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Scientific Community in a Divided World: Economists, Planning, and Research Priority during the Cold War

JOHANNA BOCKMAN

George Mason University

MICHAEL A. BERNSTEIN

Tulane University

During the Cold War, economists utilizing mathematical methods in both the Soviet Union and the United States found they shared a common research project. After the Stalinist years in which they could communicate very little, they found that they had much to learn from each other. Mathematical economics came to bridge the divide between East and West even though the meetings and collaborations between American and Soviet colleagues were fraught with tension and misunderstanding. The Cold War excitement about mathematical economics and the East-West cooperation it allowed, however, dwindled with the end of détente, the global shift in economic science away from mathematical economics, and the end of state socialism in the East Bloc. To understand the rise and fall of this cooperation and convergence, we examine the case of the connection between Tjalling C. Koopmans, a Dutch economist living in the United States as a result of World War II, and Leonid V. Kantorovich, a Russian scientist living in the Soviet Union. These two men jointly won the Nobel Memorial Prize in Economic Science in 1975. Their scientific connections reflect the broader experiences of East-West scientific collaboration during the Cold War. At the same time, they offer insights for scholars

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regarding struggles over scientific research priority and the potential for convergence between capitalism and socialism during the Cold War era.

The intellectual and personal friendship sustained by Kantorovich and Koopmans personified what had been a more deep-seated historical process of the postwar period. For some decades after World War II, Western social scientists had commented on the potential for convergence of the economic practices of capitalist nations with those of the Soviet Union.[1] After all, Russia had faced exceptional problems with respect to national mobilization, first during Stalin's program of collectivization in the interwar period (at which time Kantorovich had begun path-breaking research), and later with respect to military mobilization in the face of Germany's "Operation Barbarossa" that had commenced in the summer of 1941. With the advent of the Cold War, the need for systematic allocative methods only increased as the military contest with the United States and the European member nations of the North Atlantic Treaty Organization intensified. Earlier and in similar fashion, with the coming of World War II, the United States and other Allied nations had faced dramatic needs for appropriate methods to manage mobilization; it was in this context that Koopmans had made crucial discoveries on behalf of British and American merchant-shipping administrators. Again, during the Cold War era, the importance and usefulness of the work that Koopmans and later researchers did simply became more and more apparent.

Even so, for economists, the seeming convergence of ideas in their disciplines, from East and West, appeared to be less the result of particular historical and political forces than the outcome of intellectual trends of longer gestation. As one Yale University economist put it shortly after Koopmans won the Nobel Prize, "The techniques of activity analysis [perfected by Kantorovich and Koopmans] exemplify the pure theory of decision-making, and, as such, are remarkably indifferent to economic institutions and organizational forms." It was thus "one of the great achievements of this methodological revolution" that "economists of the East and West" could enjoy "continued dialogue—free of ideological overtones" (Scarf 1975: 712). Scientific rigor and objectivity had at long last triumphed over sectarian posturing; a new era of understanding, comity, and peace had potentially arrived.

Speculation concerning the apparent similarities of advanced economic practice in the East and the West disappeared as soon as the Soviet Union collapsed in December 1991. In its place emerged an unabashed celebration of the virtues of capitalist markets as engines of growth and rising material welfare. The

[1] Jan Tinbergen (1961) contributed an early article to the thriving literature on the potential convergence of "socialist" and "capitalist" systems. Outside official circles, intellectuals in the Soviet Union also argued that these two systems were converging (Kelley 1973). Also see Meyer (1970) on convergence theories at this time.

shared 1975 Nobel Memorial Prize thereby stood less as a symbol of intellectual parallelism than of an older, albeit inspired, statism that had spanned the great context of the Cold War itself. Reversing the timing of scientific research priority that Koopmans himself had been so scrupulous to preserve, commentators cavalierly declared the triumph of capitalist over socialist principles, suggesting that "the pure theory of decision-making" was a wholly Western discovery that then penetrated to the East.

Scholars have turned to studying the Cold War connections between scientists that served both as a forum for international science and for diplomacy (Evangelista 1999; Richmond 2003). These connections were eased by the fact, which has only been recently recognized, that both the United States and the Soviet Union had many similarities, sharing similar "dreamworlds" arising from the Enlightenment ideals of progress and control (Buck-Morss 2002), a common fascination with planning (Engerman 2004), state support of big science, and the cooperation of science and the military (Bernstein 2001; Gerovitch 2002). As Bernstein (2001) has shown, economics paradoxically became identified with free markets, when in fact it gained its prominence through working on planning within the military and with significant state support. However, the end of détente and renewed geopolitical tensions, as well as a shift in the economics profession away from an interest in planning, dismantled the bridge that mathematical economics had once built between East and West.

Mitchell (2002: 81–83) has argued that between the 1930s and the 1950s, the notion of "the economy" emerged as a distinct sphere, the object of social science, statistical enumeration, and government policy. In the early Cold War world, Soviet and American economists had argued that there was a clear sphere of the economy, but they did not agree that the Soviet economy and the American economy were similar. They also did not perceive that they shared a common economics profession. In 1948, an editorial in the journal *Voprosy filosofi* proclaimed the view that Soviet and American sciences were different, noting: "Marxism-Leninism shatters into bits the cosmopolitan fiction concerning supra-class, non-national, 'universal' science, and definitely proves that science, like all culture in modern society, is national in form and class in content."[2]

Americans also saw a fundamental difference between Soviet and American science. In 1950, James Conant wrote: "Scholarly inquiry and the American tradition go hand in hand. Specifically, science and the assumptions behind our politics are compatible; in the Soviet Union by contrast, the tradition of science is diametrically opposed to the official philosophy of the realm" (in Pollock 2006: 13). The notion that economists might share a common project in, for example, linear programming meant an ideological shift would

[2] "Against the Bourgeois Ideology of Cosmopolitanism," (1948), cited in Merton (1957: 641).

have to take place in the economics professions in both countries.[3] To utilize a technique like linear programming in the economy, a fundamental change in the perception of "the economy" and in the professional community of economists would necessarily have to take place.

One of the main issues surrounding the collaboration of Koopmans and Kantorovich was that of scientific research priority. Koopmans identified Kantorovich's work as having established Kantorovich's priority in the development of linear programming. This claim was immediately disputed. As Merton recognized, scientists define originality as a supreme value, and with it, the recognition of originality as a major concern (1957: 639). In general, priority is impossible to establish by any external standard, since the "discovery" or "invention" is based on previous scientific work and can only be evaluated afterwards. As Kuhn has noted, that something is discovered and what is discovered must be established over a long period and usually involves many people (1977: 166). Thus, priority is socially constructed primarily by professions using a variety of social resources, expectations, and practices.[4]

Claims to priority can result in major scandals and controversies because of "significant historical shifts in the social organization of science" (Collins and Restivo 1983: 200). New organizations of science, such as new patrons or new institutions, provide resources for scientific competition and alternative norms for scientific practice and for the recognition of priority. We present the case of scientific connection between Koopmans and Kantorovich not to argue that they created mathematical economics or fundamentally changed economics on their own, but rather to illuminate the difficult dialogue that emerged in the new organization of science from the late 1950s. Their scientific dialogue is just one particularly illuminating example. When Koopmans sought to establish Kantorovich's priority in linear programming, Soviet and American economists did not share a field of economics or a common, unified economics profession. Both Soviet and American mathematical economists had experienced censorship of their work for national security reasons and by 1950s

[3] Our work thus contributes to that of Amadae (2003), Bernstein (2001), and Mirowski (2002) by including the difficult dialogue between American and Russian economists within the history of American mathematical economics. However, social scientists from East and West in dialogue also developed knowledge and techniques that could not clearly be labeled American/capitalist or Soviet/socialist/Marxist. As Gerovitch (2002) has shown, economists in the East Bloc played an active role in knowledge creation and thus cannot be assumed to have been the passive victims of American military strategy. Using the case of Hungary, Lampland (1995) has demonstrated that within "East-Bloc" nations themselves, socialism at times created a culture of individualism, utilitarianism, and economism—very capitalist values.

[4] There is no absolute priority, but rather the observer of history can only document the social construction of scientific priority with impartiality toward each side. Priority is related to the more general issue of credit. Since knowledge about things requires knowledge about people and their virtues, we can see that the priority debate also revolved around the perceived credibility of Kantorovich and Koopmans, their symbolic capital, and their avoidance of claims that they had special or extra-scientific interests (Bourdieu 1977; 1984; Shapin 1994).

sought professional recognition for their earlier work. Furthermore, not only did their works have to be declassified in order to be professionally recognized, they also had to be translated, which was a politically dangerous process. Scientific priority disputes reflect the problems of establishing priority within an emerging field of transnational economic science, a transnational economic profession, and seemingly converging national economies.[5]

TJALLING C. KOOPMANS

Koopmans (1910–1984) originally trained in mathematics and physics at the University of Leiden. By 1936, having already published two articles in physics, his interests turned toward economics and econometrics, stimulated in part by work he had done for the League of Nations in Geneva on the abstract analysis of time series and the applied study of merchant shipping networks. His experience with the League served him well when, on his emigration to the United States ahead of the Nazi invasion of his homeland in 1940, he undertook service for the Combined Shipping Adjustment Board in Washington, D.C. At the end of World War II, Koopmans moved to the Cowles Commission for Economic Research at the University of Chicago (which subsequently became the Cowles Foundation at Yale University). Within these venues, he executed the research on "activity analysis" that had such a dramatic impact on the trajectory of modern economics in the latter half of the twentieth century.

