. H. Vogel, S. Kuznets, et al.); works by 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, 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—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 contents, as well as that he continues 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, 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 and some adherents of the econometric movement, 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

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<sup>21</sup> For instance, in the letter of February 9, 1933 Kondratiev asked his wife to address W. Mitchell with the request to send several volumes from the 13-volume series of books on trend issued under Mitchell’s leadership.

the endogenous nature of socio-economic changes 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, like many of his contemporaries, 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 amount 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, 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 of “reconciliation” between the static and dynamic theories, and emphasized: “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, 199). Thus, the general theory must have the same degree of generality as the theory of statics and not be connected with the 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 the principle of methodological individualism were static. Kondratiev connected 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 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>22</sup> and activities of the great numbers of individuals who constitute a population.<sup>23</sup> This can

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<sup>22</sup> While recognizing the significance of the marginal utility, Kondratiev considers it as insufficient for explanation of the price as a social phenomenon.

<sup>23</sup> In fact, Kondratiev shared Moore’s view that “these laws of large numbers may be mathematically described and be made the foundation of a mathematical science of economics” (Moore 1929, 177). We can well assume that Kondratiev considered it as providing a sound ground for forecasting. He was no doubt well acquainted with Moore’s book. It was in his home library, while in Suzdal, he asked his wife to send it to him (Kondratiev 2004, 383).

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 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, 376). Certainly, all these discourses are related to a particular market and to the static equilibrium.<sup>24</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. 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 entail cumulative effects; and, finally, the different velocities of the adjustment processes (addressed by Kuznets), etc. The last, but by no means the 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 by Kondratiev to his wife, and he never returned to it after that.<sup>25</sup> We may just make assumptions as 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 problems of economics dynamics in general and that of trend in particular, which featured economic studies in the West, 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 of 1934, he quite proudly writes that he has arrived to rather unexpected and quite pessimistic conclusions related to regularity of economic development, and that these conclusions, once published, thereof may cause the “even stronger assault” than his other works did (Kondratiev 2004, 328).<sup>26</sup> Those results made it possible to build the small-sized 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>27</sup>

This model was innovative by many aspects, and according to some experts, its creation forestalled appearance of similar models in the West at least for 20 years;

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<sup>24</sup> In this case, Kondratiev makes a direct reference to Marshall (Kondratiev 1998, vol. 2, 261).

<sup>25</sup> We do not know for sure, when and where this manuscript was handed over to his wife: just before Kondratiev’s dispatch to Suzdal in February of 1932, or ever before.

<sup>26</sup> We may just suggest that he meant the system of equations described dynamic variation of cumulative variables (such as capital and/or population) in the economic system.

<sup>27</sup> The short version of this model was presented in the letter of September 5, 1934 (Kondratiev 2004, 405–410).

in particular, he applied the Cobb–Douglas production function with Hicks-neutral technical progress (Belyanova and Komlev 1989, 33–35). It is most probable that Kondratiev arrived at the function independently of both Ch. Cobb and P. Douglas, as well as J. Hicks, whose works he did not mention at all—unlike the works by K. 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>28</sup> version of the “theoretical ground of statistics,” deeply impressed Kondratiev (Davydov 1991, 456). Application of the concept of population to the phenomena of social life and, above all, 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 the formal mathematical and up-to-date statistical methods in economic research. His efforts corresponded to his perception of the economic science as being close in terms of the methods and tools to the natural-science disciplines dealing with the measurable values, and verifying theories empirically, even in terms of the forecast reliability. We may say that Kondratiev was working in 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.

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<sup>28</sup> Kondratiev makes references not only to works by A. A. Chuprov (first and foremost to his “*Ocherki po istorii statistiki*” [“Essays on the History of Statistics”] St. Petersburg, 1910), but also to works by V. I. Romanovsky, K. Pearson, L. Bortkiewicz, P. L. Chebyshev, S. N. Bershtein, and other scientists, who worked in the theory of probability and statistics.



## 5 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, 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 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 being imprisoned, Kondratiev continued to belong to the world 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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# Jacob Marschak 1898–1977: From a Russian Revolutionist to President-Elect of the American Economic Association



Robert W. Dimand and Harald Hagemann

## 1 An Active Menshevik in Revolutionary Russia

Jakob (since 1933 Jacob) Marschak was born in Kiev, Russia on 23 July 1898 as son of the jeweller Israel Marschak and his wife Sophie Khailowsky. He had a bar mitzvah and learned Hebrew. He also became fluent in German and French being taught by governesses.<sup>1</sup> Marschak grew up in a liberal and cosmopolitan family of assimilated Jews who believed in the tradition of the enlightenment. His parents sympathized with the first Russian revolution of 1905. As a child Marschak also experienced pogroms and discrimination due to the antisemitism in the Ukraine. The admission to the classical high school was denied, and Marschak attended the high school of commerce where he passed the final examination in 1915. Over the following summer Marschak prepared for the entrance examination to the Kiev Polytechnical Institute for which he had to write an essay on the topic “What we know is limited; what we do not know has no limits”. With an A plus he received for his essay Marschak was admitted within the quota for Jews (Marschak 1971, pp. 14–15).

At the Polytechnical Institute Eugen Slutsky (1880–1948) became Marschak’s teacher in statistics, just after Slutsky (1915) had published his pioneering article “On the theory of the budget of the consumer” which was rediscovered and became famous two decades later when Hicks and Allen published their work on value

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<sup>1</sup> For greater details see Marschak (1971) and Hagemann (1997).

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theory.<sup>2</sup> Slutsky showed that the effect of a price change can be divided into two effects: income effects and substitution effects (residual variability in Slutsky). The algebraic sum of these two independent effects gives the “Slutsky Equation” or fundamental formula of value theory which is devoid of any reference to measurable utility. Marschak points out in his *Recollections* that Slutsky who had been dismissed as a student from the University of Kiev in 1902 and came back from Munich in 1905 “was not a revolutionary and in fact under the Soviet regime abandoned economics just because he was not a Marxist and did not want to get mixed up in such things, and applied statistics to other things, like meteorology, or did purely mathematical work” (*ibid.*, p. 11).

Marschak, on the other hand, soon became a political activist and the speaker of a student group of Menshevik Internationalists. He was arrested by the Tsarist regime on 6 December 1916 and released in February 1917 due to an amnesty by the new Minister of Justice Kerensky. Marschak soon became Secretary of the Social Democratic committee for the city of Kiev and was elected to represent the Mensheviks in the Ukrainian parliament, the Rada. He cooperated with many activists who were on the staff of the “*Kievskaja Mysl*”,<sup>3</sup> a liberal newspaper similar to the *Manchester Guardian* or the *Frankfurter Zeitung* for which Marschak later worked in Germany from 1924 to 1926.

Five days after the Bolshevik revolution in St. Petersburg of October 25, a similar attempt in Kiev failed, and a week later the Rada proclaimed the constitution of a Ukrainian Republic. In these turbulent times Marschak’s parents decided to send their son, who suffered from lung problems since he was imprisoned, into the Northern Caucasus with its warmer climate for recovery. It turned out that this region with its great amount of historical conflicts, and rather isolated from the rest of Russia, became even more turbulent. Nevertheless, at the age of nineteen, Marschak was appointed Secretary of Labour of the short-lived Terek Republic from March to July 1918. He later called this government, which was led by a coalition of Mensheviks and social revolutionaries, a “paediocracy”, a government led by children (Koopmans 1978: X). Marschak’s main task was to prepare a social legislation for the small group of industrial workers, as in the oil industry around Grozny, according to the German role model (Marschak 1971, pp. 41–51). Conflicts with the Bolsheviks whose power was rising, and an escalating civil war induced Marschak to return to Kiev in October 1918. He took a route via Georgia where he reunited with his sister Frania (Frances), who had been engaged in dangerous revolutionary activities in the Northern Caucasus before, in Tiflis. There they stayed with the family of Wladimir Woytinsky (1885–1960), a close friend who as a member of the Bolshevik wing of the Social Democratic Labour Party had been arrested in 1905 and exiled to Siberia until 1917, but after the October Revolution joined the Georgian Mensheviks and became a representative of the Democratic Republic of Georgia from 1919 until its fall in March 1921. Woytinsky emigrated to Germany in 1922 where he became research director and

<sup>2</sup> On the history and interpretation of Slutsky’s 1915 article see Chipman and Lenfant (2002).

<sup>3</sup> Even Trotsky, under the pen name Antide Otto, contributed articles, among others his observations at the roulette table in the casino of Monte Carlo.

chief of the statistical department of the German trade unions in 1929 and the main architect of a famous employment program in 1932.

Pogroms by the nationalists in Kiev and the escalating civil war in Russia caused Marschak to emigrate, together with his elder brother, to Germany in January 1919. Other family members followed in fall after experiencing the conquest of Kiev by the Red Army in February and pogroms in the wake of the reconquest by Denikin's White Army in September.

## 2 The Formation of a young Economist in Weimar Germany 1919–1933

“When I got to Berlin there were the Spartacus fights. I was still interested and later sat through the three or four days of the Second Räte congress, which met about February” (Marschak 1971, p. 77). In contrast to the Soviet one, the German council surrendered its power to the constituent assembly of the young Weimar Republic.

Marschak was the first of a group of Menshevik economists who emigrated to Germany. He was succeeded by Woytinsky, Leontief and many others.<sup>4</sup> In the summer semester 1919 Marschak studied at the University of Berlin, mainly with Ladislaus von Bortkiewicz, before he moved to Heidelberg where Emil Lederer (1883–1939) became his main mentor. Schumpeter, who compares Lederer with Maurice Dobb in England, describes him “as the leading academic socialist of Germany in the 1920s”.<sup>5</sup> Lederer was one of the most active members of the socialization commissions which had been founded in Germany and Austria at the end of World War I. Eduard Heimann, the general secretary of the first and second socialization commission in Germany, and later a colleague of Lederer at the New School for Social Research in New York where Lederer became the founding Dean of the “University in Exile” in 1933, described Lederer in his obituary as “practically the leader of the socialization commissions in Germany and Austria during the years 1918–1920.”<sup>6</sup>

Cherrier (2010: 460) states that “Marschak’s approach to policy ... was heavily tied to the Marxist and socialist intellectual environment of Russia and Germany”. Whereas this certainly holds for some of his publications and activities, such as in the Socialist Association for Economic and Social Research of which he became a founding member in May 1929, it is also true that “Jacob Marschak was a warm and tireless member of the working parties seeking scientific truth. At age 79, as at age 30, Marschak worshipped the pursuit of science” (Samuelson 1988: 323).

Both sides come out very well in Marschak’s first scholarly publication “Economic calculation and the socialist commonwealth” (Marschak 1924a), which Kenneth

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<sup>4</sup> See Hagemann (2021: 74).

<sup>5</sup> Schumpeter (1954, p. 884 n. 10). For a detailed analysis of the relationship of Schumpeter and Lederer see Hagemann (2015).

<sup>6</sup> See Sect. 3 ‘Economics of Socialism’ in Marschak et al. (1941: 93–100, esp. 94).

Arrow (1979: 502) classifies as one of Marschak's "papers with the greatest permanent interest".<sup>7</sup> In his article Marschak critically inspects the argumentation of Mises (1920) whose essay shifted the calculation debate to the human reason side of socialism at a time when practical problems of implementation came to the fore. For Mises socialism implies the abolition of rational economic calculation as the consequence of the destruction of free markets. His core thesis therefore runs; "Where there is no free market, there is no pricing mechanism; without a pricing mechanism, there is no economic calculation" (Mises [1920] 1935; 111).

Marschak's critique of Mises is of an empirical as well as of a theoretical nature. He points out that an exact price formation would only be possible if the Edgeworth–Schumpeter conditions of free competition, such as an unlimited number of participants and freedom to (re-)contract, are fulfilled. With the rise of heavy industries, the unequal distribution of natural resources and the necessity of larger capital, these conditions, outside the stock exchange, were less and less fulfilled. "The empirical reality today lies somewhere in the middle between the one extreme—free competition with an infinite number of contractors—and the other extreme of an entirely monopolized economy" (Marschak 1924a: 510). This inexactness of price formation in modern capitalism mainly results from increasing monopolization.

In his theoretical analysis Marschak states that the advantages of monopolization exist precisely in those two areas which are particularly affected by Mises's scepticism: in the economic calculation for goods of higher order and in the sphere of dynamics. For Mises it was essential that with private ownership of the means of production the producer implements an allocation of the goods of higher order in which they earn the highest possible return. Marschak, on the other hand, pointed out that even in the case of complete vertical integration the imputation problem could be solved and no difficulties in the determination of prices and quantities would arise.<sup>8</sup> For Marschak the decisive problem consists in the flexibility of the economy to absorb new information and to adjust to structural change. He distinguishes between two cases of dynamics: changes which originate from outside, for example in foreign trade, and those originating from inside the economy as endogenous technological change. Marschak's early analysis links up with his later interest in the economics of information systems. Marschak and Radner's *Economic Theory of Teams* (1972) provides a powerful tool for the analysis of the relative informational efficiencies of decentralized price expectations, emphasizing the importance of communication and its limits in the transmission of information.

Marschak argues strongly against a non-democratic centralized socialism which runs into insurmountable difficulties as soon as the condition of homogeneity of value scales is not fulfilled. This homogeneity does exist for basic commodities to a greater degree but not beyond them, which implies that the market cannot be substituted by an ideal general will. Marschak's democratic socialism links up well both with his earlier political activities as a Menshevik and with the scholarly and political views

<sup>7</sup> For a more detailed analysis of Marschak's essay see Hagemann (2019).

<sup>8</sup> See Marschak (1924a: 506–507). Marschak implicitly assumes the Austrian case dating back to Böhm-Bawerk in which only working capital, i.e. intermediate means of production exist.

of his mentor Lederer. Intellectually Lederer was a genuine liberal. He was managing editor (with Joseph Schumpeter and Alfred Weber as the two associate editors) of the *Archiv für Sozialwissenschaft und Sozialpolitik* which had been the leading journal in social sciences in the German language area ever since Werner Sombart, Max Weber and Edgar Jaffé had been appointed editors in 1904 until its termination in 1933. Although many papers in the *Archiv* were written by authors who were critics of the capitalist system, such as Karl Polanyi or Herbert Marcuse, Lederer's intellectual liberalism towards scholars with diverging *Weltanschauungen* is reflected in the fact that Ludwig von Mises published no less than ten articles between 1913 and 1929.

Nikolay Kondratieff's "The long waves of economic life" (1926), which had a deep influence on Schumpeter, and Wassily Leontief's Berlin dissertation "The Economy as a circular flow" (1928) were also published in the *Archiv*. The fact that Robert Michels, a former socialist who converted to Italian fascism, published several articles in the *Archiv* until 1932 is another sign of Lederer's liberal editorial policy. Many of the essays by leading contemporary economists also demonstrate that they had a much broader view of economics being integrated in the social sciences than all those modern mainstream economists who favour abstract mathematical models, often based on irrelevant assumptions, or narrow micro-econometric studies. A good case in point is Marschak's two-part study "The corporative and the hierarchical idea in Fascism" (Marschak 1924c). Marschak, who had received an early training in Italian during his childhood in Kiev, wrote the two long essays in the wake of a longer visit to Italy. It is one of the first well-informed socioeconomic analyses of Italian fascism which also reveals the comprehensive interdisciplinary education Marschak had received in Heidelberg.<sup>9</sup> His most remarkable early and deep analysis revealed the character and despotic opportunism of fascism.

One of the central topics in Lederer's scientific lifework was the rapid growth of the salaried employees since the 1890s which had changed the structure of modern capitalist societies considerably. Lederer did pioneering work on the diverging attitudes and political consciousness of white-collar workers whose absolute numbers and relative shares compared to manual or blue-collar workers increased in the phase of high capitalism. In two joint articles with his teacher Lederer (1926, 1927) Marschak analyses the separation of the employees into the two groups of workers and salaried employees and its socio-political implications. Their analysis is characterized by the confrontation of existing theories, particularly the Marxian theory of classes with her prediction of a growing polarization process which makes white-collar workers members of the proletariat, and the strong belief in the evolution of society through enlightenment which later was shaken to a certain degree by the successes of the Nazis. This twofold intention comes out most clearly in the analysis of the role of the new middle classes. Whereas the few members of the "old" middle classes had widely been regarded as members of the bourgeoisie, the exact position of the increasing number of persons who belonged to the "new" middle classes became an

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<sup>9</sup> Marschak's brother-in-law Dmitri Tschizewsky (1894–1977), who had married his sister Lydia, studied philosophy in Heidelberg from 1921–1924, where after coming back from Harvard, he became Professor of Slavistics in 1956.



issue of heavy controversies. Lederer and Marschak made a major attempt to come to a differentiated view by a thorough study of the complexities of the social position of this group which not only considered the mere economic facts but also the social self-interpretation of this group which in modern language is called “collective mentalities”.

Lederer and Marschak started with the confrontation of the optimistic or harmonic view, which sees the new middle classes playing the role of a mediator between capital and labour whose function it is to reduce the fragility of the social system and to act as a lubricant for the unity of the social classes with the opposite Marxian view which recognizes an aggravation of the class struggle in capitalism as necessary and inevitable and consequently perceives the new middle class as “*Stehkragenproletariat*” (stand-up collar proletariat).

Lederer and Marschak shared the belief that the new middle classes were critical for a greater stability of the democracy, followed the political purpose to attract the salaried employees to the trade union movement and to progressive political action. This was based on an analysis which came to the final conclusion “that the idea of social harmony is a literary idea” which was not realized because the social stratification of the population into classes had considerably progressed compared to pre-war times.

After his period as a member of the editorial staff at the leading liberal newspaper *Frankfurter Zeitung* Marschak worked from 1926 to 1928 at the Research Centre for Economic Policy in Berlin, which was financed by the German trade unions and the parliamentary group of the Social Democrats in the *Reichstag*, and thereafter as a Senior Researcher at the Kiel Institute of World Economics. In this phase distributional questions became a core theme, as reflected in his inaugural lecture “On the policy and theory of distribution” (Marschak 1930c) at the University of Heidelberg on 22 February 1930 and extending into his British period.<sup>10</sup>

The wage-employment debate intensified dramatically after the outbreak of the Great Depression. Marschak joined forces with Lederer and Adolph Löwe, Gerhard Colm and Hans Neisser at Kiel. They took a very active and leading role against austerity and wage-cut policies as they were favoured by Chancellor Brüning and the great majority of economists in the years 1930–1932. Despite some internal differences they all argued strongly against a deflationary wage policy as the central element of economic policy in the Great Depression.<sup>11</sup> The common position can be summarized in the famous statement by Lederer: “The primitive conception that, whenever unemployment exists, one could always restore equilibrium by a reduction in wages belongs into the junk-room of theory” (Lederer 1931, p. 32). Like Keynes they were worried by the threat of an aggravation of the deflationary process. The simple neoclassical causal nexus that wage cuts imply higher profits and are thereby a condition for an increase in investment and employment becomes fragile at a decisive period. Precisely in a depression with its low degree of capacity utilization and high

<sup>10</sup> See his contribution on wages to the *Encyclopaedia of the Social Sciences* (Marschak 1935).

<sup>11</sup> For greater details see Garvy (1975) and Hagemann (1999).

debts with the banks entrepreneurs would be stimulated to use lower wage costs for the repayment of debts instead of the financing of new investment activities.

Although they were all active social democrats and close to the trade unions, they explicitly did not follow the exaggerations of a simplified purchasing power theory. Marschak (1930a, b) came closest with his view of a differentiated pro. Wage reductions (increases) primarily implied a reallocation of purchasing power from workers to entrepreneurs (and vice versa) which is associated with a change in the structural composition of production. He considered it important that a reallocation of purchasing power in favour of workers associated with higher wages would stimulate production since the consumption goods demanded by workers are subjected to the law of mass production, i.e. economies of scale, to a much higher degree than luxury goods consumed by capitalists. This “law of strongly decreasing costs with expanding production” (Marschak 1930b, p. 26) has two components which must be distinguished: a *short-run* cost degression due to a higher degree of capacity utilization, and a *long-run* cost degression resulting from the fact that many technical and organizational improvements will only become profitable and realized with the exceeding of certain sales thresholds. Marschak considers this long-run cost degression as decisive, since the short-run cost degression resulting from a higher utilization of existing capacity is limited to a reduction of fixed costs per unit of output. Due to additional costs associated with increasing production it would not suffice to compensate for the higher wages. Marschak’s understanding of unexploited reserves and savings opportunities is of a genuinely dynamic nature comprising also the new methods of production which lie fallow because of sales shortage. In his argument the scale component and the progress component are inseparably interlinked.

In December 1931 Marschak (1931) published sixteen theses on crisis policy in which he recommended “productive credit creation” and made a strong plea for internationally coordinated employment programs. In retrospect he attributed the rise of National Socialism in Germany, and the success of radical political parties in other countries, to the failure of the existing democracies to solve the problem of mass unemployment. “No peace will be a lasting one with the economic problem unsolved” (Marschak 1940: 283). Unilateral policies of austerity or devaluation are doomed to fail. In contrast, internationally coordinated public works programs between the major countries eliminating fears of balance of payments problems are a precondition for overcoming the depression and thereby a major contribution to peace.

Speaking in Axel and Earlene Leijonhufvud’s oral history project on emigré scholars, Marschak recalled that in or around 1928, Leo Szilard organized an informal seminar in Berlin on applications of mathematics, with Marschak talking about the use of mathematics in economics, presenting Gustav Cassel’s version of the equations for Walrasian general equilibrium. “One of the mathematicians became extremely agitated and began a stream of interruptions, arguing that the equilibrium relationship should be described by inequalities instead of equations. The mathematician was [John] von Neumann” (Weintraub 1985, p. 74n). Von Neumann’s reaction to Marschak’s talk led to papers by von Neumann ([1937] 1945) and by Abraham Wald in Karl Menger’s Vienna mathematics colloquium (translated

in Baumol and Goldfield 1968), pioneering fixed-point methods for proving existence of equilibrium in place of the claim by Walras and Cassel that equality of numbers of unknowns and equations sufficed to guarantee existence of a meaningful equilibrium. Marschak's talk was presumably an early version of the paper eventually published in translation after his second emigration as "Econometric Parameters in a Stationary Society with Monetary Circulation" (1934), an important step both towards Marschak's work on monetary theory in the later 1930s and towards his promotion of the Cowles Commission program of estimation of simultaneous-equations macroeconomic models.<sup>12</sup>

In February 1930 Marschak got his habilitation from the University of Heidelberg with a thesis on *Elasticity of Demand* (Marschak 1931) which he had written during his time at Kiel where Wassily Leontief was a close colleague whose work focused on a statistical supply and demand analysis in this period.<sup>13</sup> Since the late 1920s Marschak had been engaged in the growing econometric movement. When Schumpeter wrote a letter to Wesley C. Mitchell on 19 April 1933 asking for support of Hebrew colleagues in Germany, he classified Marschak as "[p]robably the most gifted scientific economist of the exact quantitative type now in Germany. Wrote an excellent book on the Elasticity of Demand and about half a dozen excellent papers on various subjects in Econometrics" (Schumpeter 2000, p. 247). For three years Marschak had been Privatdozent and a very successful teacher at the University of Heidelberg<sup>14</sup> until he was dismissed for his "non-Aryan descent" on 20 April 1933. He was among 7 of 11 faculty members, the highest percentage of any German faculty of economics and the social sciences followed by the Goethe University in Frankfurt, who were dismissed for racial and/or political reasons on the basis of the "Restoration of Civil Service Act", which the Nazis had launched shortly before on 7 April.

### 3 At Oxford from 1933 to 1938

The Nazis' rise to power caused Marschak's second emigration in Spring 1933. Marschak had already left Germany to Vienna, where his sister Frances was living,

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<sup>12</sup> Kenneth Arrow (in Arrow et al. 1991, pp. 10–11) wondered how von Neumann thought of alternative activities for producing the same goods and multiple outputs from activities when the only economist cited in von Neumann ([1937] 1945) was Gustav Cassel, who had only fixed coefficients for production and no circular flow: "Perhaps the activity analysis formulation of production was simply obvious to a genius like von Neumann." But Marschak (1934) went far beyond Cassel's system of equations in that respect. To the extent that those aspects of Marschak (1934) were already present in his 1928 talk, he may have influenced von Neumann, while Marschak (1934) may have been influenced by von Neumann's remarks in the 1928 seminar.

<sup>13</sup> For further details see Sect. 5 of Hagemann (2021).

<sup>14</sup> Musgrave (1997, p. 64) recalls: "Serious study of economics began with my transfer to Heidelberg in the Fall of 1931. Marschak, then a young Privatdozent offered a seminar on Keynes' *Treatise [on Money]* and on integrating fiscal flows into the national income accounts."

in March. His family followed in April.<sup>15</sup> After short academic stays in Spain and in the Netherlands, Marschak was appointed Chichele Lecturer in Economics at All Souls, University of Oxford that autumn. Two years later he was promoted Reader in Statistics. In 1935 he also became the founding director of the Oxford Institute of Statistics (OIS) which got substantial funds from the Rockefeller Foundation (RF) which had already financed some of Marschak's earlier research projects in Germany. Under Marschak's directorship the OIS soon got a high reputation in theory-guided empirical research.<sup>16</sup>

The OIS also hosted the famous meeting of the Econometric Society in September 1936 with the symposium on Keynes's *General Theory* where Harrod, Meade and Hicks presented their interpretations. Their contributions marked the beginning of the IS-LM model in macroeconomics and the birth of the neoclassical synthesis, with Hicks's paper marking the beginning of the IS-LM diagram. At Oxford Marschak also played some role in Roy Harrod giving birth to post-Keynesian growth economics.<sup>17</sup>

While at Oxford, Marschak, both on his own (Marschak 1938) and in collaboration with Helen Makower (Makower and Marschak 1938), examined the role of money in a theory of asset market equilibrium, using the term "safety preferences" for what later came to be known as risk aversion. The mathematical formalism of Marschak (1938), following from Marschak (1934), contrasted with the verbal, intuitive discussion of money and assets in a world of risk and uncertainty given by Hicks (1935), but Hicks had spoken on a more formal mathematical treatment of the problem in an unpublished paper briefly summarized in Marschak's report on the 1933 Leyden meeting of the Econometric Society (see Dimand and Hagemann 2020). Marschak's monetary theorizing at Oxford provided a bridge from his doctoral dissertation on the equation of exchange to postwar work on monetary economics at the Cowles Commission and Foundation by Marschak and others. Mehrling (2002, p. 182 n12) finds "a straight line from Marschak's 1938 'Money and the Theory of Assets' to his 1949 'Role of Liquidity under Complete and Incomplete Information' [1949b] and 1950 'The Rationale of the Demand for Money and of "Money Illusion"' [1950b]. Important works that follow Marschak's preferred line include Harry Markowitz's 'Portfolio Selection' (1952) and James Tobin's 'Liquidity Preference as Behavior Towards Risk' (1958)", influential works written at the Cowles Commission in Chicago by Marschak's doctoral student Markowitz and at its successor, the Cowles Foundation at Yale, by Marschak's colleague Tobin (who was also influenced by Hicks 1935).

Marschak's career at Oxford ended in December 1938 when he embarked for the USA with a one-year fellowship by the Rockefeller Foundation. He stayed in

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<sup>15</sup> Marschak had married Marianne Berta Kamnitzer (\*1901 Berlin, †1993), a journalist and later psychologist, 1927 in Heidelberg. The two children were born in Heidelberg: Ann (Angela) Jernberg, a well-known psychotherapist (1928–1994), and Thomas in 1930. Thomas Marschak is Professor Emeritus of the Haas School of Business and of the Graduate School at the University of California in Berkeley.

<sup>16</sup> In the late 1930s and early 1940s émigré economists from Central Europe dominated the research work at the OIS. See Hagemann (2007: 335–340) and Lampa (2020).

<sup>17</sup> For greater details see Young (1989), Young and Lee (1993), Besomi (1989) and Sember (2010).

the USA after the outbreak of the Second World War in the following September and finally became an American citizen in 1945. His academic career therefore extended over fifty-eight years and across three countries: Weimar Germany, the UK and the USA. However, his life-long research interest in monetary macroeconomics originated already during the revolutionary turbulences in his country of birth Russia where he had made his experiences with inflationary processes, the creation of local currencies and so-called “white money”.<sup>18</sup> Thus in his first scholarly work *The Equation of Exchange* (Die Verkehrsgleichung), written in the middle of the German hyperinflation after WWI, for which he received the doctorate from the University of Heidelberg *summa cum laude* on 10 November 1922, Marschak (1924b) investigated the conditions for the equation of exchange to transcend a mere tautology into a causal relationship between the quantity of money and the price level. In his dissertation, which got inspiration from Irving Fisher’s *The Purchasing Power of Money* (1911) and Schumpeter’s essay “Money and the Social Product” (1917–18), Marschak’s main contribution is the elaboration of the “concept of relative elasticity” among the variables in the equation of exchange. His last German publication “National Wealth and the Demand for Cash” (Marschak 1932–33), which was inspired by new contributions of Marius Holtrop, Friedrich A. Hayek and particularly Keynes’s *Treatise on Money*, interestingly was listed as his first publication in the official information leaflet of the American Economic Association when Marschak became candidate for office as president-elect in 1977.

#### 4 Marschak in the USA

While at Oxford, Marschak travelled to Colorado to present papers at the 1937 and 1939 summer research conferences of the Cowles Commission for Research in Economics (Dimand 2021). Affiliated with the Econometric Society and dedicated to the promotion of formal statistical and mathematical methods in economics, the Cowles Commission had been founded in 1932 by newspaper heir Alfred Cowles 3rd. Disillusioned with stock market forecasts in the wake of the 1929 Wall Street crash, Cowles was interested in showing statistically that stock forecasters were no better than naïve models (such as random portfolios) in predicting stock price movements (see Cowles 1933). This interest led Cowles to the then newly established Econometric Society, a small organization which had assets of less than twenty-two dollars when Cowles became its treasurer and benefactor, and the business manager of the society’s journal *Econometrica*, founded thanks to his guarantee to cover its deficits. Marschak’s two papers at the 1937 conference so impressed Alfred Cowles that he tried to bring Marschak to Colorado as research director of the Cowles Commission. However, on 19 April 1938, Cowles cabled to Marschak that “Our failure to secure Rockefeller grant makes it impossible at present to offer salary mentioned in your letter of April third, letter follows” (quoted in Cowles letter to Marschak,

<sup>18</sup> See Marschak (1971, pp. 31–32 and 48–49).

20 April 1938, Marschak Papers, UCLA, quoted by Dimand and Hagemann 2020, p. 907). Marschak returned to Colorado Springs to present two papers at the 1939 Cowles summer conference. Marschak's appointment to direct the Cowles Commission, which began on the first day of 1943, had thus been in Alfred Cowles's mind even before the Cowles Commission moved from Colorado Springs to the University of Chicago in 1939.

In September 1939 Marschak was named a professor of economics at the New School for Social Research in New York, where Emil Lederer had been the founding dean of the University in Exile in 1933 (see Krohn 1993). Marschak succeeded Gerhard Colm, his colleague in Kiel from 1928 to 1930, who moved from the New School to the Roosevelt administration in Washington. Marschak contributed to the New School's journal *Social Research* on peace economics (stressing the need for an international business cycle policy as necessary for the preservation of peace), the role for government in economic stabilization, the effect of lack of confidence (animal spirits) on investment, and Wicksell's natural and market interest rates (the last two are reprinted in Marschak 1974, Volume II), as well as a memorial article about Lederer, who died in 1939 (Marschak, Kähler and Heimann 1941). Beatrice Cherrier (2010, p. 447 n7) reports a letter by Marschak, probably written in 1940, attributing Hitler's election to the German government's failure to cure unemployment and to its deflationary policy.<sup>19</sup> Such concerns motivated Marschak's increasing involvement with Keynesian macroeconomics and with simultaneous-equations structural econometric models to guide Keynesian stabilization policy, an involvement that did not reflect Keynes's own reservations about econometric modelling.<sup>20</sup>

Together with Oskar Lange (then visiting Columbia University on leave from the University of Chicago), Marschak organized a series of econometrics seminars sponsored by the National Bureau of Economic Research and attended by Trygve Haavelmo, Tjalling Koopmans (later Marschak's colleague and successor at the Cowles Commission), Marschak's New School doctoral student Franco Modigliani and the Columbia University statistician Abraham Wald (Modigliani 2001, p. 19). Marschak and Lange ([1940] 1995) collaborated on a contribution to the Keynes-Tinbergen debate, defending Tinbergen's use of structural econometric models to test business cycle theories (Tinbergen 1939), but Keynes rejected their submission to the *Economic Journal*.

Lange played a key role in bringing the Cowles Commission from Colorado to the University of Chicago in 1939 to fill the gap left by the death of the university's lone econometrician, Henry Schultz. Together with Cowles Commission research director Theodore Yntema, Lange edited *Studies in Mathematical Economics and Econometrics: In Memory of Henry Schultz* (Lange et al. 1942), to which Marschak contributed on "Economic Interdependence and Statistical Analysis." As Yntema's

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<sup>19</sup> Marschak's opinion is supported by recent findings: see "Hard lessons: New research on the 1930s and 1940s uncovers the links between hardship and political ruptures" (*The Economist*, January 23, 2021, p. 61).

<sup>20</sup> Keynes's critical reception of Tinbergen (1939) did not deter him from serving as president of the Econometric Society for 1944 and 1945 or from welcoming as his successors Jacob Marschak for 1946 and Jan Tinbergen for 1947.

interests shifted from the quantitative focus of the Cowles Commission to the public policy advocacy of the Committee for Economic Development, Lange's support combined with Alfred Cowles's previous interest in Marschak, resulted in bringing Marschak to Chicago on the first day of 1943 as research director of the Cowles Commission, with a professorship in economics at the University of Chicago. Koopmans joined Marschak in 1944 as a research associate at the Cowles Commission, combined from 1946 with an associate professorship in the university.

Under the directorship of Marschak from 1943 to 1948 and then of Koopmans from 1948 to 1955, the Cowles Commission became the world's leading centre of mathematical economics and econometrics (apart from game theory, dominated by Princeton's mathematicians), with contributions on the identification and estimation of simultaneous-equations models (Haavelmo 1944, Koopmans 1950, Hood and Koopmans 1953) and by Kenneth Arrow, Gerard Debreu and Lionel McKenzie on general equilibrium analysis, Arrow on social choice, Harry Markowitz on portfolio diversification, Koopmans on activity analysis, Lawrence Klein on empirical macroeconomic modelling and Don Patinkin on monetary theory (Markowitz 1959; Hildreth 1986; Klein 1991; Christ 1994, Dimand forthcoming). Many of the Cowles Commission monographs reported work later recognized by the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel. During the Colorado years of the Cowles Commission in the 1930s, the intellectual background for the work of Cowles researchers such as Charles F. Roos and Harold Thayer Davis had been periodogram analysis of economic fluctuations as multiple superimposed cycles, with limited reliance on formal economic theory (Dimand and Veloce 2007). Together with Haavelmo and Wald (an active Cowles consultant after his move from the Cowles Commission in Colorado Springs to Columbia University), Marschak and Koopmans reinvigorated the research program of the Cowles Commission, repositioning it as the continuation of the pre-war European econometrics and mathematical economic theorizing of Marschak's teacher Eugen Slutsky, Koopmans's teacher Jan Tinbergen, Haavelmo's teacher Ragnar Frisch and Karl Menger's Vienna colloquium, of which Wald had been a leading participant (and with Karl Menger participating in Cowles seminars after he joined the Illinois Institute of Technology).

Oskar Lange helped bring the Cowles Commission to the University of Chicago and then Jacob Marschak to the Cowles Commission but, after publishing a Cowles monograph on *Price Flexibility and Employment* (1944), he left academia, amidst a storm of controversy, to serve the Soviet-backed Polish government as envoy to the USA and the United Nations. In the socialist calculation debate of the 1930s, Lange (1938) had urged Marxian economists to embrace Walrasian general equilibrium, Keynesian macroeconomics and simultaneous-equations econometric models as tools for managing a socialist economy. Lange's position strengthened suspicions of mathematical economics and the Cowles Commission's research on the part of Milton Friedman, who had written a sharply critical review article on Lange's Cowles monograph (reprinted in Friedman 1953) and who returned to his *alma*

*mater* Chicago as an associate professor in 1946<sup>21</sup>: “I believe that mathematicians, whether pure mathematicians or economists or statisticians, tend to be favorable to central planning ... When they enter a field like economics, they carry over the belief that all problems have clear-cut solutions and that they are competent to find them” (Friedman and Friedman 1998, p. 262).<sup>22</sup> As a social democrat, Marschak did not share Lange’s Marxism, but he and other Cowles Commission researchers such as Lawrence Klein saw the construction of structural econometric models as motivated by a need to guide Keynesian demand management policy (see Marschak’s chapter on “Economic Measurements for Policy and Prediction” in Hood and Koopmans, eds., 1953).

Marschak gave a twenty-lecture course on *Income, Employment, and the Price Level* in 1948, the notes from which, together with three supplementary lectures the next year, were published by his students David Fand and Harry Markowitz (Marschak 1951). With a pioneering presentation of aggregate demand and supply in price level and real income space and a short-side rule for the labour market, Marschak (1951) was the first graduate-level macroeconomics textbook, as distinct from works on the theory of money that focussed on the price level in contrast to Marschak’s Keynesian focus on the determination of aggregate income and employment. In addition to his published lectures and his articles on money demand (1949b, 1950b), Marschak also contributed to advancing Keynesian macroeconomics as a system of simultaneous equations through his supervision of doctoral dissertations by Franco Modigliani (1944) at the New School and Don Patinkin (1948a, b, 1949) at Chicago (see Hagemann 2017 on Modigliani’s thesis, Rubin 2012 on Patinkin’s thesis, and Klein 1947, 1991, Dimand 2020a on Keynesianism at the Cowles Commission). This representation of Keynesian macroeconomics as a general equilibrium system of equations suitable for econometric estimation and for policy guidance, with real effects of demand management resulting from inflexible wage rates or from money illusion, contrasted both with interpretations of Keynes that emphasized fundamental uncertainty and with the revival of the quantity theory of money in which Milton Friedman and his students were engaged at Chicago in the same years (Friedman ed., 1956).

Where Tinbergen (1939) had estimated a multi-equation model of the USA by single-equation ordinary least squares, at a time when a computer was a person rather than an electronic device, Trygve Haavelmo, Tjalling Koopmans, Leonid Hurwicz, T. W. Anderson, Abraham Wald, Herbert Simon and others at the Cowles Commission

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<sup>21</sup> Marschak had urged the Economics Department to offer positions to both Friedman and Paul Samuelson, invoking the example of stimulating debates around Keynes and Pigou at King’s College, Cambridge, but Samuelson declined when he received an offer from Chicago the year after Friedman was hired (Mitch 2016).

<sup>22</sup> Friedman, an acerbic critic of the Cowles Commission (“I developed a reputation as something of a hair shirt since I was, and am, a persistent critic of the approach to the analysis of economic data that became known as the Cowles approach”), nonetheless recalled that “Marschak was a warm, outgoing human being ... a truly learned person who had wide interests and contributed to different areas of economics”—but he made these observations in support of the statement that “Marschak and Koopmans had very different personalities” (Friedman and Friedman 1998, pp. 197–198).



pioneered the use of full-information and limited-information maximum likelihood (FIML and LIML) to estimate the equations of such a model as a system, avoiding simultaneity bias, and derived the rank and order conditions for identification of the equations (Haavelmo 1944, Koopmans, ed., 1950, Hood and Koopmans, eds., 1953, Hildreth 1986, Epstein 1987, Klein 1991, Malinvaud in Arrow et al. 1991, Christ 1994, Bjerkholt 2015, Dimand 2020b)<sup>23</sup>. Klein (1950) provided an empirical implementation of this approach. Edmond Malinvaud (in Arrow et al. 1991, p. 57) described the January 27 to February 1, 1945, Cowles Commission conference that was the basis for Koopmans (ed., 1950) as “what turned out to be the most influential conference on statistical inference in economics ever held.” Marschak took the lead in providing a systematic overview of these new developments in econometric modelling and making them accessible to students, giving a twenty-lecture course at the University of Chicago on introduction to econometrics (Marschak 1949a) that was based on<sup>24</sup> his long chapter “Statistical Inference in Economics: An Introduction” in Koopmans (ed., 1950, pp. 1–50). Although Frisch and Tinbergen had given pre-war courses in econometrics, and Tinbergen had published a textbook for such a course, their courses had not been about system-wide methods of dealing with simultaneous equations nor the focus on probability theory introduced to econometrics in Haavelmo’s dissertation. Marschak pioneered in offering a course on the identification and estimation of simultaneous-equations structural macroeconomic models and on their use in guiding stabilization policy.

Marschak’s work on the demand for money had emphasized the completeness or incompleteness of information, and decisions to hold money and other assets in a world subject to randomness. From the early 1950s he was increasingly concerned with stochastic decision theory, so that his selected essays, reprinting fifty-five articles published in English (apart from one in French) after his second emigration, were entitled *Economic Information, Decision, and Prediction* (1974). This focus on information, communication and organization led him to interdisciplinary collaboration, and increasingly to identification with the emerging field that called itself behavioural science, moving beyond the disciplinary boundaries of economics to explore behavioural and experimental approaches without losing interest in the neoclassical rationality that characterized Arrow–Debreu–McKenzie general equilibrium or Koopmans’s activity analysis (see Cherrier 2010). Marschak had, already during World War II, been receptive enough to behavioural economics to bring the psychologist and economist George Katona, who had taught a course at the New School on the psychology of inflation while Marschak was there, to the Cowles Commission to write a Cowles monograph on wartime price control and business, and Herbert Simon had begun publishing on behavioural theories of choice while

<sup>23</sup> They failed to persuade Tinbergen that simultaneous-equations estimation was an improvement over OLS, because Tinbergen argued that in the Cowles Commission approach misspecification of one equation would affect the estimated coefficients of the other equations.

<sup>24</sup> Malinvaud (in Arrow et al. 1991, p. 57) reports that “the manuscript [of Koopmans, ed., 1950] was completed in early 1947, but publication was delayed until 1950 by typographical and other printing difficulties” so Marschak’s introductory chapter (Marschak 1950a) was written before he gave the course of lectures.

affiliated with the Cowles Commission. Beginning with Cowles Commission Discussion Papers co-authored with Roy Radner in 1951 and 1954 and with an article in the inaugural issue of *Management Science* in 1955 (Marschak 1974, Vol II, Chap. 21), Marschak moved from monetary economics and econometric method to a new research interest on firms as teams, that is, as organizations whose members have the same interests but not the same information. This research program culminated after two decades in a Cowles Foundation monograph on *The Economic Theory of Teams* (Marschak and Radner 1972).

Friction between the Cowles Commission and the emerging Chicago school of economics in the late 1940s and early 1950s concerned the role of mathematics in economics, the usefulness of simultaneous-equations macroeconomic models such as those in Klein (1950) and Walrasian general equilibrium analysis as opposed to the Marshallian price theory upheld by Friedman (see Dimand forthcoming, also regarding the rest of this paragraph). Koopmans's critique of the business cycle analysis of Arthur Burns and Wesley Mitchell (Friedman's teachers at Columbia) as "measurement without theory" and Friedman's defence of Mitchell as an economic theorist was a point of conflict. Marschak endorsed mathematical economics, structural econometric modelling and general equilibrium, and wrote a comment on Mitchell's posthumously published final book on business cycles but generally played a supporting rather than leading role on the Cowles Commission side of these disagreements. As is common in academic departments, the crucial disputes within the Economics Department involved hiring. When Koopmans was to take a year's sabbatical in 1954–55, he, Marschak and Alfred Cowles looked for a new research director to succeed him. When James Tobin came from Yale to consider the offer, he asked Theodore Schultz, the department chair, whether the department would have been interested in hiring him apart from the Cowles Commission's offer and was told that it would not. He then decided to stay at Yale, whereupon Koopmans asked whether he could spend his sabbatical year at Yale. During that year, Koopmans, Marschak and Cowles negotiated the movement of the Cowles Commission from Chicago in 1955 to become the Cowles Foundation at Yale ("Poor Yale" gloated Friedman in a letter to Burns).

