No. 3

TRONDHEIM STUDIES
ON EAST EUROPEAN CULTURES & SOCIETIES

Mark B. Adams

NETWORKS IN ACTION: THE KHRUSHCHEV ERA, THE COLD WAR AND THE THE TRANSFORMATION OF SOVIET SCIENCE

October 2000

Mark B. Adams is Associate Professor and Graduate Chair, Department of History and Sociology of Science, University of Pennsylvania, Philadelphia, USA. He received his Ph.D. (1973) in the History of Science from Harvard University. Since 1970 he has taught at the University of Pennsylvania, where he helped to found the Department of the History and Sociology of Science. He has won the Lindback Award for Distinguished Teaching, has held the Bers Chair in Social Science, and has been a Mellon Fellow at the Aspen Institute of Humanistic Studies. Adams's teaching and research have covered the general history of Western science, the history of biology, Russian science, science and politics, and the history of science fiction. As an historian of biology, he has published widely on genetics, population genetics, evolutionary theory, morphology, Darwinism, eugenics, medical genetics, and the nature-nurture controversy. He is also a leading authority on Soviet science, has worked in the Russian archives, and actively collaborates with a number of younger Russian scholars. His publications include The Wellborn Science: Eugenics in Germany, France, Brazil, and Russia (Oxford, 1990), and The Evolution of Theodosius Dobzhansky (Princeton, 1994). Professor Adams is currently engaged on three projects: a study of Soviet science during the Khrushchev era; an analysis of the science and politics of human heredity in the USSR; and a history of 20th-century biological futurism in science and literature.

ISSN 1501-6684

Trondheim Studies on East European Cultures and Societies
Editors: Knut Andreas Grimstad, Arne Halvorsen, Håkon Leiulfsrud, György Péteri

We encourage submissions to the Trondheim Studies on East European Cultures and Societies. Inclusion in the series will be based on anonymous review. Manuscripts are expected to be in English (exception is made for Norwegian Master's and Ph.D. theses) and not to exceed 150 double spaced pages. Postal address for submissions: Editors, Trondheim Studies on East European Cultures and Societies, Department of History, NTNU, N–7491 Trondheim, Norway.

For more information regarding the Program on East European Cultures and Societies and our paper series, visit our WEB-site at:

http://www.hf.ntnu.no/hist/peecs/main.html

NETWORKS IN ACTION: THE KHRUSHCHEV ERA, THE COLD WAR, & THE TRANSFORMATION OF SOVIET SCIENCE

Mark B. Adams
Department of History & Sociology of Science
University of Pennsylvania

Abstract

Based on more than a quarter century of evolving work, this "thought piece" is a re-evaluation of Soviet science during the Khrushchev era (1954–1964). It focuses on a series of remarkable events occurring during the so-called "Thaw" (1955–1958) that give evidence of a fundamental transformation then occurring in Soviet science under the influence of the Cold War. These developments suggest the critical role played by personal networks in the evolution of Russian science. The piece concludes with some methodological implications for studying Soviet science, the Cold War, and the history of science writ large.

MARK B. ADAMS

NETWORKS IN ACTION: THE KHRUSHCHEV ERA, THE COLD WAR, & THE TRANSFORMATION OF SOVIET SCIENCE

1. INTRODUCTION

I began my study of Soviet science as a student of Everett Mendelsohn, during the Khrushchev era, even before that era had ended. At that time, in 1963, the "big news" in science was the deciphering of the genetic code: the quest for the so-called "secret of life" kept the lights in the bio lab buildings on my campus burning through the night—so of course I studied genetics. It was also the age of "Sputnik": the Russians had beat us into space, and we all knew it. For my "history and science" major, then, it was perhaps natural that my "history" would be Russian and my "science" biology. When it came time to do my undergraduate thesis, however, I learned to my astonishment that, at least officially, Lysenko and his crackpot pseudoscientific biology still reigned supreme in the land of Sputnik.

How could it be, I wondered, that a scientific powerhouse like the Soviet Union—the country that had launched the first satellite—had somehow missed out on the central scientific breakthrough of the age? Just then, the "little October revolution" of 1964 unfolded and Khrushchev was ousted from power. I followed with rapt attention as the objects of my rather arcane research became big news, Lysenko's depredations were publicly bruited, and Soviet

genetics reemerged. It was intoxicating to see my private passion make the headlines of the *New York Times*. I was hooked, and have been ever since.

More than a third of a century has passed since then, and much has changed. The history of science has emerged as a substantial international scholarly discipline, and within it, so too has the field of the history of Russian science. Molecular genetics has begun to show its remarkable potential, and, in the process, has become a big business. The Khrushchev era is no longer front page news (the only recent reminder of him was when his son became an American citizen). Indeed, even "the Soviet experiment" itself seems to have come to its uncertain end, and the Cold War that so influenced so much over the past half century is now history.

With the Cold War over, we can now look back at the "Soviet experiment" and the Khrushchev era with a new perspective. Those forty intervening years (and especially the past ten) have provided much to sharpen our vision and unclutter our view: the waning of ideological sensitivities, the new access to archival sources, intensified interactions with Russian colleagues, and the efflorescence of work in the social history of science—all have provided new ways to understand that history and its broader significance. Together at long last, Russian, European, and American scholars are beginning to unravel the story of Soviet science and the "Soviet experiment" in its full dimensions. Our divisions are no longer ideological, but rather interpretive and methodological, and we struggle to develop a common language and framework and to figure out the central storylines.

In our story of Soviet science, as it eventually unfolds, I suspect that the Khrushchev era will loom rather large, much larger than it has thus far. Lasting barely a decade (1953–1964), and perched between the eras of Stalin and Brezhnev, it is often treated as punctuation—as the

denouement of a story about Stalinism, or the preface to a story about something that was to follow (e.g. the dissident movement). Yet that decade was at the very cusp of Soviet history: 1956 was the precise midpoint of the seventy years of Soviet rule. Immediately before the Khrushchev era had been a quarter-century of Stalinism, with its turmoil, turbulence, and terror; immediately after would come twenty years of Brezhnevism, with its bland bureaucratic modernity. Just before it, Trofim Lysenko's "Michurinist biology" was enthroned; just after it, Lysenkoism was abandoned. This was the first full decade of the "atomic age" and the Cold War, of de-Stalinization and the Hungarian Revolt, of Sputnik and its aftermath, of the "U2" and the "Cuban missile crisis," when the Cold War stabilized and "normalized" into "missile gaps" and the "space race". It was, in short, a key decade of transition, when the Cold War settled in and Stalinism somehow became transformed into Brezhnevism.

In the course of my own studies of Soviet science over the past forty years, I found it illuminating to move from a comparative disciplinary perspective to the analysis of the institutional matrix of Soviet science. Then, in the late 1980s, as the Cold War was flagging and the Soviet Union was beginning to unravel, I returned to my earlier study of the Khrushchev era, and this time was able to obtain much more information about events, not only from personal archives and documents, but also from candid interviews with firsthand participants.

Just within the last few years, I have come to see those events in a somewhat new way, one which, I believe, has broad implications for the way we study science and its history. In this short essay, I want to sketch that view by discussing four matters that I now see as closely interrelated: the Khrushchev era, the Cold War, the transformation of Soviet science, and the key

role played by a somewhat neglected factor—*networks*— which, I believe, has helped to shape modern science.