Economists working in Europe and the United States in the 1930s were fascinated with economic planning and sought to make improvements in planning that would allow it to succeed in capitalist countries within the military, government agencies, and corporations. The mainstream model in economics—the Walrasian model—suggested that economists could make inefficiency and waste a thing of the past by calculating prices that would balance consumer demand and producer supply, or even motivate factories or economies to produce certain items in larger quantities. In the early 1900s, two Italian economists, Enrico Barone and Vilfredo Pareto, had argued that, in principle, a socialist system could work just as efficiently as a capitalist system by fixing prices using a series of equations, but the calculations were impossibly large. Ludwig von Mises (1935 [1922]) argued that such calculations were

[5] In contrast to assuming the agonistic fields of scientists battling for credibility and truth presented by Latour (1988), Latour and Woolgar (1986 [1979]), as well as Bourdieu (1977; 1984) more generally, we follow the conflicts over priority as they emerged. While we seek to avoid falling prey to functionalist views of a harmonious scientific community, we do find institutional studies of science very helpful for our particular research questions. For example, Brannigan and Wanner (1983) follow Merton's 1957 contentions that all discoveries are in principle multiple discoveries and that the announcement of a discovery might get lost due to various communication failures. Brannigan and Wanner find that priority disputes "have declined in time with the rise of professional scientific establishments which are particularly efficient in organizing the communication of research outcomes" (p. 138). We bring together an institutional focus with a study of the content of knowledge, thus crossing the social and cognitive boundary.

impossible and thus socialism was not viable. Economists took up the challenge of proving mathematically that different forms of planning could be as efficient, or even more efficient, than a free market. Oskar Lange (1971 [1938]) famously showed that it was possible to integrate markets and socialist planning, envisioning a kind of market socialism. Koopmans specifically saw the work of Lange and others in the socialist calculation debate, including the challenge of von Mises, as a source of inspiration for linear programming (1951: 3). With the development of the computer, economists built on this work, exploring how decentralized planning might work without markets (Scarf 1973).

Koopmans and the researchers at the Cowles Commission greatly benefited from their close connections with RAND and the military. During the 1950s, what had been Project RAND (an acronym for Research and Development) under U.S. Air Force supervision, and later devolved as the nonprofit RAND Corporation, underwrote much of the work on linear and dynamic programming that had been stimulated so decisively by the experience of the war years and their immediate aftermath.[6] RAND was a center for military planning and cybernetics where the top minds in mathematical economics and other sciences could discuss the most exciting scholarly and policy issues of the day, and have their ideas put into practice.[7] RAND developed a great interest in Soviet developments in mathematical economics and mathematics more generally, not only to learn more about the Soviet Union but also because the military generally lacks competitive market conditions and requires other forms of economic allocation. For example, RAND had a research project on "Economic Analysis on Soviet Union, Mathematical Programming and Industry" directed by C. J. Hitch, and Kantorovich's 1957 work had been translated at RAND. Numerous RAND reports were written about Soviet mathematical economics, and RAND even created its own journal, the *Soviet Cybernetic Review*. Interestingly enough, Koopmans was committed to world peace; he was in fact quite opposed to the procurement and use of nuclear weapons. He earnestly believed that cooperation with Soviet and East European researchers could be the foundation of the kinds of confidence-building necessary to forestall World War III.

LEONID V. KANTOROVICH

Kantorovich (1912–1986) was a mathematical prodigy, entering St. Petersburg (later Leningrad) University at the age of fourteen, becoming a university teacher at eighteen, and gaining the title of professor at twenty-two. At the

[6] According to Mirowski (2002), RAND covered 32 percent and the Office of Naval Research covered 24 percent of the Cowles Commission's budget in 1951.

[7] For example, in the late 1940s, there was a Colloquium on Planning held at RAND (Dorfman 1984: 287).

age of eighteen, he had presented his first major work at the First All-Union Mathematical Congress and met foreign mathematicians at that time (Kantorovich 1992). He had been in contact with John von Neumann, A. W. Tucker, and other eminent mathematicians, whom he met at the Moscow Topological Congress in 1935. His work started in functional analysis, and he was one of the founders of the theory of semi-ordered spaces—later called K-spaces in his honor. He ultimately moved on to economic questions.

Russian economists were the first to apply mathematics to national economic planning (Gerovitch 2002; Zauberman 1960).[8] According to Ellman, "Much of the 'new' Western economics of the post-World War II period, such as the discussion of the economic problems of the developing countries, growth models and input-output, was simply the rediscovery and development of the fruitful Soviet work of the 1920s" (1973: 1). In 1929, Stalin attacked mathematical economics at least in part because this work did not support his agricultural collectivization plans (ibid.). Stalin called for what he named "political economy," which would provide ideological support for party policies, and many mathematical economists were exiled or killed. Mathematical economics was seen as anti-Marxist and as defending capitalism. Economists were to focus on political economy, elaborating Marxist-Leninist theories and providing theoretical justifications for Soviet practice (Ellman 1973; Pollock 2006). Political economists and their political allies attacked the new mathematical methods in large part because of politics, not only because of ideology. Economists using mathematical methods threatened the authority and power of political economists and their political patrons in the Party-state. These political economists and their political patrons, in turn, threatened the emerging power of the mathematical economists and their own supporters (Bockman 2000; Judy 1971; Gerovitch 2002).

Even though mathematical economics was a politically contentious and dangerous area of research, Kantorovich began working in it with zeal. In 1938, he had been hired to be a consultant for the Laboratory of the Plywood Trust to examine "a problem of distributing some initial raw materials in order to maximize equipment productivity under certain restrictions" (Kantorovich 1992). This analysis led him to see that many different economic problems had the same mathematical form. His short book, *Mathematical Methods of Organizing and Planning Production*, was published in the Soviet Union in 1939, and was immediately condemned as "capitalist apologetics" (Josephson 1997: 270). Kantorovich's 1942 work *Economic Calculation of the Best Use of Resources* was not published until 1958. Like Koopmans, Kantorovich conducted research for the military, applying his techniques to the problems of

[8] Nemchinov did the first Soviet "chess-board" balance of the national economy for 1923–1924, which was the first effort of its kind and, according to Nemchinov, was the inspiration for Wassily Leontief's input-output analysis (Zauberman 1960: 1).

cutting metal for tanks and laying minefields (Gardner 1990: 640). Some military and political elites saw benefits in mathematical economics and supported Kantorovich, and they successfully awarded him the Stalin Prize in 1949.[9]

With the death of Stalin and Khrushchev's condemnation of Stalin at the Twentieth Soviet Communist Party Congress in 1956, mathematical economics and the broader category of cybernetics received official support. As a result, newly organized economics institutes turned to publishing American management science works (Josephson 1997: 208). In 1959, Kantorovich openly criticized orthodox political economy at a conference. In 1961, cybernetics was legitimized in the Party program and there were more than forty institutes conducting research on mathematics economics (Gerovitch 2002: 269). Kantorovich, in fact, was awarded the Lenin Prize in 1965. Yet he was restricted in his foreign travel and publications because he worked in fields considered central to national security, including nuclear energy. Mathematical methods had gained powerful political patrons, especially Khrushchev, and a large amount of institutional resources. By the time Koopmans first contacted Kantorovich, Kantorovich was under attack from economists in the Soviet Party mainstream (and their allies) for his use of mathematical applications.[10]

THEIR FIRST CORRESPONDENCE

Koopmans learned about Kantorovich's work from Merrill Flood, a researcher at RAND. Flood had been inspired to analyze the "transportation problem" at a conference Koopmans organized in the summer of 1949, which resulted in the publication of a remarkably influential anthology, *Activity Analysis of Production and Allocation*. In late 1949 at RAND, Flood gave a paper based on RAND Navy tanker fleet data with his solution to a transportation-programming task, what he called the "Hitchcock Distribution Problem."[11] At the talk, Max Shiffman, "a mathematician whose duties included the reading of Russian technical literature," told Flood that the results reminded him of an abstract he had just read summarizing Kantorovich's work from 1942.[12] Flood asked Julia Robinson to follow up on the article abstract, and

[9] According to Katsenelinboigen, the military saw his ideas as helpful and sanctioned his ideas (1978–1979: 143).

[10] Koopmans recognized this: "Kantorovich is currently the target of a campaign to challenge his orthodoxy, accuse him of 'marginalism' and of going against the 'labor theory of value.'" Tjalling Charles Koopmans Papers, Yale University Archives, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Koopmans to Churchman, 5 Apr. 1960. (Henceforth cited as "Koopmans Papers").

[11] Koopmans Papers, Box 14, Folder 251, "Kantorovich, Translations, 1960," letter from Flood to Koopmans, 19 Apr. 1960, p. 1. Also see Koopmans Papers, Correspondence, Group 1439, Series 1.

[12] Flood wrote to Koopmans, "There seems no need to me to include the phrase 'a mathematician whose duties included the reading of Russian technical literature,' after naming Max Shiffman, because I am confident that Max simply cam [*sic*] across the Kantorovich abstract in the course of his normal work as a good research mathematician." Koopmans Papers, Box 14, Folder 251, "Kantorovich, Translations, 1960," letter from Flood to Koopmans, 13 June 1960, p. 2.

they "agreed that the problems treated were essentially the same."[13] In papers published in 1952 and 1953, Flood included a reference to Kantorovich's 1942 article (Koopmans 1960: 363).