Marschak, like Koopmans, became a full professor of economics at Yale. But while Koopmans remained at Yale, Marschak moved in 1960 to the Graduate School of Management at the University of California at Los Angeles in 1960 as a professor of economics and operations research and as director of the Western Management Science Institute. Yale reminiscences posted on Irwin Collier's website "Economics in the Rear-view Mirror" reveal that disputes between the Cowles Foundation and the Economics Department over control of hiring were a factor, particularly over delays in offering tenure to Gerard Debreu and over which positions would be paid out of the Cowles Foundation's endowment rather than the department's general budget. At UCLA Marschak continued to be a productive scholar on information economics, behavioural science and the economic theory of organizations as teams, writing with statisticians, mathematicians, management scientists and psychologists rather than with economists (see Marschak 1974, Volumes I and II). In 1976 a nominating committee chaired by Kenneth Arrow nominated Marschak for the presidency of the

American Economic Association but Marschak passed away suddenly in 1977 while still president-elect. His long-time friend and colleague Koopmans served as AEA president in Marschak's place.

## 5 Conclusion

Despite being caught up in political upheavals at an early age (see Marschak 1971) and such disruptions as two emigrations, Jacob Marschak achieved a long, productive and distinctive scholarly career. He contributed to monetary and financial economics and macroeconomics, to econometric modelling, to the adoption of formal mathematical methods in economics, to decision theory and the economics of information and organizations, through his own extensive writings, through his supervision of the doctoral dissertations of Markowitz, Modigliani and Patinkin, through his innovative teaching of courses in econometrics and graduate macroeconomic theory, and through his leadership in the Cowles Commission and the Oxford Institute of Statistics (see Arrow 1978, 1979, 1991). His career took him from a Tsarist prison and then being a teenaged cabinet minister in a short-lived republic in the Caucasus mountains to being president-elect of the American Economic Association. Econometrics courses, maximum likelihood estimation of simultaneous-equations models, proofs of existence of general equilibrium, social choice, linear programming and optimal portfolio diversification went from being unusual interests of the Cowles Commission led by Marschak and Koopmans to being the common property of the discipline of economics, with Marschak's focus in his later years on behavioural science and laboratory experiments being taken more recently by behavioural economics and finance.

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# Simon Kuznets and Russia: An Uneasy Relationship



Moshe Syrquin

Simon Kuznets, Nobel Prize in economics for 1971, emigrated to the US in 1922. Some credit him with having arrived a “sophisticated and well-trained economist ... with practical statistical experience” (Maddison 2004). What was his training? How much did it influence his work in the US and to what extent can we say that his career in turn influenced back Soviet and Russian economics after 1990?

## 1 From Pinsk to Columbia

Direct evidence about his early life and studies is scant. He did not write memoirs or record interviews with recollections other than his remarks at the celebration of his 80th birthday which Fogel rescued and published posthumously (Kuznets 1989). A real biography of Kuznets remains to be written. There are several useful essays that focus mainly on his later achievements and methods but have virtually nothing on his early life, studies, and heritage.<sup>1</sup> Arguably, one of the most influential and consequential economists of the twentieth century yet almost absent from the literature other than as the prefix to the Kuznets-curve and an occasional reference to long waves. There is no repository of his papers. Scattered bits can be found at Harvard, the Rockefeller Foundation, and probably at the NBER but not digitalized and not easily available.

There is no intellectual or personal biography, and as nature abhors a vacuum, many have hazarded guesses which often cannot really be defended. Mistakes also

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<sup>1</sup> See in particular Kapuria-Foreman and Perlman (1995), Easterlin (1979), Lundberg (1971), and Fogel (2001).

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abound. The little we had was substantially increased around 2010 by two serendipitous events: Glen Weyl turned an undergraduate term paper into a major work based on archival material and interviews with Kuznets' children (2012), and Kharkov University decided to reclaim Kuznets as her own and name the school of economics after him. For that occasion, a group of scholars interviewed family members, unearthed documents, and wrote various pieces for now available only in Russian.<sup>2</sup>

Much of his young life experience and schooling are still vague even today. His birthplace was listed as Kharkov [Kharkiv in Ukrainian] by himself in official documents, but after his death, his widow told Abramovitz that it was actually Pinsk, then part of the Russian Empire, later Poland, today Belarus.<sup>3</sup>

A brief summary of what we know that may have some bearing on the topic at hand. Kuznets was born in Pinsk, then part of the Russian Empire in 1901 into a Jewish household. In 1909 (or 1907), the family moved south to Rovno where they lived with his maternal grandparents. There he was raised in a combination of Russian from his mother and aunt and Yiddish from his grandparents (Weyl 2012). Following the start of the war in 1915, the tsarist government expelled the Jews from Rovno which was close to the front and the government feared the Jews might collaborate with enemy forces. They were given 24 h to leave, and all the family moved to Kharkov aboard trains that were picking up refugees across the Ukraine going east. In 1910, the father who had been working in a bank left for the US where with the help of his wife's parents he went into the fur business (Judith Stein, personal communication, June 27, 2021). In Kharkov, the family with other refugee families lived in the building of a circus theater (Moskovkin 2011a). Simon and his brother Solomon studied in different schools because of the quota for Jews; Kuznets finished the two-year program of the 2nd scientific gymnasium in May 1917 and in May 1918 an additional higher course that allowed him to enter a university.

## 1.1 *University—Commercial Institute*

Kuznets enrolled at the Kharkov Commercial Institute (not the University of Kharkov as often asserted) in the Fall of 1918 and had less than two years of studies which were chaotic.

What type of institution was the Institute? The Merchant Society of Kharkov had operated since the 1890s a Higher Commercial Course for the training of personnel

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<sup>2</sup> I cite below several works by V. Moskovkin and by M. Mikhaylichenko translated with the help of Anna F. Syrquin.

<sup>3</sup> Wikipedia identifies Kuznets as “an American economist.” In Belarusian this becomes “American economist from Belarus” and in Ukrainian “American economist of Jewish descent, educated in Ukraine”. An article in Time magazine calls him a “Belarusian economist.” That reminds us of Einstein's statement: “If my theory of relativity is proven correct, Germany will claim me as a German and France will declare that I am a citizen of the world. Should my theory prove untrue, France will say that I am a German and Germany will declare that I am a Jew.” (*New York Times* 16 February 1930).

of commercial and industrial enterprises. In 1916, the Duma granted the Course the status of a Commercial Institute. Some saw in the commercial institutes a tool to promote Jewish dominance; a few years before when the courses were being expanded, Kharkov's Governor expressed concerns about the role of Jews. A 5% quota for Jews remained in place up to the 1917 revolution. (Mykhailychenko 2017). Academically its standards were high, and some of its staff might have also worked at the University.

Many have argued that the Institute closed in the wake of the October revolution. The Institute did not close in 1917 but only in 1920 after years of intense turmoil. Moskovkin and Mykhailychenko (2012, M&M below) deride the "stereotypical ideas about turbulent times" stressing that "Kharkov Commercial Institute continued its usual learning activities. Changes of fronts and regimes could not significantly disrupt the life of a big city." Maybe, yet Kharkov was serially occupied by Bolsheviks (November '17), Germans (April '18), troops of the Ukrainian National Republic (November '18), Red Army (January '19), Denikin's whites (June '19), and Red Army (December '19) which established Kharkov as the capital of the Ukrainian Soviet Socialist Republic. Some of the faculty at the Institute left with the retreating units of the whites and went into exile, others were put on trial or even shot (M&M). Commissars were appointed, and additional social and political courses with a Marxian basis were introduced into the curricula (Mykhailychenko 2017). P. I. Fomin, who headed the Institute at the time and one of the more important professors that taught Kuznets described the harrowing conditions under which they attempted to carry on from the end of 1919: "forced paralysis of institutional life; ... deprived of premises, library, of the possibility of organizing lecture work; without offices and laboratories, ... operating in buildings of the Jewish almshouse,..., with insufficient audience rooms, ..." (M&M: 31).

In 1921, the Institute was reorganized, partly in response to the anti-Soviet activity of members of the faculty, and became the Kharkov Institute of National Economy, the kernel of today's "Simon Kuznets Kharkiv National University of Economics." In October 1919, a large group of professors from Kharkov signed an "Appeal of Russian scientists to Europe." Compiled by N. Levitsky (another important professor of Kuznets), it contained sharp criticism of "the leaders of Bolshevism ... hiding under pseudonyms (Lenin, Trotsky), utopians and fanatics with criminal past ..." Shortly thereafter, Levitsky was arrested. (He returned to the University years later and rose to the status of Academician; M&M). That Kuznets managed to come out with a decent education attests more to his resilience and dedication than to the quality of the education at that time.

What exactly did he study and for how long did he attend classes is not at all clear. The available transcripts prepared (maybe) when applying to Columbia (M&M) show that he took examinations in the spring of 1919 and then just two additional exams in statistics a full two years later in July 1921. After less than two years in the Institute, he worked for 1–2 years in the Division of Statistics of the Central Soviet of Trade Unions where he published his first paper (below).

## 1.2 Courses and Teachers

M&M found the program of studies for the first two years for the academic year 1916/1917 (a year prior to Kuznets' enrolling). It shows, as course catalogs are wont to do, an impressive array of courses and instructors. However, in Kuznets' transcript for the first year (fall' 18-spring' 19), there are only two substantive courses: Political Economy with P. Fomin and History of Economics with N. Levitsky. In both courses, the grade was "Very Satisfactory." As for the other names mentioned, it is doubtful whether Kuznets took any of their courses or ever met them.

Levitsky and Fomin were probably significant influences in the economic education and subsequent career choices of Kuznets and are further discussed presently. A third and potentially most important influence might have been Antsiferov. He and Levitsky were students of A. I. Chuprov who was influenced by the Historical School. At Kharkov, Antsiferov was the main lecturer on statistics and published books on *Elementary Statistics* and *Population Dynamics*, topics that were to be of key importance to Kuznets. However, he left Kharkov with the white forces at the end of 1919, just as Kuznets was entering into his second year where he would have attended his courses. The transcript shows two grades for statistics (theory and practical) dated July 1921 and signed by I. I. Popov. By then, Antsiferov was long gone, and the University had been closed for a year, so it is not clear what he learned and with whom. We therefore concentrate only on the two main professors.

N. Levitsky was a former dean of the law faculty of the Kharkov Imperial University and rector before its closure. He did postgraduate studies at Heidelberg and Berlin including classes with von Schmoller (M&M: 21). Levitsky was the main author of the curriculum, and from him, Kuznets got a thorough introduction to economic doctrines and then thought that a critical assessment of economic schools was essential before proceeding to scientific economics. Levitsky's path to economics came through Chernyshevsky's translation of Mill's *Principles of Political Economy*, an influence which he passed on to Kuznets in his inductive methodology and in the overall approach to the study of the national economy. There are interesting parallels between the structure of Mill's *Principles* and the order of the series of essays on "Quantitative Analysis of the Economic Growth of Nations" published by Kuznets in *Economic Development and Cultural Change* between 1955 and 1967.

In his first book fresh out of Schmoller's seminar, Levitsky did not reject Austrian ideas. He acknowledged the contribution of Menger to methodology and approved of his critique of Marxism. He advocated separating the history of the national economy into an independent discipline and subject of teaching (Maidachevsky 2011: 77). Levitsky regarded the stages approach of economic historians as unscientific and mechanical: "historical research is unable to determine when one phase of historical development ends and another begins" (ibid. 84). There will be echoes of Levitsky at the Konstanz conference where Kuznets criticized Rostow's stages approach (Kuznets 1963a).

P. I. Fomin was among the founders of the Commercial Institute and dean of its Faculty of Economics during the turbulent years. His course on Political

Economy was the most substantive course of the program. He called for replacing the outdated methods of Schmoller historicism with the study of the “real conditions of modern industrial reality” (M&M: 27) advocating the use of methods of mathematical statistics. The methodological empiricism learned at Fomin’s course must have come handy when meeting Mitchell at Columbia. Fomin might have also influenced Kuznets in one more way: He was the originator and founder of the “Institute for the Economic Study of Russia” (usually referred to as the “Cabinet of Economic Study of Russia”) in 1914, probably inspired by the similar institute founded at Kiel on the same year. The aim of the institute was the collection and systematization of statistical and economic information of the industrial, commercial, and financial life of Russia. It published a *Bulletin* with statistics on the main indicators of the national economy and carried out research for private firms and government departments (ibid., 27–8). This was the first economic research institution in Russia which might have later influenced the conjuncture institutes in Moscow and Kiev. (Moskovkin 2011b).

### 1.3 *Statistical Office and First Paper*

Much has been made of Kuznets’ first job and of his first published paper, (Kuznets 1921), that he became head of the statistical office or of “a section of the bureau of labor statistics of the Ukraine” (Kuznets 1989), and that this bureau was similar to the National Bureau for Economic Research (NBER). What seems clear is that this was not an embryonic NBER and that he was not a senior associate. It was an opportunity to supplement the family income and maybe acquire some experience of a very practical type. It was never intended to be a stepping stone but a holding place before the family could rejoin the father in the US.

Kuznets published a paper in Russian in July 1921 on “Money Wages of Factory Employees in Kharkov in 1920.” Some have argued (almost certainly without having seen it, let alone read it) that it hints already to later work on fluctuations etc. and see it as the start of an academic career. Thanks to Moskovkin who dug out the paper and made it available (Moskovkin 2002), we can now read it ourselves. The paper is a detailed compilation and presentation of data on the remuneration of workers classified by trade unions in several industries, type of pay, hours worked, etc., over some months in 1921. It was written right after the establishment of the Ukrainian SSR and is of obvious interest but far from being a precursor to his work on time series, fluctuations, or any other analytical work. One element discussed is the system, or mode, of pay (time worked, output, premium, or other). This “mode mindedness” was to recur later as an impediment to the inclusion of communist countries in his comparative analysis.<sup>4</sup>

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<sup>4</sup> “Acute ‘mode-mindedness’ among the leadership was revealed in intense concern with whether or not goods were produced and sold under ‘capitalist conditions.’ The journals and statistics of the NEP and the first Five Year Plans were careful to spell out the different ownership structures

What type of articles were published in that journal, *Materials on Labor Statistics in Ukraine*?

The preface to the 2nd issue where Kuznets' paper appeared (only four issues were published) states that it comes out with great delay because of "incredibly difficult conditions of life and work in Ukraine during the winter of 1920–21" and that in the future, it will endeavor to publish papers of not just numbers but of methodological aspects. The editor of the journal N. Dubinskaya may have been a significant influence on Kuznets as argued by Filatov (2002). Dubinskaya was the head of the Department of Statistics (and not Kuznets) and as editor wrote a paper in each of the four issues. A Kharkov professor of renown, I. A. Trachtenberg was a consultant to the Statistical Office. Did they meet? Did Kuznets even know him? Fun to speculate but not likely.

### 1.4 Exit—Leaving Russia

In March 1921, the Peace of Riga was signed ending the war between Soviet Russia and Poland. Pinsk and Rovno were now in Poland, and refugees were required to return to their former lands (Moskovkin 2011b). Refugees, mostly Jewish, started heading west including the Kuznets family. Here, the story becomes even more cloudy. Kuznets had a permit (as a worker of a soviet office?) to travel in the Dnieper vicinity close to the border and tried to prepare the return of the family but without success. A variant tells of him being briefly arrested as they crossed into Poland and charged with espionage (as a soviet bureaucrat?), but soon released. Since emigrating from Poland was not an option, Simon and his older brother Solomon left for the US through the free city of Danzig, probably with false documents.<sup>5</sup> The ailing mother, younger brother George, and the rest of the family moved to Warsaw.<sup>6</sup>

### 1.5 Columbia

Kuznets arrived in the US in 1922 and immediately entered the School of General Studies at Columbia University where in very short order he got all his degrees: BA in 1923, MA in 1924, and the PhD in 1926 but probably completed in 1925.<sup>7</sup> Economics

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of each industry. Concern with changes in 'mode' was placed on a par with the recovery of output itself" (Boss 1986: 247).

<sup>5</sup> False documents for moving freely or perhaps to avoid being drafted. This may explain Kuznets sticking with the story of Kharkov as the place of birth.

<sup>6</sup> Moskovkin (2010) based on correspondence with Judith Stein (daughter) and Ruth Kuznets-Hauptman (niece).

<sup>7</sup> "The degree requirements also stipulated that [it would be] conferred only after a publisher's contract had been signed." (Ginzberg 1987).

at Columbia at the time was ranked just after Harvard at the top.<sup>8</sup> It is remarkable that it accepted two Jewish immigrant brothers at the time and no less remarkable the speed of Kuznets' accomplishments.<sup>9</sup> We have no information on what he studied there. He met Mitchell in graduate school and caught his eye fast, especially with his erudite and impressive master thesis on Professor Schumpeter.<sup>10</sup> Mitchell was the main influence on Kuznets as a teacher, researcher, and mentor. "Wesley Mitchell, ... had a tremendous influence on me" said Kuznets in his "Brief Talk" (1989); the only person so recognized here or anywhere else. The influence came through example, through their personal relation, and the courses: "Business Cycles" and "Current Types of Economic Theory." The courses of Mitchell and his research methodology were a natural continuation of his studies with his two professors at Kharkov. Difficult to disentangle where Kharkov gives way to Columbia; I'm inclined to think that Mitchell was the main forming influence on Kuznets, while recognizing that some of the groundwork had already been laid.

Before discussing the relation of Kuznets to Russian economics, I briefly note some of his main contributions emphasizing details that may have some bearing on the Russian connection.

## 2 Some Contributions

### 2.1 Cycles

Kuznets began his academic career with his PhD dissertation on *Cyclical Fluctuations* published in 1926. He came under the umbrella of Mitchell publishing two more books on the other commonly identified components of the cycle: secular trends (1930b) and seasonal variations (1933a). In 1929, he published a major article with an exposition of the work of Slutsky on the summation of random factors as a source of cycles which had been published in Russian with only an English summary (1929c). An English translation was published in *Econometrica* in 1937.

After Columbia, he worked for almost two years as a SSRC fellow on what became his book on *Secular movements in production and prices* (1930b), his only major work for more than two decades, not under the aegis of the Bureau and Mitchell. The 1930 book on six countries was truly the beginning of his interest in growth and comparative analysis. He presents there his thesis on retardation [below] and his influential finding about the existence of 15–20 year cycles in output and prices in the US which he labeled "secondary secular movements" later to be known as

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<sup>8</sup> Raymond M. Hughes, *A Study of the Graduate Schools of America*, (January 1925), cited in Irwin Collier, *Economics in the Rear-View Mirror*, <http://www.irwincollier.com/economics-graduate-programs-ranked-in-1925/>

<sup>9</sup> Academic antisemitism was rife in major eastern universities in the 1920s including Columbia even if it was not the worst offender (McCaughey 2003).

<sup>10</sup> The master thesis is extensively discussed in Perlman (2001) and M&M (2012).

“Kuznets cycles.” An implication of this work was that the study of long-term growth requires observations over periods that extend well beyond the duration of the cycle itself; half a century or more as a minimum. The 1933 book presents novel methods for measuring *Seasonal Variations* and emphasizes the burdens that such variations impose on the productive factors and society as a whole.

In the late 1920s, he applies his solid statistical background to explore various issues of time series analysis (taken to mean business cycles analysis) publishing the results primarily in the *Journal of the American Statistical Association*. He also begins writing entries for successive volumes of the *Encyclopaedia of the Social Sciences* being coedited by Columbia Professor Edwin Seligman (with Alvin Johnson), and where his older brother Solomon was among the assistant editors. The entry on *Time Series* is a compendium on business cycles with an up to date bibliography on the statistical analysis of cycles with prominence given to Mitchell and also a generous listing of German sources but none in Russian. In the entry on *Conjuncture*, Kuznets traces its origins to the German literature where it moved from astrology to “the totality of uncontrollable and variable market conditions.” It was widely used in German countries, Scandinavia, and Russia, while English-speaking countries preferred the term cycle. Thus, the proliferation of *Conjuncture* institutes in Europe at the time. Years earlier, Kuznets translated a summary of Pervushin 1925 book into English and still used “conjuncture” in the title—“an irksome cognate” to Owen (2009: 223).

He wrote three major papers in 1930 mostly on methodology, criticizing static equilibrium analysis and arguing for the necessity of an inductive approach for a general theory of change. He appears a bit extreme and uncompromising at this stage defending Mitchell and attacking others. The “mathematical school” is criticized for simplifying too much and thus unable to explain development, while Schumpeter’s *Theory of Economic Development* is hailed as a significant milestone. “Whatever the interpretation of the equilibrium approach, it seems to be a blind alley from the point of view of business-cycle theory” (1930a: 399).

## 2.2 *National Income and Capital Formation*

The very first publication of the NBER in 1921 was on National Income with a focus on personal income and its distribution, but by 1930, it had run into difficulties. In 1931, Kuznets was asked to make a survey to consider renewing the project at the Bureau “under the special supervision of Dr. Mitchell” (NBER 1933: 14). “He spent six months or a year going through the foreign as well as domestic literature, ... to organize the field. ... when Mitchell first asked him to work on national income he was not too keen; only when he got in to the literature did he begin to see the structural and conceptual problems that made the subject far more interesting than mere accounting” (Carson 1971; 81). The enterprise achieved national prominence when in 1932 Senator La Follette introduced a resolution instructing the Secretary of Commerce to prepare a report on the National Income of the US for each of the

years 1929, 1930, 1931. The report written by Kuznets was published in 1934. After that, he became the key player in an active field of research. He published various reports in the *NBER Bulletin* and books on national income, capital formation, and commodity flows. Kuznets's effort at estimating national income, while not the first, was so distinctive that it became the benchmark in the field.

For Kuznets, national income estimates were indicators of economic welfare and less so measures of short-run productive capacity. In the postwar period, the short-run perspective of the Keynesian approach and the related requirements of the political system for a more active macroeconomic policy to maintain full employment with price stability lead to the development of systems of national income and product accounts that Kuznets considered inadequate as gauges of economic growth and welfare (1972a). The short-run approach, focusing more on production than on consumption, prevailed in part because of the spread of Keynesian theory but also, paradoxically, because of the application of the Kuznets system of national income accounts to the war effort (below).

### 2.3 *The Economic Growth of Nations Project*

Kuznets doggedly pursued his idea of a comparative long-term study of economic growth for several decades. The work in the late 1920s that resulted in his 1930 book was the first substantive foray of Kuznets into growth and development. The national income project then fell into his lap, and this brought about a useful hiatus in the preoccupation with growth. The analysis of growth had to wait for the development of national income measures as he acknowledged in a retrospective account in 1983.

As I look back on the sequence of studies that I was instrumental in completing, they began with a group of related studies of factors—cyclical fluctuations, secular movements, seasonal variations—that affected the development of the American economy. Then they shifted to national income for a single country, the United States. Then they shifted to a wider view, using national income estimates and their components to compare the performance of different countries in many parts of the world on an international scale. That sequence of studies would not have been feasible between 1900 and 1920 or even between 1910 and 1930. It was feasible only between 1930 and 1970 (Kuznets 1989: 434–36).

By the early 1940s, Kuznets felt ready to finally embark on the study of economic growth. In a remarkable letter to Mitchell in 1943 while still at the WPB, he writes about his plans for a longer range study “related to a project which I discussed with you before Pearl Harbor and concerned a comprehensive analysis of the longer term economic trends in the industrial countries of the world. . . . I would now like to return to this broad area; and spend in the work on it whatever long years it requires and are within my capacity to give.” He concludes by apologizing for writing about such issues “at a time that objective conditions in the civilized world are in such a state of turmoil and human lives are wasted in such cruel fashion.”

Upon his return to the NBER in 1945, he drafted a fifteen-page document, “Preliminary Notes on the Study of Comparative Economic Growth,” circulated at the



NBER. The proposal was discussed, received favorable reviews from Mitchell and others but with the selection of Burns to replace Mitchell as research director, it seems to have been shelved. The missed opportunity was taken up by the Social Science Research Council (SSRC), under whose aegis most of the growth study was done. The SSRC in 1949 established a Committee on Economic Growth with Kuznets as chairman, and this enabled him to sponsor, with Ford Foundation funding, a score of studies on comparative long-term economic growth in advanced countries. Results from the various pioneering country studies were presented at the biennial conferences of the International Association for Research in Income and Wealth (IARIW) founded in 1947 with Kuznets as its first chairman (see below for a sample of papers). These studies formed the backbone of the ten long articles on the quantitative aspects of the economic growth of nations published in *Economic Development and Cultural Change* between 1956 and 1967. Growth was conceived not just as the increase in income per capita but was extended to encompass the most important changes in structure, distribution, and international trade. More elaborate and succinct summaries of the project appeared in *Modern Economic Growth* (1966) and *Economic Growth of Nations* (1971).

*Modern economic growth* (MEG) is Kuznets's term to describe the economic epoch of the last 250 years, distinguished by the pervasive application of science-based technology to production. The principal quantitative characteristics observed during the process of MEG are high rates of growth of per capita product, of population, and of factor productivity, and a high rate of structural transformation (Kuznets 1973). For Kuznets, "advancing technology is the *permissive* source of economic growth, but it is only a potential, a necessary condition, in itself not sufficient" (247). Its realization requires institutional and ideological adjustments. Keynesian analysis elevated aggregate measures to the center stage, which may have been appropriate for short-run stabilization but less so for long-term growth and development. One effect was the disappearance for decades of any analysis of industrial structure and structural change.

### 3 Acquaintance with Russian Economists

In Kuznets work, there is very little reference to Russia, her economy, or direct reference to economic ideas originating there. It seems that he did follow and read on Russian affairs and literature but was reticent to even speak Russian and refrained from visiting the Soviet Union even when invited.<sup>11</sup> This had to do more with the Soviet regime than with things Russian. We will therefore explore indirectly the influence of his Russian background on his work and observe his changing attitude toward Russia as it evolves from his emigration up to the award of the Nobel Prize. There is not much information but what there is, is powerful.

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<sup>11</sup> Paul Kuznets, personal communication, January 30, 2021.

We try to learn from (a) acquaintance with Russian economists in Russia, (b) interest as shown by reviews, citations, and dealing with Russian economics and affairs, (c) choice of research topics and approach, and d) any evidence of having absorbed some of the Slavophile ethos about a distinct Russian economics.

### 3.1 *Acquaintance Travels and Contacts*

From his studies and early career interest, it is clear that he was well acquainted with the main works on business cycles and statistics. In the late 20s, his linguistic skills are put to use and he serves as a conduit for introducing Russian work on cycles to English speakers, particularly Mitchell. He translated a summary from Pervushin's book *The Business Conjunction* for publication in the *Quarterly Journal of Economics* (Pervushin 1928).<sup>12</sup> Earlier, he had prepared a synopsis of Pervushin for Mitchell who referred to it in his 1927 book as a source for the relations between business cycles in Russia and in Western Europe. In 1929, Kuznets published the first paper in English on the "remarkable memoir by Professor Eugen Slutsky" on the summation of random causes as a source of fluctuations. Mitchell has a very brief mention of Slutsky at the end of his "Addenda" (1927: 478), referring to the "English summary" published with the original Russian article. It was most likely Kuznets who brought this to his attention.

Mitchell met Kondratiev during the latter's visit to the US in December 1924, yet it is likely that for the discussion of Kondratiev in his 1927 book, he again relied on Kuznets good offices. Kuznets (1930b) was the first to study Kondratiev's work in depth (Louca 1999), including his original 1925 study in Russian (and not just the abridged version in German), and the debate, also in Russian, with Oparin. In 1930, Kuznets gave an extensive summary of previous (and untranslated) work on long waves including van Gelderen (using the pen-name J. Fedder) in Dutch, de Wolff in German, and Trotsky in Russian. Kondratiev's 1926 German paper translated only in 1935 states at the end that only in 1926, did he become acquainted with de Wolff's article which "in many points reaches the same result as I do. The works of J. van Gelderen, which de Wolff cites and which have evidently been published only in Dutch, are unknown to me" (1935: 115). Kuznets may have been the source of this acquaintance of Kondratiev. Frisch also benefited from Kuznets' erudition and linguistic skills; in the spring of 1927, Kuznets lent him the draft of the book published in 1930 which, as seen above, included an account of the Kondratiev-Oparin debate (Louca 1999: 192).

Mitchell's addenda also mentions a Russian paper by Albert Vainshtein (spelled Wainstein there) on *Harvests, Meteorological and Economic Cycles* (1926) that reviews the recent literature on weather theories of business cycles. Vainshtein was a member of Kondratiev's Institute, one of the lucky few that survived (see below for Kuznets' review in 1962 of a book by Vainshtein). Mitchell mentions "the synopsis

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<sup>12</sup> But he did not translate Kondratiev, Slutsky, or Vainshtein as claimed by Filatov (2002).

of Wainstein's paper, supplemented by notes which Dr. Kuznets has made" (1927: 475). Weather is a major factor in seasonal variations in agriculture, an important topic at the Conjecture Institute at the time, and also one that should have interested Kuznets who was starting his own research that led to *Seasonal Variations* in 1933.

### 3.2 *Did He Know Any of Them?*

Contrary to some claims, during his brief university studies in Kharkov, Kuznets most likely did not meet its two stars on cycles: Tugan-Baranovsky and Slutsky. They were not in Kharkov during the period that Kuznets was there, and they probably did not lecture at the Institute. But, he did meet Kondratiev when the latter came to the US (Makasheva 2021) and remained in contact with him including the time Kondratiev was imprisoned at Suzdal.

It is not clear when Kuznets might have been in Europe in the decade following his arrival in the US. His visas showed visits of about two months to Danzig (Gdansk) in 1923 and 1927.<sup>13</sup> A niece (daughter of younger brother George) wrote that both brothers visited the ailing mother in 1925 or 1926 but met in Danzig not wanting to cross the border to Poland. As recounted in Barnett (1998: 92–3), Mitchell wrote Kondratiev after they met in December 1924 that one of his students working on cycles had told him that he had met him "in the old days in Petrograd," and that Mitchell had given him Kondratiev's address. Kondratiev did not remember meeting Kuznets. Could Kuznets have been in Petrograd in 1923 (in 1924 it became Leningrad) and met Kondratiev? Was he able to travel from Danzig to Germany and perhaps meet economists working at various Conjecture Institutes there?

In 1930, he published a booklet in German (never translated) on *Nature and meaning of the trend: On the theory of the secular movement* (1930c). It was published by the Frankfurt Society for Business Cycle Research where Eugen Altschul was the Director. Altschul had earlier edited the German version of Mitchell's *Business Cycles*, and when he emigrated to the US, he worked at the NBER. Other works in the Frankfurt series mention their origin in "the seminar of Dr. Altschul." Though Kuznets does not write about it, he may have visited Altschul's institute and perhaps also the one in Kiel where Marschak and Leontief worked before leaving the continent, Marschak for England and Leontief for the US where he was greeted at Ellis Island by Kuznets.<sup>14</sup>

The relation with Kondratiev goes further. In a 2004 interview, Angus Maddison "recalled that Kuznets told him Kondratieff was a 'family friend' and that the Kuznets family had tried to bring the great Russian economist to the U.S." (De Rouvray 2005:

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<sup>13</sup> He obtained a passport only after 1928 when he became a naturalized citizen. Judith Stein, personal communication, June 27, 2021.

<sup>14</sup> Kuznets not only greeted Leontief at the port but probably was instrumental in his getting the NBER research associate position. Leontief omits mentioning their exchanges; they appear in Bjerkholt (2016).

9). I considered this unlikely until I came across a paper on Kondratiev's Suzdal letters written between 1932 and 1936 to his wife and daughter while detained there in political isolation.<sup>15</sup> Klyukin and Yakovets (2006: 129–30) write that the Suzdal letters of 1932–1936 include dozens of references to Kuznets and that Kondratiev thanks him for books sent to Suzdal. They add that: “Kuznets is supposed to be the main Western informant for Kondratiev-prisoner.” Makasheva (2021) also reports of extensive interaction between the two.

## 4 Interest in Russian Economics

There is no indication of an interest in Russia and its economy during his studies in 1922–1926.

His first book in 1926 on *Cyclical Fluctuations* has no Russian references and nothing that would make us think of Tugan-Baranovsky or Slutsky.

### 4.1 *Statistics and Revealing Early Book Reviews*

Kuznets' engagement with Russia begins with book reviews and statistics. Russia, not least Kharkov, contributed to the development of statistics in the early twentieth century. The turmoil during 1919–1920 may have prevented Kuznets from benefiting more from the resources at Kharkov, but he probably arrived in the US with a solid base. He continued following developments particularly about time series analysis and its application to business cycles. In the 1920s, he made repeated reference to methods devised by Russian statisticians for the computation of moving correlation which he regards as superior to current practice. In particular, he refers to S. P. Bobrov's Russian “translation and adaptation for the Soviet reader” of the *Handbook of Mathematical Statistics*, edited by H. L. Rietz, and to the entry, there by Bobrov himself on “Changing Correlation” (Kuznets 1928). Other prominent Russian statisticians mentioned are N. S. Chetverikov and B. S. Yastremski.<sup>16</sup> In an article on measuring the seasonal amplitude of fluctuations (Kuznets 1932), Kuznets refers to “suggestion made some years ago by the Russian statisticians, Bobrov and

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<sup>15</sup> Kondratiev was dismissed from the Institute in 1928, sentenced to eight years in a prison camp near Suzdal in 1930, tried again in 1938 during Stalin's Great Purge, condemned to ten years without the right to correspond with the outside world and executed on the same day the sentence was issued.

<sup>16</sup> Bobrov and Slutsky are thanked by Pervushin in the article translated by Kuznets in the QJE. Bobrov was a most picturesque character known mostly as a “writer, poet, literary critic, and translator” but also as an “economist, statistician, chess theorist and author of mathematical works.” [http://archivsf.narod.ru/persona/bobrov/bobrov\\_s.htm](http://archivsf.narod.ru/persona/bobrov/bobrov_s.htm).

Tschetverikov to use the coefficient of regression” citing now a book by Bobrov on *Economic Statistics*.<sup>17</sup>

The book reviews are particularly of interest for this paper, especially two reviews of 12 books on Russia in 1929 in the same issue of the *Political Science Quarterly*. The first reviews five studies (published in three books) of Russian finances during the Great War, and the second reviews nine books on various current issues of Russia in the mid-1920s. All in English. After 1929, Kuznets falls silent on almost anything Russian for three decades.

The nine books, all part of the *Vanguard Studies of Soviet Russia*, were written by American and British communist writers or at least extreme sympathizers of the Russian experience “who since 1918 have visited the country and liked it” (Kuznets 1929b: 272). They are mostly apologetic tracts at a time when the young Soviet regime was being scrutinized and criticized in the West. Kuznets, with an open mind, takes these pamphlets seriously and tries to winnow what’s of value: “Sympathy and critical awareness, are both necessary if an honest study of such a live and controversial subject is to be made” (ibid). These reviews are of particular interest for assessing the early attitude of Kuznets to Russia and the USSR. First time that Kuznets expresses himself and quite freely on Russian affairs. It will also be the last time for many decades. In the reviews, he evinces already a very critical approach to loose use of data and to unsupported theories but also shows that at that time, he was somewhat sympathetic to the Russian experiment without overlooking the many clouds there and those approaching. Soon thereafter, he became much less sympathetic arguably because of the violation of rights and freedom that were there for anyone willing to see. The firing, trumped up charges, and jailing of Kondratiev and other economists were among the triggers for his recoiling. In the review article, he writes, for the first and perhaps last time in his academic output, of himself in the third person revealing some of his Russian background and continuing interest in Russian affairs: “The paragraphs that follow present the opinion of a layman, who has spent most of his life in Russia and who maintains a lively interest in current Russian history” (1929b: 273). The lively interest was soon to be extinguished.

A few additional revealing quotations from the review:

... the positive bias of some of the less critical authors has found too much play”; “The opening volume ... *How the Soviets Work*, ... indicates, if somewhat too briefly, the spots on the Soviet sun: the problem of democracy, the treatment of the intellectuals, and the troublesome economic question of capital accumulation”; “In *The Economic Organization of the Soviet Union* ... too much insistence upon achievements, and too little attention paid to the formidable problems to be solved.

Kuznets sympathizes with the dire international situation of Russia. Given the “attitude of the foreign powers towards Soviet Russia... One begins to understand the causes of the suspicions which present-day Russia entertains towards her powerful neighbors, and which may disappear only as years of peace roll by.” He refers to the plight of women in Russia who “do not yet have opportunities equal to those of

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<sup>17</sup> A recent study on the “Ukrainian Educational Literature On Statistics” discusses methods in the analysis of seasonality and mentions Bobrov and Chetverikov but not Kuznets (Kaplenko 2014).

men, may never have them, and in the hardships of current Russian life they may be bearing the heaviest load.” But, he adds with evenhandedness, “a number of purely medieval restrictions have been lifted and family life is on a healthier basis.” “The two books on health and on education lack interest because there does not seem to be a clear distinction between programs and achievements.” In conclusion: “in the ever increasing flood of journalistic literature about Soviet Russia, these books are among the best for the average uninformed reader. They do indicate, however, a need for a more thorough investigation of Russia by impartial foreign scholars” (276). Paul Douglas wrote a less even handed review of five of the books displaying an affinity with the socialist cause and a preference of it over capitalism (Douglas 1928).

In the companion review article, the three books reviewed were part of the Russian series of Carnegie’s *Economic and Social History of the World War*. These were written mostly by emigres some with government experience before leaving Russia. It is one of the few places where Kuznets presents a diagnosis of the ills of Russia’s economics before and after the Revolution. He inserts his own succinct summary of the economy of Russia before and in the aftermath of the war:

Russia, a country which depended so much upon imports of raw materials and of machinery, a country in which there has been both too much and too little alliance between the government and the significant industrial groups, a country in which the crucially important basic industries were a recent growth fostered to a very large extent by foreign capital and handicapped by an inadequate railroad net -- this Russia became almost completely isolated at the outbreak of the war and had to defend a frontier much larger than any other frontier in the world struggle. Add to that a lack of foresight and of statesmanship, a chauvinistic confidence based upon a history of conquests in wars essentially different in type from the industrial warfare of modern times, and one begins to understand the debacle that followed (Kuznets 1929a: 267-68).

Kuznets addresses issues of continuity and change between the Tsarist and Soviet regimes. “From the point of view of historical succession, and in spite of the different social basis of the governing groups, the peculiar system of economic organization which is now characterized as Military Communism was largely a direct continuation of the regulative organization which had its small beginnings during the first years of the war. And the essential economic problems of present-day Russia are mainly the same that were the source of trouble before 1917” (268).

A theme that figures prominently in his own work on cycles, the distribution of the burden, appears in recounting “the inadequacy of government regulation in Russia during the years 1914–1917.” Even though the regulation extended “toward a complete monopoly, yet at no time under the Tsarist rule was the regulative pressure effective enough to distribute the war burdens differently from the way in which the untrammelled contest of the social groups had put it. There was little effective attempt to change the social consequences of a war subsidized largely through inflation” (269).

He ends with his characteristic mix of praise and detailed criticism:

One leaves the volumes with the impression that here is a rich treasury of material, and that when completed it will be unmatched by books in any other language for its wealth of data. Defects in presentation, interpretation and omission there are, to be sure. One does not find

sufficient indication that the economic prosperity of Russia after 1905 and prior to the war was not sound all the way through. ... Nor is it emphasized that in the pampered conditions in which the large-scale Russian industry grew, the large size of its units was not at all a sign of technical efficiency. One might also object to the constant use of money terms, and to comparisons carried through without deflating money volumes to purchasing power (271).

Kuznets fell silent afterward. Kondratiev's arrest and the various Stalinist purges are probably a main reason. Vainshtein, a survivor from the Conjuncture Institute who spent many years in jail, told Alec Nove that the change in attitude toward such "specialists" as Chayanov, Kondratiev, and other Bolsheviks took place "By December 1927, before that, they listened to us as specialists. After that, we were enemies" (1993). The next encounter of Kuznets with Russian economics is precisely linked to Vainshtein. In 1962, he reviewed for the first and only time a book in Russian: Vainshtein's book on *National Wealth and Accumulation in Prerevolutionary Russia*. He wrote no more reviews after that. Vainshtein was best known for his work on national income and wealth. His lifetime work, published in 1960 and submitted as a doctoral dissertation, was started in the 1920s when he was deputy director of the Conjuncture Institute under Kondratiev and was completed only after his rehabilitation in 1956. The work was repeatedly interrupted during Vainshtein's some 18 years in prison, concentration camps, and exile (Campbell 2012: 425). It was fitting for Kuznets to review this book as if closing a circle and signaling that bringing attention to the work of the old colleague of Kondratiev was good reason to return to Russian writings if only for this one time. Reviewing or noting the work of associates of the Conjuncture Institute after 1930 might have put them in harm's way especially if done by an émigré like Kuznets.

Vainshtein's 1960 book dealt with historical data and with the methodology for calculating national wealth and accumulation. It was followed by a second book in 1969: *National income of Russia and the U.S.S.R.* (reviewed by Nove in 1970) where Vainshtein criticized official Soviet definitions of productive and unproductive services but also the Western definitions in use about the inclusion of civil service and the army and cited Kuznets in support. As typical of Kuznets' reviews, this too points to limitations but no less to what Kuznets sees as the merits of the work. Thus, he writes that the "lengthy review of conceptual and methodological questions ... fails to cover many of the recent writings" but still is of value "for the references to the Soviet literature and to earlier estimates of wealth in pre-revolutionary Russia. The presentation is sufficiently detailed so that the user can omit or include various components" (1962: 235). Then comes a disclaimer followed by praise. "This reviewer, not being familiar with the primary statistics for pre-revolutionary Russia, is not capable of judging how fully the available data were exploited and how judiciously the material was used. But, on the evidence in the book, a long and consistent effort appears to have been made to use a wide variety of data and to cover explicitly all significant components of material reproducible wealth within the country" (236).

A mild rebuke on the lack of international comparisons: "nor does he examine the level of the implied net capital formation proportion with that in other countries and consider the questions that would have arisen as a result of such comparisons" and the concluding endorsement. "But these strictures, relating to the estimates of capital

accumulation, ... should not lead to a neglect of the significant contribution made by the volume. It is clearly of wide reference value to readers interested in the economic history of Russia and of the USSR, and in the magnitude of national wealth, in its relation to national income and its components, in the process of economic growth" (237). Two years after the review Vainshtein published a paper on "Problems of International Comparison of National Wealth" where he laments the "insufficient attention ... given to ... international comparison of national wealth indices" (1964, abstract). Possibly prodded by Kuznets' review.

## 4.2 Conferences

There were various NBER and SSRC conferences, including one organized by Bergson in 1952, where Soviet issues related to income and growth were discussed. Kuznets did not participate or hardly commented on any of those before the 1961 conference that he organized with Bergson. By the mid-1940s, Kuznets was the undisputed authority on National Income. At that time, Sovietologists (and the OSS) started to construct estimates for the Soviet economy during the war and intensified the effort during the Cold War. Bergson was a key player; his 1961 book on *The real national income of Soviet Russia* lists Western scholars on the topic and their arguments for not relying on the Soviet estimates. Leontief and Gerschenkron appear in the acknowledgments, but not Kuznets. The text does include several references to "Professor Kuznets and the Department of Commerce," but none of those is related to the USSR.

Bergson's conference in 1952 on Russia's economic growth (published in 1953) was in response to concerns by social scientists about "the implications regarding Russia's military potential, [and] ... because of their interest in the recurring question of the comparative economic efficiency of different social systems, and in the problems of industrializing backward areas" (Bergson 1952: 29). All of the mentioned topics were of eminently current interest to Kuznets who was pursuing his project on comparative economic growth largely under the aegis of the Committee on Economic Growth of the SSRC of which he was chairman since its establishment in 1949. Bergson's Conference was organized by the joint Committee on Slavic Studies of the same SSRC, and yet, Kuznets avoided any link with it. I was not able to find even an oblique mention to it in the minutes of the Committee on Economic Growth for those years. The lead paper on National Income in the volume is by Gregory Grossman followed by comments by Gerschenkron, Leontief, and Bergson. No mention of Kuznets.