2. "THE THAW"

What happened in Soviet science during the Khrushchev era, and what can it tell us about Soviet science as a whole, and about the history of science more generally? To begin exploring these questions, let us focus for a moment on a cluster of events in Soviet science. They all took place during the time of what has been aptly called the "thaw" (*ottepel'*)—a three-year period between late 1955 and late 1958, beginning just as Khrushchev was coming to power, and remembered for his "de-Stalinization" campaign, the launching of Sputnik, and a brief time of cultural and political liberalization, which largely came to an end in December 1958. It was during this time, I will argue, that things were set into motion that would effect the key *transition* from what we know as "Stalinism" into what became "Brezhnevism".

Before discussing these key transitional events, let me set the scene by briefly rehearsing the "legacy" of Stalinism—the "status quo ante". In early 1953, the Michurinist campaign, supplemented by the "results of the Pavlov session" and numerous other offshoots, was still in full swing. Lysenko's "Michurinism" ruled Soviet biology, and his followers had taken over all key administrative posts. The geneticists had been stripped of their institutions and, in most cases, their jobs. The elections in the Academy of Sciences in 1953 had flooded its Biology Division with corresponding members of Michurinist persuasion (or, at least, coloration). Lysenkoism was as thoroughly in place, administratively and ideologically, as anything could be. Then quite

suddenly, in the spring of 1953, two portentous events happened that would open an age of new possibilities: Stalin died; and Watson and Crick published their double-helix model for the structure of DNA. In the decade that followed, there were at least six political zigzags: it looked, for a brief time, that Lysenko would lose his legitimacy in the mid-1950s. When things settled out, however, he had managed to cultivate a personal relationship with Nikita Khrushchev, and, on the official level, his "Michurinist genetics" remained the politically correct, state-sanctioned variant until after Khrushchev's ouster in October 1964.

When Lysenkoism was officially debunked in 1966 and Soviet genetics reemerged as legitimate science, a curious fact became immediately apparent: although called other names, and protected in odd places, Soviet genetics was in fact reestablished not in 1966, but a decade earlier, in 1956! Now, this fact has been noted before—by me among others[1]—but I'm not sure we have fully come to terms with its implications.

What happened around 1956 that got Soviet genetics going again? The answer, in its most concrete form, is that not one, but *many* things happened, almost simultaneously. There were at least twenty "initiatives" taken in those months, and each is a story unto itself, complete with its own setting, characters, plot, drama, and narrative line. I have managed to reconstruct almost all these stories in some detail, but I am not going to tell any of them here. Instead, I want to simply tick them off, and then ask what they have in common, and what patterns they form.

---

[1] See my articles: "Biology in the Soviet Academy of Sciences, 1953–1965: A Case Study in Soviet Science Policy," in *Soviet Science and Technology: Domestic and Foreign Perspectives*, edited by J. R. Thomas and U. M. Kruse-Vaucienne (Washington: NSF/George Washington University, 1977), pp. 161–88; and "Biology After Stalin: A Case Study," *Survey: A Journal of East and West Studies*, No. 102 (winter 1977–78), pp. 53–80.

Here, then, are a few of the things that happened between roughly December 1955 and December 1958:

- Under botanist V. N. Sukachev, the Botanical Society launched a journal campaign criticizing and scientifically disproving various of Lysenko's theories.

- A "private" letter signed by 300 biologists and many physical scientists was delivered to the Central Committee, calling for an end to Lysenko's stranglehold and the reestablishment of genetics. (It became known as the "Letter of 300".)

- Lysenkoist A. I. Oparin was replaced as head of the Academy's Biology Division by biochemist Vladimir Engelhardt, who used his office to support genetics and molecular biology.

- At his Institute of Physical Problems, Petr Kapitsa sponsored a major "event" on molecular genetics, where subsequent Nobel physicist Igor Tamm and geneticist Nikolai Timoféeff-Ressovsky spoke to an overflow audience.

- Moscow mathematician A. A. Liapunov, a Soviet pioneer of "cybernetics", created a "home seminar" on genetics.

- Timoféeff-Ressovsky began holding summer workshops on experimental biology and genetics at Miasovo in the Urals.

- Under its rector, mathematician Alexander D. Aleksandrov, Leningrad University appointed Mikhail Lobashev to head its Genetics Department, and genetics began to be taught there once again.

- The Academy's Institute of Biophysics got a new director, biophysicist Gleb M. Frank, and was reorganized to include a new Laboratory of Radiation Genetics, headed by the one remaining geneticist in Academy ranks—Nikolai Dubinin.

- Nikolai Semenov (1956 Nobelist in chemistry) opened a new Laboratory of Mutagenesis at his Institute of Chemical Physics.

- The Institute of Atomic Energy, directed by the "father of the Soviet A-bomb", Igor Kurchatov, opened a Radiobiology Section which soon included some five or six separate genetics laboratories.

- Engelhardt created a new Institute of Radiation and Physicochemical Biology (later renamed the Institute of Molecular Biology).

- The Academy of Sciences launched a major discussion and conference on the philosophy of science that gave scientists primacy in interpreting science according to "dialectical materialism" and officially endorsed the role of physical scientists in biology.

- Under the leadership of mathematician Mikhail A. Lavrent'ev, the Academy of Sciences created a wholly new "Siberian" Division of the Academy and began construction of a science

city, *Akademgorodok*, which included a new Institute of Cytology and Genetics, directed by Dubinin.

Although far from exhaustive, this list gives a flavor of the variety of initiatives that took place during the months of the "thaw".

We may note three features of these initiatives at the outset: they were undertaken by a whole range of the most distinguished Soviet scientists, spanning mathematics, physics, chemistry, and biology; they were breaking out all over, within a wide range of institutions, divisions, and locations; and almost all survived the end of the "thaw" intact. When we think about it, this is rather remarkable: just a few short months after the peak of "High Stalinism," with its show trials, strident ideological campaigns, and arrests, the government's official line was being tested and challenged on a wide range of fronts. Furthermore, these initiatives, initially so fragile and vulnerable, were able to build such momentum and become so successfully consolidated and established, in such a short time, that even when the "thaw" ended in late 1958, they were able to continue and expand.

In the early 1960s, when I began to study these events, it took some digging to decipher them and to figure out how and why they belonged together. That is hardly surprising: even in the 1960s, when some of the basic facts became known, the Government and Party never did manage to admit that they had made a mistake about genetics, so, aside from details of Khrushchev's misbehavior (which conveniently helped to legitimate his ouster), official published accounts about the resurrection of genetics were generally triumphal in tone and uninformative about process. In addition, of course, the Party and Government still ruled, so, not surprisingly, contemporary scientists were anything but forthcoming about how they were able to

manipulate the system to achieve their own objectives, especially since these objectives were, at least officially, at variance with state policy at the time. And, of course, tactics that worked wonderfully to hide, protect, or legitimate genetics in Khrushchev's Russia also served to mask the trail from innocent historians: one had to know, for example, that "genetics" sometimes meant "genetics," and sometimes did not; that "biochemistry" or "biophysics" *could* mean genetics; and that "radiobiology" and "physicochemical biology" often *did*—as did (to cite a particularly ornate example) "the biochemistry of physiologically active high-molecular-weight polymer compounds"!

In recent years our knowledge of many of these cases has become much more detailed. In the journal *Priroda* especially, documentation concerning one or another of these events has tended to appear every few issues, often accompanied by published excerpts from the archives and substantial biographical and historical reminiscences.[2] These are marvelously rich and informative. In historical terms, however, they have had the effect of turning one triumphal drama—the struggle for the rebirth of Soviet genetics—into fifteen or twenty distinct tales, each with its own chronology and plot, its moments of humor and drama, and its turning points, and each built around its own hero.

