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Turning Back the Clock: Retrospective Renderings of Social Field Research

I was recently invited to give a talk at the Academic Council of the Branch Institute for the History of Science and Technology in St Petersburg. The request was for me to reflect on how I became interested in Soviet science and to outline the research I did with Soviet scholars and scientists from the 1970s through the 1990s. The audience was also interested in my activities since that time and, in particular, my 2011 publication on governmentality through science communities. This article is a summary of my presentation, which took place on September 23, 2014. It is a blend of research interests and personal commentaries, necessarily succinct in its attempt to survey such an extensive period.

In order to address the question of my initial interest in Soviet science, we must go back to the mid-1950s. I was at Hunter College High School, which had an accelerated academic program for young women to prepare for the top colleges in the United States. The School was selected by NASA to introduce a course on experimental physics. The idea was to learn physics, not from books, but through the creative process of team research. I was fascinated by the concepts and laws of physics, especially as they related to the nature of the universe and the mysteries of subatomic particles. Along with physics came courses in calculus, which I found equally engaging – solving puzzles and figuring things out by myself. I was 13 years old.

In the following year, Sputnik transformed the way people thought of the possibilities of space travel. I dreamed of becoming an astronaut, but those hopes faded when I learned more about the NASA program. The early astronauts were test pilots, not physicists, and women were not being recruited. It wasn't until twenty-six years later (1983) that Sally Ride (physicist) made history as the first American woman in space aboard Space Shuttle Challenger. I continued my study of physics, nonetheless, and looked for other ways to channel those interests. I started to learn Russian, initially as a language to support my studies in theoretical physics, but soon fell in love with Russian literature and culture. I was also deeply immersed in the study of Russian history and was intrigued by what was then the “mystique” of the Bolshevik revolution. The science student was moving further into the humanities, where issues seemed equally complex and inviting.

My introduction to politics came with the John F. Kennedy presidential campaign in 1960 and the rhetoric of the cold war. The Soviet Union was seen as a technological power and adversary to the United States, while at the same time it was appreciated as a country with beautiful traditions in the performing arts and classical literature. I wanted to understand more about Russian society and politics in order to place this in perspective. I therefore moved from the natural sciences and mathematics to Russian area studies and political science as I completed my undergraduate degree at Hunter College of the City University

of New York. When I became a doctoral student at Indiana University in 1963, I met Loren Graham, who was in his first year of teaching there. His course on the history of Russian science opened the way for me to bring together my long-standing interest in science and my more recent work in politics and society. An inspirational teacher, Professor Graham encouraged me to pursue the study of science and scientists in the Soviet Union. I decided to do so, but from a different perspective — namely the vantage point of political and social theory.

My approach to the study of Russian science and scientists was influenced by the development of political science as an academic discipline. In the 1960s there was a major shift from institutional and descriptive studies to a greater focus on individual actors and how they behaved in social-psychological environments. Political scientists borrowed key concepts and research techniques from sociology, psychology, and (later) anthropology. These shifts were evident in my publications, which were initially focused on the formal institutions of the USSR Academy of Sciences and science policy issues, and later on the behavior of scientists in research collectives. I maintained my interest and involvement in policy research, but my field research in the Soviet Union and most of my collaboration with Russian (and Ukrainian) colleagues had to do with the sociological perspectives on science.

I want to highlight three subject areas where I conducted my research with Soviet scientists and scholars from the late 1960s through the late 1990s: (1) social studies of science as an academic field of inquiry, (2) survey research and public attitudes toward science and scientists, and (3) professional activities and informal networks in Soviet scientific communities. I worked with Russian/Soviet scholars and conducted my own field research in each of these three areas.

The first such collaboration took place in 1974 when I first met Samuil Aronovich Kugel' and Semyon Romanovich Mikulinsky at this institute. I had been researching Soviet work in the field of *naukovedenie* since 1970 and had drafted my manuscript on *Soviet Sociology of Science* based on materials available at the U. S. Library of Congress. I had also been conducting research on the career patterns of Soviet scientists. In 1974 I was accompanying a group of students on a study program that included a week in Leningrad. I arranged to have some free time so that I could come to this institute. I arrived here without the official *propusk* and no formal appointment. I simply told the security guard that I wanted to meet S. A. Kugel'. Fortunately for me, he was here, along with Mikulinsky, who was Director of the Institute for the History of Science and Technology in Moscow. I had the pleasure of meeting both of them, and that was the beginning of a collaboration that continued for many years. In my subsequent trips to Russia, Professor Kugel' hosted me at the Leningrad Affiliate, and Professor Mikulinsky hosted me at the Institute in Moscow.