It was only in the late 1950s that the committee on economic growth started discussing the sponsoring of a conference project which eventually was held in 1961 and led to a volume edited by Bergson and Kuznets and published by the NBER in 1963. This conference came just a few years after the Nixon-Khrushchev kitchen debate of 1959, a time when US-USSR comparisons were being pursued assiduously in government and academia. The Joint Economic Committee of the US Congress



held hearings on the same topic in 1959. The 1961 conference differed from the one in 1952 in that this time rather than speculate on prospects, they were asked to do comparisons with other countries, especially the US.

The volume with the papers from the conference has seven studies by distinguished students of Soviet economics (Bergson, Kaplan, Holzman, and others) and an appraisal of comparative performance by Kuznets. The appraisal sheds light on his general attitude toward the Soviet Union and may explain why he had been reticent to include it among the countries studied in his large project on the economic growth of nations (below). The increased availability of data and the apparent success of the Soviet economy in the postwar period plus the Cold War may have made it difficult for Kuznets to continue to refuse to participate in such conferences and studies. His not engaging with the Soviet case in his comparisons was not just the result of his recoil at the events there. It was mostly due to his assessment that the usual measures of growth and structure may have been inadequate for such comparisons—their meaning may have been so different as to vitiate any such attempt. Here, he addressed the issue head on.

The concluding chapter compares the USSR and a number of other countries, for rates of growth, industrial structure, capital formation proportions, consumption expenditures, and foreign trade proportions. The list reflects the contents of the preceding chapters. At the end of the thorough quantitative comparison, described as a “sketchy review,” Kuznets concludes by describing the distinctive characteristics of the economic growth of the USSR:

It is a case of high rates of growth, with large inputs of resources and heavy human costs; of rapid shifts in industrial structure, away from agriculture and with emphasis on the industrial sector - both in terms of shares and relative product per worker - that differed in its speed and concentration from other countries; of limiting consumption and maximizing capital investment, achieved in combination with relatively moderate capital-output ratios to permit rapid aggregate growth; and of deliberate isolation from the rest of the world, so that the selective borrowing of production devices and the very limited exposure to the example of high and free consumption levels in other countries could be assured (Kuznets 1963b: 367–368).

The last section of the paper offers some “Concluding Comments.” After 30 years of intense work on the theory, conceptualization, and empirical estimation of national income, capital formation, and growth, Kuznets sits back and reflects on what it all means when applied to a system like the Soviet Union. He is willing to regard the estimates for the USSR by Western scholars as broadly comparable with the measures for other countries and proceeds to questions relating to the institutional peculiarities of the USSR experience.

There were clearly costs and returns in the economic growth that occurred in the USSR ... absent or of much smaller relative magnitude in the experience of other countries. Thus, there was a substantial loss of human life in the 1930s, occasioned by the violence of collectivization and experiences in labor camps that greatly reduced the life expectancy of their inmates—all in order to force the kind of economic growth that occurred. In our accounts the consumption of material capital is included as a cost but the extra consumption of human capital that may be induced by means aimed directly at economic goals is excluded.

...what about the cost of repression of freedom, the losses in human creativeness and welfare, that result from the forceful replacement of the judgment of the members of the community by the judgment of the dictatorial "planners"? The losses that resulted from the dead hand of control, from the conversion of literature, theatre, painting, and all the arts into production-propaganda devices, from the ruthless breaking of family and other group loyalties in service to the state, and from persistent isolation of the community from the rest of the world have been far-reaching indeed - as anyone comparing these fields of human life in the USSR and other countries (including pre-Communist Russia) can see (368-69).

But, typically, Kuznets adds that there are also returns that have escaped account:

The removal of inequalities based on inherited wealth and private monopolies, with the distortions in the consumption and the whole pattern of life that such inequalities introduce, ... The greater possibility of employment in productive work, and of a rise in the economic and social scale commensurate with ability, in the service of what appeared to be a social ideal, ...[and the] removal of much *economic* if not political, uncertainty [were] positive elements in the economic growth of the USSR not fully matched in other countries (369).

Kuznets then returns to a point made repeatedly in his work but that seems even more relevant in the present context, the fact that at times, the economic calculus is not sufficient or even appropriate.

Some of the costs and returns could be estimated by dint of statistical ingenuity: ... But even for some of these the economic price tag may seem rather irrelevant. For some others, such as restraint of freedom, the curbing of creative arts, and the like, no economic calculus is appropriate (369-370).

And ...

Of course, one can define economic growth in terms of the measures that we in fact employ; and if this is done, these questions are ruled out of account. But since the study of a defined process pursues some rational goal, one may ask what that goal is, if the study of economic growth, as gauged by standard methods, may be so overshadowed by ignored important costs and returns as to dwindle into insignificance. I assume that the measurement and analysis of economic growth along what might be called traditional lines is justified in the belief that noneconomic costs and returns are not so large and different as to spell misery and failure despite relative success with respect to traditionally measured economic growth. Indeed, an implication of such consonance between economic and other values is woven into the very fabric of economic definitions and measurements, geared as they are to the framework of a libertarian, nonslave society. It is therefore better to raise some of these questions relating to noneconomic costs and returns, rather than dismiss them implicitly as noneconomic.

Whether these missing, and economically unmeasurable, aspects of the economic growth of the USSR are really so large as to overshadow the economically measurable results, is a judgment that must be faced ... (370).

But then maybe in this case the objective is different:

The preceding paragraphs were written with a national product concept in mind that has marked welfare implications, if only in the sense that consumption outlay is considered a final product ... In the case of the USSR, we could perhaps abandon that concept entirely, and shift to the notion of increase in national power as the only substance of final product (370-71).

Finally, given the idiosyncrasy of the Soviet case and the vagueness about the objective pursued, the question arises of what countries to select for a meaningful comparison:

If, for example, the interest stems from an assumption of latent conflict between the USSR and the United States, the economic growth of the two will be compared in terms that bear most directly upon that interest (with perhaps a shift toward a concept of national power). If we are interested in comparing the economic growth of the United States and the USSR as typical examples of free and forced growth, respectively, our choice of aspects and periods will be different. Or we may want to compare the economic growth of the USSR and Japan, because they are the two latecomers among the major countries that have entered upon the phase of industrialization and modern economic growth.

Finally, we may be quite catholic in our approach, and assume that all countries entering modern economic growth—early or late, large or small, libertarian or authoritarian—display some significant common characteristics of growth; and that deviations from them, within limits, are reflections of the historical and locational peculiarities of individual countries. In this case we would compare the economic growth of the USSR with that of every other country in which modern economic growth has occurred (371).

## 5 Russian Influence on the Growth Study

Unlike the earlier period of research on business cycles where Mitchell's presence is paramount, for the growth project, it is more difficult to identify sources of influence. The beginnings in the 1940s were a break with Mitchell's and NBER thinking at the time. There is a strong emphasis now on comparative analysis and on the necessity of long-term records not obscured by cycles: "it may be urged that adequacy of data must be judged in terms of the uses of results: the detail needed for the study of annual fluctuations may be superfluous in a study of long-term changes" (Kuznets 1952: 17).

The Soviet case Kuznets saw (initially) as an interesting experiment to be observed and studied. But as we just saw, by 1963, his assessment is quite negative. His critique is not just about the trampling of human rights but also of the dubious economic achievements. Even if the official data were not suspect it still forced us to rethink what growth is for. Here, he finds the Soviet experience wanting unlike many of his contemporaries in the West blinded by the sun.

Some of the approaches and concepts in the study of growth may draw on the Historical School or on Marx (the nation as the unit, aggregate growth, stages, structural changes, interdisciplinary), but then we find him reflecting in 1952, a time of "rekindled interest in the subject of economic growth," on the inadequacy of all of the extant approaches:

Professional economists have long since abandoned the theories of economic growth of their Classical ancestors, [which] were naive, unhistorical, and were proved invalid by subsequent events; have rejected, largely for the same reasons, the Marxian doctrine of the growth and decay of industrial capitalism; have found little constructive promise in the methodological polemics and empirical studies of the Historical school and its successors (10–11).

For good measure, he also takes a swipe at Adam Smith:

Adam Smith's contemptuous reference to the limited service that political arithmetic could render stemmed partly from the conviction that his basic theory of social organization- liberty as the condition of full application of self-interest and hence of vigorous economic progress -was the key to the understanding of the basic economic problems of his age - and perhaps of ages to come. To him the empirical diversities of the past that could be revealed by political arithmetic, even disregarding its cavalier treatment of standards of empirical evidence, were largely irrelevant, .... And what was true of Adam Smith was equally true of his successors in the nineteenth and twentieth centuries since their firm convictions about the dominance of one or another factor, derived from a limited range of empirical reference, precluded the need or desire to record the evidence so that the full variety of experience could emerge (11-12).

Finally, he is also not much impressed by the early theories of development economics and simplistic models of the Harrod-Domar type:

The wider circles, ... have had the impression, ... that the factors making for economic growth of nations are well-known; that the pattern of growth is well charted; and that any country that wanted to could follow the path of economic progress by adopting well-established remedies.

More the pragmatism of Hume and Mitchell than the lessons of Marx or Keynes.

## ***5.1 Industrialization Debate***

So maybe the Soviet experience after all? Of the experience itself as we have seen, he was critical, rejecting the Soviet reliance on heavy industry and planning as panaceas. His emphasis on the importance of technology (the mainspring of Modern Economic Growth) made him less than enthusiastic about the Soviet case of high rates of growth, with large inputs of resources and heavy human costs. Any influence would have come from the "Industrialization Debate" that took place in the early 1920s primarily within Gosplan and the Conjunction Institute.

The industrialization debate, up to the point where holding the "wrong" view would get you canceled, was the first thorough discussion of strategies of growth. Even if it differed from the careful documentation and distillation of common trends and factors that was to characterize Kuznets' work, the conceptual framework of long-run growth and change probably influenced him.

In a nutshell, the debate centered on the speed at which industrialization could proceed. Related issues were the role of agriculture (balanced growth?), heavy industry, and overall balance between resource availability and plan requirements. Two of the key participants were V. G. Groman and V. A. Bazarov. For Groman, "Equilibrium, be it a dynamic one, of the economic body of a country is the highest requirement, each violation of which at once leads to a crisis" (Jasny 1972: 110). The Bolshevik left wing wanted industrialization at all costs and at maximum speed. In the second half of the 1920s, this was endorsed by Stalin and soon thereafter dissent became tantamount to sabotage.

The research of Groman on the planned economy was, according to Bazarov, “inspired by a single guiding idea of ‘the national economy as a whole’, ... In this respect he is to some extent a pioneer.”

This idea appears already in the title of the 1927 article: “Economic planning and the Groman conception of the national economy as a whole” (Jasny: 117). Bazarov in turn, according to Groman, offered the following as the criteria for an objective appraisal of the situation:

... the optimum combination of the development of productive forces, growth of well-being of the working masses, and the development of socialist forms of the economy. I used always to add: concern for the maintenance of a dynamic equilibrium in the national economy, which requires a balanced development of its components (Jasny: 110).

Balance, welfare, and socialism (in that order) are the desiderata.

Kuznets must have followed with interest some of the debates about planning and industrialization. He probably would have not supported comprehensive planning and would have favored a more cautious approach—more emphasis on agriculture, less on heavy industry, and more attention to the welfare of the individuals. More like Bazarov and Kondratiev than the Bolsheviks.

S. G. Strumilin, a well-known economist, statistician, and future academician, also advocated overly ambitious targets and strongly criticized those that called for cautious attention to resource constraints. Years later, Strumilin mocked Pervushin, Kondratiev, and colleagues “calling them ‘zealous devotees of the conjuncture’ (*userdnye kon"iunkturshchiki*); ... a savage pun, as the word has another negative meaning: a person who, lacking moral principles, seeks immediate advantage under shifting circumstances.” (Owen 2009: 236). Years earlier, Strumilin had written a book that became the Soviet standard book on planning. It was to a large extent a polemic against Bazarov’s 1927 book on *Capitalistic Cycles and the Rehabilitation Process in the U.S.S.R.* (Zauberman 1949). This is the book that Kuznets cited in his 1930 book after presenting the symmetrical logistic curve as best suited for his statistical investigation of the growth and decline of industries. In a footnote with references to the biology studies of Lotka and Pearl he adds: “Some interesting implications are discussed in T. B. Robertson, *The Chemical Basis of Senescence and Growth*. See also an interesting recent book in Russian, *The Cycles of Capitalism and the Economic Reconstruction of the U.S.S.R.*, by V. Bazarov, Moscow. 1927 (especially Chap. v).” (1930b: 64). The Robertson source also appears in Bazarov as does Bobrov and his method of sliding correlation (Bazarov 1927: 142) later strongly advocated by Kuznets in his *JASA* papers (1928 and 1932). The book also has references to Chetverikov, Chuprov, and various other statisticians. These coincidences may suggest that they were aware of each other’s work, or at least one of them had seen the others drafts. It is possible that Kondratiev was the link since Kuznets had met him shortly before and may have shared with him a copy of his draft manuscript or heard from him about Bazarov’s work. Worth pursuing.

In the preface, Bazarov states that the book originated in a study published in *Planned economy*, 1926, Nos. 4, 5, 6, on “‘Curves of development’ of the capitalist and Soviet economy.” This was probably the most significant influence on Kuznets

from early Soviet economics. When Gosplan initiated the planning of the national economy, they faced the question of whether the very high rates of recovery with the initiation of NEP could be maintained (Jasny: 126). Bazarov expected the fast growth during reconstruction to slacken and wrote of a “declining curve of growth” (sometimes referred to as “extinguishing” or “diminishing” curve). In his book, he illustrated it with a logistic curve that showed first accelerating, then decelerating growth with an upper asymptote. Dangerous concepts as they turned out to be. Sustained attacks on Bazarov and Groman followed and became virulent after 1928 when they raised objections to the directives for drafting the first Five-Year Plan (Jasny: 132). An article in *Planned economy* (one of many) in 1930 by A. Boyarsky<sup>18</sup> focused specifically on Bazarov and Groman in order to “refute the conclusions of the ‘learned’ saboteurs.”<sup>19</sup>

I single out two charges relevant for this paper: “the total absence of differentiation in his approach to our economy under capitalism and under the Soviet system” and that Bazarov’s theory is just “a repetition of Groman’s theory about the constant proportion between agriculture and industry.” Any consideration of balance that may hinder the drive to industrialization at full speed is seen as subversive. The first point implies that no comparative analysis is or will be possible given the sui generis nature of the Soviet experience. The article reiterates this point when it presents the use of the logistic curve by Bazarov only to state that it “does not apply at all to the growth of the Soviet economy.” Finally: “our plan is not to attempt to adapt ourselves to the free market, but ... to drive it out of existence; our plan is ... a leap from the domain of necessity into the domain of liberty. All this the members of the Kondratiev-Bazarov-Groman group of saboteurs cannot, or rather refuse to, understand” (Boyarsky 1930: 296).

*Retardation*, was one of the key findings of Kuznets in his 1930 book analyzed with logistic and Gompertz curves. At the national or sectoral level, we observe shifts in leadership among nations and shifts among branches as retardation inevitably sets in. Within a nation, the progress of technique makes new goods available (tea cotton, radios, ...), but eventually, demand reaches saturation, the pace of technical change slackens, new goods emerge, and possibly also competition from younger nations. With general retardation, come shifts in the relation between capital and labor, in the distributive process, in the character of the market, in the type of business organization, and in the roles of industry and agriculture.

A main theme in Bazarov’s book was business cycles and specifically secular trends and long waves. Mitchell in the same year (1927: 212) in a section on “The Problem of Secular Trends” writes: “Secular trends of time series have been computed mainly by men who were concerned to get rid of them. ... the economic statisticians have paid slight attention to their trends beyond converting them into horizontal lines”

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<sup>18</sup> A. Y. Boyarsky - Soviet economist-statistician, demographer, Doctor of Economics (1940), professor (1934), Honored Scientist of the RSFSR (1967).

<sup>19</sup> “On the theory of the diminishing growth rates of the Soviet economy” translated in the essential Spulber (1964). The article formed part of a special section on “Planned sabotage and statistical theory” from the transcript of the meeting of the Society of Marxist Statisticians of November 12, 1930. Three months later the Menshevik Trial began.

and then adds in a footnote: “So far as I know, the only one working upon secular trends as a problem in its own right is Dr. Simon S. Kuznets, one of the Research Fellows of the Social Science Research Council.” Bazarov writes similarly that with very few exceptions economists have not considered secular trends an independent problem of research. He mentions Warren Persons as “the only American economist who gave a rather deep and comprehensive *explanation* of the trends of the cycle” (80) but criticizes him for being overly preoccupied with short cycles and not very careful with terminology. He adds that Crum and Patton in their recently published textbook on economic statistics are even less careful and even negligent in their formulation (85). Interestingly, among the textbooks referred by Mitchell for computation of secular trends he lists Crum and Patton. Kuznets in his book on the subject just a few years later omits this and all other works in Mitchell’s reference.

## 5.2 *Déjà Vu at the War Production Board?*

Kuznets work during the war at the War Production Board (WPB) is not well known. In 1942, he went to Washington at the invitation of his former pupil Robert Nathan who was the head of the Planning Committee of the WPB. Their task was to estimate the impact of the victory program on the economy. Nathan went further and asked Kuznets to analyze whether the munitions production programs for 1942–43 could stay on schedule without a catastrophic impact on the economy or on other military objectives. The invasion of Europe was contemplated for 1943.

As was his wont, Kuznets immersed himself into the minutiae of the information and came up with a series of studies involving

the analysis of trends of civilian production in relation to national income, studies of national income and gross national product, and the examination of industrial capacity, materials, labor supply, and all other resource factors that operated as limiting elements on production. These studies, largely the work of Simon Kuznets, were directed to bringing production objectives into line with the ability to produce and they led to specific recommendations that had far reaching - effects on the magnitude and composition of the Nation’s production program (US 1947: 240).

In brief, the appraisal exposed the infeasibility of much of the war program in terms of resources and planning capabilities (see Kapuria-Foreman and Perlman 1995 and US 1947 for details). The generals were not amused. Nathan, a sharp and well-trained economist, was a big man with a kettledrum voice; Kuznets was a scholarly archetype: low-key, thoughtful, deliberate, balanced, and soft-spoken (Wolf 2011). They now took on the generals and an acerbic exchange followed. Brig. Gen. Brehon Somervell considered the findings about production goals and capacities and was “not impressed with either the character or basis of the judgments expressed in the reports and [he recommended] they be carefully hidden from the eyes of thoughtful men.” As for the judgments concerning the mechanism necessary for the formulation of the production program, these were just “an inchoate mass of words” (ibid.: 287).

The Chief of Staff, General George C. Marshall later added his stamp of disapproval. They were not alone.

There is a remarkable similarity to the industrialization debate. Luckily, there was no Stalin, Kuznets was not canceled, his views prevailed, and the US went on to win the war.

A final note, highlighting again the similarity to the debates in the USSR in the 1920s. Nathan and Kuznets had been willing to entertain the notion of “having a program sufficiently above the levels of feasibility to constitute the maximum inducement to an all-out effort” (285), but this only if there had been effective scheduling and production control permitting sensitive adjustments. In their absence, the only choice left was the overall size of the program. Nathan expanded on this in October 1942 in a presentation that strongly evokes Groman on equilibrium and balance. A large program, he argued, although acting as an incentive to total production, had several disadvantages:

The creation of a lack of balance in end items, complementary items, and components; the creation of excess fabricating capacity, which resulted in wastage of materials; failure to meet the objectives of the most important segments of the program while meeting less urgently needed items relatively easy to produce; ... increased difficulty in effectively controlling the flow and distribution of materials because of demands far exceeding supply ... (286).

The dramatic demonstration of the feasibility of applying the national income framework for measuring economic potential was an important factor in determining the direction of the postwar development of systems of national accounts. Kuznets continued to argue for a “peacetime concept” of GNP as opposed to the practice during World War II when “success in war and preservation of a country’s social framework [were] a purpose at least equal in importance to welfare of individuals” (Kuznets 1951: 184–185). Years later when Kuznets came to appraise Soviet performance in 1963 (discussed above), he reiterated his position that the definition of National Income depends on the objectives pursued, consumer’s welfare, or national survival in time of war. But in the postwar period with the ascendancy of Keynesian economics, his “peacetime concept” did not stand a chance.

### ***5.3 Russia in (or is It Out?) the Modern Economic Growth Study***

Why was Russia not part of the sample in the study on Modern Economic Growth (MEG)? The many publications on MEG by Kuznets beginning with the 10 essays (1955–1967) on “Quantitative Analysis on the Economic Growth of Nations” and all through his volumes in 1966 and 1971 Russia or the USSR are almost never mentioned. Russia had been mentioned as a possible country in the various proposals and outlines that Kuznets produced since the early 1940s. The change in emphasis is revealing.



In the 1943 letter to Mitchell mentioned above, he writes that so far his work and reading have been related to the advanced industrial countries of Western Europe and North America “But I suspect that a great deal can be learned from countries that have been relatively backward and have gone recently through rather surprising secular movements (such as the **USSR** and Japan) or countries for which the period of rapid industrialization is still to come (such as China).” In 1945, he writes in the proposal for the NBER Board that in selecting units for study “... priority ... is to be given to the major national units, as measured by their present size.” Namely “the United States, the United Kingdom, Germany, France, **Russia**, Japan, China, and India.”

In 1948, Kuznets was invited to write a proposal for a study on industrialization by the sub-commission on economic development of the UN Economic and Social Council (ECOSOC). In the “Outline of the Study of Patterns of Industrialization,” the long list of countries suggested included the advanced ones and “China; India; Netherlands East Indies; one or two Latin-American countries; and, **separately, Imperial Russia and the Union of Soviet Socialist Republics.**” (Kuznets 1948). The proposal was discussed in several meetings of the sub-commission before it was turned down. One non-enthusiastic representative was Mr. Morozov of the Soviet Union. In one of his interventions: “Mr. MOROZOV wished to limit himself, for the present, to one general observation, namely, on the basic distinction to be drawn between industrialization on a capitalist and on a socialist foundation, that is, in the Soviet Union and in other countries” (UN 1948). No basis for comparative analysis but rather: “Oh, East is East and West is West, and never the twain shall meet” (Rudyard Kipling, *The Ballad of East and West*, 1889).

As mentioned above, Kuznets, through the Committee on Economic Growth, recruited leading economists in various advanced countries to study long-term growth. Availability of long-term records of income and growth and competent researchers were the basic requirements. Neither Imperial Russia nor the USSR were part of the study. Results for other countries were presented at the biennial conferences of the IARIW. Between 1951 and 1963, there were no less than five sessions on growth (four of them organized alone or jointly by Kuznets): 1951, 1953, 1955, 1959, and 1963. A sample of the country studies and authors (see the site of the IARIW for full details):

1951: Germany—Paul Jostock, US—Simon Kuznets, Japan—Shigeto Tsuru;

1955: Italy—Benedetto Barberi;

1959: Netherlands—H. B. Bos, Norway—Juul Bjerke, France—J. Marczewski, Sweden—Osten Johansson.

In 1963, the session was devoted to developing countries including Israel, Yugoslavia, Taiwan, UAR.

Russia’s absence becomes now very visible.<sup>20</sup>

<sup>20</sup> There was one curious exception. In 1955 Raymond Goldsmith presented a paper at an IARIW conference on “The Economic Growth of Tsarist Russia 1860–1913.” The long essay was prepared as an introduction to a study of the growth of the Soviet economy at the NBER under a grant from the Rockefeller Foundation. Several NBER books were eventually published by the director of the

Even in 1955, Kuznets still entertains the possibility that “current work on **Russia** should eventually permit addition of that country as well.” It did not come to pass (Kuznets 1955a). Then comes the 1961 conference where Kuznets does present a comparison between the USSR and other countries followed by a devastating appraisal. In 1963, his growth committee undertook a study on Postwar Economic Growth in Europe, Japan, and the US directed jointly with Moses Abramovitz. The USSR was not even considered.

Finally, in his summary treatise on MEG (1966), he addresses the issue again but now the tone is sharper; less “should we include the USSR?” than “why we should not.” In the opening chapter, Kuznets wonders “whether the social structure and price system of the **USSR** (and even more, of Communist China) are sufficiently similar to those in the free countries to give meaning to quantitative comparisons of economic growth except for such inadequate indexes as steel production, etc.” (1966; 24) By the concluding chapter, he is more categorical:

The Communist countries, however, have shown the following characteristics: rejection of the “capitalist” forms of economic growth and the treatment of these other developed countries as the “enemy”; authoritarian management by a minority party of forced economic growth, with primary emphasis on producers’ goods; sharp restriction of individual producers’ and consumers’ freedom and of the supply of consumer goods; and autarky that has erected iron curtains partly to foster the image of a fortress besieged by enemies and partly to isolate the population from free discussion and possibly unfavorable comparisons of their social and economic attainments with those under a more liberal type of social organization. In all these respects, the Communist countries represent a major ... deviation from the traditional social and institutional structures within which modern economic growth was accommodated (347).

## 6 Heritage

Did Kuznets adapt the lessons from his Kharkov professors and from Mitchell and others at Columbia to the “specific Russian political, ideological, cultural circumstances”? If those circumstances are taken to reflect some of the mystical, patristic, Russian Orthodox approaches of Slavophiles then no. But, his early exposure to Russian economics and statistics during the most turbulent period around the October Revolution and his particular heritage as a wandering, evicted, and displaced Jew must have contributed to shape his approach and outlook in his professional endeavors. Many menshevik economists in the US probably did have a strong identification and affinity with Slavophile ideas or sided with Westernizers focusing on the reforms needed for Russia. During Communism, they recoiled and withdrew

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project, G. Warren Nutter and by others but Goldsmith’s contribution disappeared. A much reduced version came out in a volume in honor of Kuznets’ 60<sup>th</sup> birthday in *Economic Development and Cultural Change* where Goldsmith explains that “a change in the orientation of this whole study led to an exclusion of this paper from final publication by the National Bureau” (Goldsmith 1961; 441). A Kuznets was supposed to be the author of the chapter/book on agriculture, but because of problems raised by gaps and changes in the Soviet statistics *George Kuznets* was unable to deliver.

but remained wedded to the Russian fate and future, if only as a chosen subject in their academic pursuits. Kuznets by background, heritage, and worldview was probably less identified with such groups and did not see himself as part of the socialist vanguard that hoped the Soviet revolution was to be the harbinger of world brotherhood and freedom of want. Richard Easterlin, who was his student, collaborator, and friend, writes “the one thing that I remember is his saying that the foremost thought ... at the time of the Revolution was ‘Survival’.”<sup>21</sup> He followed the events and the literature but was keen to become American and horrified by Stalinism.

He moved from Pinsk to Rovno after the father had left for the US, was evicted from Rovno, witnessed various occupations of Kharkov, and finally was expelled again after the Riga peace. Virulent antisemitism was rife in that part of the Pale of Settlement and especially in the Ukraine where, as he was growing up, pogroms and blood libels were not uncommon. Notable gruesome events like the Kishinev pogrom and the Beilis affair must certainly have left their mark. We may try to relate some of his work to that heritage but must keep in mind, as Kuznets himself no doubt would have insisted, that simple association, without exposing a clear mechanism and providing empirical verification, proves nothing.

## 6.1 *Jewish Heritage*

His Jewish heritage may show up, as Perlman argues, in his lifelong interest in income distribution. Well before his famous presidential address that gave rise to the Kuznets-curve, he assiduously considered the burden of cycles and policies on immigrants and other vulnerable groups. More directly, we find this facet in work where Russia appears only as a source of migrants into the US without singling out Jews (Kuznets and Rubin 1954) and decades later with a focus on Jewish immigration (Kuznets 1975). Studies directly related to Jews other than as an important source of migrants into the US are discussed in the next section.

According to Perlman, Weyl, and family members, Kuznets aimed to become an American and emphasized, in his life and in the upbringing of his children, a liberal and secular (in the other meaning of the word) way of life, but he never reneged his Jewish roots or tried to uproot them.

There is no evidence of any activity in Jewish or Zionist affairs before the 1940s when the news from the Holocaust must have shaken him. The first evidence of any involvement (even if minor) is his membership together with Mitchell, Burns, and others, in the Editorial and Advisory Board of the American Palestine Institute, a Zionist non-partisan research organization founded by Maurice H. Blinken.<sup>22</sup> In 1945, the Institute sponsored a major research project to fill the need for an authoritative and objective appraisal of the economic potentialities of Palestine and the economic viability of establishing an independent Jewish state. The report authored

<sup>21</sup> Personal communication, January 14, 2021.

<sup>22</sup> Grandfather of Anthony Blinken, the current Secretary of State of the US.

by Robert Nathan (recently from the WPB and former student of Kuznets), Oscar Gass, and Daniel Creamer was reviewed by the Board and published as *Palestine: Problem and Promise*. (Nathan, Gass, and Creamer 1946). The heavy presence of the NBER in the Board was to ensure that this would be an impartial, factual economic investigation.

Kuznets had been teaching at Philadelphia, but only after the war did the family move there and into a predominantly Jewish suburb. A few years later, he began to make regular trips to Israel and was instrumental in the founding of the Falk Institute for Economic Research in Israel. Daniel Creamer helped in this endeavor and served as Director during 1954–55. Don Patinkin became its director in late 1956 but only after spending a sabbatical year with Kuznets at Johns Hopkins to learn from him about empirical research.

## 6.2 *Research on Jewish Economics*

Until recently, this facet of Kuznets' work was almost unknown. A decade ago, Weyl with the assistance of Lo did yeoman work and put together some major works of Kuznets on the topic written between mid-1950s and mid-1970s (Lo and Weyl 2012). These studies, they write, “reveal a lesser known side of Kuznets: the Eastern European Jewish immigrant, who persistently pursued the topic of Jewish history, yet hesitated to make his work more generally known due to his personal interest in the subject and therefore, by his reasoning, his bias” (ix).

Kuznets first substantive work on the topic began around 1955 when the Chancellor of the Jewish Theological Seminary of America, Louis Finkelstein, approached him to undertake the section on economics for a major compendium on *The Jews* he was editing. Busy as he was with his growth project, he still found the time and interest to quickly sift through masses of historical information and prepare a 150 page draft on the *Economic Structure and Life of the Jews* published five years later as a “slim” 69 page article (Kuznets 1960). It spurred a cottage industry on the Economic Structure of Minorities, especially Jewish ones. In the early 1970s, he returned to the topic with a focus on US Jewry. He delivered a brief presentation at the home of the President of Israel (1972b), based on a 150 page long manuscript available now in the Lo and Weyl collection. In 1973, Martin Feldstein wanted to include the long manuscript in the working paper series of the economics department at Harvard. Kuznets response, in a remarkable letter (he wrote a couple that merit the label), reveals his motivation for writing the paper and his unwillingness to have it published in the series. He writes: “I did this paper (and other in the series) because of my interests and associations as a Jew (I frankly doubt that were it not for these interests and associations, I would have, as a general economist, devoted much thought or effort to this topic),” but “I would deem it inappropriate to (publish the paper in the series)... the very choice of topic reveals a concern with, and interest in, a highly specialized aspects (sic). I would feel differently if this were a paper on trends in the structure of several ethnic minorities in the United States” (the letter

is printed in Lo and Weyl 2012). The last work on the topic, which he did publish in an issue of *Perspectives in American History*, was on the mass immigration of Jews from Tsarist Russia to the US. Russia is in the title, but the focus is on Jews and the US. One illustrative quote about Tsarist Russia in the “long century after the partition of Poland”:

Russia was largely a pre-industrial Christian state, with a record of intolerance of Jews for centuries before the nineteenth and with an economic structure that left little room for the more advanced economic activities in which the Jewish minority had a comparative advantage. This was particularly true since the government was unwilling to grant the newly acquired population sufficient freedom of residence, choice of occupations, and free economic intercourse with the Christian majority to permit the minority to profit from access to a much larger population and potentially wider market than was available earlier (Kuznets 1975: 60)

Though he does not refer here or elsewhere to personal or family experiences, the feelings must have been close to him. Years earlier in a comment on the large borrowing in international markets by Russia before World War I, he writes:

... the Russian government may actually have spent a good part of this borrowed money on police, on the organization of pogroms, and on the state church; ... The assignment of capital funds imports to capital formation within the country is thus arbitrary (Kuznets 1955b: 34).

## 7 Reciprocal Influence?

Some final observations on the reciprocal influence between Kuznets and Russian economics.

As argued above, the notion that Kuznets arrived as a formed economist with a baggage of ideas and approach cannot be sustained. Kuznets developed his theories after he completed his studies in the US. The influence of Western economic theories would have come through his teachers at Kharkov, his voracious reading, and then, and predominantly, through Mitchell’s influence who in turn was much influenced by the Historical school, by the Social Gospel of the founders of the AEA and by American pragmatism (Veblen, Dewey, Peirce). What he does after completion of his studies at Columbia will reflect what he brought with him in his toolbox, but probably more important, how this was sifted through the Mitchell sieve. His admiration for Mitchell ensured that the latter’s views would prevail at least in the early phases.

Various authors, especially Barnett and many citing him, have held that mainstream economics in the US was a significant beneficiary of work undertaken in Russia/USSR by emigres such as Marschak, Leontief, and Kuznets. Maybe for the first two but not proven for Kuznets. Barnett (2004: 33) has also argued that “The works of Tugan-Baranovsky, Pervushin and Dmitriev all had significant influence on Western accounts of cycles such as that developed contemporaneously by Wesley Mitchell, Simon Kuznets and J. M. Keynes.” Again, perhaps correct for the other two but less so for Kuznets. Pervushin may have influenced him, and he certainly was aware of Tugan-Baranovsky, but what he did in his three books on cycles was

not close to Tugan and less so to Dmitriev. Barnett and others try a bit too hard to find links. That someone in Russia mentioned “amplitude” which figures also in Kuznets’ *Seasonal* book does not imply a link. Not “everything is in Smith.”

One final example again due to Barnett (an indispensable source on early Russian economics to be sure) relates to National Income. Kuznets’ work was path-breaking and became the gold standard for many years, but it was obviously not the first study on the subject, not even within the US. After all he started where King and others had left off. Prokopovich estimated national income in 1906 Barnett tells us. Fine, but so did many others. Barnett argues that in Kuznets’ 1941 book on *National Income*, “the concept of national income itself owes a clear debt to his pre-NBER Russian context” and offers as proof page 8 where Kuznets refers to the market “as a complex of social relations of a certain type, and marketability as the characteristic of goods involved in them.” “Marketability” Barnett tells us was frequently employed in Soviet debates in the 1920s, and “the idea of the market as a set of social relations had obvious Marxian paternity.” Perhaps, but one can also find similar use in Commons which probably was a closer source through Mitchell. As for “marketability” here is what Kuznets writes in the preceding paragraph: “Unless an object is a source of satisfaction, relatively scarce, and disposable, it is not bought or sold. Marketability implies these three attributes...” In his work on National Income, beginning with the Senate report of 1934, Kuznets emphasizes utility and not cost but ends up looking at both, yet the end result is welfare. More like Bohm Bawerk (or perhaps Mill) than Marx.

We come now to the final question: to what extent did Kuznets’ writings in economics influence back Russian and Soviet economics? A preliminary answer pending a more thorough examination of Russian sources suggests that the reciprocal influence was almost nil. I am aware of only two reviews of Kuznets’ books in the Soviet Union: one of the 1926 book on *Cyclical* in 1927 in the journal *Planning economy* by a cycles specialist that finds it a useful reference but is quite critical of the procedures. The second is mentioned in a memoir by A. V. Anikin (1995) that recounts befriending Kuznets in Thailand and that after receiving from Kuznets a preliminary version of *Capital in the American Economy*, Anikin together with S. M. Nikitin reviewed it in a Soviet magazine [no source]. Anikin also writes that “neither Mitchell nor Kuznets were aspired to be ideologists, and that was their great advantage” a necessary clarification at the time, just in case....

It appears that Kuznets’ main areas of interest—time series analysis, business cycles, income, and growth—were problematic in the Soviet Union. A few examples: On statistics, while Kuznets initially cites approvingly theoretical advances in Russia, statistical analysis (as distinct from data collection) became suspect or irrelevant since under socialism statistics as a science based on stochastic assumptions, was going to wither away (Holubnychy, 1958). For Maslov (influential statistician, wrote textbooks, and later tried to introduce econometrics) a random sample study of collective farms used to compute the average income of their members amounted to “bourgeois methodology.” The same applies more generally to business cycles. Zauberman (1949: 189) cites the “well-known economist Aizenshtat ... summary

review of American economic writing” based on the *Readings in Business Cycle Theory* (1946)<sup>23</sup> where he writes about.

the bourgeois apologetic business-cycle theory of long waves ... a notorious attempt by fierce defenders of capitalism to contest the uncontestable fact of capitalism’s cyclical development through over-production crises which repeat themselves every seven to ten years.

Aizenshtat criticizes specifically Schumpeter and Neisser and, by implication, Kondratiev. Kuznets who had done much on long waves is not even mentioned. Better to ignore emigres. The MPS system of Soviet national income accounting was significantly different from the Western definitions (see Boss 1986). Modern economic growth was regarded as a study of the spread of capitalism, and comparative analysis was frowned upon given the uniqueness of socialist development.

We might have expected this to change after 1990 but there is very little evidence that it has. A perusal of several years of issues of *Problems of Economic Transition* reveals that except for Gaidar (1997 and 2004), there is no trace of Kuznets, certainly not of his growth studies—the project singled for the award of the Nobel. A partial exception is the long-run study of growth within the international KLEMS project directed by Ilya Voskoboynikov at the Higher School of Economics at Moscow.

It is only because of the fortuitous fact that Kharkov decided to reclaim Kuznets as their own Nobelista that there has been some renewed interest in the man if not in his work.

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<sup>23</sup> Published in *Planned Economy* in 1947 with the title: “Scholarly Minions of American Capital”. Zauberman gives a gentler translation “Learned Attendants on American Capitalism”.

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Andrei A. Belykh

A. Gerschenkron (1904–1978), historical economist, is famous for his concept of the positive role of relative economic backwardness in economic development. «His insights continue to influence a new generation of scholars and guarantee him a central place in any assessment of the evolution of the discipline of economic history» (Fishlow 2018, 5287). The formation of his scientific mentality was strongly influenced by his life events, which explains the structure of this article—at first, it will focus on his biography, and then, it will analyze his theoretical views.

## 1 An Odyssey

### 1.1 Russia

Alexander Gerschenkron was born on October 1, 1904, in Odessa. By the beginning of the twentieth century, Odessa, with population of about 405,000 people, was the fourth largest city in Russia. It was a multi-national city: 49%—Russians, 31%—Jews, 9%—Ukrainians, 4%—Poles, 2,5%—Germans, 1%—Greeks. Odessa's Jewish community united 124 500 people, being the largest in Russia (Demoskope 1991). Odessa was a big cosmopolitan city-port with crowds of foreigners and with unique business and cultural ambience. It was much more alike a Mediterranean town in Europe than a typical Russian one.

Gerschenkron's father, Paul, graduated from Odessa University. Thanks to assistance of Samuel Gourary, a businessman, who financed his studies, he continued

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his education first in Sorbonne and then in London School of Economics. Later, Paul Gerschenkron became Gourary's junior partner and manager of his two factories—a tobacco one and one producing matches. Paul was a well-educated person. Besides Russian and Ukrainian, he spoke nine languages. His family owned a rich library. Alexander's father was Jewish. His mother, Sophie Kardon, was half-French half-Russian. Sophie was quite indifferent to the matters of religion but showing respect to her husband's sisters—orthodox Hebrewesses—agreed to circumcision of her two sons. However, Alexander always referred to himself as “typically Russian” (Dawidoff 2002, 31–33).

Gerschenkron learned to read when he was 4 years old and during all his life remained a passionate reader. Once he admitted that he even did not remember how many times he had read Leo Tolstoy's “War and Peace.” Meanwhile, he was not a book worm—he liked mischief, was a good swimmer and sometimes could come to blow with his mates.

Alexander entered Gymnasium but did not manage to finish it. The reason was the beginning of the Russian revolution and the outbreak of the Civil war.

After the October revolution of 1917, power in Odessa often changed hands. As Samuil Gourari had immigrated immediately after the revolution, Paul Gerschenkron, who stayed in Odessa, continued to be manager of his tobacco factory. The factory being a joint stock enterprise, Paul was supposed to pay dividends to shareholders. When the Bolsheviks were in charge, he suspended the dividends. When the Whites regained power, he resumed paying them. As a result, in 1920, when the Bolsheviks finally seized power, Paul Gerschenkron was accused of giving away the state profits (Dawidoff 2002, 49). Being under threat of arrest, Paul had to escape. In a hurry, he packed his things, and together with his elder son Alexander, they started out. Having reached a village on the bank of the Dnestr, they crossed the frozen river and found themselves in Romania.

## 1.2 *Austria*

The Gerschenkrons came to Vienna. Initially, they were planning to get to Paris but Jules Gourari, Samuel's son, suggested Paul to become manager of a turbine factory in a small town Stockerau to the north-west from Vienna. So, they stayed in Austria. All in all, Alexander Gerschenkron had been living in Austria for 18 years—longer than in Russia which he had left at the age of 16.

In 1914, the Austrian Empire covered the area of 676 000 sq. km, with population estimated at 52 750 000 people. Defeat in the World War I brought Austria to the collapse of the Empire. By 1920, its territory decreased to 84 000 sq. km, and population—to 6,455,000 people. Bread was rationed, to find job was next to impossible, Austria suffered an acute shortage of accommodation.

In comparison with many others, the Gerschenkrons lived a decent life. Paul had a well-paid job, which allowed Alexander to continue education. To enter an Austrian gymnasium, he had to learn German first. In 1921, he tried to pass exams but failed

in Latin and Geometry. In July 1922, he managed to pass all the exams and was admitted to the 7th class of gymnasium. However, Alexander was not overjoyed—by that time he was already 18, his achievements were next to nothing and he even did not know for sure what future occupation would fit him.

In gymnasium, he shared his table with the only girl in his class, a blue-eyed blonde named Erica Matschnig. Soon, Alexander realized he was deeply in love with her. But to his disappointment, when he declared his feelings, she answered that they would only be good friends.

After Alexander had finished gymnasium, a “Handbook on economics” quite accidentally caught his eye, which predetermined his future occupation. In autumn 1924, he entered the University of Vienna’s famous school of Nationalökonomie. At that time, he started dating Erica.

It is not clear, what exactly did Gerschenkron study in the university and for how long did he attend classes in economics or economic history. But Paul Samuelson, who knew Gerschenkron very well, had written after his death: “It was his tragedy that the economics he learned in Vienna from 1920 to 1935 was unbelievably primitive” (Dawidoff 2002, 90).

In 1928, Alexander Gerschenkron graduated from the University with a diploma of Doctor rerum politicarum. Immediately after the gala dinner organized by his parents to celebrate the event, Alexander registered his marriage with Erica, who by this time had been expecting a baby. Soon, she gave a birth to a daughter named Susanna. Alexander got a position at a Belgium motorcycle firm as its representative in Austria and in the Balkan countries.

While a student, Gerschenkron got interested in politics. His thesis was called “Crises of Democracy and Political Parties.” In the 1920s, he became a member of the Austrian social-democratic party. He was no longer interested in commerce. In 1931, he quit the Belgian firm and engaged in political activity. For 3 years, he was working as an activist of the socialist-democratic party. It was at this time that his first publications appeared (Gerschenkron 1931, 1932).

In February 1934, workers’ uprising against the dictatorship government of Chancellor Dollfuss started. However, it was severely suppressed, with about 1500 people being killed. Alexander was seriously wounded and found shelter at his parents’ house. It is a well-known fact that later, after he had emigrated to the USA, Gerschenkron never recollected about these events in Austria.