That is, no doubt, exactly the way these events look when viewed closely. But what do we see when we look at all of them, together, and from a distance?

---

[2] See especially the monthly issues of *Priroda* [Nature], 1989–1998, which include heroic biographical treatments of more than half of the scientists in the listing above, each detailing, among their other accomplishments, their struggles to protect and cultivate genetics.

3. THE PRIMACY OF NETWORKS

Suppose we eschew the exotic, "exceptionalist" emphasis that has so often colored Western discussions of Soviet science, and try to look at it more or less the way we might look at any set of events in the history of science in any country.

Viewed from the perspective of the modern social history of science, the individual events or initiatives I have enumerated take on a somewhat different character, one that is rather more familiar. What was involved in the struggle over genetics was a *disciplinary* crisis involving competing groups and the theories, practices, traditions, and paradigms they represented. In 1948, one group was given complete control of the discipline by the central authority and patron of science. Part of the resultant crisis—and one of the reasons why Lysenko's new hegemony was so successful—was Lysenko's mandated takeover of all the *institutions* where the discipline was practiced. Under the new conditions of the mid-1950s, with Khrushchev's government and Party willing to endorse Michurinism, it became difficult for Soviet geneticists to retake the institutions they had lost. So, most of the initiatives during the "thaw" understandably involved creating new institutional niches, and in some cases, whole new institutes—away from, outside of, or beyond their rivals' reach.

Thus far, our story has been about *disciplines* and *institutions*. This is natural enough: both are central to the social structure of science, and both leave public traces. Disciplines have their associated societies, journals, texts, and international reference groups and congresses. Institutions have their budgets, buildings, organizational charts, publications, and patrons. Each

has, by its very nature, essentially public dimensions, each creates its own public record and leaves its paper trail. And the interrelation between the two is not simple: a single discipline can be an aggregation formed around an object of study, theory, and/or method, and it encompasses members based in many different institutions of different sorts; likewise, a single institution can encompass members of many different disciplines. Each social form has its own special characteristics, politics, trajectory, and dynamics.

But this social picture, structured around institutions and disciplines, is clearly incomplete. Consider: on their own, the geneticists did not have the clout to retake their discipline *or* its institutions, nor to create new institutional niches for themselves. Where do disciplines and institutions come from, after all, and what makes them change and evolve? In particular, why were so many seemingly separate and apparently unrelated initiatives, by so many, directed at the same end, happening at the same time? Put somewhat differently, we may ask: with the relevant *disciplines* and *institutions* seized, what other factor could come into play to reverse this apparent stranglehold?

The answer, I think involves a third dimension—one which has been, until quite recently, the most difficult to document: *networks*. Here, I am not referring to anything arcane or technical—not to the "networks" of the sociologist, dynamist, or social studies theoretican, much less the computer specialist[3]—but to the looser, more evocative meaning the word has come to

[3] For a fine survey of actor-network theory, and criticisms of it, see Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science* (Cambridge: Cambridge University Press, 1999), pp. 37–43. Those familiar with the theory will be aware that the epistemic concerns of these approaches distinguish them from my own use of network, which refers rather to an informal, voluntaristic, private, and fluid set of interlinking personal relations

have in everyday language, one familiar to every kind of historian: personal networks. A personal network is a much looser, less coherent "structure" than either an institution or a discipline. Developed out of extended family, old school ties, mutual experience, hobbies, private passions, and shared interests, such networks involve ramifying contacts that are multiple and complex—as are all the free associations that underlie civic society. They can also form "nodes" or "ganglia" where various networks interface and new connections are made—sometimes in the form of informal circles, private societies, clubs, salons, soirées, and the like, sometimes in more organized forms, ranging from things we might call "movements" to interest groups, political organizations, and even "mafias."[4] And some of these, in turn, might eventually gain further structure as would-be disciplines or proto-institutions.

---

and associations based on ties of trust, family, friendship, "old school ties," shared concerns, common fascinations, and so forth. Here, I am consciously using "networks" and "networking" in the evocative way the word has come to be used in everyday speech, which I find simpler, looser, richer, and more metaphorical, useful, and suggestive than the technical ways various theoretical specialties—including our own—sometimes use the term. The same, of course, is also true of my use of the terms "discipline" and "institution," which have also been given technical meanings by sociologists and others. (Just to be clear, my use of "institution" here does not include either "marriage" or "the family," but refers to organizations that have names, administrators, and various ranks or positions, are usually housed in buildings, employ people, organize work or practice of some kind, and spend money that has to be gotten from somewhere.) If we are to rescue our own field from the fragmentation of subspecialization, and keep our own "theory and practice" together, there is much to be gained by using terms in ways that all historians can understand and relate to.

[4] See, for example, the memoirs of Richard Goldschmidt, *Portraits from Memory: Recollections of a Zoologist* (Seattle: University of Washington Press, 1956) for his vivid descriptions of the ways long-lasting associations were formed among students in Europe at the turn of the century, ranging from student drinking and dueling groups, to the "groupies" collecting around certain prominent professors, to the bondings that occurred at various marine zoological stations, such as those at Naples and Villefranche.

For all their looseness—indeed, perhaps because of it—such networks can be remarkably influential, resilient, and enduring. Note, however, that, as a form of social organization, networks are *by their very nature* more private than public. They are informal, voluntaristic, "personal" associations whose nature and ramifications can shift and change quickly over time. They leave historical traces, to be sure—but such traces are likely to be scattered in personal photographs, reminiscences, diaries, and private letters. Further complicating the matter is the fact that the *legitimacy* of disciplinary and institutional actions often may require *hiding* the networking behind them. (In justifying a new appointment to his government patron, for example, an institute director is more likely to stress needs and qualifications than to admit, "He's the son-in-law of a colleague who gave my daughter-in-law a job.") So powerful are networks, indeed, that both disciplines and institutions have often formally implemented procedures to minimize or regulate their influence—for example, blind peer review, or anti-nepotism rules. Nor should we imagine that such networks are simply "scientific" or "professional" or "disciplinary"—they can take many different forms, as can all voluntary personal associations.

Networks cut across disciplines and institutions with promiscuous impunity—and a shifting, evolving, interconnected web of such networks is almost impossible for any agency to control. In Russia, in particular, one suspects that the political authorities may have tried. From their own "underground" years, the Bolsheviks were no doubt well aware of the importance of "private" networks; by the late 1920s, their new state had effectively seized control of all disciplines and institutions, both by government seizure of all funding and appointments, but also by seeding all organizations with Party members who reported directly up its separate chain of command. When even these measures did not produce the desired results, the authorities may

have begun to suspect that something like interest groups and networks were subverting their well-laid plans.[5] Indeed, judging from what we know about the kinds of questions asked by the interrogators of the victims of the various Stalinist purges (who are you related to, who do you know, when did you see them, what did you talk about, who do *they* know, and so forth) one might suspect that it is precisely these kinds of personal networks that the secret police agents were trying to uncover.[6] In a system where official sources are not trusted, and public speech has to be politically correct, we might well expect that networks would become all the more important.