My book *Soviet Sociology of Science*, which was published in 1976, benefited from the inclusion of publications that I received from both of them. This volume was the first systematic review published in the United States on Soviet scholarship in the field of *naukovedenie*. It made the sociological work done in Russia, Ukraine, and the Baltics more accessible to academic and policy communities outside the USSR. When I returned to Leningrad and Moscow under the US-USSR exchange program between the National Academy of Sciences and the USSR Academy of Sciences in 1978, I had the opportunity to meet the scholars whose work was cited in my book. For part of that trip I was hosted in Leningrad by I. I. Leiman at the Institute of Socio-Economic Problems and in Moscow by Mikulinsky. In addition to the Institute for the History of Science and Technology, I worked with colleagues at the Institute of Sociological Research (both in Moscow and Leningrad), the

Institute of Cybernetics in Kiev (where I started a collaboration with Gennady Dobrov), and Akademgorodok (where I interviewed prominent scientists about working conditions in their institutes). These meetings and research activities contributed directly to my chapter on "Scientific Collectives," in *The Social Context of Soviet Science*, which I edited with Susan Gross Solomon (published in 1980).

Meanwhile, I was continuing my collaboration with Samuil Kugel', which took shape through the direct exchange of publications, the sharing of scientific contacts, and discussions of common research interests. I found his work on professional mobility to be extremely useful and directly related to my own research on the career patterns of American and Soviet scientific elites. Professor Kugel's work was initially part of my investigation into the sociology of scientific research and the broader academic field of *naukovedenie* in the Soviet Union. But his projects also addressed social dynamics *inside* scientific collectives, where I had a particular interest. The specificity and breadth of Kugel's research changed the way we looked at scientific personnel in Russia. It allowed for the possibility of comparative assessments across different regions and time frames. This was very valuable for the building of hypotheses and empirically based science policy.

Among the scholars in Russia whose work was closest to mine was George Dumenton, with whom I maintained a long-standing professional relationship. His careful documentation of professional ties within and between scientific institutes documented how informal relations led to the re-structuring of formal institutional arrangements. Although we used different methodologies, we were exploring very similar social activity, and we drew similar conclusions. I met with many other Russian scholars over the years, all of whom were generous in sharing their ideas with me and from whom I learned a great deal, including for example Svetlana Gurchich and Eduard Mirskii. The social system of science, informal network ties, scientific communications, and the ethics of scientific research were among our common interests.

A second area of my work in Russia was survey research and public attitudes toward science and scientists. To place this in a broader context, I must again refer to my first trip to Leningrad in 1974. Before my trip I had read the publications of Vladimir Yadov, who was already highly regarded in the United States as a pioneering sociologist for his empirical social science research and theoretical approach. I arranged to meet him in 1974 to discuss the nature of his surveys and their implications for the future of sociology in the Soviet Union. He gave me a deeper insight into the challenges of social science inquiry in Russia and a greater appreciation for the limits placed on how one could interpret the results of such research. In 1981 I coordinated a project with several American colleagues on survey research and public attitudes in the Soviet Union (see Welsh 1981, 13–79), and I continued to follow the publication of Soviet survey data on a wide range of social issues. I was also doing research on the broader public image of science and scientists. This interest stemmed from my earlier work on science as a cultural and social phenomenon and, as more survey data became available, I focused on public knowledge of, and attitudes toward, science and technology in different countries.