A modern historian M. van der Linden, basing on Gerschenkron’s articles of the late 1920s—early 1930s and some archival documents, suggests his explanation to this fact. He argues that in fact Gerschenkron’s political views had been much more of leftist than later was admitted by himself and by his biographers. In his article written to commemorate the 20th anniversary, the Lena massacre (April 1912) Gerschenkron sympathetically wrote about “gigantic storming of the Russian working class” of 1917. In his article, “The Constitution of the Soviet Russia,” he described the Soviet state as “the realization of a new, higher form of democracy—the Soviet democracy” (Linden 2012).

Linden also published Gerschenkron’s letter to Otto Bauer (dated June 10, 1935) where Gerschenkron wrote: “In November last year, I joined the C.P. together with

some comrades. Central to our political aspirations was the proletarian United Front, ... which we have recognized to be one of the most important preconditions of the revolution” (Linden 2012, 559). According to the memoirs of a social-democrat D. Buttinger, also cited by Linden, Gerschenkron was an active Communist party member under the name of Turk. Curiously, at the Viennese gymnasium he was nicknamed “Russ” (Russian). Later, in the USA, Gerschenkron’s views changed—he became strongly critical toward communism and the USSR political system.

Gerschenkron had to provide for his family, so he lectured at the Public University. From 1931 to 1935, he worked as a research analyst with the Austrian wholesale cooperative society, gave private lessons. In 1937, his second daughter, Heidi, was born. In 1937, Gerschenkron started working at the Austrian Institute for Business Cycle Research established in 1927.

In 1937, a Californian University professor, Charles Gulick, came to Vienna. He had won a grant on writing a book on Austria, and he needed an assistant. Gulick addressed the Institute for Business Cycle Research and was recommended to hire Gerschenkron. Alexander agreed to take this job. Gerschenkron’s decision to cooperate with Gulick shaped his future destiny.

On March 12, 1938, when the German troops occupied Austria, the Gerschenkrone found themselves in a very complicated situation. Erica with their elder daughter, Susanna, managed to leave for Zurich by train. One of their acquaintances, a Dutch minister, got the younger daughter, Heidi, out of Vienna passing her off as his own daughter. Alexander was lucky to cross the borderline with Switzerland.

Later Alexander used to tell a story about his crossing the border on the passport of a dog. His student from Harvard University, D. McCloskey, wrote that as Gerschenkron was known to often invent different funny stories about himself, no one believed in that one about the dog’s passport (McCloskey 1992, 241). In fact, he got from Vienna to a tiny town near the border with Switzerland. There was a salt mine there which hired workers from Switzerland who used to commute to their job in Austria in the morning and return home in the evening. One of the workers had a pass for himself and one attendant. A German security guard at the borderline was outraged: “What does it mean “an attendant”? It could be a person as well as a dog. That is the last time I allow you to cross the border” (Dawidoff 2002, 113). But this “last time” was perfectly enough for Gerschenkron—he was on the other side of the Rhine, in Switzerland.

### ***1.3 The USA***

Alexander Gerschenkron arrived in Zurich to meet Erica who was waiting for him there. Alexander, Erica, and their daughter Susanna immediately left for Great Britain, to A. Gerschenkron’s father. There an invitation to the USA from Ch. Gulick had already awaited Alexander. Gulick provided him with Affidavit of Support. No wonder, Gulick’s invitation was readily accepted. So, that is how the third, American, period of Gerschenkron’s life started.

The beginning of Gerschenkron's scientific career was not very impressive. He was 35 years old and still assisting well-known scientists with their publications. The result of Gerschenkron's joint research with Gulick was a fundamental book "Austria from Habsburg to Hitler" (Gulick 1948). Many years later, Gerschenkron admitted that it was he, not Gulick, who authored the larger part of the book. Though in the 1940s Alexander believed that "a book for a passport was a good deal," later he deeply regretted that his authorship had been lost (Dawidoff 2002, 116).

In December 1941, after Japan attacked Pearl Harbor, the USA entered the World War II. In 1942, Gerschenkron even attempted to join the army but was rejected because of his age. He got a position of a lecturer at Berkeley University. At first, his salary was \$1300 a year. It was not enough to support a family of four members. Gerschenkron had to start working on a regular basis as a shipfitter and a flanger at the shipyard in Richmond, near Berkeley. At the shipyard, he was paid \$62 a week—twice his weekly payment at University, which allowed him to buy a car and household appliances.

It was at this time that he started doing his own research on Germany's economic history. In 1943, his first book entitled "Bread and Democracy in Germany" was published, which immediately was ranked as classical study on the history of economics. His reputation of a serious researcher seems to influence the decision of the USA Federal Reserve System to invite Gerschenkron in 1944 to take a position of an economist-analyst. So, Gerschenkron with his family moved to Washington.

In 1944, FRS managers realized that Germany was going to be defeated. This made the relations between the USA and the USSR after the war an issue of top priority. Starting his scientific career with the study of European economy, Gerschenkron shifted to the problems of the Soviet economy. Soon, in 1945, his work "Economic Relations with the U.S.S.R." appeared. Basing on the analysis of the Soviet import and export structure, he concluded that under favorable political conditions "we may hope that the end of the war will mark the beginning of an extensive and fruitful economic collaboration between Russia and the rest of the world" (Gerschenkron 1945, 73). However, after the end of the war, the political relations of the USA and USSR deteriorated and Gerschenkron in the article "Russia's Trade in the Postwar Years" made a conclusion: "Should a political settlement with Russia take place, then trade presumably would develop very favorably, and its growth might in turn contribute in some measure to the further stabilization of the world situation. But at present, not even the contours of a settlement are visible" (Gerschenkron 1949, 100).

In his works of that period, Gerschenkron paid much attention to the analysis of the Soviet trading policy, which was important for American prognosis about trade with the USSR. This problem was investigated in such works as "Russia and the International Trade Organization" and "Soviet Policies versus International Cartels: Four Historical Case Studies," the latter having been classified and published only in 1974. This work studied the history of the Soviet trade in matches, phosphates, potash, and platinum during 1930s. Gerschenkron's conclusion was of a general character and could be applied not only to the specific area of trade, but to the Soviet policy as a whole: "Soviet policies, both internal and external, both economic and noneconomic, were essentially determined either by exigencies of given situations,



or, much more generally, by the mechanics of the exercise of dictatorial power. ... the function of ideology was not to determine but to vindicate policies which were embarked upon and pursued for reasons other than ideology” (Gerschenkron 1974a, b, 69).

In 1945, with the Cold War looming, American intelligence needed experts on the Soviet Union. In summer, Gerschenkron was suggested a job at the Office of Strategic Services (OSS). Chief economist of the USSR Division in OSS was A. Bergson. A future Nobel Prize winner Simon Kuznets was working there for a short time. On September 20, 1945, OSS was disbanded. Gerschenkron moved to work at the State Department. Then, in 1946, he returned to the FRS where he headed the International section of the Department of research and statistics (Rosovsky 1979, 1010).

In 1948, Gerschenkron was invited to Harvard University as Professor of economic history. As D. McCloskey wrote: “I do not know what possessed the Harvard faculty of economics in 1948, well-known at the time for its anti-Semitism, to hire Gerschenkron ... Perhaps he impressed them” (McCloskey 1992, 245). It did not take Gerschenkron long to accept the invitation. His Odyssey came to an end. He finally settled in Cambridge and was working at Harvard from 1948 till 1975. It was in Harvard University where he became “that very Gerschenkron” and earned reputation as a scholar who knew all about everything.

## 2 Science

### 2.1 *The Gerschenkron Effect*

Beginning with the 1940s, the main area of Gerschenkron’s research was the economy of the USSR. The excellence of his analysis is felt even in his reviews, be it a review on N. Voznesensky’s book “Military economy of the USSR during the Great Patriotic War” (Gerschenkron 1948), or on “History of National Economy of the USSR” in 2 volumes by P.I. Lyaschenko (Gerschenkron 1952a, b).

Besides, at this time Gerschenkron’s interests were also focused on Soviet statistics. There was a lack of consensus among American experts on official Soviet statistics. The largest part of specialists (Gerschenkron included) held the opinion that statistics in the USSR was generally reliable. Naum Jasny supported an alternative point of view (Jasny 1950). He thought that it was falsified and considered Gerschenkron’s position to be erroneous. According to Gerschenkron, Soviet statistical data could be used but with great caution.

Gerschenkron himself used Soviet statistics very carefully in his articles of 1947: “The Rate of Industrial Growth in Russia since 1885” and “The Soviet Indices of Industrial Production.” To estimate the industrial growth, it is necessary to measure the impact of prices development on the value of output. Typically, it is done by using two indices.

Let  $q$  be the quantity of products,  $p$ —the price of these products. The upper index  $0$  corresponds to the base period and index  $t$  corresponds to the current period. The formulas given below imply that all products are summed up, index  $i$  of particular product being omitted for simplicity. There are two ways to measure the impact of price development. Firstly, it is possible to measure the value of output in the base period prices. As a result, we get a so-called Laspeyres price index:

$$P_L = \sum q^t p^0 / \sum q^0 p^0$$

Secondly, we can measure the value of output in the current period prices and get a so-called Paasche index:

$$P_P = \sum q^t p^t / \sum q^0 p^t$$

Analyzing Soviet statistical data, Gerschenkron found that “in a country in the first stages of industrialization the spread between prices of industrial goods of a low degree of fabrication and prices of highly fabricated goods is relatively larger than in a well-developed industrial countries”(Gerschenkron 1947, 220). It follows that for the countries still in the process of industrialization Laspeyres index will systematically give higher figures of production than Paasche index. This effect was named “the Gerschenkron effect,” and now, his concept is considered to be an important component of modern economic statistics.

Gerschenkron’s description of this effect is as follows. In preindustrial economy, the output of tractors is relatively low, tractors being expensive, while sickles are common and cheap. As an economy industrializes and technology improves, the mass production of heavy machines brings the price of tractors down. Therefore, if statistics of economic growth uses the base period prices (the formula with Laspeyres price index), total output and growth rates, correspondingly, will be exaggerated. Thus, as Soviet statisticians used the base prices for 1926–1927, their estimations of growth rates were exaggerated.

Alongside the studying of the Soviet economy growth rates, of great interest for American scholars was the problem of comparability of outputs in the Soviet Union and in Western countries. It was very important for the USA to correctly estimate the potential of the Soviet economy. The research of this problem was carried out by RAND corporation which was engaged in scientific investigations commissioned by government and the Armed forces. Gerschenkron’s contribution to this area of research was a series of books with his calculation of the dollar indices of Soviet industries. His book on machinery output, written with A. Erlich (Gerschenkron and Erlich 1951) was reviewed by A. Nove. Nove appraised this work and noted that “there cannot be many copies in existence, and this is a positive crime against knowledge” (Nove 1953, 423).

In the end of his article on the Soviet indices of industrial production Gerschenkron wrote: “It can only be hoped that the Russians at length will acquiesce in the necessity of abandoning what has become a badly distorted measure of their economic growth,

and will accept whatever loss in prestige and propaganda value may be involved in a recomputation of industrial output series on the basis of a realistic and homogenous price system” (Gerschenkron 1947, 226). However, until now commonly accepted statistics of economic growth in the USSR has not yet been worked out.

## 2.2 *Advantages of Economic Backwardness*

In the 1950s, Gerschenkron started to investigate general issues of economic development. In 1951, in his presentation at the conference “The Progress of Undeveloped Countries” he outlined the basic ideas of his theory of economic backwardness. The text of his presentation was published in the conference proceedings under the title “Economic backwardness in historical perspective” (Gerschenkron 1952a).

The main thesis of his concept is “advantage of backwardness”; i.e., undeveloped countries have an opportunity to use technological achievements of developed countries and, as a result, can overleap certain stages of development which more developed countries have already overcome. In a compressed form, his approach can be described as follows:

1. The more backward a country’s economy, the more likely was its industrialization to start discontinuously as a sudden great spurt proceeding at relatively high rate of growth of manufacturing output.
2. The more backward a country’s economy, the more pronounced was the stress in its industrialization on bigness of both plants and enterprise.
3. The more backward a country’s economy, the greater was the stress upon producers’ goods as against consumers’ goods.
4. The more backward a country’s economy, the heavier was the pressure upon the level of consumption of the population.
5. The more backward a country’s economy, the greater was the part played by special institutional factors designed to increase supply of capital to the nascent industries.
6. The more backward a country’s economy, the less likely was its agriculture to play an active role by offering to the growing industries the advantages of an expanding industrial market based in turn on the rising productivity of agricultural labor (Gerschenkron 1962, 353–354).

In his later works, Gerschenkron added two more factors which help to overcome backwardness:

1. The reliance on technological borrowing and perhaps financial assistance from abroad
2. The virulence of ideologies, under auspices of which the industrialization proceeds (Gerschenkron 1970, 99).

Undoubtedly, Gerschenkron’s theory reflected his personal experience. For a long time, he had been living in Russia and Austria—the countries which were at different

levels of development. Nonetheless, he never claimed that “a great spurt in industrial development” is inevitable—whether it occurred or not depended on specific conditions of a given country. As Gerschenkron put it, “my approach is entirely conditional or contingent.” (Gerschenkron 1977, 52).

Gerschenkron’s concept was an alternative both for a simplified interpretation of the Marxist theory (which distinguishes five social-economic formations: prehistory, slavery, feudalism, capitalism, and communism) as well as for a popular at that time W. Rostow’s theory of stages of economic growth (traditional society, preconditions to take-off, take-off, drive to maturity, age of high mass consumption).

Gerschenkron admitted that “nothing of course is entirely new under the sun and everybody has his predecessors” (Gerschenkron 1968b, 13). He wrote that he was influenced by two outstanding historians—Walther G. Hoffmann and Eli F. Heckscher. He was also influenced by J. Schumpeter who was the first to pay attention to important role of banks in the process of innovation and by P. Rosenstein-Rodan, the author of the “big push” theory. Besides, R. Hilferding’s works known to Gerschenkron could not but have an impact on him.

However, there was one more person who could have influenced Gerschenkron’s views—that was Leon Trotsky. B. Selwin who carried out a comparative analysis of their concepts cites R. Roxborough: “... Trotsky’s work is the Marxist equivalent of Gerschenkron’s theory of the advantages of backwardness” (Roxborough 1979, 24–25).

The first chapter of Trotsky’s “History of Russian Revolution” called “Peculiarities of Russia’s development” clearly formulates the idea of economic backwardness: “An undeveloped country assimilates material and ideological achievements of developed countries. ... An undeveloped country which has to keep up with developed countries doesn’t observe the rules of standing in lines: the privilege of historical backwardness—such a privilege does exist—affords or, more precisely, compels to use what had been developed earlier, long before its time, overleaping a number of intermediate stages” (Trotsky 1997, 34–35). Besides, Trotsky wrote about an important role of banks and foreign capital in the development of Russian industry. The first volume of “History of Russian Revolution” was published in Berlin both in Russian (Trotsky 1931) and in German (Trotzki 1931). Was Gerschenkron acquainted with Trotsky’s works? B. Selwin writes that he asked Gerschenkron’s grandson and biographer Nicholas Dawidoff about it and got the following answer: “Of course my grandfather read Trotsky, but what specifically he read, when he read it and what he thought of it I cannot say” (Selwin 2011, 445). It is hard to believe that Gerschenkron, who always kept an interested eye on publications on Russia, did not read this work by Trotsky.

Anyway, whoever Gerschenkron’s predecessors might be, it is he who should be credited not only with working out a theory of overcoming backwardness but with its application to European countries. This fact justifies P. Gregory’s appraisal of Gerschenkron’s theory: “Gerschenkron’s relative-backwardness hypothesis is perhaps the most widely accepted model of European industrialization and has served as a basis for analyzing the industrialization experience of a whole series of countries, including even the Soviet Union” (Gregory 1974a, b, 654).

Gerschenkron's ideas had a great impact on scientific society. To analyze and evaluate literature devoted to his works, it would be necessary to write a separate book, probably several books. Here, we can only review the main trends in literature on Gerschenkron. Some attempts were made to evaluate his approach as a whole (Barsby 1969), to analyze how his theory was applied to the processes of industrialization of Europe (Sylla 1991), of some countries (Esposito 1992), in different industry branches in Central and Western Europe (Landesmann and Stehrer 2003), at various enterprises (Matheus 2006), in some countries outside Europe (Barsby 1973), in the countries of Latin America (Gootenberg 2001), East Asia (Japan, South Korea, Taiwan, and Singapore). Jung-Sup Shin's interesting work "The East Asian industrialization in the Gerschenkron Mirror" aims to demonstrate that Gerschenkron's theory is of great importance for understanding the processes of industrialization in undeveloped countries in the twentieth century (Shin 2002, 9). Of particular note are the works by Paul Gregory, who tried empirically to test Gerschenkron's conclusions on the Russian industrialization (1974a, 1974b, 1977). It is worth mentioning that Gerschenkron's approach was also applied to the sphere of politics (Acemoglu and Robinson 2006).

One of the promising trends in the discussion of Gerschenkron's theory is the debate on the role of banks in the process of industrialization. His concept implied that in the countries with medium level of backwardness banks played a compensatory role in providing industry with capital. Moreover, banks acquired enterprises' stocks, bankers became members of the Boards of directors and took an active part in management of these enterprises. It was especially typical of the German banks. In Gerschenkron's opinion, as industrialization progressed, the role of banks decreased, enterprises relying more and more on their own resources.

Alongside positive evaluation of Gerschenkron's views on the role of banks (Good 1973), his approach received much criticism. At first, his opponents constructed a model of German banks. Its analysis demonstrated that banks' interference in economy as well as focus on heavy industry can cause inefficient distribution of resources. Then, an expert on German banks of the Kaiser period R. Tilly opposed the idea of a dominant role of banks on the industrial market, which was popular among German scholars. He argued that large steel and electrical conglomerates were relatively independent of their financial partners. Nevertheless, he agreed that banks tended not to pay much attention to the interests of small and medium-sized companies (Tilly 1980).

The authors of the articles included in the book "The Origins of National Financial Systems. Alexander Gerschenkron reconsidered" tested Gerschenkron's ideas on seven European countries (Forsyth 2003). Generally, their attitude was skeptical, which was reflected even in the title of the book. Of larger interest for our purposes is R. Deeg's article "On the Development of Universal Banking in Germany." After having analyzed a large body of scientific literature, he came to conclusion that balance between banks and large firms could be different. Besides, a lot of large firms were independent of banks (Deeg 2003, 88).

The problem of the role of banks in the development of different countries has been widely discussed. One of the first countries to receive attention was Japan which

managed to make a significant push in the beginning of the Meiji era. Japanese economy was often referred to as “banking capitalism,” which serves an evidence for an important role of banks in Japan. Some experts believe that relations between banks and enterprises in Japan developed in correspondence with Gerschenkron’s concept: Originally, the leading role belonged to banks, but as companies started to accumulate their own financial resources, they became more and more independent of the banking system (Ozawa 1999).

An alternative view states that Gerschenkron’s ideas cannot be applied to Japanese *Zaibatsu*—large conglomerates including enterprises and banks. According to Y. Miwa and M. Ramseyer, banks in Japan did not render serious assistance to large industry. They argued that amounts of credits which banks provided to the firms being parts of conglomerates only slightly exceeded deposits placed by companies included in the same conglomerates. *Zaibatsu* were mainly financed at the stock market, credits playing lesser role. The authors concluded that these firms were successful not because they belonged to *Zaibatsu*—on the contrary, they were included in *Zaibatsu* because they were successful (Miwa and Ramseyer 2002, 153, 157).

In authoritative, “The New Palgrave Dictionary of Economics,” McCloskey on the basis of the analysis of Italian industrial growth (Fenoaltea 1987) makes a statement: “... the components of the industrial index—the steel output and the chemical output—are the real units of economic analysis. If the components started accelerating before the new banks appeared, becoming bulky only later, then the new banks could not have been the initiating force. Alas, the components did just this. They spoil Gerschenkron’s bank-led story: The components accelerated not in the 1890s, but in the 1880s, not after but before the banks” (McCloskey, 2186–2187). However, in his later work, Fenoaltea demonstrates that the rates of growth of Italian industry became higher after the new banks appeared in Italy (Fenoaltea 2017).

In fact, the issue of banks is rather complicated. Gerschenkron’s analysis related mostly to Germany. The official site of Deutsche Bank, the largest one in Germany, gives the date when it was established—1870, and some facts about its history. In 1879, the bank started issuing the Krupp company bonds, in 1881 arranged initial public offering of Bayer on the Berlin stock exchange, in 1887 the bank participated in creating AEG, in 1890—in creating Mannesmannröhren-Werke. All these companies have always been the symbols of the German industry. We cannot fully agree with McCloskey’s—it is difficult to deny that in the 1880s Deutsche Bank became a universal bank which greatly assisted both steel and chemical industries.

Besides, Gerschenkron never argued that during the period of industrialization banks were the only initiating force in industry. What he stated was that banks financed companies and provided them with managerial competence. Gerschenkron’s opponents sometimes lost from view the fact that banks are, first of all, intermediators at the credit market as well as at the stock market. In its turn, this implies the following conclusions:

1. Contrasting the credit and the stock markets as sources of capital for industrial spurt does not seem to be correct as banks have always played an important role both in enterprises' financing and in placement of their stocks and bonds.
2. Even Gerschenkron's opponents admit that the German banks played much more significant role in the process of industrialization than the British ones.
3. The researcher who made conclusions about banks' assistance to industry on the basis of long-term credits share in portfolio of enterprises seems to be mistaken. In the nineteenth century, as well as today, short-term crediting is a wide-spread means for financing long-term investments. Gerschenkron himself noted this fact.
4. Gerschenkron was consciously distancing from any judgment of industrialization in Japan. Besides, he never claimed his theory to be applicable to every country. Nevertheless, it is to be noted that the balance between deposits and credits in Zaibatsu is an evidence of the important role of banks as well as of competent management of conglomerates.
5. Credit and interest rate policy have not been seriously studied yet—it is still not clear in what form this policy was carried out toward affiliated companies as well as to large, medium-sized and small clients. Even if affiliated companies did not need credit at a certain moment, they were guaranteed the possibility to get it if necessary. Sometimes, this opportunity was more valuable than obtaining credits.

However, the most significant argument to support Gerschenkron's concept is of "arithmetical" nature. It was necessary to somehow compensate the lack of capital in industrial companies. In case companies faced the deficit of their own capital, they could receive necessary funds from different sources: from banks which would provide them with credits, or from the state which would assist them by means of tax, fiscal or tariff policies.

The opponents of Gerschenkron were correct in pointing out that some large companies, even being in the process of a big spurt, could easily survive without substantial credits and were independent of their partner banks. However, this argument only demonstrates that in social processes no model can be used in its pure form—influenced by different factors, it could always be modified. Gerschenkron never insisted that in some undeveloped countries under industrialization the lack of capital is compensated only by the state meanwhile in others—only by banks.

Nevertheless, it is difficult to deny that some of the aspects of Gerschenkron's concepts could be reconsidered and modernized.

### ***2.3 Russia and Europe***

The problem of relationships between Russia and Europe, Russia and the West is as old as the hills. Even the list of the works devoted to this issue would take many

pages. Russia for Gerschenkron was not only his native land—it was the main object of his research.

Gerschenkron described specific features of the Russian economy:

1. The state, moved by its military interest, assumed the role of the primary agent propelling the economic growth in the country.
2. Economic development proceeded fast whenever military necessities were pressing and subsided as the military pressure relaxed.
3. Whenever a considerable upsurge of economic activities was required, a very formidable burden was placed on the shoulders of population.
4. In order to exact effectively the great sacrifices it required, the government had to subject the reluctant population to a number of severe measures of oppression.
5. Because of the magnitude of the government exaction, a period of rapid development was very likely to give way to prolonged stagnation, because the great effort had been pushed beyond the limits of physical endurance of the population and long periods of economic stagnation were the inevitable consequences (Gerschenkron 1962, 17).

Surely, one can agree with such a description. But does it allow to consider Russia a European country? Theoretically, there could be three answers to this question:

1. Geographically, Europe does not exist.
2. Russia is not part of Europe. It is a specific country with its own history.
3. Russia is part of Europe.

Though the first answer seems to be absurd, it is not totally unreasonable. N. Ya. Danilevsky, a Russian historian, wrote that, from geographical point of view, “there is no Europe, but a Western peninsula of Asia” (Danilevsky 1871, 58). Today, it makes sense to speak not about “Europe,” but about “the West,” including European developed countries as well as the USA, Canada, Japan, and Australia. With such an approach, geographical factor disappears. Nevertheless, Danilevsky argued that from cultural and historical point of view Europe surely exists. As he put it, “Europe is the arena of Germanic and Romanic civilizations.” His unambiguous answer to the question “Does Russia in this sense belong to Europe?” was: “No, it does not, to my regret, or to my pleasure, fortunately or unfortunately” (Danilevsky 1871, 59).

Gerschenkron addressed himself to the topic of relations between Russia and Europe not once. A detailed analysis of this issue was provided in his lectures read in 1969 in Cambridge (England). In 1970, these lectures were published under the title “Europe in the Russian mirror.” For us of principle importance is Gerschenkron’s conclusion that “the Russian experience is seen as an integral part of a general European pattern” (Gerschenkron 1970, 97) and that the history of the Russian industrialization can assist in understanding the history of Europe (Ibid, 130). Thus, it seems that Gerschenkron would choose the third answer—Russia is part of Europe.

The specific features of the Russian industrialization model described by Gerschenkron were also typical to some extent of other European countries. For example, in all European countries the state played an active role. In his different works, Gerschenkron cited a letter from F. Engels to N. Danielson, which reads:



“There can be no doubt that the present sudden growth of modern “grosse Industrie” in Russia has been caused by artificial means, prohibitive duties, state subventions, etc. The same has taken place in France, where the prohibitive system has existed ever since Colbert, in Spain, in Italy and, since 1878, even in Germany. ... And America has done exactly the same” (Engels 2010, 440–441).

In fact, Russian history has much more in common with European history than it is usually believed. Russia had not only economic but tight cultural relationships. All Russian emperors, starting from Alexander I, married German princesses. Russian empress Catherine the Great was of German origin, (born Sophie Auguste Friederike von Anhalt-Zerbst-Dornburg). Once a famous Russian semiotician and cultural historian Yuri Lotman joked: “Napoleon intended to occupy the throne of the Russian Empire ... he did not receive the support he had hoped in; the Russian people expelled aliens because a Frenchman cannot become Russian emperor. Only a German can be a real Russian emperor” (Ardov et al. 1997, 203).

But even if it does not suit a Frenchman to be Russian emperor, a Frenchwoman can become a real Russian; moreover, ironically enough, a Frenchwoman was a personification of Russia. As Gerschenkron wrote in his interesting article “Notes on *Doctor Zhivago*” (341–352): “... Lara is real Russia. She is not westernized Russia as is Tonia, the wedded wife, who is so properly shunted away to France where she belongs” (Gerschenkron 1962, 347). Meanwhile, Lara’s mother was French, and her father was Belgian.

Philologists and literary critics saw Gerschenkron as a representative of their profession. After his article on “*Doctor Zhivago*” had been published, he started reading lectures on Pasternak and was even offered to head the Russian Literature department at Harvard University.

## 2.4 *Time Horizon*

Gerschenkron obtained his main scientific results in the late 1940s–early 1950s. In the 1960s, he completed his concept. By that time, he had been already a world-known scholar and a distinguished researcher of Harvard university. His course of lectures “Economic History of Europe” was included in core curriculum. As H. Rosovsky, one of his former students, put it, “it was virtually the only course in the graduate economics curriculum that directly assaulted the provincialism of most students. They emerged from it more civilized” (Rosovsky 1979, 1009).

He was an influential teacher and a prominent scholar. However, he aspired to move forward and to find a new scientific path. And so, he became the founder of an absolutely new concept—the problem of time horizon. By time horizon Gerschenkron understood “... the distance at which men in making economic decisions are willing and able to look into the future” and hence it “relates to their appraisal of the future as compared with the present” (Gerschenkron 1975, 692). He was planning to implement an ambitious project—to empirically study the changes of time horizon during long periods, at different stages of economic development

of economy as a whole and of different industrial sectors. He aimed to understand how people make decisions which later are to become the basis for their economic behavior.

According to the main Gerschenkron's idea, the more undeveloped is a country, the less people think about future because their behavior is determined by traditional society. In developed countries, economic situation is different—entrepreneurs can and must act on the basis of personal long-term forecasts of future events.

As it was not easy to find data showing real people's attitude to time horizon, Gerschenkron started with studying literary characters. The result of his investigation in this field were two articles—"Time Horizon in Russian Literature" and "Time Horizon in Balzac and Others" devoted to the novels by Balzac, Zola and other European writers. He hoped that «"Western" literature having been created in countries economically so more advanced than Russia might lead to illuminating comparisons» with Russian literature» (Gerschenkron 1978a, 79). By the end of the nineteenth century backwardness of European countries had decreased, which, he thought, might have been reflected in fiction. However, it turned out that Western literature could offer him nothing of this sort. His analysis of Western belles-lettres did not support his idea of close connection between economic backwardness and economic (particularly commercial) dishonesty on which such an emphasis was placed in Russian fiction (Ibid, 90). Moreover, it became evident that capitalism introduced into economic life new elements of dishonesty. So, Gerschenkron's expectations did not come true but he did not provide any clear explanation.

Gerschenkron did not complete his project of the time horizon investigation, and not surprisingly. In fact, his plan was to create a general theory of economic behavior. However, even for a big team of prominent scholars, such a task was rather ambitious, and Gerschenkron was a single elderly researcher. But even the formulation of this problem was his important achievement.

The last significant project accomplished by Gerschenkron was his research on the history of Austria, which had been conceived in the early 1960s. In April 1973, he delivered two lectures on this topic at Princeton University. These lectures provided the basis for his new book—"An economic spurt that failed: four lectures in Austrian history." Materials obtained in the archives of the ministry of finance underlay the third chapter that was also called "a lecture." The fourth chapter contained retrospective analysis of Austria's large-scale industrialization—an attempt made by E. Koerber, Cisleithanien prime minister, between 1900 and 1904.

Koerber was planning to create a network of railroads and to build canals. Implementation of these infrastructural projects might have brought Austrian economy to a new stage of development. However, this big spurt of industrialization failed, one of the main reasons being the resistance of bureaucrats from the government. Narrow-minded officials from the ministry of finance saw their key task in preserving stable rate of exchange but not in developing Austrian economy—a factor which would provide economic growth of the country (Gerschenkron 1977, 128).

At that time, Gerschenkron took some steps to publish his 3rd collection of essays at Princeton University but was rejected. The editors thought he should have written

a “big book” and criticized it as “hors d’oeuvres disguised as an entrée” (Dawidoff 2002, 325).

Gerschenkron really was a well-read erudite and a knowledgeable scholar. In one of his last articles, he wrote that during the time when he lived in the USA he used to read up to 100 books per year not counting many journal articles or parts of books (Gerschenkron 1978b, 329). He was reading a lot, he was writing reviews to other scholars’ books, and he simply did not have enough time to complete his own research. Still he was planning to write “a big book” on time horizon. However, fate intended otherwise.

He was dissatisfied with the situation at Harvard. After 1973, his course of lectures on economic history of Europe was no longer compulsory. Besides, the Ford Foundation cut off its funding for the Economic History Workshop, putting it out of business (Dawidoff 2002, 325). He was increasingly bad at terms with other people and felt himself more and more lonely. His health deteriorated. In June 1975, he stopped teaching. In October 1978, he died. As his former student Rosovsky said, “one of the last cultivated European intellectuals, a great scholarly model” passed away (see Crimson 1978).

### 3 Conclusion

Gerschenkron did not live to see Gorbachev coming to power and the beginning of perestroika in Russia—he had died less than 7 years before. Undoubtedly, he would have greeted these events. It was long before, in 1968, that he wrote; “... a gradual and peaceful transformation of modern dictatorship would be a historical *novum*, as has been its continual survival for five decades” (Gerschenkron 1968a, 5).

In such a country as Russia, the state has played and will always play the leading role. If “the great spurt failed to materialize, all our approach can do is to attribute that failure to the inability or unwillingness of the government to discover and apply the appropriate pattern of substitution” (Gerschenkron 1962, 362). Efficient vertical power structure, working not for self-preservation but for development, and the attitude of the state and of society toward business—these are the key factors which can compensate the lack of prerequisites for modernization.

A prominent nineteenth century Russian historian V.O. Kliuchevsky once noted, that “history is not a teacher but a wardress, *magistra vitae*: It does not teach us anything—only punishes when we are not ready with our homework” (Kliuchevsky 2009, 347). Yes, that is true: history never teaches us anything. However, we can learn a lot from history and Gerschenkron’s works can assist us in this endeavor.

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# Wassily Leontief and His German Period



Harald Hagemann

## 1 Introduction

The paper focuses on Wassily Leontief's life and work in Germany. The first section contains an overview of Leontief's German biography. Thereafter, three topics are discussed in greater detail. The first one is his Berlin Ph.D. thesis "The economy as a circular flow." The second section covers the employment consequences of new technologies, which is a topic Leontief came to very late in his life. He devoted most attention to it in the 1980s, but it clearly links to the German period, when he worked in Kiel. The third topic is the statistical analysis of supply and demand curves which had been the main focus in Leontief's work at the Kiel Institute, but which also marks his traverse to the USA, because it was the famous "pitfalls" controversy with Ragnar Frisch. It continued after Leontief moved to the USA and became a researcher at the National Bureau of Economic Research NBER and shortly after a professor at Harvard University.

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<sup>1</sup> See the special theme in honor of Leontief's 90th birthday edited by Duchin, *Structural Change and Economic Dynamics*, 6(3), August 1995.

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## 2 Leontief's "German" Biography

In 1994–1995, I was invited to contribute to a *Festschrift* in honor of Leontief's 90th birthday<sup>1</sup> by Faye Duchin who was running the *Institute for Economic Analysis* at New York University which Leontief had founded after his retirement from Harvard in 1975. Duchin is one of three outstanding women economists who came out of Leontief's group besides Anne Carter, the founding President of the International Input–Output Association (1987–1991), and Karen Polenske, and she was one of the managing editors of the journal *Structural Change and Economics Dynamics* (SCED). When I asked Faye in Summer of 1995: "When is the deadline for the submission of the paper?" she told me: "You are too late now; Wassily recently has been in Saint Petersburg and found out that in fact he was already born in 1905 and not in 1906." However, Leontief was not born in St. Petersburg, but in Munich, i.e., his very first and short German period began with his birth. The certificate of his birth is now placed on the website of the International Input–Output Association,<sup>2</sup> and this document dates from 2005 from the city of Munich (see Appendix 1). It was given to Leontief's daughter Svetlana Alpers, after the mother Estelle née Marks (1908–2005), whom Leontief had married in 1932, had died. The document shows the 5th of August 1905 in Munich as the date and place of birth.<sup>3</sup> This is quite rational, because his father Wassily Leontief senior (1880–1966) had been a Ph.D. student at the University of Munich at that time. He had met his wife Slatá (Evgenia) Becker (1881–1979), who was born in a Jewish family from Odessa, in Paris one year before, and had married her in London on March 17, 1905. The following year the father, Wassily Leontief senior, got his Ph.D. from the University of Munich. After earning his first degree in St. Petersburg, the father had already studied in Germany before and got his diploma from the High School of Commerce in Leipzig in 1901. Thereafter, he studied for one year in Paris before he became a doctoral student in Munich in 1902.

Wassily Leontief junior was almost one year old when his father finished his Ph.D. thesis on "The cotton industry in St. Petersburg and its workers," and shortly thereafter, the family moved from Munich to St. Petersburg, where the parents registered the birth of their son a second time with the Orthodox Church, exactly one year after his birth in Munich.<sup>4</sup>

The young Wassily therefore was already 16 years old, not 15 years, when he started to study at the University of Petrograd in 1921. Directly after finishing his

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<sup>2</sup> <https://www.iioa.org>.

<sup>3</sup> In April 2020 on the website of The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel it still reads that Leontief was born in St. Petersburg on 5 August 1906. This error is even engraved on Leontief's tombstone in Connecticut (see Bjerkholt and Kurz, 2006, p. 332). On the history of the Leontief family, see Leontief (1987), Kaliadina and Pavlova (2006), and Kaliadina (2006).

<sup>4</sup> His mother had converted to the Orthodox faith three days before and received the name Evgenia at her baptism. The Leontief family remained faithful to the Old Believers. The reservations of the grandmother towards a Jewish daughter-in-law probably contributed to the confusion regarding Leontief's birthdate.



studies at the University of Leningrad (the city was renamed shortly after Lenin's death in January 1924) in April 1925, with the beginning of the summer semester, Leontief moved to the University of Berlin to study there, mainly with Werner Sombart (1863–1941) and Ladislaus von Bortkiewicz (1868–1931) who later became the two referees of his Berlin Ph.D. thesis.

Leontief submitted his dissertation, which he had finished in Kiel to the University of Berlin already on December 9th, 1927<sup>5</sup>, but it took about one year for the handing out of the final document, although the oral exam<sup>6</sup> had already taken place on March 1st, 1928, due to several obligations, because the Russian degree was not fully accepted and because of additional formal administrative problems which delayed the finalizing of the Ph.D. exam.<sup>7</sup>

The Ph.D. thesis was also printed as a book, but it was mainly published as an article in the journal *Archiv für Sozialwissenschaft und Sozialpolitik* (ASS), which was the only journal in economics and the social sciences in Germany, which had to stop publication after the Nazis came to power in 1933 (Hagemann, 1991). At that time, the journal was edited by Emil Lederer in connection with Joseph Schumpeter and Alfred Weber, the younger brother of Max Weber. It had been a leading journal for 30 years. In 1926, the first article by Nikolai Kondratiev on long waves was published in that journal which made Kondratiev known to the Western world (Kondratieff, 1926). Schumpeter later initiated the first abridged English translation by his Ph.D. student Wolfgang Stolper (Kondratieff, 1935).

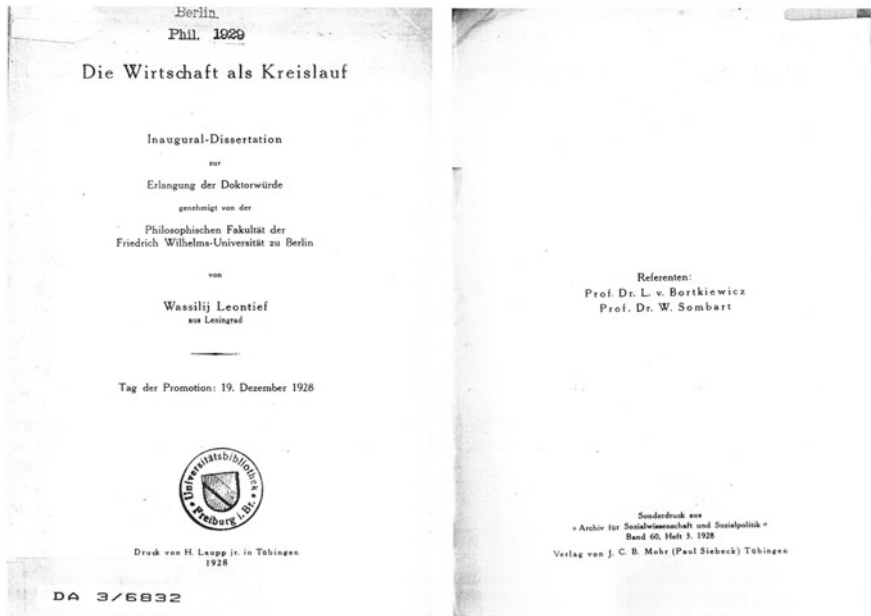
On the front page of the special offprint of his ASS article, of which Leontief had to submit 150 copies to the Philosophical Faculty of the Friedrich-Wilhelms-University in Berlin<sup>8</sup> as an official document of his thesis “Die Wirtschaft als Kreislauf” [The economy as a circular flow], it can be seen that the finalizing of the Ph.D. was dated the 19th of December 1928 (Fig. 1). On the right side, the names of the two referees are listed. Originally, Leontief was Sombart's Ph.D. student, but Sombart did not understand the mathematics of the dissertation well, so Bortkiewicz became

<sup>5</sup> In his letter to the Dean of the Philosophical Faculty Leontief opted for Economics as the main field for the oral exam, Bortkiewicz and Sombart as the two referees, and for Philosophy and History as subsidiary subjects.

<sup>6</sup> The overall result of the oral exam which was chaired by Bortkiewicz was cum laude. The examiners (in this order) were Professor Breysig in History (summa cum laude), Kähler in Philosophy (cum laude—magna cum laude), Sombart (rite) and Bortkiewicz (cum laude) in Economics. Bortkiewicz concludes the minutes with the statement that, similar to his colleague Sombart, “he has gained the impression that the positive knowledge of the candidate is not fully on a level with his perceptive capability and talent”.

<sup>7</sup> In 1924–1925 the activity of the Faculty of Social Sciences at the University of Leningrad was gradually curtailed. The prolonged period of closure contributed to the bureaucratic difficulties Leontief had to face in Berlin because his Russian diploma from 1924 was only a first degree (equivalent to B.A.). On the claims of the Faculty in 1924–1925, on the opening of a new Faculty of Economics at Leningrad State University in 1940 and the fate of this faculty in late Stalinism see Melnik (2018).

<sup>8</sup> On the initiative of Michael Burda the Economics Faculty of the Humboldt University presented an honorary renewal of his doctoral degree to Leontief on April 18th, 1995. See Burda (1995).



**Fig. 1** Die Wirtschaft als Kreislauf (The economy as a circular flow). Publication of Leontief's 1928 Ph.D. thesis

the first referee who wrote the main detailed report already on January 10th, 1928, approvingly signed by Sombart two days later.

In the CV Leontief had submitted to the University of Berlin when applying for the Ph.D., he explicitly says in the first line that he was born in St. Petersburg on August 5th, 1906. There he also mentions as his main teachers in St. Petersburg Iossif Kulischer,<sup>9</sup> Sergei Platonov,<sup>10</sup> Sergei Solntsev<sup>11</sup> and Evgeny Tarle.<sup>12</sup> Kaliadina

<sup>9</sup> Iossif Michailowitch Kulischer (1878–1934) was one of the leading economic historians in Russia in the 1910s and 1920s. He had close connections with the German Historical school. See Bjerkholt (2016, p. 23) for Kulischer's recommendation letter for Leontief to Sombart dated March 20th, 1925.

<sup>10</sup> Sergei Fyodorovich Platonov (1860–1933) was a leading Russian historian who led the St. Petersburg school of imperial historiography before and after the Russian Revolution. The beginning of the Stalinization of Soviet academia was marked by the "Platonov affair." In one of the first show trials Platonov was accused of taking part in a Royalist conspiracy, purged, imprisoned and exiled to Samara where he died.

<sup>11</sup> Sergei Ivanovich Solntsev (1872–1936) was among the few pre-revolutionary professors who remained active after 1917 and became influential in the history of Russian economic thought in that period. Like Kulischer he belonged to the prerevolutionary group of economists who pursued a midway approach between Marxism and the Historical School. Solntsev had been well known to Bortkiewicz whose lectures at the University of Berlin he had attended twenty years before Leontief.

<sup>12</sup> Evgeny Viktorovich Tarle (1874–1955) was an historian who had become famous for his works on Napoleon's invasion of Russia and on the Crimean war. In the course of the Platonov affair he was arrested and exiled to Almaty. In the late 1930s he was rehabilitated and reemerged as a kind of court historian to Stalin.

(2006) mentions a report Leontief delivered on the “Analysis of the formulas of social reproduction,” written under the guidance of Professor Solntsev. However, there can hardly be any doubt that the greatest influence was exerted by his own father who himself was teaching at St. Petersburg University since 1915.<sup>13</sup>

In 1928, Berlin University was still called Friedrich-Wilhelms-University after the former Prussian king. The University was renamed as Humboldt University in 1949. Leontief says that he started to study in Berlin in the summer semester 1925 which in Germany starts on the first of April. His main teachers in Berlin were Sombart, Bortkiewicz, and Kurt Breysig (1866–1940), an historian.