We have long known that networks have been remarkably consequential in Russian history, although we have not put it in exactly those terms. In political history, we know of "the Decembrists," which seems, in retrospect, to have been much closer to a socially active network of likeminded acquaintances than a Party or movement. Much has been spoken and written about the "Russkii kruzhok" or "Russian circle," which appears so often in Russian cultural history. In Russian musical history, for example, there is the famous Balakirev Circle—Balakirev, Borodin, Kui, Mussorgskii, and Rimsky-Korsakov—also referred to as the *Mogúchaia kúchka* (the "Mighty Little Group"). In Russian science, such informal groupings have sometimes been

---

[5] In a pathbreaking article ("The Politics of Technology: Stalin and Technocratic Thinking among Soviet Engineers," *American Historical Review* 79, 1974, pp. 445–469), for example, Kendall Bailes has demonstrated that the Shakhty Trial and the so-called "Industrial Party Affair" (which marked the "Great Break" and the advent of Stalinism) were not simply arbitrary in their victimization; however unfair the charges of "sabotage," the trials targeted a network of professionals who had been active in Russia's technocracy movement.

[6] This notion, proposed to me by Nikolai Krementsov, is quite suggestive and well worth exploring through further research.

included under the loose, catch-all term of "school," even when there were no formal teaching relations involved.[7] Other groupings we are learning about brought people of more diverse interests together.[8] Soviet conditions soon discouraged private organizations as being potentially subversive; one of the reasons for the arrest of Sergei Chetverikov in 1929 and the dispersal his group in population genetics, we may recall, was the so-called "Drosophila Screeching Society" he led—a discussion club that met regularly in private homes and apartments, with a closed membership and new nominees subject to blackballing.[9] If Stalinism discouraged these kinds of gatherings, however, it soon animated and reinforced other forms of networking, including the famous "rumor networks" and "grapevines" that operated throughout the Soviet period; it was

---

[7] For example, see S. R. Mikulinskii et al., ed. *Shkoly v nauke* (Moscow: Nauka, 1977); and N. P. Bekhtereva, ed., *Fiziologicheskie nauchnye shkoly v SSSR* (Leningrad: Nauka, 1988). The use of the term "school" in the Soviet context has sometimes been complicated by professional agendas: under a system in which various scientific "stars" were accorded almost saintly status, much reflected glory and unimpeachable legitimacy could come to those who could be seen as members of such a "school." This explains, in part, both the debates about which "pretender" was a great man's true heir (such as those which followed the deaths of Pavlov, Michurin, and Severtsov in the mid-1930s), and also the vague catch-all quality of "school" as employed in Soviet (and subsequent Russian) lexicon.

[8] For example, Daniel Alexandrov has been studying a number of intriguing circles that formed in Russia, including the so-called "Biological Circle" in pre-revolutionary St. Petersburg, which included not only biologists, but also humanists, including the butterfly taxonomist (and novelist!) Vladimir Nabokov.

[9] "*DrozSoor,*" short for "Sovmestnoe oranie drozofilshchikov"; many years ago, Theodosoius Dobzhansky and I. M. Lerner first translated it into English for me, using the phrase I have given. For details of the "society" and the arrest, see Mark B. Adams, "Sergei Chetverikov," *Dictionary of Scientific Biography*, Vol. 17, Supplement II (New York: Charles Scribner's Sons, 1990), pp. 155–165.

these networks that managed to inform Western sources (not always accurately) about arrests or executions during the darkest days.[10]

During my year-long research trip to Moscow in 1976–1977, in the midst of the Brezhnev era, I witnessed many examples of networks in action. One of the notable events of my stay was the fire in the Hotel "Rossiya." At a secret meeting in the Kremlin, I learned, the hotel director had been rebuked for sloppiness and arrested. The news was not public: I heard it from a friend during a specially arranged walk in Gorky Park on a cold winter's day. As it happened, for a brief time in the mid 1930s, her father had been an up-and-coming Party figure, so she had been able to attend a special Party primary school. There she had become good friends with a classmate, and had subsequently kept up with her chum over the years. During the succeeding four decades, as it happened, he had managed to survive and rise through the Party ranks, until he had found himself at that "secret" Kremlin meeting that decided the hotel director's fate. The news of what happened there must have rippled through Moscow networks remarkably fast: as best I can tell, despite the complicated arrangements, I learned what had happened only four hours after the secret meeting had ended!

---

[10] For example, (probably) spurious information about Nikolai Vavilov's "arrest" in 1936 was actually published in the *New York Times,* to be hotly contested in a subsequent interview given by Vavilov himself. One can also see from the obituaries of his former colleagues written by Theodosius Dobzhansky for various Western scientific journals in the 1940s and early 1950s that he was receiving information from a variety of networks; during this period, correspondence with émigrés and "unpersons" (as Dobzhansky was regarded at that time) was strictly forbidden. Given the isolation of Russia, what is remarkable is not how much of the information was faulty, but rather how much turned out to be true.

A second example involves an impeccable scholar and scientist of my acquaintance, a man in his sixties, whom I saw on an almost daily basis. My evening visits to his Moscow flat were usually interrupted by frequent phone calls from his sundry acquaintances, old friends, and contemporaries, usually with the purpose of exchanging the latest news. There was almost no one I had heard of or wanted to talk to whose name and home phone number could not be found in his thick, dog-eared directory. One evening, out of the blue, he asked me: "Do you believe in flying saucers?" It seems he had heard from many of his sources that, the previous Tuesday, flying saucers had landed in one of Moscow's major squares, stopped all clocks and machines, and made it so that none of the people there could talk about it afterwards. "Of course, I didn't believe the rumors," he said, "until today's paper"—at which point he showed me a sober announcement of one sentence on the front page of a leading Moscow daily. It read: "Rumors that flying saucers recently landed in Moscow are completely unfounded." "Do you see?" he pressed. "If this weren't true, why would they take the trouble to deny it?"

Of course, this episode illustrates the profound and longstanding Russian distrust of political authority, but, on reflection, it tells us rather more. Consider: a "hard-headed" scientist had come to credit not merely the possibility of flying saucers in the abstract, but to take seriously the possibility *that they had actually landed in Moscow*, because of information he had heard through his network. Surely, all of Moscow must have been alive with these rumors for the government to feel obliged to deny them in print so promptly…which, in turn, had the result of lending them credence. This example reminds us that, far from undermining personal networks, totalitarian systems can actually strengthen and reinforce them as the only reliable loci of

trustworthy information. Indeed, one suspects that no matter what the government had said or done in this particular case, the network account would have gained legitimacy.

I do not believe that my experiences with Russian networks are all that unusual: everyone who has spent serious time in Russia has almost certainly experienced something similar. Indeed, much of what Western scholars know firsthand about Russia has derived from their good luck in tapping into or being "adopted" by personal networks, by being introduced and passed along to "friends of friends". Knowing, as we all do, how powerful, wide ranging, and vigorous personal networks in Russia can be, it is curious we have not paid more attention to their influence on maintaining and shaping Soviet science.

In many of my earlier studies, I now realize, there were strong traces of networks in action. For example, my studies of the founding of the Kol'tsov institute revealed the rich Moscow network of scientists, educators, philanthropists, civic activists, and entrepreneurs to whom Kol'tsov repeatedly turned for funds, staffing, support, and protection.[11] Other, comparable networks in St. Petersburg, many involving the Bekhterev Institute, would make possible Iurii Filipchenko's institutional success in creating genetics and eugenics in that city.[12] In my work on the history of Russian eugenics, I have been able to show that the same networks which created

---

[11] For example, see my articles: "Science, Ideology, and Structure: The Kol'tsov Institute 1900–1970," in *The Social Context of Soviet Science*, ed. Linda Lubrano and Susan Gross Solomon (Boulder: Westview Press, 1980), pp. 173–204; and "Sergei Chetverikov, the Kol'tsov Institute, and the Evolutionary Synthesis," in *The Evolutionary Synthesis: Perspectives on the Unification of Biology*, ed. Ernst Mayr and William Provine (Cambridge: Harvard University Press, 1980), pp. 242–78.

