SURROGATE EPISTEMOLOGY:

THE TRANSITION FROM SOVIET TO RUSSIAN BIOMEDICINE

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SURROGATE EPISTEMOLOGY:

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This dissertation follows the transformations in Russian biomedical epistemology occasioned by the collapse of the Soviet Union. The analysis focuses on the epistemic foundations of Russian biomedical science—what counted as valid clinical evidence, what the appropriate methods for producing such evidence were, and who got to make these decisions—and how these criteria, methods and power relations have changed in response to the collapse of the Soviet political economy and the introduction of a capitalist system. Drawing upon extensive archival research as well as ethnographic data, the analysis proceeds on three levels—that of institutions and organizations, practices, and ideas and rhetoric. On the level of institutions and organizations, the project focuses on the Soviet system of health protection, particularly those institutions within it that were involved in biomedical research and science policy, and examines their operation in the context of the Soviet-American exchange in oncology during the 1970s. On the level of practices, it examines Soviet drug development efforts in oncology, paying particular attention to clinical trial practices. And on the level of ideas and rhetoric, it traces two attempts to revise biomedical knowledge production and knowledge application practices. The first takes place in the Soviet period, and centers on the attempt to introduce the methods and concepts of cybernetics into Soviet biomedicine. The second takes place after the fall of the Soviet Union, and centers on the attempts to integrate the epistemic commitments of evidence-based medicine into Russian medical practice.

BIOGRAPHICAL SKETCH

Anna Geltzer was born in Stavropol, Russia. She left what was at the time still the Soviet Union for San Francisco, but only made it as far as New York. She completed her education in the New York City public school system. Geltzer holds a B.S. in Biology from Brooklyn College, where her training included work at the Aquatic Research and Environmental Assessment Center. It was in the course of this training that she first developed an interest in analyzing science and technology as social phenomena, and the Honors Program at the college gave her an opportunity to explore this interest. Upon graduation, Geltzer pursued a marketing career while earning an M.A. at New York University.

For my mother, who makes everything possible.

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INTRODUCTION

The world is vulnerable, the world is fragile,
Surrogates are replacing everything.
...And people are growing
anabolic muscles,
silicone chests
and plastic faces.
Timur Shaov, Surrogates

In the summer of 2005 I went to Russia to investigate the impact of the monetization reforms on access to medication.[1] The plan was to write an article, and then get back to my main research interest at the time—the global pharmaceutical industry and the impact of its business practices on American medicine. Of course, nothing ever goes according to plan.

In one of the first interviews I conducted with a Russian physician, I came across the puzzling term *dokazatel'naia meditsina*, or, as it is known in the English-speaking world, evidence-based medicine (EBM). Actually, it was not so much the term that was puzzling (the term itself struck me as redundant—what other kind of medicine is there, I naively thought to myself) as the way that my informant, an endocrinologist in her early forties dressed in a crisp, dazzling white robe, used it in our discussion. When I asked her about her drug selection practices under the new benefit scheme, she threw the term out at me like a shield: "there is evidence-based medicine… By the way it is the same all

[1] Monetization reforms were supposed to replace long-standing social benefits, such as free public transportation for seniors or free medication, with cash payments. They were getting a lot of Western press coverage at the time as a cause of civic unrest in Russia. Upon arriving in Russia, however, I found that there was no there there, at least not where medical benefits were concerned. With the possible exception of Moscow, Russians were long resigned to the fact that these benefits—particularly the free medication—had become a fiction since the collapse of the Soviet Union, and actually preferred the cash payments, no matter how small.

over the world…we have already reached the level where we have become interested in this."[2]

This response was puzzling for two reasons. For one thing, coming from a family of physicians who trained, practiced and taught in the very same town where the interview was taking place, it would have never occurred to me to suggest that Russian medicine was based on anything other than scientific evidence.

Nor did I understand what exactly she meant by this word 'level', and how this 'level' that Russian medicine has reached was in any way superior to that of an earlier period. In fact, the doctor's professional demeanor notwithstanding, what I saw around me indicated that the hospital (a branch of the regional hospital, and thus theoretically a better funded facility than most) had, at least on the material level, slid down several steps from where it had been in the Soviet era.[3] We were talking in a very old, cramped building that sat in the back of what, from my outsider perspective, had at first seemed to me to be an overgrown empty lot. The façade of the building, obscured in part by gnarled old plum trees, was more reminiscent of an abandoned tool shed than a hospital wing, with peeling walls and dark windows thrown open against the summer heat. Outside the hospital doors a small group of patients in battered bathrobes chatted and smoked, enjoying the refuge of the tree shade. The office we were sitting in was cramped and bare, and was being shared by at least four physicians. The room did have an old rotary phone, but none of the desks in the room had a computer.

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[2] Interview, Stavropol, July 2005.

[3] For more on the crisis in Russian medicine following the collapse of the USSR, see Mark Field and Judith Twigg (eds.), *Russia's Torn Safety Nets: Health and Social Welfare during the Transition* (New York: St. Martin's Press, 2000) and Vicki Hesli and Margaret Mills, Margaret (eds.), *Medical Issues and Health Care Reform in Russia* (Lewiston, NY: Edwin Mellen Press., 1999) as well as D.D. Venediktov, *Zdravoohranenie Rossii: krizis I puti preodoleniia* (Moscow: Meditsina,1999).

In this particular local context, the narrative of progress the physician was presenting seemed to be out of place. One can, of course, erase the incongruity by adopting a more global perspective. In fact, this is exactly what my interlocutor was doing by invoking Evidence Based Medicine and its global prevalence. From this global perspective, when it comes to the question of the efficacy of a drug, there is only one right way to go about producing a reliable answer—by conducting a double blind randomized clinical trial (RCT).[4] The RCT is taken as the gold standard because it is seen as the methodology best able to produce objective medical knowledge.[5] Properly designed trial protocols, and strict adherence to them, promise to rein in the biases of the investigator and the research subject. The former might feel invested in an experimental treatment or the well-being of the research subject and allow that investment to color his or her interpretation of the results. The latter may place a lot of faith in the experimental treatment or the investigator, and the power of this faith could actually alter those results (this is known as the placebo effect).

The double blind RCT promises to take the biases of both the investigator and the research subject out of the equation, rendering the results objective. The RCT accomplishes this in two stages. First, at the stage of producing the actual trial results, it removes clinical research from the realm of therapeutic medicine, thereby reining in the subjectivities of the investigator and the research subject.[6] This is accomplished by

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[4] Although within the EBM hierarchy, the RCT is not the absolute highest grade of evidence—the meta analysis is.
[5] Gerald Kutcher, *Contested Medicine: Cancer Research and the Military* (Chicago: University of Chicago Press, 2009).
[6] Harry M. Marks, *The Progress of Experiment: Science and Therapeutic Reform in the United States, 1900-1990* (Cambridge: Cambridge University Press, 1997) and Ilana Löwy, *Between Bench and Bedside: Science, Healing, and Interleukin-2 in a Cancer Ward* (Cambridge: Harvard University Press, 1996).

forcing both to subordinate their agency to the trial protocol.[7] For investigators, this means reformulating their goals and accepting limits on their judgment in accordance with the protocol. Medical decision-making in a clinical trial is driven by the imperative to maintain the integrity of the data, not to produce the best outcome for individual subject.[8] For research subjects, it means surrendering their agency virtually completely (now with informed consent, of course) to the trial protocol.[9] Criteria for drug efficacy are defined largely in quantitative terms so as to prevent investigators from forming qualitative evaluations of the drug's impact on the condition of the test subjects. Because numerically defining the impact of a drug on the condition of a test subject is a fraught undertaking, definitions of efficacy often focus on what is known as surrogate end points—physiological phenomena that lend themselves to quantification and precise measurements, such as T-cell counts or cholesterol levels.[10] Finally, at the stage of result evaluation, statistical methods are employed to abstract trial outcomes from the level of individual subjects to the level of the population. Individual research subjects and their bodies' reaction to the drugs are translated into numerical values subjected to various statistical manipulations, and the clinical trial produces population data on the impact of drugs on surrogate end points—information that is purged of the subjectivity of both the investigator and the research subject and abstracted from the messy reality of the doctor-patient encounter.

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[7] Practice does not always map exactly onto theory, of course. For example, Steven Epstein has shown how AIDS patients successfully resisted the clinical research system in their quest for access to new medications. See Steven Epstein, *Impure Science: AIDS, Activism, and the Politics of Knowledge* (Berkeley: University of California Press, 1996).
[8] Jill Fisher, *Medical Research for Hire: the Political Economy of Pharmaceutical Clinical Trials* (New Brunswick, NJ: Rutgers University Press, 2009).
[9] Carl Elliott, "Guinea-Pigging," *New Yorker,* January 7, 2008.
[10] Jeremy A. Greene, *Prescribing by Numbers: Drugs and the Definition of Disease* (Baltimore: The Johns Hopkins University Press, 2007).

Evidence-based medicine strives to bring the same kind of objectivity promised to clinical research by the RCT to the realm of therapeutic medicine—that is, to the doctor-patient encounter. However, unlike the RCT, EBM targets only the subjectivity of the doctor. The goal is to achieve a uniformity of practice so that a patient can rely on receiving the same standard of care whether he turns to a physician in a teaching hospital of a major metropolitan area or a country clinic.

Since the ability to discover and employ objective truths is the hallmark of science, the RCT and EBM fit neatly into the narrative of scientific progress in medicine which the endocrinologist who introduced me to the term *dokozatel'naia meditsina* invoked. This narrative traces the progression of medicine from an art of healing into a science of disease, marking the bacteriological revolution as the first major event in this transformation, because it provided medicine with a firm scientific basis by yielding a new understanding of disease causation. A scientific approach to the etiology of disease, however, still left a gap between the bench and the bedside—figuring out how to arrive at equally reliable knowledge in clinical research, as well as how to use this knowledge at the patient bedside was far from a straightforward matter.[11] The development of the clinical trial promised to bring this progress into clinical research itself by ensuring the objectivity of clinical research.[12] EBM, according to its advocates, is the current culmination of this progress because it brings objectivity into therapeutic medicine, enabling the objective *application,* not just *generation* of medical knowledge.

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[11] A problem that persists beyond the bacteriological revolution. See Löwy, *Between Bench and Bedside.*
[12] Marks, *Progress of Experiment* and J. Rosser Matthews, *Quantification and the Quest for Medical Certainty* (Princeton: Princeton University Press, 1995).

What my informant was telling me was that Russian medicine was finally catching up to this global medical progress by adopting the epistemic commitments that had fueled this progress in the rest of the world. From this global perspective, the peeling walls and the bare office were comparatively trivial problems. What mattered was that Russian medicine was now on a firm epistemic footing—that it mastered the right way of knowing, and was committed to mastering the right way of acting on its knowledge.

Implicit in her invocation of progress was a condemnation of Soviet science as backwards—a condemnation that was later made explicit by other Russian physicians I spoke with. As I go on to demonstrate, this condemnation mirrors the understanding of Soviet science held in the West—both in the Soviet period itself and in the present. Soviet science was then in the West and is now in the West *and* in Russia largely understood to be a 'dwarf' version of its western counterpart—a practice co-opted and deeply distorted by the Soviet state and its ideological and bureaucratic apparatus.[13]

The global perspective reproduced by Russian advocates of EBM, with its narrative of epistemic progress, is problematic for two reasons. First, because it isn't global at all. Epistemology has a history, with various times and places giving rise to different epistemic forms.[14] And second, because the invocation of global progress obscures the evolution of knowledge production and power relations in the local context.

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[13] The terms 'West' and 'Western' are very loaded and problematic, but they occur so often in the Russian context that avoiding them would distort the representation of that context. I do not want to rehash long-standing debates or pick at festering wounds here. Instead, I use the terms as my Russian actors use them—to refer to capitalist bloc countries with the United States in the lead.

[14] Karin Knorr Cetina, *Epistemic Cultures: How the Sciences Make Knowledge* (Cambridge: Harvard University Press, 1999), Lorraine Daston & Peter Galison, *Objectivity* (New York: Zone Books, 2007), and Alberto Cambrosio, Peter Keating, Thomas Schlich, George Weisz, "Regulatory objectivity and the generation and management of evidence in medicine," *Social Science & Medicine* 63 (2006):189-199.

By invoking EBM, the endocrinologist was trying to disarm potential criticism of the impact of the monetization of benefits on access to medication, but I argue that this rhetorical move has an unintended consequence as well—it foregrounds what I call the epistemic erosion of Russian biomedicine. The condemnation of Soviet knowledge production and the attempt to seek legitimacy for current Russian biomedical practices in the 'global' template of evidence-based medicine entails the abandonment of a local epistemic culture—a culture with a distinct imagination and set of values, practices and power relations—for one that is more powerful.[15]

The surrender of epistemic culture and imagination is not a simple process. It requires not only the reformulation of rhetoric, the reconfiguration of practice and the redistribution of power, but also the surrender of the goals and values that underpin the existing epistemology and the adoption of the value system associated with the ascendant epistemic culture. This process is both lengthy and difficult to direct, and in Russia it is still in comparatively early stages, with the medical profession divided in its commitment to EBM (largely along generational lines). I argue that at this historical juncture, EBM is a surrogate epistemology for the Russian medical profession. I use the term surrogate in the same way that it is used in the song couplet that opens the introduction—to designate an artificial substitute for a complex component of an organic structure (whether that organic structure be a body part or a relationship). Although the surrogate may be quite complex in its own right, its presence impoverishes the whole structure because the surrogate is by definition a reductionist device. As surrogate markers in drug evaluation provide a way to get around the juggernaut of complex biological phenomena and

[15] I take the term epistemic culture from Knorr Cetina, *Epistemic Cultures*.

subjective experience in order to make a decision about a compound's efficacy, so evidence-based medicine provides its advocates with a way to skirt the seemingly intractable problems facing them.

Methods

In this dissertation, I try to answer the questions that arose for me during the encounter with the endocrinologist—what constituted evidence in the Soviet period, how was such evidence produced, who got to make these decisions, and how these epistemic commitments and power relations changed following the collapse of the USSR? In other words, was there in fact something different about Soviet biomedical epistemology, or is the evidence-based movement in Russia just another illustration of the adage that the new is simply the thoroughly forgotten old?

I argue that the collapse of the Soviet Union has occasioned an epistemic shift in Russian biomedicine. The disintegration of the Soviet state, of which the system of health protection was an integral part, undermined not only the institutional structure within which biomedicine was practiced but also the epistemological foundations of biomedical practices—the normative structures of Soviet biomedicine fell apart along with the institutional ones, creating a space for the redefinition of medical science that brought with it a reconfiguration of professional identity.

I appropriate the term 'epistemology' from philosophy, where it refers to the branch of philosophical inquiry concerned with the nature and scope of knowledge, and use it to refer to a system of norms of knowledge production that can be studied

empirically.[16] Epistemology is a difficult object to study, not only because it does not lend itself easily to direct observation or precise measurement, but also because it inherently poses a level problem for the analyst, operating simultaneously on three levels—on those of institutions and organization, practices, and ideas and rhetoric. The temporal focus of the analysis further compounds the level problem in that it is still woefully understudied and poorly understood by Western and Russian scholars alike, necessitating a lot of background work. I begin in the period of what nameless party ideologues of the Brezhnev regime christened developed socialism and follow the downward spiral of perestroika through the collapse of the U.S.S.R. and the trials and tribulations of the transition period through 2005.

In what follows, I do not try to escape the level problem by restricting my focus to any one aspect. Nor do I claim to resolve it by trying to collapse the gaps between the three levels. Instead, I take this problem as a roadmap for the analysis, and approach it with the methodological toolkit developed within the field of science and technology studies—a toolkit that encompasses qualitative sociological and historiographic methods that allow for an analysis of all these levels in turn, and an exploration of the connections and contradictions between them. The analysis is grounded in a series of case studies chosen for the access they provide to the various levels of the problem.

On the level of institutions and organizations, I look at the Soviet system of health protection—particularly those institutions within it that were involved in biomedical research and science policy. The institutions I focus on include the Ministry of Health

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[16] This is not a novel move within STS—a number of scholars have performed a similar operation on objectivity. See, for example, Daston & Galison, *Objectivity* and Cambrosio et. al. "Regulatory Objectivity."

Protection (hereafter MinZdrav),[17] the Academy of Medical Sciences (hereafter AMS), which was subordinate to MinZdrav and was charged with formulating and implementing biomedical research policy on a national level, and the Institute of Experimental and Clinical Oncology (which in 1975 became the Oncology Scientific Center)—one of the most prominent institutes in the AMS network and the premier oncology research and treatment facility in the U.S.S.R. Other institutions that play an important role in my analysis are the Pharmacological Committee, which was part of MinZdrav and was charged with approving drugs for use in the Soviet system; and the All-Union Scientific Research Chemico-Pharmaceutical Institute (the Russian acronym, which will be used henceforth, is VNIKhFI)—part of the MinZdrav network of institutes whose responsibilities included the development and testing of new drugs, as well as the synthesis for domestic use of drugs developed abroad.

On the level of practices, I examine Soviet drug development efforts in oncology, paying particular attention to clinical trial practices. Although my focus is on Soviet practices, I consider them in the context of Soviet-American healthcare exchanges that began in 1956 and grew in size and importance until, in 1972, they were formalized by an inter-governmental agreement signed by Brezhnev and Nixon.

My aim in selecting this context is not to attempt a side-by-side comparison between American and Soviet practices—such a comparison would not only render the project unmanageable but would not provide answers to the questions that are guiding the

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[17] Western experts on Soviet healthcare have commonly dubbed this entity the Ministry of Health (see Michael Ryan, *Doctors and the State in the Soviet Union* (New York: St. Martin's Press, 1990) and Mark G. Field, *Soviet Socialized Medicine: An Introduction* (New York: The Free Press, 1967), perhaps as a matter of convenience. Although this formulation spares readers from having to keep track of a longer name, it is misleading in that it obscures the complexity of the organization's mission. Therefore I refer to this body either by its full name, or by its Russian abbreviation—MinZdrav.

analysis. Rather, I bring the American perspective in because the reflection of Soviet institutions and practices in American eyes provides a useful blueprint for describing their structure and workings and throws into relief their unique characteristics. Finally, including the American scientists and their perspective in the analysis is a necessity because they played a major role in Soviet biomedicine both directly, through personal interactions in the course of cooperative projects or participation in international organizations, and indirectly, by serving as a benchmark against which Soviet scientists and planners evaluated their own efforts.

On the level of ideas and rhetoric I look at two attempts to revise the practices of producing medical knowledge and applying it in the therapeutic context. The first takes place in the Soviet period, and centers on the attempt to introduce the methods and concepts of cybernetics into medical knowledge production and practice. The second takes place after the fall of the Soviet Union, and centers on the attempts to integrate the epistemological commitments of evidence-based medicine into the Russian context.

The descriptions and analysis of biomedical science in the Soviet period presented here are based primarily on archival research. Although the challenges of conducting such research in Russia have certainly changed since the Soviet period, they remain formidable. While Soviet record keeping was usually meticulous, many records did not survive the political and economic crises that surrounded the collapse of the USSR. Those that did survive are not easy to locate, as they are often scattered throughout many archives in Moscow and beyond. Furthermore, many Russian archives are not exactly user-friendly. Structural division and date usually organize collections, with no reference to the content of the records. When combined with stringent restrictions on the number of

folders one can request and have on hand at any given time, this often makes narrowing down one's search in the archives an impossible task.

To be able to address all three levels adequately, I consulted numerous collections in several archives, most of them in Moscow. Among the Moscow archives are the State Archives of the Russian Federation (GARF), which contain the records of the Soviet Ministry of Health Protection, including the Ministry's Pharmacological Committee; the collections of the Russian Academy of Medical Sciences (RAMN), which contain the records of the Soviet and the Russian Academies of Medical Sciences, including the plans and reports of the Academy institutes; the scientific archive of the Russian Oncology Center (RONC), which contains uncatalogued institutional records pertaining to the research programs and administrative activities of the Institute of Experimental and Clinical Oncology (IECO), the All-Union Oncological Scientific Center (VONC), and the All-Union Chemotherapeutic Center (VHTs); the Russian State Archive of Scientific and Technical Documentation (RGANTD), which contains the records of the All-Union Scientific Research Chemico-Pharmaceutical Institute (VNIKhFI); the archives of the Russian Academy of Sciences, which contains the records of the Scientific Council on Cybernetics of the Soviet Academy of Sciences; and the Russian State Archive of Contemporary History (RGANI), which contains the archives of the Central Committee of the Communist Party. In addition, I consulted the collection of records documenting the Soviet-American exchanges in healthcare and medicine at the National Archives and Records Administration (NARA) in College Park, MD.

The archival materials are supplemented by and triangulated against published biomedical literature from the Soviet period obtained at the Central Scientific Medical

Library and the Russian State Library in Moscow as well as at the Library of Congress in Washington DC and the National Library of Medicine in Bethesda, MD. Finally, to access individual perspectives I was able to conduct five oral histories from scientists and administrators who participated in the Soviet-American healthcare exchanges in the 1970s.

The portion of the project dealing with developments in the post-Soviet period draws on some archival research as well, but is mostly based on a qualitative study conducted that uses data collected in the course of 50 semi-structured interviews with Russian physicians, hospital administrators, medical students, and users of the healthcare system (the latter were included in order to gain a better understanding of the state of the healthcare system).

Geographically, the study sample was split almost evenly between two locations: Moscow and Stavropol, a city in southwestern Russia that serves as the administrative center of Stavropol Krai. Two sites were necessary because focusing on Moscow alone would have yielded very skewed results. Moscow enjoys material resources and administrative privileges that set it far apart from other Russian cities.[18] But while the situation in Moscow differs dramatically from the rest of the country, in a qualitative study it is also equally dangerous to exclude it, precisely because of its role as an administrative and financial center.

To successfully recruit participants from the medical community, a snowball sampling methodology was used. A few physicians within hospitals and polyclinics in Stavropol and Moscow were initially recruited, and they agreed to refer their friends and

[18] Moscow held a privileged position in the Soviet period as well, but in recent years the gap between the center and the periphery seems to have grown exponentially.

colleagues for the study. This strategy resulted in a very diverse sample. Among the practicing physicians there are representatives of various specialties (cardiology, endocrinology, oncology, general practice, anesthesiology) and of different levels within the hospital/clinic hierarchy (from rank and file physicians to hospital directors and administrators). The interviews were conducted and transcribed by me in Russian and subsequently coded for thematic content. The interview data were supplanted with published sources, both from the popular press and various biomedical publications.

Questions and significance

The question of what distinguished Soviet biomedical epistemology resonates with one of the central concerns of western historiography on Russia in the Soviet period, although in much of this work reflecting this concern is not formulated as a question. Much of this scholarship operates on the assumption that what was distinctive about the Soviet Union was the one party state and its stranglehold on power, the maintenance of which drove the expansion of the state apparatus into every facet of Soviet life. In this kind of analysis, western democracy provides the benchmark against which Russia in this period is compared, and the result of this comparison is that Russia is conceived of as being, by definition outside the norm—as pursuing an aberrant path of historical, political, cultural and economic development that puts it in opposition to and casts it outside of the developmental path of the west.[19]

[19] It is tempting to characterize this portrayal of Russia as a manifestation of the notion of Russian/Soviet exceptionalism. Like any ism, however, this is a very loaded term that, for all its ubiquity, is much more likely to confuse matters than clarify them.

This conception is reflected in Soviet historiography in several ways. First, in the questions that have dominated the field since its inception—questions about the inner workings of the party apparatus, the mechanisms of state terror, and the degree of popular support for or resistance to the regime. Although the paradigmatic answers to these questions have changed thanks to the acceptance of revisionist approaches that have repudiated the hitherto hegemonic totalitarian model and have brought more nuanced and complex analytical strategies to bear on the problem, the questions themselves have been much slower to diversify and continue to dominate the field.[20] Second, it is reflected in the field's empirical focus, most of which is on the period of the Russian Revolution and on various aspects of Stalinism and de-Stalinization.[21] While this is beginning to change, there is still much we do not know about the period following Khrushchev's removal.[22]

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[20] On broad trends in Soviet historiography, see Stephen Cohen, *Rethinking the Soviet Experience: Politics & History Since 1917* (New York: Oxford University Press, 1985); for a personal view on same, see Sheila Fitzpatrick, "Revisionism in Retrospect: a personal view," *Slavic Review* 67/4(2008): 682-704.

[21] This is by no means intended as a comprehensive literature review. On the Russian revolution, see Orlando Figes, *A People's Tragedy: A History of the Russian Revolution,* (New York : Viking, 1997), Richard Pipes, *The Russian Revolution* (New York: Knopf, 1990), Sheila Fitzpatrick,*The Russian Revolution* (Oxford: Oxford University Press, 1982) or Peter Holquist, *Making War, Forging Revolution: Russia's Continuum of Crisis, 1914-1921* (Cambridge:Harvard University Press, 2002), each of whom has a slightly different take. The historiography of Stalinism is too extensive to fit in a footnote. My favorite work on the topic tends to be social history. In particular, Stephen Kotkin, *Magnetic Mountain: Stalinism as a Civilization* (Berkeley: University of California Press, 1997); Sheila Fitzpatrick, *Everyday Stalinism: Ordinary Life in Extraordinary Times: Soviet Russia in the 1930s* (New York : Oxford University Press, 1999) and *Tear Off the Masks! Identity and Imposture in Twentieth-Century Russia* (Princeton: Princeton University Press, 2005). On the history of science in the Stalinist period, see Nikolai Krementsov, *Stalinist Science* (Princeton: Princeton University Press, 1997), Ethan Pollock, *Stalin and the Soviet Science Wars* (Princeton University Press, 2006) and Alexei Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (London: Imperial College Press, 2004). See Loren Graham, *Science, Philosophy, and Human Behavior in the Soviet Union*, (New York, Columbia University Press, 1987) for a history of Soviet science that is not constrained either temporally or analytically by the Stalinist period.

[22] Several recent conferences have showcased work in progress that considers the Brezhnev period: "What Was the Soviet Union? Looking Back at the Brezhnev Years," Wesleyan University, October 20-21, 2011 and **"**The End of the Soviet Union? Origins and Legacies of 1991," Forschungsstelle Osteuropa Bremen, May 19-21, 2011. On Soviet history after Stalin, there is currently a potpourri of work on a variety of topics, and this also is a very partial listing: Mark Edele, *Soviet Veterans of the Second World War: A Popular Movement in an Authoritarian Society* (Oxford: Oxford University Press, 2008); Juliane Fürst,

While the historiographic picture is changing, it is still a question for historians when, if at all, the Soviet Union achieved any kind of normalcy.[23] This state of affairs has its corollary in the subfield of the history of Soviet science, where questions of the relationship between science and the state and the impact of Marxist ideology on scientific ideas have reigned supreme. There is no doubt but that the answers to these questions are getting increasingly sophisticated. However the paradigmatic conception of Soviet science as, first and foremost, Soviet—that is, as a scientific enterprise at best crippled and at worst corrupted by the ideological and bureaucratic control of an omnipotent (or at the very least, omnipresent) state—continues to hold sway. Repeated attempts to draw attention to the narrowness of this perspective testify to the persistence of the problem.[24]

The questions that have been the focus of much of the western historiographic literature are crucially important and productive, and there is clearly a lot more to learn about Soviet history in the first half of the twentieth century. Practical reasons such as the availability of secondary literature also play an important role in reinforcing this focus in the literature. But casting the history of the USSR as lying outside the norm set by western liberal democracy makes it all too easy to conclude that the lessons of this history are irrelevant outside the Russian context.

Stalin's Last Generation: Soviet Post-War Youth and the Emergence of Mature Socialism (Oxford: Oxford University Press, 2010); Anne E. Gorsuch, *All This Is Your World: Soviet Tourism at Home and Abroad After Stalin* (Oxford: Oxford University Press, 2011); John Lamberton Harper, *The Cold War* (Oxford: Oxford University Press, 2011); John L.H. Keep, *Last of the Empires: A History of the Soviet Union 1945-1991* (Oxford: Oxford University Press, 2002). For an attempt to synthesize this work, see Stephen Lovell, *The Shadow of War: Russia and the USSR, 1941 to the Present* (Oxford, Wiley-Blackwell, 2010).

[23] Lovell, *The Shadow of War.*

[24] See, for example, Michael D. Gordin and Karl Hall, "Introduction: Intelligentsia Science Inside and Outside Russia," *Osiris* 23(2008):1–19.

My point of departure for the analysis that follows is that the Soviet Union was normal—that is, that it was a society as well as a state, and that Soviet science was first and foremost science—that is, a collective endeavor to produce reliable knowledge about the natural world and to apply that knowledge to collectively defined problems. In this I draw heavily on the growing sociological and anthropological analysis of the Soviet and post-Soviet period that examines meaning-making in the Soviet and post-Soviet space and puts the individual experience at the center of analysis,[25] and on the analytical tools of science and technology studies—a discipline that puts scientific practice at the core of its methodology.[26]

The application of this approach doesn't silence the debate on what distinguished Soviet science—rather, it opens it up to additional questions and answers that take the analysis beyond the binary focus on science and the state. The first question that must be addressed in this regard is the question of the distribution of power. In the binary approach, the power relations between science and the state are simple—the power is seen to belong to the party state, and scientists are relegated to, at best, the role of state employees who must surrender their agency over their work and pursue the goals set for

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[25] Anthropological analysis of the Soviet period has been steadily growing since the collapse of the Soviet Union. See, for example, Alexei Yurchak, *Everything Was Forever, Until It Was No More: The Last Soviet Generation* (Princeton: Princeton University Press, 2006); Nancy Ries, *Russian Talk: Culture and Conversation during Perestroika* (Ithaca: Cornell University Press, 1997) and Caroline Humphrey, *Marx Went Away—But Karl Stayed Behind* (Ann Arbor: The University of Michigan Press, 2001). Anthropological analysis of the post-Soviet period has exploded. Examples include Adriana Petryna, *Life Exposed: Biological Citizens after Chernobyl*, (Princeton: Princeton University Press, 2002); Douglas Rogers, *The Old Faith and the Russian Land: A Historical Ethnography of Ethics in the Urals* (Ithaca: Cornell University Press, 2009); Elizabeth C. Dunn, *Privatizing Poland: Baby Food, Big Business, and the Remaking of Labor* (Ithaca: Cornell University Press, 2004).

[26] The breadth of this literature makes a thorough review impractical. For the iconic examples of the application of this method, see Bruno Latour and Steve Woolgar, *Laboratory Life: the Construction of Scientific Facts* (Princeton: Princeton University Press, 1986) and Steven Shapin and Simon Schaffer, *Leviathan and the Air Pump: Hobbes, Boyle, and the Experimental Life*, (Princeton: Princeton University Press, 1985).

them by the party bosses.[27] I argue that this hierarchical picture of the distribution of power within the Soviet system does not accurately describe the power dynamics of the late Soviet period. The problem with this model isn't just that there are counter examples. The problem is that the model is wrong. I argue that the dynamics of power during this period were Foucauldian—that is, power was (unevenly) distributed throughout the system rather than concentrated in the hands of the party apparatus or the security organs, and it was vested in individuals as well as institutional structures.[28]

How then did Soviet science work, if not according to plan? Looking closely at the workings of the institutions of Soviet biomedicine and tracing the activity of individual scientists within them, one sees a collaborative and competitive collective enterprise of knowledge production that has much in common with science found in other national, political and economic contexts. But while there are many similarities, I demonstrate that Soviet biomedical epistemology was, in fact, distinct in that it defined medical science as simultaneously biological, social and humanistic—a definition that did not tie the reliability of medical knowledge to the elimination of the subjectivity of its producers, but to the cultivation and disciplining of this subjectivity. This disciplining centered not on the development and enforcement of a set of rules of conduct, but on the inculcation of a sense of duty. This definition had important practical consequences in that Soviet biomedical practitioners did not try to adhere to an absolute separation between therapeutics and clinical research when producing medical knowledge, leading to a set of clinical research practices that contemporary advocates of EBM argue were unequal to the task of producing reliable knowledge.

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[27] Alexei Kojevnikov, "The Phenomenon of Soviet Science" *Osiris* 23 (2008):115–135.
[28] Michel Foucault, *The History of Sexuality: An Introduction, Volume I* (New York: Vintage Books, 1990).

These epistemic commitments to the primacy of the therapeutic imperative and the centrality of the individual contrast sharply with the normative structures of American biomedical research in this period, which were undergoing a process of rationalization which emphasized the subordination of therapeutic goals to the research protocol, the primacy of statistical over qualitative data and the production of objective knowledge through collective action. The contrast is especially clearly visible in the interactions between Soviet and American practitioners. What explains this difference? One explanation is that Soviet biomedicine was behind the curve—that it was slower to shed the kinds of practices that American practitioners had left behind as improved methods of knowledge production, such as the RCT, were developed and gained acceptance. This is certainly how American scientists saw their counterparts during the Soviet period, and it is the favored explanation of contemporary Russian EBM advocates. But this explanation is based on a Whiggish conception of medical progress which assumes the inevitability of the course of these transformations. While I do not assume that this assumption of inevitability is wrong, I do argue that by itself it does not constitute an explanation. Epistemic commitments and values, such as objectivity, have a complex, non-linear history and typology, as has been beautifully demonstrated by a number of scholars.[29]

The rationalization of biomedicine is a relatively new and dynamic subject of analysis within the social studies of medicine. But although this scholarship is not in imminent danger of producing a consensus, there are several underlying assumptions that structure the discussion. The first is that this process of rationalization is a political

[29] Daston & Galison *Objectivity*; Cambrosio et. al. "Regulatory Objectivity"; Michael Lynch, "Protocols, practices, and the reproduction of technique in molecular biology," *British Journal of Sociology* 53/2 (2002): 203-220.

project. Some analysts have argued that this process of rationalization is driven by the desire of democratic societies to regulate medical judgment, and the faith of regulatory agencies that statistical methods are able to provide the objectivity that the medical profession lacks when left to its own devices.[30] Others see it as a project of professional politics, with the medical profession employing rationalization to unify its ranks and police its boundaries as well as underwrite its legitimacy.[31] For still others, rationalization is a neutral tool that can be employed in any number of political projects (and thus can be easily turned on those who deploy it).[32] The second assumption that underlies this analysis is that this process is tied to the economic logic of global capitalism. Some analysts have gone so far as to suggest that we understand "political economy as an *epistemology*" since the biomedical sciences are over determined by the capitalist political and economic structures within which they emerge.[33] Others stop short of collapsing the differences between these categories while documenting their interpenetration.[34]

Jeremy Greene, for example, demonstrates very convincingly through his case studies of chronic diseases in America that clinical research practices shape our understanding of health and disease, transforming the line between the normal and the pathological into a numerical abstraction. He compares the impact of the commercial practices of drug development on medical knowledge and practice with Max Weber's

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[30] Matthews, *Quantification.* Ted Porter makes a similar argument about quantification more generally. See Theodore M. Porter, *Trust in Numbers: the Pursuit of Objectivity in Science and Public life* (Princeton: Princeton University Press, 1995).
[31] Marks, *Progress of Experiment.*
[32] Stefan Timmermans and Mark Berg, *The Gold Standard: the Challenge of Evidence-Based Medicine and Standardization in Healthcare* (Philadelphia: Temple University Press, 2003).
[33] Kaushik Sunder Rajan, *Biocapital: The Constitution of Postgenomic Life* (Durham: Duke University Press, 2006), 11.
[34] Fisher, *Medical Research for Hire.*

'iron cage' of capitalism and concludes that contemporary American medicine

> … in many ways conforms to Weber's vision: equal parts science, commercialism, and the extension of bureaucratic rationality, this system threatens to enclose humanity within a process of physiological monitoring and pharmaceutical consumption. However, whereas Weber's iron cage was built on the inflexible certainty of technological rationality, the structure we now inhabit is flexible, for its links are bound not in certainty but uncertainty: in probability, statistics, and calculations of risks. Within this contemporary understanding of health and medicine, the concept of disease itself enjoys far more freedom of motion than does either doctor or patient.[35]

In this narrative, the medical progress of which the clinical trial is both emblem and constitutive element becomes a dystopian phenomenon that expands the boundaries of the pathological at the expense of the normal, enclosing an ever-greater proportion of the population in the rubber cage of market rationality.