Throughout the 1980s and into the early 1990s, for example, there was growing interest in the United States, Canada, Europe, and Japan in the possibility of expanding the countries involved in cross-national research on public attitudes toward science. Toward this end, the Science Indicators Study Group at the National Science Foundation asked me to travel to the Soviet Union in 1990 to assess the feasibility of including the USSR in these comparative studies. I met with sociologists, public opinion specialists, and *naukovedy* in

Moscow and Leningrad, including, for example, Samuil Kugel' (who had a great deal of interest in the idea, but no funding), Vladimir Yadov (who discussed methodological issues and other limitations), Yurii Levada at VTsIOM (all-Union Centre for the Study of Public Opinion, which at that time did not have a fully trained professional staff to do this kind of research), and with other researchers who expressed interest. Although some of the scholars I spoke with were enthusiastic about the idea doing a survey of public attitudes toward science and technology, comparable to the ones conducted by NSF, many of them cautioned that the early 1990s was not the best time to embark on such a project.

The first two subjects that I have addressed – namely *naukovedenie* as a field of study, and surveys of public attitudes toward science and technology – were potentially useful in the formation and administration of government policies in areas of education, employment, project planning, and support for scientific research. Of course, that would make such research politically relevant to policymakers and managers. The next subject area is political in a different sense.

A third area of my research was (and continues to be) the professional activities and informal networks in Soviet scientific communities. This project was initially designed as a challenge to the traditional paradigms of Western scholarship that depicted Soviet society as a totalitarian system constrained by institutional rigidity and regime dominance. My preliminary research in the late 1960s and my visits to Russia in the 1970s supported an alternative view of social and political change from below. The professional achievements of Soviet scientists and their capacity “to work the system” suggested a more fluid social fabric and a more politicized community. This may not have been the case in all sectors of society, and certainly not to the same degree, nor expressed in the same way, in different communities and social strata. Nonetheless, it was important to study wherever it was present. Therefore, instead of focusing on the institutional and political limitations on scientific research, I decided to focus on Soviet scientists as professionals who engage in the intellectual pursuit of knowledge in their respective fields and in the social networks that are a normal part of the scientific process.

To begin the study, I created an extensive biographical data base of all academicians and corresponding members of the USSR Academy of Science in the fields of physics, chemistry, and biology between 1920 and 1985. I used this to trace the personal and career histories of 577 scientists within and across institutional boundaries. I complemented this with a second data base consisting of co-authorship histories for 103 of the physicists, chemists, and biologists in the study population. The co-authorship data were processed through sociometric techniques that measure the proximity of pairs of coauthors and takes into account the cumulative co-authorship connections of each individual, both directly with other scientists and indirectly through intermediary co-authors ($n = 694$).

The results showed that Nikolai Dmitrievich Zelinskii was at the center of co-authorship networks for the period 1929–1940, and Aleksandr Nikolaevich Nesmeianov was at the center for the periods 1953–1964 and 1965–1976. Research on informal networks brought me deeper into the history of Soviet scientific communities (from the end of the 19th century through the Stalin, Khrushchev and Brezhnev periods) and allowed me to document a “hidden” social structure of science with a political dynamic of its own. The informal networks among Soviet physicists, chemists, and biologists revealed the development of a complex system of interlocking and overlapping channels of professional communication that cut across the formal, hierarchical chains of command in the USSR Academy of Sciences. These preliminary results were published in a journal article on “The Hidden

Structure of Soviet Science” in 1993. The article demonstrated how professional cliques and their associated networks overlapped during the period of 1965–1976 and centered around four individuals: Nesmeianov, Viktor Ivanovich Spitsyn, Aleksandr Naumovich Frumkin, and Nikolai Markovich Emanuel’. Each of them combined formal positions of influence in Academy decision making with informal ties to colleagues in different research institutes. In other words, they represented the points at which social circles were “pegged” to the Academy’s power structure.

At the political center of the bargaining process for the allocation of resources, these four scientists served as power brokers for the interests of chemists in different fields of research. From other information in their life histories, it is clear that they sometimes cooperated and sometimes competed with each other. Individually and collectively, therefore, they tried to influence decisions in ways that were beneficial to their research and professional careers (Lubrano, 1993, p. 158–159).

Quantitative data are useful for an empirical “mapping” of networks, but they cannot provide contextual interpretations of realities underlying those networks. For that we need qualitative data. In order to see how and why the Zelinskii and Nesmeianov networks developed and what they meant, I conducted in-depth interviews in the early 1990s with scientists in Russia who were either members of their networks, or closely associated with them. I interviewed research partners, students, and family members wherever possible, including individuals whose historical memories were vital to authenticate the personal and professional relationships that existed among scientists around Zelinskii and Nesmeianov. Interviews with other scientists and science administrators further clarified the distinctions between mentor-student relations, research partnerships, friendships and family ties. I also maintained a correspondence with some of the people I interviewed in which they commented on my initial summaries and interpretations. Some of them gave me copies of unpublished manuscripts that had additional information about the scientific communities and periods under study. This helped me to understand the social relations represented in these communities and their political significance.