[12] See "Iurii Aleksandrovich Filipchenko," *Dictionary of Scientific Biography*, Vol. 17, Supplement II (New York: Charles Scribner's Sons, 1990), pp. 297–303.

Russian eugenics in the 1920s managed to survive its official "demise" in 1930 intact, and went on to create "medical genetics" four years later as a politically acceptable substitute.[13] In a study of the Soviet nature-nurture debate, I was able to trace a continuous tradition of interconnected networks that created and sustained Russian genetics, originating around 1900 and still active in the 1980s and 1990s.[14]

I had long suspected that networks played a key part in the rebirth of genetics during the Khrushchev years, and had spotted telling patterns.[15] Only in the last few years, however, has it been possible to chronicle and detail some of the roles networks played in the events in the 1950s. To cite but one example—the "Liapunov home seminar"—one can trace the interlocking family ties of the Liapunov family, show how it became involved with the genetics matter, how and when it interconnected with other networks, and even track the young students who visited Liapunov's home in the early 1950s as, some years later, they moved into jobs in virtually every newly created center of genetics in Moscow.[16] A similar account can be given about each of the

---

[13] See my articles: "Eugenics as Social Medicine: Prophets, Patrons, and the Dialectics of Discipline-Building in Revolutionary Russia," in *Health and Society in Revolutionary Russia*, ed. Susan Gross Solomon and John E. Hutchinson (Bloomington: Indiana University Press, 1990); "Eugenics in Russia, 1900–1940," in *The Wellborn Science*, ed. Mark B. Adams (Oxford University Press, 1990), pp. 153–216; and "The Politics of Human Heredity in the USSR, 1920–1940," *Genome*, Vol. 31, no. 2 (1989), pp. 879–884.

[14] Mark B. Adams, "The Soviet Nature–Nurture Debate," in *Science and the Soviet Social Order*, ed. Loren Graham (Cambridge, Mass.: Harvard University Press, 1990), pp. 94–138.

[15] See my Ph.D. dissertation, Mark B. Adams, *Genetics and the Soviet Scientific Community, 1948–1965* (Ann Arbor: University Microfilms, 1972).

[16] My detailed knowledge of this I owe to Nikolai Vorontsov, his wife Elena Liapunova, and her sister, Natalya, all of whom attended the seminar. (As a student in Leningrad in 1956,

events I cited above. Suffice it to say that our disciplinary and institutional chronicle is now greatly enhanced by what is becoming clear about networks.

4. TRANSFORMING SOVIET SCIENCE

By focusing on the role of networks, a picture emerges of what was happening in Russia in the 1950s that informs our view of its role at the cusp of the Soviet experiment.

Before I sketch that picture, however, let us recall, for purposes of contrast, how the story of Soviet science is so often told. The traditional story has been one of repression, of brilliant scientists fighting for the truth who fell victim to the system—Vavilov, Vernadsky, Sakharov, and the many others who are treated variously as heroes, saints, or martyrs. According to this traditional view, their academy was taken over and Bolshevized, forced to a new ideological line, purged and harassed. That new "USSR" Academy of Sciences—a vast, centralized, bureaucratized, Bolshevized, politicized, hierarchical structure which Vucinich has justly termed an "empire of knowledge"[17]—has been seen as the instrument devised by the Party-State

---

Vorontsov was the person who brought the "Letter of 300," then making the rounds there, to the attention of his father-in-law and his Moscow colleagues.) Needless-to-say, I am immensely grateful not only for their friendship and help, but also for their willingness to "certify" my legitimacy to sometimes doubting members of their extensive, multiple networks. An overview of the circle and its impact has just been published by Natalya Liapunova and Nikolai Vorontsov: "Delo sester Liapunovykh," *Znanie—Sila*, 1998, pp. 34–47.

[17] For a masterful survey, see Alexander Vucinich, *Empire of Knowledge: The Academy of Sciences of the USSR (1917–1970)* (Berkeley: University of California Press, 1984).

apparatus in order to control science, to create a truly *Soviet* science subservient to its will. Hence the 1948 Lysenko meetings and the extraordinary Michurinist campaign that followed.

The new picture is in sharp contrast. The first thing we may note is that for all its sweeping, drum-beating character, the Michurinist campaign seems to have left the Soviet scientific community in general, and its geneticists in particular, both thoroughly unconverted and remarkably uncowed. Not only did some find haven in "secret" research, or elsewhere in the Academy's vast system: At the earliest possible opportunity, even while Stalin lived and was preoccupied with his "Doctors' Plot,"[18] they were maneuvering to retake their discipline and oust Lysenko. One reason, I think, is because the networks that had been there before—some of them dating from pre-revolutionary days—remained as strong and vital as ever. By government edict, the Lysenkoists could seize the discipline and the institutions where it was practiced, but their reach did not extend to the networks that had built them in the first place. By tracing how the "letter of 300" made the rounds among a whole series of networks (at least six), couriered by their shared members, we can better understand how three hundred biologists and many physical scientists could manage to sign what amounted to a petition—which, not so long before, might well have bespoken "conspiracy"—and to do so within two years of Stalin's death.

In all of the other "initiatives" during the thaw, there were also networks in action. Particularly consequential were networks that linked geneticists with physicists, chemists, and

---

[18] In the final months of his life, Stalin launched a campaign against a group of elite physicians (almost all of them Jewish) who, in their capacity as Kremlin doctors, were accused of being part of a conspiracy to murder the aging Soviet leadership. See Yakov Rapoport, *The Doctors' Plot of 1953: A Survivor's Memoir of Stalin's Last Act of Terror, Against Jews and Science* (Cambridge, Mass.: Harvard University Press, 1991).

mathematicians. There were, of course, good intellectual reasons why such scientists should suddenly take an active interest in genetics, given the flurry of activity centering on DNA: a molecule, base pairs, bonding, a helix, a code, information—now, this was their meat. It is no wonder, for example, information theorists (such as S. L. Sobolev or A. A. Liapunov) or synthetic organic chemists (such as I. Knuniants, the developer of "plastics" in Russia, or Academy President Alexander Nesmeianov) would be interested.[19]

But reinforcing and informing this natural scientific curiosity were a series of personal networks of various kinds.

Complex family relations formed some of these links. Of course, we all know of the case of the Vavilov brothers: one, the leading Soviet geneticist, who died in prison in the early 1940s, while the other—a physicist—became President of the Academy of Sciences a short time later.[20] This is only the tip of the iceberg. To cite just a few examples: The founder of *Akademgorodok*, Lavrent'ev, was related by marriage to a someone who had worked in T. H. Morgan's lab. Liapunov's wife was the daughter of Academician Nametkin, a chemist, and the family used his dacha (located not far down the path from Kapitsa's). The newly appointed director of the Institute of Biophysics I mentioned above, Gleb M. Frank, had worked along side Semenov and others in the Ioffe Institute in the 1920s; his brother was the physicist (and future Nobel Prize

---

[19] A synthetic organic chemist by profession, Academy President Alexander Nesmeianov seems to have been especially won over by the implications for new kinds of organic syntheses that DNA work would make possible; he later became famous in the Soviet Union for creating a commercial version of synthetic caviar from petroleum by-products.

[20] See my article, "Nikolai Ivanovich Vavilov," *Dictionary of Scientific Biography*, XV (supplement I) (New York: Charles Scribner's Sons, 1978), pp. 505–513.

winner) Ilya M. Frank. Given the inbreeding of the Russian intelligentsia, we should not be surprised to learn that family ties often played an important role.