Georg Erber, from the German Institute for Economic Research (DIW) in Berlin, and a member of the editorial board of SCED, was able to get the official document of Leontief’s doctoral degree (see Appendix 2) after the fall of the Berlin Wall. It was impossible for West Germans to get these documents before this. Leontief was very happy when the documents were handed over to him because he had lost them in the meantime. Almost everything is written in Latin as it was standard practice at that time including the name of the University of Berlin. It was the Ph.D. from the Philosophical faculty. In the German language area, there were two different approaches. In Austria, economics traditionally had been part of the law faculty, and in Prussia, economics had been part of the philosophical faculty.

Two issues might be interesting: the title of the Ph.D., which is the only text in German—“Die Wirtschaft als Kreislauf” [The economy as a circular flow]. The other topic is the degree he got: *cum laude*.

This is only the third grade out of four possibilities. Normally, today if you would make your Ph.D. with the degree *cum laude*, you cannot become a professor. It is the Latin system where the best degree is *summa cum laude*, which is excellent. The second grade would be *magna cum laude*—very good. The third is *cum laude*. And, the fourth would be *rite*—which means “just passed.”

So he did his Ph.D. in December 1928 in Berlin, but since May 1927, Leontief was already working at the Kiel Institute of World Economics, succeeding Max Schönwaldt in the department of statistical international economics and international business cycle research.<sup>14</sup> The founding director of the Kiel Institute in 1914 had been Bernhard Harms (1876–1939), who also was kicked out of office after the Nazis’ rise to power in 1933.

Adolf Löwe (since September 1939 Adolph Lowe), the director of the department for business cycles founded in April 1926,<sup>15</sup> which was the new department where theoretical work was done, hired Leontief. According to personal conversations I

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<sup>13</sup> For a much more detailed description of Leontief’s early years in St. Petersburg and his travels to Berlin see Bjerkholt (2016).

<sup>14</sup> See Beckmann (2000, pp. 76–77). The great grandnephew of Georg Friedrich Knapp, Claus Wittich, who deserves great appreciation for clarifying Leontief’s early period and the St. Petersburg heritage, later systematically put together the documents of Leontief’s Berlin Ph.D. (Wittich, 2006).

<sup>15</sup> On the excellent research done in this department in the years 1926–1933 see also Hagemann (2020).



**Fig. 2** Building of the Kiel institute of world economics 1920–1943. *Source* Institut für Weltwirtschaft (1964, p. 34)

had with Lowe in the 1980s, he got a phone call from Sombart in Berlin,<sup>16</sup> who said “I have here a young genius from Russia, are you interested?” Lowe responded “I am always interested in geniuses.”

In a letter which was sent by Leontief on February 8, 1993, he congratulated Adolph Lowe on his 100th birthday (see Appendix 3). Lowe, who came back to Germany in 1983 fifty years after emigration, was living with his daughter Hannah in Wolfenbüttel at that time and died two years later at the age of 102. Two aspects are worth noting: Leontief mentions Gerhard Colm (1897–1968) and Hans Neisser (1895–1975), two other excellent economists, who later emigrated to the USA. The other interesting point is that Leontief is referring to “Herr Geheimrat Professor Dr. Harms” who was the President of the institute in that time and one of Leontief’s favorite activities in Kiel—namely sailing.

The photograph on Fig. 2 shows the *Institute of World Economics* as it was looking during Leontief’s time until the year 1943, when many parts of the building were destroyed in the bombing of Kiel. Kiel, like Saint Petersburg, is located on the Baltic Sea and is also the only place in Germany where the Olympic Games took place twice, but only the sailing events, in 1972, when the Olympic Games were held in Munich and in 1936, when they took place in Berlin. In 1936, the Olympic harbor with the sailing boats was located directly in front of the Kiel institute where Leontief

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<sup>16</sup> Lowe had been a regular participant in Sombart’s research seminar during his Berlin years, when he was working at the Ministry of Economics and the Statistical Office before his move to Kiel.

had his working place some years before. On the right side, one can see the only part of the building which survived the bombing.

In a long interview with the scientific journalist of the *New York Times* Silk (1976, p. 156), Leontief explained that in fall 1928, a group of Chinese were passing by a coffeehouse near the institute during the lunch break, and shortly afterward, the Chinese ambassador in Berlin recruited Leontief as an advisor to the Chinese Ministry of Railroads in Nanjing with a one-year contract.

Altogether, Leontief worked in Kiel from spring 1927 until April 1931, with the interruption of the year which he spent in China from April 1929 to March 1930. Thereafter, Leontief moved to New York and became a research associate of Wesley Mitchell at the National Bureau of Economic Research, which had been founded in 1920 and was located in New York until the end of World War II as long as Mitchell was the founding director and also professor at Columbia University. In 1945, the National Bureau of Economic Research was shifted from New York to Cambridge, Massachusetts, close to Harvard and MIT. Lowe had good contacts with Mitchell, because they both were working on business cycles during that time. The closest research associate of Mitchell at that time was another Russian, Simon Kuznets, who later received the Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel in 1971, two years before Leontief.<sup>17</sup>

With the beginning of the new academic year in September 1932, Leontief moved from New York to Harvard, where he stayed until his retirement in 1975. Then, he moved back to Manhattan, where he founded the Institute for Economic Analysis at New York University, where Duchin was the acting director from 1985 to 1996.

Some more details should be given on Leontief's father, Wassily senior. There exists a lot of confusion in the literature. Sometimes, father and son are mixed up with each other because their publications in Germany were both signed as Wassily Leontief.<sup>18</sup> Only the first two articles by Wassily Leontief junior on "The balance of the economy of the Soviet Union" (Leontief, 1925) and "On the theory and statistics of concentration" (Leontief, 1927) are signed as Wassily Leontief junior. Thereafter, it is always Wassily Leontief, but his father was continuing to publish in German journals at that time, and preferably in the *Weltwirtschaftliches Archiv*, the journal of the Kiel Institute of World Economics where his son had worked from 1927–1931.<sup>19</sup>

The original German title of the Ph.D. thesis of Leontief senior at the University of Munich is "Die Baumwollindustrie in St. Petersburg und ihre Arbeiter" [The

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<sup>17</sup> Leontief's review of Kuznets' "Seasonal variations in industry and trade" (1933) is his last publication in German in the *Weltwirtschaftliches Archiv* (Leontief, 1934d).

<sup>18</sup> For example, Tooze (2001, p. 201), erroneously ascribes the article of the father (Leontief sen., 1931) to the son.

<sup>19</sup> See, for example, his articles on the Russian economy, all signed as Wassily Leontief sen. (1931, 1934), his reflections on the views of Americans and French on Soviet Russia (1936), or his review of Chamberlin's book on the Russian revolution (Leontief sen., 1937). When I was visiting the Leontief Centre in St. Petersburg in 2010, we moved to the old place on the Krestovsky Island, where the grandfather had the cotton factory, which was socialized after the October Revolution and does not exist anymore.

cotton industry in St. Petersburg and its workers]. The main referee and supervisor of the thesis was Lujó Brentano (1844–1931), who was one of the best-known German professors at that time. Among the members of the German Historical School Brentano was the strongest supporter of trade unions, which he considered to be the decisive means to solve the labor question. Brentano had also been in closer contact with Alfred Marshall over more than three decades, in particular on the social question and the labor movement. He was instrumental in publishing a German edition of Marshall's "Principles of Economics", to which he wrote a preface and which was published in 1905 shortly before Leontief sen. finished his thesis. Leontief's father got better marks for his Ph.D. than his son two decades later. He got the best grade *summa cum laude* for the written thesis and *magna cum laude*, the second best grade, for his oral defense.

The family went back to St. Petersburg directly after Wassily sen. got his Ph.D. on July 17, 1906. Later, the father became private docent at the Imperial University of Jurjev (Dorpat), today's Tartu in Estonia. He started in Estonia, before in 1915, he got a professorship for labor economics in St. Petersburg. Leontief junior came back to Germany in 1925, and his parents followed him two years later in 1927. The father was working in the Russian Embassy in Berlin as the representative of the Soviet Ministry of Finance from 1927 to the early 1930s. The father got an order from Moscow to come back, but he refused. So, he was living with his wife in Berlin but not working in the Russian Embassy anymore. From 1930 till 1939, he was Lector on the Russian economy at the University of Berlin. Shortly after the outbreak of World War II, in November 1939, Leontief junior managed to bring his parents<sup>20</sup> over to the USA (via Italy), which was certainly not easy at that time.

Leontief junior was an outstanding example of a larger group of Russian economists, most of them Mensheviks and well trained in mathematics and statistics, who emigrated from the Soviet Union and came over to Germany in the years of the Weimar Republic. There were many others, for example, Boris Brutzkus, an agricultural economist. Several of them later became well known internationally.<sup>21</sup> The most important one is Jacob Marschak (1898–1977), who was the very first one who came to Germany in January 1919, exactly on the day when Rosa Luxemburg and Karl Liebknecht were murdered in Berlin. Another important one was Vladimir Voitinsky (Woytinsky) (1885–1960). In 1918–1919, he was imprisoned in the Peter and Paul Fortress for three months. He was a former leading economic socialist, but he was more a Menshevik than a Bolshevik. Voitinsky became chief advisor of the German trade unions on economic issues in the Weimar Republic. And like all the others, he had to emigrate a second time in 1933 after the Nazis' rise to power.<sup>22</sup> There were two centers of gravitation in Germany for all the emigré Russian Menshevik economists. One was Berlin, and the other place was Heidelberg. The key figure in

<sup>20</sup> See Estelle Leontief's detailed memoir (Leontief, 1987) on her parents-in-law.

<sup>21</sup> Others were Naum Jasny, Nathan Leites, Mark Mitnitzky, Paul A. Baran, who did his Ph.D. with Lederer in Berlin in 1932, and later became famous in the 1968 student movement with his book "Monopoly capital," co-authored with Sweezy (1966).

<sup>22</sup> See also his fascinating autobiography: Woytinsky (1961).

Berlin who attracted the younger Russian mathematical economists was Bortkiewicz, himself born in St. Petersburg. Heidelberg had a long liberal tradition particularly in economics and the social sciences where Max Weber was once a towering figure, and it is located a little bit north of Baden-Baden, which was a fashionable place for many Russians in the nineteenth century (and has become again after 1990). Dostoevsky, for example, made his observations in the casino in Baden-Baden.

Marschak got his Ph.D. and his habilitation from the University of Heidelberg. But, he also worked about two years at the Kiel Institute from 1928–1930, where he was directing the section on trade statistics. For more than a year, Leontief was his colleague there. Leontief's very first paper in economics on "The Balance of the Russian Economy" was published in German in the *Weltwirtschaftliches Archiv*, which still exists today and is the economic journal of the Kiel Institute. The Kiel Institute gave a prestigious prize in economics, the Bernhard Harms prize, to Leontief in 1970.<sup>23</sup> I was a student at that time at the University and could follow the lecture which was given by Leontief in a very good German with a strong Russian accent. Major colleagues of Leontief in Kiel were Lowe, Colm, and Neisser. Colm, who chaired the department from 1930–1933, became the chief architect of the German currency reform in June 1948 and was the first to receive the biannual Harms Prize in 1964 (succeeded by Roy Harrod in 1966). All these economists were forced to emigrate after the Nazis' rise to power. This included also some others, of whom two worked on topics with a strong connection to Leontief. One is Fritz (later Frank) Burchardt (1902–1958) who emigrated to the UK in 1935, where in 1949, he became director of the Oxford Institute of Statistics founded by Jacob Marschak in 1935. Two of Burchardt's works, namely "The schemes of the stationary circular flow in Böhm-Bawerk and Marx" (Burchardt, 1931–1932) and "Quesnay's *Tableau Économique* as a foundation for business-cycle theory" (Burchardt, 1933), have a certain connection with Leontief's work. Burchardt's ground-breaking comparison of the two most important methods of modeling the production system and his innovative synthesis of the Austrian or vertical approach and the interindustry or horizontal approach has given inspiration to Ragnar Nurkse's essay "The schematic representation of production" (Nurkse, 1935) as well as to Leontief's essay "The significance of Marxian economics for present day economic theory" in which Leontief (1938) discusses the relative merits of Marx and Böhm-Bawerk's contributions to linear analysis.<sup>24</sup>

The Ph.D. thesis by Alfred Köhler (1900–1981), "The theory of labor displacement by machinery," basically the machinery problem as it was called by

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<sup>23</sup> Leontief's Harms lecture "Structural Approach to the Analysis of International Economic Interdependences" was held on June 10, 1970 in the Kiel castle and published with the Laudatio by Herbert Giersch, director of the Kiel Institute from 1969–1989 (Leontief, 1971). Giersch is also the "unknown" German (no. 15) standing next to Leontief on the photo of the August 1948 seminar in Salzburg where he became a close friend of Robert and Barbara Solow (see Foley, 1998, p. 123).

<sup>24</sup> Burchardt's influence on Leontief was also noted by Clark (1984, pp. 424–425) who would have been more outspoken if he knew of Burchardt's habilitation thesis which was already accepted by Goethe University in Frankfurt. However, due to the Nazis' rise to power the habilitation process was not finalized.

Ricardo, covers a topic, on which Leontief worked in the 1980s. Kähler had already an advanced embryo of a static input–output model in his Ph.D. thesis.

### 3 The Economy as a Circular Flow

Leontief’s first article is on the balance of the economy of the Soviet Union soon after a committee of twenty economists under the direction of Pavel Popov, the chairman of the Soviet Statistical Administration, had published their preliminary results for 1923/1924. It was written immediately after his arrival as a student in Berlin and published in the same year in the German original and shortly afterward also in Russian. In 1964, an English translation, “The balance of the economy of the USSR,” was published in a larger project which originally had been initiated by another famous economist of Russian origin, Evsey Domar, who was professor at the MIT since 1958. It is included in a collection of essays on the *Foundations of Soviet Strategy for Economic Growth* edited by Spulber (1964). Many of the articles by Grigory Feldman, Popov (who was dismissed by Stalin soon after the final “Balance of the national economy” was published in 1926),<sup>25</sup> and other important Russian works of the 1920s were translated into English here for the first time.

In this very first paper by Leontief, which is published in his country of birth, Germany, two points are important. First, his emphasis that a country which favors a planned economy has a high need for detailed statistical information. The second point is that already on the very first page of his very first article he made explicit reference to Quesnay’s *Tableau Économique*.

Leontief finalized his Berlin Ph.D. thesis when he was working as a research associate in Kiel. In 1991, an abridged English translation of his Berlin Ph.D. thesis of “The Economy as a circular flow,” was published in *Structural Change and Economic Dynamics*. Paul Samuelson wrote an introduction on the importance of that work. In his assessment, he refers to the famous composer Richard Wagner and his *Ring*, commenting that Leontief’s Ph.D. thesis “sounds the first note of the overture to his *Ring of Input–Output*” (Samuelson, 1991, p. 177).<sup>26</sup>

The content of Leontief’s Ph.D. thesis fitted very well into the research program of the Kiel group. This work matched with the major research interest of the Kiel group to construct a theoretical model of cyclical growth, with the basic working hypothesis that a satisfactory explanation of industrial fluctuations must fit into the general framework of an economic theory of the circular flow as it was developed by Quesnay and Marx.

Leontief, who was still alive at that time, was aggrieved that the publishing house Elsevier, known as the “Journal Industrial Complex,” which is very capitalist, did

<sup>25</sup> For greater details see Spulber and Dadkhah (1975).

<sup>26</sup> Samuelson also regretted that Leontief and Piero Sraffa never cited the other’s work. “The tub of genius stands on its own bottom” (Ibid). For a more detailed analysis of the interaction between Leontief and Sraffa see Parys (2016).



not give much money for the translation. For financial reasons, a short reduction in the translation had to be made. The first 10 pages were cut down to 2. That was not a great problem or loss, because the introductory part was basically written to please his supervisor Sombart, a descendant of the German historical school. The more interesting stuff starts thereafter. But, four points should be emphasized which show that there is no full anticipation of his later work. For example, in contrast to a statement in his 1925 article, the dissertation contains nothing of manageable empirical measurement. You will not find matrices. The thesis is primarily taxonomic and “topological.” Furthermore, there is no reference to either Quesnay or Marx.

According to Leontief, economic concepts should be observable and measurable. Otherwise, they would be meaningless and become potentially misleading. He considers the circular flow as fundamental and objective fact of economic life. Therefore, it should be placed at the center of economic analysis. The concept of the circular flow is considered as a tool to identify important interconnections and causal relationships existing in the economy. For the construction of an economic system comprising the interconnections between economic processes, a careful and thorough inquiry of the technological aspects is a necessary precondition. In Leontief’s approach, “[t]he two basic concepts are cost and returns. *Cost items* (inputs) are those elements whose consumption in production causes the generation of corresponding return items (output)” (Leontief, 1928, 1991, p. 181). Leontief elaborates in his dissertation a two-sectoral static input–output system with constant technical coefficients to give a clear picture of the production, distribution, and consumption side of the economy. The adjustment problems resulting from new combinations or changes in technical coefficients are indicated. He assumes constant returns to scale.<sup>27</sup>

Leontief insisted that before the beginning of the English translation of his thesis, a short passage of the statement by the referee—Bortkiewicz, should be included. The original documents from the University of Berlin include this letter by Bortkiewicz, which he had sent to the Dean of the Faculty already on the 11th of January 1928 together with his report on the thesis. Bortkiewicz was the one who was basically responsible for Leontief only getting *cum laude* for his thesis. So, he states:

Although I find much that is objectionable in it, this dissertation is without any doubt acceptable. In developing his—in my opinion very doubtful—theoretical constructs the candidate received no guidance whatsoever from his academic teachers. He arrived at his present position quite independently, one might say, despite them. It is very likely that he will maintain this scientific point of view also in the future.

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<sup>27</sup> For a more detailed analysis and integration of Leontief’s dissertation and his subsequent early work on input–output analysis in the USA see Kurz and Salvadori (2000).

## 4 Technical Progress and Unemployment

Technical progress and unemployment was the key topic of Leontief in the 1980s with the main study being “The future impact of automation on workers” (Leontief & Duchin, 1986), growing out of research done in the Institute for Economic Analysis in New York. In this work, predictions for the US economy about the employment consequences, particularly of industrial robots, until 2000 were made. The main novelty in this study, which comprises 89 sectors and 53 different occupations, consisted in the treatment of investment demand, making use of some version of the capital stock adjustment principle. The endogenization of private investment demand is a decisive advantage of this study which thus takes into consideration the machinery production argument associated with the diffusion of new technologies. It is also interesting for input–output specialists, because a special dynamic input–output model had been developed for this project (Duchin & Szyld, 1985). Similar work on the consequences of new technologies on employment was done in Germany in the late 1980s and early 1990s. However, there are two major differences. You cannot treat Germany, which is a very open economy, in the way they did in the US model, as a closed economy. The second modification consists of the fact that the elaborated model also allowed for shrinking sectors, and this is quite difficult to handle mathematically with negative growth rates of some sectors of the economy. Leontief in those years wrote many articles, for example, his major article “The distribution of work and income” in the *Scientific American* (Leontief, 1982), in which he argued against drastic general wage cuts as well as against the erection of Luddite barriers as palliative remedies against technological unemployment.<sup>28</sup> In the following year, he was emphasizing the fact that “the specter of technological unemployment [...] is here again. But there are good reasons to believe that this time it will not retreat” (Leontief, 1983, p. 405).

Concerning the employment consequences of new technologies, there is a strong parallel to the work which was done by the Kiel group, when Leontief worked there. The analysis of cyclical growth and the relationship between capital accumulation, technical progress, and employment was a key research topic in the department. Neisser, who was Vice Chairman from March 1930 to April 1933, in a famous paper (Neisser, 1932) not only made a seminal contribution to general equilibrium theory but also inspected Wicksell’s critique of Ricardo’s analysis of the machinery problem. In his essay, Neisser developed Ricardo’s argument that the demand for labor will continue to increase with an increase of capital only. In a subsequent paper, published a decade later in *The American Economic Review*, Neisser (1942, p. 70) stated clearly: “It never has been doubted by any theorist of rank that accumulation of capital in the form of fixed equipment raises the demand for labor.” Overcoming the bottleneck of capital formation is a necessary, but not a sufficient condition for a successful reabsorption of workers who have been displaced by the introduction of

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<sup>28</sup> See Leontief (1982/1986, pp. 369–370).

new machinery into the production process.<sup>29</sup> In his essay, Neisser (1942, p. 70) also coined the metaphor of “the capitalistic process as a race between displacement of labor through technological progress and reabsorption of labor through accumulation” whose outcome “is impossible to predict [...] on purely theoretical grounds.” His conclusion is clear. There is no adjustment mechanism which would guarantee a successful compensation process or even the maintenance of full employment over time when dynamic forces such as technological change are at work. The outcome of the race is open, and it may differ with changing times and between various countries.

Leontief himself did not work on the employment consequences of technical progress in his time at the Kiel Institute. An important work in that context is the Ph.D. thesis by Kähler “Die Theorie der Arbeiterfreisetzung durch die Maschine” [The theory of labor displacement by machinery], which was published as a book in 1933 (Kähler, 1933), but the thesis was already accepted in 1932.<sup>30</sup> Kähler did not refer to Leontief’s dissertation “The Economy as a Circular Flow,” but he drew on Burchardt’s essays on the schemes of the stationary circular flow in Böhm-Bawerk and Marx (Burchardt, 1931–1932). Burchardt, however, had not discussed the machinery problem more intensively.

Kähler was not a member of the research team; he was an external Ph.D. student of Lowe. So, it is not very clear how well Kähler and Leontief knew each other. But in Kähler’s work, you can find an input–output model. What Kähler used in his dissertation to estimate the employment consequences of new technologies within his “total circulation scheme” in today’s language, we would call a static closed input–output model. Table 1 shows Kähler’s initial input–output scheme for an economy in a stationary equilibrium (zero profits, no investment) before the introduction of technical progress.

Kähler’s multisectoral model comprises nine sectors, but two are linearly dependent, so in fact it is an 8-sector model.<sup>31</sup> Kähler’s attempt to cover the employment effects of new technologies by means of a sequence of static input–output tables in a transition process from an old to a new technique is an important advancement in the direction of dynamic input–output models developed in the 1980s to calculate the impact of the rise of microelectronics and industrial robots on overall employment. Leontief, however, never referred to Kähler’s work which in the view of Lowe (1959, p. 64) contains “the first attempt at input–output analysis, applied not only to stationary equilibrium but also to the intersectoral shifts required for capital formation.”

As Tooze (2001) has pointed out in his ground-breaking study on the development of national accounts in Germany, official estimates of national income by the Statistical Office and by the newly founded Berlin Institute for Research on Business

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<sup>29</sup> For a more detailed assessment of Neisser’s contributions to the analysis of the problem of technological unemployment, see Hagemann (2008, pp. 356–360).

<sup>30</sup> Kähler emigrated in 1934 to the USA where he became Professor at the University in Exile of the New School for Social Research in New York and continued to publish on technological unemployment (Kähler, 1935).

<sup>31</sup> For greater details on Kähler see Gehrke (2000, 2003).

**Table 1** Kahler's total circulation scheme

Inputs	Flows and stocks in the production										Total flow
	Coal and iron		Machines		Buildings		Agriculture		Labor		
	Flow	Stock	Flow	Stock	Flow	Stock	Flow	Stock	Flow	Stock	
Coal and Iron	90.8	6	156.4	26	41.5	30	65.2	16	100	30	454
Machines	45.4	225	39.1	195	41.5	203	65.2	240	200	200	391
Buildings	45.4	360	39.1	390	0	0	130.4	800	200	2000	415
Agriculture	45.4	5	0	0	41.5	30	65.2	70	500	10	652
Labor	227.0	25	156.4	30	290.5	210	326.0	250			1000
Total production	454.0	621	391.0	641	415.0	475	652.0	1376	1000	2240	

Total stock of productive capital = 3113; total wages = 1000; productive capital: total wages = 3.11

Source Gehrke (2003, p. 145).

Cycles, both chaired by Ernst Wagemann, began in summer 1925. This was exactly at the time when Leontief published his article on the balance of the Russian economy, whereas in the Weimar period, advances in national accounting were induced by the analysis and calculation of reparation payments, and in the Nazi period, advances in economic planning were mainly induced by the preparation and supervision of war production. As Tooze has elaborated, Leontief's early attempts at input–output analysis had a considerable influence on the construction of input–output tables by German statisticians for a rudimentary system of central planning.

## 5 Statistical Supply and Demand Analysis

The main work which Leontief had done in those years when he was employed at the Kiel Institute consists of the statistical analysis of supply and demand. In those years in the late 1920s, it had become fashionable to do statistical supply and demand analysis. Henry Schultz (1893–1938) from Chicago was an economist who, in the wake of his teacher Henry Ludwell Moore, mainly pioneered that work (Schultz, 1925a, 1925b, 1928).<sup>32</sup> Schultz was born into a Polish-Jewish family in the former Russian empire in a place which today belongs to Belarus. Schultz was also responsible for circulation of an early translation of Slutsky's famous article on random shocks (Slutsky, 1927), which became enormously important for modern equilibrium business cycle theory.

<sup>32</sup> In his application to sugar, Schultz's work could benefit from the fact that he focused on a commodity that entered strongly into international trade.

At Kiel, Leontief got his position in the department for business cycle research where he did not work very much on business cycles. Nor did he work very much on traffic either which was the area where his predecessor had specialized, but maybe this was the reason Leontief had been hired by the Chinese.<sup>33</sup> Leontief became responsible for the newly established section “Market analysis and elasticity studies.”

So, basically, Leontief was primarily engaged in the derivation of statistical supply and demand curves which led to two major papers (Leontief, 1929, 1932), of which it was the first one that played the decisive role in the “pitfalls” controversy. Leontief’s proposal for a solution to the problem that the relevant data to estimate a supply function were different from the relevant data needed to estimate a demand function caused a fierce critique by Frisch (1933), which launched a heated debate on the proper method of deriving statistical supply and demand curves. This started when Leontief was still in Germany and continued after he had arrived at Harvard. The *Leontief-Frisch controversy* was so vehement that the editors of the *Quarterly Journal of Economics* called Marschak, who himself had written his Heidelberg habilitation thesis on the “Elasticity of demand” (Marschak, 1931)<sup>34</sup> during his Kiel years from 1928–1930, as a referee and mediator. Leontief (1931) was a reviewer of Marschak’s book.

Frisch’s first work on pitfalls in the statistical construction of supply and demand curves was still published in Germany, but in English. Frisch attacked Leontief because he did not accept the premise of Leontief’s method, namely the independence of the schedules of both functions. You then find a continuation of the debate across the Atlantic in the *Quarterly Journal of Economics* in 1934. Marschak’s contribution concluded the debate.

This controversy deals with complex and tricky issues of econometrics. It would require a full long seminar by specialists which would probably end in a controversy. The main issue was whether you could deal with supply and demand independently or not. Leontief proposed a solution to the problem that the data needed to estimate a demand function (consumption) were different from the relevant data to estimate a supply function (production). He assumed that demand and supply relations were linear in the logarithms, with constant slopes (elasticities) over time, and were subject to random shifts that were independent as between demand and supply relations. His method (Leontief, 1929, p. 29) was to divide the time series into two periods and perform regressions in each of the two periods and then solve the resulting equations jointly to obtain two elasticity estimates, one of which would be interpreted as a demand elasticity and the other as a supply elasticity. Later in the period of 1943–1948, important work was done at the Cowles Commission in Chicago when Marschak was the director there. In those years at the Cowles Commission, the

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<sup>33</sup> The institute had been founded as *Institut für Seeverkehr und Weltwirtschaft* [Institute for Sea Traffic and the World Economy] at the University of Kiel, to which it was associated as an independent entity.

<sup>34</sup> In his preface, written in October 1930, Marschak thanks Lederer, Schumpeter, Colm, Leontief, Löwe and Neisser for numerous suggestions made throughout the process of writing the book. Marschak also translated Umberto Ricci’s paper on the classification of demand curves on the basis of the elasticity concept from Italian into German (Ricci, 1931).

simultaneous equations approach in econometrics became the trademark due to the work of Frisch's student Haavelmo (1943, 1944), for which he later received the Nobel Prize.

A few more publications listed here are important in that debate, in particular, the last publication by Leontief in German on delayed adjustment of supply and partial equilibrium being published in 1934 in the Vienna-based *Zeitschrift für Nationalökonomie*, in which he analyzes the cobweb dynamics of nonlinear supply and demand curves (Leontief 1934a).

Schultz had been the first critic of Leontief's method to determine the elasticities of supply and demand.<sup>35</sup> In his final summary of several shortcomings of Leontief's method, Schultz (1930, pp. 96–97, 1938, pp. 94–95) concludes “that Leontief's method is an extremely arbitrary method, and that the results obtained by it are apt to be arithmetical accidents.” Nevertheless, “[s]tudents of the subject will, however, always be grateful to Leontief for his bold and painstaking attempt to deduce the true static, Cournot-Marshall demand and supply curve from statistics.”

The best modern text on these issues is John Chipman's contribution to the memorial symposium for Frisch (Chipman, 1998, pp. 78–84). Chipman has also a much higher opinion of Leontief's contribution, and he tries to find a balance between Leontief and Frisch given the rule of different approaches to estimate supply and demand curves. Chipman in greater detail summarizes Frisch's critique:

Frisch carried out an exhaustive classification of cases, culminating in a table (Frisch, 1933, p. 30). His general conclusion was *that there were only three cases in which Leontief's method would give correct results under his assumption of uncorrelated shifts*: (1) The two elasticities are known to be equal in magnitude, but of opposite signs; but in that case an ordinary regression would give the elasticities. (2) There is a pronounced Cournot effect on the demand side in one data set, and a pronounced Cournot effect on the supply side in the other; but in that case, too, straightforward regression would give the correct result. (3) Both the “relative violence” and the correlation have significantly different values in the two data sets. Only in the third case would Leontief's method do better than straight regression. But, he reasoned, for Leontief's method to have any *raison d'être*, it would have to give good results in other cases (Chipman, 1998, pp. 80–81; emphasis added).

Frisch did not accept the premise of Leontief's method, namely the hypothesis of independence in the supply and demand shifts and insisted on the importance of studying how the shifts of demand and supply curves are correlated. But with regard to the character of Frisch, two points should be emphasized: Although he was engaged in this bitter controversy with Leontief, he later supported Leontief to become President of the Econometric Society in 1954. Furthermore, as early as 1970, Frisch suggested Leontief for the Nobel Prize in economics for his contributions to input–output analysis. In 1969, the Bank of Sweden Prize in Economic Sciences

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<sup>35</sup> See Schultz (1930, pp. 84–97) and slightly modified Schultz (1938, pp. 83–95). For a survey on different methods to obtain elasticities of demand see also his contribution to the first volume of *Econometrica* (Schultz, 1933). Gilboy (1931) soon compared the methods of Leontief and Schultz to describe “demand” curves. Although preferring Leontief's method in theory, she considered it useless in practice because the hypothesis of independence in the supply and demand shifts was improbable to be realized on the original data.

in memory of Alfred Nobel was given the first time to Frisch and Jan Tinbergen together. According to the practice of the Nobel Prize Committee in Stockholm, former Nobel Prize winners have a strong say in making recommendations. In 1973, Frisch was successful, and Leontief received the prize “for the development of the input–output method and for its application to important economic problems.”

Just to give a flavor of the “pitfalls controversy”: Leontief (1934b, p. 357) wrote: “Professor Frisch is tilting at windmills.” At the same time, Frisch (1934, p. 755) ended his discussion with the following statement: “One cannot help feeling that the prestige of economics as a science must suffer when papers containing such mistakes and oversights as Dr. Leontief’s last paper appear in a journal of high international standing.”

So, one may understand why the editors of the journal summoned Marschak as an expert mediator. The arbiter had previously discussed Leontief’s method in his own study (Marschak, 1931, pp. 23–28). The mediator demonstrated his qualities as an expert in the comparison of the different methods used by Frisch and Leontief (and his mathematical collaborator Robert Schmidt<sup>36</sup>). Marschak (1934, p. 760) concluded that Frisch “succeeded in giving to this [Leontief’s] method an elementary mathematical exposition which is considerably simpler and at the same time more general.” In his exposition, Marschak specified five assumptions necessary for the application of Leontief’s method:

- (1) elasticities of the demand and supply curve to be constant all along the curves;
- (2) constant over time;
- (3) demand shifts are non-correlated with supply shifts;
- (4) the price–quantity correlations must be significantly different in both materials;
- (5) the same must hold for the relative violences.

“Even granting Assumption I, R. Frisch denies that the four other assumptions are likely to hold good simultaneously except by a ‘miracle’” (Marschak, 1934, p. 761), whereas for Leontief, they are a “mathematical necessity” (Leontief 1934c, p. 758). Marschak elaborates the assumption of constant elasticities over time as the “vulnerable point in Leontief’s method” (Marschak, 1934, p. 763). The Leontief–Frisch controversy also revealed differences in economic reasoning, whereas Leontief was more concerned with invariance and autonomy, and Frisch worried more about correlations and spurious results.

Mary Morgan, who discusses Leontief’s method in her comprehensive history of the development of econometric ideas in demand analysis,<sup>37</sup> comes to the conclusion that “Leontief’s paper marked an ingenious and challenging attempt to estimate a two-equation demand and supply model simultaneously” (Morgan, 1990, p. 180).

One other very interesting later commentator on the econometric issues is Edward Leamer, who very much regretted that the modern development in econometrics had

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<sup>36</sup> Schmidt himself published a subsequent article on the conciseness of the elasticity coefficients (Schmidt, 1930).

<sup>37</sup> See Part II of Morgan (1990).

widely overlooked Leontief's contribution. Leamer (1981, pp. 321–322) concluded as follows:

The method [...] rests on the unlikely assumption that the slopes  $\beta$  and  $\theta$  are constant over time but the variances are not. Still, Leontief did have the hyperbola properly defined, which is only one short step from the results in this paper. It is therefore surprising that Leontief's contribution has been so completely ignored by the post-1940 econometrics literature. The fault seems to me to lie with excessive attention to asymptotic properties of estimators and insufficient interest in the shapes of likelihood functions.

## 6 Concluding Remarks

Let me conclude with a quotation from a letter which was written by Schumpeter to the Dean of Harvard University when in 1935, the issue of Leontief's prolongation as an assistant professor came up. Schumpeter himself had moved from the University of Bonn in Germany to become a Professor at Harvard University in September 1932. Schumpeter had been a cofounder of the Econometric Society and was a close personal friend of Frisch. So, Schumpeter knew the Leontief–Frisch controversy very well. According to Samuelson, “[i]t must have been the newly-arrived-in-Cambridge Schumpeter who plucked Leontief from a brief National Bureau stint to Harvard [...] a brilliant investment decision even if not 100% cogent” (Samuelson, 2004, p. 4).<sup>38</sup>

In the American university system at that time in the 1930s, just like today, normally you get a first contract as an assistant professor for three years, which then has to be renewed for a second three year period. When Leontief was in the 3rd year of his first period as an assistant professor, Schumpeter wrote that letter to support the prolongation of the contract, which was endangered.

But, when 23, he followed this up by a paper on the simultaneous derivation of logarithmically linear demand and supply functions, which won international attention. (Dr. R. Schmidt, the Kiel mathematician, helped with the mathematics. Perhaps you know him.) Everybody read, discussed, criticized, admired, or damned it—young Leontief was, in this field, in the center of discussion. Much may be said for and against the method itself, but no doubt is possible about the question relevant here, viz., the supreme force and brilliance of the author as displayed by it. No similar case, of similar success of so young a man, is known to me either from experience or from the history of my science. (Joseph A. Schumpeter, letter of November 10, 1935 to George Birkhoff asking for the promotion of Leontief for a second term as Ass. Professor at Harvard—Schumpeter, 2000, p. 281)

Schumpeter here refers to the very first 1929 article by Leontief, which provoked the fierce critique by Frisch. The first statement is wrong since Leontief was 24 not 23, when he wrote this article on the simultaneous derivation of logarithmically linear demand and supply functions.

Schumpeter wrote also a letter with similar content to John Maynard Keynes to ask him to support the prolongation of Leontief's contract. As is well known today,

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<sup>38</sup> For a detailed analysis of the intense relationship between Schumpeter and Leontief in the decisive years 1929–1935 and Schumpeter's role as a promoter of Leontief's professional career see Bjerkholt (2016).



Schumpeter was successful: Leontief's contract was renewed. But now, we have left Germany permanently and are fully in the USA where Leontief's work became increasingly preoccupied with input–output analysis.

**Acknowledgements** I would like to thank Vladimir Avtonomov and Denis Melnik for valuable comments and the late Olav Bjerkholt (1942–2020), with whom I communicated over many years. I am also grateful to Klaus-Rainer Brintzinger, the director of the library of the Ludwig-Maximilians-University in Munich, for providing me with the material on the Ph.D. of Wassily Leontief sr.

## Appendix 1

See Fig. 3.

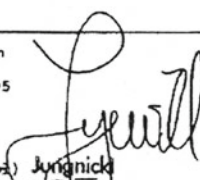
## Appendix 2


See Fig. 4.

## Appendix 3

See Fig. 5.

**Formule A**

1	Staat/État/Country <b>Bundesrepublik Deutschland</b>		
2	Standesamtbehörde Service de l'état civil de <b>München</b> Civil Registry Office of		
3	<b>Auszug aus dem Geburtseintrag Nr. 2445/1905, Standesamt I München</b> Extrait de l'acte de naissance n° Extract from birth registration no.		
4	Tag und Ort der Geburt Date et lieu de naissance Date and place of birth	Jo Mo An 05 08 1905	München -----
5	Name Nom/Name	Leontief -----	
6	Vornamen Prénoms/Forenames	Wassily -----	
7	Geschlecht Sexe/Sex	M -----	
8	Vater Père/Father	9 Mutter Mère/Mother	
5	Name Nom/Name	Leontief -----	Leontief geb. Becker -----
6	Vornamen Prénoms/Forenames	Wassily -----	Slata -----
10	Andere Angaben aus dem Eintrag/Autres énonciations de l'acte/Other particulars of the registration -----		
11	Tag der Ausstellung Date de délivrance Date of issue	Jo Mo An 04 10 2005	 Heide Jungnick Unterschrift/Signature/Signatura



Steg/Seau/Seal

14102 Internationale Geburtsurkunde Seite 1 von 2 Verlag 08 Standesamtbeamter-Großh. Frankfurt am Main, Birth 2004

SYMBOLES/ZEICHEN/SYMBOLS/SIMBOLOS/ZYMBOLI/SIMBOLI/SYMBOLEN/SÍMBOLOS/SARETLER/SIMBOL/SYMBOLÉ

Jo: Jour/Tag/Day/Dia/Hμέρα/Giorno/Dag/Día/Gün/Dän/Dziesń  
 Mo: Mois/Monet/Month/Mesi/Mήνας/Mese/Maand/Mese/Ay/Mesec/Miesiac  
 An: Année/Jahr/Year/Año/Έτος/Anno/Jaar/Ano/Yil/Godina/Rok  
 M: Masculin/Männlich/Masculine/Masculino/Αρσεν/Μaschile/Mannlich/Masculino/Erkek/Muško/Meska  
 F: Feminin/Weiblich/Feminine/Femenino/Θήλυ/Feminile/Vrouwelijk/Feminino/Kadın/Zenska/Zenska  
 Mar: Marriage/Eheschließung/Marriage/Matrimonio/Tάμος/Matrimonio/Huwelijk/Casamento/Eviañma/Zaključenie braka/Matzenstwo  
 Sc: Séparation de corps/Trennung von Tisch und Bett/Legal separation/Separación personal/Χωρισμός από τραπέζης και κοίτης/  
 Separazione personale/Scheidung von tisch und bett/Separação de pessoas e bens/Ayrılıq/Fizicka rastava/Separacja  
 Div: Divorce/Scheidung/Divorce/Divorcio/Διαζύγιον/Divorzo/Echtscheidung/Divorcio/Boşanma/Razvod/Pozwód  
 A: Annulacion/Nichtigerklärung/Annullamen/Anulación/Ακύρωσις/Annullamento/Nietigverklaring/Anulación/psst/Ponistenje/  
 Unwólazhenie  
 D: Décès/Tod/Death/Defunción/Θάνατος/Morte/Overlijden/Óbito/Ötüm/Smrt/Zgon  
 Dm: Décès du man/Tod des Ehemannes/Death of the husband/Defunción del marido/Θάνατος του αυλούγου/Morte del marito/Overlijden  
 van de man/Óbito do marido/Kocainin ölümü/Smrt muža/Zgon meža  
 Df: Décès de la femme/Tod der Ehefrau/Death of the wife/Defunción de la mujer/Θάνατος της αυλούγου/Morte della moglie/Overlijden  
 van de vrouw/Óbito de mulher/Karlinin ölümü/Smrt žene/Zgon žony

**Fig. 3** Birth certificate of Wassily Leontief jr., dated October 4, 2005, city of Munich. *Source* Handed to his daughter Svetlana Alpers

QVOD FELIX FAVSTVMQVE SIT  
VNIVERSITATIS LITTERARIAE  
FRIDERICAE GVILELMAE  
BEROLINENSIS

RECTORE MAGNIFICO  
WILHELM HIS

MEDICINAE ET CHIRVRGIAE DOCTORE IN HAC VNIVERSITATE PROFESSORE PVBLICO ORDINARIO A CONSILIIIS MEDICIS  
INTIMIS INSTITVTI PRIMI CLINICI DIRECTORE ORDINIS CRVCIS FERREAE IN SECVNDA ET PRIMA CLASSE ORDINVM  
REGIORVM ET AQVILAE RVBRAE ET CORONAE IN TERTIA CLASSE MVLTORVMQVE ALIORVM ORDINVM EQVITE

EX DECRETO ORDINIS AMPLISSIMI PHILOSOPHORVM  
PROMOTOR LEGITIME CONSTITVTVS

ALBERT BRACKMANN

PHILOSOPHIAE DOCTOR IN HAC VNIVERSITATE PROFESSOR PVBLICVS ORDINARIVS SEMINARI HISTORICI DIRECTOR  
ACADEMIAE SCIENTIARVM BORVSSICAE SOCIVS ORDINARIVS SOCIETATIS LITTERARIAE COTTINGENSIS SOCIVS  
EPISTVLARIS ORDINIS CRVCIS FERREAE IN SECVNDA CLASSE ALIORVMQVE ORDINVM EQVES

ORDINIS PHILOSOPHORVM II. T. DECANVS

VIRO CLARISSIMO ATQVE DOCTISSIMO

WASSILY LEONTIEF

RVSSO

POSTQVAM EXAMEN PHILOSOPHIAE CVM LAVDE SVSTINVIT  
ET DISSERTATIONEM LAVDABLEM CVIVS TITVLVS EST

·DIE WIRTSCHAFT ALS KREISLAUF·

AVCTORITATE ORDINIS PROBATA EDIDIT

PHILOSOPHIAE DOCTORIS  
ET ARTIVM LIBERALIVM MAGISTRI

ORNAMENTA ET HONORES

DIE XIX. M. DECEMBRIS A. MCMXXVIII

RITE CONTVLIT

COLLATAQVE PVBLICO HOC DIPLOMATE

PHILOSOPHORVM ORDINIS OBSIGNATIONE COMPROBATO  
DECLARAVIT



BEROLIN  
VERLAGS-ANSTALT DER UNIVERSITÄT

Fig. 4 Official document of Leontief's Ph.D. Source Archive, Humboldt University Berlin



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**Wassily Leontief**  
 University Professor

February 8, 1993

Dear Professor Lowe:

*I congratulate you on your 100th Birthday!*

*I never had an opportunity to congratulate someone whose life spanned an entire century and I am doing so with particular pleasure, because I had the privilege of meeting you for the first time seventy-four years ago.*

*I was a member of your seminar in Kiel. We met not in the main building of Institute but in the small club house standing on the shore of the "Kieler Fjorde"; while discussing deep theoretical problems we could watch the sails of slender yachts gliding back and forth.*

*I remember how on many occasions, while "playing hooky" --instead of sitting dutifully at my desk-- I was steering a sailing boat passed the Institute and had to drop flat on the deck in order to escape the watchful eyes of the Hern Geheimrat Professor Dr. Harms surveying the blue waters from his desk facing the front window of his splendid office located on the second floor.*

*I still cherish the discolored snapshot taken in 1927 or 1928 during one of the institute dances. It shows three rows, one above the other of laughing faces of young assistants and their girlfriends, with Gerhard Colm and Haus Neisser (sporting a naval officer's cap) on the side, and you standing in the background with a benign smile on your face.*

*Those were happy times and then came the terrible times. You lived throughout all of them keeping on a strait course, teaching and guiding generations after generations of your students and admiring colleagues.*

Sincerely,

Wassily Leontief

WL/mp  
 [S:\WU1600.06\Love]

**Fig. 5** Fax letter from Leontief (New York) to Lowe (Wolfenbüttel, Germany) congratulating Lowe to his hundredth birthday on March 4, 1993. *Source* Copy given by Lowe to the author

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## 1 Introduction

Paul Alexander Baran (1910–1964) was born in Russia and spent the first half of his life there and in Poland and Germany before settling in the United States in 1939. As a teenager he studied economics in Moscow, probably, under Evgeny Preobrazhensky, a Left Oppositionist who was subsequently executed on Stalin's orders. Baran learned two important lessons from Preobrazhensky. The first was that Marx's analysis in part VIII of *Capital* Volume I of 'primitive accumulation' in Britain could also be applied to the Soviet Union. The second lesson was that a new and highly contradictory stage of capitalism, which Preobrazhensky termed 'monopolism' or 'monopoly capitalism', had important characteristics that distinguished it from the earlier, competitive stage that Marx had dealt with in *Capital*. Baran put these two propositions to good use, the first in his analysis of the problems of economic development in poor countries in his *Political Economy of Growth* (1957), and the second both there and in the posthumously published *Monopoly Capital* (1966), co-authored with Paul Sweezy. In this Chapter I begin by outlining the details of Baran's life, before discussing the two big ideas that he almost certainly learned from Preobrazhensky. I then summarise Baran's own analysis of the economics of development and his discussion (to some extent co-authored with Paul Sweezy) of the monopoly stage of capitalism, before concluding with a brief critique of these important ideas.