While the case for the connection between political economy and biomedical epistemology is compelling, the nature of this connection and the mechanism of this relationship need further analysis. Russian biomedicine is a fruitful site for considering these questions. First, because examining it during the Soviet period allows us to observe both a biomedical epistemology and a political economy distinct from those prevalent in the west. And second, because following this case through the Soviet collapse provides a unique historical opportunity to see these structures change together.

Structure

Two organizing principles structure the following chapters. On the one hand, the chapters are organized around case studies. Chapters 1 and 2 deal with various aspects of the Soviet-American healthcare exchange, chapter 3 follows at the history of medical

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[35] Greene, *Prescribing by Number*, 6.

cybernetics, chapter 4 focuses on the problems faced by scientific planners and administrators in the perestroika period, and chapter 5 looks at the adoption of evidence-based medicine rhetoric among the medical profession.

On the other hand, the chapters are organized around the analytical elements that together make epistemology visible. Thus, chapter 1, which tells the story of the evolution of Soviet-American healthcare exchanges and documents how the Soviets administered them, analytically is a chapter about institutions. It provides a functional portrait of some of the key institutions that constituted the system of health protection, and begins to make the case for reconsidering the distribution of power within the Soviet system.

Chapter 2 details Soviet clinical research methods and practices to draw out some of the unique features of Soviet biomedicine. It uses the American perspective to highlight the idiosyncratic features of Soviet drug development practices and argues that what American observers saw as chaos and lack of discipline was in fact the direct result of a distinct epistemic culture in which therapeutic concerns were prioritized over those of clinical research.

Chapter 3 continues the focus on the normative structures of Soviet biomedicine through a case study of medical cybernetics. It follows the failed attempt by advocates of medical cybernetics to simplify the definition of medical science and redistribute authority over medical practice—rhetorical moves employed by the practitioners of this new discipline to legitimize their expertise and secure for themselves a role in the regulation of both medical knowledge production and practice.

In Chapter 4, institutions, practices and normative structures intermingle as I attempt to make sense of the changes in the Soviet system of health protection during the 1980s. I argue that the Soviet biomedical establishment in this period was undergoing not only an institutional crisis brought on by a shortage of funds, but also through an epistemic erosion that undermined its normative structures and made normal scientific practice impossible.

Chapter 5 examines the adoption of evidence-based medicine rhetoric in Russian medical circles. It returns to the theme of authority, examining the efforts of the medical profession to define a new epistemic basis for professional authority necessitated by the destabilization of the institutional and normative structures of the system of health protection.

CHAPTER 1: IN THE KINGDOM OF CROOKED MIRRORS

We are dwarf birches.
We have cleverly made up our poses,
But all this is merely pretense.
Our bends are a form of resistance.
Yevgeniy Yevtushenko,
Dwarf Birches, 1966

On February 27 1956, at 3:15 pm local time, a group of Americans landed in Leningrad and, as Dr. Michael B. Shimkin, a participant in the American Mission on Microbiology and Epidemiology to the Soviet Union recalled in his report of the trip, "entered a different world."[36] The differences that struck Shimkin's senses first were physical—the dark, oddly quiet airport with a few heavily dressed people scattered along the mostly empty benches and a lonely kiosk offering a "pitiful selection of toiletries."[37] As Shimkin and his colleagues progressed on their tour of 21 medical research institutes throughout the USSR, however, other differences began to emerge.

Perhaps the most obvious of these were differences in organization. What stood out the most, of course, was that Soviet science was planned. That is, Soviet scientists were embedded in a highly complex administrative structure that coordinated their activities and set their research agenda in accordance with the needs and priorities of the state. As Shimkin observed in his report, "having no faith in any Divine plan, the Soviet State has elevated human plans to that level. Five-year plans are not only expressions of purpose; they are religious dedications."[38] Hand in hand with planning went centralization—and compartmentalization. The practical manifestation of this trend was

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[36] "The American Medical Mission on Microbiology and Epidemiology to the Soviet Union, February-March 1956," p. 6, in National Archives and Records Administration (NARA), RG 443, Box 144, Folder: INTL 4-1 US-USSR, 1956-1969. It should be noted that this was not Shimkin's first time in the Soviet Union—he had visited the USSR on a similar mission in 1944.
[37] Ibid.
[38] Ibid., 20.

that "every activity in science and related technology is grouped into Institutes: educational ones, research ones, production ones."[39]

The American Mission on Microbiology and Epidemiology was one of the first American medical delegations to the Soviet Union since the interruption in scientific contacts that followed the 1947 KR affair and the onset of the Cold War.[40] Perhaps that is why in his report Shimkin refrained from evaluating the Soviet system of research, emphasizing instead that the delegation was of the opinion that:

> …it would be arrogant, stupid and even dangerous for the United States to ignore Soviet medicine and research; we agree that all appropriate steps should be taken to develop channels of communication between the medical scientists of the two countries by exchange of literature, materials, and personnel; we agree that we have something to learn from their achievements, but that *understanding* of their medicine and research is even more important than mere *knowledge*.[41]

This view—that an understanding the logic of the Soviet system of health protection was intrinsically important—seems to have been shared in American political and scientific circles. On January 27, 1958 the US and the USSR signed the Lacey-Zarubin agreement—the first of a series of comprehensive, formal intergovernmental agreements that were to govern cultural, educational, scientific and technical exchanges between the two superpowers until 1972, when a more comprehensive cooperative

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[39] Ibid., 18.

[40] The KR affair is the story of two Soviet scientists who had developed a biological preparation that they believed had potential as a cancer treatment. The preparation attracted a lot of attention both within and beyond the USSR, and this attention got the scientists in trouble—they were tried before a 'court of honor' for divulging state secrets. The affair did not cost the scientists their lives (nor did it put an end to their careers), but it did send a signal to the Soviet scientific community that close contact with and attention from foreign colleagues could be dangerous. For more on this episode, see Nikolai Krementsov, *The Cure: A Story of Cancer and Politics from the Annals of the Cold War* (Chicago: University of Chicago Press, 2002) and V.D. Esakov and E.S. Levina, *Stalinskie 'Sudy Chesti': Delo 'KR'* (Moscow: Nauka, 2005).

[41] NARA, RG 443, Box 144, Folder: INTL 4-1 US-USSR, 1956-1969. Emphasis in the original.

program in health and medicine was launched.[42] These agreements included exchanges in the health and medical field, and the number of reciprocal visits by medical specialists grew very rapidly during the sixties.[43]

That these exchanges contributed to an increase in knowledge—both scientific and cultural—on both sides is beyond doubt. But, as this chapter demonstrates, an understanding of Soviet medicine and research proved elusive. In this chapter, I use the Soviet-American exchanges in health and medicine during the 1960s and 1970s as a prism for teasing apart Soviet and American representations of the Soviet system of health protection, provide a functional institutional portrait of that system.

I argue that American observers involved in these exchanges misinterpreted the Soviet system, and that the origins of this misinterpretation are to be found in the Cold War interactions between the two superpowers. The Cold War was more than a structural constraint in these interactions. It wasn't just that political tensions between the superpowers interfered in the process of scientific cooperation, or that the geopolitics of the period inflected science with political significance. Rather, I demonstrate that the Cold War was an important analytical framework that exchange participants employed in their interpretations of and responses to interactions with their foreign colleagues, and that the adoption of this framework by the exchange participants posed a barrier to achieving mutual understanding not only of the other side's motives or intentions, but of the science itself.

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[42] "A Summary Report on the United States Exchanges Program with the Soviet Union." NARA, RG 514, Box 6, Folder: 1964 Agreement: Press Releases and Memo.

[43] It is worth noting that the exchanges preceding this agreement, as well as those that followed in the first couple of years after its signing, were apparently funded not by the US government but by grants from pharmaceutical companies and private foundations. Only in 1962 did the NIH step in to provide the program with a firmer financial basis. Verne G Robinson, "Historical Notes for Dr. King", September 18 1972. NARA, RG 514, Box 7, Folder: OIH Reports & Notes re US-Soviet Health Exchange.

Although the distortions were equally prevalent on both sides, my focus here is on the representations of Soviet science. Articulating and confronting the misinterpretations of Soviet institutions is important because in many ways they continue to structure contemporary perceptions of the Soviet system of biomedical[44] research both in the west and now in Russia as well. At present, Soviet medicine remains woefully understudied and poorly understood, particularly in the period of developed socialism. While a number of contemporary analysts have provided descriptions of the organizational structure within which Soviet biomedical research operated, in the course of the Soviet Union's existence access to information was both limited and carefully managed by Soviet authorities. The result is that this scholarship can tell us very little about what this organizational structure was like in practice. [45] In addition, much of this scholarship has focused on the Soviet system of healthcare provision, glossing over the biomedical research enterprise. Although historians are beginning to turn their attention to the subject of Soviet medicine, most have focused on the post-Revolutionary and Stalinist periods, and even here the surface has barely been scratched.[46]

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[44] The Russian term for this in the Soviet period was *mediko-biologicheskie issledovania*—that is, the order of the two words forming the portmanteau word is reversed. The significance of this will become clear later in the chapter.

[45] Field, *Soviet Socialized Medicine* and Ryan, *Doctors and the State.*

[46] See Susan Gross Solomon, ed., *Doing Medicine Together: Germany and Russia Between the Wars* (Toronto: University of Toronto Press, 2006); Krementsov, *The Cure*; Tricia Starks, *The Body Soviet: Propaganda, Hygiene, and the Revolutionary State*, (Madison, WI: The University of Wisconsin Press, 2008). There have also been a number of dissertations at the University of Chicago which remain unpublished: Christopher Burton, "Medical Welfare during late Stalinism: A Study of Doctors and the Soviet Health System, 1945-1953" (PhD dissertation, University of Chicago, 1999) and Michael David, "The White Plague in the Red State: The Control of Tuberculosis in Russia, 1900-1941" (PhD dissertation, University of Chicago, 1999). The body of scholarship produced on the subject in Russia is also quite small—although the Soviet period is covered in some sweeping history of medicine texts (see M.B. Mirskii, *Meditsina Rossii X-XX Vekov: Ocherki Istorii*, (Moskva: Rosspen, 2005), there have been very few published historiographies dedicated to the period.

While little is known about the Soviet system of health protection, much is assumed. In particular, there are long-standing assumptions about the inner workings of the system and the relationship between biomedical scientists and the Soviet state that were formulated by American observers in the course of these exchanges and have found their way into western scholarly analysis of Soviet science and medicine. The assumptions, which I characterize in greater detail below, are that the Soviet system of health protection was strictly hierarchical and subordinate to the state, that it operated according to plan, and that it disempowered clinicians. As I demonstrate below, these assumptions don't hold up when one looks at the system in practice. I argue that, rather than being concentrated in the hands of the state, power was distributed (albeit unevenly) throughout the system; that the planning system was in fact quite flexible in practice, allowing individuals considerable opportunity to exercise agency over their work; and that therapeutics were well represented in the circles of power.[47]

Soviet-American cooperation provides a useful lens both because it circumscribes an otherwise unmanageable area of inquiry and because placing the issue of Soviet-American relations at the heart of the case study allows me to address the questions of the relationship between biomedical scientists and the Soviet state and the place of Soviet biomedical science within the global biomedical enterprise while tackling the problematic legacy of the Cold War head on. Engaging with this legacy is important first because the Cold War in this period was a powerful epistemic resource.

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[47] In fact, clinicians dominated the Academy's leadership positions, and the bulk of the Academy's resources appear to have gone to the Division of Clinical Medicine (at least that is the division that boasts the largest number of Scientific Centers—large conglomerates of research and clinical institutes united by their specialty, such as VONC—N.N. Blokhin, the director of VONC, served two terms as Academy president, the first from 1960-1968 and the second from 1977 to 1987).

Although the tensions between the Soviet Union and the United States, as well as the misunderstandings and stereotypes that accompanied them, preceded the Cold War, they acquired a renewed intensity in this period and became a fixture in the cultural landscape of both countries, becoming a kind of analytical framework that the actors of the period drew on to make sense of their world. In elaborating this framework, the work of Paul Edwards, who conceptualized the Cold War as a closed-world drama in which the globe was divided between rigidly separated and actively antagonistic camps, serves as a departure point.[48] This concept of a globe firmly divided into two mutually exclusive, antagonistic worlds was the foundation of the Cold War analytical framework, both on the Soviet and the American side. The 'Cold War' here is not simply a historical period, but a term that designates a particular system of international relations and domestic politics that held sway in the US and the USSR for much of the twentieth century. An 'analytical framework' is not a conceptual tool available only to analysts, but, as Erving Goffman defined it, it provides the answer to the question "what is it that is going on here?"[49]—a conceptual tool that is available to both actors and analysts.[50]

Another reason why addressing the legacy of the Cold War is important is that it colors much of the scholarship not only on Soviet medicine, but even Soviet science more generally. For example, while the question of the relationship between Soviet

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[48] Paul Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, MA: The MIT Press, 1996).
[49] Erving Goffman, *Frame Analysis: an Essay on the Organization of Experience* (New York: Harper & Row, 1974).
[50] I am not so much intentionally blurring the boundaries between the analytical tools of actors and analysts as pointing out that this boundary has, in fact, already been frequently blurred. Susan Gross Solomon has called attention to this problem in early Western scholarship on Soviet science (see Susan Gross Solomon, "Western Studies of Soviet Science," in Linda L. Lubrano and Susan Gross Solomon, eds., *The Social Context of Soviet Science* (Boulder: Westview Press, 1980)), and more recent attempts to call attention to the problem testify to its persistence (see Loren Graham, *What Have We Learned about Science and Technology from the Russian Experience?* (Stanford, CA: Stanford University Press, 1998) and Michael Gordin and Karl Hall, "Intelligentsia and Science."

science and the state has received considerably more attention from historians,[51] the prevailing characterization of this relationship is incomplete and somewhat misleading. This characterization can be summarized as follows. Although the Soviet government had been very consistent in its favorable predisposition towards science, it was equally consistent in its drive to subordinate the scientific community to state goals and priorities. Achieving this subordination took time—the fledgling Soviet state was too weak to accomplish it immediately after the Revolution, and for a time the scientific community was able to wield real power not only over its own work, but also over some issues of national policy. But as the Bolsheviks consolidated their power, Soviet science essentially became a branch of the civil service, with the scientific community losing much of its autonomy and agency not only when it came to matters of national importance, but in their work as well. Although certain disciplines such as physics prospered in this environment, this was because their strategic importance to a certain extent exempted them from strict government control and provided them with privileged access to material and intellectual resources.[52] On the other side of the spectrum were less fortunate disciplines such as genetics, which were mangled by political interference and ideological distortions. For the most part, however, the system effectively stifled research, condemning Soviet science to mediocrity. While scholars have begun to problematize this characterization of the Soviet science-state relationship, painting a

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[51] Among others, see David Holloway, *Stalin and the Bomb: The Soviet Union and Atomic Energy, 1939-1956* (New Haven: Yale University Press, 1994); Loren Graham, ed., *Science and the Soviet Social Order* (Cambridge, Mass: Harvard University Press, 1990); Linda L. Lubrano and Susan Gross Solomon, eds., *The Social Context of Soviet Science* (Boulder: Westview Press, 1980); Paul R. Josephson, *New Atlantis Revisited: Akademgorodok, The Siberian City of Science,* (Princeton: Princeton University Press, 1997).
[52] David Holloway, "Physics, the state, and civil society in the Soviet Union," *Historical Studies in the Physical and Biological Sciences*, Vol. 30, part 1, 1999:173-193. It should be noted that, while the discipline of physics as a whole prospered, individual physicists didn't always fare as well.

detailed portrait of the intricate interdependency between the two, the nuanced understanding they provide has yet to become dominant.[53]

Finally, the Cold War legacy needs to be addressed because it is visible not only among Soviet and American scientists in the course of the exchange but also, in a different way, among analysts of Soviet science. And, to a certain extent, it continues to play a role in current understandings of Russian biomedical science—both among analysts and among the biomedical community itself, which has picked up on Cold War rhetoric to discredit old approaches and erect a new foundation for legitimating professional authority.

I begin outlining the history of the Soviet-American exchanges in health and medicine with an exploration of the way American scientists and administrators understood the Soviet system of biomedical research.[54] I then look at the way the Soviets understood their own system, and trace how the system functioned in the context of the exchange. The chapter concludes with an analysis of the dissolution of the exchange.

The road to cooperation

Scientific exchange with colleagues in the US was a not-uncommon practice in Soviet biomedical science in the years leading up to the Cold War, but its onset brought a significant disruption of such contacts. This interruption in contact between Soviet and American biomedical scientists lasted almost a decade, ending in January 1956 when a Soviet medical delegation arrived in the US in search of information on the newly

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[53] See for example Krementsov, *Stalinist Science* or Slava Gerovitch, *From Newspeak to Cyberspeak: A History of Soviet Cybernetics*, (Cambridge: The MIT Press, 2002).

[54] This examination of American participation in the exchange is by no means comprehensive, and is not meant to be the focus of the chapter. The nature of the subject makes ignoring the American perspective altogether impossible, however.

developed polio vaccine and laboratory equipment for the new Poliomyelitis Institute in Moscow.[55] Only days after the Soviets returned home, an American delegation reciprocated this visit, setting the pattern for an increasingly active exchange of personnel that continued into the early 1970s.

Both the Soviet and the American sides were guided by certain assumptions about each other's intentions and about the biomedical research enterprise of the other party in their conduct in the exchange. A 1964 Summary Report on the United States Exchanges Program with the Soviet Union put together by the Soviet and Eastern European Exchanges Staff at the Department of State articulates the American assumptions and goals very clearly.

The report begins by noting that, "since the internal systems and external policies of the United States and the USSR differ radically, it is to be expected that the goals and methods of the two countries in a program of bilateral exchanges are considerably at variance."[56] One difference was that whereas Soviet society was seen as "largely closed and controlled," US society was "open."[57] The other major difference was that "the Soviet Union was still a developing country, very successful in fields to which it gives top priority, far behind the West in many other fields it considers less important."[58]

Interpreting Soviet goals for the exchanges as being to obtain scientific and technical information and promote a favorable image of the USSR and its policies, the Department of State thus concluded that the top priority of the US government in these exchanges was to safeguard against a one-way flow of information and to protect national

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[55] NARA, RG 443, Box 144, Folder: INTL 4-1 US-USSR, 1956-1969.
[56] Press releases & memo, p.2, NARA, RG 514, Box 6, Folder: 1964 agreement.
[57] Ibid.
[58] Ibid., 3.

security. Soviet propaganda was not seen as a major concern, since the US system was "based on freedom of information" and thus "Americans can reasonably cope with propaganda."[59]

The Department of State's own stated goal for the exchange was to promote the "normal flow of information and persons between the two countries."[60] The purpose of this was twofold: to learn more about the "world's second strongest power"[61] and, in the long term, to perhaps influence it to pursue more constructive directions. Although the report acknowledged that the exchanges were "not a strong enough vehicle to reform the Soviet Union or to solve fundamental problems,"[62] it was nonetheless hoped that "since the Soviet system operates on the basis of propaganda, providing Soviet citizens with factual information" would be "of great importance" and could potentially have "great effect."[63]

This approach to the exchanges and conception of the Soviet research enterprise was the lens through which American scientists viewed at their Soviet colleagues, and it had a direct impact on their understanding of Soviet biomedical science. American observers perceived Soviet biomedical science, like all Soviet society, as closed and controlled in the sense that it was isolated and planned. Soviet biomedical scientists were isolated both from their colleagues abroad—with the result that their science had a somewhat "parochial" character, remaining "outside of the channels of world medical developments"[64]—and from each other, resulting, among other things, in the apparent

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[59] Ibid., 4.
[60] Ibid.
[61] Ibid., 5.
[62] Ibid.
[63] Ibid., 4.
[64] Field, *Soviet Socialized Medicine*, 183.

separation between medical practice and medical science. A virologist who spent six months in the Soviet Union in 1962 pointed to this in his report:

> A most important factor to recognize in the Soviet medical sciences is the relative status of clinical versus research people. The line is very clear and heavily drawn between research and clinical workers in terms of pay, prestige, membership in the Academy, position in medical politics and, above all, a sense of personal security. For the most part, the clinician (defined by his training and organization, regardless of whether he is personally engaged in research) is the 'low man' in the pecking order. Exceptions do exist, but they are rare. Because of his status the clinician has little latitude to act on his own initiative.[65]

The Soviet planning system was understood to envelop biomedical scientists in a highly complex administrative structure that coordinated their activities and set their research agenda in accordance with the needs and priorities of the state. The consequence of this was that "Soviet research is goal-oriented and highly centralized … all planning and thinking is centered in one man at the head of each institute."[66] The problem with this was that, "although he may be quite capable, there are serious difficulties in establishing or changing programs, clearing manuscripts, etc."[67] Furthermore, this system fostered too strong a respect for authority which, "in science, tends to inhibit the evolution of concepts."[68]

Medical science was understood to be an area of low priority for the Soviet government, and consequently for the most part inferior to what was being done in the US. Nonetheless, American observers did not discount Soviet science completely. For example, the Pharmacology and Physiology of the Nervous System Medical Mission noted in their report that "when one reflects that the majority of Soviet research institutes

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[65] NARA, RG 514, box 9 , Folder: Dr. Jacob A. Brody, 4.
[66] Ibid., 7.
[67] Ibid.
[68] Ibid., 8.

are postwar in origin, that their application to modern research techniques is relatively recent, one cannot be complacent about their potential for rapid progress in the future" and recommended "that research in the United States in neurophysiology and neuropharmacology be supported even more generously than it has been, otherwise we shall be overtaken."[69] Some Soviet research programs were even considered potentially valuable. The Second Cardiovascular Mission to the Soviet Union granted that "in one or two fields of cardiovascular research Soviet science is possibly making unique contributions."[70] In any case, exchanges with the Soviets were deemed to be of value because "Soviet problems in cardiovascular disease are very similar to the American, their approaches are identifiably ours, and we work on completely common ground."[71] Individual researchers and institutes even garnered admiration.

American scientific planners also thought the information gathered in the course of the exchange potentially valuable. The lessons in organization that could be derived from a close study of the Soviet system were deemed particularly important. In a memo to the Director of the NIH and the Chief of the Office of Program Planning, OD, an official with the Department of Health, Education, and Welfare suggested that "it would be useful to consider an exchange mission centered upon the examination and review of the structure, organization and administration of the medical research activities."[72] Information gathered by such a mission would be useful given that

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[69] NARA, RG 514, Box 9, Folder: Report of the Pharmacology and Physiology of the Nervous System Medical Mission to the USSR, 10/18-12/13 1958, 6.
[70] NARA, RG 514, Box 6, Folder: Report of the Second Cardiovascular Mission to the Soviet Union, September 21-October 16, 1964, 47.
[71] Ibid.
[72] NARA, RG 443, Box 144, Folder: INTL 4-1 US-USSR, 1956-1969, "US-Soviet Health Exchange, January 21 1965."

> … we have long wondered about the institutional form in which the further expansion of medical research in this country should take place. We have contemplated the development of 'institute' mechanisms either as a part of or separate from the academic scene. The great diversity in the use of this institutional form in the USSR may provide information contributory to our further study of such mechanisms.[73]

On the part of most American medical researchers who traveled to the USSR under the auspices of these exchanges, however, increasing familiarity bred contempt. While the American reports from the 1960s seldom contain harsh assessments of individual Soviet scientists, tending to absolve them of personal responsibility for the state of their work, they frequently note the relative poverty of Soviet laboratories and convey a sense of disappointment with and sometimes contempt for the research. Assessments range from cautiously critical, such as the report of Dr. David Ashler, a virologist who spent over a year in the Soviet Union beginning in 1969 and noted that although "most of the Soviets with whom I worked were sincere and enthusiastic researchers …their laboratories did not seem to be as productive as comparable laboratories in the USA.;"[74] to dismissive, as in the report of the 1959 Radiobiology Mission which concluded that "some of the work seems very good, some very bad, and most of it quite pedestrian;"[75] to downright contemptuous, as in the report of Dr. Robert Van Citters, who noted that the Soviets still relied on research methods that have been "largely discarded in this country for approximately twenty years"[76] and that there was "nothing in their diagnostic or therapeutic facilities which could be interpreted as new or

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[73] Ibid.
[74] NARA, RG 514, Box 7, Folder: Asher, Dr. David M.
[75] NARA, RG 514, Box 10, Folder:Radiobiology Mission Report, October-Nov 1959,18.
[76] NARA, RG 514, Box 9, Folder: Dr. Robert Van Citters, 33.

exciting" and "their approach …resembled much of what goes on in this country, or rather what had gone on in this country five to eight years ago."[77]

Despite the prevalence of such attitudes among American scientists returning from the USSR, by 1970 HEW and Department of State officials concluded that although the exchanges were not "necessarily producing substantive benefits to the American scientific community," the reciprocal visits and the occasional joint meetings were sufficiently valuable to warrant an expansion of the exchange. [78] Noting that the "repetitive nature" of the exchange activities had reduced the visits to "stereotyped, almost sight-seeing expeditions with little or no meaningful long-term professional collaboration among the physicians and scientists participating" and that this had "taken a toll in interest and support for the program among the senior members of the medical communities of both countries,"[79] HEW staff proposed "altering the structure of the program to make it more scientifically rewarding and productive."[80] The proposal, modeled on the US-Japan Cooperative Medical Science Program, provided for a small joint medical policy committee responsible for agreeing on matters of mutual interest and setting the general program structure. The Joint Committee would meet annually to set a comprehensive agenda for cooperation. The exchange of delegations and individual scientists would continue, "but now would be within an organized pattern having more specific objectives than merely first hand, one-time observations."[81] The proposal found

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[77] Ibid., 21.
[78] "Staff Paper: US-USSR Program in Health," NARA, RG 443, Box 144, Folder: INTL 4-1 US-USSR 1971, 1.
[79] Ibid., 2.
[80] Ibid., 3.
[81] Ibid., 5.

support among US policymakers and was presented to MinZdrav officials by the Surgeon General, Jesse Steinfeld.

The obvious question, of course, is why. Why go to the trouble of negotiating with the Soviets and the expense of funding an expanded program if the exchange was thought to yield no substantive scientific benefit? One major motivating factor for this move towards increasing cooperation was, paradoxically, the Cold War itself. As Mark Field, a prominent analyst of Soviet medicine, articulated in his 1967 book *Soviet Socialized Medicine: An introduction,*

> Soviet socialized medicine must …be considered an important and integral component of [the Soviet] challenge… This challenge is …not limited to the political, economic, or even military spheres; it is also part and parcel of Soviet propaganda and of its claim of having, among other things, pioneered and developed an advanced form of 'socialized' health service unique in many of its features and possible only under Soviet conditions. The Soviet regime thus seeks to evoke, among its own people, an attitude of gratitude toward itself as the fountainhead of progress and the organizer of medical care; and, by inference perhaps more than by direct statements, the regime tells the people of other countries (particularly the former colonial nations) that only the adoption of a Soviet or 'socialistic' form of government will make it possible for them to provide for their health needs.[82]

That is, whatever the shortcomings of Soviet medicine, it was an important weapon in the Cold War. While the U.S. may not have been afraid of the effects of Soviet propaganda at home, it could not dismiss the dangers of such propaganda abroad, particularly in the developing world to which the Soviets were offering an alternative model of development.

Thus, American scientists approached the exchanges with their Soviet counterparts with an analytical framework that conceptualized Soviet biomedical science

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[82] Field, *Soviet Socialized Medicine,* vii.

as fundamentally backward—constrained by utilitarian goals that limited the creativity of research, stifled by a rigid bureaucracy that stripped scientists of initiative and erected artificial barriers between biomedical science and practice, and prevented by an oppressive government from freely participating in global biomedical knowledge production. Although some quality research programs were acknowledged to exist, they were perceived as anomalies within an otherwise clearly inferior system. While American observers thought that the inferiority of Soviet science was indisputable, they could not deny their Soviet colleagues a role in the enterprise of global biomedical knowledge production. The Soviets' place in this enterprise was guaranteed mainly by the political threat posed by the Soviet Union as an alternative model of development for newly independent nations.

This conception of Soviet biomedical science—as a research enterprise shackled by an overbearing bureaucracy and sapped of creative energy by an intrusive state that deprived its scientists of their agency and initiative—is, as the next section attempts to demonstrate, the distortion produced by the Cold War analytical frame. Although Soviet scientists undoubtedly struggled with bureaucratic hurdles and the state clearly did intrude on some aspects of the scientists' daily activities (notably, though not exclusively, on the scientists' ability to travel abroad), focusing on these constraints alone obscures the multifaceted nature of the Soviet biomedical research enterprise. The next section offers a corrective to this interpretation.

Searching for common ground

The State Department was only partly accurate in its assessment of the Soviet agenda for the exchanges. Obtaining useful scientific and technical information and

advertising the achievements of Soviet science were certainly important objectives.[83] They were, in some sense, the traditional goals of Soviet foreign scientific and technical policy[84] and consequently were also the objectives that Soviet planners and scientists at all levels and across all disciplines placed rhetorical emphasis on in virtually every discussion of international contacts.[85] But by the 1970s the Soviet biomedical establishment had developed a much broader policy agenda, which was equally informed by the Cold War lens and of which these objectives were only a part.

Active efforts to reintegrate Soviet medicine into the international biomedical community resumed with Stalin's death.[86] Already in 1953 the USSR joined its first international health organization, adding dozens of such memberships in the subsequent decades.[87] Through the 1960s, Soviet entanglements on the international arena continued to expand with the establishment of numerous bilateral exchanges with both socialist and capitalist countries, as well as through aid programs in the developing world. The responsibility for these contacts rested with MinZdrav, which, through its Division of

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[83] Reconstructing the Soviet viewpoint on these exchanges presents a considerable challenge for the historian. While Soviet record-keeping was usually meticulous, some records did not survive the political and economic crises that surrounded the collapse of the USSR. Those that did survive are not easy to locate. Because the exchanges involved a number of government agencies and academic institutions, records are scattered throughout many archives in Moscow and beyond. Finally, the Russian government has, in recent years, reclassified a large number of Soviet-era records pertaining to foreign relations. I was, however, able to locate a large number of records both pertaining specifically to the Soviet-American exchanges and to Soviet foreign relations in medical science more generally. I was also able to interview a number of the participants—both rank and file researchers and high-ranking decision makers. Finally, there are a number of publications on the subject from the period.

[84] For a discussion of the international dimensions of early Soviet science policy see Nikolai Krementsov, *International Science Between the World Wars: The Case of Genetics* (New York: Routledge, 2005). Also Gross Solomon, *Doing Medicine.*

[85] For an excellent account of the role of rhetoric in Soviet science, see Gerovitch, *From Newspeak to Cyberspeak.*

[86] In fact, it's possible that they began even earlier. See V.V. Venediktov, *Ocherki Sistemnoi Teorii I Strategii Zdravoohraneniia*, (Moscow: 2008).

[87] D.D. Venediktov, *Mezhdunarodnie problemy zdravoohraneniia* (Moscow: Meditsina, 1977).

Foreign Affairs, delegated a part of their administration to the Soviet Academy of Medical Sciences (which in turn did the same to its institutes).[88]

Beginning in 1965, the Deputy Minister for Foreign Affairs was Dmitrii Dmitrievich Venediktov. A surgeon by training, Venediktov came to MinZdrav very early in his career—in fact, he completed his post-graduate training in surgery while already in MinZdrav's employ, where from 1952 to 1962 he headed the Information Sector of the Department of External Affairs. Subsequently he spent three years in New York as the medical advisor of the USSR mission to the UN, and when his teacher and mentor B.V. Petrovsky (a very prominent surgeon) became the Minister of Health, he returned to MinZdrav—this time as the Deputy Minister of Foreign Affairs—where he remained until the end of Petrovsky's tenure as minister in 1981.

A proponent of systems theory, Venediktov saw medicine and healthcare as global entities, and argued that the development of "international healthcare" was an important task of national institutions, since threats to human health respected no national boundaries and no single national system could hope to solve them alone.[89] International cooperation—both through bilateral agreements such as the one the Americans were offering (the USSR already signed several such agreements by 1970) and through international organizations such as the WHO—was the way to achieve this goal.

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[88] Though the first Soviet-American agreements (which included provisions for biomedical exchanges but did not focus on them exclusively) were signed by the head of the Government Committee of the Council of Ministers for cultural contacts, they were administered by MinZdrav, and eventually such agreements began to be concluded there as well. See D.D. Venediktov, *Sovetsko-Amerikanskoe sotrudnichestvo v oblasti zdravoohraneniia*, (Moscow: Meditsina 1977).
[89] D.D. Venediktov, *Mezhdunarodnie problemy*.

In his 1977 monograph on *International Health Problems*,[90] Venediktov lays out a four-pronged foreign policy for the USSR with different strategies for cooperation with socialist, capitalist and developing countries, as well as international bodies such as the World Health Organization. These areas of cooperation were based on very different principles. Working with socialist and developing countries was characterized in the monograph as 'selfless assistance' while the cooperation with capitalist countries was based on a strict tit-for-tat exchange aimed at achieving 'mutual benefit.'

The principles underlining Soviet cooperation with the WHO were especially complex and at times contradictory. On the one hand, the WHO was perceived as a global stage on which superpower competition for influence over the developing world took place. On the other hand, it was thought to be the most promising site for developing an effective methodology of international cooperation, as well as the most important tool of this cooperation. Capitalist countries (chief among them being the US), whose leadership in many fields of medical science was acknowledged, were conceptualized both as rivals and as critical partners.

The most valuable contribution that the USSR had to offer in all these areas of international cooperation was the dissemination of the methodology of the Soviet system of health protection—"the most progressive and effective"[91] system in the world. Although the system was not perfect, particularly when it came to the development of medical science and technology, its organizational and methodological superiority were beyond doubt, and it was only a matter of time for this superiority to be recognized

[90] More accurately translated as *International Problems of Health Protection.*
[91] Ibid., 232.

internationally. These ideas about the global scale of biomedicine and the international significance of Soviet system of health protection constituted the Soviet Cold War lens.