Some years later, Koopmans followed up on the reference and wrote Kantorovich for the first time. In a letter dated 12 November 1956, Koopmans wrote the Russian: "Recently I had the opportunity to see a copy of your article, 'On the Translocation of Masses,' in the Comptes Rendus of the Academy of Sciences of the U.S.S.R. of 1942. It become immediately clear to me that you have in part paralleled but in greater part anticipated a development of transportation theory in the United States which has stretched out over the period from 1941 to the present and is still continuing."[14] Koopmans sent him offprints of some articles and a list of the most important articles in linear programming, as well as the names of relevant journals. He also requested offprints of Kantorovich's work, inquired where he published his work, and asked for further indication of its practical uses or theoretical developments. He ended his letter expressing the hope "that this letter may lead to an exchange of information between us."[15]

Kantorovich wrote back in early 1957, sending a list of works. He said that he was very interested in the American work in this area and knew in particular of Koopmans' "interesting book" *Activity Analysis of Production and Allocation*.[16] In the meantime, Robert Campbell, a young economist who was an expert on the Soviet economy, initiated a correspondence with Koopmans. Campbell sent an article he had translated by Kantorovich and his colleague M. K. Gavurin that was published in 1949, stating, "I was greatly interested to find in a soviet source the enclosed article dealing with the sort of transportation problem to which you have applied the technique of activity analysis."[17] Kantorovich then sent Koopmans *Mathematical Methods of Organizing and Planning Production* (1960 [1939]). Koopmans asked Campbell to translate it, and upon receiving the translation, wrote back, "The contents of the paper are simply amazing."[18]

THE ORGANIZATION OF SCIENCE IN THE STALINIST ERA

When Stalin took power in 1929, he sought to separate and isolate the emerging Soviet sciences from their disciplinary counterparts in the rest of the world. The

[13] Koopmans Papers, "Kantorovich, Translations, 1960," letter from Flood to Koopmans, 19 Apr. 1960, p. 1.

[14] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Tjalling C. Koopmans to Professor L. Kantorovich, 12 Nov. 1956.

[15] Ibid.

[16] Koopmans Papers, Box 13, Folder 238, "Kantorovich, Leonid V., 1960–78, n.d.," letter from Kantorovich to Koopmans in English, ca. 1956 or early 1957.

[17] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Robert Campbell to Koopmans, 13 Feb. 1957.

[18] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Koopmans to Campbell, 27 Mar. 1958.

Soviet Party-state highly restricted the communication of Russian scientists with those outside of the Soviet Union and their travel outside the Soviet Union. This isolation reached its peak between the end of World War II and the start of the Cold War. As a result of the isolation, few scientists could communicate across the Cold War border, and thus scientists had a monologue about the science practiced on the other side. Five years after Stalin's death in 1953, *Management Science* published Kantorovich's 1942 work, "On the Translocation of Masses." The publication of that work occurred at the end of the Stalinist-era organization of science just as a new organization of science emerged. In the Stalinist-era, neither side in the Cold War recognized the intellectual property of their supposed enemies. Though likely with some reservations, Koopmans merely informed Kantorovich that his "On the Translocation of Masses" would be published in *Management Science*. Kantorovich was never asked whether he wanted to have his article published and how he might want it published. However, reflecting a shift in the organization of science, Koopmans asked his colleague A. W. Tucker how he and his co-author, Harold Kuhn, felt when their edited book was translated and published in Russian without their knowledge. Tucker responded: "Harold and I have not been especially upset by the fact that the translation was made without our knowledge. This seems to be quite standard. Mr. Bailey, the Director of the Princeton University Press, is surprized [*sic*] that we were sent copies of the translation so promptly. I explained that this was due to you. . . ."[19] During the Stalinist-era organization of science, both sides of the Cold War border published each other's work without any concern, but the emerging conversation between American and Russian scientists rapidly changed this practice.[20]

American scientists did not consider Russian economists their colleagues and so wrote about their work without being concerned about what they might have thought of these comments. In his short preface to Kantorovich's article, Abraham Charnes stated that Kantorovich "is one of the most distinguished of Russian mathematicians" and did not identify him as an economist. He provided some reasons why readers of *Management Science* might want to read the article. He warned, however: "An effective method of actually acquiring the solution to a specific problem is *not* solved in this paper. In the category of development of such methods we seem to be, currently, ahead of the

[19] Koopmans Papers, Box 14, Folder 251, "Kantorovich, Translations, 1960," letter from Tucker to Koopmans, 13 June 1960.

[20] Cramer (1965) states that between 1917 and 1950, one billion copies of books protected by foreign copyright were published in the Soviet Union. Through the 1950s and into the 1960s, the Soviet Union did not adhere to any international copyright treaties. Tsarist Russia also did not respect international copyrights. At the same time, the United States did not sign the Berne Convention for the Protection of Literary and Artistic Works until 1988 (World Intellectual Property Organization, http://www.wipo.int).

Russians" (1958: 1). Kantorovich complained to Koopmans about Charnes' preface. When Koopmans told the editor of *Management Science*, the editor agreed with the complaint, writing, "I should have caught this myself, but I read Charnes' statement out of context and therefore allowed it to get into the Journal."[21] He had read it out of context because during the Stalinist-era organization of science it was perfectly acceptable to write negative and competitive comments about Russian scientists.

In the Stalinist-era organization of science, scientists on both sides of the Cold War border had restrictions on their publications. A broad range of scientific topics, including mathematical economics, was considered relevant to national security and thus could not be written about publicly. In addition, the majority of mathematical economists in both countries worked in some way, either directly or through grants, with the military. Many published works were printed for small lists of readers with security clearances—secrecy had priority over publication. With the shift in the organization of science, scientists sought to declassify their work and have it published in professional journals, meaning that it would be made public and thus allow them to have their work recognized.

The publication of "On the Translocation of Masses" occurred before the shift to a new organization of science and thus it sparked no scandal or controversy. Koopmans referred to Kantorovich's work in his enormously significant *Three Essays on the State of Economic Science* (1957). Upon receiving the bibliography of Kantorovich's work from Koopmans, George Dantzig added his contributions to linear programming to his chapter on "Origins and Influences."[22] A. Charnes and W. W. Cooper supported the publication of this text.[23] Just a few months later, the attempt to publish Kantorovich's older work would provoke an enormous controversy.

THE ESTABLISHMENT OF KANTOROVICH'S PRIORITY

The opening of channels of communication between scientists in the Soviet Union and the United States created a new organization of science worldwide. An emerging dialogue replaced the isolated monologue, intellectual property slowly began to be recognized, and works would now have to be published openly to establish priority. These changes created major controversies.

In early 1957, Koopmans told Campbell, "I do think that Professor Kantorovich's work is independent of similar work in the United States, and in fact has

[21] Koopmans Papers, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Churchman to Koopmans, 13 Mar. 1959.

[22] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Dantzig from RAND to Koopmans, 23 July 1957.

[23] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Cooper to Churchman, 27 Mar. 1958.

priority over most of it. I found this out recently when I traced a reference in the Proceedings of the Academy of Sciences of the U.S.S.R. in 1942. This abstract contains in only a few pages the mathematical essence of developments that took about ten years on this side."[24] After receiving Kantorovich's 1939 work, *Mathematical Methods*, Koopmans became more certain of Kantorovich's priority and committed himself to getting it published. Other economists agreed with him. Montias (1961) saw Kantorovich's work as a long-neglected historical contribution to the development of linear programming. Zauberman declared that it was more legitimate to claim "Soviet parenthood" for linear programming (1960: 2). Even *The New York Times* declared, "Professor Kantorovich is the inventor of linear programming" (Schwartz 1960). However, Koopmans also recognized, "I have the impression that some sensitivity has grown up on both sides of the curtain on the priority question involved."[25]

Koopmans sought to publish *Mathematical Methods* in a professional journal, such as *Management Science*. He did not want to have it published for only a small group of American scientists with security clearances. Neither Kantorovich nor Koopmans had been able to publish freely the work that would have clearly established either economist's priority. Wartime security restrictions stopped Koopmans from publishing his 1943 discovery until 1947, and even then, this publication only summarized his findings and contained no equations (Dorfman 1984: 284).[26] Koopmans' work thus remained within the world of those with high-level security clearances. Many of Kantorovich's writings were first delayed, and then printed with portions censored. As Mirowski has remarked, "Linear programming and game theory grew up in compartmentalized spheres in each country, with only a very few figures with security clearance having substantial access to cutting-edge research in both" (2004: 205).[27] This not only severely limited any dialogue between American and Russian scientists, but it also fragmented professional communication in the United States and Russia.[28] In response to several suggestions that Kantorovich's work be published by RAND, Koopmans said publishing

[24] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Tjalling C. Koopmans to Professor Campbell, 25 Feb. 1957.

[25] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Koopmans to Wassily Leontief, 28 Apr. 1959.

[26] We are using the reprint of the 1947 article, which appeared in *Econometrica* (Koopmans 1949).

[27] Mirowski states further: "There was also the phenomenon of some scientists having special access to Soviet developments through their military ties, but not fully acknowledging the nature and extent of their influence" (2004: 215).