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## 2 Paul Alexander Baran

Baran's life falls neatly into two halves of almost equal length, the first spent in Europe and the second in the United States (Sweezy, 1965). In every other aspect, however, the story of his life is a very complex one, which leaves a number of unanswered questions.

Paul Alexander Baran was born on 8 December 1910 in Nikolaev, on the Black Sea in Ukraine (then a part of Tsarist Russia), into a family of Polish Jews. His father, Abram Baran, was a medical doctor and a Menshevik sympathiser. Baran was educated at home by his parents until he reached the age of eleven, when the family left the Soviet Union for their ancestral home in Vilna (then part of Poland) and acquired Polish citizenship. After spending some time in Dresden the family moved back to Russia, leaving Paul to finish his secondary school education in Germany. Then, in 1926, he rejoined his family in the Soviet Union, studying at the Plekhanov Institute of Economics for the next two years.

In 1928 Baran returned to Germany, working in Berlin at the Agricultural Academy on a research project for the International Agrarian Institute of Moscow. Alarmed at the persecution of the Left Opposition in the Soviet Union, Baran broke with Communism in 1930 and joined the Social Democratic Party. He found employment at the Frankfurt Institute as a research assistant to Friedrich Pollock, a job which took him first to Breslau and then back to Berlin. Here he wrote a thesis on economic planning, supervised by Emil Lederer, and wrote articles for the socialist journal *Die Gesellschaft* under the pseudonym Alexander Gabriel.

Baran left Germany in 1933, soon after Hitler's accession to power, spending some time in Paris before visiting his parents in Moscow. When his visa expired in January 1935 he was forced to leave the Soviet Union, returning to Vilna, where he worked in the family timber business. In 1938 this took him to Britain, where he learned English and tried unsuccessfully to find an academic job. The first, European, half of his life ended when Baran moved to the United States in October 1939.

The first ten years of his new life there were almost as complicated as his final decade in Europe had been. Until mid-1941 he was a graduate student at Harvard, earning a Master's degree but running low on funds and therefore being unable to complete a doctorate. In the academic year 1941–1942 he was a Fellow at the Brookings Institution in Washington, working on the economics of price controls, which qualified him for a job at the Office of Price Administration and then at the Office of Strategic Studies, under his former Harvard teacher E. S. Mason. By now a US citizen, he was conscripted into the army, and in March 1945 went to Germany on the initiative of another Harvard veteran, John Kenneth Galbraith. Here he worked for the Overall Effects Division of the US Strategic Bombing Survey, and in September 1945 he went to Japan for six months as deputy head of the Division there.

The now demobilised Baran would have moved on to Poland as economic adviser to the UNRRA mission there, but on account of his political views he was denied a passport by the State Department and so had to return to the US. He spent the next three years in New York, first at the Department of Commerce and then at the Federal

Reserve, also teaching at George Washington University. Baran finally settled down in June 1949, when he moved to the West Coast, working for the last fifteen years of his life at Stanford University (where his Marxist opinions earned him a lower salary and higher teaching load than his colleagues enjoyed). Having finally obtained a US passport, Baran travelled widely overseas from his base in California, including a spell at Oxford University in 1953 where he gave a series of lectures that ‘were in effect the first draft of *The Political Economy of Growth*’ (Sweezy, 1965, p. 45). Paul Baran succumbed to the last of a series of heart attacks in San Francisco on 26 March 1964.

Paul Sweezy notes that Baran ‘was not given to reminiscing’ (Sweezy, 1965, p. 29), and there are at least four significant gaps in our knowledge of his early life. The first, and by far the most relevant to the theme of this chapter, concerns his two years as a student at the Plekhanov Institute in Moscow (1926–1928). How much contact did he have with its then director, Evgenii Preobrazhensky? What did he learn from Preobrazhensky, and how strong was his influence on Baran’s later economic thinking? I shall return to these important questions later in the chapter.

The second gap involves his activities in Germany in the early 1930s. What did Baran learn from Pollock and his Frankfurt Institute colleagues, and from Emil Lederer at the University of Berlin? What exactly did he argue in his dissertation on the economics of planning, and what was his position at this time on the lessons to be drawn from the early stages of Stalin’s forced industrialisation of the Soviet Union?

The third, and for me the most frustrating, gap in our knowledge concerns Baran’s brief period in Britain in 1938–1939. What exactly did he do there when he was not selling timber? Whom did he meet and take advice from? Which universities did he try to find work in? In the course of my career I have read a great deal about left-wing economists and political activists in the Britain of the late 1930s, yet I do not recall ever finding a reference to Paul Baran.

Fourth, and finally, we know surprisingly little about the intellectual relationship between Baran and Paul Sweezy when they were both at Harvard in 1939–1941. In the preface to his *Theory of Capitalist Development* Sweezy briefly acknowledges the ‘valuable criticisms and suggestions’ of ‘Mr. Paul Baran’, together with seven other colleagues with full doctorates. But he devotes half a paragraph to the apparently much more substantial contribution made by Dr. Shigeto Tsuru (Sweezy, 1942 [1970], p. vi). When Mike Howard and I wrote on this question, many years ago, we assumed that it was predominantly Sweezy who influenced Baran, rather than the reverse (Howard & King, 1992, Chap. 6). After all, it was Sweezy who was the instructor and Baran the student. But there is no real documentary evidence either way, and so it is entirely possible that Baran used his knowledge of the contemporary German and Russian literature to make a significant contribution to the later chapters of Sweezy’s book.

### 3 Evgeny Preobrazhensky

Evgeny Alekseevich Preobrazhensky was born on 15 February 1886 in Bolkhov, in the central Russian province of Orel. The son of an orthodox priest, he was radicalised in his teens, joined the Social Democratic Party in 1903 and soon became a member of its Bolshevik wing. He worked for the Party, moving to Moscow in 1918 and serving briefly as joint secretary of its Central Committee in 1920–1921 and also as joint editor of *Pravda* for several years in the 1920s. Preobrazhensky was strongly opposed to the New Economic Policy, which he regarded as much too favourable to the peasantry, and was thus associated with the Left Opposition; his close but somewhat uneasy relationship with Leon Trotsky is discussed at some length by Isaac Deutscher (1959 [1970]). His antagonism to Stalin led to his expulsion from the Party in October 1927. After a spell in provincial exile Preobrazhensky had his membership restored in January 1930, but his efforts to reconcile with Stalin failed, and in 1933 he was again expelled and exiled. He was arrested in December 1936 and executed on 13 July 1937.

As Deutscher notes, Preobrazhensky ‘was primarily a scholar, pursuing his line of reasoning to no matter what unpopular conclusions it might lead him and no matter what damage it might do to his standing in the party’ (*ibid.*, p. 206). His earliest writings have recently appeared in an English edition (Day & Gorinov, 2015), but he is probably best known for his *ABC of Communism*, written jointly with Nikolai Bukharin in 1921 and reissued as a paperback half a century later with an introduction by E. H. Carr (Bukharin & Preobrazhensky, 1921 [1969]). In 1924 Preobrazhensky published a very important article on the subject of ‘primitive socialist accumulation’, a notion that he derived from Marx (the German word *ursprünglich* is better translated, in this context, as ‘primary’, ‘original’ or ‘initial’ rather than ‘primitive’). He developed the arguments at book length in *The New Economics* (Preobrazhensky, 1926 [1965]; see also Preobrazhensky, 1980, 1985).

The underlying principle of primitive socialist accumulation is very simple: if the industrialisation of a predominantly agricultural country is to proceed at a rapid pace, the peasants must produce a surplus of food over and above their own minimum requirements and transfer it to the towns, where it is needed to feed the growing industrial labour force. This can be achieved through compulsion, as in the case of serfdom; through the payment of rent in cash or kind to the landlords; through the imposition of taxes by the state; or through the provision of manufactured consumer goods that the peasants can buy with the cash receipts from the sale of their own surplus product. Marx had discussed all this at some length in part VIII of *Capital*, volume 1, in the context of the early industrialisation of Britain and other Western European nations, which had relied on a combination of the first two methods, compulsion and the payment of rent.

Preobrazhensky’s achievement was to demonstrate that the same issues faced the Soviet government in the 1920s, once the prospect of socialist revolution in other European countries had faded and the new state could no longer expect to receive economic assistance from overseas comrades in economically more advanced

nations. Having destroyed the final remnants of serfdom and seized the land, the Russian peasantry could not be parted from their surplus food production in the ways that Marx had described, and the Soviet state was unable to impose high levels of taxation on them. That left only one method of securing the necessary food for the growing numbers of industrial workers: the sale to them of consumer goods on an ever-increasing scale. This was the fundamental principle of the New Economic Policy, which Preobrazhensky criticised as conceding too much to the peasantry.

The basic objection to the ‘market solution’ to the problem of primitive socialist accumulation, he argued, was that it required far too much to be invested in the production of consumer goods (Marx’s department II) rather than in means of production (department I), and this would considerably reduce the overall rate of growth of the manufacturing sector (Preobrazhensky, 1925 [1965]). Thus he was greatly encouraged by Stalin’s great policy change in 1928, which involved the adoption of all the basic principles of the Left Opposition. The collectivisation of agriculture allowed the compulsion mechanism to be introduced, paving the way for a much higher rate of growth of industrial output, in particular that of department I. So Preobrazhensky ‘responded eagerly to Stalin’s left course. He perceived it as a confirmation of his own theory ... [and] was convinced of its momentous significance’ (Deutscher, 1959 [1970], p. 416). Sadly, Stalin did not reciprocate,

Preobrazhensky set out his ideas on the new stage of capitalism in a book published in 1931 with the title *The Decline of Capitalism*. It was always an innately contradictory and unstable system, he argued, but in the earlier stage of free competition there had been a number of factors working in favour of relatively successful macroeconomic outcomes. The first and possibly the most important was a ‘balance of forces more favourable to the proletariat in terms of the relation between labor power and capital on the labor market, resulting in pressure from the workers to raise wages’ (Preobrazhensky, 1931 [1985], p. 69). This encouraged technical progress and the resulting growth of labour productivity and also expanded the internal market. Second, the establishment of new enterprises was relatively easy, which led to increased investment and again stimulated the adoption of new technology. Other favourable elements were the relatively low level of excess capacity that competitive firms maintained, the high degree of mobility of capital from one industry to another, and the ability of the market to select the most viable enterprises and to reconstruct them where necessary. In addition, ‘the existence of territories as yet unoccupied by the capitalist system of production and exchange’ allowed ‘a temporary breathing space in periods of especially serious overproduction’ (*ibid.*, p. 70). Finally, the absence of cartels meant that reductions in wholesale prices flowed quickly through to the retail market.

All of these advantages were undermined in the subsequent stage, which Preobrazhensky referred to as ‘monopolism’ or ‘monopolistic capitalism’. First and foremost, ‘the balance of forces in the economic struggle between the proletariat and the capitalists’ had shifted in favour of employers, due to ‘the growth of absolute (or what is more frequently referred to as structural) unemployment; by a closer merger of trusts with the state; and by subordination of the reformist trade union bureaucracies to the monopolies’ (*ibid.*, p. 72). The result was both a slower rate of introduction

of new technology and a lower rate of growth of ‘society’s effective demand’ (*ibid.*, p. 73). Second, monopolies also tended to maintain much higher levels of excess capacity, which acted as a barrier to entry and reduced the prospect of major new investment as a way out of depression. Thus there had been ‘a change in the character of capitalist cycles’ (*ibid.*, p. 74), made even worse by price rigidity, which led to sharp cuts in output when demand fell. Finally, Preobrazhensky maintained that ‘[m]onopolistic capitalism brings with it a growth of unproductive consumption’ (*ibid.*, p. 75). This was the *effect* of the slowdown in economic development, he insisted, not the cause. It resulted in the growth of circulation expenditure at the expense of productive activity, and also in a ‘more rapid increase in salaried employees than of workers, even in countries enjoying rapid industrial development such as the United States’ (*ibid.*, p. 76).

The extent of Preobrazhensky’s influence on Marxian political economy is debatable. In his *New Palgrave* entry Michael Ellman claimed that ‘Preobrazhensky’s work has had an enormous influence throughout the world .... He is rightly considered one of the outstanding Marxist economists of the twentieth century’ (Ellman, 1990, p. 217). His ideas have indeed been taken seriously by many later writers, including Carr (1958), Dobb (1929, 1948, 1967), Erlich (1950), Howard and King (1989, 1992) and Nove (1969 [1992]). Yet there is no index reference to him in Joseph Schumpeter’s authoritative *History of Economic Analysis* (1954), and—three generations later—Stephen Kotkin’s monumental biography of Stalin makes only a handful of brief references to Preobrazhensky in the first volume, which ends in 1928, and none at all in the second volume, which takes the story down to 1941 (Kotkin, 2014, 2017). There is no published biography or unpublished doctoral dissertation devoted to Preobrazhensky in either English or German, and little or nothing seems to be known about his work at the Plekhanov Institute in 1926–1927. Did he teach there, or simply serve as an administrator and/or prestigious figurehead? Did he socialise with the students or maintain his distance from them? The best biographical source in English, which draws on extensive interviews with Preobrazhensky’s son, makes no mention of this important aspect of his career (Gorimov et al., 1991).

## 4 Baran on the Economics of Development

Thus it is difficult to say anything with any degree of confidence about the extent and nature of the contacts that Paul Baran may have had with Preobrazhensky while he was a student in Moscow. Howard and King (1992, p. 174) describe the two men as friends, but I have not been able to find any documentary evidence to confirm this claim. There are no index references to Preobrazhensky in any of Baran’s books and (as already noted) he was never very forthcoming about his sources, even in his extensive conversations with Paul Sweezy.

The bibliography of Baran’s publications provided by Huberman and Sweezy (1965, pp. 132–135) shows that he was regarded as a reliable authority on the Soviet economy, and on Soviet economics, by several leading journals to which

he contributed a number of articles and book reviews in the 1940s. However, he seems not to have presented his own ideas on the theory of economic development before 1950, when he participated in a session at the American Economic Association annual meetings on 'Economic progress: general considerations'. Despite its title, Baran's three-and-a-half-page 'Discussion' makes no reference to the arguments of the other participants, but it does set out his own ideas very clearly and with commendable brevity.

There was certainly an urgent need for the rapid accumulation of capital to raise output and living standards in much of the world, Baran conceded. However, he maintained,

The very low aggregate incomes at the disposal of the nations inhabiting the world's backward areas leave little room for capital accumulation. Even with popular consumption at the rock-bottom level (or below), the "surplus" that can be mobilized for investment is bound to remain small. Thus in the absence of foreign aid, economic growth in underdeveloped countries would have to remain slow under any circumstances (Baran, 1951, p. 355)

This is possibly his first published use of the word 'surplus' (the inverted commas are his own). Baran did not expect such rapid accumulation to occur, since the meagre surpluses that did exist were being 'frittered away on unproductive purposes. Accruing, as they do, to a relatively narrow upper-income stratum, these surpluses are used largely to support extravagant living on the part of their recipients' (*ibid.*, p. 356). While in principle it was possible to eliminate non-essential consumption through capital levies and a highly progressive tax system, this would be strongly resisted by the ruling elites. 'Set up to guard and to abet the existing property rights and privileges', he concluded, these governments 'cannot become the architect of a policy calculated to destroy the privileges standing in the way of economic progress and to place the property and the incomes derived therefrom at the service of society as a whole' (*ibid.*, p. 358). Thus radical political change was an essential precondition for genuine economic progress.

Baran soon developed these ideas in three longer articles. In a paper published in the *Manchester School* he again asserted the need for a radical fiscal policy that would 'syphon off all surplus purchasing power, and in this way eliminate non-essential consumption' (Baran, 1952a, p. 263; this time there are no inverted commas). But for this to happen, a drastic change in the political framework was required. 'The alliance between feudal landlords, industrial royalists, and the capitalist middle classes has to be broken. The keepers of the past cannot be the builders of the future' (*ibid.*, p. 268). In the same year Baran contributed a long chapter on economic planning to an authoritative *Survey of Contemporary Economics*, edited by B. F. Haley. Here he dealt principally with the macroeconomics of economic growth rather than the microeconomic aspects of the planning process, arguing in two long sections that much could be learned from the Soviet experience concerning both 'the mobilisation of the "economic surplus"' (Baran, 1952b, pp. 155–159) and 'the allocation of the "economic surplus"' (*ibid.*, pp. 159–163); these are the section titles (and the inverted commas have returned).

Finally, in a paper published in the independent Marxian journal *Science and Society*, Baran explained his thinking on the economic surplus at some length. He

began by noting that the rate of economic growth is a function of net investment, which ‘depends ... on the *size* and the *mode of utilization* of the currently generated *economic surplus*’. He then identified three variants of the concept of economic surplus. ‘(1) *Actual* economic surplus, i.e. the difference between society’s *actual* current output and its *actual* current consumption. It is thus identical with current saving’. This must be distinguished from ‘(2) *Potential* economic surplus, i.e. the difference between the output that *could* be produced with the help of *actually* employed productive resources and what might be regarded as *essential* consumption’. To realise the potential surplus would require ‘a more or less drastic reorganization of the production and distribution of social output and may imply far reaching changes in the structure of society’ (Baran, 1953, p. 273; original italics). The third concept, the *planned* economic surplus, ‘is relevant only to comprehensive planning under socialism’ (*ibid.*, p. 279). It is the difference between the optimum output and the chosen optimum level of consumption and is determined by a conscious set of economic and social decisions.

Baran devotes five pages to a detailed discussion of the potential economic surplus, which has three distinct components. ‘One is society’s excess consumption predominantly on the part of the upper income groups, the other is the output lost to society through the existence of unproductive workers, the third is the output lost because of the irrationality and wastefulness of the prevailing economic organization’. The measurement of the potential surplus, Baran continues, is a matter of some difficulty, since the category itself ‘transcends the horizon the existing social order—relating as it does not merely to the easily observable performance of the given socioeconomic organization, but also the less readily visualized image of a more rationally ordered society’ (*ibid.*, p. 274). However, it is possible to estimate what constitutes essential consumption by reference ‘to scientific inquiry and to common sense judgement’. Similarly, ‘the classification of unproductive workers is less complicated than usually assumed’ (*ibid.*, p. 277), while the ‘waste and irrationality of production ... can be observed in a great number of instances’ and its magnitude inferred from the large increase in wartime output that had been achieved by its elimination in the United States, Britain and Germany (*ibid.*, pp. 278–279).

Baran devotes the second half of the paper to an analysis of the growth of the actual and potential economic surplus from antiquity through to the competitive stage and then to the monopoly stage of capitalism, and the problems that the latter faced in disposing of its massive surplus (*ibid.*, pp. 280–297). These themes are developed at some length in the only book to appear under his name in his lifetime, *The Political Economy of Growth*, which was completed in 1955 but only published two years later by Sweezy’s Monthly Review Press (see Sweezy, 1965, pp. 45–46, for the grisly details of its earlier rejection by Basil Blackwell, with whom Baran had a contract).

The 1973 Penguin edition, to which I shall refer in what follows, begins with Baran’s ‘Foreword’ to the 1962 reprint, in which he responds to some of his critics (Baran, 1957 [1973], pp. 19–58). Then there is a lengthy ‘Introduction’ by the British economist R. B. (Bob) Sutcliffe (pp. 59–104). Chapter 1 of the 1957 text is a general introduction (pp. 107–131), and Chap. 2 provides a discussion of the concept of the



economic surplus that draws heavily on the 1953 article (pp. 132–157). The long Chaps. 3 and 4 are devoted to Baran's analysis of monopoly capitalism and will be discussed in detail in the next section of this chapter (pp. 158–208, 209–264). Finally he turns to 'the roots of backwardness' (Chap. 5, pp. 265–299) and to what he terms 'the morphology of backwardness' (Chap. 6, pp. 300–344 and Chap 7, pp. 345–401), ending with a dissection of the conditions required to enable the necessary 'steep ascent' from backwardness to be accomplished (Chap. 8, pp. 402–63).

Baran's analysis of the roots of backwardness hinges on his explanation as to why the 'primary accumulation of capital' succeeded in some parts of the world and not in others. The successful industrialisation of Western Europe was 'by no means a matter of fortuitous accident or of some racial peculiarities of different peoples' (Baran, 1957 [1973], p. 273), as is still sometimes argued by academic historians today (see Henrich, 2020 for a recent example). It was instead the result of predatory behaviour by the Europeans in their contact with the peoples of Africa and Asia, where the Western European visitors.

... rapidly determined to extract the largest possible gains from the host countries, and to take their loot home. Thus they engaged in outright plunder or in plunder thinly veiled as trade, seizing and removing tremendous wealth from the places of their penetrations (*ibid.*, p. 274).

This was most blatant in India, where the failure to industrialise was not 'something accidental or due to some peculiar inaptitude of the Indian "race"'. On the contrary, it was 'caused by the elaborate, ruthless, systematic despoliation of India by British capital from the very onset of British rule' (*ibid.*, p. 278). In sharp contrast, Japan's successful industrialisation overcame the country's initial backwardness, poverty and paucity of natural resources, but precisely because 'Japan is the only country in Asia (and in Africa and in Latin America) that escaped being turned into a colony or dependency of Western European or American capitalism, that had a chance of independent national development' (*ibid.*, p. 294).

The two long chapters on the morphology of backwardness discuss in great detail the reasons why the economic surplus that is produced in poor countries has not been used to promote their rapid industrialisation. It is not in the interests either of foreign capital or of the local 'comprador' bourgeoisie for such genuine progress to occur:

The economic surplus appropriated in lavish amounts by monopolistic concerns in backward countries is not employed for productive purposes. It is neither ploughed back into their own enterprises, nor does it serve to develop others. To the extent that it is not taken abroad by their foreign stockholders, it is used in a manner very much resembling that of the landed aristocracy. It supports luxurious living by its recipients, is spent on construction of urban and rural residences, on servants, excess consumption, and the like. The remainder is invested in the acquisition of rent-bearing land, in financing mercantile activities of all kinds, in usury and speculation. Last but not least, significant sums are removed abroad where they are held as hedges against the depreciation of the domestic currency or as nest eggs assuring their owners of suitable retreats in the case of social and political upheavals at home (*ibid.*, pp. 316–317).

Again, none of this is accidental. The ‘main task of imperialism in our time’, Baran insists, is ‘to prevent, or, if that is impossible, to slow down and to control the economic development of underdeveloped countries’ (*ibid.*, p. 340).

The principal barrier to the rapid industrialisation of poor countries, he maintains, is not the restricted size of either the actual or the potential economic surplus but rather the way in which it is utilised. ‘It is absorbed by various forms of excess consumption of the upper class, by increments to hoards at home and abroad, by the maintenance of vast unproductive bureaucracies and of even more expensive and no less redundant military establishment[s]’, while ‘a very large share of it ... is withdrawn by foreign capital’ (*ibid.*, pp. 376–377). The ‘steep ascent’ that is required will be possible only if the potential surplus is no longer squandered in these ways, and this requires social revolution. Baran’s account of the problems faced by the Soviet Union in the 1920s, which dominates the final chapter of the book, draws heavily on Preobrazhensky (though he is not referred to by name). Baran argues that ‘the mobilization of the potential economic surplus that was dormant in the structure of the pre-revolutionary capitalist society becomes the first and foremost problem that has to be solved by the socialist government if it is to be able to embark upon a planned programme of economic development’ (*ibid.*, p. 423). And this requires above all the collectivisation of peasant agriculture.

## 5 Baran and Sweezy on Monopoly Capital

The second lesson that Baran learned from Preobrazhensky was the need to analyse the operation of the new stage of monopolised capitalism in the West. A brief account in English of the principal features of monopoly capital was first set out in 1942 by Paul Sweezy in the two chapters (XIV, XV) of his *Theory of Capitalist Development* that were devoted to the question, and are conveniently summarised at the end of Chap. 15. Here Sweezy lists the five most important general effects of monopoly on the functioning of the capitalist system. First, prices are (of course) higher under monopoly. Second, a hierarchy of profit rates is created, ‘highest in the most completely monopolized industries, lowest in the most competitive’. Third, this results in a higher rate of accumulation, accentuating the existing economic contradictions of capitalism (here he refers specifically to the falling rate of profit and the tendency to underconsumption). Fourth, investment is crowded into the more competitive industries, making depressions even more likely. Fifth and finally, ‘the labor-saving bias of capitalist technology is enhanced, and the introduction of new techniques is so arranged as to minimise the need for new capital’ (Sweezy, 1942, p. 285).

Sweezy then identifies a number of counter-acting tendencies, in a passage that deserves to be quoted in full:

1. The costs of selling are raised, and the distributive system is expanded beyond what is socially necessary. This in turn has the following consequences:

- (a) Monopoly extra profits are reduced, in many cases to no more than the competitive level.
- (b) New segments of surplus value are created, and a large number of unproductive consumers are brought into existence. Therefore the rate of accumulation is reduced, and the rate of consumption increased. This acts as an offsetting force to the tendency to underconsumption.
- (c) The new middle class which provided social and political support for the capitalist class is enlarged. (*ibid.*, pp. 285–286)

But Sweezy is quick to note that this is ‘not a case of simple cancellation of opposed forces’. On the contrary:

The growth of the distributive system under monopoly eases the difficulty and softens the contradictions, but it does so not by making it possible for capitalism to harness the expanding productive forces, but rather by diverting their use into socially unnecessary and hence wasteful channels. There is an important difference here which should not be overlooked. When it is appreciated, the ‘favorable’ effects of monopoly appear in anything but a favourable light (*ibid.*, p. 286).

There is no mention of Baran, still less of Preobrazhensky, in these two chapters. By far the most references are those made to Rudolf Hilferding, who is praised for his analysis of the modern corporation and the rise of cartels, trusts and mergers but is also strongly criticised for exaggerating the role of financial capital in the new stage of monopoly capitalism. On this question, Sweezy maintains, his position is ‘fundamentally mistaken. Hilferding mistakes a transitional phase of capitalist development for a lasting trend’ (*ibid.*, p. 267). This is a serious error, since it ‘precludes an understanding of the most important recent changes in the character of the accumulation process, particularly the growth of internal corporate financing’ (*ibid.*, p. 268). Apart from the expected references to Marx, Engels and Lenin, the only other authorities that Sweezy cites in this context are two publications by the Temporary National Economic Committee, a book by Henryk Grossmann, a 1941 paper by Sweezy himself and a volume published by the Twentieth Century Fund entitled *Does Distribution Cost Too Much?* (*ibid.*, pp. 262n, 268n, 282). He also acknowledges comments from the sociologist Robert K. Merton on the possibility that innovations may be bypassed in monopoly capitalism ‘because of the absence of competitive pressures to introduce them as they became available’ (*ibid.*, p. 276n). But, to repeat, there is no reference here to either Baran or Preobrazhensky.

Baran added a great deal to Sweezy’s, 1942 analysis in the two long chapters that he devoted to monopoly capital in *The Political Economy of Growth* (107 pages in total, compared to Sweezy’s 33 pages), drawing on sources that were not available to Sweezy, including recent work by Kalecki (1954) and Steindl (1952). He began Chap. 3 by suggesting that the loss in output due to unemployment and excess capacity was much greater in twentieth-century capitalism than it had been in the nineteenth century (Baran, 1957 [1973], p. 169). Citing Kalecki, he maintained that the increasing degree of monopoly had affected the distribution of profits among capitalist enterprises rather than raising the profit share in total output. ‘Thus with the growth and propagation of large-scale enterprise, monopoly, and oligopoly, the

distribution of the economic surplus has become incomparably more uneven than in the age of small competitive business' (*ibid.*, p. 178).

One consequence of this 'strikingly uneven distribution of profits' was that 'only a relatively small share of the aggregate economic surplus' had gone into capitalist consumption (*ibid.*, p. 179). Matters were made much worse by what Alvin Hansen had described—but had been unable adequately to explain—as 'the problem of inadequate investment outlets' (*ibid.*, p. 182), itself the result of the takeover by monopolists of previously competitive industries and the resulting disincentive for increased production. Thus 'the volume of investment tends to be smaller than the volume of the economic surplus that would be forthcoming under full employment. There is consequently a tendency towards underemployment and stagnation, a tendency towards overproduction that was precisely identified by Marx a hundred years ago' (*ibid.* p. 207).

Chapter 4 begins with Baran's assertion that 'the requisite incentives for a profitable utilization of the currently generated economic surplus ... can only be provided by the state' (*ibid.*, p. 215). He continues by exploring 'the acceptance on the part of monopoly capital of the so-called full employment policy', which has created 'an atmosphere of unanimity on the political scene' (*ibid.*, p. 223). But this is an illusion. While there is general agreement on the need to avoid major catastrophes like the crash of 1929–1933, monopoly capital's version of the full employment policy.

... does not aim at the elimination of 'normal' crises or at [the] abolition of 'normal' unemployment. These in fact are considered to be 'healthy readjustments', desirable not merely for the preservation of the indispensable industrial reserve army but also as welcome conditions under which monopolistic and oligopolistic firms can pick up bargains, swallow up weaker competitors, and consolidate their market condition. (*ibid.*, p. 228)

Monopoly capital was also opposed to an extension of the welfare state to provide generous income maintenance for the poor, which would boost consumption expenditure but would also be 'wholly alien to the fundamental system of ethics and values associated with the capitalist system' (*ibid.*, p. 231), quite apart from the increased tax burden that it would impose on the rich.

Even if there were to be a substantial increase in civilian government spending, Baran concluded, the benefits would only be temporary. Capacity would inevitably increase faster than was warranted by the growth of aggregate demand, reinstating the original problem 'in a magnified and more acute form' (*ibid.*, p. 252). Increasing military expenditure might avoid these dangers, but only at the catastrophic risk of nuclear war. Baran ends the chapter by quoting Keynes on the possibility that securing full employment might greatly reduce the economic grounds for conflict between nations, and Joan Robinson on the abolition of capitalism as both a necessary and a sufficient condition for such favourable outcomes (*ibid.*, pp. 263–264, citing Keynes 1936, p. 382 and Robinson, 1936, p. 693).

This raises another important question: what precisely was Baran's attitude towards Keynes? Sweezy had described the *General Theory* as 'undoubtedly the most important work by an English economist since Ricardo's *Principles*' and devoted several pages to an appendix comparing Keynes's theoretical framework with that of

Marx's reproduction schema (Sweezy, 1942, pp. 52n, 371–374), but otherwise had very little to say in the body of the text about Keynesian macroeconomics. Baran's most substantial discussion of Keynes came early in the opening chapter of *The Political Economy of Growth*, where he drew a parallel between the Cambridge economist and a celebrated German philosopher:

At the risk of grossly exaggerating the intellectual performance of Keynes, it might be said that what Hegel accomplished with respect to German classical philosophy, Keynes achieved with regard to neo-classical economics. Operating with the customary tools of conventional theory, remaining well within the confines of 'pure economics', faithfully refraining from considering the socio-economic process as a whole, the Keynesian analysis advanced to the very limits of bourgeois economic theorizing, and exploded its entire structure. Indeed, it amounted to an 'official' admission on the part of the 'Holy See' of conventional economics that instability, a strong tendency towards stagnation, chronic under-utilization of human and material resources, are inherent in the capitalist system. It implicitly repudiated the zealously regarded 'purity' of academic economics by revealing the paramount importance for the comprehension of the economic process of the structure of society, the relations of classes, the distribution of income, the role of the state, and other 'exogenous' factors (Baran, 1957 [1973], p. 115)

The remaining references to Keynes add little to this rather faint praise, with the only substantial comment being that 'the so-called Keynesian Revolution' had in its 'discussion of the theory of income and employment bypassed, so to speak, the problem of the impact of monopoly and oligopoly on the volume and the long-run effect of investment' (*ibid.*, p. 181).

And yet, as Bob Sutcliffe notes, the liberal American economist Martin Bronfenbrenner once described Baran as 'a Keynesian of the Left'. Sutcliffe concurred, arguing that 'Baran accepts much of Keynes's analysis, not finding it in conflict with his own' (Sutcliffe, 1973, p. 93). Crucially, 'if surplus creation (saving) outstripped the intentions of business to invest, then output and employment would fall; he agreed with Keynes that this was the normal situation' (*ibid.*, p. 94). However, Baran objected strongly both to Keynes's preoccupation with the short period and also, as we have seen, to his failure to trace deficiencies in effective demand back to 'the growth of monopoly capitalism into an all-embracing system and the effects of growing monopolization on technical progress and growth' (*ibid.*, p. 94). The two brief references to Keynes in *Monopoly Capital* are also distinctly unenthusiastic (Baran & Sweezy, 1966, pp. 65–146).

## 6 A Critical Appraisal

The first thing that needs to be said is that Baran's work, on his own and in cooperation with Paul Sweezy, constituted one of the most original and important contributions to Marxian political economy that were made, anywhere in the world, between 1940 and 1965, and has been widely recognised as such. Second, his extension of the concept of primitive accumulation from the Soviet case analysed by Preobrazhensky to the economic development of all poor countries makes a great deal of sense. It was

an original and perceptive way to formulate the options for rapid industrialisation that were confronted by vast areas of the world in the 1930s and early 1940s, when large-scale capital imports were not available to them.

By the same token, this points directly to one of the most important criticisms that have been made of Baran's analysis. He failed to anticipate the globalisation of capitalism in the second half of the twentieth century, which offered new opportunities for the poor countries (and also, of course, significant dangers). Globalisation presented a serious challenge to his analysis of monopoly capital, since it greatly increased the degree of competition in all markets that were actually or potentially open to imports, or that offered export opportunities. The single most telling objection to the entire monopoly capital approach, in fact, was precisely that Baran and Sweezy had generalised too readily from the experience of the 1930s, which should have been seen more as an aberration than as a new stage in the history of capitalism. Twentieth-century capitalism was unstable, exploitative and environmentally damaging, but it was not prone to stagnation. In 2000 world GDP was roughly ten times as large as it had been in 1900. This was not a stagnant system!

Critics also identified two fundamental theoretical weaknesses in the Baran and Sweezy analysis of monopoly capital (Howard & King, 1992, pp. 120–124). First, they had significantly underestimated the strength of the competitive forces in advanced capitalist economies, even before the accelerated globalisation of the post-1970 period. Few if any industries were closed to potential competition, with the threat of cross-entry by firms from other industries imposing severe restrictions on pricing decisions and profit margins. My first boss at the University of Lancaster, Philip Andrews, developed a very influential theory of competitive oligopoly, in which price mark-ups were constrained precisely by existing firms' anxiety to deter potential entrants (King, 2021). Second, critics objected that Baran and Sweezy had never responded effectively to the Keynesian—and especially the Kalecki-inspired Post Keynesian—models of aggregate demand in a two-class capitalist economy. They had therefore inevitably failed to demonstrate that effective demand would necessarily lag behind the growth of potential output. For this, they needed precise and testable models of workers' and capitalists' consumption expenditure, and of aggregate investment, which they had never provided.

Defenders of the monopoly capital approach would respond by arguing that Baran had indeed been asking the right questions, even if he did not always come up with convincing answers. His work, and also that of Preobrazhensky, can be seen as a vindication of Marxian political economy, though more as a method of conducting research in the social sciences than a precise set of doctrines. Causation runs primarily from changes in the forces of production, through changes in the relations of production, to new political ideas and political movements. Capitalism is inherently unstable, if also profoundly dynamic. Class conflict is unavoidable, both within national economies and (through the various mechanisms of surplus transfer) between them, and it reacts back on them by inducing further changes in the forces and the relations of production. Baran would probably not have been surprised by the massive increase in inequality in the distribution of income and wealth that has occurred in the United

States in recent decades, much less by the growing political and ideological power of the super-rich.

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# Evsey Domar and Russia



Mauro Boianovsky

## 1 Domar's Russian Heritage

In August 1936, Joshua Domashevitsky arrived at the port of Los Angeles to study economics in the USA, after crossing the Pacific Ocean on a Japanese vessel that had left from Kobe. The point of departure of his journey was the city of Harbin in Manchuria, where his family had settled in 1916 coming from Lodz (now a Polish city, then Russian), where he was born in 1914 to Sarah and David Domashevitsky, a small-scale businessman (importer) who regarded himself a social democrat Menshevik (Domar, 1992; Johnson & Ley, 2013). Joshua would change his name to Evsey David Domar upon migrating to America, where he received a B.A. in economics at the University of California at Los Angeles (1939), followed by an M.A. in mathematical statistics from Michigan University (1941) and a Ph.D. in economics from Harvard University in 1947.

By that time, under the influence of Alvin Hansen, his Ph.D. advisor and a leading Keynesian economist, Domar was already well known due to his seminal contributions, together with, but independently from, Roy Harrod (1939, 1948) to the founding of growth economics as a new research field in Keynesian macroeconomics, expressed in the so-called Harrod–Domar growth model (Domar [1946] 1957, [1947] 1957; see Boianovsky, 2017, 2021a). Domar became an American citizen in 1942; he lived in that country until his ultimate death in Massachusetts in 1997. Domar's main appointments throughout his professional lifetime were held at the departments of economics of Johns Hopkins University from 1948 to 1958 and of the Massachusetts Institute of Technology (MIT) from 1958 until retirement in 1984, with shorter periods at the Federal Reserve System (1943–46), the department

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of economics and the Cowles Commission at the University of Chicago (1947–48) and as Visiting Professor at Brandeis University (1986–1990).

Evsy Domar was among interwar period émigrés who “took readily to the American scene” and made key contributions to the creation of a “single international ‘mainstream’ economics” increasingly dominated by American economic research (see Craver & Leijonhufvud, 1987: 175). Indeed, Domar was one of the protagonists of the Keynesian avalanche that took the economic profession by the storm in the USA and other countries from 1930 to 1950s (Colander & Landreth, 1996; Domar, 1996). Most of his essays on Keynesian growth economics, including one drafted in 1944 on the dynamics of public debt, were collected in Domar (1957).

Nevertheless, that does not mean that Domar did not address Russian or Soviet topics, especially after the early 1950s. In fact, from 1949 to 1951, he served as Director of Russian Studies, Operations Research Office, at Johns Hopkins University (1949–1951), and from 1951 to 1955 lectured as Visiting Professor at the Russian Institute of Columbia University. As put by Domar (1950b: 75), “the study of the Soviet Union is a most fascinating task, because it challenges our established set of ideas and gives rise to new ones”. In 1956, Robert Solow invited Domar to come to MIT as Visiting Professor.

We would love to have you teach a graduate course in Russian Economics. It’s something we can’t offer ourselves, and I think there would be a lively interest. It could fit in our Economic Development sequence, or almost anywhere else, depending on what you decide to cover. (Solow, 1956)

That was also the year Solow published his neoclassical growth model, which would soon dominate growth economics, a topic he and Paul Samuelson taught MIT graduate students at the time. Domar accepted the invitation and eventually took up a permanent position at MIT. He kept his interest in growth and development economics (which he lectured at MIT at undergraduate level in the 1960s), but it was above all Russian and Soviet economic performances and history that attracted his attention. The shift in Domar’s research agenda is clearly displayed in his second and last collection of essays, which featured papers on comparative economic systems, socialism and Russian nineteenth-century history, written from 1960 to 1980s (Domar, 1989).

The Cold War context goes a long way explaining the Western demand for economic expertise about the Soviet Union in the post-war period (or even during World War II), which became known as Sovietology (see, e.g. Engerman, 2010; apart from economists, Sovietology engaged several political scientists). Many economists involved were Russian (or Ukrainian) émigrés, including Alexander Gerschenkron (1904–1978), Gregory Grossman (1921–2014) and Alexander Erlich (1913–1985). Of course, a number of important Sovietologists were not émigrés (e.g. Abram Bergson), and some prominent émigrés did not produce much about the Soviet Union or Russia throughout their careers (e.g. Jacob Marschak, Simon Kuznets and Wassily Leontief). Gregory Grossman (born in Kiev) spent part of his youth in Manchuria, like Domar, before emigrating to the USA in 1937.

Harvard economic historian Gerschenkron was particularly influential as a leading Sovietologist and economic historian of Russia, partly through the Russian Research Centre at Harvard University, which funded the research that resulted in Domar's (1950a) article on the sometime leading Stalinist Hungarian-Soviet economist Eugen (Jenö) Varga. Domar was a member of that network, even if he did not regard himself a Sovietologist (Domar, 1989: xi). Indeed, it was Grossman (then Gerschenkron's student) who first called Domar's attention in the early 1950s to Soviet engineer and economist Grigory A. Feldman's (1884–1958) mathematical growth model of 1928, based on Marx's reproduction schemes and intended as a theoretical foundation for the long-range planning of the Soviet economy (Domar, 1952: 480, n. 3; 1957b: 223, n. 1). Feldman and his model had fallen into oblivion in the USSR and the West after the early 1930s and throughout the Stalinist era, until Domar (1957b) rediscovered and formally restated it.

Likewise, it was through another member of the Russian studies and Sovietology network, Steven Rosefielde (then Bergson's student at Harvard) that Domar (1966a: 735, n.3) heard about the Ukrainian/Russian economist Tugan-Baranovsky's ([1915] 1921) pioneer theoretical and empirical study of producer cooperatives, currently called "Labour-Managed Firms", a topic Domar (1966a) discussed in detail in his path-breaking article on Soviet collective farms (*kolkhozes*). Moreover, Erlich's (1950) article about Evgeni Preobrazhensky (1886–1937) and the seminal 1920s Soviet industrialization debates brought the work of that Russian economist – which Domar (1966b) found even more interesting than Feldman's in some respects – to Domar's attention.

The search for Domar's Russian heritage has focused so far on some formal similarities between Feldman's (1928) and the Harrod–Domar growth models. From the fact that Domar was born in the Russian Empire and lived in a Russian town until 1936, some authors have inferred that the similarities were not a coincidence, but the result of Domar's familiarity with the Russian 1920s economic literature on growth, supposedly acquired in Manchuria before he emigrated and elaborated in his 1940s growth models. Hence, Barnett (2008a: 4) has suggested that "Domar's [1940s] growth theory ... owed an important debt to [its] Russian origins". According to Easterly (2001: 31), "the Soviet experience inspired the Harrod-Domar model". Similarly, Boettke (1990: 147) has stated that the formulation of the Harrod–Domar model was "directly influenced by the Soviet [1920s] discussion and later experience". Screpanti and Zamagni (2005: 315) went as far as asserting that "Domar was one of the few contemporaries of Feldman who appreciated his work, from which he was inspired to construct his own model" in the 1940s.