The move towards cooperation on the part of US biomedical community was interpreted as a clear sign that this time of recognition was drawing near. As Venediktov asserted in the monograph that highlighted the Soviet-American cooperation, the looming domestic health care crisis in the US and the increasing familiarity with Soviet achievements that came out of the reciprocal visits during the 1960s had "brought on a reevaluation of the assumptions about the superiority of American medicine which had taken root in the USA."[92]

The fact that the Americans took initiative in proposing the exchange meant that they were beginning to recognize the failures of their own system. At a meeting with the Presidium of the Academy of Medical Sciences, Venediktov offered the following assessment of American motives for expanding the exchange:

> … the meaning of this agreement is clear, why Nixon assigns it such importance. It's because the crisis in the area of health protection right now is acute, during the campaign they almost drowned, and the only way out is in health protection.[93]

I want to dwell for a moment on the meaning of the term system of health protection, because this is more than just a matter of semantics—it is what set the Soviet system apart from healthcare and public health systems found in the West. The Soviet system of health protection was from its beginnings "established as a unified system

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[92] D.D. Venediktov, *Sovetsko-Amerikanskoe Sotrudnichestvo*, 74.
[93] Protokoly #19,20, postanovleniia, stenogrammy zasedanii Prezidiuma AMN SSSR i materially k nim, Archive of the Russian Academy of Medical Sciences (hereafter RAMN) F 9120, Op 2, D 6359.

combining the preventive (prophylactic) and curative systems under one administration."[94]

The Soviet definition of the concept of prophylaxis requires explanation as well. Although in daily usage the term was pretty much equivalent to the English word 'prevention,' it also had certain distinctive connotations. According to Marxism-Leninism, true prophylaxis was a function of the socialist order—the organization of public life in a way that maximized the health and wellbeing of the people. Accordingly, pursuing prophylactic goals required a planned, multi-layered system that could combine economic, social and public health initiatives aimed at improving working and living conditions, promoting a healthy lifestyle and raising public awareness and support, and preventing illness and injury.[95] This understanding of prophylaxis was taught in medical schools, stressed in professional publications, and dwelled upon in numerous methodological seminars, which were a fixture of professional life at every level of the biomedical research enterprise. To what extent these Marxist-Leninist connotations were internalized by individual actors who comprised the system of health protection is an unanswerable question, but that they were a major component of the Cold War epistemology and as such had an impact on the organization and function of the system of health protection cannot be denied.

The system of health protection had three main goals:

> …to develop medical science in order to understand the human organism and its complex relationship to the environment, as well as the possible causes and mechanisms of disease; learning to prevent illness and to use for individual and public prophylaxis all available

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[94] Odin Anderson, "A review and impressions of the health services in the USSR, the month of September, 1972, Moscow and Leningrad," NARA RG 514, Box 6, Folder: Anderson, Odin W (Daniels).
[95] M.I. Barsukov, ed., *Ocherki Istorii Zdravoohraneniia SSSR: 1917-1956*, (Moskva: Medgiz, 1957).

> knowledge, methods and tools; and finally, in case of the appearance of illness provide everyone with universally available and qualified medical help in a timely manner. [96]

Of course, these goals were not fully realized in practice. This does not, however, diminish their practical importance in having a very direct impact on the structure and function of the system. As Odin Anderson, the director of the Center for Health Administration Studies at the University of Chicago, observed after returning from his trip to the USSR in 1972, "although there is a division of labor…the system appears to be so interlocked by preventive and curative concepts in the same personnel that it is difficult to differentiate between the two types of activities operationally and financially."[97] That is, although the development of biomedical science was an important goal, the primary concern of the system was the development of therapeutic medicine. The fact that the upper echelons of both MinZdrav *and* the Academy of Medical Sciences were dominated by clinicians (usually surgeons) supports this interpretation and contradicts the American perception noted earlier that therapeutic medicine was lower in status than basic research.

Thus Soviet medical science was an integral part of the larger system of health protection that blended curative and preventive concerns. This system, in turn, was an *integral part of the Soviet state*.[98] This point requires emphasis as well, as it is one that is frequently lost sight of by western analysts and observers, who often conclude that because the system of health protection was the subject of planning, it was strictly

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[96] Venediktov, *Sovetsko-Amerikanskoe Sotrudnichestvo.*
[97] Anderson ibid., 10.
[98] In fact, Russia had a long history of government regulation and even provision of medical care—the first administrative structure charged with regulatory functions, the *Aptekarskii prikaz*, was instituted in 1581, and its functions and structure expanded and evolved in response to the needs of the state (see Mirskii *Meditsina Rossii*). The Soviet innovation consisted in reconceptualizing healthcare provision as a key function and obligation of the government.

subordinate to the state and all its employees were required to do the state's bidding.[99] This assumption does not stand up when we look closely at Soviet practices—although the elite planners and administrators of the health protection system clearly did not occupy the highest positions in the state hierarchy, they were nonetheless statesmen in their own right, with considerable power to set their own agenda (though, of course, they were not always assured of being able to carry it out successfully).

This picture of the Soviet healthcare system differs in important ways from the one that emerges out of the American Cold War analytical framework. The isolation of Soviet biomedicine from the rest of the world was a fairly short-lived phenomenon—when the 1956 American delegation landed in Leningrad, the MinZdrav had already been actively re-activating scientific contacts and expanding its entanglements abroad for several years, and by the time the comprehensive exchange was proposed had a sophisticated foreign policy and was providing opportunities for individual Soviet scientists to establish contacts and build reputations abroad. The system was effectively *unified*, intertwining curative and preventive concerns, not effectively *centralized*. Although Soviet scientific planners placed a great deal of rhetorical emphasis on the importance of planning and centralization to the effective functioning of the system of health protection, in fact the system was very fragmented and the practices of planning and control flexible and porous, as will be demonstrated below. Finally, the practitioners of Soviet biomedicine were not mere cogs in the machine. And although bureaucratic

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[99] See Field *Soviet Socialized Medicine* and also Mark G. Field, *Doctor and Patient in Soviet Russia* (Cambridge, MA: Harvard University Press, 1957). Also Ryan, *Doctors and the State*. This interpretation is also consistent with the conception of the relationship of Soviet scientists to the state found in much of the more general historiography of Soviet science (see Kojevnikov, "Phenomenon of Soviet Science," for a summary of this conception), although new interpretations have been put forward (for example, Krementsov, *Stalinist Science*).

hurdles that had to be negotiated on a daily basis were considerable (and some, such as the party organs that granted or withheld permission to travel abroad were almost impossible to circumvent), individual scientists had a variety of resources to draw on in order to exercise control over their own work.

The mechanism of the expansion of the Soviet-American exchange illustrates these points. When Steinfeld approached him with a proposal to expand the sporadic exchanges into a more systematic cooperative program, Venediktov readily agreed to personally work towards the realization of these plans. Following the initial discussions in Moscow, the two sides met again in Geneva in May of 1971 on neutral ground, at a meeting of the World Health Assembly. In order to give the cooperation a fixed form without entering into an actual agreement, a novel diplomatic dance was invented—the Soviet Minister of Health and the Secretary of Health, Education and Welfare would exchange letters expressing their readiness for mutual cooperation.[100] After working out the text (in itself a cooperative process that took nearly a year), the letters were finally exchanged by Petrovskii and Secretary Richardson on February 11, 1972, and in March the first session of the US-USSR Joint Committee for Health Cooperation convened in Moscow to lay out the preliminary goals and agree on the mechanisms of the exchange. The exchange got the official blessing from the highest level of both the Soviet and the American government in May of 1972, when Richard Nixon and Leonid Brezhnev signed the Health Cooperation Agreement in Moscow.

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[100] It seems that the reason for inventing this new diplomatic form was precisely to preserve a space for moving the agreement to still higher levels of government. By getting the final stamp of approval from Brezhnev and Nixon themselves, MinZdrav and HEW officials were putting themselves in a much stronger position when it came to securing funding for the joint ventures.

It seems that throughout this convoluted process, the MinZdrav officials were acting on their own initiative. At least I have been able to find no evidence to the contrary. Although it was, of course, necessary to secure if not the blessing, then at least the non-interference of the Communist Party, as well as the cooperation of relevant state structures such as the Ministry of Foreign Affairs and the Council of Ministers, this doesn't seem to have posed a major problem. The only reference to the exchange I was able to find in the Party archives is dated late April 1972 (a month after the exchange had gotten under way), and consists of reports put together by Petrovsky and Venediktov regarding the prospects for Soviet-American cooperation, with a particular emphasis on oncology. The reports were put together for Kosygin,[101] who forwarded them to the Central Committee for review. On May 3, 1972—after the exchange had already effectively been under way for several months—the Division of Science & Education and the Division of Propaganda of the Central Committee rubber-stamped the reports, adding only that it would be nice if the US would, as part of the exchange, supply Soviet biomedical institutions with equipment and reagents (either for free or on a commercial basis).[102]

To return to the main line of argument: what were the driving considerations for the Soviets in entering into this exchange? Expanding access to scientific and technical developments in the US was, as previously mentioned, obviously a major consideration. Venediktov acknowledged as much in his monograph on the exchange when he said that "the acquaintance with the achievements of medical science and medical industry in the USA, with the construction and utilization of large hospital complexes, the uses of

[101] Chairman of the Council of Ministers and thus, de jure, the head of the Soviet government.
[102] Russian State Archive of Recent History (RGANI), F 5, Op 64, D 121.

computers in medicine, etc., has shown Soviet specialists the potential of wider cooperation."[103] Just as important, however, was the conviction that such cooperation was objectively necessary given the global nature of medical problems and the increasing complexity and cost of medical science. As the countries with the greatest scientific potential, the US and USSR had both a moral obligation and a very practical incentive to work together. Finally, just as the State Department hoped that ensuring a normal flow of information between the two countries would have an impact (however small) on the Soviet system, the Soviets also hoped that a first-hand knowledge of their system of health protection would give the US a push in the right direction—a particularly promising prospect given the belief so often expressed by Venediktov that the US was starting to understand the error of its ways when it came to health and medicine.

Soviet biomedical scientists were enthusiastic about the exchange. By the 1970s, American science had been firmly cemented as the benchmark for many Soviet biomedical disciplines and scientific endeavors.[104] One would be hard pressed to find a single scientific meeting—whether at the level of a laboratory, a scientific council meeting of a particular institute, or the Soviet Academy of Medical Sciences—where no reference is made to scientific practices either in the United States explicitly or 'in the West' more generally.[105] The opportunity to get not only a direct glimpse of how one's

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[103] Venediktov, *Sovetsko-Amerikanskoe sotrudnichestvo,* 75.

[104] How and when this happened is a question beyond the scope of this dissertation, but I suspect that the origin of this development lies in the period just after the war. In addition to the atomic bomb, the American research efforts during the war also yielded a method for the mass production of penicillin, and the development of the Salk vaccine for polio in the early 1950s further bolstered American leadership in biomedical research.

[105] There are rich archival resources on such meetings. The Soviets not only kept careful track of meeting attendance and agendas, but very often transcribed the proceedings (though the level of detail in the transcripts varies). An analysis of Soviet record keeping practices is beyond the scope of this project, but the presence of these resources is worth noting.

science was pursued in the States, but to actually exchange materials, equipment and protocols with American colleagues and even pursue joint projects was tremendously exciting.

An equally important factor in generating enthusiam was the fact that the participation in the exchange opened the door to the possibility of securing more of the perpetually scarce resources needed to carry out research—equipment, funding, reagents and trips abroad. Although in theory Soviet planning was supposed to, as the slogan goes, give to each according to his needs and take from each according to his abilities, in practice the competition to secure the material basis necessary for one's work was as much a part of doing science in the Soviet Union as it was in the United States. Just how important the exchange was in this respect is illustrated by a meeting of the Presidium of the Academy of Medical Sciences with MinZdrav officials on June 21, 1972. The purpose of the meeting was to organize the scientific work of the exchange, and one of the most important points on the agenda was assessing the resources already available within the Academy system for the exchange and figuring out what additional resources would have to be provided by the government. As one of the MinZdrav officials at the meeting pointed out, this was crucial because "now this is getting much more attention then in the past [because] the Americans are actively engaged in carrying out this agreement… and we need to be fully armed so as not to land face first in the mud."[106] There were rumors that Nixon had gone to Congress to request substantial funds for the exchange, and the Council of Ministers, not wanting to be outdone, had charged the MinZdrav with providing a proposal of the resources that would be needed.

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[106] RAMN, F 9120, Op 2, D 6359.

The promise of big spoils unleashed an argument on the Presidium's floor. Representatives of research areas that did not make it into the initial agreement (which at the outset covered only cardiology, oncology and environmental health) argued that their institutes should also be allowed to participate. Meanwhile the directors of the Academy's cardiology and oncology institutes—the institutes that would be coordinating cooperation with Americans in these areas of research—had already put together detailed research plans that included orders for equipment and other materials, both for their own institutions and for the other Soviet institutes that would be participating in the exchange under their direction. This ruffled the feathers of Academy officials who took issue with being bypassed in this manner. V.D. Timakov, the president of the Academy, criticized the plans put together by institute directors on the grounds that their interests were too narrow and their requests too modest, and spoke in support of his colleagues whose specialties had been left out of the exchange, arguing that Soviet medical science would be better served if the exchange was not restricted only to those disciplines in which the Soviet Union was already strong but included weaker disciplines such as genetics.

This meeting illustrates not only the importance of the Soviet-American exchange as a resource for obtaining additional funding, but also the flexibility of the system of scientific planning. In proceeding with the exchange, the MinZdrav officials went directly to academic institutes, bypassing the Presidium of the Academy, which had jurisdiction over such decisions, and then pushing the Presidium to rubberstamp the directors' plans. It also underscores the technocratic nature of this system, illustrating that the people responsible for the planning and administration of biomedical research were the physicians and the scientists themselves (even if sometimes, as in the case of

Venediktov, their scientific or medical practice ended up taking a back seat to administrative careers). This does not mean, of course, that all scientific and medical personnel enjoyed positions of power, nor does it negate the crucial role of political factors and the structural limitations of the bureaucratic machinery of planning and administration. What it does mean is that scientific and political concerns were inextricably intertwined in the minds and practices of Soviet scientific planners and scientists themselves as they made decisions about how—and whether—to work with the Americans.

The Cold War was an important resource in formulating these concerns, as illustrated by another meeting at the Academy of Medical Sciences at which Soviet scientists and administrators weighed political and scientific factors in their decisions about the exchange, as well as some of the misgivings they had about cooperating with their rivals. This was a meeting convened by Venediktov with Academy officials and scientists, among them Nikolai Nikolayevich Blokhin, a former President of the Academy, the coordinator of the Soviet-American cooperation in oncology and director of the Institute of Experimental and Clinical Oncology of the Academy of Medical Sciences; V. Zhdanov, coordinator for the third problem area of the oncology exchange focused on Leukemia and Tumor Viruses of Animals and Man and director of the Ivanovskii Institute of Virology; and Boris Arkad'evich Lapin, the director of the Institute of Experimental Pathology and Therapy in Sukhumi. The subject of the meeting, held in the at once grand and dreary headquarters of the Academy just a few blocks from the Kremlin on the last day of October 1972, was a leukemia virus that had been isolated

by Lapin's group in Sukhumi and was believed to be the first human leukemia virus isolated anywhere in the world.

Venediktov called the meeting as a response to Lapin's letter to discuss whether or not to pass samples of this virus to American collaborators and, if a decision to give the Americans the virus were reached, how to structure joint efforts on the virus. A group of American cancer virologists was expected in Moscow in November, and Nixon officially communicated to the Soviet Minister of Health the American interest in the Soviet virus and the American side's willingness to provide the Soviets with any of the American viruses that Soviet scientists might be interested in, without exception.

The question of whether or not to pass on the virus was an important one because, as Venediktov noted, the virus was thought to be "one of the biggest accomplishments of Soviet science that we have in reserve."[107] Lapin expressed several fears about passing on the virus. The first was that the Americans would find a way to isolate the virus under another name—that is, simply steal the credit. Another fear was that, not having any experience of working with the virus, they would 'lose' it—that is, fail to replicate Lapin's work, thereby discrediting it and inflicting 'moral damage' on Soviet science. Finally, given the technical superiority of American laboratories, it was possible that the Americans would quickly advance work on the virus, producing proof that it did indeed cause leukemia in humans and start work on a vaccine, leaving the Soviets in the dust.

Ultimately, voices in favor of transferring the virus to the Americans prevailed. Supporters of this decision argued that Lapin's work was beyond reproach, and that the fear that his findings would not be confirmed was entirely groundless. More important,

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[107] RAMN, F 9120, Op 2, D 6368, 2.

given the fact that all the work done on this virus so far had already been published, it was difficult to find a legitimate excuse for refusal. Holding out on this virus, when the Americans had been so forthcoming in fulfilling Soviet requests, would compromise the spirit of cooperation. Even if the Americans took the research on the virus further, "this could only be more advantageous for science … because in the grand scheme of things this will still be a Soviet virus."[108] Finally as N.N. Blokhin, perhaps the loudest voice in support of transferring the virus, argued, "if we are going to carry on this work [of cooperation] we need to do it on a broad front and not give the Americans the impression that we want to get something from them but not give them anything."[109]

One other thing that stands out in these meetings is their banality. Delving into the details of squabbles over funding and the venting of professional insecurities makes it possible to momentarily lose sight of the fact that one is reading an account of Soviet science. To be sure, the context is never far from the surface—it bubbles up in the confidence that American overtures are a sign of the growing global recognition of the system of health protection, in the self-conscious admiration of American technological prowess, and in the can-do attitude of the rhetoric of catching up and overtaking. But the important point is that the institutional structures do not figure in quotidian practices as a looming outside presence—the power of these institutions is vested in the individuals who comprise them as much if not more than in the institutional structures themselves.

To sum up, the Soviets took the American proposal to expand the exchange in stride as a clear sign that Soviet biomedical science, and the system of health protection of which it was a part, were finally getting the global recognition they deserved and that

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[108] Ibid., 7.
[109] Ibid., 4.

their foreign medical policy was on the right track. At the same time they admired the scientific and technical achievements of American medicine, acknowledging its superiority in a number of areas, were acutely self-conscious about being behind and eager to catch up. This acknowledgement of the achievements of American scientists did not, however, shake their conviction that their system was ultimately superior. Soviet science was relatively behind only because it was starting from a position of disadvantage—it was a young system, and a major portion of its brief history had been characterized by severe hardships. This disadvantage would, however, be quickly overcome thanks to the inherent organizational and methodological superiority of the system.

The unraveling of cooperation

Although both sides had some misgivings about the exchange and were unable to fully overcome a certain level of mistrust of the other, following the exchange of letters by Petrovskii and Richardson the cooperation got off to an active start and quickly gathered momentum. Joint meetings and reciprocal visits followed each other in rapid succession. Taking the Cooperation in Malignant Neoplasms as an example, already at the first session of the Joint Committee in March of 1972 a work plan targeting four problem areas—chemotherapy, immunotherapy, cancer virology and genetics of tumor cells—was agreed on by Carl Baker and N. Blokhin.[110] By the end of June of the same year a delegation of American chemotherapists headed by Gordon Zubrod was in

[110] Blokhin remained the Soviet coordinator for the duration of the exchange, while already by the time of the second meeting of the Joint Committee Frank Rauscher took over for Carl Baker. This was a general trend—Soviet personnel participating in the exchange was quite stable, while on the American side there was a high degree of turnover.

Moscow to hammer out a detailed work plan for their problem area, and in October N.I. Perevodchikova, head of the chemotherapy department of the Institute of Experimental and Clinical Oncology, went to the US to lay the ground-work for beginning joint clinical trials on compounds the two sides had agreed to exchange in June. Also in the fall of that year James Holland, a prominent American oncologist with extensive expertise in clinical trials of anti-cancer drugs arrived at the IECO for a nine month residency to facilitate the joint trials. The other groups kept up a similar schedule and by 1977, when the exchange came up for renewal, the Malignant Neoplasms group could boast numerous joint articles and monographs that grew out of cooperative projects. The exchange as a whole had also expanded to include several other areas—arthritis, infectious diseases and artificial heart research.[111] There were other, less measurable results as well. Personal networks were expanded and friendships—sometimes close, life-long friendships—formed.

Nonetheless when the program came up for renewal in 1977, its continuation was far from certain. The era of détente was rapidly coming to an end, and rising tensions were making it harder to maintain the spirit of cooperation. American scientists were becoming increasingly indignant over the plight of Soviet dissidents and some, such as Robert Goldberger, chief of NCI's biochemistry laboratory, even contemplated taking direct political action. In a memo to the Deputy Director for Science at the NIH, dated April 11, 1977, Goldberger was informing NIH officials of his intent to travel to the USSR as a private citizen to participate in a symposium organized by Soviet 'refuseniks'—"mostly Jewish scientists who have been fired from their jobs because they

[111] NARA RG 443, Box 145, Folder: INTL 4-1 US-USSR, 1977.

or members of their families have applied for visas to emigrate from the Soviet Union."[112] Such incidents created resentment and strained interpersonal relationships.

Despite the growing tension, American coordinators of the exchange publicly defended the cooperation. In January of 1977 several of them—Dr. Joseph Saunders (NCI), Dr. John Decker (NIMS), Dr. Paul Ehrlich (HEW) and Dr. Michael DeBakey (a prominent cardiac surgeon with a long history of working with the Soviets)—appeared on the MacNeil/Lehrer Report to face intense questioning about the value and politics of the exchange (I found a translated transcript of the program in the archives of one of the Academy Institutes). MacNeil started the program with the observation that the American government under Gerald Ford had become disenchanted with the policies of détente, and began to question the guests on what, precisely, the U.S. was gaining in the course of this cooperation that would have been impossible to achieve by other means and how Soviet science compared to American science in the areas under discussion.

Although the scientists could cite some examples of advances that the Soviets brought to the table—Joseph Saunders pointed to the Soviet anti-cancer drug Ftorafur, which, although analogous to an American drug, had fewer side-effects and was better tolerated by patients, and DeBakey discussed Soviet contributions to the engineering of an artificial heart—the preferred answer to this set of questions was to emphasize the ways in which Soviet research complemented American research programs, contributing to the greater store of human knowledge on which both countries could then freely draw. While the scientists were unanimous in their defense of the exchange, managing to effectively parry even the treacherous political question of whether it is possible to

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[112] NARA RG 443 Box 145 Folder: INTL 4-1 US-USSR, 1977.

cooperate with one's rival, the transcript of the program conveys the sense that both the hosts and the guests on the program shared the perception that America was acting as a donor in the exchange. This is especially clear in the way Ehrlich and the others responded to the question of whether it wasn't better to spend the money that was going to the exchange on domestic research. Every respondent emphasized the insignificance of the sum spent on the cooperation compared to what was being spent on domestic research, and only DeBakey pointed out that the money wasn't simply being donated, but was yielding a return on American investment (although the return that DeBakey saw was the leverage the program provided American cardiologists over the Soviet research agenda).

For their part, the Soviets also continuously evaluated what they were getting out of the exchange. The evaluation process took place on every level of the scientific bureaucracy—from the Academy Institutes, which included a section on their international relations in every annual report to the Academy; to the Academy's divisions, which compiled the data from the institutes under their supervision and discussed the results; to the Presidium of the Academy, which did the same for the Academy's three divisions—and examined not only the Soviet-American exchange, but all the international collaboration undertaken by scientists employed in the Academy's institutes. The question that was asked to gauge the value of international cooperation, however, was not 'what did we learn from them that we didn't know already' but 'what are we doing with the information obtained in the course of cooperation?' Trips abroad were deemed 'effective' when scientists came back with concrete proposals (anything

from novel protocols to new equipment) that could be implemented both within their own institutes and beyond.

It was widely understood, however, that this was a very problematic metric (as were the statistical metrics of number of trips, projects, man-weeks, etc.), and that "real effectiveness is felt just like that, without mathematical formulas."[113] This was because one could bring back not only specific techniques and protocols, but also "creative energy" that can't be "formatted" or "reduced to indicators in a report."[114] The value of these exchanges was, to a large extent, understood to be intangible, and thus "it can't be said that if a person hasn't formulated anything, that means he brought nothing back, that the trip didn't reach its goal."[115]

There was a further problem with measuring the value of international cooperation in this way—particularly when applied to the Soviet-American exchange. As one of the academicians participating in the Division of Clinical Medicine's discussion of international relations in 1977 noted, in reporting the results of the cooperation it was important not to cast Soviet scientists as "schoolboys":

> They often demand of us to say what the cooperation with the U.S.A. has given us. I think that before we answer that question, we need to think it through. Sometimes we write things that are far from the truth. We are ready for particular surgeries, we know about them, we saw them and we know how to do them, but this isn't a result of just the cooperation. In no case. This is a result of our extensive training and preparation, but we are forced to write that we did this or that because we worked with the USA.[116]

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[113] RAMN, F 9120, Op 2, D 6621, 15.
[114] Ibid., 234.
[115] Ibid.
[116] RAMN, F 9120, Op 2, D 7420, 67.

That is, demanding that the value of cooperation be expressed in terms of concrete knowledge gained and techniques learned often led to an exaggeration of the benefits of the exchange and a corresponding under-appreciation for the independent achievements of Soviet science. The picture of the cooperation produced in this way was deemed inaccurate, because the projects undertaken in the course of the cooperation were "not so much cooperative as parallel research."[117] Carrying on truly cooperative research "over such a distance" and "in two countries with different orders and approaches" was a difficult task, and the actual value of the exchange was to be found in the contacts between Soviet and American scientists.[118] Although these contacts didn't necessarily yield brand new information, they provided Soviet scientists with a broader picture of their field, rounding out their education.[119]

In the event, the agreement was renewed for another five years in May of 1977, but the wind had gone from its sails and, as superpower relations continued to worsen, exchange activities slowly petered out until the agreement was finally allowed to expire despite the active attempts on the part of both American and Soviet coordinators to keep it going. The expiration of the intergovernmental agreement did not mean an end to scientific exchange and cooperation—Soviet and American scientists in many cases maintained the contacts established in the course of the exchange. It only meant that attempts to work together were now more sporadic and less structured, and that the logistics of working together became more complicated.

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[117] Ibid., 66.
[118] Ibid.
[119] RAMN, F 9120, Op 2, D 6621, 230.

How are we to interpret the story of this attempt at cooperation by the rival superpowers? On the one hand, it seems like a straightforward and familiar story of science being thwarted by politics. But that kind of interpretation is too simplistic, obscuring both the extent to which science and politics were intertwined in the minds of the exchange participants, and the many different levels on which the Soviets and the Americans misunderstood each other. Urie Bronfenbrenner, a social psychologist who went to the USSR in 1960 as part of these exchanges, captured the essence of the problem in his report of the trip. Admitting that his "Soviet journey was a deeply disturbing experience," Bronfenbrenner explained that "what frightened me was not so much the facts of Soviet reality as the discrepancy between the real and the perceived."[120] The reason this was disturbing was that:

> …the Russian's distorted picture of us was curiously similar to our view of them—a mirror image. But of course our image was real. Or could it be that our views too were distorted and irrational—a mirror image in a twisted glass?
>
> It was—and is—a frightening prospect. For if such reciprocal distortion exists, it is a psychological phenomenon without parallel in the gravity of its consequences.[121]

While Bronfenbrenner's observation of the mutual misunderstanding between the Soviet and American side is accurate, the phenomenon he is describing is not so much psychological as epistemic—that is, its origins are to be sought not in the mysteries of human psychological constitution but in the mundane characteristics of the epistemic resources the actors from the period employed in developing their understanding of the world around them. The Cold War, with its assumptions of mutual exclusivity and inherent antagonism, was a key epistemic resource employed by the exchange

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[120] NARA, RG 514, Box 10, Folder: Urie Bronfenbrenner, 40.
[121] Ibid.

participants in formulating their understanding of each other—the crooked mirror from which scientists on both sides derived their picture of the world.

In this chapter, I focused in the way that the crooked mirror of the Cold War distorted American perceptions of the institutional context of Soviet science. American scientists perceived Soviet biomedicine as defined by its institutional context—the unwieldy planning bureaucracy, the rigid hierarchy and the meddling state were understood to hold a monopoly on power, depriving the science of its creative energy. In the next chapter, I make the case that the quotidian practices of Soviet science were perceived no more clearly than its institutional context.

CHAPTER 2: THE WILD WEST IN THE COMMUNIST EAST

On November 4, 1972, an American family of seven crossed the Soviet-Finnish border in a newly purchased Volkswagen and set out on the long drive to Moscow accompanied only by a light snow and the silent pine forests that lined the mostly empty road. The family was that of Dr. James Holland, who along with his wife, Dr. Jimmie Holland and their five children, ages fifteen, thirteen, eleven, nine and seven, was on his way to Moscow to spend eight months at the Institute of Experimental and Clinical Oncology (IECO)—one of the institutes under the jurisdiction of the Soviet Academy of Medical Sciences and the premier cancer research and treatment facility in the USSR.[122] Dr. Holland, at the time Chief of Medicine at the Roswell Park Memorial Institute and "a leading authority in drug development," was to facilitate the drug exchange program in malignant neoplasms, which was part of the larger Soviet-American healthcare exchange that had begun between the two superpowers earlier that year.[123] His task was to bridge the gap in definitions, standards, and research techniques which made the execution of joint projects difficult.[124]

The journey held many surprises and new impressions, but one of the things that stood out the most for Holland from his first days in Moscow was a passing remark made by Nikolai Nikolaevich Blokhin, the director of the IECO, during Holland's fist day at

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[122] Dr. Jimmie Holland spent her time in Moscow as a staff member of the Institute of Psychiatry of the Academy of Medical Sciences. She detailed her experiences in Moscow and at the Institute in vivid reports back to the NIH.

[123] Memorandum to Assistant Secretary for Health and Scientific Affairs, DHEW, 11 September, 1972, NARA RG 514, Box 6, Folder: Dr. James Holland.

[124] In addition to this appointment, one of the first projects of the exchange was the joint publication of a monograph that came out simultaneously in English and Russian. It detailed the organization and methodology of chemotherapy research in both countries, and covered all stages of drug development from initial screening to phase III clinical trials. The monograph was titled *Methods of Development of New Anticancer Drugs: USAA.-USSR Monograph*, DHEW publication No. (NIH) 76-1037, 1977 and *Sistema sozdaniia protivoopuholevih preparatov v SSSR I SShA,* Moskva: Meditsina, 1977, respectively.

the institute. As Holland reported the remark in one of his progress reports back to the NIH, "Dr. Blokhin emphasized that the Soviet Union was at a state of development comparable to our 'Wild West' of 50 years ago."[125] The remark proved so memorable because, as Dr. Holland recalled in a personal interview more than thirty years later, "no one had ever admitted it before, that it was the Wild West."[126]

Asked whether he did indeed find Soviet medicine of the time to be like the Wild West, Dr. Holland did not hesitate for a second in his response: "Oh, worse!"[127] There was "very little pharmaceutical industry" and consequently a chronic shortage of drugs.[128] In his tours of cancer treatment facilities throughout the Soviet Union, Holland was particularly outraged to discover that

> they didn't have Dactinomycin and Vincristine, which are two important drugs for treating children with cancer, and I thought there was a shortage and people couldn't treat children with a curable cancer because they didn't have drugs, and I thought that was inexcusable.[129]

Holland was also largely unimpressed with Soviet drug development efforts. Although he readily acknowledged Soviet contributions to the world arsenal of chemotherapeutic agents, such as Larionoff's synthesis of the peptide mustards and the drug ftorafur that the Soviets had passed on to the US for clinical trials as part of the Soviet-American exchange in oncology, he found their methods wanting. In particular, he didn't think

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[125] James Holland, "First progress report," NARA RG 514, Box 6, Folder: Dr. James Holland, p.12.
[126] James Holland, personal interview, March 2007.
[127] Ibid.
[128] Ibid.
[129] Ibid.

> they had done a significant realistic clinical trial before I talked to them about randomization and comparison. Everything was a single arm recitation of what they'd encountered.[130]

The problem with this approach was that it rendered all the Soviet results completely "subjective."[131] Holland was not alone in this characterization of Soviet science. Throughout their exchanges with Soviet colleagues during the 1960s and 1970s many American scientists reached for the word "subjective" when attempting to characterize Soviet research practices in their reports. In characterizing Soviet research methods as subjective, American scientists not only cast suspicion on the reliability of the results produced by these methods, but also gave voice to their moral suspicions of mainstream Soviet science, which was thought to be corrupted by pressures from the Soviet state and forced to deviate from the norms that governed scientific research in democratic countries.

It is important to understand the intellectual roots of this characterization. For American chemotherapists at this time, the best method of clinical problem solving was the randomized clinical trial.[132] Pioneered by Bradford Hill of the British Medical Research Council in 1946, this methodology was adopted at the National Cancer Institute in the early 1950s for the multicenter studies of leukemia.[133] The approach was so well received that "by the late 1950s large-scale multicenter trials were perceived to be the

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[130] Ibid.
[131] Ibid.
[132] The randomized clinical trial continues to be the gold standard of clinical research methodology. Although advocates of evidence-based medicine place it below the meta-analysis in terms of reliability, such analysis can only be performed on the data of multiple RCTs.
[133] Holland, along with Gordon Zubrod, championed this method at the NCI. For more on clinical trials in cancer see Kutcher, *Contested Medicine.* For more on Hill Matthews, *Quantification.* For more on the rise of clinical trials in American medicine more generally see Marks, *Progress of Experiment.*

only reliable means of answering clinical questions in cancer therapy."[134] What endowed clinical trials with reliability was their high degree of regimentation as well as "size and randomized structure, which provided the capability of producing unbiased and statistically significant differences between the various treatment options on offer and, thus, answering important clinical questions."[135]

The word 'unbiased' is crucial here. Clinical trials provide access to objective knowledge by taking drug testing out of the realm of therapeutic medicine and transforming doctors and patients into investigators and research subjects, as well as by distancing the producers of medical knowledge from the knowledge produced.[136] Cambrosio et al. have argued that clinical trials have provided a vehicle for the emergence of a new form of objectivity which is specific to western biomedicine—regulatory objectivity. What distinguishes regulatory objectivity from other forms of objectivity is its reliance on collective action. As the authors argue, under the regime of regulatory objectivity "what counts … is not whether or not the results produced by a particular laboratory are true, in some absolute sense, but whether or not they are compatible … with results produced by other laboratories."[137]

In western biomedical discourse of the time, clinical trials were incorporated into a narrative of progress and taken to be both the result and evidence of scientific advance in medicine.[138] Clinical trials occupy this position in western biomedical discourse to this day, although they have been displaced from the apex of the evidence pyramid by the

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[134] Kutcher, *Contested Medicine*, 2.
[135] Ibid., 24.
[136] At least this is the promise of clinical trials. In practice, the process of answering clinical questions via the conduct of RCTs is fraught and complex. See Epstein, *Impure Science* and Fisher, *Medical Research for Hire*.
[137] Cambrosio et. al., "Regulatory objectivity"; 192.
[138] Marks, *Progress of Experiment.*

meta-analysis.[139] Thus for Holland, the failure of his Soviet colleagues to adopt this methodology was a sign of the backwardness of Soviet medicine, and Blokhin's Wild West remark a starkly frank admission to backwardness.[140] In this chapter, I take Holland's characterization of Soviet science as subjective as the starting point of an exploration of Soviet biomedical epistemology as it was instantiated in drug development practices. I argue that this epistemology had several distinct features that were reflected in the organization and methodology of drug development.