[28] Following Brannigan and Wanner, we can perceive these kinds of publications as obstacles to scientific communication. Once military restrictions on publications were loosened, we see priority disputes. As mentioned earlier, Brannigan and Wanner find that priority disputes "have declined in time with the rise of professional scientific establishments which are particularly efficient in organizing the communication of research outcomes" (1983: 138).

in the RAND series "is not really publication. It goes to names on a mailing list, and one can write in for it, but it does not really enter into the public record in that way."[29] Only through a completely public channel could priority be established.

Furthermore, Koopmans was concerned that a military-related publication might pose risks to Kantorovich: "He could not be expected to approve of that medium except possibly in ignorance of its connection with the Air Force."[30] In the former organization of science, it would not have mattered whether RAND published Kantorovich's work, but in the new organization, there was new concern about foreign scientists, who were now considered colleagues. The consideration of the possible threat of publishing Kantorovich's work seemed not to have dawned on people before. W. H. Marlow, a translator and collector of his work for the Office of Naval Research, asked Koopmans, "Do I infer correctly that you anticipate possible unfavorable repercussions for Kantorovich in case certain things happen as a result of his published papers?" He then asked Koopmans' opinion on the potential effects of different types of publications he had either already done or planned to do.[31]

In contrast to the Stalinist-era organization of science, where intellectual property rights were disregarded, Koopmans sought to ask Kantorovich whether he wanted his article published and whether he approved of the translation. However, asking permission did not mean that the authors in East and West had full control over their publications. Koopmans told Cooper in early 1958, after they found a place to publish Kantorovich's article, "I would like to have an opportunity to seek the approval of the author, since the one available copy from which the translation is being made was sent to me by the author."[32] From the correspondence about Kantorovich's work, it is clear that, in the United States, the person who received the publication from the Soviet Union (and sometimes the translator of the article) was considered owner of the paper. If someone wanted to obtain the paper, they asked the person who "owned" the copy in the United States. With the lack of copying machines at universities at this time, often the paper was sent through registered mail at great risk of loss.[33] Even translators like Campbell did not necessarily

[29] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Koopmans to Campbell, 6 Feb. 1958.

[30] Ibid.

[31] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from W. H. Marlow to Koopmans, 26 Mar. 1958.

[32] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Koopmans to Cooper, 22 Jan. 1958.

[33] Koopmans sent the only copies of Kantorovich's work in the United States through the mail and declared: "Both of these pamphlets are, to my knowledge, the only originals in the United States. May I ask you to return them to me by registered mail when you have finished with them." Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Koopmans to Wassily Leontief, 28 Apr. 1959.

have copies.[34] While the American economists had trouble obtaining copies, Kantorovich himself had little control over his own work in the United States, its translation or its use, until it was made public in *Management Science*.[35]

GETTING KANTOROVICH PUBLISHED IN THE UNITED STATES

Trouble started brewing as soon as the work of translating *Mathematical Methods* began. The first problem with translation was that in the 1950s economists with Russian translation skills were in short supply and great demand. Many scholars relied on translators who had little knowledge of their fields or had large backlogs of translations.[36] Robert Campbell himself was very busy and had to delay his translation of Kantorovich's work several times.[37] Quite a few economists who knew some Russian made rough translations of Russians' work for themselves. Koopmans often relied on his colleagues at Yale, such as economist John Michael Montias, to make quick translations because he was too excited to wait for a full translation. Reflecting the intense American governmental interest in Soviet science, there was a great shortage of Russian translators. The Office of Naval Research funded two centers that did translations of Soviet mathematical economics works: the Logistics Research Project at George Washington University and the Logistics Project in the Department of Mathematics at Princeton University. Russian scientific works were also translated through a variety of other organizations, including the Library of Congress' National Translation Center, the National Bureau of Standards' Institute for Numerical Analysis at UCLA, the Foreign Documents Division at the CIA, and the National Advisory Committee for Aeronautics at NASA. Shortages of skilled people regularly hindered translation.

Often translators were not credited, which meant that the reliability of their work was unclear. For example, W. H. Marlow, the translator and collector of Kantorovich's work for the Office of Naval Research, had received a copy of an article by Kantorovich and his colleague Gavurin, which he was told was acquired in Europe by a private citizen who had no information concerning

[34] After sending his translation of Kantorovich (1939), Campbell wrote, "So I do not have a copy, and if it turns out that one of the copies should become surplus, I would like to have one back" (Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Campbell to Koopmans, 20 Mar. 1958).

[35] Ibid.

[36] In response to the demand for translations, U.S. government officials called for computerized language-translation programs. There were concerns that there were not enough translators to meet all the interest in Soviet science. Some of the largest translation operations were going on at RAND. These demands led to further development in computers. See *Language and Machines* (1966) for a full discussion of the concerns and recommendations.

[37] Campbell wrote, "It does go rather slowly. Particularly in view of the fact that I have to do it in time left over from other work." Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Campbell to Koopmans, 29 Jan. 1958.

the translator.[38] With the opening conversations among Russian and American economists, he found out that Robert Campbell had made the translation. Marlow was relieved that he would not have to retranslate the article, translated by so trusted a colleague. Marlow himself helped with the translation of *Mathematical Methods*, but Koopmans wanted him to state only his university affiliation, George Washington University, and not his links to the military establishment.[39]

Campbell and later Marlow declared that they made a literal translation of Kantorovich (1939), but it was not clear what an acceptable translation was. Koopmans had received *Mathematical Methods* from Kantorovich around mid-1957. Campbell completed his translation in March 1958, but said that it was literal and not yet finished. He stated: "It would probably be a good idea to read the translation with the original right at hand to refer to when equations or numbers don't seem to make sense. . . . I thought that the best thing for me to do was to make a literal, if somewhat flatfooted, translation, and then let a possible future editor worry about improving the style."[40]

Yet that Campbell did not fully finish his translation, and that arguments continued about the text itself, led to a long drawn-out period of "final editing." *Management Science* did not publish *Mathematical Methods* until July 1960. It took two years to publish the translation, which finally appeared twenty-one years after its publication in Russian.

According to Walter Benjamin, "Any translation which intends to perform a transmitting function cannot transmit anything but information—hence, something inessential" (1969: 69). Building on Benjamin, Ives (2006) argues that time lapses and changes in historical context can make literal translation impossible. In the present case, given the changes in the Cold War context, which allowed for the communication between Kantorovich and Koopmans and the potential formation of a common economics profession, translation of a text from the Stalinist period required further historical interpretation and evaluation. Are the terms used in the Soviet Union the same as those in the United States? How do you separate a translation from an innovation? Furthermore, though Campbell was an expert on the Soviet economy, as well as in American economic science, his knowledge of the Soviet Union was in flux since the system itself was changing. Would his particular perspective and training change the translation? Gramsci argued that a good translation could

[38] Koopmans Papers, Box 14, Folder 246, "Kantorovich, Papers and Bibliographic Materials, 1957–60," Inquiry. From Marlow, Serial 6648/58, 18 Mar. 1958. Marlow said he had been told "a colorful story" about how the text was obtained. Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Marlow to Campbell, 7 Apr. 1958.

[39] Koopmans Papers, Box 14, Folder 250, "Kantorovich, Translations, 1958–60," letter from Koopmans to Churchman, 9 Sept. 1958.

[40] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Campbell to Koopmans, 20 Mar. 1958.

change both the cultures involved because translation allows for the coming together of people and for them to change their points of view (ibid.). During the Cold War, however, translation could also be dangerous.

Translation requires assuming that a concept in one language is similar enough to that in another language. Both Kantorovich and Koopmans developed their ideas out of specific practical problems, reflecting the specificities of the contexts in which they worked (Dorfman 1984). The different nature of the Soviet socialist and American capitalist economies added further to the difficulties of translation. Kantorovich generalized his ideas from the specific case of the plywood industry within the Soviet planned economy. In an example from his 1939 work, Kantorovich shows how to obtain the maximum output of a given mix of woods from eight wood-peeling machines. The plan determines the proportion of each kind of wood that should be produced, and each machine has different levels of productivity for each of five kinds of wood. One seeks to calculate the time needed for each machine to produce each kind of wood ($h_{i,k}$) and multiplies this by the amount of each kind of wood each machine can produce ($\alpha_{i,k}$), which then would result in the maximum amount of each kind of wood (z_k) [41]:

$$\sum_{i=1}^{n} \alpha_{i,k} h_{i,k} = z_k$$

This equation also has constraints, such as the full use of all the machines, the total length of a working day, and minimal scrap or waste. The solution is calculated through "resolving multipliers," which are related to Lagrangian multipliers and would become a central point of contention for Western economists. Kantorovich's explanation reflects the world of planning. He focuses on production (rather than consumption), physical quantities, work norms and time, and only very briefly on prices and cost (1960 [1939]: 372, 378). He presents his method as a way to solve the common problems of planners and enterprise managers: shortages of materials and labor, the possibility of cooperation across enterprises, and problems with data collection. He also applies his method to a variety of areas of industrial production: woodworking, metalworking, oil refining, construction, agriculture, and freight transportation.

Koopmans launched into his writing with a different perspective, immediately discussing prices, costs, and "the satisfaction of the ultimate consumers" (1949: 136).[42] However, Koopmans developed his ideas at the Combined

[41] The equation here is slightly modified to allow for easy description. The left side should be divided by the proportion of each type of wood determined by the economic plan. This example is discussed in Kantorovich (1960 [1939]: 374–76).