Such claims, however, are historically inaccurate and make no sense whatsoever. They disregard the fact that Domar only got to know about Feldman's model (and the 1920s Soviet industrialization debates in general) around 1952, which led to his 1957b essay with its central point that Feldman (1928) differed from Domar's (1946, 1947) and Harrod's (1939) models in some key aspects. Domar was a teenager when the industrialization debates took place in Moscow. He did attend for a semester lectures at the Economics Department of Harbin's State Faculty of Law in 1930–31, when he learned basic statistics, and notions of European history, accounting

and law, but no economic theory (Domar, 1992, 1996). Domar's (1946, 1947) and Harrod's (1939) growth models tackled dynamic issues of capitalist economies from a Keynesian perspective, with no influence of or inspiration from Soviet theoretical debates and planning experiences. Whereas Domar, due to his Russian background, would later become attracted to those 1920s debates in the context of the 1950s/60s development economics, Harrod never showed interest on Feldman and the Soviet literature on growth planning (see Boianovsky, 2018).

The Russian heritage that Domar brought with him to America in 1936 should be sought elsewhere, not in some alleged Soviet influences on his 1940s growth models. As Domar (1992, 1996) recollected, during his youth in Manchuria – in the cities of Harbin and Dairen, where he moved to in 1934 to work as an accountant – he became attracted to history as a subject and to socialism as a political-economic system. The “first love of my youth was, and now of my old age is, history” (Domar, 1992: 118). Domar's interest in history, upon reading Russian novelists such as Leon Tolstoy, would take him to economics as a key to understanding society and its history. As for socialism, Domar explained how.

To a person of Russian background, socialism has a significance that an American reader may not appreciate. In tsarist Russia, being a socialist made one an automatic opponent of autocracy; it was almost required for maintaining a minimum degree of self-respect ... To my relatives, as to many millions of others, socialism was a secular religion, the great hope for a better world. Harbin was too close to the Soviet Union to escape its influences. After my graduation from high school, I made good use of the socialist collection that our modern library possessed. My interest in socialism never disappeared, but my serious work dates back to the fifties. (Domar, 1992: 126)

Harbin Central Library was directed by Nikolai Ustrialov, a professor at the Faculty of Law, regarded by Domar (1992: 120) as one of his three “great teachers” (the others were Schumpeter and Viner). Ustrialov (1890–1937) was a leading pioneer of National Bolshevism who, after a period of exile in Harbin, returned to the Soviet Union in 1935. In 1937, during Stalin's Great Purge, he was sentenced to death. Harbin was founded by Russians in 1898, a couple of years after the Russian government obtained permission from the Chinese to build, across Northern Manchuria, a continuation of the Trans-Siberian Railroad to Vladivostok (Domar, 1996: 180). It soon became a boomtown that attracted many immigrants, comparable in its growth to St Petersburg, or San Francisco during the Gold Rush. By the time of the 1917 October Revolution, its Russian-speaking population reached around 127,000 people (Karlinsky, 1989: 284–85). Around 1920, the Chinese took over the administration of Harbin, but it remained well into the 1940s essentially a Russian town from both cultural and economic perspectives, with a high, cosmopolitan standard of living.

The young Domar and his émigré family found in Harbin in 1916 an “almost totally Russian city, populated mainly by people with roots in the south of European Russia” (Karlinsky, 1989: 285). Distinguished Russian, Swiss and Italian architects planned its urban landscape. The Domachevitsky family's expectations when they fled Lodz were confirmed as Harbin escaped such turbulent events as the October Revolution, the civil war and Stalin's collectivization. Opera and theatre attracted renowned international artists, which, together with a large number of high-quality

Russian schools, periodicals, art galleries and libraries, attested to the “outstanding intellectual level of the multinational Harbin community” (Karlinsky, 1989: 286) in which Evsey Domar grew up.

Barnett (2008b) has examined a small group of prominent Russian émigré economists who came to the USA. (Marschak, Kuznets and Leontief) to argue that their intellectual Russian “baggage” and influences played significant roles in the initial stages of their American careers, but not later. In the case of Domar (who, like Gerschenkron, is not mentioned by Barnett, 2008b), on the other hand, his Russian heritage became increasingly relevant as he progressed through the mature stages of his work as an economist. The themes of the working of the Soviet socialist economy and of the origins and role of serfdom in pre-socialist Russia became dominant in his agenda since the mid-1950s (Domar & Machina, 1984). As one of the representatives of the new generation of economic model builders in the 1940s (see Boianovsky, 2017), Domar would produce some of his most mathematically sophisticated models as part of his treatment of economic growth and efficient allocation under socialism (Domar 1957b; 1966a; 1974).

Domar would return to the Soviet Union for the first time, for an academic visit, in October–November 1959, when he met a group of economists at the Academy of Sciences in Moscow, among other appointments and travels to some other main Soviet cities. His travel impressions were recorded in a lecture delivered at Amherst University (Mass.) shortly after. “For one time”, he reported, “I talked to a man who did not throw Marxism at me” (Domar, 1959a).

He noticed some positive signs of change, as compared to the poverty of Soviet economics in the Stalinist era (Stalin had died in 1953). “It’s only now that Soviet economists have become interested on linear programming and input–output matters, invented in the USSR” (ibid). Domar probably had in mind recent works by Kantorovich, Novozhilov, among other prominent Soviet mathematical economists. Leontief (1960), who visited the USSR earlier that same year, shared that opinion (see Campbell, 1961 for a contemporary assessment, and Wagener, 1998 on the general poverty of Soviet socialist economics). A few years later, Domar attended a conference on Mathematical Techniques and Soviet Planning, held at the University of Rochester in May 1965, which gathered a select group of American, but not Soviet economists (see Hardt, 1965).

Domar played a significant role as a link between Russian (or Soviet) and West economics. Those were especially the cases of Domar’s (1957b) restatement of Feldman’s (1928) two-sector growth model with non-shiftable capital, and of his 1970 well-known thesis that the economic basis for the introduction of coerced labour serfdom or slavery was a low land-to-labour cost. Domar (1970) formally elaborated a hypothesis advanced by Russian historian Vasily Kliuchevsky (1841–1911) in his classic [1906] 1937 account of Russian serfdom in the sixteenth and seventeenth centuries. Domar was also instrumental in bringing to the attention of Western economists the “Varga controversy” of 1947–1949 as an illustration of the overall poverty of Stalinist economics (Domar, 1950a).

Together with the Feldman essay (Domar, 1957b), Domar’s (1966a, 1974) articles on self-management and incentives mechanisms constituted key contributions to

the theory of the working of socialist economies (Wagener, 1998). Domar (1966a) noticed some important similarities between his analysis and Tugan-Baranowsky's ([1915] 1921) work on cooperatives. The starting point of Domar's, 1974 article was Premier Alexey Kosygin's famous announcement on 27 September 1965 of Soviet Economic Reforms, including an emphasis on efficiency and profits by socialist firms. As discussed below, the consumption of Domar's economic ideas by Soviet economists was generally restricted to aspects of his growth models – particularly Domar's ([1953] 1957) analysis of depreciation in growing economies – and his 1957b “Soviet model”, which eventually brought Feldman's model to light in the USSR in the late 1960s after decades of silence.

## 2 The Varga Controversy, Soviet Economics and Underconsumption

Domar's (1950a) detailed account of the “Varga controversy” was his first foray into Soviet economics, written when he was Director of Russian Studies at Johns Hopkins. Varga – Director of the Moscow Institute of World Economy and World Politics from 1927 to 1947, a prominent Marxian economist and the Soviet Union's most influential analyst of the capitalist world economy during that period – has been described as the 1930–40s “Stalin's economist” (Mommen, 2011). Varga's, 1946 book on *Changes in the economy of capitalism resulting from the Second World War* was subjected to public discussion and criticism in May 1947 in Moscow. The book was charged as anti-Marxist and revisionist. That was followed by several critical articles in the Soviet economic journal *Voprosy Ekonomiki*, founded in 1929 as *Problemy Ekonomiki* and renamed in 1948, and on Soviet press, together with discussions in international newspapers and journals.

The controversy turned into a *cause celebre*, as put by Domar (1950: 132). After some resistance and attempts to defend himself, Varga eventually recanted in 1949 his views of post-war capitalism. Domar's 1950 *AER* article became the main contemporary economic source about the Varga affair. Domar (1950a: 133) focused on parts of Varga's, 1946 book, on some other writings by Varga and on aspects of the debate that “may be of particular interest to American economists”.

Varga made two main predictions in his 1946 book and in the public debates that followed its publication. He argued that world capitalism would last for at least another decade, when it would be hit by a major overproduction crisis caused by excessive capital accumulation in relation to consumption demand. His second prediction – which partly contradicted the former one – was that the war had brought about significant changes in the ability of capitalist states to engage in economic planning. Both predictions raised criticism from other Soviet economists, who complained about the low theoretical standards, not just of Varga's book, but of Soviet economics in general (Domar, 1950a: 143, 150). However, no analysis was provided during the debates about the causes of that “strange intellectual timidity among

a people who in other fields, such as mathematics, have achieved most admirable results” (150). According to Domar, the problem came mostly from the requirements imposed on economic theoretical work in the USSR at the time.

It must conform to a theoretical structure whose creator died in 1883, and to the interpretation of this structure by Lenin, now gone some twenty-five years. Further, no statement may challenge any article or pronouncement by Stalin ... These three sets of restrictions do not result in many [degrees of freedom] left. Domar (1950: 143)

As discussed by Domar (1950a: 136–139), Varga based his thesis that capitalism was moving towards its end on two Marxist laws: the effect of the (relative) impoverishment of the proletariat on underconsumption and the declining rate of profit. However, Domar contended, Varga did not maintain those “laws” consistently. Domar wrote the definition of the rate of profit as  $\pi = P/K$ , where  $P$  and  $K$  are total profit and capital stock.  $P$  is a fraction  $\gamma$  of national income  $Y$ , so that:

$$\pi = P/K = \frac{\gamma}{K/Y}$$

A decline in  $\pi$  would result from either a lower  $\gamma$ , a higher  $K/Y$  or both. However,  $\gamma$  was supposed to rise because of the “impoverishment” of workers. Hence, a falling  $\pi$  must depend on a rising  $K/Y$  due to marginal diminishing returns (or to increasing organic composition of capital for a Marxist economist). However, due to technical progress, Varga denied the relevance of the law of diminishing returns to capital accumulation. Domar agreed that capital deepening had not been an observed feature of the American economy. But, then, if  $K/Y$  is stable, a rising  $\gamma$  should bring about an increasing  $\pi$ , against Varga’s hypothesis. Again, American data indicated that  $\gamma$  had remained stable in the long run, and, consistently with Domar’s accounting identity, so had the rate of profit (Domar, 1952: 492).

Varga’s notion of over-accumulation of capital in relation to consumption was close to Paul Sweezy’s (1942, Chap. X) theory of underconsumption as the cause of the upper turning point, as Domar (1950a: 136) noticed. Domar probably had attended Sweezy’s lectures on Marxian economics at Harvard in the early 1940s. He would discuss Sweezy’s Marxian underconsumptionist model in some detail a few years later (Domar [1948] 1957, Sect. IV). Sweezy’s (1942) model was an attempted demonstration of the flaws of Mikhail Tugan-Baranovsky’s well-known argument – developed in the first chapter of his 1901 *Studien*, a revised German version of the first Russian edition of 1894 – about the logical impossibility of underconsumption crises. Curiously enough, Domar did not refer to Tugan-Baranovsky (1901), although he was certainly aware of it from Sweezy’s (1942, Chap. X) long quotations and critical assessment, and probably from Sweezy’s Harvard classes as well. Nevertheless, one may surmise that Domar ([1948] 1957: 109–10) was implicitly addressing Tugan’s point when rejecting Frank Knight’s (1944) similar assumption that the possibilities of capital deepening were unlimited and its corollary that profitable capital accumulation was unbounded.

Tugan-Baranovsky (1901) deployed, for the first time after Karl Marx, the expanded reproduction schemes in order to argue that “capitalist production creates

its own market” in the sense that, no matter how low consumption is, the supply of goods cannot exceed demand, provided correct “proportions” are kept between the various sectors of the economy. Hence, crises can only arise from partial overproduction, not from general excess supply (Tugan-Baranovsky [1901] 2002: 26). Domar did not have time for Tugan’s “anarchy of the market”. He probably agreed with his Harvard professor Schumpeter’s (1954: 1126, n. 9) assessment that the first chapter of Tugan-Baranovsky’s *Studien* was a “distinctly poor performance”.

However, Domar did share with Tugan-Baranovsky (1865–1919) the rejection of the notion that the purpose of all production is consumption. In a growing economy, investment for further investment is the rule (Domar [1948] 1957: 123; 1957b: 235). Hence, the capital-output ratio – instead of the capital-consumption ratio as in many accelerator models and in Sweezy’s (1942: 182) formulation – should be the relevant variable. Domar parted company with Tugan (and Knight), however, by assuming that capital deepening was limited and that the “capital coefficient” between output and capital was relatively stable. If that coefficient could be “anything”, there could be no over-accumulation of capital, as in Knight and Tugan, Domar [1948] 1957: 111) maintained.

Domar’s ([1948] 1957: 122–128) correction and reformulation of Sweezy’s (1942, Appendix to Chap. X) model “salvaged” underconsumption theory from Tugan’s previous attack (Howard & King, 1992: 121). Domar achieved that by deploying a stable capital-output ratio and applying the general framework of his 1946–47 growth model to show that a rising propensity to save (as assumed by Sweezy) was not necessary to produce excess increase of capacity in relation to the path of aggregate demand, which was Domar’s sense of “underconsumption”. Excess capacity resulted from an actual growth rate below the required equilibrium growth rate given by the output-capital ratio ( $\sigma$ ) times the saving ratio ( $\alpha$ ), as expressed by Domar’s famous formula  $r = \sigma \alpha$  (Boianovsky, 2017, 2021a). Hence, from Domar’s standpoint, underconsumption provided an important link in the history of macroeconomics, despite its often-imprecise formulation (see Boianovsky, 2021b).

### 3 Feldman, Preobrazhensky and Economic Development

Domar ([1946] 1957: 70; 1950b: 74; 1952: 480) often praised Marx and Marxian economists for their concern with economic growth and the relation between capital accumulation and employment. However, the development of a “substantial theory of economic growth” along Marxian lines had been delayed by the “time and effort” wasted by Marxian economists in “defending their master’s virtue” (Domar, 1952: 480). Domar (1957a: 12), in the foreword to his 1957 collection, described Marx as a “great sage” but a “poor theorist” and model-builder. That situation would change, at least temporarily, by the appearance of some “highly elaborate and interesting models” in the 1920s Soviet economic literature, especially by Feldman (1928). Such multi-sector growth models (based on Marxian reproduction schemes), Domar



(1952: 480, n. 1) pointed out, were “more fully developed than similar attempts made in the West, with the exception of Leontief’s works”.<sup>1</sup>

Before he got to know about Feldman’s, 1928 model around 1952, Domar (1950a: 140) had described Marx’s reproduction schemes, with their division of total output into producer goods (Department I) and consumption goods (Department II), as a “logical monstrosity” and a “stone axe”. The main problem, from Domar’s perspective, was the confusion between variable capital (a stock) and the payroll (a flow) and between constant capital (a stock) and gross investment and depreciation (flows). Sweezy (1950) reacted by pointing out that no stocks are involved in Marx’s schemes; all items are flows. Domar (1950c) accepted Sweezy’s point, but insisted that the results obtained from the schemes – such as the equilibrium condition that the demand for consumption goods coming from Department I must equal the demand for capital goods by Department II – may have been significant in nineteenth century economics, but were “hardly so” in the 1950s. Moreover, once the apparatus was applied to the theory of value and profit rate, the stock-flow issue came back, as witnessed by difficulties involving rates of turnover and production periods.

Domar (1950c: 407) claimed that problems with the Marxian schemes had prevented their general use. Preobrazhensky’s (1931) attempt was an exception, but he got lost in “hopelessly involved numerical examples” as he tried to incorporate into the schemes the corresponding allocation of output by expenditures (investment and consumption) in a growing economy (see also Domar, 1957b: 225, n. 5). Surprisingly, Domar did not mention Tugan-Baranovsky’s (1901) early numerical exercises with the schemes, even though Tugan’s results were not much better. Domar’s low opinion should dispel any notions that he was influenced by the Marxian scheme of expanded reproduction of capital when formulating his own 1946–47 growth models.

Nevertheless, upon studying carefully Feldman’s, 1928 Soviet growth model in the mid-1950s, Domar changed his mind about the usefulness of the Marxian schemes. Stock-flow problems persisted in computations of the rate of profit, but that was not a matter of concern for Feldman (1928) or Domar (1957b: 226, n. 5). For the first time, according to Domar, mathematics was applied to the reproduction schemes, which resulted in the first Marxian growth model ever. The Soviet engineer-economist modified the schema so that Department I included all activities that enlarged productive capacity, while Department II encompassed all activities that sustained the level of output, a division not always feasible to implement (Domar, 1957b: 225–227). Moreover, along Marx’s original schema, the existing capital stock could not be shifted from one sector to another, although the division of the investment flow between the two sectors was flexible. Therefore, the proportion of consumption and investment in total output was determined not by the propensity to save, but by the respective capital stocks and capital coefficients in each category. The choice of the current composition of output was dependent on the inherited structure of capital. The key variable determining economic growth was the capacity to produce capital goods in Department I, as determined by the fraction of total investment retained by that

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<sup>1</sup> See Hagemann, this volume, for a comparison between multi-sector growth models by Feldman, Maurice Dobb and Adolph Lowe.

sector. If such capacity is low, the *potential* propensity to save cannot be turned into investment and is wasted.

Domar was attracted to the ability of Feldman's model to illuminate the planning of capital accumulation as part of the economic development process. Unlike Domar's (and Harrod's) growth model, Feldman assumed a perfectly elastic labour supply and absence of effective demand constraints and business cycles in a planned socialist economy with two sectors. Again, differently from Domar and Harrod, that model was designed for the formulation of development *policy*. It led naturally, especially through Domar's (1957b) reconstruction, to the study of optimal growth paths. Feldman's (1928) formulation was long and often hard to follow. Domar (1957b) demonstrated rigorously Feldman's point that an increase of investment in Department I at the expense of investing in Department II will generate a permanently higher level and growth rate of consumption in the long run (= rate of growth of investment), after a gradual decrease of the rate of growth of consumption in the transition. Feldman's model was discussed and mentioned in Russia for the last time by Kovalevsky (1930), Feldman's colleague at Gosplan, the Soviet State Planning Commission (Domar, 1957b, Sect. V).

Domar's thorough restatement of Feldman's two-sector growth model with non-shiftable capital became quite influential, especially at MIT, the main centre of research of growth economics in the 1950 and 1960s. Ronald Findlay (1962: 85), for instance, thanked Domar for his MIT lectures on Soviet economics, which brought Feldman and Preobrazhensky to Findlay's attention. Domar (1957b) would soon be combined with new mathematical techniques – such as the Soviet mathematician Pontryagin's et al. (1962) maximum principle – in the then new literature on optimal growth. That is well illustrated by highly formal articles on optimal growth in multi-sector economies with non-shiftable capital, based on MIT Ph.D. theses, such as Weitzman (1971) and Bose (1968), with references to Domar (1957b) and to Feldman's model. Surely, the Marxian origins of Feldman's original model all but disappeared in those optimal growth approaches. The Marxian flavour was kept by a distinct group of authors influenced by Domar's (1957b) rediscovery, such as Maurice Dobb (1967).

It helped to spread the model the fact that the Indian planner Mahalanobis (1953) had independently developed a brief discrete-time version featuring some similarities with the Domar–Feldman continuous-time version, as Domar (1957b: 230, n. 16) noticed. References have sometimes been made to a “Domar–Feldman–Mahalanobis model” (e.g. Findlay 1966). The eventual translation of Feldman's original article (Feldman [1928], 1964) made it better known in the West, but that did not diminish interest in Domar's (1957b) restatement. Indeed, Nove and Nuti (1972) chose to reproduce Domar (1957b) in their collection of readings about socialism, instead of Feldman ([1928] 1964). Likewise, Jones' (1975, Chap. 5) careful textbook rendition of Feldman's growth model was based on Domar (1957b).

As Engerman (2010) has pointed out, essay on Feldman, as well as Sovietology in general, should be read in the context of the intense interest in development economics and economic development at the time. Domar (1967: 636), as part of a discussion on the occasion of the centenary of Marx's *Capital*, praised the reproduction schemes,

as modified by Feldman, as a main contribution to development planning of countries beset by limited capacity of the capital goods industry, such as the USSR in the 1920–1930s and developing countries in general. Besides Feldman (1928), there was yet another contemporary Soviet development model of Marxian extraction, articulated but not formalized by Preobrazhensky ([1926] 1964). It was also a two-sector model, but of a different sort: a socialist industrial sector drawing on the surplus of the peasant-owned agriculture, a situation Preobrazhensky called “primitive socialist accumulation” after Marx’s “primitive capitalist accumulation”. Domar (1967: 636) found Preobrazhensky’s 1926 implicit model even “more interesting” than Feldman’s formulation.

Preobrazhensky’s ideas on development had been introduced into Western economics by Erlich (1950) and, at MIT in particular, by Domar’s classes. The 1965 translation of his 1926 book added to the interest. In an insightful review-essay, Domar (1966b: 252) remarked that “for all its virtues”, Feldman’s model had “one basic defect: it was concerned with capital only and completely excluded labour”, as Feldman did not regard labour as a scarce limiting factor. Even under the assumption of unlimited supply of labour – a concept later turned into the backbone of development economics by Lewis (1954); see Boianovsky, 2019 – “workers had to be fed, clothed and housed” (Domar, 1966b: 253).

Like many other developing countries, modern industry in 1920s Soviet Union was “but an island in a peasant sea”. The inter-relations between the industrial and agricultural sectors comprised a main economic issue that could be tackled in ways analogous to the terms-of-trade problem between a domestic (industry sector) and a foreign (agriculture sector) country (Domar, 1966b: 255). Every extra rouble obtained from peasants, in exchange for manufactured goods, could be used to feed a larger industrial labour force and speed up capital accumulation. The essence of Preobrazhensky’s problem was, according to Domar (1966b: 253), to decide how the Soviet government should deal with the peasants in order to maximize industrial output.

Domar (1957b: 229, 245) had already observed that “production is independent of consumption” in Feldman (1928), who excluded the effect of consumption standards on the “ability and incentive” of people to work and on their “willingness to obey”. Analytically, whereas the Domar–Feldman model was an “open” model – in the sense that consumer goods only play a role as component of final demand – Preobrazhensky’s formulation pointed to a “closed” model, meaning that consumer wage goods are treated as inputs that determine labour supply, as in classical economics. Rigorous “closed” multi-sector models, such as von Neumann’s well-known general equilibrium growth model, do not feature primary production factors. Arthur Lewis (1954) shared that notion, although only implicitly (see Feldman 1966; Boianovsky, 2019). Preobrazhensky did not develop a formal model; Domar (1966b: 253) indicated how such a model should be built, referring to Findlay (1966).

Models incorporating Feldman’s and/or Preobrazhensky’s assumptions were “popular” among Domar’s MIT graduate students in the 1960s (Domar, 1967: 636). Dixit (1969) provided a first formalization of the problem of the marketable surplus

and growth in dual economies, partly under Preobrazhensky's inspiration. Domar's lectures and writings on the 1924–1928 Soviet industrialization debates continued to bear fruits later on. Another former MIT student from the 1960s would take up the issue again in the 1980s: in articles with Sah, Joseph Stiglitz discussed in detail under what conditions Preobrazhensky's proposition, that the socialist state should increase its surplus and capital accumulation by turning the terms of trade against the peasants, was valid (Sah & Stiglitz, 1984, 1986).

#### 4 Tugan-Baranovsky, Cooperatives and Incentives Under Socialism

The Soviet policy of fast industrialization through intense capital accumulation and transfer of peasants into industry, suggested by Feldman and Preobrazhensky, could be deduced from Marx. After the end of the stage of “primary socialist accumulation”, however, further economic growth of the USSR depended on the planning of efficient resource allocation. But here, Domar (1967: 637) pointed out, “there is practically nothing that the Russians can take from Marx”. He anticipated that Russian economists would continue to “venerate [Marx] in word and disregard him in deed”, a process that had already begun in the mid-1960s as they increasingly managed the Soviet economy by means of prices and profit. Domar was implicitly referring to Premier A. Kosygin's announcement of the 1965 economic reforms, a process that featured Liberman (1965) and other Russian economists.

The central issue, according to Domar, was the incentives mechanism to induce managers of socialist quasi-monopolistic companies to achieve efficiency. Inspired by the Soviet reforms, Domar (1974) would put forward a bonus plan formalized in a detailed mathematical model. He did not mention Liberman or other Russian economists, but instead referred readers to Felker's (1966) overview of the economic discussions preceding the reforms. From Domar's standpoint, the main theoretical reference was Oscar Lange's (1936–37) classic essay on socialist planning. Domar (1949) had been long attracted to that topic. He accepted Lange's result that socialist managers should be instructed to equate marginal costs to prices in order to produce optimal quantities of goods. But, “what incentives will society offer to make them behave in this manner? And what method will be used to determine that they so behave?” asked Domar (1949: 174). Lange and economic theory in general did not provide an answer to that question. It took Domar (1974) some time to provide – as provoked by the Soviet reforms – a first full-fledged model showing that the dependence of price-setting managers' bonuses on a weighted sum of profits and revenue pushes monopolies towards marginal cost pricing through an iterative process (see Persky, 1991; Tam 1980).

Apart from his 1974 piece, Domar produced yet another microeconomic paper related to allocative efficiency under socialism. Instead of monopolistic behaviour as in 1974, Domar (1966a) tackled the economics of producer cooperatives in a

competitive environment. His mathematical model was supposed to apply, under ideal conditions, to the working of Soviet collective farms, which he had visited in 1959 (Domar, 1959a). He was inspired by Benjamin Ward's (1958) pioneer essay on theoretical aspects of the Yugoslavian experience with cooperatives (based on Ward's Ph.D. thesis supervised by Grossman). Apart from Ward, Tugan-Baranovsky ([1915] 1921) was the only theoretical work on the topic mentioned by Domar (1966a). Tugan's long book on the *Social basis of cooperation*, although a classic, has never been discussed in any detail in the literature, probably because it is only available in Russian (Rapoport's, 1918 review is informative but not strong on the analytical side).

Domar (1966a: 735, n. 3) informed that a "very interesting book on cooperatives was published by Tugan-Baranovsky. His conclusions were very similar to mine..." Apparently, Domar had a better opinion of Tugan as an expert on cooperatives than as a macroeconomist (see Sect. 2 above). Domar's (1966a) article – an influential contribution to the economics of property rights in general – effectively launched the new field of "Labour-Managed Firms" (LMF) (see Bonin & Putterman, 1987; Putterman, 2008). Domar followed Ward in assuming that the objective of LMF was to maximize revenue per worker net of other charges. Ward (1958) had established the surprising result that, with labour as the only variable input, the LMF would respond to a price increase by reducing optimal employment and output level. Domar (1966a) generalized Ward's analysis for any number of inputs and outputs, but proved that a perverse reaction by a cooperative to higher prices was unlikely. Domar's results followed from his investigation of the effects of the incorporation of labour supply in the model, together with the presence of other variables inputs besides labour, and the reallocation of labour between different outputs of the LMF.

However, if Domar's analysis solved the problem of Ward's paradoxical conclusions, it reaffirmed the puzzle of why, if cooperatives were democratic efficient arrangements, were they relatively rare. Tugan-Baranovsky ([1915] 1921) had anticipated as much. "Has Tugan-Baranovsky's pessimistic prognosis (of 1921) been vindicated? Does co-op democracy interfere with efficient management?" asked Domar (1989: xvi) when his 1966 article was reprinted. According to Tugan-Baranovsky ([1915] 1921: 249–250), successful cooperatives have an incentive to substitute non-members hired workers for retiring members, leading to a concentration of profits by a reduced member group, until it collapses to a capitalist firm with just one member (the owner):

The better the business of the producer cooperative, the more numerous becomes the group of hired wage labourers. This process comes to its conclusion when the members of the cooperative ... stop working themselves and become shareholders of the enterprise. Absolutely nothing remains of the producer cooperative – in its place grows a capitalist enterprise. (Tugan-Baranovsky, [1915] 1921: 249; quoted from Ben-Ner, 1984: 249).

Domar brought Tugan's analysis and forecast to the attention of Ben-Ner (1984: 249), who formalized the cooperatives instability process. After that, references to Tugan-Baranovsky ([1915] 1921) have become common in the LMF literature, although his book, due to linguistic barriers, has remained largely unread in the West.

## 5 Russian Serfdom and Factor Endowments

The central institution in the history of pre-socialist Russia was agricultural serfdom. Although serfs were emancipated in 1861, serfdom continued to influence the Russian economy and society until much later. In 1970, Domar put forward a path-breaking model of coerced labour, applicable not just to serfdom but to American slavery as well. “The causes of slavery or serfdom: a hypothesis” became one of Domar’s most influential papers, second only to his 1946–47 pieces on growth economics. That paper represented the culmination of Domar’s passion for history, born during his days in Harbin. Domar (1970) resulted from reading Kliuchevsky’s ([1906] 1937) analytical historical account of Russian serfdom. He started working on the ideas for the paper and teaching on the topic shortly after his wife presented him in the 1950s with the five volumes of Kliuchevsky’s “Course of Russian History”. Domar (1970) brought Kliuchevsky – whom he regarded as “the greatest Russian historian” – to the attention of economic historians worldwide (Domar, 1992: 125).

As brilliant and suggestive as Kliuchevsky’s description of serfdom was, the Russian historian (“being a historian and not an economist”, as put by Domar, 1992: 125), did not elaborate a model to explain its causes, which Domar (1970) set out to do. From a broad perspective, Domar’s, 1970 paper shared with his 1966 and 1974 analytical pieces on socialism a concern with general aspects of property rights. Summing up Kliuchevsky’s account, Domar (1970: 18–19) noted that after mid-sixteenth century, “as the central areas of the [Russian] state became depopulated because of peasant migration into the newly conquered areas in the east and southwest ... under the pressure of the serving class ... the government gradually restricted the freedom of the peasants ... to move ... [until] they became enserfed by the middle of the seventeenth century”.

That was the starting point of Domar’s well-known hypothesis that both serfdom and slavery alike are caused by an abundance of land relatively to labour. The scarce factor of production in Russia (or in the American South) was not land but labour. If non-working landowners were to obtain a rent, it had to come from the ownership of a scarce factor, that is, labour, with its relatively high marginal productivity. Hence, the assumption was that the net return of enslaving a fraction of the population rises with the land-to-labour ratio. As put by Domar (1970: 21), one could not have simultaneously free land, free peasants and an aristocracy of landowners – only two elements, but never all three, could be found together in reality. Domar’s (1970: 23) model predicted that, as population increases and the economy turns Malthusian, labour becomes abundant with a falling marginal product towards subsistence level, which brings about the end of coerced labour. Interestingly enough, both Domar (1970) and Lewis (1954) were concerned with the implications of factor endowments for underdeveloped economies. However, they focused on symmetric cases: labour scarcity and land abundance by Domar and labour surplus and land scarcity by Lewis (see Engerman & Sokoloff, 2002: 50).

Domar’s (1970) verbal model about the origins of serfdom has been formalized (e.g. Acemoglu & Wolitzky, 2011; Conning, 2004). Moreover, it was an important

element of Domar's course on Russian economic history at MIT over the years – see Temin, 2014, who was Domar's graduate student in the mid-1960s and colleague from 1970 to 80 s, both times at MIT. Domar's graduate students in the 1970s (some of them from neighbour Harvard) included a number of Brazilian researchers who wrote papers and Ph.D. theses about Brazil's long deep experience with slavery – the most important one globally, together with the American South – partly based on Domar's (1970) theoretical framework (see, e.g. Reis, 1974; Camargo, 1977; Lago, 1978; and Martins 1983, who wrote his thesis under Samuel Morley's supervision at Vanderbilt University). Of course, Domar's model has been extensively deployed in the study of American slavery, even if critically at times (e.g. Fogel & Engerman, 1974).

Domar's, 1970 hypothesis about serfdom, inspired by Kliuchevsky, became fruitful as a building block of historical studies about slavery in the West. Domar's last venture into the theme was a joint paper with Mark Machina (Domar & Machina, 1984) about the end of Russian serfdom. Again, a Russian historian (Pokrovsky, 1934) was at the centre of the argument, but this time critically. Domar and Machina disputed theoretically and empirically Pokrovsky's claim – endorsed by Soviet Marxian historians and some Russian–American economic historians as well, such as Gerschenkron 1965 that Russian serfdom became unprofitable for landowners before the emancipation of serfs in 1860s. Domar's attempt to understand Russia centred on what he regarded as key aspects of its economic history, taking Russian historians as reference points.

## 6 Domar's Soviet Readers

Whereas Domar's Russian background affected his research agenda on socialism and economic history, the readership of his papers in the Soviet Union was restricted to some reactions to his 1940s growth papers, 1953 depreciation model and a delayed interest on his 1957 restatement of Feldman (1928). Google Scholar search indicates that his papers on socialist efficiency (Domar, 1966a, 1974) and Russian serfdom (1970) did not elicit reactions in the USSR. Unlike Harrod's, 1948 *Dynamics* (see Harrod, 1959), Domar's collection of essays was never translated into Russian. The reason probably was the inconvenient (for Soviet authorities) chapter on Feldman, whose 1928 growth model, although available in Russian libraries, practically vanished from Soviet economics until about 1968. Indeed, by 1966 Domar (1966b: 252, n. 3) still complained that Feldman's work “has been recognized in many countries ... but not in the Soviet Union”. Despite Kovalevsky's (1930) report on the planned long-run development of the USSR, Feldman's model did not influence Soviet planning, even though the observed acceleration of the production of machinery was consistent with that model (Domar 1957b, Sect. V; Domar, 1965; Spulber, 1964, Chap. 2).

Due to Domar's (1957b) rediscovery, Soviet economists eventually acknowledged Feldman's growth model as a landmark contribution (see e.g. Vainshtein and Khanin

1968). Feldman had left Gosplan in 1931. He was presumably in prison or labour camp in the 1940–50 s. It was only in 1953 that he was released and allowed to return to Moscow. He died in 1958, most likely with no knowledge of Domar's (1957b) essay about him.

During his 1959 visit to the USSR, Domar, unaware of Feldman's death, enquired some Russian economists about him. They did not know either that he had passed away the year before. In Moscow, Domar approached an academic economist ("Professor Koch") who had met Feldman in the late 1920s but had never seen him again since. Domar (1959b: 11) gathered from the conversation that "Feldman is now in his 70s, if not more, and of course it is entirely possible that he had died".

Around 1968–69, a couple of articles on Feldman came out in the Soviet journal *Problems in Economics*, formed by English translations of articles by Soviet economists, started in the late 1950s. Belyanova (1969: 53), as part of an overview of Soviet growth planning models of the 1920s, pointed out that Feldman's models "did not gain the acceptance they deserved and were forgotten for a long time". Belyanova did not refer to Domar (1957b), but another article in that same journal, by Alter and Pochkin (1968), did. After discussing Feldman (1928) in some detail, Alter and Pochkin (1968: 12) observed that the "most complete evaluation of the model by a bourgeois economist has been made by Domar". However, they regarded Domar's assessment of the model "frequently prejudiced and superficial", as indicated particularly by Domar's (1957b: 236) side remark that Feldman's model exaggerated "the rigidities of the real world". Alter and Pochkin's charge was unwarranted, as they did not engage with Domar's (1957b) restatement as a whole.

Alter (1962) provided a first detailed critical discussion of the Harrod–Domar growth model by a Soviet economist, with emphasis on Domar's version. Alter (31) quoted from Domar's (1952: 481) assertion that the post-war interest in economic growth was explained to a large extent by "the present international conflict which makes growth a condition of survival". Alter could have also quoted from Domar's (1957a: 15) statement that "when an aggressive part of the world is strongly and quite successfully committed to rapid growth the other can disregard this objective only if it is tired of its own existence as a society". From Alters (1962: 38) perspective, Harrod–Domar growth theory failed in its "main function, in its attempt to strengthen the position of capitalism in the competition with socialism". Soviet Union's high growth rates were perceived as indication that it was ahead in the Cold War against the USA. The Harrod–Domar conditions for sustained equilibrium growth could only be achieved in a planned economy, according to Alter. From a Marxian standpoint, a main flaw of Domar's model was the analysis of capital accumulation "in isolation from the production and distribution of surplus value" (Alter, 1962: 34).

The misgivings about Domar's growth model were all gone as Soviet economists approached the depreciation problem, building explicitly on Domar ([1953] 1957). Domar's pioneer mathematical discussion of depreciation, replacement and growth brought to the fore the links between the life span of capital goods, factor ratios, propensity to save and economic growth. That study, funded by the RAND Corporation as part of its research agenda on the Soviet economy, included comparisons between the practice and theory of depreciation of real assets in the USA and the



USSR – one of many Domar’s incursions into comparative economic systems, a field he helped to create. Among other results, Domar ([1953] 1957) established that in a growing economy replacement falls short of depreciation.

The topic attracted the attention of Soviet planners from 1960s to 1980s as part of their concern with the time structure of production. The Soviet interest in Domar’s 1953 model, especially during the 1980s, came from the attempt by Soviet theoretical economists to use it as a starting point for the development of a new concept: the notion that a reduction in investment does not necessarily cause a decline in economic growth rates, since the excess of depreciation allowances over annual requirements to replace fixed capital assets can offset the fall in the growth of investment (see Rumer, 1984: 261–62 for detailed treatment and references to the Soviet literature).

The publication in the USSR of Irina Osadchaya’s 1974 book in English, about the history of macroeconomics since Keynes, with comparisons drawn to Marxian economics, was a key moment in the assessment of Domar’s growth economics by Soviet economists. Osadchaya (1974, Chap. 2) stressed the significance of Domar’s ([1946] 1957) conclusion that dynamic equilibrium required steady growth rate of income. Moreover, she carefully discussed aspects of Domar’s ([1953] 1957) model of what she called “the reproductive structure of investment”, with references to the Soviet economic literature of the 1960s on depreciation, prompted by that model. In Chap. 7, Sect. 3, Osadchaya provided a Marxian view of Feldman’s growth model along the lines of the original 1928 articles. She acknowledged that Domar had “discovered” the model, but did not deal with Domar’s (1957b) restatement.

In a previous paper, Osadchaya (1959: 51) had described Domar as an “American economist”, with no mention of his Russian background or his status as émigré. That generally applied as well to portrayals in the Soviet literature of other prominent Russian émigrés who had moved to the USA to study and/or work as economists. Domar certainly was an “American economist”, but the links he established between economics in the West and Russia were an important element of his American career. His life span largely overlapped with the period between the beginning and fall of the Soviet Union, whereas his professional life coincided with the Cold War time duration, with significant impacts on his research agenda. Together with Domar’s Russian heritage, that helps to explain his interest on Soviet and Russian economic development, as well as his investigation of analytical aspects of the working of socialist economies.

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Michael Ellman

## 1 Introduction

Kantorovich was a gifted Soviet mathematician. As a teenager, he was a prodigy, enrolling at Leningrad State University at the age of 14, and graduating and becoming an *aspirant* (postgraduate student) when he was only 18. When he graduated, he had already written many articles which had been published in leading Soviet and foreign mathematics journals.<sup>1</sup> He became a university lecturer (*dotsent*) two years later, was appointed a professor at Leningrad State University when he was 22 (only four years after graduating!), and became a D.Sc (*doktor nauk*) at 23. While he was working as an *aspirant* at Leningrad State University, he also taught in a construction institute, where he was an assistant in his first year, a lecturer in the second, and a professor in the third. The demand for his services resulted not just from his publications and his obvious mathematical gift, but also from the rapid expansion of the number and size of higher education institutions in this period, (This was a result of the industrialisation drive and the desire to replace the inherited ‘bourgeois’ engineers by loyal Communist engineers). This created a demand for qualified people to teach in them. His willingness to work simultaneously in two institutions (and a third one from 1932) partly resulted from the fact that from 1930 he was the main breadwinner in the family.

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<sup>1</sup> According to Aganbegyan et al. (1987), there were eleven of them. However, in the references to his end-of-life autobiographical notes, ‘only’ eight are listed.

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The original version of this chapter was revised: The term “Leoinid” in the chapter title and running head has been replaced with “Leonid”. The correction to this chapter is available at [https://doi.org/10.1007/978-3-030-99052-7\\_21](https://doi.org/10.1007/978-3-030-99052-7_21)

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Kantorovich was part both of the Soviet mathematical community and—to the limited extent possible in the USSR—of the international community of mathematicians (and from 1958 increasingly also of mathematical economists). Up to 1939, he frequently published in French and occasionally in English or German. He was a specialist in functional analysis<sup>2</sup> (e.g., author of the axiomatic theory of  $K$  spaces<sup>3</sup>) but, in accordance with the requirements of the Soviet system, he also devoted attention to applied mathematics, the training of engineers, and the needs of national defence. The USSR aimed at the integration of pure science with the needs of rapid industrialisation and war preparations, and Kantorovich's career was an example of that.

Besides being a professor at Leningrad State University and the Leningrad Higher School of Engineering in Industrial Construction, in 1932, he also became a professor at the Higher School of Industrial Transport. As a result of his interest in applied mathematics, he co-authored a book (published in 1936) on approximate calculation methods for partial differential equations that was used in planning some industrial developments. In 1939, as a result of work he did at the request of engineers from the Plywood Trust, he published a booklet on linear optimisation in economic problems (Kantorovich, 1939) that is generally considered the first publication on linear programming.<sup>4</sup> This interest in applied mathematics differed from other mathematical traditions of the 1930s (e.g. that of G. H. Hardy in Cambridge England) that focussed mainly on pure mathematics. Similarly, among Moscow mathematicians in the mid-1930s, calculation methods were treated as an unimportant subject and only attracted poor students (Kantorovich, 1987, 194).

In the late 1930s, Kantorovich was critical of the Soviet systems of planning and pricing and sent the authorities notes about their defects.

Kantorovich had to be very careful when writing his booklet on linear programming. Its main ideas were presented in the spring of 1939 in lectures at the Polytechnic Institute and the House of Scholars prior to publication. There he was criticised for using mathematics in economics. This, his critics said, was what bourgeois economists did. Under capitalism, his critics said, mathematical economics was part of capitalist apologetics. Hence, when he wrote the booklet he carefully avoided the term 'economic' (which belonged to political economy) and used instead the term 'organisation and planning of production'. Similarly, the role and significance of

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<sup>2</sup> Functional analysis developed as a separate branch of mathematics after the publication in 1932 by the Polish mathematician Stefan Banach of his book *Théorie des opérations linéaires*. This led to the establishment in 1934 of a seminar on the subject in Leningrad. One of its participants was the young mathematical prodigy Kantorovich.

<sup>3</sup> The linear semi-ordered spaces on which Kantorovich worked are called  $K$  spaces in his honour.

<sup>4</sup> Linear programming is the mathematical problem of the maximisation (or minimisation) of a linear function of a number of variables subject to linear inequality constraints. In economic language, it is a method for calculating the most efficient allocation of resources under conditions of constant returns to scale. In everyday language, it is about calculating how to use limited resources to achieve the best possible result.

the resolving multipliers<sup>5</sup> in his analysis was largely confined to the appendices and expressed in ‘a semi-aesopian language’ (Kantorovich, 1987, 201).<sup>6</sup>

In January 1942, he was evacuated from besieged Leningrad (the USSR was attacked on 22 June 1941 and from 8 September 1941 Leningrad was besieged) and was moved to Yaroslavl.<sup>7</sup> There he taught at a Naval College training future military construction officers and carried on with his work on the economic applications of linear programming. After the war, he resumed his work in mathematics and in 1948 published a paper extending the Newton method for solving nonlinear equations to functional spaces, thus creating what has come to be known as the Newton-Kantorovich method. This was an important contribution to both numerical analysis and pure mathematics. He subsequently wrote several further publications in this area.