At the root of this epistemology was a conception of medicine as a hybrid science—a science in equal parts biological, humanistic and social. The hybrid nature of medical science created room for a plethora of methodological approaches that belonged to one of two distinct realms—the experimental or the clinical. Soviet epistemic commitments were unlike the western notion of objectivity, which presumed a hierarchy of medical evidence in which evidence produced in the clinical setting was more vulnerable to corruption by the biases of clinicians and patients alike and therefore occupied a lower rung than evidence produced in experimental settings, where the application of the scientific method ensured the reliability of the knowledge produced, and where the randomized clinical trial was supposed to erase this disparity by making it possible to apply the same scientific method found in experimental settings in the clinic. Rather the Soviets did not conceptualize either the bench/bedside divide or the variability of the individual as an obstacle to objective knowledge.

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[139] In some sense, the RCT can be described as a precursor to EBM—as an earlier phase of the progressive rationalization of medicine.

[140] While the Wild West is a symbol of lawlessness rather than backwardness in American culture, in Russian the term evokes the absence of civilization rather than law.

I argue that, while Soviet biomedical scientists were also in pursuit of objective medical knowledge, for them the path to such knowledge lay not in striving to collapse the differences between the experiment and the clinic, but through the cultivation of a proper dialectic between the therapeutic and the experimental realms. Producing objective knowledge thus did not entail the creation of distance between the producers of knowledge and the knowledge produced, nor in abstracting individual variation out of clinical trial results. On the contrary, clinical research was understood to properly belong in the realm of therapeutic medicine, and the individual—both the researcher and the patient—had an important role to play in the processes of knowledge production.

In making this argument, I examine Soviet biomedical epistemology on its own terms, analyzing it within the context of the Soviet system of health protection. This analytical move is necessary to break free from the 'Wild West' conception of Soviet biomedicine, and to shed light on facets of the system that have thus far largely escaped analytical scrutiny. I locate this argument in a detailed examination of Soviet drug development practices in oncology. This examination begins with an analysis of idealized depictions of Soviet drug development practices, reading them against their American counterpart. I then look at the material and rhetorical practices of Soviet clinical trials and the rhetoric employed in keeping the clinical and experimental realms separate, and conclude with an examination of what the Soviets thought were the shortcomings of their system.

Idealized depictions of drug development

In his forward to the joint USA-USSR monograph on *Methods of Development of New Anticancer Drugs*, Dr. Frank Rauscher Jr. acknowledged that at the time the exchange in malignant neoplasms began,[141]

> ...there was a major effort in both countries toward the discovery and development of antineoplastic drugs. ... Years of experience existed in both nations in the synthesis of new chemical structures and their evaluation for anticancer activity in a wide range of experimental systems. Extensive pharmacologic and toxicologic testing procedures had been developed in both countries, and there was evidence of a long history of interaction with the regulatory agencies of each nation for ultimate approval of new drugs for clinical trials. Each nation had worked extensively on clinical trial methodology, and both accrued data on a wide variety of compounds that had gone through the entire evaluation system and, subsequently, had demonstrated their clinical value in the cancer patient.[142]

But while there are numerous first hand, scholarly and popular accounts of American efforts in this area, we know very little about Soviet research programs.[143] Early Soviet efforts at anti-cancer drug development date back to the 1920s, when there were attempts made to use the most promising approaches from experimental biology and experimental medicine to uncover compounds that could inhibit tumor growth. The most famous (and infamous) of these efforts was the work of Gregory Ruskin and Nina Kliueva, a husband and wife team of microbiologists who developed a biological

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[141] Malignant neoplasm is the medical term for cancer.

[142] Frank J. Rauscher Jr, 1977. "Forward," *Methods of Development of New Anticancer Drugs: USA-USSR Monograph*, DHEW publication No. (NIH) 76-1037, 1.

[143] On American efforts to fight cancer, see David Cantor "Radium and the Origins of the National Cancer Institute," in Caroline Hannaway (ed.), *Biomedicine in the Twentieth Century: Practices, Policies, and Politics* (Amsterdam: IOS Press, 2008), pp. 95-146; David Cantor, "Cancer Control and Prevention in the Twentieth Century," *Bulletin of the History of Medicine*, 81 (1) Spring 2007, pp. 1-38. For an account focused on anti-cancer drug development, see Jordan Goodman and Vivien Walsh, *The Story of Taxol: Nature and Politics in the Pursuit of an Anti-Cancer Drug* (Cambridge, UK:Cambridge University Press, 2001). Krementsov provides some background on the Soviet programs, but his focus is really elsewhere (see Nikolai Krementsov, *The Cure*).

preparation they termed KR.[144] The preparation showed great promise during animal testing, and made headlines around the world shortly after World War II, when it was touted as a potential breakthrough in cancer therapy. While the preparation was ultimately deemed ineffective during clinical trials, what secured KR's place in history was the political turmoil that engulfed the scientists connected to the project when they were tried before a 'court of honour' for divulging state secrets after sharing this work with American colleagues. But the story of KR, as Nikolai Krementsov rightly points out in his account of the episode, is not just another example of the perversities of Stalinist science.[145] Rather it is a window on the early attempts in cancer chemotherapy to reconcile laboratory research with clinical practice, and to make room within the field of oncology—which in the USSR at the time (and in Russia arguably to this day) was heavily dominated by surgeons—for another professional subspecialty, chemotherapy.[146]

The political turmoil around KR did not dampen growing enthusiasm for further research on cancer therapeutics either within administrative circles or the research community. For example, in 1946 the All-Union Scientific Research Chemico-Pharmaceutical Institute (the Russian acronym, which will be used henceforth, is VNIKHFI) received an official mandate to concentrate its drug development in eight areas, one of which was oncology—a surprising level of priority given the post-war medical and economic challenges facing the country.[147] And in 1951 the USSR Academy of Medical Sciences expanded Kliueva and Roskin's laboratory into the Scientific

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[144] Krementsov, *The Cure*.
[145] Ibid.
[146] For a treatment of these tensions in the western context, see Löwy, *Between Bench and Bedside*.
[147] Mary Schaeffer Conroy, "The Soviet Pharmaceutical Industry and Dispensing: 1945-1953," *Europe-Asia Studies,* Vol. 56, No. 7 (Nov., 2004), pp. 963-991. The mandate probably came from MinZdrav, which had jurisdiction over this institute, though Schaeffer Conroy does not specify the source.

Research Institute of Experimental Pathology and Therapy of Cancer.[148] One of the main goals of the Institute was the development of cancer chemotherapeutics, and it was fairly successful, creating two notable drugs within its first three years in operation—sarcolizin and dopan.[149]

Beginning in March 1952, this Institute was under the directorship of N.N. Blokhin—a skilled surgeon and an extremely energetic and ambitious administrator.[150] Blokhin oversaw the Institute's growth and its transformations—first into the Institute of Experimental and Clinical Oncology (IECO) in 1959 and then, in 1975, into the All-Union Oncology Scientific Center (VONTs). During Blokhin's thirty-five years as director (he was forced to retire at the end of 1987), this institution became the leading cancer research and treatment facility of the USSR and built up a solid reputation with colleagues abroad. The discovery and development of new chemotherapeutic agents were a research problem to which Blokhin assigned a high level of priority from his earliest days as director.[151]

In the first days of the US-USSR cooperation, however, the presence of these well-established institutions and research programs hindered, not eased, the process of working together. As the Joint Committee for Health Cooperation observed in the introduction to the report on its second meeting in March of 1973,

> Scientific definitions, standards, research techniques and laboratory equipment are markedly different in the US and USSR, and before meaningful joint projects can be started, even before our established

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[148] N. G. Blokhina, *Akademik Nikolai Nikolaievich Blokhin: Vrach i Chelovek* (Moskva: Meditsina, 2001).
[149] AMS F 9120 Op 2 D 6270, Dolgosrochniye prognozy na 15-20 let razvitiia vazhneishyh napravlenii meditisinskoi nauki, tom 1.
[150] Prior to this appointment Blokhin was working in his native city of Gor'kii, where he received his medical training.
[151] Among Blokhin's first organizational initiatives as director was the creation of a new laboratory dedicated to the synthesis of new cancer drugs (Blokhina 2001).

> data can be meaningfully compared, it is essential to work together systematically to establish a sound scientific common ground.[152]

The problems posed by these differences were not the usual difficulties that arise as a result of the absence of the tacit knowledge necessary to make an experiment work at a different site.[153] It was a more daunting problem of a lack of agreement as to what constitutes a proper experiment in the first place. The Committee therefore insisted that

> …our scientists first develop the proper methodology before embarking on lengthy and costly joint projects whose results may be worthless because of diverging standards of measurement, differing evaluations of clinical responses, different diagnostic criteria, and so on.[154]

The monograph on methods, which was one of the first cooperative projects of the Soviet-American healthcare exchange, was the result of this effort to establish a common methodological basis for joint projects. The way its authors went about this was not to write up an agreed-upon common methodology (although by 1977, when the monograph came out, there were numerous joint protocols in place), but to provide a detailed description of the methods used in each country, with a separate section for the Soviet and American side. Besides providing a forum for a detailed discussion of the methodology of each side, it was hoped that by presenting Soviet and American methods side by side, the monograph would serve as a reference for chemotherapists who worked outside the borders of the US and USSR, offering them a detailed portrait of the drug

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[152] Report of the second session of the US-USSR Joint Committee for Health Cooperation, 26-30 March 1973, NARA RG 443, Box 144, Folder: INTL 4-1 US-USSR, 1973.
[153] For the classic exposition of the problems posed by tacit knowledge in scientific research, see Harry Collins, *Changing Order: Replication and Induction in Scientific Practice*,(Chicago: University of Chicago Press, 1992).
[154] Report of the second session of the US-USSR Joint Committee for Health Cooperation, 26-30 March 1973, NARA RG 443, Box 144, Folder: INTL 4-1 US-USSR, 1973.

development programs pursued and the methodologies employed by the two superpowers.[155]

One doesn't need to delve deeply into the text to see striking differences between the idealized depictions of Soviet and American approaches—they jump out in the table of contents. The American section of the table of contents reads like a logically ordered flow chart. After a historical note on the NCI's drug development efforts and a brief description of the organizing principle behind these efforts, the linear array, there is a succession of eight articles detailing the separate phases of the array, logically arranged. The series begins with an article on selecting agents for screening, follows with a piece on screening methods, and so on all the way through phase III of clinical trials. The section concludes with a brief note on current drugs of interest to the NCI's chemotherapy program and a series of appendices of charts and protocols. The Soviet section of the monograph is far less systematic and jumps back and forth not only between the various phases of drug development (with an article on the experimental selection of antitumor compounds of synthetic and plant origin following the article on clinical trial methods), but also between articles on methodology and papers on the chemical properties and mechanisms of action of various classes of antitumor compounds.

This contrast in the organization of the table of contents is a reflection of the contrasting organizational approaches to cancer drug development. The Americans were describing a program that operated through a mechanistic planning and control technique

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[155] It is possible that this presentation isn't a result of wanting to put one's methods forward, but is due to the fact that they ultimately couldn't really reconcile them to come up with a single methodology, set of standards, etc. However, as I have not been able to locate the correspondence regarding the production of this monograph, this must remain pure speculation.

manifested in the linear array. This technique required that "the flow of operations within a research program be pictured logically" and differed from Soviet control methods in that its operation wasn't centered on the coordination of distinct units but on the application of standardized methodologies that enabled the flow of compounds through the process.[156] The mechanism 'flowed' thanks to the fact that "the separate phases are logically dependent; that is, they give rise to information or materials required by succeeding phases."[157] By contrast what the Soviets were describing was a collective attempt to solve a problem. In this effort, a diverse collection of interdisciplinary methods was applied to the problem by largely independent collectives of investigators. The complexity of the problem required that the efforts of these semi-autonomous collectives be coordinated.

It needs to be emphasized that the problem the Soviet biomedical bureaucracy was trying to solve was not only that of anti-cancer drug development, but of cancer as a whole. The top echelons of the state bureaucracy designated cancer as a problem of all-union significance. The lower echelons of the state bureaucracy that were charged with finding the solution—the Presidium of the Academy of Medical Sciences--broke this umbrella problem down into a number of smaller ones such as "Biochemistry of tumors and the biology of tumor cells," "Diagnosing malignant neoplasms," "The clinic, surgical and complex treatment of malignant neoplasms," and "Organization of the anti-cancer struggle and prophylaxis of malignant neoplasms."[158] New anti-cancer drug development

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[156] C. Gordon Zubrod, Saul A. Schepartz, and Stephen K. Carter, 1977. "The Linear Array." *Methods of Development of New Anticancer Drugs: USAA.-USSR Monograph*, DHEW publication No. (NIH) 76-1037, 1. Page 13

[157] Ibid.

[158] AMS F 9120 Op 2 D 7407, Protocol #33 zasedaniia Prezidiuma AMN SSSR 28 dekabria 1977 goda, "O gosudarstvennoi programme rabot na 1976-1990 gg po kompleksnoi probleme

was a large component of the problem titled "Chemotherapy of malignant neoplasms."[159] Each of these problems was coordinated by a group of experts that constituted the 'problem commission.' The experts came from the network of biomedical research institutes overseen by the Academy of Medical Sciences and MinZdrav, and it seems to have not been unusual for someone to be part of several commissions.

It also needs to be emphasized that for the Soviets, the solution of the cancer problem was to be simultaneously sought in two separate domains—the laboratory and the clinic. To be sure, there were substantive differences between the two domains in the West as well, but there was an ambition to reconcile them by freeing the clinical testing process from the constraints of therapeutic medicine and giving it the same kind of scientific basis as laboratory experiments.[160] This ambition is reflected in the incorporation of the various phases of clinical trials as just another step in the linear array, logically integrated with and qualitatively no different from the other steps in the process of drug development. As I will demonstrate in the sections that focus on Soviet drug development practices, while there was a lot of emphasis placed on strengthening the relationship between the experiment and the clinic, this relationship was not conceptualized as a harmonization of methods of knowledge production but as a dialogue in which knowledge produced in laboratory settings would inform the work of the clinic and the needs of the clinic would be reflected in the research problems pursued in the laboratory.

'zlokachestvennye novoobrazovaniia.'"

[159] Ibid.

[160] See Löwy, *Between Bench and Bedside.*

Approaches to organization (both of the text and the drug development program itself) were not the only difference. There were substantive methodological differences as well. One example is in the approaches to the search for new potential drugs. Both the Soviets and Americans agreed that there were two ways to go about this. One way to go about searching for new drugs was to screen as many compounds as possible, whether of synthetic or natural origins, which somehow entered the field of vision of researchers—an approach the Soviets labeled 'empirical.' Another was to rationally design compounds based on available understanding of pharmacodynamics. Although both American and Soviet programs employed both methods to some extent, the Americans relied very heavily on the former and the Soviets on the latter—whereas NCI screened 21,295 compounds in 1975 alone, Soviet annual totals fluctuated between 800-1000, with the majority of the research effort being directed at rational synthesis of new compounds.[161]

> The American rationale for favoring the empirical approach was that
>
> > Random input ... provides the greatest variety of new chemical classes or structures. Materials developed by rational approaches are generally extensions of existing active agents or analogues. The percentage of active materials developed by the rational approach will be much higher. Whereas the rational method is preferred, the exploratory approach may serve a useful purpose in providing new structural classes of compounds for testing. Unfortunately, the state of the art has not reached the degree of sophistication that allows for a completely rational approach to drug testing.[162]

Although Soviet scientists agreed that rational drug design was still at a rudimentary level of development, they nonetheless saw this as a more reasonable approach because massive screening was deemed to be wasteful and because it was seen as a distraction

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[161] Ibid.
[162]Harry B. Wood, 1977. "Selection of Agents for the Tumor Screen of Pontential New Antineoplastic Drugs." Methods of Development of New Anticancer Drugs: USA-USSR Monograph, DHEW publication No. (NIH) 76-1037, 1. Page 15.

from developing the rational approach, which was thought to be more promising in the long run.[163]

This difference in approaches to the search for potentially active compounds led to other methodological differences. Since the NCI screening program was fairly centralized, with compounds from a variety of sources coming in for screening, and the number of compounds screened by the NCI was extremely large, screening systems had to "be sufficiently quantitative so that an initial observation of activity is readily reproducible."[164] The articles describing these screening systems read almost like a protocol, stressing standardized steps and quality control measures and trying to incorporate as many practical details as possible. For example, we are not simply told that it is "necessary to ascertain that tumors do not harbor bacterial contaminants" and that tests are "conducted for each tumor used to maintain the stock tumor line as well as those for the preparation of inocula drug testing."[165] It is specified that these tests are usually conducted in quadruplicate, and that if bacterial growth is found in two or more vials, all the animals implanted with that tumor fragment are discarded. These detailed accounts are further fortified with actual protocols reproduced in the appendix, complete with reporting forms.

By contrast, Soviet efforts at rational synthesis were carried out in over twenty laboratories scattered throughout the country, with many labs doing their own screening of synthesized compounds. This meant that the number of drugs to be screened would

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[163] RONC, uncatalogued records. Report by Prof. N.N. Preobrazhenskaiia "The place of the rational and empirical approach in the search for new anti-tumor compounds," Protocol №24 of the meeting of the Scientific Council of the All Union Oncology Center of the Academy of Medical Sciences of the USSR together with the methodological seminar, 20 October 1982, RONC.
[164] Abraham Goldin, John Venditti, and Stephen K. Carter, 1977. "Screening at the National Cancer Institute," *Methods of Development of New Anticancer Drugs,* 37.
[165] Ibid., 40.

stay relatively small, and that as a consequence researchers had no need to develop a single model or a system of models sensitive to compounds with diverse chemical structures and mechanisms of action and could limit themselves to experimental models sensitive to the particular class of compounds they were focused on. It also meant that the compounds under study had to be compared to each other and to already developed compounds of the same class. While the idea of developing a unified screening methodology was periodically discussed, the advantages of this differentiated system always appeared to outweigh the shortcomings, and each institute continued to refine its own methodology as needed.

This does not mean that the screening methods of each lab were completely homegrown or that they differed radically from each other or the methods used at the NCI (for example, both the NCI and the IECO screened all new compounds against mouse leukemia L1210). Rather what it meant was that each lab could pick and choose from the available tools of oncology screening, modifying the combination of animal tumor strains to accommodate their research agendas. It also meant that labs could adjust the criteria of activity applied in the evaluation of new compounds. These were revised upward as new, more effective drugs were developed. For example at first the IECO would pass a new alkylating agent for clinical testing if it slowed the growth of sarcoma 298, a tumor very sensitive to such compounds, by 75%. Subsequently a drug had to result in the regression of a certain percentage of tumors. Finally sarcoma 298 was completely abandoned as a screening tool, and activity came to be evaluated on tumors which showed only moderate sensitivity to alkylating agents, with survival times of the animals serving as criteria of efficacy.

Not surprisingly, there is no appendix with Soviet protocols, and most of the articles outline general trends and report significant results rather than conduct detailed discussions of methodology. Some of the articles don't discuss methodology at all. Somewhat surprisingly, given the consistent complaints of American scientists that the Soviet methods of clinical testing were primitive, perhaps the most extensive discussion of methods is to be found in the article on clinical trial methodologies. Although the Soviets distinguished four phases of clinical trials to the American three, in reading the description of the different methodologies one is struck more by their similarities than the differences. As in American trials, the objectives of the first phase were to determine the maximum tolerated dose of the drug under several dose schedules (although some details differed—for example, American investigators calculated doses by correlating milligrams to square meter of body surface, whereas the Soviets used kilogram of body weight). The first of these was a long course of continuous treatment (that is, the drug was administered daily for 30-35 days), the others were short and interrupted schedules.

Phase II tested the long dose schedule of the drug developed during phase I for efficacy in a number of 'signal tumors.' If the results were positive, the other dose schedules were also tested against the signal tumors. The drug was tested against an additional spectrum of tumors and underwent detailed clinical pharmacology study. This meant that, as in the U.S., there were in fact multiple phase II studies. If the drug showed efficacy during these studies, it would go on to phase III, which was oriented to carving out a space for the drug in general medical practice. In this phase, the drug was compared to existing treatments through randomization, as well as testing in various combinations with available compounds.

Phase III concluded with a decision about whether or not to allow the drug in general medical practice. A drug would be allowed into practice if it met one of the following criteria: it was effective in no fewer than 20% of patients with tumors that were unresponsive to existing modes of chemotherapy; it demonstrated significant advantages over existing drugs (either being significantly more effective or less toxic than, but equally effective as, existing preparations); or it was a compound of a new class with efficacy comparable to that of existing preparations. Phase IV was essentially a continuation of phase III that went on after the drug was permitted to enter general practice.

This extensive discussion of clinical trial methods creates the impression that this phase of drug development was characterized by very concrete ideas of what constituted proper methodology and a strict adherence to procedure and protocol that is in many ways akin to American trials. But as the next section of the chapter will show, in practice the story was quite different. Soviet clinical trials (like the pre-clinical phases of drug development) were characterized by a remarkable level of procedural flexibility that grew out of a conviction that clinicians had both a right and an obligation to exercise their discretion in the course of clinical drug testing. Although a discrepancy between protocol and actual research procedure is by no means unique to the Soviet context, what sets this context apart is that clinical trial protocols seem to have no force as a normative guideline.[166] That is, in practice there is no expectation that they will be followed to the letter, and researchers alter and even directly violate their provisions in order to achieve the best possible outcome for the patients enrolled in the trial. This flexibility of

[166] On the normative dimensions of protocols in science, see Lynch, "Protocols."

procedure was necessary because clinical trials in the Soviet Union remained in the domain of therapeutic medicine. It meant, however, that standards could not be relied upon to guarantee the production of reliable knowledge—their lack of normative force meant that they could not endow the science with legitimacy.

The distinctions described here between Soviet and American approaches to cancer drug development are not absolute. Both the Soviet and American cancer efforts were examples of big science. Both were highly complicated endeavors involving motley interdisciplinary networks of professionals scattered across distinct institutions and governed by a group of elite experts through a combination of peer review and bureaucratic mechanisms of control. But it is precisely because of these structural similarities that the differences in the systems of knowledge production are so important.

Material practices of Soviet clinical drug testing

In this section, I turn to the practices of Soviet drug development—in particular, clinical trials. Before turning to specific examples, however, a few words need to be said about Soviet drug regulation. Every drug in the Soviet Union, whether developed domestically, imported from abroad or copied from an established foreign drug, had to be approved by MinZdrav's Pharmacological Committee.[167] Gaining approval for a drug to enter medical practice entailed multiple reviews by the Committee—its authorization was required for initiating clinical trials, passing from one phase of clinical testing to the next

[167] The structure of MinZdrav was reshuffled fairly frequently, and the Committee's position in the organizational chart changed accordingly. In the 1960s, for example, it reported to the Scientific Medical Council of the Ministry, and in the 1970s to the Directorate for the Implementation of New Drugs and Medical Technology. What effect if any these changes had on the work of the Committee is beyond the scope of this project.

as well as for implementing the drug into medical practice.[168] In brief, every drug, whether foreign or domestic, had to undergo clinical testing in the Soviet Union before it could be approved for use in its healthcare system.

The Pharmacological Committee was composed of medical experts drawn from the network of research institutes subordinate to MinZdrav and the Academy of Medical Sciences, and as a result was deeply integrated into the biomedical community in two ways. First, the integration proceeded through overlapping membership of staff with other committees and organizations. To illustrate this point, it is enough to consider the example of Vladimir Aleksandrovich Chernov, who occupied the position of deputy chair of the Committee in the late 1970s. In addition to that position, Chernov headed a sub-commission within the committee, was the deputy chair of the Academy of Medical Sciences commission on cancer chemotherapy that operated out of the All-Union Oncology Center, and at the same time directed an important laboratory dedicated to anti-cancer drug development within VNIKhFI. Other members of the committee similarly combined research or clinical work in one of the Academy or MinZdrav institutes with multiple regulatory and administrative duties.[169] The Committee was also integrated as a direct result of its regulatory functions. The Committee worked very closely with VNIKhFI, whose clinical group coordinated the clinical trials of most drugs, and starting in 1967 with the All-Union Chemotherapy Center, which was based at the Institute of Experimental and Clinical Oncology and took over coordination of the testing of anti-cancer drugs (the Russian acronym is VNHC). It was VNIKhFI's and VNHC's

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[168] The Committee was responsible for chemical preparation. Biological preparations such as vaccines were regulated by the Committee on Vaccines and Serums.
[169] RONC, uncatalogued records. Protocol №22 of the meeting of the Scientific Council of the All Union Oncology Center of the Academy of Medical Sciences of the USSR, 29 December 1979.

responsibility to recruit sites for the trials, supply these sites with the experimental drugs and trial protocols (the protocols were reviewed and approved by the Pharmacological Committee before permission to carry out the trial was granted), and then collect and analyze the data in order to recommend a course of action to the Pharmacological Committee, which reviewed their reports and rendered the final verdict.

In other words, drug development was essentially regulated through peer review. Although this system of peer review was embedded in the bureaucratic structures of planning and control, it did not operate through the bureaucratic procedures of these structures. This is because, as I will demonstrate later in the chapter, the bureaucratic structures did not work and getting things done strictly through the bureaucratic channels was effectively impossible. Instead, individual researchers and teams pursued their own practical agendas through whatever means available to them, and then retrospectively rendered their actions compatible with the bureaucratic methods of planning and control to the extent possible. To illustrate this point, I take a closer look at how VNHC and VNIKhFI shepherded drugs through clinical trials.

It should be noted that I was not able to pursue a systematic examination of this process. The state of the archival record makes a systematic approach impossible. The vast majority of the VNHC records appear not to have been preserved. And although more VNIKhFI records are available, limitations in the way the records are organized and in researchers' access to them is limited, made a systematic examination impractical. Therefore in what follows I rely on the case study method, describing the practices of the VNHC by tracing the progression of the drug gossipol through clinical trials, and VNIKhFI practices in the clinical testing of 5-fluorouracil.

Gossipol had been developed at the Scientific Research Institute of Chemistry and Technology of Cotton Cellulose—an institute of the Ministry of Chemical Industry located in the republic of Uzbekistan.[170] The institute housed a laboratory of physiologically active compounds charged with conducting extensive testing of every compound isolated in the course of cotton production—the primary crop of several of the Central Asian republics—including by-products of manufacturing processes. By 1970, the laboratory had isolated and studied nearly 120 organic compounds, some of which found practical applications. One of these compounds, a substance named gossipol (and some of its derivatives), showed promise as an antitumor agent, an anti-viral substance and a relatively non-toxic immunosuppressant.

The substance was passed for pre-clinical evaluation to the All-Union Institute of Medicinal Plants (VILR), the Institute of Physical Chemistry of the Academy of Sciences, USSR, and the Uzbek Scientific Research Institute of Oncology and Radiology and on the basis of these tests recommended by MinZdrav's Pharmacological Committee for clinical trials in cancer patients in June of 1959. The results from the trials, which were at first carried out in only four clinical institutes (three of them in Moscow, one in Tashkent) were sufficiently encouraging that the Pharmacological Committee passed a resolution to expand testing, passing the responsibility for coordinating further trials to the newly created All-Union Chemotherapy Center. The Center was asked to coordinate the clinical trials of this substance in 1967.

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[170] RONC, uncatalogued institutional records. Folder "Gossipol, correspondence." "Justification for conducting additional research by the laboratory of Physiologically Active Compounds of the Scientific Research Institute of Chemistry and Technology of Cotton Cellulose, Ministry of Chemical Industry USSR for 1972."

The trials entrusted to the Center were to be conducted at ten clinical research institutions scattered throughout the Soviet Union. Although supplies of the drug were more than adequate (thanks in part to the fact that the raw materials for its production were so plentiful and in part to the fact that Uzbek authorities were clearly making gossipol development a priority, going so far as to develop new more efficient and less costly methods of manufacturing of the drug even before its efficacy was established), only five institutions actively took part in the trials. Three others were apparently doing nothing with their supply of the trial drug and another two simply didn't report their activities. Attempts by the Minister of Health of Uzbekistan and the President of the Uzbek Academy of Sciences to exert pressure on the Center through the Government Committee on Science and Technology (a division of the Soviet Council of Ministers) to speed up the trials yielded no results, although the GCST was apparently sympathetic to their request and drafted a resolution in support of it.

The institutions that were taking an active part in the trial were doing so very much on their own terms. By the time the All-Union Chemotherapy Center got involved in coordinating clinical trials of gossipol, the drug had already been evaluated against a variety of tumors, showing highest activity against adenocarcinoma of the stomach. Thus the protocol designed by the Center was intended to test the efficacy of the drug as a prophylactic of this type of cancer following radical surgery**.**

The protocol designed by the Center was a three page document which began with a brief description of the drug—its chemical structure and origins, as well as the preliminary results of earlier studies which, although very promising (70% of

adenocarcinoma patients given the drug were alive without metastasis or recurrence four years later), had involved too few observations to be conclusive.

The trial was to enroll patients with operable forms of stomach cancer in stages 1, II, and III, who met a fairly long list of criteria (the tumor could not be a recurrence, they had to be under 75 years of age, had to be free of a variety of complications, and had to live within an easy distance of the institution conducting the trial, but could not be affiliated with that institution). Enrolled patients were to be divided into two groups by birth year. Those with an even number birth year would get gossipol after surgery, those with an odd number birth year would not. In all cases, the institution conducting the trial was to send the Center a sample of the tumor along with a standardized form that was to be carefully completed upon the discharging of a patient after surgery.

The drug was to be administered starting 15 days after the surgery and continuing for three years, with the patient under regular observation for the duration of the trial. It was to be administered in tablet form, three times a day at 10mg doses in 15 day intervals The intervals were intended to reduce nausea which resulted from the build-up of the drug in the body. All patients enrolled in the trial were to be examined at six month intervals throughout the duration of the trial, the results of the examinations were to be carefully recorded on standardized forms and copies of the documentation were to be forwarded to the Center, which would compile the results and share the findings with the participating institutions.

Neither the protocol nor the standard reporting forms, however, were strictly adhered to by the participating clinicians who conducted the trial. Some participating institutions simply didn't do the trial. Others sent one page perfunctory reports that listed

only the number of patients under observation. Still others took up the testing with enthusiasm, modifying the protocols as they saw fit. The Moscow Regional Oncology Dispensary, for example, had enrolled 65 post-operative patients in the trial in its first year. Reasoning that separating out a control group would double the time needed for the trial, the investigators at the dispensary decided to draw on their institutional resources and use historical controls instead. To this end, they analyzed the case histories of 533 patients who had been operated upon at the Dispensary between 1948 and 1957, and calculated survival times for the control and the test groups. Although survival times in the test group were slightly higher than the historical control, the investigators concluded that the difference was not statistically significant. Still, the investigators recommended that the trial be continued because it was too early to draw any definitive conclusions.

The Center's attempts to enforce adherence to their protocols through written warnings and site visits appear to have been largely ignored by the participating institutions. Still, after three years of testing, the Center wrote to the Scientific Research Institute of Chemistry and Technology of Cotton Cellulose, informing the creators of the drug that "in our opinion, there is currently no valid basis to consider gossipol an anti-tumor therapy."[171] What is interesting is that the center's staff claimed to be basing this conclusion largely on the results of their own clinical trials, dismissing much of what was done at external sites—including at the Moscow Regional Oncology Dispensary—as

[171] RONC, uncatalogued records. Letter from V. A. Grishka, VNHT to A.I., Scientific Research Institute of Chemistry and Technology of Cotton Cellulose, laboratory of Chemistry of Physiologically Active Compounds, September 2, 1970. Folder "Gossipol, correspondence."

insufficiently rigorous methodologically because of a failure to follow the issued protocol.[172]

This is interesting for three reasons. First, the detailed trial protocol and the rejection of results from most external testing sites shows that researchers at the Center had very concrete ideas about appropriate methodology for producing clinical evidence of gossipol's efficacy. Second, it shows that these ideas included randomized clinical trials (at least five years before this methodology was supposedly introduced by Americans in the course of the exchange). Finally, it is interesting because the Center's dismissal of the trials done at other sites is so unusual. VNHC was able to do this because its affiliation with the Institute of Experimental and Clinical Oncology provided it with a clinical base on which to conduct its own trials and because the affiliation gave it access to the considerable authority of the Institute within the field of cancer chemotherapy.

VNIKhFI, which specialized in drug development generally and had neither a clinical base of its own nor an authoritative reputation in any medical discipline, did not have the luxury of relying on its own trials. As a result it accepted very heterogeneous reports from its various clinical sites and used them for formulating its recommendations to the pharmacological committee. This becomes clear when looking at the trials of the anti-cancer drug 5-fluorouracil, which VNIKhFI coordinated between 1963 and 1966, before VNHC assumed oversight of clinical trials for anti-cancer drugs.[173]

On June 28, 1963 Professor G.N. Pershin, the deputy director of VNIKhFI wrote to the Pharmacological Committee that VNIKhFI had developed a method for producing

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[172] This despite the fact that ultimately all the trial reports, whether they followed the issued protocol or not essentially agreed in their findings—that the results were inconclusive.
[173] RGANTD, F P-186, Op. 1, D. 1841.

the drug 5-fluorouracil, an antimetabolite developed in the late 1950s by Hoffman-La Roche, and asked the Committee's permission to carry out clinical trials of the drug at ten institutions.[174] Pershin's request was granted fully at a meeting of the Pharmacological Committee on the same day. The speedy review of the request no doubt facilitated by the fact that, in addition to being the deputy director of VNIKhFI, Pershin was also the chair of the Pharmacological Committee.

The clinical trial protocol developed by VNIKhFI was approved by the Committee in September of the same year. It is interesting to contrast the trial protocol developed by VNIKhFI for 5-fluorouracil with the gossipol protocol. It too started with a brief description of the drug's chemistry and pharmacological properties, and referred to results of previous trials (in this case, those carried out abroad) by specifying that the drug had been found to be effective in cancers of the stomach, intestines and breast cancer. But unlike the gossipol protocol, it provided no list of inclusion criteria and made no provisions for randomization, leaving these decisions entirely to the discretion of the investigators. It only asked that investigators test two intravenous treatment regimens—one that administered the drug slowly through a drip, and another that consisted of a simple injection daily for twelve days. In both cases, the dose to be tested was specified as 10-15 mg/kg of body weight, although for the simple injection method, which was thought to be less well tolerated, investigators were free to reduce the dosage or skip injections once patients began to feel unpleasant side effects. Investigators were asked to keep a close watch on the patients 'blood picture' (levels of white blood cells and

[174] The institutions included the IECO and the Leningrad Oncology Institute, which were under the jurisdiction of the Soviet Academy of Medical Sciences; the Gertzen Oncology Institute, which was under the jurisdiction of MinZdrav; several oncology hospitals in Moscow and Riga; and institutes belonging to the Latvian Academy of Sciences and the Russian Republic MinZdrav.

platelets). For patients that tolerated the drug well, it was recommended that treatment be repeated in 4-6 weeks after the first course.