[42] Based on work done in 1943, this article had first appeared in the *Proceedings of the International Statistical Conferences* in 1947 (Dorfman 1984: 284). We use a 1949 reprint.

Shipping Adjustment Board in Washington, D.C. during World War II (Dorfman 1984). His work thus reflects a non-market, state-organized environment similar to Kantorovich's plywood industry, though with some significant differences. In his article, Koopmans presented, in a tellingly non-mathematical fashion, the result of his wartime work on the most efficient ways by which to utilize allied shipping capacity to move troops and materiel among and between the various theatres of military operations. In that work, he had discovered that the *direct* costs of a particular transportation program (fuel, wages of crews, and so forth) were hardly more important than the *indirect* costs of deploying a ship for one mission rather than another. This is to say that, in Koopmans' words, the "controlling bottleneck" of the transportation problem was not the operating costs of a particular voyage but rather the number of ships available for that given purpose (1949: 137–38). Optimization thus required that the flow of empty ships be minimized—this in an effort to reduce the indirect costs (or "opportunity costs") as much as possible. That effort at minimization would have to be pursued subject to various operational constraints, such as the fact that the number of ships going into a particular geographical area (whether empty or laden) had to be equal to or greater than the number of ships leaving that area, and that the flow of empty ships out of a given region had to be zero or more. Similar to Kantorovich, Koopmans immediately recognized, as a result, that the entire "transportation problem" could be represented as a maximization function delimited by certain linear inequalities. Also similar to Kantorovich, his solution to this optimization problem was linked to the use of Lagrangian multipliers, although Koopmans interpreted his solution explicitly to involve the determination of efficiency prices.

The focus of the priority debate was on the nature of Kantorovich's method. Since he could not find a precise English term, Campbell left untranslated Kantorovich's term for his method, "*razreshaiushchie mnozhiteli*," which strictly translated meant "resolving multipliers."[43] Koopmans approved of "resolving multipliers" and went on to say, "The 'resolving multipliers' are, of course, the 'shadow prices,' 'efficiency prices,' 'Lagrange multipliers,' 'dual variables' or whatever they have been called in various contexts."[44] On the same day, Cooper wrote to C. West Churchman, the editor of *Management Science*, "I would prefer something like 'resolving ratios' to avoid confusion with

[43] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Campbell to Koopmans, 20 Mar. 1958.

[44] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Koopmans to Campbell, 27 Mar. 1958. To Koopmans, Kantorovich made an "explicit statement of the price implications of efficient allocation of resources" and he understood that the resolving multipliers "define[d] marginal rates of substitution" as early as 1939. Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Koopmans to Wassily Leontief, 28 Apr. 1959.

things like 'Lagrange multipliers.'"[45] While Koopmans immediately understood Kantorovich as talking about prices, it was politically dangerous to write overtly about prices in the Soviet Union because Stalin was directly involved in a long-running debate about the nature of prices in socialism (Pollock 2006).[46] Koopmans recognized the dangers of translation for Kantorovich. He pointed out: "We cannot be too careful in this matter, because Kantorovich is currently the target of a campaign to challenge his orthodoxy, accuse him of 'marginalism' and of going against the 'labor theory of value.'"[47] While Koopmans may have seen an "explicit statement of the price implications of efficient allocation of resources," and that Kantorovich understood that the resolving multipliers "define marginal rates of substitution" as early as 1939,[48] others would not have seen this to be obvious.

Koopmans recognized that George Dantzig and Abraham Charnes also did not agree with his interpretation of Kantorovich's work because they gave "greatest weight to a systematic and surefire computation method, and from that point of view they feel that Kantorovich's early work did not arrive at anything like the[ir] simplex method."[49] Koopmans in fact felt uncertain about his ability to evaluate Kantorovich's computational method and sent to many people a letter in June 1960, stating that his introduction to Kantorovich's *Mathematical Methods*, "was on purpose somewhat vague and noncommittal on the subject of the Kantorovich computational proposals . . . I do not feel particularly competent in this area. . . . Undoubtedly more work is needed to compare the Kantorovich methods with the computation methods in use in this country."[50]

Dantzig's simplex method was unquestionably a path-breaking discovery, but it remained unclear how Kantorovich's method might have also been an innovation and how it might relate to Dantzig's method. Marlow told Koopmans that it was reasonable that he was vague because further research was needed to evaluate the techniques.[51] Flood told Koopmans that Dantzig's

[45] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Cooper to Churchman, 27 Mar. 1958.

[46] Russian mathematical economist Aron Katsenelinboigen noted that more traditional Russian economists "were strongly opposed to the idea of interpreting the resolving multipliers as prices. Thus, conservative Soviet economists attacked Kantorovich's proposals as anti-Marxist on the grounds that he advocated a bourgeois theory of marginal utility for price formation" (1978–1979: 140).

[47] Koopmans Papers, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Koopmans to Churchman, 5 Apr. 1960.

[48] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Koopmans to Wassily Leontief, 28 Apr. 1959.

[49] Ibid.

[50] Koopmans Papers, Box 14, Folder 251, "Kantorovich, Translations, 1960," letter from Koopmans to Churchman, 6 June 1960.

[51] Koopmans Papers, Box 14, Folder 251, "Kantorovich, Translations, 1960," letter from Marlow to Koopmans, Serial 7542/60, 7 June 1960.

method did not work for a particular case of his, so he invented his own method, but Flood's assistant gave him "the impression that the Kantorovich computational procedure would be essentially equivalent to the simplex procedure."[52] Flood reassured Koopmans, "I am very glad that you are bringing out the Kantorovich literature so that we can all eventually understand the history of this interesting sequence of events better than we do now."[53] While the term "resolving multipliers" would remain in the text, that something had been discovered, and what precisely it was, were not clear.

What was at stake in these translations? The preexisting priority established by American economists in this and other areas was put into question. Furthermore, if, according to Koopmans, Kantorovich had made an "explicit statement of the price implications" and had found a means to solve linear programming problems in a planning system, then prices might work the same way in socialist and capitalist economies. If that were the case, then maybe socialist and capitalist economies were in fact similar. Moreover, if planning and markets were essentially the same, and if the Soviet system was as efficient, or even more efficient, than the capitalist system, then maybe capitalist economies should become more like socialist ones. The translation of Kantorovich's work and the determination of priority could dangerously bring together two cultures and change them.

The "final editing" became increasingly more difficult. In mid-1958, Churchman wrote, "Very simply, we need someone who understands Russian" to fix the English translation.[54] Koopmans had suggested a RAND translator, "Miss Nancy Nimitz"—who had translated and edited Russian works but did not seem to know mathematics—with Charnes to help with the mathematics.[55] After several attempts to find an acceptable editor of Campbell's translation, *Management Science* sent the text to W. H. Marlow.[56] By spring 1959, Churchman wrote, "The general confusion which has accompanied the 1939 article seems to be going on."[57] Then in 1960, Churchman wrote that the draft "has needed so much reworking that I have reluctantly had to postpone its publication until July . . . It seems to me that the English needs to be as

[52] Koopmans Papers, Box 14, Folder 251, "Kantorovich, Translations, 1960," letter from Flood to Koopmans, 19 Apr. 1960, p. 2; and letter from Flood to Koopmans, 13 June 1960, p. 2. Scarf later mentioned Kantorovich's 1939 work and Dantzig's 1951 work as two examples of the simplex method (1973: 7).

[53] Koopmans Papers, Box 14, Folder 251, "Kantorovich, Translations, 1960," letter from Flood to Koopmans, 19 Apr. 1960, p. 2.

[54] Koopmans Papers, Box 14, Folder 250, "Kantorovich, Translations, 1958–60," letter from Churchman to Koopmans, Cooper, Charnes, 23 June 1958.

[55] Koopmans Papers, Box 14, Folder 250, "Kantorovich, Translations, 1958–60," letter from Koopmans to Churchman, 8 June 1958.

[56] As mentioned earlier, Marlow was employed at George Washington University and funded by the Office of Naval Research.

[57] Koopmans Papers, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Churchman to Koopmans, 13 Mar. 1959.

smooth as possible."[58] Koopmans then asked whether the person doing the final edits knew Russian and mathematics. "What worries me," he noted, "is the possibility that the smoothing of the English might introduce unintended changes of meaning."[59] To Koopmans, Campbell lamented, "Had I known what was going to happen, I would have gladly spent some extra days on the translation to avoid the nearly two-year delay that has actually resulted."[60] Churchman then wrote, "I would think that in all fairness to the author, the translation should be tried out on someone who is not already familiar with the work."[61] The final edits seemed like they would never end.

A major obstacle to the publication of *Mathematical Methods* was that Charnes and Cooper had both requested that an interpretative article accompany the publication of Kantorovich's work. Koopmans refused to allow this, reminding others that Kantorovich did not want this and arguing that such an interpretative article might embarrass the Russians and thus damage scientific cooperation between the Russians and the Americans.[62] Charnes and Cooper, however, argued for a "summary descriptive article" that would place Kantorovich's piece "in perspective."[63] One can interpret their demand as a reaction to the new competitive space in which the priority of their work was put into question by a new Russian competitor. One can also interpret their demand as recognition that translation can be dangerous and that a literal translation is impossible. How could one literally translate an article from the Stalinist Russia of 1939 into the language of American mathematical economics of 1960? If these contexts are mutually translatable, then might this

[58] Koopmans Papers, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Churchman to Campbell, Charnes, Koopmans, Marlow, and Cooper, 11 Feb. 1960.