Also in 1948, a top secret (*sovershenno sekretno*) decree of the USSR Council of Ministers ordered: ‘Within two weeks to organise in the Leningrad branch of the Mathematics Institute of the Academy of Sciences, a calculation group with a staff of up to fifteen people and appoint as the leader of this group prof. Kantorovich’. (Kutateladze, 2012, 3) These calculations were for the Soviet atom bomb project, and his work for this national priority project led to him receiving the Stalin prize in 1949. His work in Leningrad on nuclear weapons seems to have continued till 1953. He was invited to work in Arzamas-16 (a closed city where the design of Soviet nuclear weapons was concentrated) to organise a mathematical sector for Sakharov—the physicist who played a major role in the development of the Soviet hydrogen bomb (Vladimirov & Kublanovskaya, 2002, 155). He declined.<sup>8</sup> His motivation for this is

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<sup>5</sup> Every linear programming problem has a dual problem. What Kantorovich termed ‘resolving multipliers’ (or later ‘objectively determined valuations’) are variables from the dual problem. They are usually understood as prices, and referred to in the Western literature as ‘shadow prices’, but that was at variance with the Soviet Marxist understanding of prices. In 1939, criticism for having ‘anti-Marxist bourgeois views’ could have fatal consequences.

<sup>6</sup> ‘Aesopian language’ means a text written in such a way as to convey an innocent meaning to an outsider but a hidden meaning to someone who knows how to read between the lines. It was widely used in the Russian Empire and USSR to evade the authorities.

<sup>7</sup> According to Bollard (2020, Chapter 5) and Wikipedia (the article on Kantorovich), before he was evacuated to Yaroslavl, Kantorovich was responsible for safety on the Road of Life (the precarious route over iced-up Lake Ladoga that was the only surface link between besieged Leningrad and the rest of the USSR). This allegedly involved calculating the temperatures, wind speeds, and ice thickness necessary to ensure the ice was thick enough to prevent vehicles falling through it. It also allegedly concerned calculating the safe distance between vehicles. However, I have been unable to find any reliable source that confirms this. Neither in his own end-of-life autobiographical notes (about his scientific achievements) nor in the reminiscences of people who knew him (Dmitriev 2019) is this mentioned. If true, it would be a pioneering example of operations research. Kantorovich’s friend and colleague Sergei Golushkevich was indeed involved in calculating safe routes over the Road of Life for tanks (personal communication from Ivan Boldyrev).

<sup>8</sup> This seems to have been in 1950. The person he recommended for the post was appointed instead and was already busy at work in November 1950 (Vladimirov and Kublanovskaya 2002, 155).



uncertain. Later on, he was occasionally a consultant for the military.<sup>9</sup> In the 1960s, one of his military tasks concerned the organisation of naval bases, e.g. the efficiency of minefields in defending them (Dmitriev, 2019, 645).

His interest in calculations was not confined to the atom bomb project. From 1947, he supervised work on the use of calculating machines for mathematical calculations at the Leningrad branch of the Mathematics Institute of the Academy of Sciences. In 1951, a group in computational mathematics was set up in the mathematics faculty of Leningrad University. Kantorovich lectured at it and taught the students programming on a virtual computer (real ones at that time were secret). In 1956–57, he published papers on the possibilities of electronic computers and on programming them.<sup>10</sup> He (jointly) obtained two certificates of invention (for different calculating machines), one in 1950 and one in 1958.<sup>11</sup>

From 1959 onwards, he was the intellectual inspiration for the optimal planning school in Soviet economics.<sup>12</sup> In recognition and support of this, he received the Lenin prize in 1965 jointly with two colleagues, Novozhilov and Nemchinov, who were also involved in the struggle to recognise the legitimacy and usefulness of mathematical economics. In 1975, jointly with the US economist Koopmans, he was awarded the Nobel Prize in economics for his work on the optimum allocation of resources. He was also awarded honorary doctorates at many foreign universities. (This enabled him to travel abroad several times to accept them.)<sup>13</sup>

The range of his work was unusually large: from pure mathematics to helping to build atom bombs; the development of calculating machines; computational mathematics; challenging the Soviet systems of planning and pricing; inventing a method for calculating the best use of resources; and working out the best system for calculating urban public transport fares. This reflected his remarkable abilities and the society in which he lived.

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<sup>9</sup> This continued to give him advantages denied to those not involved with the military (Dmitriev 2019, 648). When he lived in Novosibirsk, he had a piano in his flat. He had been able to obtain it despite the fact that pianos could not be freely bought in shops.

<sup>10</sup> For an overview of his work in computational mathematics in 1947–1957, see Daugavet and Romanovsky (2012).

<sup>11</sup> Certificates of invention were official recognition of an invention and its inventor/s. Unlike patents in capitalist countries, they did not confer ownership rights on the inventor. The first of these inventions was electro-mechanical and was soon replaced by electronic computers. The second was more successful. Over a period of ten years, about forty thousand of them were manufactured (Kantorovich 1987, 206).

<sup>12</sup> This school treated national economic planning as an optimisation problem.

<sup>13</sup> For a very well-informed overview of Kantorovich's life and work see Boldyrev and Düppe (2020). For an overview of Western reactions to Kantorovich's economic results and proposals up to the end of the Soviet period, see Belykh (1990).

## 2 A Case of Independent Discoveries

After publishing his 1939 Russian language booklet about linear programming and its application to increase efficiency, Kantorovich did not publish his discovery in foreign languages. His only wider dissemination of his discovery was a 1940 Russian language article in a publication of the USSR Academy of Sciences. That article was purely mathematical and did not contain any reference to his 1939 booklet. In his autobiographical notes published in 1987, he explained this by the clear danger of war at that time ‘taking into account the circumstances I did not want my practical work to be used outside the country’. (Boldyrev & Düppe, 2020, 7).

Linear programming was invented independently by the American mathematician and operations researcher George Dantzig, whose first publication on the subject was in 1947 (although it was based on secret war-time operations research work that he had done in 1941–46). Since Kantorovich’s earlier work was unknown outside the USSR, this was naturally thought at the time in the USA to be an original discovery. What really was original was the algorithm that Dantzig proposed for solving linear programming problems (the simplex method), which differed from the algorithm (the resolving multiplier method) that Kantorovich had developed. Dantzig also formulated explicitly the duality theorem of linear programming, which in Kantorovich’s work was only implicit.

## 3 Kantorovich and the Soviet Economy 1937–41

Kantorovich’s knowledge of the Soviet economy (derived from everyday experience in Leningrad, his 1929 summer job as a junior accountant in Tashkent, his experience with linear programming, and the critical remarks of the Party leadership at the 1941 18th Party Conference) led him to the view that it was inefficient in many respects. In 1937, as a result of the adoption of a new constitution (1936), the Supreme Soviet was created. Kantorovich sent a note to the Supreme Soviet about the absurd situation in the book trade, in particular the system for calculating print runs and prices. He drew attention to the resulting shortages of books, their high prices in informal markets, and the money some people earned by buying books at low state prices and then reselling them at higher market prices. Prior to the 18th Party Congress (1939), the public were invited to participate in a public discussion about desirable policies. Kantorovich used the occasion to submit an article about the extreme distortion of the price system. In particular, he criticised the failure of prices to reflect capital-intensity and drew attention to the economic losses this caused. The article was not published, but he did get a formal response from Gosplan’s Price Office (*Byuro tsen Gosplana*) rejecting his ideas (Kantorovich, 1987, 199). These two criticisms were behind the scenes criticism which fitted in with the Soviet custom of people sending letters to the authorities about things going wrong in their area/factory/collective farm

and were not loud public criticism, which is understandable in the circumstances of the late 1930s.

In his 1939 booklet, Kantorovich confined the application of linear programming to rather local and limited issues, such as the allocation of tasks between machines; the best way of cutting materials; the best way of organising freight transport; and the location of production. However, he himself thought that it had wider implications. These concerned, in particular, the principles for calculating prices, and the efficiency of investment decisions. However, the booklet confined itself to lower-level matters, partly because the criticism of his ideas prior to publication (see Sect. 1) indicated that anything more ambitious was not without danger (Kantorovich, 1987, 201).

In cooperation with M.K.Gavurin, a fresh PhD (*kandidat nauk*), Kantorovich wrote an article about the most efficient way to plan railway freight transport. This was submitted to the journal of the railway industry in 1940 and was rejected. Kantorovich ascribed this to ‘fear of mathematics’ (Kantorovich, 1987, 202). Similarly, an article by Kantorovich about cutting timber in the most efficient way was also rejected in about 1940–41. The first article was eventually published in 1949 as a book chapter, and the second was eventually published, also in 1949, in the wood industry journal to which it had been submitted. By 1949, Kantorovich’s work on the atom bomb and the resulting Stalin prize had given him the status that enabled his articles on economic planning questions to be published.

On the other hand, Kantorovich had no difficulties in publishing the mathematical basis of the railway planning problem in Russian and English in 1942. This article was seen later by US specialists and made a favourable impression. In 1956, the US economist Koopmans sent him a letter of congratulations about it. Subsequently, the English version was reprinted in a US journal (Kantorovich, 1958).

Evidently, in 1937–41, Kantorovich was quietly critical of the Soviet systems of pricing and of planning, but his practical suggestions for improving the situation were often not welcome. On the other hand, he was free to publish mathematical articles and to teach mathematics<sup>14</sup> and played a major role in the establishment of the Leningrad branch of the Mathematics Institute of the Academy of Sciences. It was opened in March 1940.

## 4 Developments in the USSR 1942–58

From January 1942 onwards, Kantorovich in Yaroslavl was concerned with his teaching (at the Navy’s Higher Engineering Technical College which had been evacuated to Yaroslavl) and with elaborating his vision of the economic implication of linear programming. He wrote a long work entitled *Ekonomicheskii raschet, obespechivayushchii naibolee tselesoobraznoye ispol’zovanie resursov* (*Economic*

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<sup>14</sup> For example, he gave a course of lectures on probability theory in a military college, using military examples. This was in about 1940–41. A textbook based on it was later published (Kantorovich 1946).

*calculation providing the most effective utilisation of resources*) (Zalgaller, 2003, 5).<sup>15</sup> His famous and very influential book *Ekonomicheskii raschet nailuchshevo ispol'zovaniya resursov*<sup>16</sup> (*Economic calculation of the best use of resources*) although only published in 1959 was actually an edited version of the book written in Yaroslavl in 1942. Papers containing its main theses were read at the Leningrad Polytechnical Institute in 1940 and the Institute of Economics in 1943. However, because of the comprehensive pre-publication censorship, it could not be published in the USSR during Stalin's lifetime. It was incompatible with Stalin's view of the role of economics in a socialist economy. This view is clearly set out in his essay 'Concerning the errors of Comrade L. D. Yaroshenko' in his 1952 booklet *Economic problems of socialism in the USSR*.<sup>17</sup> Furthermore, Kantorovich's view of the shadow/dual prices that emerge from linear programming as guides to the rational allocation of resources was incompatible with the Marxist-Leninist view of prices as representing the value of the labour used in production. In fact, the closeness of Kantorovich's interpretation of shadow prices to mainstream Western economics was a heresy that might have had—in the absence of the usefulness of linear programming for engineers and Kantorovich's war work—serious adverse consequences for him.

Kantorovich was courageous about putting forward his ideas about prices in a hostile environment. In 1939, he gave a lecture in the main hall of Leningrad State University in which he discussed his resolving multipliers (shadow prices) and their economic significance. Some of the listeners were afraid that they would be arrested and face accusations of participating in an anti-Marxist meeting (Dmitriev, 2019, 655). This illustrates the atmosphere in which he had to work on economic issues.

Kantorovich did try to persuade Gosplan of the need to apply his ideas to national economic calculations and planning. His 1942 draft book was sent to Gosplan. In 1943, a discussion of Kantorovich's draft book took place in Gosplan. The general reaction was negative. One speaker said that 'An optimum has already been proposed by the fascist Pareto, a favourite of Mussolini'. This was a very threatening remark in a police state engaged in total war. After the discussion about his work, a meeting was held in the office of the chairman of Gosplan (Voznesenskii) at which it was discussed whether it was necessary to arrest Kantorovich. (Zalgaller, 2003, 5). Shortly afterwards, he presented his ideas at the seminar of K. V. Ostrovityanov, a leading Soviet economist. (In 1947–53, he was Director of the Institute of Economics, in 1952 became a candidate member of the Central Committee of the Party, in 1956 became a full member, and from 1953 was an academician.) The response was

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<sup>15</sup> Kantorovich himself gave a slightly different title for this (unpublished) work but added that he later changed its title (Kantorovich 1987, 203).

<sup>16</sup> The English translation is Kantorovich (1965).

<sup>17</sup> Yaroshenko proposed that Soviet political economy should concern itself with the rational organisation of production and economic planning. This was something for which Kantorovich—quite independently—supplied an instrument (linear programming) that could have enabled Yaroshenko's proposal to be realised in some cases. Stalin, however, considered that the rational organisation of production and economic planning were matters of economic policy, to be decided by the leadership and not by economists.

hostile (Zalgaller, 2003, 5). The advice to him of the participants of the seminar can be summed up as ‘Don’t fancy yourself as Marx. It would be better to burn your manuscripts’. In 1944, he was informed that Gosplan considered that the practical application of his ideas by Gosplan was impossible. This was a severe blow to him and for a time he was depressed (Boldyrev & D ppe, 2020, 9). He realised that he would have to cease his activities to get his ideas on the widespread applicability of linear programming accepted. To carry on with these efforts would be dangerous (Kantorovich, 1987, 204). Accordingly, he concentrated on mathematics and calculating machines and from 1948 on his work on the atom bomb.

After Stalin’s death, some colleagues promised the national leadership that the application of Kantorovich’s ideas would greatly increase the output of the economy.

In July 1954, Sergei Vallander, vice rector of Leningrad University, sent a letter to Premier Georgy Malenkov, focussing on the importance of mathematics in economic policy, bemoaning the ignorance of mathematical methods among economists and planners, and actively promoting Kantorovich’s work. It promised that Kantorovich’s optimization methods would increase final output<sup>18</sup> by no less than 50-70 per cent and asked for a cross-disciplinary committee of economists, mathematicians and technical experts to evaluate his work. The letter was forwarded to Gosplan and the Central Statistical Administration (TsSU). Thus the highest authorities were involved (Boldyrev & D ppe, 2020, 10).

This promise of the great practical improvements in the economy which the application of linear programming could bring was a major part of the reason why, within a few years, the use of mathematical methods in economic planning was supported by the national leadership. However, this change in the evaluation of the relevance of linear programming for national economic planning was slow and met resistance. In the mid-1950s, Kantorovich’s works were once again submitted to Gosplan (and other organisations), and once again, the possibility of applying them was rejected. However, this rejection was less sharp than in 1943. (Kantorovich, 1987, 204).

According to Safronov (2016), a particularly important reason for the support of mathematical economics by the leadership in 1957–64 was the hope that it would help overcome the difficulties that had been created by the (1957) replacement of the industrial ministries by regional economic councils (*sovnarkhozy*). Safronov argues that the Khrushchev leadership saw mathematical economics as a possible alternative to a return of the industrial ministries (that would have been politically embarrassing and only happened after the fall of Khrushchev).

In 1957, Moscow introduced a new system for calculating its public transport fares. Leningrad decided to do the same. Kantorovich gathered a group of colleagues from the Leningrad branch of the Mathematics Institute of the Academy of Sciences and also some economists, to study this problem and make recommendations. They considered that it was necessary to distinguish between the fixed costs of each mode of transport and the additional costs when it was actually used. This required a basic fixed fare plus a fare that depended on the distance travelled. For example,

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<sup>18</sup> The article from which this quotation comes uses the term GDP. However, in a personal communication, co-author of the article Ivan Boldyrev has stated that the Russian original is *vypusk konechnoi produktii* and that final output is a better translation.

for taxis, they advocated a fixed fare of 10 kopecks plus an additional fare of 20 kopecks per kilometre travelled. The fixed charge was intended to cover fixed costs, and the additional charges the marginal costs, of running that type of transport. This was a variant of the marginal cost pricing advocated in mainstream textbooks on microeconomics and welfare economics. The Leningrad Party committee rejected the general application of this proposal but accepted it for taxis.

## 5 Developments in the USA 1947–1956

In this period, linear programming was increasingly used by operations researchers and engineers to solve problems of the efficient allocation of resources. Profit-seeking firms were keen to apply techniques that could save them money and increase their profits. It was also used by the Defence Department for solving logistics problems. There was also a lively discussion among theorists. Theorists noticed the relationship between linear programming and game theory and also the implications of linear programming for economic theory understood in the tradition of Lionel Robbins—the allocation of scarce goods between competing ends to achieve the maximum satisfaction of consumer needs.

## 6 Developments in the USSR 1959–1991

In 1958, Kantorovich was elected Corresponding Member of the USSR Academy of Sciences in economics. (In 1964, he became Full Member in mathematics.) In 1960, he moved to Novosibirsk to take up the position of Head of the Laboratory on the Application of Mathematics in Economics of the mathematics department of the newly established Siberian branch of the Academy of Sciences. In 1959, Kantorovich's book *Ekonomicheskii raschet nailuchshego ispol'zovaniya resursov* was at last published. These developments reflected the change in the political situation after the 20<sup>th</sup> Congress of the Communist Party (1956).

Getting consent to publication was not easy. Kantorovich decided to publish the book in 1957 and wanted Leningrad University to publish it. However, the university's deputy rector for social sciences told Kantorovich that while the book was very interesting, he was not prepared to give up his Party membership for sanctioning its publication (Zalgaller, 2003, 6). It was published two years later in Moscow by the Academy of Sciences. To make it acceptable, Academician Nemchinov, who had a long experience of the interface between science and politics,<sup>19</sup> was the

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<sup>19</sup> On the one hand, he provided the statistical data for Stalin's speech 'On the grain front' (May 1928). On the other hand, he publicly opposed Lysenko's views on genetics at the notorious August 1948 session of the All-Union Academy of Agricultural Science named after Lenin (for which he lost his main job). Furthermore, in 1950–55, he struggled against the liquidators in statistics (people

‘responsible editor’, i.e. the person responsible for getting approval of the publication from the Academy of Sciences and censors and guaranteeing the theoretical acceptability of the book. He also wrote a critical Introduction which appeased opponents of Kantorovich’s ideas. In addition, to help make it palatable to the authorities, Kantorovich’s introduction included references to Lenin; Party documents are referred to throughout the book; and Marx is cited. These were the usual techniques used in the USSR to smuggle controversial works into print. The book had a big impact.

The publication of Kantorovich’s book, and also of three volumes edited by Nemchinov (Nemchinov, 1959–65) on the use of mathematical methods in economics, led to a lively debate about the role of these methods in the Soviet economy (Ellman, 1973, 4–17). This led to the institutionalisation of research in this area. In 1958, Nemchinov and Kantorovich organised a laboratory of economic mathematical methods. This had one group in Moscow, headed by Nemchinov, and another in Leningrad, headed by Kantorovich. The Moscow group would later become the core of the Central Economic Mathematical Institute (TSEMI) of the Academy of Sciences, which was founded in 1963 (Boldyrev & Düppe, 2020, 13). It also led to the incorporation of mathematical methods into the teaching of economics in Leningrad and Moscow Universities. Already in 1959 Leningrad State University, influenced by Kantorovich, decided to add an additional (i.e. sixth) year to the economics course to enable selected students to study mathematical methods. Kantorovich played an important role in organising and teaching this one-off sixth year course, which made a major contribution to training a cadre of young and enthusiastic mathematical economists. In 1962, Nemchinov created a Chair (*kafedra*) in mathematical economics in the Economics Faculty of Moscow State University.<sup>20</sup> Subsequently, while mathematical economists at TSEMI wrote about the optimally functioning economy (Russian acronym SOFE), the main focus of official policy switched to developing a country-wide automated system for planning and managing the economy (Russian acronym OGAS). Part of this system was intended to be an automated system for plan calculations in the creation of which TSEMI would play a leading role.

By the time the USSR collapsed, the once-ambitious aspirations of the optimal planners had vanished. Linear programming had become a practical tool of decision making, but a minor one which had difficulty fitting into the bureaucratic-command economy. Officials judged by the achievement of targets for gross output (such as tonne-kilometres of freight transport) were not interested in techniques for reducing that output. People who took the inputs for samogon (illicitly distilled vodka) from food processing factories were naturally opposed to optimal plans that would have used all the factory’s supplies of the necessary inputs. Similarly,

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who wanted to do away with statistical theory), even taking the risky step of publishing an article entitled ‘Statistics as a science’ in 1952 when Stalin was still alive and inspiring attacks on science.

<sup>20</sup> I was affiliated to this Chair as an international exchange student in 1965–67. As a young resident outsider from the UK, I found the search for optimality bizarre. In view of the difficulties of everyday life, it seemed to me rather obvious that more modest measures, such as attention to providing quality food in shops open to all and without queues, would have been more appropriate.

optimal factory plans which reduced waste aroused opposition from other factories that previously used that waste as a raw material.<sup>21</sup> When it came to the 1985–91 perestroika period, actual and former members of TSEMI contributed to the policy discussion, but to no avail. It rapidly came to be realised that optimal planning theory had paid insufficient attention to: the state and its leaders; militarisation; institutions; the behaviour and motivation of people, such as officials, managers, and workers; property ownership; entrepreneurship; misleading official statistics; information problems; competition; the role of money; and macroeconomics; to be a basis for a successful economic system. It had confused a social system with a technical system. It had also assumed that a successful economic reform could be based on microeconomics alone. As Gardner (1990, 646) has observed, macroeconomics was also important and Kantorovich as a microeconomist did not contribute to that. As Adam Smith and Allyn Young long ago pointed out, for achieving economic growth the main way of raising efficiency is not by the reallocation of given resources with given productivity between alternative uses, i.e. the method of linear programming. It is by increasing the productivity of resources by increasing specialisation, utilising economies of scale, raising the qualifications of the labour force and fostering technical progress.

That concentration on linear theory, rather than on socio-economic issues, could lead to ignoring crucial aspects of economics was long ago considered by Hicks<sup>22</sup> (1960, 707–708):

Economics, surely, is a social science. It is concerned with the operations of human beings, who are not omniscient, and not wholly rational; who (perhaps because they are not wholly rational) have diverse, and not wholly consistent ends. As such, it cannot be reduced to a pure technics; for we can then say that its concern is with the use that can be made of pure technics by man in society.<sup>23</sup>

Kantorovich and Gorstko (1972, 179) did recognise that: ‘Economic decisions are carried out by people, and their activities and individuality leave their mark on implementation of the plan’. Nevertheless, this basic truth plays only a minor role in

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<sup>21</sup> In 1951, Kantorovich co-authored a book with A.Zalgaller on the rational cutting of industrial materials (a revised edition was published in 1971 and a third updated edition in 2012). Their results were applied for many years in a factory building railway freight carriages and saved a considerable amount of metal. However, the reduction of waste meant a reduction in the amount of scrap iron available for steel mills. The steel plants complained and Kantorovich was summoned to the Leningrad Regional Party headquarters and accused of complicity in economic sabotage. However, this did not have adverse consequences for him because of his military work. Nevertheless, co-author Zalgaller was persecuted because he was a Jew, and Kantorovich, whose position was much stronger because of his military work, helped him. He also helped other colleagues who were persecuted for the same reason (Dmitriev 2019, 646 and 696). However, Kantorovich himself occasionally encountered state anti-Semitism. On one occasion he set up a Chair (*kafedra*) in computational mathematics but, because he was a Jew, he was not permitted to head it and a Russian was appointed instead (Dmitriev 2019, 684–685).

<sup>22</sup> Hicks was a joint winner of the 1972 Nobel Prize.

<sup>23</sup> Hicks followed up these remarks with speculation about the role of game theory in analysing social processes.



their theory and the attempts to implement it. That is not surprising since Kantorovich was a mathematician not a behavioural economist.

The Soviet mathematical economist Katsenelinboigen, after emigration to the USA, also pointed out the socio-political limitations of the theory of optimal planning considered as a basis for reconstructing the Soviet economic system.

As to the future of optimal planning in the USSR, the part this concept can play in solving that country's economic difficulties should not be exaggerated. Its troubles are rooted in the very nature of the autocratic political system — a system ill-equipped to work out new strategies, change them in an emergency, or correct mistakes within that framework. Even if we confine ourselves to the economic mechanism, effective turnarounds are hard to make without institutions such as private property, unemployment, and competition. The theory of optimal planning makes little mention of these, although it may be tied up with them (Katsenelinboigen 1978–79, 145).

The idea that economic planning is an optimisation problem assumes a united government with stable and agreed goals (in linear programming jargon an objective function). Kornai (2010, 193) has argued that this is a mirage.

One of the worst properties of much standard analysis is the assumption that the governments have a maximand.

Governments are very complicated institutions. Apart from rhetoric they do not maximise anything. Politics is full of mutually conflicting, inconsistent goals, and therefore full of hesitations, vacillations, ups and downs. Creating a consistent government objective function makes the model alien to real political decision-making and therefore almost irrelevant.

This Kornai argument is less relevant to the USSR than to the countries Kornai is referring to. The USSR had an autocratic political system (subject to opportunistic behaviour by regions, ministries and other organisations and inadequate information). This system gave priority to the achievement of military superiority over actual and potential enemies and this can be thought of as its objective function (Kantorovich and Wein 2009).

Kantorovich himself was well aware of the differences between the version of the economy described by linear programming models and the real Soviet economy. In a book he co-authored, published in 1972, three decades after he developed his ideas about the economic significance of linear programming and one decade after widespread attempts to introduce it on a national economic scale, he was able not just to present the theory but also to summarise the lessons of experience of its application. In that book, attention was drawn to seven significant ways in which the Soviet economy differed from the economy modelled by linear programming (Kantorovich and Gorstko 1972, 171–180). These varied from the lack of the necessary data for the calculations, the existence of nonlinearities, such as increasing returns to scale and indivisibilities, and social influences on labour productivity, to the fact that the interests of enterprises often differed from those of the state. However, Kantorovich pointed out that also in the natural sciences and technological processes, there were often discrepancies between reality and the mathematical models used to describe it. This did not prevent the mathematical models being useful. Hence, he remained

optimistic about the usefulness of linear programming models in the Soviet economy (Kantorovich and Gorstko 1972, 180–214).<sup>24</sup>

It should be noted that Kantorovich did not claim a monopoly for linear programming and related optimal planning models in describing and analysing the economy. He argued that they should be used together with other methods such as statistical analysis, sociological research, game theory, simulations, etc. (Kantorovich and Gorstko 1972, 179).

As far as the vexed question of the desirable relative importance of central planning and decentralised decisions (of state-owned enterprises) in the Soviet economy was concerned, Kantorovich argued for a combination of the two (Kantorovich and Gorstko 1972, 201–208). Experience in the USSR and Eastern Europe with such combinations was not positive, and in those countries, it did not provide a lasting and successful management system for the national economy.

Mathematical programming methods were widely applied in investment planning, both in the USSR and in the CMEA<sup>25</sup> countries, notably Hungary. However, their usefulness was hindered by lack of the necessary data, technical conservatism, and departmentalism (Ellman 2014, 156–160). Furthermore, experience of these calculations led János Kornai (a leading Hungarian economist, who wrote a thick book (Kornai 1967) about the optimal investment plans that he had been engaged in calculating) to adopt an approach to the significance of the application of mathematical programming that sharply diverged from that of Kantorovich.

Using mathematical programming methods, the programme computed is of course ‘optimal’ in the mathematical sense, it is a constrained maximum or minimum solution of a given mathematical extremum problem. This optimality, however, is a *relative* one, valid only under given simplifying assumptions, regarding definite political targets and expressed in the constraints and in the objective function of the model. In a series of computations we determine 10 or 50 ‘optimal’ plans, each of them is *relatively* optimal. The significance of mathematical planning is not the search for ‘optimality’, which is only the blue bird of economic theory, but the exploration of feasibilities; the explanation of interdependencies between conflicting goals; and the improvement of efficiency (Kornai 1970, 12–13).

Kantorovich’s ideas also had some influence on pricing. This concerned both specific issues such as Leningrad taxi fares, and national policy issues such as the introduction of a payment by enterprises to the state for the use of their fixed capital as part of the 1965 economic reform and the introduction of some rent payments by enterprises for the use of natural resources. (The new industrial prices were introduced

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<sup>24</sup> According to prof. A.V.Bukhvalov (Dmitriev 2019, 636), ‘In his last interview he [Kantorovich] said approximately the following: “Maybe I was mistaken in where and when it is possible to apply the mathematical models which I develop. Perhaps they require not the socialist person but the communist person”’. This last sentence means people who put the interests of society above their own personal interests. It is a striking recognition of the fact that it was often difficult to implement linear programming in the USSR because to do so was against the interests of many people. It also draws attention to the difference between technical and social systems.

<sup>25</sup> Council for Mutual Economic Assistance (Russian acronym SEV), often referred to in the West as Comecon.

in 1967.)<sup>26</sup> In the 1970s, he was deputy chairman of the interdepartmental scientific council on problems of pricing of the State Committee on Prices and the Academy of Sciences. What influence on pricing he had in this position is uncertain. In the 1970s, he also worked on problems of transport and innovation (Boldyrev and Düppe 2020, 21).

## 6.1 *Linear Programming and Ideology in the USSR*

After the 20th and 22nd Congresses of the CPSU (Communist Party of the Soviet Union) at which Stalin was strongly criticised, the fact that Kantorovich's approach to economics was similar to that of Yaroshenko ceased to be of any importance. Nevertheless, the relationship between mathematical economics and Marxism-Leninism remained controversial for many years.

Four main themes can be distinguished in the lengthy debate which accompanied the rise of mathematical economics in the USSR. First, many of the old school of political economists regarded the 'mathematical' theory of prices, in which prices are numbers which help a decision maker to arrive at optimal solutions, as contrary to the labour theory of value. Secondly, there arose the intellectual and organisational question of the relationship between the new discipline of mathematical economics and the traditional subject of political economy, which continued to be taught and to be an integral part of the Marxist-Leninist world outlook. Thirdly, there arose the question of the quality of the traditional planning methods. Fourthly, there arose the question of the practicability of the proposals of the mathematical economists (Ellman 1973, 5–6).

The relationship between mathematical economics and political economy was of great importance because political economy was an integral part of the official doctrine of the USSR—Marxism-Leninism. It was disseminated at all levels from evening classes for workers via *Pravda* editorials to the Academy of Social Sciences attached to the Central Committee (of the Communist Party). Since Marxism-Leninism legitimised the political and economic systems that existed in the USSR and functioned as an apologia for the waste, shortages, low incomes, and inequality that marked the USSR, the authorities naturally did not want to abandon it. Initially, the mathematical economists seized the opportunities provided by the 22nd Party Congress (1961) and the 1960s discussions of economic reform. They were very radical and proposed replacing political economy by optimal planning (i.e. the application of linear programming to planning the national economy). However, they soon had to retreat and to accept the primacy of political economy. This was especially the case after the overthrow of the Czechoslovak Communist Party leader Novotny in 1968 and the emergence in Czechoslovakia of 'socialism with a human face' had shown the dangers to the Soviet nomenklatura of theoretical debates and the need to maintain orthodoxy. From 1968, the optimal planners had to recognise (at any

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<sup>26</sup> For the criticism by Kantorovich of how the payments for the use of capital goods were calculated in the 1967 price reform, see Kantorovich and Gorstko (1972, 195). For an evaluation of the 1965 reform (of which the 1967 prices were a part), see Khanin (2008, 313–17).

rate in public) the primacy of political economy. Mathematical economics became a specialised academic discipline taught to future planners. Political economy was expounded in the press; lectures on it were given in the factories; and it was taught to students throughout the higher educational system.

The US economist Robert Campbell published an article in the USA (Campbell 1961) that made what was treated in the USSR as an intervention in the Soviet ideological debate. He argued that Novozhilov had introduced opportunity costs rather than labour as a measure of value, and Kantorovich had adopted the Robbins definition of economics and explained that not only labour creates value but also natural resources and capital goods. In their theories, he argued, there was no room for the Marxist labour theory of value. This article was a big nuisance for Novozhilov and Kantorovich who made major efforts to reconcile their theories with the Marxist-Leninist labour theory of value. It was also a gift for the Marxist-Leninist critics of Kantorovich since it confirmed their suspicions. They were also horrified by the first three words of the title of the article (Marx, Kantorovich and Novozhilov) which they saw as a blasphemous attempt to raise Kantorovich and Novozhilov to the same level of genius as Marx.

Campbell was correct in discerning that Novozhilov gave a key role to opportunity costs and that Kantorovich's theory of pricing overlapped with the international mainstream in economic theory and had little to do with the Marxist labour theory of value.<sup>27</sup> However, his assertion that 'it seems quite clear that he [Kantorovich] did not then [in 1939] realise the broad significance of these multipliers as indexes of value' (Campbell 1961 p.407) can now be seen to be quite wrong. Similarly, his account of Nemchinov's Introduction was unable to recognise what we now know of Nemchinov's importance in getting the book published after Leningrad University had declined to do so. Furthermore, his discussion of Marx's labour theory of value was somewhat one-sided. That theory is primarily a theory which argues that, whereas liberals perceive the capitalist employment relationship as a fair purchase of some work from the worker in exchange for wages, it is actually a case of exploitation (Marx 1865). Novozhilov and Kantorovich did not dispute that. Moreover, the institutional framework of their theories was a socialist planned economy and not a capitalist market economy as in the international mainstream. Also, Campbell's article is superficial and ignores all the problems, practical and theoretical, which prevented the application of linear programming in the Soviet economy having the results hoped for (see Sect. 6). In addition, the general equilibrium theory, which Campbell assumed to have provided a satisfactory explanation of value (unlike Marx's theory), itself expired soon afterwards (see Sect. 7.1).

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<sup>27</sup> However, the British Marxist economist Dobb (1967) argued that there was no contradiction between the Marxist theory of value and Kantorovich's optimal valuations (shadow prices) since the latter corresponded to Marx's 'market prices' (which he only tackled in the incomplete Volume 3 of *Capital*) and Marshall's 'short-period' prices. They had nothing to do with the analysis of long-period prices in Volume 1 of *Capital*. Dobb's argument was welcomed by Novozhilov, as was apparent at a meeting I had with him in Leningrad in 1966.

## 7 Developments in the USA from 1957

In 1957–58, two important books were published in the USA about linear programming and its implications for economic theory. They were Koopmans (1957), and Dorfman et al. (1958), the authors of which are sometimes referred to as DOSSO.<sup>28</sup> They were major contributions to integrating linear programming into mainstream economic theory. Another contribution to integrating linear programming into mainstream economics was made by a British economist—Hicks (1960).

Gradually, the initial excitement about linear programming and its apparent support for the mainstream theory of the market mechanism as a means to achieve an optimal allocation of resources died down. The inflation of the 1970s led to a concentration on macroeconomics rather than microeconomics, and the re-emergence of monetarism. A major contributor to this were the numerous publications of Milton Friedman which spanned the 1960s and 1970s. In 1976, he was awarded the Nobel Prize. Also, Hayek played an important role in economic and political thinking. His stress on market *processes* over time rather than optimal *solutions*, and on the use of continually varying information (not just prices) by decentralised agents, was at variance with the search for optimal equilibria. He was jointly awarded the Nobel Prize in 1974. Real business cycle theory emerged, followed by dynamic stochastic general equilibrium (DSGE) models. At the present time, economics in the USA is divided into a variety of schools and traditions. Besides microeconomics and macroeconomics, there are evolutionary economics, information economics, behavioural economics, new institutional economics, public choice economics, post-Keynesian economics, feminist economics, modern monetary economics, law and economics, social economics, business economics, Austrian economics, etc., and a variety of analytical tools such as econometrics, computable general equilibrium, game theory, experimental economics, and artificial intelligence. Attention to linear programming among economists has faded away, although it continues to be used by engineers and operations researchers to solve many day-to-day business problems, and mathematical modelling remains the main instrument of mainstream economic theory.

### 7.1 *Linear Programming and Ideology in the USA*

Linear programming, with its focus on the maximisation of production and on the corresponding shadow prices, could be fitted smoothly into the liberal ideology (using the word ‘liberal’ in the traditional European sense as meaning support for the free market and individual freedom) which was dominant in US mainstream economics (although there were other views such as the old institutionalism and Keynesianism). This was possible for seven reasons. First, it viewed the economic

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<sup>28</sup> DOSSO entirely ignored Kantorovich but Koopmans did mention him. Already in 1956, Koopmans corresponded with Kantorovich—see Sect. 3.

problem as the allocation of scarce resources between competing ends. In this it agreed with Robbins's well-known definition of economics. Secondly, it treated economic decision making as a task of maximisation subject to constraints (a view earlier formalised in Samuelson (1947)). Thirdly, it regarded prices as guides to the efficient allocation of resources (rather than as a reflection of costs of production and/or the class struggle). Fourthly, it treated prices as the main parameters decision makers should take account of. Fifthly, it treated labour and its pricing as just another input to be treated like other inputs. Sixthly, it argued that the price mechanism was not just one way of organising an economy efficiently but was inherent in any efficient economic system. Seventhly, it 'proved' that reliance on the market mechanism will lead to an efficient allocation of resources.

This apparent fit was demonstrated most clearly in what was called the basic theorem of welfare economics, which DOSSO (1958, 410) formulated as 'Every competitive equilibrium is a Pareto-optimum and every Pareto-optimum is a competitive equilibrium'. DOSSO themselves pointed out that this does not necessarily mean that over time competition will arrive at an equilibrium, and that it is based on restrictive assumptions about the technology used in production and about consumer preferences. However, in some US textbooks and classrooms, it was interpreted in a clearly ideological way. Commenting on this theorem, the author of a textbook on microeconomics published by a leading US academic publisher wrote ironically that 'You should now be hearing choirs of angels and choruses of trumpets. The "invisible hand" of the price mechanism produces equilibria that cannot be improved upon'. (Kreps 1990, 200). However, a couple of pages later, Kreps pointed out that: 'There are good reasons still not to hear them [the angels and trumpets] having to do with all the hidden assumptions made in the story of Walrasian equilibrium'. Furthermore, the evolutionary economist Nelson (1981) had earlier pointed out that the economy described by general equilibrium theory is so far removed from reality that the basic theorem of welfare economics does not provide an adequate defence of a private enterprise economy. In addition, mathematical economists such as Sonnenschein proved that the equilibrium in a general equilibrium model is not necessarily either unique or stable (Ackerman 2002).

Furthermore, admiration for the market mechanism was subsequently undermined by: the financial crash of 2008–9 and the resulting Great Recession; the very large inequalities of income, wealth, and health in the USA; the increase in the USA of 'deaths of despair'; the inefficiency of the US medical care system (which is largely a market system)<sup>29</sup>; and the large debts incurred by many US university students.<sup>30</sup> A striking result of these developments was that in the 2020 US presidential election

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<sup>29</sup> For a popular book on the last two issues, see Case and Deaton (2020) a book co-authored by the 2015 Nobel Prize winner (Angus Deaton) and his wife. That medical care differs fundamentally from ordinary commodities, which can be efficiently produced and distributed through the market system, because of uncertainty and asymmetrical information, was pointed out by Arrow (1963).

<sup>30</sup> These result from treating higher education as an ordinary commodity bought by students and sold by universities. This ignores the external effects of education on the economy and society as a whole.

a strong (but ultimately defeated) candidate to be the Democratic Party's nominee for the presidency was a self-proclaimed socialist.

## 8 Interaction Between the USSR and USA

The publication in full of Kantorovich's 1939 booklet in English translation in a prominent US journal in 1960 was an important contribution to developing relations between Soviet and American specialists in linear programming. In an introductory note, Koopmans (1960) explained that he had corresponded with Kantorovich to obtain the Russian text. In his evaluation of the work, Koopmans wrote that: 'All problems considered in the 1939 paper reprinted in this issue are what would now be called linear programming problems'. He also wrote that 'the wide range of applications perceived by the author make his paper an early classic in the science of management under any economic system'. The 1958 publication in a US journal of Kantorovich's 1942 article on the mathematics of the transport problem also increased his reputation among US specialists. There were also some face-to-face contacts between Kantorovich and US specialists. Koopmans met Kantorovich in Moscow and Novosibirsk in 1960. Kantorovich met Harold Kuhn (a prominent US mathematician who jointly won the 1980 John von Neumann Theory Prize) and Richard Bellman (a US applied mathematician who developed dynamic programming) in Budapest in 1963 at an international symposium on mathematical methods for economics.

In 1965, David Gale (a mathematician, mathematical economist and author of a standard textbook on linear economic models) and Koopmans separately made the pilgrimage to Novosibirsk to meet Kantorovich. Kantorovich was only allowed to make a return visit to the USA a decade later shortly after receiving his Nobel Prize in Stockholm (he attended the 1976 annual meeting of the American Economic Association). He returned home via India and Vienna (where he attended a conference at the International Institute of Systems Analysis at which Koopmans and Dantzig were also present). A decade later, he attended the 1985 World Congress of the Econometric Society (of which he was an Honorary Fellow) in Boston (Gardner 1990, 639).

Koopmans' positive evaluation of the 1939 booklet and 1942 article, and the personal meetings between Kantorovich and US specialists, contributed to the international recognition of Kantorovich's priority that led to his (joint) Nobel Prize. They also led to attention to Kantorovich's work in leading US economics journals, e.g. Gardner (1990), after his death.

## 9 Conclusion

Kantorovich was a distinguished Soviet mathematician who made important contributions in pure mathematics and also in applied mathematics (both military and civilian). According to his obituary in a Soviet mathematics journal (Aganbegyan et al., 1987, 178), his four main contributions to mathematics were: the theory of  $K$  spaces; the Newton-Kantorovich method; linear programming; and his work on functional analysis summed up in the textbook on functional analysis written jointly with Akilov.<sup>31</sup> He also devoted much attention to teaching, the dissemination of his ideas, the invention of calculating machines, the use of computers, and managing research projects. His career combined science, the application of science to help resolve real economic problems, the introduction of new technology in the economy, the training of engineers, and war work, a combination aimed at by the Soviet state. However, although he had some influence on both planning and on pricing, his ambition to fundamentally reform Soviet planning and pricing was not realised.

For economists, the most important part of his work concerned linear programming. Linear programming was invented independently in the USSR (by Kantorovich in 1939) and in the USA (by Dantzig in 1947), but Kantorovich and Dantzig developed different algorithms for solving linear programming problems, and Dantzig explicitly formulated the duality theorem. In the USSR, linear programming originally clashed with the official ideology but in due course became part of a new academic discipline, variously known as mathematical economics or economic cybernetics. Its application in the USSR was much discussed, and at one time, its potential for improving efficiency was greatly overrated, but its practical fruits were very limited. In the USA, it was fitted into the dominant ideology and for a time was, as a result, given an important role in economic-theoretical thinking. However, in due course, attention switched to other areas and it ceased to play an important economic-theoretical or ideological role. Nevertheless, it did become a routine part of the arsenal of engineers and operations researchers in both the civil and military sectors.

The overlapping of the work of Soviet and US researchers in this field led to some contacts between Soviet and US scientists. This was a rare example in the Cold War of collegial, even if very limited, contacts between scientists from the USSR and USA, and resulted in the sharing of the 1975 Nobel Prize for economics between Kantorovich and Koopmans. This enabled him to visit the USA for the first time. It also led, a decade later, to the participation of Kantorovich in a World Congress of the Econometric Society in Boston.

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<sup>31</sup> The English translation of this book is Kantorovich and Akilov (1982).



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# Correction to: Leonid Kantorovich



Michael Ellman

**Correction to:**  
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In the original version of the book, the term “Leoinid” in the chapter title, running head and TOC of Chapter 20 has been replaced with “Leonid” so that it should read as “Leonid Kantorovich”.

The correction chapter and the book have been updated with the change.

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The updated original version of this chapter can be found at  
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C1

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