The protocol concluded with a list of possible side effects (which included nausea, leucopenia and thrombocytosis, ulcers and intestinal bleeding—in the case of the latter, treatment was to be discontinued immediately) and contra-indications which recommended that weak patients, as well as patients who have recently undergone major surgery, chemotherapy with alkylating agents or radiotherapy should not be included in the trial. No standard reporting forms, or indeed reporting instructions of any kind, accompanied the protocol. In fact, the protocol was less like a strict methodological instruction than a manufacturer's drug insert (on which it seems to have been closely based).

Although the protocol made few procedural requirements of researchers, even its loose recommendations were regularly violated. For example in their initial report on the progress of the trial, filed in December of 1963, doctors from Moscow City Clinical Hospital #1 reported that they had administered the drug to a total of 7 patients, only four of whom had stomach cancer (the others suffered from cancers not specified in the protocol, including one patient with lung cancer). The report did not go into much detail on what had been done and only offered a vague description of some of the side effects observed (for example, "short term (2-3 days) leucopenia to 1800-3000 ml"). Still, the physicians shared their "impression that side effects appeared only with the rapid administration of the drug" and that all patients demonstrated a positive therapeutic effect (including the lung cancer patient, whose tumors shrank). The report concluded that trials should be continued, and requested more of the drug so that treatment could be repeated

for the patients already enrolled and doses given to others who would be added to the trial.

In 1965, N.I. Perevodchikova, the head of the chemotherapy department at the IECO, presented the final results of the trial to the Pharmacological Committee. Although the final report was not preserved, it seems safe to assume that it was consistent with the initial reports that found the drug to be effective—5-fluorouracil was recommended for approval, a request was sent to the relevant authorities to organize production of the drug as soon as possible, and the IECO was charged with putting together instructions for use that could be disseminated throughout the healthcare system (these instructions, which looked much like the original manufacturer's brochure that informed the protocols, were approved by the Pharmacological Committee in 1966).

What do these case studies tell us about Soviet drug development? How are we to interpret the apparently almost total procedural flexibility that characterized clinical knowledge production? When one adopts the point of view of the western observer, the answer is clear—this is an illustration of the dysfunction of the Soviet system and the backwardness of Soviet medical science. It does, in fact, look very much like the Wild West: lawless and chaotic, with every man for himself. The conclusion is especially easy to accept given how little is known about biomedical sciences in the Soviet Union, particularly in the period under consideration.

The Wild West argument, although easy to make, obscures more than it reveals. Although there is no doubt that the Soviet biomedical system did not work as described on many levels, it was in fact (at least for a time) surprisingly effective when it came to developing new anti-cancer drugs.

In a report on the long-term prognosis for the development of chemotherapy prepared for the Science Coordination Department of the Academy of Medical Sciences in 1970, L.F. Larionoff reported that the Soviet Union was second only to the United States in terms of its successfil record in developing new anti-cancer drugs, with a total of 11 novel original preparations to the USA's 19 (slight variations on existing drugs and copies of drugs developed abroad were not counted). Although the US not only had more drugs, but also a greater variety of drugs—with five of its drugs being alkylating agents, four antimetabolites, two plant extracts, three antibiotics and four hormone preparations, whereas the majority of the Soviet drugs (nine out of eleven preparations) were alkylating agents—Larionoff was not particularly impressed or threatened by American achievements in chemotherapy. In the report he notes that

> The leading role currently occupied by the U.S.A in the development of various types of antitumor preparations is determined, first of all, by the fact that in the U.S. this work started five years earlier than in the USSR, during World War II. Second, in the U.S. the potential of chemotherapy was understood early on, and in 1955 huge sums were allocated to its development, a special chemotherapeutic center and two journals were organized: one—for the rapid dissemination of research results …, the other—a review journal that covers all world literature. But the effect of the invested sums could have been greater had the US not adopted the untenable approach of a massive search for preparations among practically all synthesized compounds.[175]

And, although Larionoff emphasized that there were plenty of systemic problems that were hampering drug development efforts, such as insufficient number and capacity of existing laboratories of chemotherapy, the unsatisfactory state of biochemical research and the inadequate supply of laboratory animals and drugs among them—he still expressed confidence that in the Soviet Union "clinical chemotherapy of cancer is

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[175] AMS F 9120 Op 2 D 6270, 54.

developing quickly… and is almost first in the world."[176] He added that the creation of the Oncology Center (which at the time of the report was already in progress, and which was slated to become the largest facility of its kind in the world) would ensure its continued uniform development.[177] Given that new drugs had been developed at the rate of 5-10 new preparations in the course of five years, Larionoff expected to see steady growth in the number of new domestically developed drugs available to Soviet oncologists.

How do we account for this confidence and level of productivity in the face of such obvious and persistent systemic problems? This phenomenon must remain a paradox unless we step out of the 'Wild West' analytical frame and attempt to understand the system on its own terms. I argue that these cases are better understood as illustrations of a different assemblage of epistemic commitments—an alternative system of material and discursive practices of knowledge production in which the clinical phases of drug development were integrated into therapeutic medicine and subordinated to the priorities of patient care.

To illustrate this point, I will now examine closely the methodological discourse relating to clinical research in one of the settings in which the peer review of knowledge production took place: the Scientific Council of the Institute for Experimental and Clinical Oncology.

The experiment and the clinic

[176] Ibid.
[177] Ibid., 55.

The Scientific Council of the Institute was a hybrid institution. On the one hand, it was another link in the bureaucratic chain of planning and control, intended to direct and coordinate the research (both clinical and experimental) of the institute. As such, it was charged with periodic performance reviews of various departments and laboratories, the election (and re-election) of department and laboratory directors, and review of work plans and reports for the institute as a whole. On the other hand, it was a site of a uniquely multi-disciplinary form of peer review. Service on the Council was not compensated and was carried out on top of the scope of official job descriptions. It was a place where the Institute's scientists and clinicians with diverse disciplinary backgrounds met on a regular basis to discuss their work with their colleagues, review the progress of junior scientists, and occasionally hold seminar-like talks on various subjects of interest.

Various methodological questions were a subject of continuous interest, so much so that Scientific Council sessions were regularly dedicated to sessions of the Institute's 'methodological seminar.' Such seminars were universal within the AMS and were part of the ideological training of Academy workers. However despite this official function, questions of Marxist philosophy occupied only a fraction of their agenda. Even those lectures that engaged directly with the ideological canon seem to have been doing so not for the sake of the canon itself, but in order to garner legitimacy for their proposals. For example, V.V. Dvoirin, a leading biostatistician at the Center, titled his appeal for the reorganization of the VONC Center of Calculation "The Methodological Basis of Statistical Analysis in the works of V.I. Lenin."[178] Although the title of the talk made a

[178] RONC, uncatalogued records. Protocol #12 zasedaniia Uchenogo Soveta ONTs AMN SSSSR sovmestno s metodologicheskim seminarom ot 19 maya 1980 g. Doklad d.m.n. V.V. Dvoirina "Metodologicheskie osnovy statisticheskogo analiza v rabotah V.I. Lenina. The Center of Calculation

reference to Lenin's teaching and one of the major arguments highlighted in the abstract was that the proposal to decentralize biostatistics and embed biostatisticians in every research unit of the Center was consistent with Lenin's teaching that statistics are only meaningful when the statistician understands the nature of the problem, the discussion that ensued after the talk made no reference to any Marxist tenets, and the proposal was summarily rejected.[179] Most seminar sessions made no mention of ideology at all, and were devoted to mundane topics such as "Contemporary state of the chemotherapy of cancer and major trends in the development of new anti-cancer drugs."[180]

On September 12, 1977, the Scientific Council and the methodological seminar convened to hear a controversial talk by professor S.P. Yarmonenko on the "Methodological Principles of the Clinical Experiment."[181] Taking a clinical study of a new radiation therapy protocol as his case, Yarmonenko advanced an argument that therapeutic progress was founded on what he termed the clinical experiment. The clinical experiment enabled therapeutic progress by bringing deductive reasoning to bear on the therapeutic problem (as opposed to inductive reasoning, which was more characteristic of clinicians), and also helped overcome the 'conservatism' that Yarmanenko claimed characterized the thinking of practicing clinicians. This argument, though not particularly well developed by Yarmonenko, is still very revealing of Soviet ideas about what constitutes medical science. At first glance, it appears to resonate closely with the narrative of medical progress through the application scientific methodology which

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is the translation of the official name.

[179] This is consistent with the growing consensus among historians of the period that over the course of the Brezhnev period, ideology became increasingly less important.

[180] RONC, uncatalogued records.

[181] Protocol №14 of the meeting of the Scientific Council of the All Union Oncology Center of the Academy of Medical Sciences of the USSR, 12 September, 1977. RONC, uncatalogued records.

characterized American oncology, but there are substantial differences as well. Yarmonenko's clinical experiment, like the randomized clinical trial in the West, promised to erase the divide between experimental and clinical medicine, placing both on the same kind of scientific foundation. But the clinical experiment promised to do this not by harmonizing procedure—Yarmonenko was not offering methodological prescriptions for the clinical experiment—but by giving clinicians access to the same *modes of reasoning* as those employed by experimenters. Thus, Yarmonenko's clinical experiment intervened in medical knowledge production not on the level of the collective (through the imposition of methodological standards), but on the level of the individual.

The reactions that Yarmonenko's presentation provoked from the Scientific Council members is even more interesting then the argument itself. Several members of the scientific council challenged the premise that there was a difference in the modes of reasoning of experimenters and clinicians. As M.O. Raushenbach argued,

> The concepts of inductive and deductive—these are concepts of purely formal logic. We use the methods of dialectical logic, into which formal logic enters as a part. It is dialectical logic that has overcome the split between induction and deduction. In dialectical reasoning they are inseparable.[182]

Along with rejecting the premise that there was any substantial difference between the modes of reasoning employed by experimenters and clinicians when producing biomedical knowledge, members of the council also rejected the argument that medical progress ought to be pursued through the methodological equivalency of the lab and clinic. As G. V. Guliaev, another seminar participant emphatically argued,

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[182] Ibid., 220

> I think that there is no such thing as a clinical experiment nor can there be. The clinic is based on good experiment, but in the clinic it is no longer an experiment—it's an observation.[183]

The majority of the participants agreed that an experiment had no place in the clinic, but their rejection wasn't premised on the idea that the clinical investigator relied on a different form of logic than the experimenter. It was premised on the idea that the clinical investigator occupied a moral position fundamentally distinct from that of the experimenter because he was first and foremost responsible for the wellbeing of the patient—a constraint not present in the experimental set up.

Participants argued that the responsibility of the clinical investigator for the patient meant that the criteria employed in the evaluation of a new therapy would of necessity differ significantly between the lab and the clinic. But this wasn't because clinicians were, as Yarmonenko claimed, more conservative in their thinking. As A.I. Ruderman, another council member, objected, "when a patient dies the doctor experiences a deep spiritual trauma. That's why we are not talking about 'conservatism,' but about clinical responsibility."[184] N.N. Blokhin concurred with this assessment:

> It is true, the psychology of the physician differs from the psychology of the experimenter, but this is determined by the responsibility for the fate of a human being. This isn't conservatism, this is a certain relationship to the patient.[185]

This emphasis on the relationship to the patient, the responsibility for the fate of human beings indicates that in the Soviet biomedical establishment, clinical research properly belonged in therapeutic medicine. This ensured individuals (both researchers and patients) an important role at the center of knowledge production, and enabled the

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[183] Ibid., 224.
[184] Ibid.
[185] Ibid., 229.

procedural flexibility that was so characteristic of the Soviet clinical trials examined earlier. The clinicians from the Moscow Regional Oncology Dispensary who decided to forego randomization in favor of historical controls and those from the Moscow City Clinical Hospital #1 who administered an experimental drug intended for stomach cancer patients to a patient suffering from lung cancer were not violating norms of collective knowledge production. They were exercising their explicit right as physicians to modify clinical trial protocols so as to provide their patients with the best care they could offer.

Where was the 'Wild West' in Soviet biomedicine?

One thing still needs explaining, however—if the Soviets saw their methods of knowledge production as valid, what did Blokhin mean when he compared Soviet medicine to the Wild West? In this section, I explore what Soviet biomedical researchers thought were the problems impeding their work in anti-cancer drug development, and argue that what troubled them the most were not methodological but organizational and structural problems.

As has already been mentioned, encouraging anti-cancer drug development was a long-standing administrative priority as the Soviet economy recovered from World War II, but there were significant impediments to the effort. In April 1965 MinZdrav formed an eight-person commission that was charged with assessing the "condition of scientific research aimed at finding and implementing chemotherapeutic agents in the fight against malignant neoplasms" in the country.[186] The commission was composed of two chemists, two experimental and two clinical chemotherapists and a pharmacologist, and it delivered

[186] GARF, F.8009, Op. 2, D. 2691,180.

its report to MinZdrav's Scientific Medical Council in 1966.[187] The commission's report divided the process of drug development into three stages: the synthesis or isolation of new compounds, the evaluation of their anti-tumor activity and mechanisms of action, and "the most important stage" of the process—clinical trials to "select those preparations which are good, effective, and least toxic for implementation into practice."[188]

Although professor Chernuh, who read the commission's report at the council meeting, prefaced its assessment with the optimitistic observation that in all three stages the Soviet Union had seen a "significant movement in terms of an improvement of work" in recent years, the report outlined a litany of daunting problems plaguing anti-cancer drug development.

At the first stage, there was still a significant lag in the development of expertise in biochemistry and molecular biology at many research institutions, which inhibited the detailed understanding of cancer etiology. The negative impact of this lack of expertise was further exacerbated by a shortage of equipment, which wasted on scientifically unproductive work the time of the few qualified personnel who were available. There was a shortage of institutions, equipment, and animals for initial screening of compounds. And although a number of well known scientists were working on resolving this problem, it was still the case that "drugs are being synthesized and there is nowhere to put them through an initial evaluation. "[189]

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[187] The specialty of one of the committee members was unspecified, though he was apparently a candidate of science.
[188] Ibid, 182.
[189] Ibid, 184.

As for clinical trials, they were being "delayed by many years."[190] The commission based this conclusion on an examination of the activity of MinZdrav's Pharmacological Committee—the regulatory body responsible for reviewing pre-clinical testing data on new compounds before permitting clinical trials of the drug, and for the final review of clinical trial data before either permitting or forbidding the use of the drug in Soviet medical practice. The report observed that in the past ten years, the Pharmacological Committee approved seventy-five anti-cancer preparations for clinical trials. Of these, fifteen passed through clinical evaluation to enter medical practice. The trials on eleven others were halted.[191] Testing of the rest had been delayed. That is, of the seventy-five drugs that needed to be clinically evaluated, only one third were able to complete the testing process, with the testing of the rest chronically delayed. The commission report blamed these delays on a whole slew of factors many of which resulted from "the insufficient number of clinical institutions" qualified to conduct such trials.[192]

The lack of qualified clinical institutions was not the only problem, however. The supply of experimental (and even approved) drugs was another major obstacle to completing clinical trials in a timely manner. For example when the commission tried to determine the need for anti-cancer drugs nationwide, it found that

> ...a whole number of preparations, such as cyclophosphamide, instead of the 400 kg needed was produced in the amount of 40 kg, and a preparation such as fluorouracil wasn't produced at all,

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[190] Ibid.
[191] The report does not specify the reasons for the cessation of testing, but from looking at records of the Committee's proceedings it seems that tests were usually halted because the drug appeared to be too toxic, too difficult to administer, or insufficiently effective when compared to existing preparations. It was not common practice to abandon trials because of problems with drug supply or a shortage of appropriate test subjects.
[192] Ibid.

> despite the fact that there was an order of the Council of Ministers on this subject …published in 1962.[193]

That is, drugs were being manufactured in amounts that fell far short of the needs of researchers conducting clinical trials (and sometimes, drugs needed for testing were not being manufactured at all).[194] Even worse, the problem appeared to have no solution—not even direct orders from the highest levels of government could get the pharmaceutical factories to produce the necessary preparations.[195]

One might expect that the commission would attribute many of these problems to a lack of funding and ask for more resources. But while the report did not shy away from pointing out the ways in which lack of resources hampered cancer drug development efforts and asking for additional financial support, it located the core of the problem elsewhere. What the commission perceived to be the major problem confronting the successful development of cancer chemotherapy at all stages was the "poor coordination and insufficient integration[196] of this problem."[197] In particular, what was missing was a single interdepartmental center, such as the Cancer Chemotherapy National Service Center in the US, which could take on the functions of coordination. As things stood, "the

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[193] Ibid,186.
[194] This was equally true of drugs already approved for use in Soviet medical practice.
[195] While a detailed analysis of the workings and malfunctions of the Soviet pharmaceutical industry are beyond the scope of this project, the fact that MinZdrav did not exercise continuous direct control over the manufacture of medical supplies was surely a contributing factor to the chronic shortages. To get some idea of the industry, see Mary Schaeffer Conroy, *The Soviet Pharmaceutical Business During its First Two Decades (1917-1937),* (New York: Peter Lang, 2006); Mary Schaeffer Conroy, *Medicines for the Soviet Masses During World War II* (Lanham: University Press of America, 2003) and Mary Schaeffer Conroy, *In Health and in Sickness: Pharmacy, Pharmacists, and the Pharmaceutical Industry in Late Imperial, Early Soviet Russia* (Boulder: East European Monographs, 1994).
[196] The Russian word for designating this quality is комплексность (*kompleksnost'*). There is no direct translation, but the word denotes the quality of encompassing a whole group of objects, phenomena or processes. In the archival records of MinZdrav and the Academy of Medical Sciences, the word usually denotes an integrated mutli-disciplinary approach to the solution of a particular medical problem. I translate *kompleksnost'* as integration, since this seems to me to be the word that most accurately conveys the meaning of the term.
[197] Ibid.

large number of institutes of chemical, experimental and clinical profile work essentially without contact and without coordinating day-to-day operations."[198] This was because the way the problem was being coordinated at the time—through a commission that met once or twice a year to review plans and reports—made it impossible to "guarantee operative coordination and leadership of the many institutions that are working on this."[199] The commission proposed creating such a center to be based at one of the larger oncology institutes—namely, the Institute of Experimental and Clinical Oncology, which in many ways was the only obvious choice.[200]

The report and the proposal sparked a lively discussion during which the members of MinZdrav's Scientific Medical Council shared practical problems that confronted their research on drug development which the commission's report neglected to bring up, while focusing on large-scale organizational shortcomings. One of these concerned the sharing of information. As Larionoff pointed out, "without good information effective coordination in chemotherapy is impossible."[201] What interfered with the availability of good information was a law that stipulated that results of experimental drug evaluations could not be published until they were confirmed through clinical evaluation.

The clinical evaluation of anticancer drugs, for its part, was a process hampered not only by the shortage of drugs and qualified institutions to carry out the trials that were discussed in the commission's report, but also by organizational shortcomings at the

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[198] Ibid., 257.
[199] Ibid., 187.
[200] Not only did the IECO enjoy the reputation of the best clinical oncology facility in the USSR, it also had the most active research program in cancer chemotherapy staffed by some of the best-known researchers in the field. Finally, it was located in the capital, which meant that its director and staff were in a much stronger position when it came to forming networks and securing all kinds of resources than its nearest rival—the Petrovskii Institute in Leningrad.
[201] Ibid., 195.

available institutions. The trials that managed to actually get underway were often being conducted badly because, as Professor Berlin, one of the meeting participants emphasized,

> …they are being conducted on a voluntary basis. This needs to be emphasized and included in the transcript. In an oncology hospital, where you have the sickest patients, trials are conducted on a voluntary basis. There are twenty-five patients to a physician. What do you want? You want him [the physician] to conduct trials of drugs. He conducts them badly, this is good for nothing and this data is invalid.[202]

That is, the problem was that clinical research was an activity that was assigned to overworked physicians, who were supposed to take on research tasks in their non-existent spare time. What was needed to rectify this situation, according to Berlin, were dedicated personnel—full time positions allocated to the hospitals and clinics engaged in drug testing throughout the country whose sole responsibility would be conducting trials.

While members of the Scientific Council held a variety of opinions on how to prioritize the problems facing cancer drug development, the proposal to create an interdepartmental center dedicated to the coordination of clinical trials was in principle supported by all (although details of the logistics of organizing such a center—in particular, the suggestion that it be based in the Institute of Experimental and Clinical Oncology—elicited some vehement opposition). And MinZdrav wasted no time in implementing this recommendation. By 1967 the All-Union Chemotherapy Center (VHTs), located in the Institute of Experimental and Clinical Oncology and headed by August Mikhailovich Garin, a well-regarded chemotherapist and a long-time employee of the IECO, was already operational.[203]

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[202] Ibid., 205.
[203] RAMS F 9120 Op 2 D 6270, 55.

When reading the transcript of the meeting, what is striking is how profoundly disorganized the bureaucratic structure was on just about every level. And what is equally striking is that in detailing these problems, the Scientific Council did not seem to falter in its conviction that the solution was not more resources, better training or stricter methodological standards—it was more (and more effective) planning and control. Another level of bureaucracy was needed to create the conditions of practice within which biomedical scientists could pursue their projects effectively. The Wild West was not in the absence of standard methodological approaches and collective discipline—it was in the lack of mice and reagents, the shortage of time, and the absence of facilities for producing experimental drugs—in short, in the failure of the system to live up to its promises and to create a context of practice in which individual practitioners could realize their professional agendas.

In this context, where the bureaucratic machinery frequently broke down, the individual had an extremely important role to play. It was the responsibility of individual clinicians to produce reliable scientific knowledge while pursuing the best possible outcome for their patients despite the extremely limited resources the system of health protection provided. This dual mission meant that clinical testing remained squarely in the realm of therapeutic rather than experimental medicine, and that research protocols had no normative force—clinicians felt free to violate their provisions whenever doing so was thought to result in better patient care. In fact, individual clinicians often had to violate protocol provisions, as well as other bureaucratic provisions, to overcome the limitations of the context of practice. But they had to be the right kind of individuals.

CHAPTER 3: CYBERNETICS WITHOUT COMPUTERS AND OTHER PARADOXES OF DEVELOPED SOCIALISM

> …and the princess in rage hung herself with her own braid. Because she calculated exactly how many grains in the bag, drops in the sea and stars in the skies. So let's drink to cybernetics.
>
> *Kidnapping, Caucasian Style (1967)*

In January 1969 *Literaturnai'a Gazeta*—the newspaper of choice among the Soviet intelligentsia thanks to the fact that despite its title and its affiliation with the Union of Writers, its coverage went beyond literature and poetry to include analytical pieces on world affairs, social issues, and a potpouri of topics in science and technology—posed an interesting question in its section "On Different Themes." The question was, Will a computer be able to treat patients? It served as the headline for a lengthy editorial piece by Joseph Abramovitch Kassirskii, a full member of the Soviet Academy of Medical Sciences with an extensive and diverse publication record and a well regarded clinician, on the proper role of cybernetics in medicine.

Kassirski began his editorial with a definition of doctoring as both an art and a science:

> When a doctor is at a patient's bedside or in the lab studying the patient, he is a scientist; but when he is determining an individual diagnosis or individual therapy, he must display his medical art, his intuition.[204]

Kassirski then mounted a defense of intuition against "some proponents of cybernetics" who "recommend making medicine quantitative" and mechanized.[205] While he accorded cybernetics an important role in medicine—for him, "the question of the value of its

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[204] J. A. Kassirskii, "Sumeet li comp'uter lechit'?," *Literaturnaia Gazeta,* January 22, 1969, (4)12.
[205] Ibid.

application ...already ceased to be a subject of debate." He wanted that role clearly circumscribed.[206]

In Kassirski's view, cybernetics could be a big help to physicians in processing straightforward test results, and it had potential to help reduce mistakes on the part of overworked and sometimes poorly trained physicians. But while relieving some of the cognitive burdens carried by physicians, cybernetics posed a danger to the medical profession. It could lead to the "functional atrophy of medical thinking"—an unacceptable outcome because "a physician must always be a thinker, " and must spend his life enhancing his cognitive abilities, "developing intricate observation skills and the art of diagnostic synthesis."[207]

The publication of Kassirski's editorial was intended to publicize the forthcoming All Union Conference on the Problems of Medical Deontology, to be held in Moscow at the end of the month, and to draw attention to one of the main issues of the meeting—what should be the relationship between science, technology, and art in medicine? But despite its link to the event, the appearance of Kassirski's editorial was not an isolated occurence—in 1969 the pages of the Soviet press saw many articles on medical cybernetics. Major newspapers such as *Pravda*, *Moskovskaia Pravda* and *Literaturnaia Gazeta*, as well as trade publications with more limited circulations such as *Meditisnskii Rabotnik*, carried editorials on the subject by well known, authoritative scientists and clinicians. Some, like Kassirskii, warned of the dangers of cybernetics. Others extolled its achievements and promises.

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[206] Ibid.
[207] Ibid.

In a piece that appeared under the headline "The Machine Gives a Diagnosis," another academician and clinician Aleksandr Vasil'evich Vishnevskii enthused that the application of cybernetics in medicine had made it possible "to take into account a large quantity of small symptoms, the meaning of which it is extremely difficult for a physician to evaluate."[208] Vishnevskii saw computers[209] as the way out of the paradox of modern medicine, which routinely generated much more data about patient bodies than physicians could take into account when trying to evaluate their condition. Because a physician was called upon to essentially solve a problem of differential diagnosis, "that is, to determine if the patient has one illness or another—'yes' or 'no'," mathematical formulas to aid the task could be developed and computers enlisted to accurately and quickly weigh every relevant test result and produce a diagnosis.[210] Several such formulas were already being clinically tested in a number of institutes, including Vishnevskii's own Institute of Surgery, with promising results—the differential diagnosis system developed by the cybernetics laboratory for targeting congenital heart defects there claimed an accuracy rate of 90-92 percent.

The potential utility of computers in medicine did not end with the determination of the diagnosis. Vishnevskii was confident that the "union of mathematics, cybernetics and medicine opens a wide range of appealing options" in practical aspects of patient treatment as well. For example, he was sure that it would one day become possible to develop an automated system that would monitor a patient's condition during surgery and

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[208] A.V. Vishnevskii, "Mashina stavit diagnoz," *Pravda,* January 9, 1969(9).
[209] One does not often see the word 'computer' in Soviet cybernetic discourse from this period. Instead, machine components of cybernetics are referred to as *electronno-vychislitel'nie mashiny* (EVM for short), which can be translated as electronic counting machines. For an explanation of what made 'computer' a dirty word in this context, see Gerovitch, *From Newspeak to Cyberspeak.*
[210] Ibid.

"within seconds combine and sum up data from numerous monitoring devices ... suggesting to the surgeon or the anasthesiologist the correct solution."[211] Finally, cybernetics could be employed in assembling an electronic medical archive (either within the confines of an individual institution or potentially even at the national and global levels) that would put the collective experience of the medical profession at the fingertips of individual practitioners.

The editorials for and against cybernetics amounted to a debate on the proper place of quantitative techniques and computing technologies within medical research and practice. Practitioners of cybernetics followed the debate closely and responded to it vigorously. K.N. Gurarii, a member of the Laboratory of Cybernetics of the Institute of Experimental and Clinical Oncology,[212] wrote a blistering response to Kassirskii, labeling his remarks "a crude violation of the duty and ethics of modern physicians."[213] Gurarii believed such an accusation to be justified because Kassirskii's remarks "objectively cause major harm to the efficacy of patient treatment."[214] As Gurarii saw it, the ethical obligation of the modern physician was to be 'optimally professional'—to use all the tools placed at his disposal by science and technology in the treatment of patients. Cybernetics was an absolutely crucial tool for meeting this obligation. If physicians persisted in emphasizing intuitive evaluations of patients, they would never improve their results, and not because they did not want to or because they lacked knowledge or the ability to think logically. Instead he emphasized that it was because "the most amazing

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[211] Ibid.
[212] He was probably the director of the laboratory, although I have not been able to find definitive evidence of this in the haphazardly preserved archive.
[213] RONC, Uncatalogued records. Folder 18G:Tezisy, doklady, stat'i, K.N. Gurarii s soavtorami. K.N. Gurarii, V.I. Vapnik, Untitled.
[214] Ibid.

intuition is fundamentally limited by the capacity of the human brain" which cannot adequately weigh and process all the information that needs to be carefully considered when coming up with a diagnosis and course of treatment.[215] As Gurarii saw it, the purpose of a computer was to act as an amplifier of the brain—to multiply its information processing capacity, not to replace the physician's reasoning.

There are several ways in which one could analyze this debate. The first is on the level of professional politics. The attempt to secure a place for cybernetics in medical science and practice involved an impassioned professional turf battle—proponents of cybernetics tried to gain a foothold in medicine by offering its practitioners a kind of standardizing discourse and thereby sought to ensure for themselves a place in medical science and the regulation of medical practice, while biomedical scientists and medical practitioners resisted this encroachment on their professional authority. In this sense, the history of medical cybernetics in the Soviet Union falls in line with the history of quantification as a technology of mediation in other scientific pursuits.[216] The second is on the level of scientific institutions and organizational structures. One can read the story of medical cybernetics as yet another example of how the Soviet scientific bureaucracy was broken—although medical cybernetics enjoyed the support of the Soviet state, as it acquired its own institutional base it became ensconced in Centers of Calculation that were separate from the daily work of medical research and clinial units and thus effectively isolated from the biomedical community, losing relevance and momentum.

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[215] Ibid.
[216] For a study of the role of quantification in professional campaigns for legitimacy, see Porter, *Trust in Numbers.* On quantification in medicine, Matthews, *Quantification and the Quest for Medical Certainty*. On the importance of language in defining professional identity in medicine, see Christopher Lawrence, "Incommunicable Knowledge: Science, Technology and the Clinical Art in Britain 1850-1914," *Journal of Contemporary History*, Vol. 20, No. 4, Medicine, History and Society (Oct., 1985), pp.503-520

Both of these interpretations have a place in the history of medical cybernetics in the Soviet Union. But my interest in medical cybernetics in this chapter is not intrinsic, it is practical and methodological. That is, I am interested in the history of medical cybernetics not for its own sake, but because the debate over the discipline's place within medical research and practice can help access the epistemic commitments of Soviet medicine—ideas about what constituted reliable knowledge, how this knowledge was to be produced, and how these decisions were to be made, both collectively and individually. These ideas were central to the debate on the proper role of medical cybernetics in the production and application of biomedical knowledge, as the impassioned nature of the debate attests.

In this chapter, I analyze the debates around medical cybernetics as a window on the normative structure of Soviet biomedical epistemology. Although I examine medical cybernetics in practice by following the progress of this discipline from the mid 1960s to the late 1970s within the Institute of Experimental and Clinical Oncology, my focus is on ideas and rhetoric. I argue that despite official backing, which enabled the new discipline to lay claim to space, equipment and positions within biomedical institutions, medical cybernetics failed to secure a place for itself both in medical research and practice, and that at the root of this failure lies the incompatibility of the cybernetic discourse with the prevailing epistemic commitments of Soviet biomedicine.[217] I also want to make the case

[217] The exception to the overall failure of cybernetics in Soviet medicine is the automated system of control (the Russian acronym is ASU) that was pioneered by A.I. Kitov. But although this sub-specialty of cybernetics found a place in medical settings, it wasn't a part of medical cybernetics since it acted on organizational processes of planning and control, not on the production and application of medical knowledge.

that epistemic commitments are inextricable from ethical ones—that abstract ethical principles inform practical epistemic norms.[218]

I argue that Soviet biomedicine in this period was understood by its practitioners to be a composite of three sciences—the biological, the social and the humanistic.[219] These distinct bodies of knowledge and technical skill cohered in the person of the individual practitioner. To be sure, individual practitioners were embedded in complex administrative bureacratic structures and also in a dynamic scientific community which both sought to govern individual practices—the former through the imposition of methodological guidelines and recommendations, the latter through peer review. But these entanglements were thought to be productive as well as restrictive, and they did not negate the *rhetorical* importance of the individual practitioner in Soviet biomedicine as the entity unifying both the three components of medical knowledge and the experimental and clinical realm. While decisions about what constitutes an appropriate methodology of knowledge production, as well as which knowledge claims were valid and which not, were made collectively, the selection of the right course of action in biomedical practice and research was left to the discretion of the individual practitioner, who was to make these decisions based on his or her understanding of the needs of the patient. That is, experimental considerations were subordinated to therapeutic ones and collectively produced methodological recommendations and guidelines were subordinated to individual decision making.

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[218] I don't mean to imply that the connection is linear—just to point out that it is tangible and significant.
[219] Actually, it was more complex than that, since each of the three components actually encompassed a variety of disciplines. However I use this simplified framework because this is how Soviet practitioners talked about the problem.

In trying to introduce quantitative methods of evaluation and decision making into medical practice, the proponents of medical cybernetics attempted to redefine the epistemic commitments of Soviet medicine in two ways. First, they attempted to exclude the humanistic component on the grounds of unreliability, drawing a distinct boundary between the experimental and therapeutic realms which were hitherto considered inseparable. More important, they sought to subordinate individual authority of the practitioner to that of collectively produced decision making tools—both discursively and in actual practice. In striving to mediate the doctor-patient encounter through a technological intervention, and to subordinate the judgment of individual clinicians to the rationality of numbers, medical cyberneticians anticipated the efforts of EBM advocates. But unlike advocates of evidence-based medicine, medical cyberneticians failed to advance their agenda even though, with respect to institutional and financial resources, they were arguably better placed.