Among those I interviewed, for example, were Martin Izrailevich Kabachnik (close associate of Nesmeianov at the Institute of Organic Chemistry and the Institute of Organo-element Compounds, INEOS), Nikolai Konstantinovich Kochetkov (Nesmeianov’s student and colleague at the Institute of Organic Chemistry), Zinaida Naumovna Parnes at INEOS (student of Dmitrii Nikolaevich Kursanov, who was a close friend of Nesmeianov since they were students at MGU), and Alexander Moiseevich Rubinshtein (assistant to Zelinskii at the Institute of Organic Chemistry and at Moscow State University, MGU). I also interviewed Nesmeianov’s widow, Marina Anatol’evna Nesmeianova, and Nikolai Al’fredovich Plate (son of Al’fred Feliksovich Plate, Zelinskii’s son-in-law), and others. I drafted the results of the interviews, woven together with the co-authorship and biographical data, personal memoirs, biographies, letters to me from Russian scientists, and other sources. These will be in the book I am writing on *Politics, Science, and Social Change*.

Most of the interviews focused on social relationships among people who were closely associated with Zelinskii and Nesmeianov, either directly or through other individuals in their networks. I cross-checked responses for consistency and was able to clarify the circumstances in which their relationships unfolded. As one might expect, many of the professional and friendship ties were rooted in common educational experiences at Moscow State University. Others were formed later in life through common research interests. Rather than being bounded by institutional structures, personal and social ties generally *preceded* their

work within and across research institutes. For example, scientists at the Academy's newly created Institute of Organic Chemistry in 1934 came mainly from Zelinskii's laboratory of organic synthesis at Moscow State University. When Zelinskii began working at the Institute while continuing to teach at MGU, his students (such as Boris Aleksandrovich Kazanskii and Aleksei Aleksandrovich Balandin, among others) did the same.

Formal positions in bureaucratic institutions were vehicles through which scientists could pursue their intellectual agendas and advance their careers, but mentor-student relationships continued to flourish *outside* institutional channels. In 1930, for example, Nesmeianov organized a laboratory where he could pursue his line of research on the chemistry of organo-element compounds at the Institute of Fertilizers and Insectofungicides, under the USSR Ministry of Chemical Industry. He then moved to the Academy Institute of Organic Chemistry, where he set up a laboratory for the study of organometallic compounds. This laboratory subsequently became the nucleus for the Institute of Organo-element Compounds, which he founded in 1954. In contrast to Zelinskii's science school, Nesmeianov's students did not follow him. For example, neither Nikolai Kochetkov, nor Oleg Reutov had formal positions in the Institute of Organic Chemistry when Nesmeianov was its director, even though they were linked to him through co-authorships at that time, and neither followed Nesmeianov to the Institute of Organo-element Compounds when it was first set up.

The interviews I conducted also addressed some of the issues related to the scientists' working environment. I had found, for example, that the number of Russian articles with citations of contemporary foreign publications was much less frequent in the years 1934–1936 (after the “great break”) than it was previously. It was quite common, however, for Russian publications in the mid-1930s to include references to older foreign publications (from the 1920s). At first glance, this suggested that the above chemists had access to older international journals (mostly German) and that more recent materials were not easily available. But additional investigation revealed that Soviet scientists were not completely cut off from the West in the 1930s. At least two articles by Zelinskii (1934, 1936) and two articles by Nametkin (1934, 1935) cited foreign sources published in the same or previous two years. Zelinskii's articles had the largest number of, and most current references to, foreign sources, including citations of publications from Germany, England, the United States, France, Sweden, and Japan. The fact that the most senior scientists (Zelinskii and Nametkin) were using current sources demonstrated that, indeed, they did have access to some foreign scientific journals.