[59] Koopmans Papers, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Koopmans to Churchman, 16 Feb. 1960.

[60] Koopmans Papers, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Campbell to Churchman, 18 Feb. 1960.

[61] Koopmans Papers, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Churchman to Koopmans, 1 Mar. 1960.

[62] Kantorovich insisted that no commentary accompany the American publication of *Mathematical Methods* because such a commentary could give incorrect information. Kantorovich told Koopmans that Charnes' preface to his earlier article had wrongly stated that Kantorovich had not found a solution, when on page 3, it said: "The solution to this and more complicated problems of the same type the reader will find exposed in detail in a paper by L. Kantorovich and M. Gavurin which is soon to be published" (Kantorovich 1958 [1942]) (Letter from L. V. Kantorovich to T. C. Koopmans, 3 Feb. 1959, in Kantorovich, Kutateladze, and Fet 2004). Kantorovich and Gavurin had completed this paper in 1940, but it was not published until 1949, and even then, Kantorovich states, parts were cut (Letter from L. V. Kantorovich to T. C. Koopmans, 16 May 1960, in Kantorovich, Kutateladze, and Fet 2004). Koopmans agreed with Kantorovich's perspective: "I myself had found the concluding paragraph of A. Charnes in his preface to a reprint of 'On the Translocation of Masses' unfortunate and uncalled for" (Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Koopmans to Kantorovich, 4 May 1959). As discussed above, the editor Churchman agreed.

[63] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Cooper to Churchman, 27 Mar. 1958.

mean, following Gramsci, that the communists and the capitalists might come together and change their cultures as a result? To Charnes and Cooper, an interpretative article or preface was required to put Kantorovich "in perspective," meaning that he was to remain part of the Soviet economy, a mathematician and not an economist, and outside of the field of mathematical economics.[64]

Cold War ideology and the continual separation of the Soviet and American camps obscured the reality of mathematical economists' work. American mathematical economists saw Kantorovich as completely isolated from the American mathematical economics field. For example, George Dantzig called Kantorovich pioneering, but he also found that the Russian reminded him "of Mendel and his laws of heredity, which had no effect on subsequent developments in the field, and which were rediscovered by others years later."[65] Koopmans, however, recognized in 1957 that "Kantorovich is a mathematician well known to mathematicians in this country."[66] His mathematical work had been reported on in *Math Review* and *ACTA Matematica*. Kantorovich had already met and had correspondence with well-known mathematical economists from outside the Soviet Union. Recall that in 1935, he had already met John von Neumann and A. W. Tucker at a conference in Moscow. American economists had also read Kantorovich's work. For example, W. W. Cooper wrote, "I know the Novozhilov paper through a CIA translation which I saw some years back. It deals with proposals for applying Kantorovich's method to the Russian economy. I also know something of Kantorovich's reputation."[67] Both sides of the Cold War sought to downplay how much they knew about each other's work, in order to promote their individual contribution to linear programming.[68] The mathematical economics professions in both countries were developing in similar directions because they shared a similar field of mathematics, increased abilities of computational technology, state concerns with the

[64] Venuti (1995) has argued that the convention of erasing the translator and the foreignness from a text is inherently conservative. Instead, Venuti seeks to enhance the otherness of texts and challenge the dominant norms of the target culture. However, Charnes and Cooper wanted to "foreignize" Kantorovich's work, which may properly be seen as yet another representation of the profoundly conservative cast of American society during most of the 1950s.

[65] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Dantzig to Koopmans, 18 Apr. 1958.

[66] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Tjalling C. Koopmans to Professor Campbell, 25 Feb. 1957.

[67] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from W. W. Cooper at Carnegie Institute of Technology to Dr. C. J. Hitch at RAND Economics Division, 13 Jan. 1958.

[68] Some said that Kantorovich's work was historical and thus not relevant. The editor of *Management Science*, Churchman, could not understand why Kantorovich's *Mathematical Methods* should be published in *Management Science* because this journal did not publish "historical" works (Koopmans Papers, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Marlow to Koopmans, Serial 7062/59, 5 Mar. 1959). However, this perception flies in the face of the resources put toward translating and analyzing the work of Kantorovich and other Russians, especially by the Office of Naval Research and RAND.

economy and industry, and the centrality of the military. At the same time, many mathematical economists in both countries could not correspond or communicate, lacking access to limited circulation publications. And this included Koopmans.

After talking with Koopmans, Marlow refused to work on the translation until *Management Science* assured him there would be no interpretative articles or editorial comments.[69] Kantorovich wrote to Koopmans to tell him he wanted to make some small revisions to his *Mathematical Methods*.[70] Two months later, Kantorovich wrote that he wanted changes in a formula and two references to a political figure removed.[71] Marlow made these changes, along with some typos and page numberings, arguing that these "should be viewed as corrections of typographical errors . . . and for which no mention of change should be included."[72] Churchman further delayed the publication of *Mathematical Methods* because the draft "has needed so much reworking."[73]

Finally, Kantorovich's *Mathematical Methods* was published when Koopmans agreed to write an introductory note to the article. In the introduction, Koopmans (1960) gave some background to the article and sought to translate Kantorovich's ideas into the language of Western scholars of management science. He declared the article "an early classic in the science of management under any economic system" (ibid.: 364). What Kantorovich called "resolving multipliers" "have been called, among other names, 'efficiency prices' in Western economic literature" (ibid.: 364). Koopmans recognized, "There is little in either the Soviet or the Western literature in management, planning, or economics available in 1939, that could have served as a source for the ideas in this paper" (ibid.: 365).

As still another sign of the new organization of science, Koopmans discussed the introduction with Kantorovich. He argued that Kantorovich should be treated like any other mathematical economist. His work should not be rewritten or given additional interpretative comments or introductions, and should be understood on its own terms. Koopmans bolstered his arguments that such unprofessional treatment of Russian scientists would harm ongoing communication between scientists across the Cold War divide. Another economist recognized that the publication of Kantorovich's work reflected a new organization

[69] Koopmans Papers, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Marlow to Campbell, Charnes, Churchman, and Koopmans, Serial 7297/59, 9 Oct. 1959.

[70] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Marlow to Koopmans, 21 Jan. 1959. Translation of letter from Kantorovich to Koopmans, 16 Jan. 1959.

[71] The political figure was "Comrade Molotov," one of the leaders of the Soviet party-state during the 1940s and 1950s, who lost power after Stalin's death in 1953.

[72] Koopmans Papers, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Marlow to Campbell, Charnes, Churchman, and Koopmans, Serial 7297/59, 9 Oct. 1959 (original underline).

[73] Koopmans Papers, Box 14, File 250, "Kantorovich, Translations, 1958–60," letter from Churchman to Campbell, Charnes, Koopmans, Marlow, and Cooper, 11 Feb. 1960.

of science, "ending an era, a period of almost a century during which the vast majority of bourgeois and socialist economists were unable to communicate on matters relating to the allocation of resources" (Ward 1960: 545). Koopmans and Kantorovich had helped to forge this common professional field.

POST-PUBLICATION PRIORITY DEBATE

"There is a little storm blowing up in the Kantorovich teapot," wrote Koopmans to his friend Herbert Scarf in December 1960.[74] Kantorovich's paper had been published the previous July, along with Koopmans' introduction (Kantorovich 1960). The controversy around the priority in linear programming focused on what Kantorovich had actually discovered. Again, as Kuhn noted, that something is discovered and what is discovered are not immediately clear (1977). Added to this difficulty were the problems of translation, not only from one language to another, but also from one economic system to another, and the shift to a new organization of science at the global level.

Charnes and Cooper had been revising a text critical of Kantorovich's work, which they had published in an ONR series in 1958 (Charnes and Cooper 1958). After Kantorovich's work was published, they submitted their revised manuscript to *Management Science*, which the journal editor sent to Koopmans and Kantorovich. Through correspondence, it is clear that the article was extremely critical of Kantorovich's work. Harold W. Kuhn was asked to review it for publication: "My only excuse for not carrying out your request earlier is a very personal distaste for the kind of controversy which seems to have been generated. . . . At the risk of becoming involved in this brawl, I must say straight off that I found the original Charnes-Cooper manuscript unduly bellicose."[75] It is clear also that the article as finally published in 1962 in *Management Science* was significantly changed.

Charnes and Cooper (1962) responded to the new scientific organization that had emerged with direct communications between Soviet and American scientists. Within this new environment, Charnes and Cooper admitted, rather grudgingly, that they had to give some credit to Kantorovich, but they did not agree with the credit that Koopmans gave him. In determining what Kantorovich might have discovered, Charnes and Cooper argued that the history of management science should be examined back at least to the 1700s. They declared: "When this has been done, it will then be possible to place Kantorovich's ideas in their proper perspective" (ibid.: 251). Putting those contributions into "proper perspective" meant questioning any discovery by Kantorovich.

[74] Koopmans Papers, Box 14, Folder 239, "Kantorovich, Leonid V., Cooper-Charnes discussion, 1960, 1961," letter from Koopmans to Herb [probably Scarf], 7 Dec. 1960.