The discursive incompatibility of medical cybernetics with the epistemic culture of Soviet biomedicine is only a partial explanation for this failure—under conditions of instability epistemic commitments can and do shift, and new rhetorical devices emerge to validate the new rules of practice, as I will demonstrate in subsequent chapters. But in the 1960s and 1970s the Soviet system of health protection appears to have enjoyed relative institutional stability and comparative prosperity. The IECO, for example, had a stable core leadership that shepharded the institution through a period of significant expansion and growth from a narrowly specialized institute into a multi-disciplinary center.[220] This

[220] And although there is no denying that the IECO was an exemplary institution, it was by no means unique—the Soviet Academy of Medical Sciences in this period was pursuing a coherent policy of expansion and growth which was focused on the creation of such multi-disciplinary centers.

growth and expansion slowed to a screeching halt by the late 1970s, and frustration was certainly building within the system, but there was no perception of crisis until the late 1980s—although numerous organizational and economic problems were self-evident, the principles on which the system was built seemed sound, the leadership remained stable, and the status quo seemed unshakeable.[221]

Medical Cybernetics in the Soviet Union

In the Soviet Union as elsewhere, cybernetics by the 1960s had become a heterogeneous interdisciplinary field unified by its focus on regulatory systems.[222] This in itself was a remarkable achievement, considering the hostile reception cybernetics encountered from the Soviet authorities when it first appeared on the world stage in 1948. Growing popularity of cybernetics in the west coincided with a domestic anti-American campaign, making cybernetics an easy target for party idealogues.[223] But while cybernetic ideas were condemned in the Soviet press, the discipline's connection to the US military sector ensured financial support for domestic computer and automated control projects. Upon Stalin's death, a group of Soviet intellectuals took up cybernetic discourse in an attempt to cleanse Soviet science of stalinist rhetoric, and it took off like

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[221] This characterization of the system of health protection is obviously impressionistic. But it is consistent with the broader picture of Soviet social and economic life in this period, which has been dubbed the period of stagnation. For a good synthesis of the current historiography of this period, see Lovell, *Shadow of War.* For a contemporary ethnographic perspective on this issue of stability, see Yurchak, *Everything Was Forever*; also Aleksandr Kustarev, "Zolotye 70-e—nostal'giia i reabilitatsiia, *Neprikosnovennii Zapas* 2007(52). Recently, two interdisciplinary conferences have made this period their focus: "What Was the Soviet Union? Looking Back at the Brezhnev Years," Wesleyan University, October 20-21, 2011 and **"**The End of the Soviet Union? Origins and Legacies of 1991," Forschungsstelle Osteuropa Bremen, May 19-21, 2011.

[222] For a topography of the field of cybernetics, see Ronald Kline, "Where are the cyborgs in cybernetics?" *Social Studies of Science,* 2009 (39/3):331-362.

[223] This synopsis of the history of Soviet cybernetics is based on Gerovitch, *Newspeak to Cyberspeak.*

wildfire, not only rising to prominence in scientific circles but diffusing into the popular culture as well.

The discipline of medical cybernetics got its start outside of the biomedical research establishment, under the umbrella of the 'Big Academy' (the Soviet Academy of Sciences), which in 1962 created a separate Section of Biological and Medical Cybernetics chaired by the physiologist V.V. Parin as part of its Council of Cybernetics.[224] In their 1966 *Introduction to Medical Cybernetics,* V.V. Parin and R. M. Bayevskiy described medical cybernetics as a "division of applied cybernetics which utilizes the concepts and achievements of cybernetics to deepen medical knowledge, improve the quality of medical service, and increase the effectiveness of the scientific and practical work of physicians."[225]

At first, the distance from the biomedical community made for a fairly circumscribed disciplinary agenda. The section oversaw research in three main areas: physiological cybernetics, which was concerned with developing mathematical models of physiological systems and processes; medical cybernetics, which in practice was primarily to do with problems of diagnostics and instrumentation; and biological cybernetics, which focused on mathematical models of cellular processes. By the late 1960s, the new discipline had grown substantially and appeared to make considerable headway in the biomedical community. This progress was in large part buoyed by support from the government.

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[224] AS F 1807 Op 1 D 94
[225] V.V. Parin and R.M. Bayevskiy, *Introduction to Medical Cybernetics*. Izdatel'stvo "Meditsina," Moscow, 1966 (National Aeronautics and Space Administration, July 1967), 1.

In 1966, the Central Committee of the Communist Party and the Council of Ministers passed a resolution pushing the production and utilization of computing and automating technologies in all sectors of the economy, to which MinZdrav and the Academy of Medical Sciences responded by developing their own policies of the "organized mathematicization of health protection."[226] The emphasis was placed on mathematicization (and not computerization or automation) because cybernetics was defined as a science "interested in fundamental probabilistic systems," and "the study of the general rules that are inherent to extremely diverse …systems requires a great deal of abstraction based on a number of mathematical disciplines."[227] Computers in this context were a means to an end, and not necessarily an end in themselves.

Although it took the slow-moving Ministry bureacracy a long time to formulate the policy (a directive on the matter finally came out only in 1968), its implementation was surprisingly swift. Whereas in 1968 there was a total of only twenty-six medical research institutes nation-wide that could boast a cybernetics division, by 1970 the number had more than doubled to fifty-four. The number of people employed in these divisions grew even more rapidly—from 200 to 1350 in the same period of time, although this was largely a result of reorganization that moved people with engineering and mathematical backgrounds already employed in various departments of medical research institutes into the newly created cybernetic divisions. There was some capital investment as well (4.35 million rubles) which funded the purchase of equipment. All this

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[226] RONC, uncatalogued records. Folder:18B, , Tezisy, doklady, stat'i, K.N. Gurarii s soavtorami.V.V. Parin and L.G. Suharikov, "O sostoianii rabot po primeneniyu matematicheskih metodov i EVM v meditsinskih issledovaniah i zdravoohranenii," Pages unnambured.
[227] Ibid., 4.

activity was accompanied by promotional efforts—a series of lectures, courses, and conferences on medical cybernetics.[228]

As their numbers expanded and the institutional base stabilized, practitioners of medical cybernetics began to formulate a more ambitious disciplinary agenda, attempting to expand their sphere of influence and legitimate their expertise in the eyes of medical scientists and practitioners. And, as in other divisions of cybernetics, the most important tools medical cyberneticians could resort to in their struggle for professional legitimacy were discursive. As Slava Gerovitch has persuasively demonstrated, Soviet cybernetics as a whole was in many ways a social movement that coalesced around a particular discourse.[229] The goal of this movement was to cleanse Soviet science of the ideological rhetoric that had wreaked so much havoc during Stalin's reign. Its mission was "to bring objectivity to the entire family of the life sciences and the social sciences" through the application of the "precise language of cybernetics," which "was to replace the vague and manipulative language of ideological discourse in fields that mathematics had not yet reached."[230]

Medical cyberneticians were typical of this larger movement in the sense that they took up the mission of bringing objectivity to medicine through the precise language of mathematics, and staked their professional legitimacy on this discourse. This strategy failed, but the failure of medical cybernetics was different from the failure of Soviet cybernetics as a whole. Gerovitch locates the failure of Soviet cybernetics in its success—he demonstrates that in several disciplines the language of cybernetics did in

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[228] Ibid.
[229] Gerovitch, *From Newspeak to Cyberspeak*.
[230] Ibid, 199.

fact replace ideological discourse, but that it did so by taking on the very generality and flexibility of the ideological discourse that it originally sought to combat, in effect simply substituting 'cyberspeak' for 'newspeak.'

This is where the history of Soviet medical cybernetics departs from the trajectory of the history of Soviet cybernetics in general. Despite enjoying official backing for their discipline, medical cyberneticians never succeeded in dominating biomedical discourse. In fact they attempted to modify their rhetoric to reconcile their notion of objectivity with the epistemic commitments their biomedical colleagues adhered to. To demonstrate this trajectory, I will first analyze what medical cyberneticians were offering to bring to biomedical research and practice, and will then follow the fate of medical cybernetics within the walls of a particular biomedical institution by describing the practice of medical cybernetics at the Institute of Experimental and Clinical Oncology in Moscow. I argue that although medical cyberneticians had secured resources for the establishment of the discipline within the institute, they could not make room for themselves in the scientific and clinical work of the Institute, and were ultimately relegated to irrelevance.

The promise of medical cybernetics

As advocates of medical cybernetics saw it, there were two central problems facing biomedical researchers and clinicians alike. The first was that the biomedical data with which they had to work were often unreliable. As K.N. Gurarii lamented in an unpublished editorial,

> There are still many branches of science and technology where we cannot guarantee the quality of data. This is especially true in medicine and biology, where historically there developed descriptive, qualitative methods of evaluation with a majority of nebulous, heterogeneous signs,

> and where quantitative methods are highly imperfect in the metrological sense and often depend on the subjective qualities of the laboratory technicians conducting the analysis.[231]

Biomedical data were unreliable both because they were for the most part qualitative and descriptive—that is, not amenable to precise measurement—and because, in those rare cases where data could be rendered in quantitative measurements, such rendering was often unreliable thanks to the failure of laboratory technicians to rein in their subjectivity.

The second central problem was that biomedical science routinely produced far more data than the human brain was capable of processing. Confronted with mounds of information, individual practitioners had to correctly weigh various factors and understand the relationships between them—tasks that frequently employed such unreliable cognitive tools as intuition and guessing. The result of this was errors—errors in the design of research protocols, which resulted in the production of more unreliable knowledge, and errors in diagnosing and treating patients, which could result in patient injury or death.

Cybernetics possessed the methodological tools that practitioners of biomedicine needed to navigate this complexity:

> The contemporary state of information theory and the available capacity of computers already allow us in many cases to solve with some certainty the problem of selecting factors for the composition of mathematical models without intuitive evaluation and practically without limiting the number of data points involved, and accounting for the possible interrelationships and interconnections between them.[232]

That is, what cybernetics offered to biomedical researchers and clinicians was a way to get reliable, actionable information out of their unreliable data through mathematical

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[231] K.N. Gurarii, V.I. Vapnik. Untitled. RONC, Uncatalogued records, Folder 18G:Tezisy, doklady, stat'i, K.N. Gurarii s soavtorami. Pages unnumbered.
[232] Ibid.

modeling. An investigator employing cybernetic methods had essentially only two responsibilities—to include all the factors that could possibly be relevant to the research question, and to collect enough statistical data to ensure the accuracy of the model.[233] For the clinician, cybernetics promised to take the guesswork out of the process of assigning a diagnosis and charting the prognosis of the illness.

To take advantage of these tools, all biomedical practitioners had to do was redefine their professional role and make room for cybernetic approaches. Namely, they had to understand that their art consisted not of "intuition, a special gift, or divine inspiration" but of the "art of applying fortunately available methods of objective evaluation of the informational value of various factors."[234] Objective methods for Gurarii meant quantitative methods.[235] In other words, what proponents of medical cybernetics were trying to achieve was essentially the same thing that all proponents of cybernetics in the Soviet Union were trying to achieve—a firm methodological foundation for the creation and application of scientific knowledge rooted in mathematical certainty.[236]

But although cyberneticians were actively trying to realize their vision by designing various tools that could serve as vehicles for this methodology, the biomedical profession did not take advantage of these approaches. Despite securing access to various institutional resources such as laboratory space, equipment, and positions, the discipline found itself increasingly isolated and marginalized. While cybernetic laboratories figured

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[233] Ibid.
[234] Ibid.
[235] Despite similarities between medical cybernetics and EBM, objective methods apparently did not mean standard methods. The language of standardization is absent from this discourse.
[236] That such certainly often proved elusive in practice and had to be substituted by probabilistic estimates usually went unmentioned in programmatic statements.

in the official plans, they could not get a foothold in either biomedical research or therapeutic practice.

Medical cybernetics in practice

As an institution that placed a high priority on being at the forefront of its field, the IECO was quick to make room for the new discipline of medical cybernetics within its walls. In the early 1960s, it already employed several experts in computers and mathematics, and by 1974 there was a well established laboratory of medical cybernetics with 34 employees.[237]

In the early days of medical cybernetics at the Institute, its small team of practitioners was apparently subordinate to the medical personnel and confined themselves to two research problems: the development of cybernetic tables for the differential diagnosis of stomach cancer and epidemiological modeling. The small number of the projects belies their complexity. The first of these problems, differential diagnosis of stomach cancer, was being developed in cooperation with a research team at the Institute of Automation and Telemechanics of the USSR Academy of Sciences.[238] Being part of the plan of the Government Committee on Science and Technology (which was part of the Council of Ministers of the USSR), it was a very high level project. The team at the IECO was headed by Professor V. I. Yanishevskii, a doctor of medical

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[237] The state of the records I was able to access make it impossible to determine exactly when the laboratory was organized, although my guess would be that it was some time in the mid to late 1960s.
[238] Institut Avtomatiki i Telemehaniki AN SSSR, Institut Eksperimental'noi i klinicheskoi onkoloigii AMN SSSR, Nauchnii Sovet po klinicheskoi probleme "Zlokachestvenniie novoobrazovaniia" AMN SSSR, "Otchet po teme primenenie metoda obobschennogo portreta k zadache diferentsial'noi diagnostiki raka zheludka ot polipa, iazvy i ahilicheskogo gastrita," 1963. RONC, uncatalogued records. Folder 18B:Tezisy, doklady, stat'i, K.N. Gurarii s soavtorami.

sciences, and was composed of a physician (V.I. Fokin), a candidate of technical sciences (K.N. Gurarii), and an engineer (T.K. Glazkova).

The accurate diagnosis of stomach cancer—particularly accurate initial diagnosis—presented a serious practical challenge to clinicians. As a project report put together by the IECO team in 1963 noted, only 20-30 percent of stomach cancer patients were diagnosed correctly when they first turned to their polyclinic, and only in the following six months did 70-75 percent of patients receive the correct diagnosis. The reason was that in its early stages, the symptoms of stomach cancer closely resemble those of several common and less serious conditions—ulcers, polyps and gastritis. The development of new diagnostic tests was not solving the problem both because the cumulative information from the various tests was hard for clinicians to interpret and because the vast majority of polyclinics were not equipped to carry out the specialized tests. Medical cybernetics promised a way out of this problem by applying a novel solution—teaching computers to classify complex situations.[239]

Fokin and Yanishevskii designed an initial examination card, which consisted of the various signs and symptoms routinely noted in the diagnosis of stomach cancer. The initial card spanned six densely packed letter pages, which had to be filled out in binary form (that is, with yes or no answers, the first of which was assigned a value of 1, the second 0). Using verified patient data from the IECO and the clinic of the Institute of Nutrition (another AMS institute) and an algorithm developed by the Institute of Automation and Telemechanics, the research team then culled the original table,

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[239] I use the word teaching rather than programming here both because that is a more accurate translation from the Russian and because the metaphor of teaching reflects the intention of the medical cyberneticians that computers would get better at this as data sets grew.

eliminating those signs and symptoms that were deemed to be statistically uninformative. The remaining signs were used to code histories of cancer and ulcer for patients with confirmed diagnoses, and then another computer algorithm was applied to create a mathematical portrait of the two groups. This portrait was tested for accuracy against the patient histories of confirmed cancer, ulcer, polyp, and gastritis patients, a sizeable undertaking in itself, considering the work required to make these patient histories legible to the machines. The result of this was another examination card with only 60 signs and symptoms (which fit on two letter-sized pages) that served as the basis of the differential diagnostic tables that were to undergo clinical testing. To assign a diagnosis, a clinician had to fill out all the tables, with each table rendering a cancer/not cancer verdict. Although computers were instrumental for developing the tables, clinicians were not required to use them—they filled in the table by hand in binary form, and then had to perform some basic math. The final diagnosis was cancer if two of the three tables gave a positive answer. The developers of the tables claimed a 91 percent accuracy rate for these tools prior to clinical testing.

The second project was internal to the institute, and was being carried out for the epidemiology department. It applied the principles of teaching computers to classify complex situations developed by the Institute of Automation and Telemechanics for the differential diagnosis project to come up with a methodology for multifactor modeling of cancer epidemiology.[240]

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[240] K.N. Gurarii, T.G. Glazkova, V.V. Dvoirin, Ch. A. Raihlina, "Metodika mnogofactornogo modelirovaniia oncoepidemiologicheskih sostoianii s pomoschiu electronno-vycheslitelnyh mashin," RONC, uncatalogued records. Folder 18B:Tezisy, doklady, stat'i, K.N. Gurarii s soavtorami.

In the decade that followed, the discipline of medical cybernetics seemed to expand considerably at the institute. By 1974 the laboratory of medical cybernetics was staffed by 34 employees divided into three distinct units.[241] By far the largest of these units was composed of the biomedical and programming groups, which together were responsible for numerous projects designed to support the clinical and experimental research programs at the Institute. Besides continuous work on various problems of differential diagnosis and cancer epidemiology, by 1974 this unit was also responsible for carrying out statistical analysis of the results of the experimental and clinical studies conducted throughout the various divisions of the Institute and counseling the Institute's experimental and clinical investigators on appropriate study design.[242] The second unit, which bore the clunky title of the "Group of Computer Exploitation," was responsible for the Institute's routine computer operations. This included managing the utilization of the institute's computer equipment—putting together and enforcing machine schedules, maintaining equipment and bringing new installations online, and performing routine administrative calculations such as payroll and inventory control. The last unit, the group of administrative systems and information processing, was responsible for a number of database projects.

But although both the number of people engaged in medical cybernetics and the list of their responsibilities had grown considerably, the impact on clinical and research practices of the rest of the institute remained negligible. The differential diagnostic tables for stomach cancer, which were ready for clinical testing as early as 1963 (and had apparently been approved for implementation by the Institute's Scientific Council), still

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[241] Laboratoriia meditsinskoi kibernetiki, 1974. RONC, uncatalogued records.
[242] Plan raboty laboratorii meditsinskoi kibernetiki, 1974. RONC, uncatalogued records.

had not made it into the institute's own clinical practice over a decade later, to say nothing of Soviet oncology practice in general.[243]

The laboratory's statisticians, for their part, apparently found themselves shut out of the institute's research. When James Holland arrived at the IECO in 1972 to learn more about Soviet clinical trial methods in preparation for joint tests, he came away from his eight months at the Institute convinced that the Soviets knew nothing of the role of statistical methods in medical research. And, although he was apparently taken on a tour of the cybernetics laboratory, the only impression that he retained of this experience was the recollection that the computers his hosts were so proud of were machines manufactured by GE, with the company logos awkwardly painted over.[244]

Despite the fact that the use of statistical methods was clearly gaining ground in the Institute after the creation of the All-Union Chemotherapy Center and the adoption of joint study protocols with the Americans in the course of the exchange, the isolation of medical cyberneticians only deepened as the decade wore on. At a joint meeting of the Oncology Center's Scientific Council and methodological seminar held in May 1980, V.V. Dvoirin, an expert in biostatistics and a senior staff member of the medical cybernetics laboratory, complained that methodological errors continued to be a routine part of the institute's clinical trial designs and went as far as to suggest that, instead of centralizing statistical support services in the medical cybernetics laboratory, it would make more sense to create positions for biostatisticians on the staff of the institute's various clinical and experimental departments (a suggestion uniformly rejected by

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[243] The Institute's Scientific Council assigned the blame for this state of affairs to the laboratory itself. Protocol №18 zasedaniia uchenogo soveta ONTs AMN SSSR ot 13 dekabria 1976 goda, RONC, uncatalogued records.
[244] James Holland, personal interview, March 2007.

members of the Council)—or at the very least allow the biostatisticians within the medical cybernetics laboratory to specialize.[245]

The situation within the IECO was not atypical. In fact in the rest of the Academy system, medical cybernetics seems to have fared even worse in the long term. A 1986 survey of Academy institutes conducted in advance of a meeting of the Presidium on the state of computing technologies within the AMS system found that there were approximately 298 machines of various types (not all of them functional) and only 400 computer specialists to service and operate them.[246] AMS clinical and experimental researchers, for their part, remained completely ignorant of what cybernetic methods could bring to their work, despite the fact that both the technology and the techniques were reported to be ready for immediate implementation.

What accounts for this failure of medical cybernetics to make an impact on Soviet biomedical science and practice? Lack of funds, which resulted in shortages of both equipment and expertise, as several participants of the 1986 Presidium meeting emphasized, is one obvious culprit. But lack of funds is only a partial explanation. For one thing, although there is no doubt that by 1986 medical cybernetics was severely hindered by a shortage of funding (as was all of Soviet biomedical science), this funding shortage does not explain why the discipline's impact on medical research and practice was so limited in the period spanning the late 1960s to mid 1970s, when funding was comparatively good.

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[245] Protocol №12 zasedaniia uchenogo soveta ONTs AMN SSSR sovmestno s metodologicheskim seminarom ot 19 maia 1980 goda, RONC, uncatalogued records.
[246] AMS F 9120 Op 2 D 11.

This explanation is especially inadequate considering that the vision of success for Soviet medical cybernetics advanced by its practitioners was not predicated on the ubiquity of computing technologies or even trained cyberneticians. On the contrary, Soviet medical cyberneticians were not only well aware but also accepting of the scarcity of equipment, accommodating their disciplinary agenda to this scarcity. The prevailing opinion among medical and cybernetic practitioners alike seems to have been that it was neither practical nor desirable to establish the ubiquity of computers within clinical institutions. Computers were thought to be a highly specialized tool that belonged in the hands of professionals specifically trained to work with them. Accordingly, both the computers and the cyberneticians could serve the medical profession better when concentrated in specialized centers of calculation.[247]

The example of the diagnostic tables for stomach cancer clearly illustrates this point. Although the tables were intended to transform the clinical practices of diagnosing stomach cancer, their use did not require that a clinician have access to either a computer or possess an expertise in cybernetics. On the contrary, the assumption that informed the design of the tables was that such access and expertise would not be available. What was necessary for the successful implementation of the tables was that the clinician subordinate his or her judgment to the dictates of the table—that he focus on those signs and symptoms that the table designers deemed relevant, follow instructions faithfully and accept the verdict of the numbers.

Subordinating clinical and experimental judgment to the verdict of numbers entailed not only a redistribution of professional power, but a reconfiguration of

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[247] A point of view very compatible with the Soviet bureaucratic faith in centralization.

biomedical epistemology. What cybernetics was offering—a numerical form of objectivity that rested on a mathematical foundation—entailed prioritizing of collective evaluations over individual ones. Soviet biomedicine had a strong epistemic commitment to the individual clinician's judgment. To understand the incompatibility of numerical objectivity with Soviet biomedical epistemology, it is important to first analyze how the terms 'science' and 'medicine' were defined, as well as to know something about the peculiarities of Soviet medical deontology.

The individual in the collective: more paradoxes of developed socialism

The direct translation of 'science' into Russian is '*nauka*', but the two terms are not equivalent. Whereas 'science' typically signifies a systematic body of knowledge that deals with observable facts and demonstrates the operation of general laws (as in physics, chemistry and biology), and thus excludes certain kinds of systematic bodies of knowledge (such as history or philosophy, which are categorized as humanistic rather than scientific disciplines), the word '*nauka'* applies equally to all systematized knowledge. This does not imply that in the Russian context all science is created equal—distinctions similar to those in the west between exact, social, and humanistic sciences apply—but the disciplines in each category are considered to be a science in their own right. Medicine occupied a unique position in the system of sciences because it was understood to be simultaneously an exact, a social, *and* a humanistic science.

It was an exact science in so far as it dealt with biology, chemistry, physiology and the like, and it was a social science in so far as its object of study was human

populations. As a 1971 volume on the place of statistical research methods in medicine and health protection explained,

> Medicine as a science holds a peculiar position in the system of sciences. It is, on the one hand, a biological science—a science about the human organism as such, examining it on its different levels (from the molecular and cellular to the organ and the whole) and in its different relationships with its internal and external environment; on the other hand—[it is] a social science, studying human populations from a medical position, from a position of their public health.[248]

Finally, medicine was a humanistic science in so far as it dealt with individual patients, each with his or her own unique experience of life and disease and thus requiring a unique approach to treatment. As a textbook on medical ethics admonished,

> No technical methods of treatment must nullify the personality of the physician and the individual approach to the patient. Knowledge of disease is an abstract knowledge, torn away from the human. This knowledge needs to be concretized, which requires looking at the patient as an individual and taking into account his varied connections to the social context.[249]

The role of a biomedical professional was not so much to bridge the gap between these three different sciences of medicine, but to unite them in the person of the practitioner—it was in the practitioner as an individual that these three different systems of knowledge came together, and it was the practitioner as an individual who brought these combined knowledges to bear on the treatment of each patient.

This is why Soviet medical deontology emphasized that a physician was something one *was*, and not something one *did*, focusing on outlining the individual characteristics that a physician must possess. These characteristics included professional qualifications such as extensive scientific knowledge and technical skill, and personal

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[248] L.E. Poliakov, red. *Statisticheskie metody issledovanii v meditsine i zdravoohranenii*, (Leningrad: Meditsina, 1971), 6.
[249] A.M. Izutkin, red. *Eticheskie problemy meditsiny,* (Volgo-Viatskoe knizhnoe izdanie, 1967), 34.

qualities such as honesty, integrity and selflessness—both types of qualification were equally necessary to enable medical professionals to do their job, since besides settling on a correct diagnosis and designating an appropriate course of treatment, a physician was expected to "exert a psychological and a psychotherapeutic influence on the patient" because one could not "treat a patient without treating his soul."[250]

Encountering an earnest discussion of the importance of the soul in doctor-patient interactions in a text that claims Marxism-Leninism as its philosophical foundation is surprising to say the least. Accepting the centrality of the individual to an ethical system embedded in an ideological context that emphasized the primacy of the collective over the individual is equally counter-intuitive. All the more so when this system is labeled deontological, since deontology is a term that refers to a rule-based approach to normative ethics and seems to belong to the realm of philosophical discussion rather than the practice of biomedical knowledge production. And yet in order to understand this practice, we need to take these intellectual puzzles seriously and tackle them on their own terms.

In the Soviet context, medical deontology took a different discursive form and played a very different practical role from that occupied by ethics in the American context. In the US the discipline of medical ethics has become in the second half of the twentieth century a mechanism of external regulation of medical practice, providing medical practitioners with clear rules of conduct, such as informed consent laws that are aimed at protecting patient autonomy and circumscribing the power of the medical

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[250] Ibid., 35.

profession.[251] In the Soviet context, medical deontology played the role of an internal compass rather than external regulator—its focus was not to provide rules of conduct but to outline a set of duties, and to paint a portrait of the ideal practitioner to which real practitioners could be compared and which they were supposed to seek to emulate. The goal was to ensure the authority of medical practitioners, not to circumscribe it. As an edited volume on various aspects of medical deontology explained, medical deontology was "not ...a set of rules determining the professional conduct of physicians, but ...a teaching about the physician's duty, his civic obligations."[252]

The primary duty of the medical profession was to protect and restore the health of people, and to do that its practitioners had to have the trust and respect of patients. This trust and respect could only be vested in individuals, and had to be based on a combination of theoretical knowledge, moral values, and practical skill.[253] Far from trying to impose rules of conduct on practitioners, Soviet medical deontology actually emphasized the primacy of duties over rules, and explicitly sanctioned breaking official rules of conduct when these interfered with the fulfillment of the primary obligation of the profession.[254] For example, a 1967 text on medical deontology describes the case of a war-time physician who, while serving on the frontlines during the Great Patriotic War, violated official orders to transport wounded soldiers with damage to the spinal column to hospitals behind the front lines. Knowing that these patients were likely to die in transit, he operated on them in the field hospital, and only then authorized their evacuation. The text praises the moral qualities of this physician whose "conscience ...

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[251] As Robert Zussman and others have argued, this transformation in the role of medical ethics is both a symptom and a cause of medicine's declining authority. See Zussman, *Intensive Care.*
[252] M. E. Teleshevskaia i N.N. Pogibko, *Voprosy Vrachebnoi Deontologii* (Leningrad: Meditsina, 1978).
[253] Izutkin, 77.
[254] Ibid., 80.

was expressed not in a formal attitude towards his responsibilities, not in the paragraphs of orders and instructions, but in an internal moral need to save the wounded from death."[255]

In this way the rhetoric of individual responsibility gave Soviet biomedical practitioners practical rights—rights to define what constituted the best interest of the patient and to act in accordance with their understanding of those interests even when such actions violated various bureacratic restrictions on their conduct. The second practical consequence of this rhetoric was that, by placing the interests of the patient at the top of the list of biomedicine's concerns, it ranked therapeutic concerns over experimental ones. As we saw in the previous chapter, this had very important consequences for clinical trial practices—clinicians at external testing sites felt (and in fact, were) free to violate clinical trial protocols to accommodate the needs of patient care by ignoring enrollment criteria or altering drug administration protocols in accordance with patient response.

Conclusion

The rhetoric of individual responsibility, and the practical rights with which it endowed Soviet biomedical professionals, had a crucial role to play in creating the conditions of practice in which the efforts of medical cyberneticians to introduce quantitative methods into medical research and decision making tools into medical practice could be effectively ignored despite the official backing of medical cybernetics by both the biomedical bureacracy and the highest echelons of the Soviet government.

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[255] Ibid., 81

Although government support meant that medical cyberneticians could not be kept out of biomedical institutions, securing an institutional base in the form of equipment and laboratories was not sufficient to advance the discipline's agenda—acceptance from the biomedical community was equally necessary, and this acceptance was not forthcoming.

For the biomedical establishment, the acceptance of cybernetics meant not only ceding professional authority to practitioners of the new discipline. It also required that the biomedical community accept a redefinition of their epistemic and ethical commitments. To make room for their discipline in medical science, cyberneticians attempted to simplify the definition of medical science, cutting out the humanistic component of the definition as irrelevant and leaving only those that were amenable to the use of quantitative methods of evaluation and mathematical modeling. And to make room for cybernetics in medical practice, they needed to reconfigure the identity of the medical professional, disciplining the individual practitioner and subordinating his or her authority to that of collectively produced decision-making tools. In the era of what Leonid Brezhnev termed 'developed socialism,' when the system of health protection appeared as unshakeable as the rest of the Soviet state, biomedical practitioners successfully subverted these attempts by ignoring them. The system, however, turned out to be far more fragile than most of its observers realized. In the next chapter, I provide a partial description of the degradation and collapse of the system of health protection by focusing on the Academy of Medical Sciences.

CHAPTER 4: SOVIET BIOMEDICINE DURING *PERESTROIKA*

Yesterday I was given freedom--
What am I going to do with it?
Vladimir Vysotskiy, Give the Dogs Meat, 1965

The Soviet system of health protection, and the state of which it was a part, turned out to be far from indestructible. In this chapter, I attempt a sketch not so much of the process of its destruction as of how this process was understood by members of the biomedical elite as events unfolded. Making any kind of sense of this period is a herculean task that can by itself fill several volumes, and so I make no pretense at offering a complete account of this process, nor do I address the causal question.[256] Instead my goal is simply to demonstrate that on every layer at which Soviet biomedical epistemology was constituted—that is, on the level of institutions, practices and ideas and rhetoric—the 1980s saw major upheaval and precipitous decline, and that the organizational principles, practical conventions and normative structures that biomedical professionals had employed to structure and navigate their domain failed them.[257]

Thus, what follows is a kaleidoscopic, non-linear account. I focus on the institutions of Soviet biomedicine, basing my account largely on the deliberations of the Presidium of the Academy of Medical Sciences in the final six years of Soviet history and the first year of the Russian Federation.[258] I argue that the events of the 1980s can be described as a process of epistemic erosion. While at the level of institutions and

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[256] This question will no doubt fuel historiographic debate for decades to come. For a thoughtful and nuanced attempt, see Stephen Kotkin, *Armageddon Averted: The Soviet Collapse, 1970-2000* (Oxford: Oxford University Press, 2001).

[257] The upheaval and decline are literally palpable in the archival records. Whereas collections from the 1970s are for the most part meticulously organized and carefully arranged, those from the 1980s seem to be thrown together: with incorrectly numbered pages on crumbling paper, overlapping classification codes and unfinished binding, they radiate the turmoil of their time.

[258] The Kashpirovskiy episode is recounted from my personal memory triangulated against available internet sources (as far as I know, this episode has not been the subject of investigation by any social scientists).

organizations nothing substantive appears to have changed until the Soviet Union's dissolution—the institutions of planning and administration remained as problematic as they had been in the 1960s and the 1970s—the rhetorical responses to the challenges they posed could not be more different. Whereas in the preceding decades, as we have seen in chapter two, these problems were discussed with a can-do attitude as challenges to be overcome, the discussions of the late 1980s reflect a perception of the biomedical profession as a victim of the mindless bureaucratic machinery, caught in a catch-22 and unable to come up with a viable plan of action. The profession's perceived loss of power coincided with a very public crisis of legitimacy, with the biomedical establishment increasingly unable to repel the challenges of various unorthodox medical practitioners. It is in this crisis of legitimacy that the extent of the epistemic erosion of Soviet biomedicine becomes most visible.

I begin at the end by examining how the administration and members of the AMS attempted to cope with the reality of suddenly finding themselves in a different country.

Institutions

On April 28, 1992, the Presidium of the Russian Academy of Medical Sciences called a meeting with the directors of Academy institutes to discuss the issue of strikes among medical workers in the Russian Federation.

The strikes, called by the Coordinating Council for the Defense of the Rights of Scientific Workers—a recent creation of the Workers of Health Protection union—had already begun the day before and were to proceed in three phases. In the first phase, medical research institutes with clinical facilities were to stop external consultations and

deny the transfer of patients from other treatment facilities, including hospitals. They were also to provide patients with treatment and diagnosis consistent with their financing, which meant satisfying 40 percent of the demand, and to stop signing off on sick leaves for patients. In the event that the government did not respond to their demands, on May 4th the strikes were to enter phase two—the complete cessation of routine patient appointments. If that too did not help, as of May 10th no medical assistance of any kind would be provided.

The Presidium of the AMS fully agreed with the Council's assessment that, as F.I. Komarov—a Vice-President of the AMS and the chairman of the Presidium meeting—put it, "medical science is on the verge of collapse."[259] There was still no government program for supporting and developing medical research and virtually no funding, and the few funds available were distributed on a quarterly or monthly basis, making planning impossible and exacerbating the already considerable psychological burdens of uncertainty. The Presidium also supported the Council's demands that the government must do something to address the problem, even going so far as to write an appeal to Boris Yeltsin, but their letter to the President said nothing about the strikes and did not spell out the Academy's position on them. The meeting was convened to formulate such a position.

Komarov, a military man, tried to keep the meeting under control and made his own position clear from the start—"we cannot, in the name of the Presidium of the Academy of Medical Sciences, support the Coordinating Council, all the phases."[260] Although the situation undoubtedly called for protest, strikes were not an acceptable form

[259] RAMN Op 1 D 5, 33.
[260] Ibid., 37.

for the protest to take because they ran counter to the duty of the physician. As Komarov argued, "to support [the strikes] means to say 'don't react to any pleas, not even from other doctors.' But we gave an oath. And we are violating that oath. Before all else [we must] save the sick person."[261] Instead, he suggested that the discussion should focus on coming up with other tactics for getting the government to act on the situation.

But although Komarov wasn't alone in thinking that the strikes were unacceptable, arriving at a resolution condemning them was not an easy matter. This was because, as V.D. Fedorov, head of the Vishnevskii Surgical Institute, whose staff had voted the week before to join the strike, pointed out, "the question that we are discussing today is not a simple one."[262] As Fedorov saw it,

> … we are talking about, I want to underscore, …not about the social protection of [healthcare] workers, but about the defense of health protection, …about the possibility or impossibility of providing medical assistance in those impoverished conditions in which all of health protection is operating.

Accordingly, what was at stake was much more than the welfare of the workers in the system, but the survival of the system itself,

> …the wave of strikes that has gripped Russian health protection …cannot not receive the support of the workers in health protection because they know that this is leading to the complete destruction of health protection, to the refusal to provide care only because there is nothing to provide it with.