This networking also involved old school ties. World-renowned mathematician A. Kolmogorov and geneticist Nikolai Dubinin had been classmates. Many of Russia's physicists had known their contemporary, Georgii Antonovich Gamov, before he had emigrated and become the renowned American astrophysicist George Gamow; in the early 1950s, he was publishing important articles on the DNA coding problem.[21] A particularly important role was played by physicist Igor Tamm (another subsequent Nobelist), who headed up the "theory" section of the so-called "Installation" [*ob"ekt*], the top-secret monastery where the Soviet bomb was being designed; when he became aware of the work of Western physicists on DNA and genetics (apparently through a chance encounter with field biologists on a mountain-climbing holiday), he naturally consulted an old buddy from his school days, biologist B. M. Zavadovsky, to fill him in.[22] Tamm soon became an outspoken campaigner for genetics, spreading the word among his physics colleagues at the Installation and elsewhere; his efforts would lead to the "Kapitsa-fest" mentioned above (where Tamm and Timofeef-Ressovsky spoke to an overflow audience) and,

---

[21] See George Gamow, *My World Line: An Informal Autobiography* (New York: Viking, 1970).

[22] B. M. Zavadovsky had been an active participant in the genetics debates since the 1930s; he was the brother of the renowned experimental embryologist M. M. Zavadovsky, who was fired from his Moscow position in 1948 as a result of Lysenko's takeover. For much detailed information about Tamm and his role, as recalled by many friends and colleagues in his network (not all of them physicists!), see *Reminiscences About I. E. Tamm*, ed. E. L. Feinberg (Moscow: Nauka, 1987).

ultimately, to the creation of those five laboratories of genetics at the Installation's base in Moscow, Kurchatov's "sacrosanct" Institute of Atomic Energy.

As the above examples suggest, these networks were not only interdisciplinary, but also international. Timofeeff-Ressovsky's work in Germany on radiation genetics during the 1930s had been known to Soviet physicists well before he had been spirited home from Berlin in 1945 by the KGB: while he was still imprisoned, his work had been prominently mentioned by the great physicist Erwin Schrödinger in his postwar book, *What is Life?*, which was quickly translated into Russian on the urging of Soviet physicists. It was through their influence that Timofeeff was rescued from the Gulag (where he had almost gone blind from malnutrition) and eventually relocated into radiation research at a small station at Miasovo. (While on a rock collecting expedition in the Urals, Liapunov stumbled upon Timofeeff, whom he thought had died; when it became generally known that he was still alive, young students and old friends flocked to his new base—hence the Miasovo summer seminars.) Indeed, leading Soviet physicists became so involved with genetics that, in the mid-1950s, one of them—L. A. Artsimovich—smuggled drosophila cultures back to the Soviet Union from an international astrophysics congress.[23] Meanwhile, in the West, such prominent Russian émigrés as Theodosius Dobzhansky, I. M. Lerner, George Kistiakowki, and George Gamow were making contacts

[23] In the institutional edicts following the Lysenko meeting in 1948, that mainstay of genetics research—drosophila flies—had to be set free or, in the case of KGB and military research institutions, drowned in boiling oil. The physicist L. A. Artsimovich was bringing special strains of the flies back for Dubinin's new laboratory in the Institute of Biophysics.

through their own extensive networks in American and world science with their former Russian friends and colleagues.

Like the geneticists, then, many Soviet physical scientists were part of networks that were both interdisciplinary and international, and they had their professional interests peaked by the DNA news that was just then transfixing so many of their Western colleagues. But, *unlike* the geneticists, they were in a position to do something about it.

The final years of "high Stalinism" witnessed the rise of a new and powerful group of physical scientists who were associated with "the bomb."[24] Science had become vital to Soviet power, and those who could provide the strategic products vital to the KGB, military, and Politburo—the A-bomb, the H-bomb, the rockets—became increasingly vital components of the Soviet Union's political-military-industrial complex. As they succeeded with their magic, they gained prestige, power, a measure of ideological immunity, and privileged access to the highest Party-state leaders. We now know that in 1950, physics was scheduled to undergo its equivalent of a Lysenko meeting—complete with scripted oratory and apologetic recantations. At the very last moment, the meeting was canceled, by order of Beria: the bomb-makers were apparently too important to the State to be trifled with.[25]

Rewarded with quick and disproportionate entry into Academy membership in the elections of 1953, and with real clout in higher government, military, and security echelons, these

---

[24] For the most authoritative treatment in English, see David Holloway, *Stalin and the Bomb: The Soviet Union and Atomic Energy 1939–1956* (New Haven: Yale University Press, 1994).

[25] See A. S. Sonin, *"Fizicheskii Idealizm": istoriia odnoi ideologicheskoi kampanii* (Moscow: "Fiziko-Matematicheskaia Literatura", 1994), pp. 114–160.

men had largely taken over the Presidium of the Academy of Sciences by 1956. From there they began to do for genetics what geneticists could not do for themselves. On some issues, the group was divided—the question of pure versus applied research, for example—but on the question of genetics, DNA, and Lysenko, they acted almost as one. We should not imagine these men as oppressed victims or closet dissidents. As a rule, they were Soviet patriots, and constituted a very privileged elite indeed, very near the pinnacle of Soviet power. They knew (or had learned) how to deal with the government and Party—indeed, some of them had joined during the war—and sincerely sought to provide it with all the weapons and such that it wished, and more.[26]

It was in the Khrushchev era, then, that a new dialectic of power, always implicit, came to the fore. If the Party-state had created the Big Academy as the instrument to control science, it had also thereby managed to create an instrument that had become the monopolistic supplier of what was now a top-priority product. And that "instrument" was now headed by people with very definite ideas about what they wanted for Soviet science: they wanted respect, they wanted ideologists and bureaucrats off their backs, and they wanted intellectual control over their own enterprise. If testimonials about the superiority of Marxist or dialectical materialist approaches to science were called for, fine—they would be happy to produce what was required themselves, thank you very much. They were willing to stay out of politics, and they wanted politicians to

[26] Indeed, by contrast to what we hear about the comparable communities in the United States, Britain, or Germany, Russian accounts and reminiscences seem remarkably untroubled by the moral or political implications of their work. This is even glaringly apparent in Andrei Sakharov's remarkable *Memoirs* (New York: Knopf, 1990).

stay out of science—so long, of course, as they continued to fund it lavishly. And of course, they wanted Lysenko and his Michurinist ideologues neutralized or out, and genetics in.

And so the game unfolded. Lysenko had Khrushchev's ear on agriculture and biology, true; but Kurchatov, Lavrent'ev, Kapitsa, and others had his ear on other things, such as rockets, sputniks, and bombs. Their strategies: Open labs in areas under their own control. Maneuver to isolate Lysenko in the biology division, where they would, with Engelhardt's help, chip away at his stronghold. Open a new Siberian division, part of which would involve a genetics institute of their own design, and create an administrative structure for it that closed Lysenkoists out. Orchestrate a reform in dialectical materialist philosophy that made the scientists themselves its chief spokesmen on matters of science (a goal that they accomplished in 1958, thereby legitimating their meddling in biology). In carrying out this policy, some of the group (for instance, Semenov) "adopted" particular geneticists as their tutors, advisors, or assistants.