[75] Koopmans Papers, Box 14, Folder 239, "Kantorovich, Leonid V., Cooper-Charnes discussion, 1960, 1961," letter from Kuhn to Robert M. Thrall, editor of *Management Science*, 14 July 1961.

When they did give credit to him, they stated, "We are even inclined to doubt that anything more than limited credit can be assigned to him with respect to" his resolving multipliers (ibid.: 249). Yet, according to Charnes and Cooper, more research would have been necessary to determine whether resolving multipliers actually improved computational efficiency. After that research was conducted, "Proper credit should *then* be given to the source of this inspiration" (ibid.: 251).

In opposition to Koopmans' interpretations of Kantorovich's work, Charnes and Cooper assert that Kantorovich did not discover efficiency prices because he did not discuss them explicitly (ibid.: 250), even though in 1939 such a discussion would likely have been politically impossible. Charnes and Cooper also suggest that prices were different in socialist economies, which had "mere optimality in an arbitrary linear programming model," and in capitalist economies, which had "the general conditions for efficiency" (ibid.: 249), thus making the Soviet system fundamentally different from Western economies. To Charnes and Cooper, Kantorovich also did not explicitly discuss other topics important to Western economics and his methods of calculation were *ad hoc* (ibid.: 250–51).[76] These issues related to the general problem, according to Charnes and Cooper, that Kantorovich was talking about a Soviet economy, which Kantorovich himself acknowledged. His Soviet focus meant that he had incorrect constraints (full utilization of all machines) and did not relate his work to more classical constructs in economic theory (ibid.: 252, 254). After giving little credit to Kantorovich, Charnes and Cooper concluded their article thus: "It would therefore be unfortunate if any such possibilities for success that might emanate from Kantorovich's earlier work were lost simply because of inadequate understanding of his actual accomplishments and in what respects they have, to date, differed from (or complemented) work that has since been undertaken by others" (ibid.: 261).[77] While this statement appears to open up discussion, the article places Kantorovich securely in the Soviet camp and outside any professional field involving Western economics.

When he received Charnes and Cooper's manuscript, Kantorovich immediately wrote a reply, which he sent to *Management Science* and to Koopmans. To Koopmans, he wrote, "To me it is very annoying to see such a reaction to this work, but I am still more vexed because of the unpleasantness which this apparently caused you."[78] In his response to Robert M. Thrall, the new editor of *Management Science*, Kantorovich stated that he did not want his comments to be published, though he wanted Charnes and Cooper to read

[76] The most important problem for them was that Kantorovich did not explicitly discuss duality.

[77] Interestingly, and perhaps revealingly, Charnes and Cooper also state, "We are not trying to criticize Kantorovich (or any one else) merely because his interests or tastes might differ from our own" (1962: 255).

[78] Koopmans Papers, Box 14, Folder 239, "Kantorovich, Leonid V., Cooper-Charnes discussion, 1960, 1961," letter from Kantorovich to Koopmans, 31 Dec. 1960 (Richard Judy, trans.).

his comments.[79] Kantorovich wrote, "I cannot understand the aims and internal meaning of this article. I want to observe that the tone of this article is not conducive to discussion." He recognized that some of the misunderstandings might have resulted from the fact that Soviet works had "dropped out of the sight of U.S. scholars" and that many ideas were discovered earlier than they were published. As an additional source of misunderstanding, Charnes and Cooper read Koopmans' introduction as affirming that in Kantorovich's and other Soviet articles were "contained almost completely the contemporary theory of the problems of linear programming and the methods of their solution."[80] Instead, Kantorovich wrote, Russian mathematical economists followed Americans' work and "highly value their achievements."[81] He outlined his accomplishments in the pre-Second World War period and what he had not done in this period, noting, "There are yet very many discoveries that cannot be attributed to, and no one attributes them to, Kantorovich."[82] In conclusion, Kantorovich wrote, "in the thirty years of my scientific activity, I have never encountered a mathematical work written with such lack of restraint as this one."[83] Kantorovich never published his reply, though *Management Science* tried to convince him to do so.

In their manuscript, Charnes and Cooper referred to at least one publication that had not been openly published: Isbell and Marlow's original 1958 translation of Kantorovich's 1957 article for RAND. In his response to Charnes and Cooper, Kantorovich asked for a copy of this translation. In it, Isbell and Marlow had stated that he had made a mistake in his proof. Kantorovich wrote, "We have not found any mistakes in it and I should be most grateful if the corrections made by doctors Isbell and Marlow were to be sent to me."[84] Koopmans encouraged Isbell and Marlow to publish their work in order to allow Kantorovich to respond, and in 1961 they published "an improved translation" in *Management Science*. They recognized that it was possible that Kantorovich had had the correct version in mind because he had used the correct proof in 1958. They also stated, "We have deleted from our original manuscript an erroneous remark occasioned by our insufficient acquaintance with Kantorovich's terminology" (1961: 17, n. 3). Some publications made for scientists with security clearances had to be made available and revised to fit the new scientific organization for the public record.

[79] Koopmans Papers, Box 14, Folder 239, "Kantorovich, Leonid V., Cooper-Charnes discussion, 1960, 1961," letter from Kantorovich to Thrall, 31 Dec. 1960. Parts of the translation are handwritten and others typewritten. The handwritten translation is by Richard Judy, but it is unclear who did the typewritten translation. In the same file, there is also a revised translation organized by *Management Science*, which we do not quote in this paper.

[80] Ibid., typed p. 3.

[81] Ibid., 4.

[82] Ibid., 7.

[83] Ibid., 7.

[84] Ibid., 5.

Koopmans responded to Charnes and Cooper in *Management Science*. He basically argued that Kantorovich had made a significant contribution and supported his translations. Koopmans said: "I used the terms 'dual variables of linear programming' and 'efficiency prices' because these terms are familiar to readers of the American literature" (1962: 264). He did not understand why Charnes and Cooper held that Kantorovich had not anticipated what had come after 1939 in the West. He went on, in more pointed fashion:

> Neither do I understand the preoccupation of Charnes and Cooper with temporal priority. Is the glory of American developers of linear programming in any way diminished if it turns out now that, unknown to them, important aspects of linear programming models and theory had been anticipated in another language and another economic environment, with which communication has been somewhat difficult? If an element of national pride is involved, can we not justifiably point to a conspicuous discrepancy in the time span between the development and application of linear programming ideas and techniques in two environments? (1962: 264–65)

Koopmans, remarking on Kantorovich, had opined, "The concluding discussion anticipating objections to the methods of linear programming has a flavor independent of time and place" (1960: 364). By 1970, if not earlier, Koopmans felt that there was a common field of economics. He supported scholarly exchanges, "because of a mutually perceived mutual interest in problems in this area … the fundamental unity of all economic thinking will in time bring about the spread of ideas from areas in which exchanges are at present favored into other areas … the present trend of mathematical methods in Soviet economics is upward."[85]

TRYING TO MEET

Though Soviet and American economists had many shared interests, it was difficult for them to meet except at conferences and during periodic, short visits. In the early 1960s, many American-based economists sought to establish contact with Russian and East European economists. Conferences were sold as having "Possible Soviet Participation."[86] It was extremely difficult for Russian economists to participate in conferences abroad because the Soviet authorities often restricted their travel, or would sometimes grant them travel permission

[85] Koopmans Papers, Box 22, Folder 451, "Report to the National Academy of Sciences on an Exchange Visit to the USSR, June 10—July 4, 1970," quote p. 9.

[86] One brochure stated: "The Chapel Hill Summer Conferences for 1960 are on the formal list of possible exchanges agreed upon by the National Academy of Sciences and the Academy of Sciences of the Soviet Union. Accordingly, invitations have been issued to several Soviet computer scientists, hopefully to be drawn from a list of proposed names including Academicians Dorodnitsin and Sobolev, Professors Liapunov, Shura-Bura, Kalushnin, Glushkov, and Kantorovich, and working computer scientists Ershov, Korolyuk, Zalgaller, and others." Koopmans Papers, Box 14, Folder 241, "Kantorovich, Leonid V., Invitation to The Institute of Management Sciences meeting, 1959, 1960," letter from Koopmans to John W. Carr III, 16 June 1960.

only at the last minute. Russian economists knew they were straining the patience of their Western guests. Koopmans and others unsuccessfully invited Kantorovich to the meetings of the Econometrics Society and The Institute of Management Sciences (TIMS).[87] Koopmans wrote in a letter to a colleague, "As you know, Russian visitors have been notorious for making up their minds in the last minute, presumably because their minds were made up for them by more powerful administrators who did not come around to it until they had to."[88] About nine months after being invited to one TIMS conference, and close to one month before it took place, Kantorovich finally told Koopmans that he would not attend.[89] The Soviet authorities allowed Kantorovich to visit the West on two occasions to receive honorary doctorates, but he traveled extensively only in 1975 when he received his Nobel Prize.[90]

Kantorovich did meet with foreign scholars in the Soviet Union. A colleague of Koopmans, Raymond Powell, met with him in Leningrad in 1957. He later wrote, "The interview was the oddest of my experiences in the Soviet Union. Kantorovich asked me to meet him in the reception hall of the Academy of Sciences rather than in his office, the two being some two or three miles distant from one another. He came with a young student or assistant, who said nothing. I took along my Intourist guide, a very pleasant and intelligent young lady, to serve as interpreter. No one else was in the room while we were talking. The peculiar aspect of the exchange was that Kantorovich appeared almost paralyzed with nervousness. He barely spoke above a whisper. . . ." Such meetings, especially with official "minders" present, did not provide much opportunity for open professional discussion.[91]

Koopmans finally did meet Kantorovich in person in the Soviet Union, in 1965 and 1970. During his 1965 trip, he visited Kantorovich's private home and gave a lecture to mathematical economists at a meeting his host had organized. He also met with the major mathematical economists and visited the major mathematical economic institutes in the Soviet Union, as well as in Hungary and Poland.[92] In 1970, Koopmans again met with a large number of mathematical economists and their institutes. He participated in a "Symposium on Models of the National Economy" organized by Kantorovich, with about two hundred participants. He found the quality of the papers "on

[87] Koopmans Papers, Box 14, Folder 241, "Kantorovich, Leonid V., Invitation to The Institute of Management Sciences meeting, 1959, 1960," letter from Cooper to Koopmans, 30 Nov. 1959.