For Fedorov, the gravity of the situation meant that the strikes could not be simply condemned as immoral because doing nothing was ultimately worse.

For other meeting participants, the most salient question wasn't even whether or not one should support the strike, but a more fundamental question of "how to conduct

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[261] Ibid.
[262] Ibid., 41.

oneself under these conditions?"[263] Some participants called for hunger strikes, others suggested marching on hospitals that served members of government. Still others implored senior academicians, "who have for so many years ruled medicine" to band together and pick up the gauntlet in defense of the system of health protection.[264]

The transcript of this meeting is striking on multiple levels. First, because it captures a perception in the upper echelons of the AMS that things were suddenly falling apart—that the system of health protection was either on the brink or in the throes of collapse, taking Russian biomedical science with it. As I.I. Dedov characterized the situation, "it's as though we got freedom, but something collapsed—there are no structures that are ready to support [us]."[265]

It is also striking because, by the criteria that the Presidium invoked to define the state of collapse of the system of health protection, the collapse had occurred at least ten years earlier. Finally, it is striking because it demonstrates that when faced with what the Presidium members perceived to be the collapse of the institutional structures of the system, they turned to normative structures—their understanding of the duties of the physician as the defender of patient interests and the champion of the system of health protection overall—for guidance on how to work through the crisis.[266] The normative structures, however, did not prove to be of much help—not only because, as one might expect, they were unequal to the task without the institutional component, but also

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[263] Ibid., 46
[264] Ibid., 50.
[265] Ibid., 40.
[266] Although I argue that these normative structures defined the Soviet period, they did not originate in it. A very similar understanding of the duties of the physician prevailed during the late imperial period. For more see Nancy Frieden, *Russian Physicians in an Era of Reform and Revolution, 1856-1905* (Princeton: Princeton University Press, 1981).

because, by the time the Soviet state finally collapsed, they had already been largely dismantled.

The institutional structures of Soviet health protection were undergoing rapid change in the early 1990s.[267] By decree of President Yeltsin, the Soviet Academy of Medical Sciences was reconstituted as the Russian Academy of Medical Sciences on January 4, 1992. The decree was perceived as a victory for the Academy in its administrative circles. Not only did the Academy survive the collapse of the Soviet Union (an outcome that was by no means certain), but the decree also gave the Academy long-desired independence from the Ministry of Health.

The newly independent Academy's mission was to provide "planning, coordination, expert evaluation, funding and control over the completion of [biomedical] research projects" in the Russian Federation.[268] This mission was very much in line with the historical responsibilities of the Academy, but in the new context of post-Soviet Russia they were essentially impossible to carry out. The Academy faced daunting problems in its attempt to meet these goals. As V.I. Pokrovskiy, the President of the AMS asserted in his address to the first general session of the Russian Academy of Sciences on March 24, 1992, the main problem was lack of funds. The entire Academy budget for 1991 was 324,793,000 rubles. And although for 1992 the Academy requested 809.5 million, it was clear that it wasn't about to get that amount. This was at a time when, as Pokrovskiy noted with bitterness,

> the increasing complexity of the methods of studying man as a biological object has been accompanied in the civilized world (as it's

[267] Or at least they were undergoing rapid renaming. What change this renaming affected in their actual function is unclear.
[268] RAMN, Op 1 D 1, 59.

> become customary to call western nations) by a several-fold increase in funding. Along many priority trajectories funding of medical science in developed countries has grown 20-30 times or more in five-six years.[269]

This in contrast with the Soviet Union, where medicine was next to last in the sciences in terms of funding, and where spending on biomedical research and education grew on average by 1.3 percent a year during the XI five-year plan.[270]

The shortage of funds impacted every facet of Academy operations. Construction of new research and clinical facilities came to a halt because construction sites could not be supplied with materials and equipment, and the Academy was trying to auction off several unfinished buildings on the orders of the Moscow city government—this despite the fact that many of its existing buildings were falling apart and several of its institutes were in desperate need of laboratory space.[271]

The Academy's clinical facilities were in dire straits. Although their budget for the first quarter of 1992 was nearly tripled, this increase could not begin to keep up with the increase in costs, which jumped on average 10-15 times because the government system of allocations was replaced by direct contracts with suppliers.[272] To back up his claims Pokrovskiy cited the costs of some common medical supplies: the cost of vaseline jumped from 1 ruble/kg to 15 rubles; of iodine from 7 to 73 rubles; and the unit cost of bandages went from 10 kopeeks to 5 rubles 50 kopeeks. At the same time, even in those rare instances when money was available to pay the increased prices, the direct contracts did not guarantee delivery. As a result, the Academy's clinics "experienced objective

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[269] Ibid., 62.
[270] Ibid., 61.
[271] Ibid., 63.
[272] Ibid., 64.

difficulties in maintaining a minimal level of the therapeutic process."[273] If the system of financing and supply did not change in the nearest future, Pokrovskiy warned that the institutes would soon be unable to offer treatment, including a number of surgical interventions, and that this would lead to a decline in the quality of treatment the institutes are able to offer.

The system of financing, of course, was about to get worse, not better. Per a decree of the Ministry of Economics and Finance of the Russian Federation from February 6, 1992, funding of Academy institutes for the year was to proceed on a monthly basis and was to be minimal—covering only rent, electricity, heat, communications, salaries and pension, food and medication.

That is, there was no money at all budgeted for research of any kind.[274] This meant that Academy researchers not only could forget about new expensive equipment, but even basic supplies such as laboratory animals; and that Academy divisions producing such supplies had no hope of finding customers and faced destruction. There was no money for publishing, and the salaries of Academy researchers were "significantly below the survival minimum" leading to a depletion of talent.[275]

Pokrovskiy concluded his address by emphasizing

> ...that such a catastrophic state of affairs as regards the funding of science, the question of cadres (the loss of cadres inside and outside the country), the full collapse of the publishing business, the continuing lag in material and technological supply of scientific research, etc, in which

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[273] Ibid.
[274] Ibid., 65
[275] Ibid., 70. The survival minimum (prozhitochniy minimum) is defined by Russian law as the value of the consumer basket—the minimal selection of food items and consumer goods and services necessary for the preservation of human health and the enabling of his/her life activities. The problems Pokrovskiy was describing were not confined to biomedical science—for their broader impact, see Loren Graham and Irina Dezhina, *Science in the New Russia: Crisis, Aid, Reform* (Bloomington: Indiana University Press, 2008).

> the AMS finds itself today, domestic medicine has not experienced in the 48 years of the existence of the AMS.[276]

Considered in light of the fact that the Academy of Medical Sciences was founded in 1944, when the Soviet Union was in the midst of World War II, this is a grim characterization indeed.[277]

Along with the problems caused by the shortage of funding, the system of health protection faced a crisis in the health of the Russian population. Both the quantity and the quality of the population were in steep decline (Pokrovskiy measured the quality of the population by the health of women and children—according to his figures, 75.1 percent of women of childbearing age had health problems, and the percentage of children born healthy decreased to 36.5 percent).[278]

Reading Pokrovskiy's assessment of the state of the Academy, it is hard not to agree with Dedov's characterization that something had, indeed, collapsed. But if one takes seriously the criteria by which Pokrovskiy constituted the problem, then we must date the collapse at least a decade earlier. More, in fact, because a decade earlier the collapse was already so far along that it was visible to western analysts.

In 1981, Nick Eberstadt, a political economist, published an article in *The New York Review of Books* in which he concluded that "measured by the health of its people, the Soviet Union is no longer a developed nation."[279] Eberstadt based his conclusion

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[276] Ibid.
[277] To get a sense of the devastation that WWII wrought on the Soviet Union, see Rodric Braithwaite, *Moscow 1941: A City and Its People at War* (London : Profile Books Ltd., 2006); R.J. Overy, *Russia's War* (New York : Penguin Books, 1998); Harrison E. Salisbury, *900 Days: The Siege of Leningrad* (Cambridge, MA: Da Capo Press, 2003).
[278] Ibid., 57.
[279] Nick Eberstadt, "The Health Crisis in the USSR." Originally published in *The New York Review of Books* in 1981. Reprinted in the *International Journal of Epidemiology* 2006; 35:1384-1394, 1384.

entirely on life expectancy and mortality figures, observing that the former were falling and the latter rising for every age, sex and ethnic group in the USSR. Noting that

> The spectacle of an industrial nation embarking on a path toward preindustrial standards of health is deeply disturbing. A mortality crisis of the sort the USSR is now suffering is alien to everything we understand about modern life. In the world as we know it, in fact, the Soviet health crisis should be impossible,

Eberstadt concluded that such a crisis could only mean that the Soviet social order was "in the midst of a deadly decay."[280]

As for the critical shortage of funds to which Pokrovskiy and the other Presidium members pointed as the other defining characteristic of the collapse, while the situation of the early 1990s was certainly particularly acute, it was not altogether unprecedented. Shortage of funding had been a perennial condition in which the Soviet system of health protection operated. Moreover, within the Soviet system of central planning, spending money was often as difficult as getting it, and the possession of cash was far from a guarantee that one would be able to purchase the equipment, supplies, and services necessary to sustain both therapeutic and research practices.

For example, in 1986 the Academy was flush with cash for capital projects with a budget for construction of 123,199,000 rubles (compared to only 16 million the year before), plus another 11 million earmarked for equipment. This was more money than its institutes could manage to spend, despite considerable pressure to do so. The Oncology Center, for example, only managed to spend 53% of the 4 million rubles it was allocated because it refused to accept unfinished buildings. This stance, as V.V. Gromyko, Vice President of the Academy in charge of administrative and financial questions chided the

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[280] Ibid., 1388.

Center's administrators, damaged the Academy's relationship with the construction agencies:

> And here the builders hit a dead end, and we take away the desire to continue to work with us further, because they have their own plan, they also must spend the funds. If we don't accept finished projects, then the next projects they will be constructing for us, knowing that we won't accept them, and naturally there are tons of loose ends there, that means they are going to go to other sites where they can finish and get their plans fulfilled.[281]

And while some institutes were scrambling to spend money, others such as the Pharmacology Institute continued to scrape by without adequate facilities.

The institutional crisis in Soviet biomedicine, therefore, cannot be reduced entirely to a shortage of funds. For the entirety of these institutions' history, people within them operated under conditions of inadequate and fickle funding. Yet, as the example of the Soviet anti-cancer drug development program examined in chapter two illustrates, they were not defeated by these constraints. Instead, biomedical practitioners worked around the system to achieve their professional goals.[282] Finally, as the Academy's records make readily apparent, the mere presence of money was not enough to ensure institutional functioning. Fraud and theft were major problems. In 1986, the Academy administrators audited 37 institutes and Gromyko characterized the results as too frightening to report.[283] While Gromyko did not quantify the losses from fraudulent transactions, he reported that since the beginning of the year, 153,000 rubles had been

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[281] RAMN, F 9120 Op 4 D 15, 27.
[282] It is possible that the funding shortages in this period became particularly acute, though this shortage must be understood in relative rather than absolute terms. That is, although in absolute terms the amount of funding did not decline (in fact, it steadily—if slowly—increased), in relative terms Soviet biomedicine became more and more impoverished, especially when compared against American biomedical research, which got increasingly more expensive.
[283] Ibid., 40.

stolen, and some institutes, such as the Cancer Center, which accounted for nearly half of that sum, were out of control.

While I was not able to find any systematic data on fraud and theft either in the Academy system as a whole or within the Cancer Center for this period, anecdotal evidence suggests that such infractions had become both more common and more severe by the late 1980s. Whereas in the 1970s the Center's office of the director was investigating such transgressions as the theft of personal wallets within a laboratory or institute letterhead from office stock rooms, in the 1980s the administration had to deal with trying to track down the furnishings of entire rooms which seemed to have dissolved into thin air behind locked doors.[284]

What these anecdotes illustrate is not that a shortage of funds wasn't a problem, but that it was not *the* problem—debilitating as the budget crisis of the early 1990s had been, it was the culmination of a protracted process of disintegration, not its beginning. Moreover, although funding shortages were an integral part of this process of disintegration, equally important was the loss of morale which, I argue, is a sign of the erosion of the normative structures of Soviet biomedicine. The emphasis on glasnost during the perestroika era made this loss of morale starkly visible, as the next section demonstrates..

Perestroika

In May 1987, over 1500 biomedical professionals gathered in Moscow for the 56th session of the Soviet Academy of Medical Sciences. The Academy regularly organized

[284] RONC, uncatalogued records.

such general sessions, which were usually dedicated to an in-depth discussion of a particular research problem, with talks by leading specialists and reports from high-level administrators. These meetings provided the attendees with opportunities to not only listen to the leading authorities in the field, but to be heard as well, with lively discussions following official presentations.

Although these general sessions were usually well attended, the crowd at the 56th session was exceptional not only because of its size but its diversity as well. In addition to full and corresponding members of the Academy, the session was attended by large contingents from the Ministry of Health Protection and the Ministry of Medical and Microbiological Production (headed by the ministers themselves), the ministers of Health Protection of all the republics, representatives of the Central Committee, the Council of Ministers, and the Moscow Party apparatus as well as members of the medical and the general press.

The session opened on May 13th with organizational questions. Academy members were to hold elections for a new President and a new Vice-President of the Academy—after ten years at the helm, N.N. Blokhin resigned his post, as did his right hand man for those ten years, S.S. Debov. Blokhin's resignation was forced by resolution №174, passed on February 5, 1987, which stipulated that management positions within the various academies were to be occupied by persons no older than 65 and that directors of Academy Institutes were to be no older than 70.[285] The resolution further stipulated that, once an academician turned 75, elections could be held for his post (the individuals thus forced out of their positions would retain their salaries and would occupy honorary

[285] Blokhin resigned from his post as director of the All-Union Oncology Center later that year.

positions within their organizations, acting as advisors to their replacements). As Blokhin explained at a meeting of the Academy Party members following the Presidium session, "someone has to be first,"[286] and he wanted to set an example of compliance with the Central Committee's resolution.[287]

After a ten minute break, the delegates were treated to a long talk by the newly appointed Minister of Health Protection, E.I. Chazov (himself a full member of the Academy and the former head of the Cardiology Center) on the "perestroika of medical science in light of the decisions of the XXVII Party Congress and the January plenum of the Central Committee." The new minister delivered a scathing critique of the state of biomedical science in the USSR, citing, among other shortcomings, the inordinate proliferation of scientific councils and coordinating committees, the waste of resources on anemic institutes, the failure to implement scientific and technological advances in medical practice, and the overarching emphasis on clinical rather than experimental concerns which, Chazov argued, led to a neglect of basic research.[288]

With its long-established leadership gone and the new minister attacking the very foundations of the Academy's structure and calling into question its ability to perform its functions, it is little wonder that the discussion that followed was incoherent. Some speakers took refuge behind the familiar smokescreens of party rhetoric. A.I. Potapov, the minister of health protection of the Russian Federation, for example, began his remarks with a rhetorical question: "Will we, workers of medical science, realize the decisions of the TsK Plenum or drown them in pompous rhetoric?" He then proceeded to do the latter,

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[286] RAMN F. 9120 Op. 4 D. 280, 187.
[287] In effect, this resolution essentially purged the most senior members of the academy management both at the Presidium and institute levels. How disruptive this was requires further investigation, however, since the replacement cadres were very much a product of the same system.
[288] Ibid.

working every single buzzword of the period—perestroika, glasnost, stagnation and even democratization—into what amounted to a proposal to create a duplicate academy at the level of the Russian Federation.

Others were defiant in their refusal to condemn Academy activities. N.P. Bekhtereva, the director of the Institute of Experimental Medicine in Leningrad, began her remarks with a heartfelt thanks to Blokhin for his years of service and went on to point out that all the criticisms laid at the Academy leadership were equally applicable to the directors of the individual institutes. She then went on to use the example of her own institute to argue that there was room in the Academy system for more than just shuffling papers, and that despite considerable organizational difficulties and material constraints, interesting and productive work was still being done.

Still others, such as A.V. Val'dman, the director of the Institute of Pharmacology, tried to put a positive spin on things. Val'dman characterized the current situation not as a crisis of the Academy but as growing pains, expressing hope that if some of the irrational features of the bureaucratic system were eliminated (such as the prohibition on contract research for Academy employees who had to resort to driving a taxi to make extra money), the state of affairs would quickly improve.

The majority, however, took the opportunity to express a litany of complaints.[289] V.N. Smirnov's remarks are representative in this regard. Observing that the strongest aspect of the Academy was its ossified structure, he complained that researchers working within the Academy system were always resisting developing new directions of research because, with the glacial pace of the system of funding allocation, starting new programs

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[289] For an excellent analysis of the national pastime of complaining during perestroika, see Nancy Ries, *Russian Talk: Culture and Conversation during Perestroika* (Ithaca: Cornell University Press, 1997).

wasn't worth the trouble—by the time the research got off the ground, scientists in the west would have solved all the interesting problems. Smirnov's critique was not confined to the Academy: he directed equally critical comments at the policies of the Ministry of Finance and the Ministry of Health Protection. Even the perestroika campaign came under criticism. G.I. Sidorenko in his remarks went so far as to complain that all it amounted to so far was more bureaucracy.

As the day's proceedings were coming to a close, the mood was becoming so grim that Chazov tried to salvage the situation by arguing that he had been misunderstood—that he did not mean to imply that the Academy was failing in its mission, merely that its structures could be improved, and that a program for revamping the system of health protection, including the Academy system, was on the verge of implementation and would in short order revolutionize the entire system. Such attempts to raise morale among the biomedical establishment weren't very effective, however.

The decline in morale had an impact beyond the administrative circles of the Academy—it left the biomedical establishment unable to resist challenges to its epistemic authority, leading to a crisis of legitimacy.

Crisis of legitimacy

In 1989, a well-timed visitor to the Soviet Union could bear witness to a very peculiar mass phenomenon. Public spaces would suddenly empty out—adults rushed home from work without so much as checking out what was on offer in the neighborhood store, children abandoned their games in the street, and the elderly women that occupied the benches outside virtually every apartment building would cut short their endless conversation and shuffle back to their homes as rapidly as their ailments would allow.

This behavior was not a response to a bomb drill—virtually everyone was rushing home to assume comfortable positions on the futon and tune into one of two state channels that broadcast nation-wide.[290]

The television would show a stern-looking man with intense brown eyes who wore his dark hair in a slightly uneven caesar cut. Dressed in a simple, dark shirt, the man sat at the front of a large stage, positioned behind a plain desk heaped with letters. Behind him was a thick blue curtain, and in front of him was a microphone. Soft music, dominated by a soothing piano melody, played in the background. Every seat in the cavernous, dimly lit theater was filled, and although there were many young children in attendance, the audience was rapt with attention. With his hands interlocked in front of him, the man would lock his gaze on the camera and begin speaking in a calm but firm baritone. He would start counting, interspersing the numbers with detailed descriptions of the invigorated state his viewers were to experience. Reassuring the audience that it was perfectly fine to feel as though they felt nothing, and equally fine if they found that they suddenly lost control over their neck and limbs, he slowly made his way up to twenty, firmly promising that at the end of the session those who suffered from chronic pain would find that it disappeared, those with high blood pressure would have it return to normal, new mothers having trouble with milk supply would start lactating, and everyone would feel a profound sense of health and wellbeing.

Millions of Soviet citizens watched the six televised Kashpirovskiy sessions, and uncounted numbers reported being healed of various ailments. Anataolii Mihailovich Kashpirovskiy was not the only unorthodox healer to enjoy mass popularity in the waning

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[290] A very similar picture could be observed during the showing of the first soap opera broadcast in the Soviet Union, the Brazilian *Esclava Izaura*, which also riveted the national attention around this time.

years of the Soviet Union—Allan Chumak, who, in addition to treating patients over the airwaves, claimed to 'charge' water and topical creams with healing properties, also enjoyed a large following and regular access to state television—but he was, nonetheless, unique. What set Kashpirovskiy apart from Chumak and other healers was his education.[291] Unlike Chumak, who had a background in journalism, or the numerous if generally less renowned *babki*—traditional women healers who often lacked formal medical education and employed herbal medicines as well as whispering in their craft—Kashpirovskiy was a psychiatrist with formal medical training and over 25 years of practical experience.

Kashpirovskiy's performances were remarkable for several reasons. First, for their reach. They were broadcast on national television—no mean feat considering that there were only two national TV channels—and they were watched by practically every man, woman and child. And second, for the way they transgressed the boundaries and conventions of the Soviet biomedical establishment—despite his decidedly unorthodox methods, Kashpirovskiy's gained access to the national stage with the apparent cooperation of this establishment. His first appearance on Soviet national television on March 31, 1988, consisted of remotely hypnotizing from a Moscow studio a patient undergoing a lumpectomy in Kiev (a similar appearance, this time with the patient in Tbilisi and Kashpirovskiy in Kiev, followed in 1989). The hypnosis took the place of anesthesia, to which the patient was severely allergic. This operation was organized by Nikolai Bondar', a former classmate of Kashpirovskiy and a leading Ukranian

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[291] There is not a lot of scholarly work on traditional healers in Soviet Russia. For a preliminary treatment of the subject in the post-Soviet era, see Julie Brown and Nina Rusinova, "'Curing and Crippling': biomedical and alternative healing in post-Soviet Russia*, Annals of the American Academy of Political and Social Science*, Vol. 583 (Sept., 2002), pp. 160-172.

oncologist.[292] Finally, the very occurrence of such performances in a nation with a highly educated population that espoused scientific rationality as a central value is remarkable in itself.

Not that the biomedical establishment was in any position to effectively oppose him even if they had tried. By the time Kashpirovskiy took his activities, which had made him a local celebrity in Kiev in the 1970s, onto the national stage, the biomedical establishment's ability to police its boundaries and repel attempts to undermine its authority was already severely compromised. One arena where this loss of authority can be seen is in the Academy's long-standing struggle with homeopathic medicine.

In October of 1986, the Presidium of the AMS met to hear the report of A.V. Val'dman, director of the Academy's Pharmacology Institute, on the place of homeopathy in contemporary medicine.[293] The report had been commissioned by the Presidium at the request of the Ministry of Health Protection, which had been provoked by several recent articles in the national press.[294] In April of that year, the newspaper *Izvestiya* had published an editorial titled "Homeopathy: dragging debates," which argued that, although homeopathic medicine lacked an accepted scientific basis, two hundred years of experience proved its methods effective and it deserved more resources such as training courses, publications, and a nation-wide network of dedicated hospitals and policlinics. The piece had apparently struck a chord with readers, and the paper followed up on the

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[292] Georgii Mheidze, "S vami govorit televizor." Retrieved from http://www.bg.ru/article/8299/ July 31, 2011

[293] AMS F 9120 Op 4 D 14.

[294] Members of the AMS presidium made frequent complaints of being the target of press attacks in the late 1980s. For example see AMS F 9120 Op. 4 D. 279.

editorial a few weeks later with a large spread of letters calling for state investment in homeopathic medicine.

The Ministry took such publications very seriously and felt compelled to address them, and asked the Presidium to form a commission to evaluate the claims regarding the value of homeopathy. This is not the first time these issues had come up. In 1960, the Presidium formed a similar commission in response to another article in *Izvestiya*. On that occasion, the Academy literally secured the last word in the debate, with an editorial by N.N. Blokhin, who at the time was serving his first term as the President of the Academy of Medical Sciences, closing the public discussion by arguing that homeopathy had no right to an independent existence, and that homeopathic remedies should undergo clinical testing, with standard treatments providing the control. The issue, however, came up again and again. As a result of further prompting from the Ministry, the Academy actually conducted clinical trials of homeopathic remedies in 1975-76, and concluded that they were no more effective than a placebo. Additional trials—this time on pediatric patients—were conducted by an Academy institute in 1978. In addition, there was an attempt to subject homeopathic remedies to pharmacological analysis, which had to be abandoned because no active ingredient could be isolated from homeopathic preparations.

Citing these studies as well as the expert opinions of the commission's members, the Presidium drafted yet another resolution at the 1986 meeting that condemned homeopathic medicine as unscientific on the grounds that its methods had no basis in experiment and that its approach, after having remained essentially unaltered for 200 years, was based on dogma rather than science. This unequivocally negative assessment, however, is hard to reconcile with the resolution's recommendations, which were to

include homeopathic physicians in the structure of general polyclinics and to continue systematic clinical testing of homeopathic remedies, developing standard scientific and technical documentation for those that were found to be effective so they could be mass produced and added to the state formulary.

The transcript of the Presidium's session sheds some light on this apparent contradiction. First, as the Presidium members privately acknowledged, the Academy and the Ministry were powerless to do anything to reduce the public demand for homeopathic services because from the standpoint of the patient, homeopathy worked. Although there was no reason to suppose that homeopathic remedies were effective in a scientific sense, in a pragmatic sense they helped some patients in the same way that psychotherapy would. More importantly, where homeopathy worked, biomedicine didn't. While it could produce a scientific rationale for its interventions, it often fell far short in the pragmatic sense. Citing the example of asthma, A.G. Chuchalin, a corresponding member of the AMS and the chair of the Department of Internal Pediatric Medicine of the Second Moscow Government Medical Institute, wrote in his response to the commission that

> In recent years due to the considerable growth of allergic ailments, patients are seeking modes of therapy that could improve their physical condition. In the USSR there are no domestically produced drugs. Purchase of imported products can satisfy only a fraction of the demand. The low level of professional training among physicians in the areas of allergic and immunological ailments is a cause for great concern. This explains the appearance in the national press of articles about such modes of treatment as 'Buteiko's method,' … homeopathy.[295]

Chuchalin's implication was that although homeopathy was only effective in 10-15 percent of patients, and although patients turning to homeopathic physicians ran a risk of

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[295] AMS F 9120 Op 4 D 14, 16.

incurring complications from foregoing regular medical treatment, they would continue to seek such care because standard medical facilities often offered them no care at all.

Thus the recommendation to incorporate homeopathic care into the general polyclinic system was not so much an acknowledgement of the legitimacy of homeopathy as a therapeutic tool as it was an attempt to render it worthless in the eyes of patients. Blokhin, who presided over the meeting, acknowledged as much in his closing remarks:

> Let's talk straight, that if in our health protection everything had been smoothly organized, it would probably reduce the stream of people that are trying to go somewhere outside the normal health protection. Let's invite the homeopaths (they are doctors) into our polyclinics, let them see ten patients an hour or whatever (our standards are cruel), and there he can talk with a patient for an hour, prescribe him an infusion of tarantula or an extract of marijuana in micro doses. Over there he has completely different working conditions. Of course, here [in the USSR] their longevity is predicated primarily on the weakness of our health protection.[296]

Bringing homeopathic physicians into the same setting as biomedicine was meant to sabotage their effectiveness. Since their efficacy rested on their ability to spend time with their patients, to offer them comfort through close attention to their problems, putting them alongside regular doctors who had to receive ten patients an hour would expose homeopathic interventions as worthless.

The second recommendation—to continue the clinical testing of homeopathic remedies—was similarly motivated not by any lingering doubts about the efficacy of homeopathic treatment, but by a lack of confidence in the ability of the clinical trials hitherto conducted to withstand close scrutiny. Particularly suspect were the pediatric clinical trials of 1978, which, as the director of the institute that conducted them confessed

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[296] Ibid., 153.

in the course of discussion, were so poorly designed and executed that they could not, in fact, be relied upon to say anything about the efficacy of homeopathic treatment.

The discussion at this meeting demonstrates a remarkable lack of confidence on the part of the most senior members of the country's premier biomedical research establishment in their ability to effectively dispute the claims of homeopathic practitioners and offer an effective public defense of their own practices. It demonstrates an even more remarkable lack of confidence in their own institutional structures and therapeutic practices, as well as in their ability to constructively reform these structures.

Conclusion

The meetings of the AMS Presidium discussed in this chapter offer a glimpse into the way the most privileged members of the Soviet biomedical establishment understood their system in the waning years of the Soviet Union, as well as into the kinds of problems that the system was facing in the course of *perestroika* and right after the collapse. These problems were, in many ways, nothing new. Limited resources, disciplinary issues, and low individual morale were perpetual sources of dissatisfaction for the Soviet biomedical profession—though the fact that these complaints were stable rhetorical tropes does not mean that the degree to which these phenomena impacted practice did not vary considerably over time (the shortage of funding after the Soviet Union collapsed was, in fact, so severe as to make it a qualitatively different phenomenon from the shortages of the Soviet era).

What *was* new in the era of *perestroika* was the way that the leadership of the biomedical profession responded to these problems. Except for the most vacuous rhetoric

and the most vicious attacks on the system as such, there were neither rallying calls nor concrete proposals aimed at resolving the difficulties facing the Academy or the system of health protection more broadly. The discussions convey a sense of collective confusion and quiet desperation, and this confusion and desperation is instantly visible not only in the rhetoric, but in the daily operation of the institutional structures and the public standing of the biomedical profession, resulting in yet more confusion and desperation. The downward spiral culminates in the collapse of the system, which the historical actors allow themselves to admit as a possibility only after the collapse has already taken place.

I argue that what was happening to Soviet biomedicine in the 1980s can be understood as a process of epistemic erosion—a gradual, simultaneous disintegration of the institutional and normative structures as well as the rhetorical tropes that together constitute biomedical epistemology. In the next chapter, I turn to an analysis of how the now Russian biomedical profession has been trying to renegotiate its epistemic commitments by taking up the ideas and rhetoric of Evidence-Based Medicine and adapting them to fit into the post-Soviet Russian context.

CHAPTER 5: SURROGATE EPISTEMOLOGY

The systems here are systemless,
The standards are not standard,
The space isn't Euclid's—
Hell knows whose it is.
Timur Shaov

This chapter provides an analysis of how the Russian biomedical profession has responded to the slowly unfolding crisis in the system of health protection that began in the 1980s and was exacerbated by and continued after the collapse of the USSR. Specifically, I follow the way the Russian biomedical community used the rhetoric of evidence-based medicine in its quest for greater professional autonomy and a renewed sense of identity, as well as certain practitioners' attempts to assume a leadership role in directing healthcare reform in the first fifteen years of Russian independence.

While at first glance EBM seems to have played the same role in Russia as it has in the west, I argue that the dynamics of the adoption of this term among Russian medical circles were quite different. In the context of post-socialist Russia, I argue that EBM was a surrogate epistemology—while its proponents had mastered the ideas of evidence-based medicine and become fluent in its rhetoric, they were unable to create the conditions of practice necessary for its implementation within impoverished healthcare institutes. Moreover, the normative structures that underpin EBM sometimes directly conflicted with those carried over from the Soviet era. Thus evidence-based medicine, though increasingly deployed by Russian biomedical professionals as a rhetorical device, remains mostly that—a rhetorical device rather than an epistemology that is constituted not only discursively but also through institutional structures and practices and normative commitments.

The term evidence based medicine (EBM) has come to permeate biomedical discourse in the English-speaking world and beyond. The most often cited definition of EBM has been supplied by David Sackett and colleagues, who define it as "the conscientious, explicit and judicious use of current best evidence in making decisions about the care of individual patients" by means of "integrating individual clinical expertise with the best available external clinical evidence from systematic research."[297]

While this definition of EBM is simple and widely agreed upon,[298] the simplicity is misleading. Characterizing EBM is not a straightforward matter. EBM has been described as a standardization movement, a doctrine and creed, a technology, a methodology of governance, and a paradigm, to list but a few of its definitions.[299] I argue that in the west, EBM has emerged as the latest incarnation of an evolving epistemology—proponents of this epistemology have achieved a reorganization of institutional structures, a reconfiguration of practice and a reformulation of discourse, changing the rules and power dynamics of knowledge production.[300]

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[297] David Sackett et. al., "Evidence-Based Medicine: What it is and what it isn't." *BMJ* 312(1996):71-72.
[298] For some analysts the definition of EBM is such a straightforward matter that they attribute many of the critiques leveled at EBM to the confusion of EBM itself with the context in which it is applied [Howard Brody et. al., "Evidence-Based Medicine: Watching out for its Friends," *Perspectives in Biology and Medicine* 48/4(2005): 570-84].
[299] For the various characterizations in order, see Timmermans and Berg, *The Gold Standard*; Ross Upshur, "Looking for Rules in a World of Exceptions," *Perspectives in Biology and Medicine* 48/4(2005):477-89; Brody et. al., "Evidence-Based Medicine"; Carl May et. al., "Technogovernance: Evidence, subjectivity, and the clinical encounter in primary care medicine" *Social Science & Medicine* 62(2006):1022-1030, and Evidence-Based Medicine Working Group, "Evidence-based medicine: a new approach to teaching the practice of medicine." *JAMA,* 268/17(1992):2420-25.
[300] Numerous scholars have written on various aspects of this emerging epistemology—in fact, one could argue that much of what is written on the social studies of medicine bears on this issue in one way or another. Marks, *Progress of Experiment;* Greene, *Prescribing by Number;* Sunder Rajan, *Biocapital*; Matthews, *Quantification* engage with this subject most directly.

I argue that in Russia EBM was first and foremost a discourse.[301] In its original context of development—the UK, the United States and Canada—the paradigmatic shift to which the Evidence-Based Medicine Working Group (1992) had aspired seems to have been achieved, bringing with it new institutional arrangements and practices. In Russia however EBM was still very far from achieving the same kind of status, as its institutional arrangements were only beginning to emerge and its proponents still struggle to implement EBM principles into practice. Although Russian advocates of EBM have made considerable headway in recent years, establishing an Interregional Society of Specialists in Evidence-Based Medicine in 2003,[302] opening a branch of the Cochrane Collaboration in Russia, and issuing a journal dedicated to the popularization of EBM among Russian physicians, the position of these institutions remains precarious. As for the impact EBM has on the practice of the average physician in Russia, the most frequent assessment I heard is that so far, there is almost none.

In part, this slow progress is due to the fact that the Russian context is still largely incapable of providing adequate material support for the implementation of EBM. The endemic problems of the Soviet system of health protection, exacerbated by the collapse of the Soviet Union in 1991, evolved into a profound crisis that had lasted through the 1990s. The medical infrastructure fell into disrepair and the medical profession endured a

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[301] Within the social studies of science and medicine discourse is an important, multidimensional concept. It has been shown to be a mechanism of knowledge generation [Paul Atkinson, *Medical Talk and Medical Work: the Liturgy of the Clinic* (London: Sage Publications, 1995); Elliot Mishler, *The Discourse of Medicine: Dialectics of Medical Interviews* (Norwood, NJ: Ablex Publishing Corporation, 1984)], a foundation for professional authority [(Lawrence "Incommunicable knowledge; Warwick Anderson, "The Reasoning of the Strongest: The Polemics of Skill and Science in Medical Diagnosis," *Social Studies of Science* 1992, 22(4): 653-684], and a mechanism for the expansion of that authority into other realms [Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge, MA: Harvard University Press, 1987].