Pursuing the matter further, I inquired in several of my interviews about the use of foreign scientific literature during the 1930s. In my interview with Rubinshtein in Moscow in 1991, for example, he said that Zelinskii and his colleagues continued to receive foreign publications on a regular basis, and they were available in Zelinskii's library at MGU and in other institutes. However, many of the newer chemistry students were unable to read these publications because they had advanced through the *rabfak* programs and did not have adequate foreign language skills. This made foreign manuals and periodicals less useful as teaching materials, although some scientists (especially A. P Terent'ev) gave special lectures to update students on different types of scientific information related to their work (Rubinshtein, Letter to author, January 1993). The situation was different for senior scientists in this case study, since they were capable of reading foreign language articles in their fields. If international journals were available, then why were they not being cited as frequently as before? Accustomed as we are to infer political pressures behind changes in social behavior, I asked if chemists were allowed to cite the older contributions of Western scientists, but

were discouraged from citing contemporaneous ones, because references to the latter would suggest that Russian scientists were “cringing before bourgeois science” (a slogan typically used in ideological campaigns of the Stalin period).

Rubinshtein, who was working closely with the publications of Zelinskii and other chemists at that time, provided additional insights into their informal work habits. Although there was prompt receipt of international scientific journals during the 1930s, Russian chemists were often too busy to read them. The task of abstracting recent articles was often delegated to younger scientists, and there was a relaxed attitude about specific references (even to Russian sources) unless articles were of major importance. Zelinskii, in particular, was in the habit of citing sources from memory; it was more likely that he would refer to major works in the “classical” literature. The “culture of citations” at that time was very informal. There was no specific political directive that Rubinshtein could recall regarding the citation of foreign sources in this area of chemistry prior to World War II (Rubinshtein, Letter to author, January 1993 and phone interview, March 1993).

Other interviews highlighted the importance of informal collaborations that resulted in co-authored scientific publications. It is well known, for example, that Kabachnik shared a close professional friendship with Nesmeianov, especially after the Institute of Organic Chemistry was evacuated to Kazan during World War II (Kabachnik, 1988, p. 17–19). However, it wasn’t until the mid-1950s that Nesmeianov and Kabachnik decided to collaborate on a joint paper, which they wrote together during a week at Nesmeianov’s dacha in Lutsino (*ibid.*, 28–30). The paper was on tautomeric equilibrium, a subject related to what was then the politically sensitive theory of resonance. The professional friendship that had formed earlier between Nesmeianov and Kabachnik was greatly strengthened by their experience during the resonance controversy and lasted for the rest of their lives.

When I asked Kabachnik about his co-authorships with Vasilii Vladimirovich Korshak, he said that, although they had a good relationship, they were not close friends. Scientists in their respective laboratories at INEOS worked separately, and they never conducted collaborative research. Kabachnik’s laboratory would occasionally prepare phosphorous organic monomers that Korshak used in his polymer research, and this would sometimes result in coauthored publications, but that was done very casually and outside of any formal plans for the institute (Kabachnik, Letter to author, January 1993). In fact, there was such a mixture of formal and informal research projects that it was often difficult to sort them out. Scientists I interviewed emphasized that the distinction really didn’t matter, since even research that became part of the official plan was developed at the initiative of the same scientists who were doing “unplanned” research. They only needed to formalize the projects that required additional resources.

This is consistent with what I wrote in the conclusion of my article on “The Hidden Structure of Soviet Science”:

The historical significance of social circles and informal networks is that they provided a base for spontaneous change in the social structure of science. This supports the argument that different sectors of Soviet society were developing at their own pace, with their own patterns of social behavior, despite controls set by political authorities. Therefore, whether one focuses on the boldness of scientists to use their contacts to lobby for democratization, or on the tenacity of entrenched networks to resist change, one thing remains clear: Throughout the history of the USSR Academy of Sciences, Soviet scientists demonstrated their ability to organize themselves, without waiting for directives from above. They exercised a great deal

of ingenuity and resourcefulness as they developed informal scientific communities hidden within and around the powerful centers of Soviet science (Lubrano, 1993, p. 172).