[88] Koopmans Papers, Box 14, Folder 241, "Kantorovich, Leonid V., Invitation to The Institute of Management Sciences meeting, 1959, 1960," letter from Koopmans to Cooper, 22 Apr. 1960.

[89] Letter from L. V. Kantorovich to T. C. Koopmans, 13 Sept. 1960 (Kantorovich, Kutateladze, and Fet 2004).

[90] He was allowed to go to Glasgow University in 1967 and to Helsinki University in 1971, but not to Yale University in 1970, to receive honorary doctorates (Kantorovich et al. 2002).

[91] Koopmans Papers, Box 13, Folder 237, "Kantorovich, Leonid V., 1956–59," letter from Raymond Powell to Koopmans, 19 Aug. 1957.

[92] Koopmans Papers, Box 22, Folder 450, "Report of Visit to the USSR, May 9–June 9, 1965."

a par with the best work in mathematical economics in other parts of the world," and the paper topics "not very different from corresponding papers in 'western' professional journals."[93] Nevertheless, these trips still did not resolve the difficulties of having a close working relationship between economists in the East and the West.

Established in 1972 in Austria, the International Institute for Applied Systems Analysis (IIASA) formalized the East-West mathematical economic dialogue. Koopmans was one of its main visionaries and advised its directors for many years. IIASA was created to bring together scientists from East and West to work on problems that plagued all advanced societies. The objective was to apply to such problems the techniques of systems analysis, cybernetics, operations research, and management.[94] In 1973 and 1974, Koopmans and Dantzig ran a methodology research group at IIASA. In 1975, both Kantorovich and Koopmans won the Nobel Prize.[95] Koopmans donated part of his prize money to IIASA.[96] Based on a 1975 seminar in Sweden, Koopmans and Kantorovich wrote a book together entitled *Problems of Application of Optimization Methods in Industry*. Kantorovich traveled around the world, including to the United States, India, and IIASA in Austria. In 1982, IIASA honored Kantorovich with a seventieth-birthday party, and he joined their discussions about global modeling. While IIASA suffered many difficulties, especially because it was trying to both encourage East-West collaboration and high-quality research within an increasingly hostile political environment, it provided a permanent forum for mathematical economists and other scientists from East and West.[97]

[93] Koopmans Papers, Box 22, Folder 451, "Report to the National Academy of Sciences on an Exchange Visit to the USSR, June 10—July 4, 1970," quote p. 5.

[94] In addition to encouraging communication between scientists and creating a form of détente, IIASA was intended to help undermine socialism. There was a general idea at the time that "Economic rationality can lead to ideological erosion" (Abram Bergson quoted at meeting on 23 Feb. 1967, Ford Foundation Archives, Industrial Societies Project). The organizers of IIASA thought that enabling individuals to make more free economic decisions "might also be a step toward greater political and intellectual freedom" (Ford Foundation Archives, Industrial Societies Project, Report by Herbert Block to Staples at the Ford Foundation, Feb. 1, 1967). Both East and West could also use IIASA for intelligence purposes. IIASA was thus seen as a Cold War tool.

[95] According to one report, the Nobel Prize committee had made a preliminary decision to give Kantorovich the prize in 1973, but they had to have someone investigate whether this was a good idea. The committee sent one of their members to the Soviet Union unofficially to ask Kantorovich whether he thought he should receive the prize. Kantorovich asked that the prize be delayed ("Commentary" in Kantorovich, Kutateladze, and Fet 2002: 508–11).

[96] Koopmans Papers, Box 20, File 389, "National Academy of Sciences, International Institute for Applied Systems Analysis, Advisory Committee 1976 Jan–Nov 4," letter from George S. Hammond, NAS to Roger Levien, IIASA, 19 July 1976. Koopmans has donated $34,000 to NAS to do systems analysis.

[97] Levien (2000) and Brooks and McDonald (2000) discuss many of the problems that IIASA had. These can also be found in Koopmans Papers, Box 12, Folder 218, "IIASA Directorship, 1971–1981."

DISENCHANTMENT?

At the same time as Kantorovich was given the freedom to travel, the American economics profession was losing interest in the Soviet planning system and planning in general. Economists who had before sought out trips to the Soviet Union were now disappointed by what they found there. As a part of this group, Koopmans, too, felt this way by 1977. He observed, "The principal direct U.S. gain from further and even more intensive exchange activities in economics that I perceive is not so much to learn from our Soviet colleagues (I have been disappointed on that score) as to learn about them, about their clumsy economy, and about the extent of their impact on the management and planning of the economy, which likewise falls short of its potential."[98] The Brezhnev regime had re-appropriated the innovations of cybernetics and used them to support the status quo in the Soviet Union (Gerovitch 2002). Visiting economists sensed the profound limitations of this environment.

In the East and the West, the principle object of economics shifted from the "economy" to "markets" (Bernstein 2001; Bockman 2000). Reflecting this shift, American economists left behind linear programming. As Gass recognized, "Since the early involvement of economists with linear programming, there has been, with a few notable exceptions, little research by economists in this field. The burgeoning of linear programming is due to theoretical and applied efforts of operations researchers, mathematicians, numerical analysis, and computer scientists" (1989: 148).

Economists in East and West began talking about the costs of computability and whether the large-scale plans of linear programmers were realizable. East European economists called for further reforms to create market socialism, criticizing large-scale modeling efforts and arguing for the creation of market conditions. The shift away from linear programming, cybernetics, and other fields that linked East and West was finalized with the end of the Soviet Union.[99] Replacing large-scale optimal allocation techniques, economists offered the workings of a "free market" reified to the status of natural law. According to this view, the market had to be allowed to function "naturally," which meant a new role for economists, a reconfigured view of the "economy," and the end to the bridge between East and West. New bridges were built based on other forms of economics that were not so much a dialogue between partners as a one-way conversation dominated by the American economics profession.

[98] Koopmans Papers, Box 22, File "Soviet Exchanges, 1970–83," letter from Koopmans to Dr. Carl Kaysen, Chairman, BIS Review Panel on U.S.-USSR Exchanges and Relations, 8 Feb. 1977.

[99] Mirowski observed "With the destruction of the Soviet science base in the 1990s, and the loss of interest of Westerners in Soviet science, knowledge of the achievements of specific research traditions is rapidly being dissipated" (2004: 215).

CONCLUSIONS

The academic connection between Koopmans and Kantorovich, as well as other Soviet and Western scientists, ended the Stalinist-era organization of science—an epoch during which Soviet and non-Soviet science were isolated from each other and deprived of dialogue. The shift to a new organization of science in the mid-1950s meant that Russian and American economists could start to consider each other colleagues. They could also mobilize new resources for publication and thus resolve disputes regarding priority with more integrity and clarity. At the same time, they encountered the opportunity to develop a vision of a common economics profession. Yet, as evidenced by the debate surrounding Kantorovich and Koopmans, the creation of these foundations for a new scientific community ironically stoked priority controversies themselves.

At the same time, the Cold War blinded those on both sides of the iron curtain to the collective work that Russian and American mathematical economists and mathematicians in fact continued to do in the face of Stalinist and post-Stalinist obstacles to their communication. In contrast to Dantzig's view that Kantorovich was like Mendel, working in complete isolation, American and Russian mathematical economists were working in similar ways, with similar sources, and with similar fascinations, though in the early Cold War they sometimes could not talk directly to each other. After all, mathematicians around the world knew Kantorovich. American and Soviet economists also had knowledge about each other's work through the CIA, military-funded institutions, and semi-secretive publications. So, each side influenced the other's science.

The worldwide recognition in 1975 of Kantorovich's and Koopmans' work happened at the same time as the American economics profession began to turn away from the study of the "economy" and the use of such techniques as linear programming. While engineering, management sciences, and other disciplines continued to work on linear programming and in related fields, American economists left behind this bridge between East and West. A new convergence emerged around the need for "naturally" free markets and the understanding of Soviet economics as merely ideological and thus not part of common economic science. The earlier translations of Russian economists' work, like that of Kantorovich, no longer threatened to bring together socialism and capitalism and change both cultures, as Gramsci suggested a good translation might do. Rather, the hard work of Koopmans and Kantorovich to create a common economic science based on mutual respect and collaboration was, in the end, replaced by a new form of monologue.

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