Of course, Lysenko and his allies knew perfectly well what was happening (after all, they had their own networks!), and used every lever they had with Khrushchev to unravel it. With that help, they were able to keep their strongholds, and to mobilize Khrushchev to oust Dubinin from directorship of the Siberian institute, and even attack him publicly and at some length at a December 1958 Central Committee meeting. Remarkably, however, Khrushchev's tirade at that meeting proved inconsequential: Dubinin stayed on for a time; the Siberian institute's genetics did not change, only the public descriptions of it; and Dubinin continued heading his Moscow laboratory as before. This was a harbinger. Despite the end of the "thaw" and Khrushchev's increasing alliance with Lysenko, every single center of reborn genetics that was created during the mid-1950s continued to grow and flourish in the years that followed—irrespective of the

Lysenkoists' persistent attempts to dislodge them. We may wonder: why were Lysenko's own "Michurinist" networks, so extensively involving agriculture, the farming bureaucracy, and Party ideologists, unable to overcome these initiatives to oust him? In retrospect, it seems clear that one of the main factors was the Cold War: for the Politburo and the military and KGB leadership, under such conditions, agriculture and ideological "purity" were lower priorities than bombs, satellites, and missiles—and it was these scientists, not Lysenko and his people, who were providing them.

Learning from their experience, key members of the Soviet scientific leadership continued to press their campaign ever more strongly. Piggy-backing on Khrushchev's fondness for Akademgorodok, they tried to win his support for the construction of another science city closer by, at Pushchino—this one to be devoted entirely to "physicochemical biology"—but Khrushchev demurred, saying it was just too expensive. (They would eventually get it—a few months after Khrushchev's ouster.) Having learned from their 1957 Siberian success, in 1963 they engineered a major reorganization of the Academy, transforming its 8 divisions into 15, grouped into 3 larger "sectors," each run by a vice-president. Its chief architect was Nikolai Semenov, who, as a result of the reorganization, now became head of the sector that administered all of chemistry *and* biology. Lysenko ended up even further isolated, in a much reduced division of "general biology". Along side it was now a new, powerful, hybrid division with the awkward name "Biophysics, Biochemistry, and the Chemistry of Physiologically Active Compounds"—for which read, "molecular biology."

Meanwhile, with Nirenberg's announcement in 1961 that the genetic code had been broken—an announcement at the International Congress of Biochemistry which, not

coincidentally, was being held in Moscow (again, international networks at work!)—the campaign against Lysenko gained momentum. Now new rhetoric, articulated by leading Soviet physicists, came to the fore: that the imminent "control of life" would be even more important than the atomic bomb! As a counterstroke, Lysenko—now isolated in a smaller division where he was even more firmly in control—sought to reinforce it, and gained Khrushchev's leave for four new Academy slots in his division for his own handpicked nominees. In 1964, when the Academy's General Assembly refused to elect two of them—a move unprecedented, to the best of my knowledge, since the advent of Stalinism—Khrushchev was heard to be considering the abolition of the Academy and its replacement by a state committee.[27] That was just one of the indictments against him during his ouster a few months later, in October 1964.

Thanks to their steadfast efforts in the Khrushchev era, by the mid-1960s the Soviet Academy's new leaders (and the networks they represented) had achieved their basic social agenda for Soviet science. So long as they stayed clear of politics, science was theirs: they could manage their ever-growing scientific empire, exercise freedom of scientific thought, increasingly travel abroad, and generally enjoy the secure perks of a highly privileged Soviet caste (complete

---

[27] Rumors rippled through the networks of Khrushchev's threat, and they were reported, for instance, by both Zhores Medvedev (*The Rise and Fall of T. D. Lysenko*, New York: Columbia University Press, 1969) and David Joravsky (*The Lysenko Affair*, Cambridge, Mass.: Harvard University Press, 1970). Judging by the recent publication of a previously unavailable transcript of Khrushchev's actual remarks (V. Iu. Afiani and S. S. Ilizarov, "...My razgonim k chertovoi materi akademiiu nauk," *Voprosy Istorii Estestvoznaniia i Tekhniki*, 1999, No. 1, pp. 167–173) the network information was generally accurate, but not entirely so—assuming, of course, that one chooses to credit the accuracy of the version that was transcribed, edited, and just recently published over the contemporary oral accounts provided by the networks. In any case, one suspects that it was the rumors of what he said, rather than the actual text, that contributed to his undoing.

with dachas, special stores and clubs, chauffeured limousines, luxury apartments, and salaries higher than most members of the Central Committee enjoyed). In an era of Big Science, theirs was the biggest, and the continuing competition with the West in military technology and the "space race" kept their prestige and priority high, and their coffers full.

Throughout the Brezhnev era, under the conditions of the waxing and waning Cold War, Soviet science continued to grow, cultivating itself as an increasingly chubby symbiont on the body-politic. The major upsets in that symbiosis occurred when scientists, more often than politicians, breeched the unwritten contract—as with the occasional forays to "liberate" the social sciences, or when certain scientists used their freedom and privileged status to contest state policy, and eventually the legitimacy of the entire system itself. We should not judge the scientific leadership too harshly for being unsympathetic to these efforts. After all, having spent their careers shutting the door between science and politics, it is hardly surprising that such men as Engelhardt and Semenov did not welcome the efforts of the Sakharovs to open that door all over again.

In short, the Khrushchev era did indeed see the remaking of Stalinist science into its Brezhnev variant. This happened because of a new generational scientific elite, representing the views and interests of a diverse set of highly resilient and well developed interdisciplinary and international networks. Empowered by the importance of science for the Cold War, these leaders became proactive and strategic in renegotiating the symbiotic relationship between science and the State. What they gained was substantial control over the conduct of science. Thanks to their efforts during the Khrushchev years, Soviet science under Brezhnev was bigger, fatter, freer, less ideologically vexed, and much more secure than it had been under Stalin. (That does not mean it

was *better*: Soviet physicists won a hefty slew of Nobel prizes under Brezhnev, to be sure—but, almost without exception, the prizes were for work they had done under Stalin.)

Dare we admit what, in retrospect, has become all too obvious?—In the Soviet Union as in the West, the Cold War was probably the best thing that ever happened to science and technology. Without it, the Soviet scientific elite could hardly have carried such weight with the political leadership, nor negotiated for themselves so much autonomy. For a half-century thereafter, each community, the American and the Soviet, could use the threat of the other to keep the immense and growing resources of their own countries flowing to science and technology. With so many worthy and unworthy claimants to scarce resources in both countries, there is no chance that such vast sums could have found their way to science and technology in *either* country absent the Cold War. Nothing has ever before given science such a lien on the wealth of nations, or made scientific and technical development such a high state priority, in so much of the world, for so long.

5. FINAL THOUGHTS

In closing, let me draw a few conclusions from this, and put them as provocatively as I can. I have four: one about Cold War history, a second about Soviet science, a third about its relation to the broader history of science, and the fourth about networks.

First, it seems to me that Cold War science, technology, and medicine are ripe for international and comparative study. Ironically, the very conditions that seem to have revitalized the study of the history of American science and led to such an efflorescence of excellent work in

that field have also left it strangely incomplete. In many ways, the social, ideological, financial, and even substantive development of American science and technology during this period has been shaped by its place in the great bi-polar dialectic of the Cold War. Yet, to read some of the Americanist literature, one might think all that happened was entirely the product of local home-grown politics, with the "Soviet Union" being some sort of convenient Platonic construct. Even in studying things American, there is much to gain by being less provincial.