The existence of informal networks is well known through historical case studies of prominent science schools. And, the examples I give here illustrate only a partial view of the science communities around Zelinskii and Nesmeianov. Moreover, we already know from the history of science that, at any period of time, there are many scientists with different and often conflicting intellectual and political agendas. A pluralism of formal and informal networks is a normal part of social structure, not only in activities related to science, but in most other social activities as well. While such observations are commonplace today, it is important to remember the historical period when this research took place. The project was developed in the 1970s, quantitative data were processed by the 1980s, and interviews completed by the 1990s. In the 1970s, and to a lesser degree in the decade that followed, it was common to use ideologically derived categories to frame studies of Soviet society. I started from a different premise, one in which the researcher looks at the fluid processes of social interaction that occur in all countries, and this includes the social process of scientific research in Soviet Russia (see: Lubrano and Solomon, 1980).

In the late 1990s I put the project aside and worked on other things for several years, including cross-national research on the politics of medical practice, the *ethos* and *techne* of social enterprises in Europe, the development of a comparative and regional studies program at American University, and the creation of a graduate research center at the University of Trento in northern Italy. The theoretical perspectives of Michel Foucault and Nikolas Rose strongly influenced my work in the 2000s and became a central component of new courses that I developed for undergraduate and graduate students. When I returned in 2010 to the manuscript I had drafted years earlier on Zelinskii, Nesmeianov, and their colleagues, I decided to re-interpret some of the findings in light of what I learned from Foucault and Rose.

Their writings articulate the argument that ‘government’ takes place in the daily practices of social interaction. In my 2011 article on governmentality through science communities, I wrote “the knowledge of how to *govern* the conduct of science [is] itself a form of power; it [is] a way of doing things, a set of practices that [are] simultaneously personal and social, simultaneously the management of one’s own intellectual and ethical conduct and the management of relations with students and other scientists (Lubrano 2011, p. 40).” Nikolas Rose argues that government takes place mainly at the level of community. He views ‘community’ as “a moral field binding persons into durable relations. It is a space of *emotional relationships* through which *individual identities* are constructed through their bonds to *micro-cultures* of values and meanings (Rose, 1999, p. 172).” I used this approach to discuss three events in the history of chemistry in Russia: the Kasso affair of 1910–1911; the re-organization of the Chemistry Faculty at MGU in 1930–1932; and the controversy over resonance theory in 1949–1951. In each case, scientists used their moral field of personal ethics and collective allegiances to navigate elements of the social and to act in resistance to power. In other words, they were successfully exercising the art of government through community.

Foucault’s paradigm is consistent with my initial focus on politics outside the state, and it offers a useful perspective on the politics of social action. It allows us to examine the fluid processes of social change in ways that defy neat categorizations and conventional interpretations. It thereby facilitates the integration of the three subject areas discussed in this article, and it continues to play an important role in my ongoing research.

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Historiography of the Russian Diaspora in the USA

The article is devoted to the historiography of four waves of Russian emigration to the USA. It describes perception of the migration processes in Soviet Russia/USSR (V. Obolensky, L. Bagramov, E. Nitoburg), paying particular attention to the works published by the Academy of Sciences; emigrants' self-appraisal (M. Vilchur, V. Petrov, I. Okuntsov); its interpretation by Western scholars (J. Davis, G. Govorchin, M. Raeff).

Keywords: migrations, Russian Diaspora, USA, historiography, Academy of Sciences, scientific communication.

The US “Russian” Diaspora consisting of four waves emigrants' descendants is the largest in the world. Each of migration waves had different social structure, home country escaping reasons, political views, religious beliefs and its own level of adaptation to American society. At the end of the XIXth century emigration from the Russian Empire began drawing attention of statisticians (Бородин [Borodin], 1915; Воблый [Voblyuj], 1904) and diplomats (Щербатский [Ch'erbatskii], 1915; Курчевский [Kurchevsky], 1914) who put the bases for the further studies and created the migration processes evaluation methods. They tried to describe emigrants' ethno-religious and social structure and their reasons to resettle in the USA, emphasizing illegal nature of emigration. To give a brief description of the literature devoted to the topic is a complicated problem and leads to reduction of materials into three groups: 1) Perception of the migration processes in Soviet Russia/USSR (V. Obolensky, L. Bagramov, E. Nitoburg); 2) Émigré self-appraisal (M. Vilchur, V. Petrov, I. Okuntsov); 3) Its interpretation by Western scholars (J. Davis, G. Govorchin, M. Raeff).