We can learn much by comparative study, working internationally to unearth the relevant archives and put them into proper perspective. Studies are well underway, as one can see from the remarkable CNN "Cold War" television series recently aired. Even so, there would seem to be several vital pieces of this history that are still missing. For example, if my analysis is right, the rebirth of Soviet genetics and the rise of molecular biology may well have received support from the Soviet military-industrial-research-security complex, not only indirectly (via their physicist supporters), but more directly, because of its implications for biological warfare. I am not aware of any substantial documentation, publications, or archives chronicling Soviet biological warfare research during any period, let alone the 1950s; yet, from what I know, I feel certain that something important must be there. Until we get a better handle on Soviet secret and sharashka research of all sorts, and its counterparts in the United States, our understanding will be fundamentally incomplete. It would also help if we could know what each superpower was learning about the other's research through their intelligence "networks".

Second, in analyzing Soviet, Eastern, and totalitarian science generally, it is time for us to stop imagining that everything was visited upon science from above. In fact, successful national science has always been symbiotic with the state, and has always found ways to serve its interests.

It *must*, in order to acquire the resources it needs for its sustenance and growth—and with the postwar emergence of "Big Science" these needs have become vast. It is true that the Soviet Party-state developed techniques for controlling every aspect of scientific life more complete than even the most severe critics of totalitarianism might have feared; but it is also true that the scientific community proved more resourceful at manipulating that system to serve its own agendas than even the most optimistic advocate of academic freedom might have hoped.[28] As the higher government archives become increasingly available, it will become more and more difficult to balance the view from above with the view from below. Yet we must, for, however much every boss would like to hope that "What you SAY is what you get," we all know it just isn't so.

Third, I would make a plea for a reexamination of Soviet science generally. For too long, Western studies have emphasized the unique and exotic features of Soviet science—Lysenkoism, Marxist ideology, the Terror, the Gulag, the Soviet health care system—the things that have made Soviet science appear unique, strange, alien, pathological, or particular. Boggled by lists of Russian names few American historians have ever heard of (and fewer still can pronounce), and bewildered by the shifting meanings and arcane lexicons of Soviet "Newspeak," Western historians of science have tended to feel that the history of Russian science is somehow marginal to anything they need to know about or be interested in, a "specialists' specialty" with little to tell

---

[28] This view has been forcefully advanced, in much the same language, and with considerable documentation, in Nikolai Krementsov, *Stalinist Science* (Princeton: Princeton University Press, 1997). See also his article, "Russian Science in the Twentieth Century," in John Krige and Dominique Pestre (eds.), *Science in the Twentieth Century* (Amsterdam: Harwood Academic Publishers, 1997), pp. 777–794.

us about the history of science writ large, in America or elsewhere. Few would have seen the world this way before the First or Second World War: to a certain extent, I would suggest, this attitude is itself an artifact of the Cold War. The explosive rise of our own discipline in the West is a largely Cold War phenomenon, primed by the post-Sputnik funding for science; although the Cold War is over, we may still be suffering from the effects it has had on the way we have seen and interpreted things. Yes, Russian science is different from ours in many particulars—what else could one expect after almost half a century of relative isolation?—but, as I hope to have shown, the Russian story is both comprehensible and intriguing in the same terms we use to understand our own science, and for the same reasons. Furthermore, understood in this way, Russian science can provide a helpful comparative perspective on American and European developments. It is time to go beyond our Cold War, Anglo-American, Western European biases and bring the history of Russian science again into the mainstream of our analysis.[29]

Finally, perhaps it is time to begin a systematic study of the structure, nature, and roles of the networks that have helped shape the history of science generally. We all know bits and pieces, relating to the people and materials we study. Much of this information is available, masked as biography, reminiscence, or anecdote. Yet I suspect its systematic importance will only become clear when we put the information together, and change our analytic focus from the individual to the network itself. Networks do not leave many public traces, so we must use the opportunities provided by living informants—genealogies, family archives, reminiscences, interviews,

---

[29] Loren Graham's new book, *What Have We Learned About Science and Technology from the Russian Experience?* (Stanford: Stanford University Press, 1998) is a useful first step.

photographs, and private letters—before the information disappears. Yes, networks no doubt gained a special character and importance in Russia, given its political system and historical experience. But, upon reflection, can anyone active in science or the academy *anywhere* doubt that networks of the sort I have described also play pervasive and analogous roles in the West?

Curiously, it was my reconsideration of the "anomalous" case of Soviet genetics during the Khrushchev era that forced me to see what now seems obvious: when all the relevant disciplines and institutions had been seized, what other factor could account for the overthrow of Lysenko's "hegemony"? The anomaly *compelled* bringing networks to the fore. In retrospect, this is not surprising or unusual: pathology often provides insight into "normal" functioning by forcing us to pay attention to what, in the normal course of things, is hidden. Since coming to this realization of the centrality of networks, however, I have been increasingly impressed by its power as a tool in intellectual discourse and analysis more generally: I find myself seeing and understanding in new ways subjects I had already thoroughly studied and even published on. As I read articles in *Isis* and other journals, I am constantly surprised at the omnipresence of networks: sometimes they are actually talked about casually, elsewhere they are an essential part of the story—yet almost never are they explicit or analytically central.

What I propose is not especially new or radical: it is, as it were, a slight refocus, a five-degree shift, that lets us see in a new way—that can give new meaning and significance to things we all study, but which we have traditionally treated as "marginal" or "particularistic." Details that I now understand as being about networks are everywhere, hiding in a wide range of works, but dropping from view when the analysis and explanation begin. It often seems that, like Molière's character from "Le Médecin Malgré Lui," we have all been speaking prose without

knowing it! That slight refocus can be remarkably illuminating, I have found, suddenly bringing into sharp relief patterns that had previously been only cryptic or implicit. What I propose, then, is that we liberate, reconfigure, systematize, and "center" the idea of networks in order to be able to see familiar things in a new way.

When I first entered the history of science under Everett Mendelsohn's tutelage, the "internalist-externalist" debate (between history of ideas and social history) was still raging and, in a curious sort of way, I think it still remains with us. The basic concern and indeed excitement of our field still centers, at least for me, on understanding the intricate interrelationships between the impressive and beautiful intellectual achievements of science on the one hand, and, on the other, its complex social, political, and cultural contexts—the influences of time and place read broadly. Over the intervening decades, the "social history" of science has made much progress, moving from polemical pronunciamentos to the sophisticated analyses of many case studies. From an almost Platonic focus on the "heroic scientist" or "unit ideas," and little attention to their social contexts, we have developed considerable sophistication in understanding how *disciplines* and *institutions* have mediated between science and its broader social milieu, one that, in the process, has greatly enriched our understanding of both lives and ideas in science.

It seems to me time to make it a troika. If we are to see more clearly both the unity and diversity of world science, we should consider bringing our analysis of *networks* to the fore—not as marginal, coincidental, personal, or particularistic details, but as a central focus of analysis, reflecting a real phenomenon, an influential (if often hidden) substructure in science that has helped to fashion, establish, protect, shape, and remake both *institutions* and *disciplines*. Doing so, I believe, may also enrich our understanding of those other subjects commanding such

attention in our field: the moral economy of groups; the formation of schools; the varying place of women and ethnicities; the development of forms of laboratory and field practice; the origin of new specialties; the interdisciplinary and international spread of ideas, techniques, models, and metaphors; the appropriation of science to serve sociopolitical purposes; the conflicting agendas of public policy; and the ideologies, conventions, and popular images of science. By focusing on networks, we may find new commonalities in a wide range of work in our field, as well as new and better ways to bring "internal" and "external" together in our evolving understanding of the evolution of science itself.