[302] The Russian acronym is OSDM. See www.osdm.org for more information.

period of intense economic hardship.[303] Considering that the healthcare system has lacked such basics as cotton and rubbing alcohol, it almost goes without saying that it has lacked the ability to supply its physicians and researchers with the information technology necessary to ensure timely and expedient access to the latest medical evidence. As one analyst observed, "all the evidence in the world can be of no use if the context of practice fails to provide support."[304] And in part it is because the epistemic commitments of EBM medicine, which necessitate the disciplining of both patient and physician subjectivity, are incompatible with the epistemic culture developed by Soviet biomedicine.

Despite these challenges, segments of the Russian medical profession have actively picked up the discourse of evidence-based medicine and have taken on the task of championing EBM approaches to medical practice. In arguing that the function of EBM has thus far been largely discursive I do not intend in any way to imply that this diminishes its significance. On the contrary, it is precisely its discursive nature that gives EBM its importance. As Nancy Ries has argued, "discourses are a primary mechanism by which ideologies and cultural stances are shaped and maintained."[305] This is certainly true of EBM, the discourses around which, I argue, encapsulate the emergence of a Russian biomedical epistemology out of the wreckage of Soviet epistemic culture.

I argue that in the setting of post-Soviet Russia, evidence-based medicine is a discourse of power, both in the sense of being the dominant discourse of a group that is vested with tremendous symbolic power (the western medical profession) and in the sense that it is a discourse that confers power on those who use it effectively (the Russian

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[303] D.D. Venediktov, *Zdravoohranenie Rossii: krizis I puti preodoleniia* (Moscow: Meditsina, 1999).
[304] Upshur "Looking for Rules."
[305] Nancy Ries, *Russian Talk: Culture and Conversation during Perestroika* (Ithaca: Cornell University Press, 1997), 3.

advocates of EBM). In their attempts to redefine the biomedical profession, carve out a space for this profession among the shifting institutional arrangements of Russian biomedicine, and redefine their relationship to the rest of the world, advocates of EBM are actively reconfiguring the epistemic culture of which they are a part.

I begin with a discussion of the role of EBM discourse in its original context, explain the challenges to its adoption in the context of post-socialist Russia, and then analyze the way various groups in the medical profession employ this discourse.

EBM and the western medical profession

Within social studies of medicine, there has been a great deal of work documenting the decline in the autonomy and authority accorded to the American medical profession. From the benevolent and authoritative figure theorized by Talcott Parsons, the American doctor has become increasingly dependent on a vast healthcare industry.[306] No longer master of his own practice, the doctor has become ensnared in the bureaucratic web of insurance companies.[307] The medical profession has also ceded a great deal of its control over modern medical knowledge to third parties such as the pharmaceutical industry. Pharmaceutical companies have not only taken over many of the functions of producing medical research but increasingly control the dissemination of findings as well.[308] Moreover, patients demand and assume an increasingly active role in

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[306] Talcott Parsons, "Social structure and dynamic process: the case of modern medical practice," in *The Social System* (New York: The Free Press, 1951):428-479.
[307] Donald Light, 2005. "Countervailing Power: the changing character of the medical profession in the United States," in Peter Conrad ed. *The Sociology of Health and Illness: critical perspectives* (New York: Worth Publishers, 2005): 215-223.
[308] See David Healy, "Shaping the Intimate: Influences on the Experiences of Everyday Nerves" *Social Studies of Science*, 34/2(2004): 219-245 and Arnold Relman, and Marcia Angell, "America's Other Drug Problem," *The New Republic* 227/25(2002): 27-41.

their care, and legal controls (in part in the form of ethics boards) are being erected to protect the patients' interests.[309]

In the United States, EBM is both a response to and a consequence of these changes. For its advocates within the medical profession, it is a way to re-establish professional authority over medical knowledge and practice by designating an area of scientific expertise that belongs to the profession.[310] For its critics, on the other hand, EBM is just the latest move to rob the physician of that authority. One of the most bitter points of contention around EBM is the accusation that introducing its principles into practice makes medicine into a cook-book practice, with the physician's role reduced to that of a mere technician whose job it is to match the list of symptoms the patient is presenting with to those in the guideline and then implement a standard treatment.[311] Recently, some social scientists have been developing alternatives to this critique of standardization. For example Timmermans and Berg argue for understanding standardization as "a dynamic process of change" that helps "to bring into existence new ideas, entities, values, and even subjects for medicine."[312]

From this body of work one can deduce that EBM is a double-edged sword for the medical profession. On the one hand, it is being used in the defense of professional expertise. On the other hand, the highly political nature of standards means they can also become tools for external institutions such as the state or insurance companies to regulate the professions and demand legal accountability, further eroding professional autonomy.

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[309] Robert Zussman, *Intensive Care.*
[310] May et al., "Technogovernance," Timmermans and Berg, *Gold Standard.*
[311] Ralph Horwitz, "The Dark Side of evidence-based medicine," *Cleveland Clinic Journal of Medicine*, 63/6(1996): 320-323.
[312] Timmermans and Berg, *Gold Standard*, 23.

The Russian context

While its significance to the western medical profession can be—and is—actively debated, there can be no doubt that in countries such as the US, evidence-based medicine has achieved not only the paradigmatic status to which its advocates aspired, but also the material and institutional support required for implementing it. This is not the case in Russia—EBM advocates there are still struggling to establish a solid institutional base for themselves, and the context of practice of the majority of Russian physicians makes implementing EBM principles into practice a very daunting task.

As I argued in the previous chapter, the system of health protection in Russia had been in a state of profound crisis well before the Soviet Union crumbled. Still, the collapse of the state proved devastating. Although following the dissolution of the USSR the Russian government retained its obligation to provide all of its citizens with medical care—the Russian constitution continues to promise free medical assistance provided wholly by government and municipal institutions—this was on paper only. As written, the state's obligations are very vague, and in practice it was beyond the government's means to fulfill this obligation.[313]

Beset by a litany of disasters on the domestic front, in the 1990s the state effectively dropped support of the healthcare system, leaving the medical profession to deal with the crisis.[314] The central government agencies, unable to cope, implemented a series of reforms that shifted responsibility to the regions, which were ill prepared for the

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[313] See V.N. Bobkova and V.G. Zinina, *Sotsial'nie L'goti Grazhdanam Rossiiskoi Federatsii i Napravleniia ih Reformirovaniia* (Moskva: Vserossiiski centr urovnya zhizni pri ministerstve truda i soc-razvitiya, 2001) and C. Petrosova, ed., *Dohodi Naseleniia I Dostupnost' Sotsial'nih Uslug* (Moskva, IIF Spros, 2003).

[314] Effectively, but not rhetorically. Making the withdrawal or curtailment of this support official would have been unthinkable for the Russian government during the 1990s (and is still quite politically dangerous now). Free healthcare meant a great deal (both politically and practically) to the state and the people during the Soviet period, and neither the state nor the citizenry is ready to let go of that legacy.

task of keeping the system going.[315] Privatization was embraced as a solution (although of course not everything could be privatized—as the director of one of the regional hospitals pointed out, the best equipped, potentially lucrative facilities quickly transitioned to the private sector, leaving the public system to deal with the rest). The heaviest burden fell on practicing physicians, their patients and the local hospital administrators. As a hospital administrator put it during a discussion of privatization,

> … before everything was government-owned, everything, and now in a different direction. That is the government cunningly, quietly apparently wants to, from a series of guarantees, will say that "this isn't ours, this is private, and so we do not provide for it." That's a light push to the side.[316]

Access to medical assistance was disrupted. Hospitals and polyclinics lacked not only medication but also basic supplies such as rubbing alcohol and cotton.[317] Free healthcare became virtually a myth. The situation in the provinces was particularly catastrophic. A Russian acquaintance told me of losing a friend in 2001. The young man was brought to the hospital after losing consciousness, and the physician on duty told the family that nothing could be done until they brought the necessary medications and supplies to the hospital. By the time the patient's relatives got back from their frantic search of the city's pharmacies, it was too late.[318] Another informant recalled being brought to the hospital with a broken arm suffered in a car accident, only to have the physician demand payment prior to taking x-rays. As he could not use his arm, the physician reached into his pocket for the wallet and removed the cash for him.[319]

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[315] Petrosova, *Dohodi*; Hesli and Mills, *Medical Issues.*
[316] SV, personal interview, Stavropol, July 2005.
[317] Judith Twigg, "Unfulfilled Hopes: The Struggle to Reform Russian Health Care and Its Finance," in *Russia's Torn Safety Nets: Health and Social Welfare during the Transition,* Mark G. Field and Judith L. Twigg, (eds.). (New York: St. Martin's Press, 2000).
[318] LG, personal interview, summer 2005.
[319] ZK, personal interview, July 2005. The incident recounted took place in 2004.

In Moscow, where funding and supplies were always more plentiful and where there still seems to be some faith in free access to the healthcare system, instances in which care was, in fact, provided free of charge are scarce. I had observed some instances of routine care provided free of charge when I accompanied an elderly informant on visits to her local polyclinic[320] (the physician on duty listened to her complaints, conducted a routine exam and prescribed medication), but the same informant reported that she was at times unable to get in to see her physician, and had to resort to making an appointment for a fee.[321] As it turned out, the appointment was with her regular doctor, and the informant drew particular attention to the evident embarrassment of the physician at charging a patient who was entitled to free care (according to the informant, this embarrassment manifested itself in a much greater level of attentiveness and patience on the part of the physician than was normally the case). Even in life-threatening emergencies, payment must frequently come before care. A medical researcher bitterly recounted the story of a neighbor who was taken to a hospital with a heart attack, and was left without treatment for over a day until she finally guessed to offer the attending physicians payment. "There is no medicine here, you should study something else" was how he prefaced his account.[322]

Being pushed aside and abandoned by the government on which it depended both for financial and policy support, the medical profession experienced severe pressures of uncertainty and perpetual economic problems. The financial situation of many physicians was grim. A top notch cardiac surgeon working in one of the most prestigious Moscow

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[320] It should be emphasized, however, that these visits took place in the summer of 2005, well after the economic melt-downs of the 1990s and at a time when the Moscow city administration was actively shoring up its support of pensioners.
[321] AA, personal interview, Moscow, August 2005.
[322] SI, personal interview, Moscow, March 2008.

hospitals, for example, commanded a salary of 15,000 rubles a month. In 2005, when these data were gathered, that translated to approximately 526 US dollars.[323] In the provinces salaries are lower, and while the cost of living in Russia's provincial cities is also lower than that in Moscow, it is still quite high relative to salaries. Although the income of physicians "is not limited to salary," as one of my informants delicately referred to the practice of supplementing income by accepting unofficial payments for care or taking on second jobs, the fact remains that making ends meet is a constant challenge for most.[324]

Those just starting out in the profession face a different set of difficulties. Learning the art of giving bribes is as much a part of medical education as biology and chemistry courses. The student forum of the unofficial site of the Stavropol Government Medical Academy is more akin to a rate table, listing how much a given faculty member charges for a passing grade, with the majority of posts devoted to topics such as:

> Q: How to pay Mrs. X?
> A: Very simple. Just come into her office and say: you know, I am completely behind on the grades, help me please—let's solve this problem on a commercial basis. That's what I did. She took it without a problem. The main thing is not to go overboard on the price.[325]

Given that it is possible to buy your way through much of medical school, it is not surprising that some physicians enter into practice poorly equipped for the job. One informant reported coming to her polyclinic physician with a complaint of frequent

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[323] At the time, $1=29 rubles.
[324] VF, personal interview, Moscow, August 2005It should be noted that in Russia, both during and after the Soviet period, most people's income is not limited to salary. So widely known and accepted is this fact that it was quite openly referred to even in Soviet cinema. In the iconic 1968 comedy *The Diamond Arm*, which details the exploits of a gang of criminals attempting to smuggle jewels into the Soviet Union and a hapless honest citizen who inadvertently foils their plans, one of the thieves curses at another: "May you live on your salary alone!" (*Chtob ty zhil na odnu zarplatu*).
[325] Taken from http://sgma.narod.ru/menu/index.htm on October 16, 2005.

migraines and being asked by the doctor if she could also record a complaint of sleep disruption. When asked why, the doctor replied: "otherwise, my diagnosis is not coming out."[326]

These problems made for a context of practice very inhospitable to the implementation of EBM principles. For one thing, Russian physicians simply did not have easy access to "current best evidence" in their daily practice.[327] Very few physicians had access to computers in their work space (most doctor's offices I observed are equipped only with rotary phones), and fewer still were able to gather information on the internet because of both limited access to information technology and widespread computer illiteracy.[328] Subscribing to medical journals is often prohibitively expensive for individual physicians—a marketing study commissioned by a Moscow medical publisher revealed that several physicians working in the same institution are often forced to pool their resources to subscribe to a couple of journals that they then exchanged.[329] While many hospitals subscribed to medical journals, and some cities have specialized medical libraries, these were not user friendly, and their limited hours and cumbersome procedures made consulting them for routine cases impractical. Even when journals were available, their contents were often suspect, consisting in large part of 'sponsored' articles—that is, articles commissioned by a pharmaceutical or medical device company with the primary aim of promoting a product.

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[326] LV, personal interview, Stavropol, July 2006.
[327] Sackett, "Evidence Based Medicine."
[328] Computer illiteracy is mostly a problem among older physicians. The same generational divide applies to foreign language skills, though to a lesser extent.
[329] While in the US physicians working in the same private practice do the same thing, in Russia this is something that is done by physicians employed in large state institutions. In the US, hospitals tend to have their own libraries. NI, personal interview, Moscow, August 2006.

Access to quality current medical information, though difficult, is also not enough—doctors must also be able to apply it in order to practice EBM. This condition too was frequently not met. To illustrate this point, I will examine prescription practices. Each medical institution works with a formulary of drugs that its physicians may prescribe to patients (the MinZdrav formulary serves as the basis for these) and which the patients are entitled to for free.[330] A hospital or polyclinic physician has no right to recommend drugs that do not appear on the formulary to patients. As one physician put it, "you must always say that you don't need anything else.[331] A physician who makes such recommendations risks being punished, as a Moscow informant found out first hand—having recommended a drug not on the formulary to one of her patients, she ended up having to pay for the prescription out of her own salary when the patient went to the administration complaining that the pharmacy refused to fill it free of charge.[332] In this case, the physician got off easy—punishment for such transgressions can be as severe as a reduction in category (roughly equivalent to demotion in seniority), which carries with it a reduction in salary.

EBM advocates are the first to admit that the principles they are propounding have yet to make a large impact on medical practice. In answer to the question of what effect, if any, EBM has had on medical practice in Russia, most advocates answered that so far, there has been virtually none: as one informant put it, "I can say with some certainty—the majority of physicians don't hold to this approach."[333] In part, advocates blame this state of affairs on an absence of material support, and the resulting lack of

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[330] This of course doesn't mean that patients can always actually get them for free.
[331] TL, personal interview, Stavropol, July 2005.
[332] SL, personal interview, Moscow, August 2005.
[333] IV, personal interview, Stavropol, July 2005.

motivation on the part of the majority of physicians to question the status quo and seek out new developments:

> One big problem is the extreme disinterestedness in change among…medical personnel, mid-level personnel. What's the reason? There's no motivation of activity of any kind, one of the most important stimuli is material…material incentive as such is practically absent. That is, the system works in principle so that officially, it is not necessary [to go beyond narrowly defined professional duties].[334]

There are, of course, some exceptions—several hospital administrators in Stavropol assured me that they were actively engaged in subjecting hospital practices to the tests of EBM—gathering data, revising and implementing existing guidelines. They also admitted, however, to running into some resistance from their staff. Thus even in these cases it remains unclear to what extent local medical practices have changed.

While Russian EBM advocates frequently complain of the resistance their colleagues to their ideas, despite my best efforts I failed to locate a single member of this opposition or find any trace of this opposition in print. The closest I came was an experienced Moscow physician who, in a spontaneous meeting arranged by one of my contacts, responded to my questions about his take on EBM with the evasive phrase: "sometimes, the new is the thoroughly forgotten old."[335] The only harsh critiques I heard from physicians were not directed at EBM, but at the industrial medical model of which it is a representative. Such critiques were not common, and the people offering them usually did not use many words. The words that were used, however, were quite colorful and expressive, and though a direct translation would both be impossible and

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[334] ON, personal interview, Stavropol, July 2005
[335] K, personal interview, summer 2005.

inappropriate, the point being made was very clear—that this model is a distortion of what medicine is and should be about.

Dokazatel'naia meditsina

If the problems that have beset the healthcare system are posing significant challenges to implementing EBM into practice, what is EBM's significance in the Russian context? A prominent advocate of EBM pointed to the answer when he assessed the success of his efforts: "...first of all, it's now on everybody's lips."[336] I argue that so far EBM in Russia has served primarily a discursive function, being used by the beleaguered medical profession as a discourse of power through which to shore up its professional identity and redefine its relationship to newly emergent institutions within the healthcare system.

The term evidence based medicine first sounded in Russian medical circles in the mid-nineties, during what was arguably the most difficult period following the dissolution of the USSR. It was actively introduced and promoted by western medical experts who came to Russia as part of international aid efforts to help alleviate the healthcare crisis and contain the threat of infectious diseases, and it found some enthusiastic advocates among a number of senior Russian physicians. Courses were organized, websites launched, talks given, and publications disseminated.

Slowly, these educational efforts began to yield results, and physicians not only in Moscow but on the periphery as well took up the discourse of EBM. In Stavropol, for example, active efforts to promote EBM began in 2003, after a senior physician from the

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[336] NI, personal interview, Moscow, August 2006.

local regional hospital returned from a workshop and heeded the call of EBM promoters to organize a local society. A parallel student group at the medical academy quickly followed the founding of this local professional society. Eventually, enough momentum was gathered across the country to found an Inter-regional Association of Specialists in EBM (the Russian acronym is OSDM).

At first glance EBM advocates in Russia are using the discourse of *dokazatel'naia meditsina* in much the same way as their western counterparts—to assert professional authority in the face of challenges from third parties, and to bolster professional identity. Although, unlike their American counterparts Russian patients do not at present represent a challenge to medical authority, now that the system of health protection is no longer a constitutive part of the Russian state doctors do have to contend with a number of outside parties that attempt to impose controls on their decision-making. Among these parties are the state (which, after being forced by the political and economic crisis of the 1990s to loosen its grip, is once again trying to reassert centralized control over healthcare, although its current relationship to the system is qualitatively different from what it was in the Soviet period), the emerging insurance companies, and the pharmaceutical industry.

Much as in the west, the state and the insurance companies are a threat because they have a direct interest in controlling healthcare costs, and to that end are prone to interfering in medical practice. A hospital administrator described this threat as regards the selection of medication:

> We used to be a budget institution, which means that we also got little, insufficient money, but we got it. We've been transferred to the OMS system (Mandatory Medical Insurance). From this moment on we are paid by set standards by corresponding insurance companies and the

> OMS fund. … The system centers on how much money there is. And the set of medical services a patient receives is tailored to that money. So the work allocations, this is what happens: here we've diagnosed, here's a patient with hepatitis. Well maybe approximately 30, 50 thousand (rubles) is needed for his treatment, and we get 75 rubles per patient. That's where the story starts: let's give him an IV with some fluids, and as for the rest…(*makes a gesture of empty hands*)[337]

Adopting EBM rhetoric helped counter this threat because the rhetoric provided a way for doctors to reclaim some of the decision-making authority when it came to financing by giving them a vocabulary with which to try to assert their right to decide how limited funds should be allocated.

The threats the pharmaceutical industry posed were also similar to those in the west, although the Russian physician's relationship to the industry was made somewhat more complex and problematic by the fact that physicians were dependent on it to a greater extent. In a setting where shortages of funding are severe and chronic, the perks the industry offered physicians could be critical to practice. In addition to being sometimes the only easily accessible source of up-to-date medical information (by handing out free journals and articles and promotional information), the industry's sponsorship of talks, financing for conference travel, and hosting of lunches may have been the only thing that enables many doctors to stay in touch with the larger biomedical community. Practitioners invoked EBM as a tool to mitigate the industry's influence on physician choices of medication, which was seen as undermining the medical profession's credibility by compromising physician decision-making. Explaining how decisions about medication are made at his hospital, an administrator put it this way:

> So [for a physician at the hospital] it's not just: "I want to treat with this." What about safety, and what are the ramifications? And again:

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[337] SV, personal interview, Stavropol, July 2005.

> prove that you need this specific medication. Why are you taking not the cheap penicillin but the more expensive ampicillin or gentomicin, or…list of antibiotics attached. And this is where the doctor says: "you know, we looked, here it's not sensitive, this patient has already been treated with this and it wasn't effective," so in this system it's statistically proven, proven that these types of diseases, when treated with these medications, yield a rate of recovery that's two times faster. All of this is in place and is in practice constantly.[338]

In this scenario, EBM was supposed to prevent physicians from basing their drug selection practices on pharmaceutical industry propaganda by forcing them to justify their medication choices to the hospital administration (and on up the hierarchy) in terms of evidence of efficacy.

Surrogate epistemology

While on one level the power dynamics of EBM initiatives in Russia were similar to those in the western setting, on another they differed markedly. For the Russian medical profession, EBM was more than a tool with which to defend professional authority—it was a larger strategic discourse through which segments of the medical profession were trying to redefine their professional identity, as well as the relationship of the Russian medical profession to the world. It was also a borrowed discourse whose origins in the western context endowed it with considerable power and yet also weighed it down with political baggage. The significance of such rhetoric will become clearer upon a further examination of how the Russian advocates of EBM were using this discourse, as well as how they themselves perceived their application of EBM.

As has already been mentioned, two groups of actors were involved in introducing EBM discourse into Russia. One group was made up of the western medical

[338] SV, personal interview, Stavropol, July 2005.

experts who came to Russia to help reform the healthcare system and strongly advocated EBM. The other was a handful of Russian physicians who sought it out themselves. Early advocates of EBM began their work during the time of social and economic crisis that followed the collapse of the Soviet Union. Western and international agencies quickly stepped in to mitigate and manage the crisis. In the healthcare arena, several large aid programs such as the ZdravReform program funded by USAID, were launched, which aimed at restructuring and bolstering the healthcare system. This brought a new set of Russian healthcare practitioners into close contact with their western colleagues, and gave Western medical experts an opportunity to observe the Russian system up close.[339]

In the post-Soviet context, EBM served two purposes for western medical experts. One was to set a benchmark against which to compare the Russian medical system and recommend changes. The other was to provide a relatively non-threatening and apparently value-neutral language to communicate with their Russian counterparts. Instead of directly challenging Russian practices, formulating critiques in the language of EBM allowed western experts to express them in more rationalistic and impersonal terms. Doing so reduced the potential for conflict and masked the power differential between the western experts and the Russians, making the central issue the presence or absence of sanctioned evidence and not their respective national identities. An American physician who worked in Russia as part of the ZdravReform program noted:

> There were some parallels, some things that were being done ... as far as treatment in the former Soviet Union that were similar to what was being done in the west, but a lot of things that were not. And when you have these differences, it's not particularly helpful to say "well, we're from the

[339] This opportunity was not entirely unprecedented, as there is a long history of healthcare exchange programs between the USSR and other countries—including the US—during the Soviet era. However the dynamics of the post-Soviet interactions were certainly different.

> west so we know better," I mean, that's ridiculous. … And this is why EBM was so important because we could at least say that we now consider that many of the things we used to do in the West when evaluated by randomized controlled trials, particularly with total mortality as the end point, turned out to be not helpful or potentially even harmful.[340]

However, although EBM masked the power differential between Russian and Western medical experts, it did not eliminate it. In fact, to some extent the authority that EBM enjoys in Russia was predicated on its western origins. Russian advocates of EBM very carefully cultivated this association, both for EBM and for themselves. Not one of the EBM advocates I interviewed failed to emphasize that, as one hospital administrator put it, "this [EBM] was all developed by the international medical community abroad.[341] To Russia it came just a few years ago" (NE, personal interview, Stavropol, July 2005).

This emphasis on EBM's western origins was more than just a matter of acknowledging an intellectual debt—it was an appeal to a higher authority, as well as a way to discredit local knowledge and practices while re-credentializing the Russian EBM physician at the same time. Sometimes, the local knowledge and practices were dismissed out of hand. This reaction was especially strong among the younger generation of EBM enthusiasts, who have picked up its principles from reading the writings and taking the courses offered by prominent advocates based in Moscow and St. Petersburg. For one EBM advocate, all medical interventions inherited from the Soviet era reside in a kind of purgatory of unproven effectiveness, awaiting to be redeemed or decisively damned by the highest level of testing:

> … they don't have evidence of efficacy. Maybe they were tested, of course, in those scientific trials of, let's say, not high evidence level that

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[340] ST, phone interview, June 2006.
[341] The phrase "international medical community" here refers to the English-speaking medical community, primarily to the US, UK and Canada.

> allows us to make conjectures that maybe there's something there, or maybe not, or maybe there is. But in the highest level of trials they were not properly tested. This means they don't have proven efficacy. It doesn't mean that they are not effective—we don't know that because we haven't tested them.[342]

Sometimes, the value of local knowledge and practices was acknowledged, but they were still seen as not sufficiently trustworthy, and therefore in need of confirmation. A physician occupying a high-level administrative position explained his views on the 'traditions of the fatherland's medicine' as follows:

> Let's say certain schools that have, naturally, a certain amount of experience treating patients with a particular pathology. ... It worked out that they treated tens of thousands of patients with this pathology, and in the periphery these patients were seen as single cases. That's why they, on the basis of their experience treating these patients, created their principles, recommendations that formed the basis of our textbooks and that direct our practices to this day. But nonetheless this experience is still not international experience. The quality of clinical trials that were conducted in these centers today don't deserve the kind of trust that we have in trials of an international level.[343]

For this physician, practices and guidelines formulated in the Soviet era could not be dismissed, nor could they be considered untested, since they were based on very extensive clinical experience. But they nevertheless required external (that is, not simply Russian) validation. They had to be compared to the evidence provided by the 'international' medical community.

In addition to defining the Russian medical community's relationship to colleagues abroad, EBM provided a useful tool for mobilizing the medical community and laying firmer foundations for professional unity—an important service in the context of post-Soviet transition, where the ground underneath the physician's feet was

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[342] NE, personal interview, Stavropol, July 2005
[343] YE, personal interview, Stavropol, July 2005

continuously shifting. Advocates used EBM as a foundation for a renewed professional identity. This hope for EBM came through very clearly in one of my interviews with a medical student who was involved with a voluntary EBM student group:

> It is clear why we got together, why for us-we-to put it bluntly, won't disperse [our group] even if we won't get funding for this. Because this is an essential need, primary need first of all of colleagues that understand you-we-I myself understand that if, for example, I will work in Stavropol region, without brothers-in-arms. Friends and colleagues that are working, trying to put these principles into practice, are coming up against a wall of misunderstanding. That is frightening. That is one of the components of a person, like family, successful work, is exactly this understanding, colleagues, brothers in arms. That means, that's why I personally am willing to do this in part on this [voluntary] basis, because-because, well, there's absolutely no other way.[344]

By invoking the western origins of EBM, Russian practitioners were not only trying to align themselves with global biomedical practices—they were also distancing themselves from the legacy of Soviet biomedicine, actively redefining the epistemic culture that underpins their practice. This surrender of one's culture and imagination for a culture perceived to be more powerful is the essence of colonization.[345] It would be wrong to conclude, however, that the adoption of EBM in Russia was a straightforward example of the successful colonization of the Russian medical mind by western proponents of EBM.[346] Although Russian physicians have eagerly seized on EBM discourse to bolster their position and further their attempts at directing reforms (and although for some younger physicians EBM did seem to be a fervently held creed), many

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[344] ON, personal interview, Stavropol, July 2005

[345] Partha Chatterjee, "Whose Imagined Community?," in *Mapping the Nation*, ed. Gopal Balakrishnan (Verso, London, 2000): 214-225.

[346] Ashmore, Mulkay and Pinch have argued that health economists' attempts to persuade the National Health Service that economic methods can be useful in the provision of healthcare services is an attempt at colonizing the medical mind for economic thinking. Malcolm Ashmore, Michael Mulkay and Trevor Pinch, *Health and Efficiency: A Sociology of Health Economics* (Milton Keynes: Open University Press, 1989).

saw EBM as only a means to an end and not necessarily an end in itself. This was especially true for the older generation of physicians, who took a pragmatic approach to EBM and clearly saw its limitations. This became clear in an interview with a very prominent advocate of EBM:

> The thing is, EBM is conceptually very simple and at the same time conceptually attractive. … people that are educated in philosophy, in history, they understand that it is primitive, in the intellectual sense primitive. But the majority of people don't read Hegel, yes? And that's why for them with its simple references of efficacy and verification it seems attractive. I don't think there is anything bad about that, it's natural. At three it is natural to play in the sand box, at fourteen to think your parents are out of date, at thirty to understand that at fourteen you were a jackass-same here.[347]

Although some advocates saw EBM as a limited concept, they were nonetheless actively applying it to a number of uses on behalf of the Russian medical profession. As outlined above, one of the uses of EBM was to assert the unique authority and expertise of the medical profession. Another important function of EBM was as a crucial tool for communicating with administrators and government bureaucrats. As the same EBM advocate observed, "for them evidence-based medicine is also accessible. And as a result the opinion of EBM doctor, from the EBM position, easily finds its way into the heart of the administrator."[348]

Employing EBM to communicate with the powers that be was an application of EBM discourse that Russian advocates have developed in part by watching Western medical experts wield this tool:

> I think EBM as a concept, as practice, is important for Russia. But this isn't my discovery, it is widely used, used by international organizations, organizations that provide technical assistance … British organizations,

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[347] AN, personal interview, Moscow, August 2006.
[348] AN, personal interview, Moscow, August 2006.

> Norwegian, American, as a means of bringing to life in Angola, Russia, Kyrgyzia those programs that in a different wrapping would pass with difficulty.[349]

And, as this interview excerpt subtly illustrates, it was not an easy lesson to learn. This was because for the Russian medical profession EBM was not just the 'international' benchmark for which physicians can strive in their practice—it was also a reminder of how much ground Russian biomedicine had lost with the collapse of the Soviet Union. Going from one of the most powerful medical establishments in the world whose accomplishments were widely acknowledged and whose methods served as a model for other countries (particularly in the developing world), the Russian medical profession now found itself on equal footing with 'third world' countries.

Thus for its most influential and sophisticated advocates, EBM was not so much a credo as an instrument for achieving professional goals. And while the advocates attribute great importance to the introduction of EBM into Russia, even as they are dedicating the majority of their time and energy to advocating this approach and educating their colleagues about EBM, they are already beginning to look past this movement, to the next phase of the reconstruction of Russian medicine. This came through in an interview with another prominent EBM advocate:

> …lately I've been thinking about how to… Well if you take any of ours or a Western journal, 90 or 95% of the information is their research, evidence, what's better or worse… But in the work of a physician evidence is about 15-20 percent. Everything else is things connected to human interaction, with the development of the individual, etc. It seems to me that now we need to think about how to re-work this literature… in order to develop physicians as individuals.[350]

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[349] AN, personal interview, Moscow, August 2006.
[350] NI, personal interview, Moscow, August 2006.

This concern with the development of the physician as an individual, coming from an advocate of EBM, may at first seem surprising. Not that EBM is not concerned with individual physicians—EBM advocates in the west make educating individual physicians in the principles of EBM one of the top priorities of the movement. But the intent of this education is precisely to limit the individuality of the physician, to reduce its impact as a variable in clinical practice. One of the promises of EBM is to reduce the variation in medical practice that results from different levels of physician experience and the inconsistencies of medical training. But this desire to invest in and empower the individual makes sense, when considered in light of the epistemic commitments of Soviet biomedicine, in which the individual practitioners occupied a central role both in the production and application of biomedical knowledge. It also attests to the importance of the legacy of the Soviet biomedical epistemology.

Is the new the thoroughly forgotten old?

In conclusion, the primary role of evidence-based medicine in the Russian context seems to have been as a discursive tool, which its advocates were using both to reinvent the Russian medical profession and to redefine its relationship to the world. While the material and institutional conditions of the Russian healthcare system made the implementation of EBM into day-to-day practice difficult (to say the least), its discursive power should not be underestimated. The discourse of *dokazatel'naia meditsina* was being asked to perform several roles.

On one level, EBM discourse in Russia was used in much the same way as in the west—as a tool that could be deployed in the struggle for professional authority, which

was being threatened by institutional actors such as the state, the newly minted insurance companies and the increasingly influential pharmaceutical industry. It was also an important factor in the way the Russian medical profession was redefining its relationship to the world on a global level. Its importance in the Russian context also signifies how low the status of Russian medicine has fallen; yet at the same time it provided a benchmark towards which the medical profession had to strive in order to once again become a respected member of the international medical community.

In addition EBM in Russia was a strategic discourse through which the Russian physicians were redefining their professional identity—re-evaluating their professional past and charting a new course for the future. This re-definition project is ongoing and extensive, encompassing the epistemic foundations of Russian medicine. And it is necessary. The collapse of the Soviet Union has been accompanied by changes in the institutional organization of Russian biomedicine, a reconfiguration of its practices and a profound transformation in its normative structures. And although the emerging Russian biomedical epistemology in many ways bears the stamp of its Soviet predecessor, it cannot but be qualitatively different.

CONCLUSION

This is the way the world ends
This is the way the world ends
This is the way the world ends
Not with a bang but a whimper
T.S. Eliot, The Hollow Men

In December of 1991 the Soviet Union was dissolved. The imposing Soviet empire collapsed like a house of cards with barely a whimper. In some sense, the event really was the end of the world, or at least it was the end of *a* world. As Susan Buck-Morss argues in her provocative meditation on the collapse of faith in the modernizing process, "this fundamental shift in the historical map shattered an entire conception of the world, on both sides [of the East-West divide]."[351] Her account draws out the similarities between the capitalist and socialist conceptions of the world, arguing that, far from being capitalism's polar opposite, Soviet socialism was in fact deeply rooted in the Western modernizing tradition and that its collapse calls into question the Western narrative as well.

Buck-Morss' work brings up an important question: what exactly was lost in this collapse? In this dissertation, I have offered a partial answer, arguing that the collapse of the Soviet Union has resulted in an epistemic shift in Russian biomedicine. The epistemic culture of Soviet biomedicine, once instantiated in the institutions of the system of health protection, the practices of clinicians, and the discursive practices of the field, has been giving way—though what precisely is going to take its place remains unclear. As I have shown in chapter four, the crisis of the Soviet system of health protection has eroded both the institutional and normative structures on which this system was dependent, creating a

[351] Susan Buck-Morss, *Dreamworld and Catastrophe: the Passing of Mass Utopia in East and West*, (Cambrdidge: MIT Press, 2002), x.

need and space for new approaches. A vocal and growing group of Russian physicians have seized on the rhetoric of evidence-based medicine to formulate such an approach.

What will come of these efforts remains to be seen. Although EBM has come to dominate Russian biomedical rhetoric, there is a great deal of continuity between the Soviet and Russian period at the level of institutions, practices and norms. Change is proceeding slowly and erratically. As one EBM advocate complained,

> … the healthcare system, the one we have now, it's so to say perpetually under reform, and at the same time there is an absence of reform, yes? Because there is no understanding in society of what needs to be done, no consensus and that's why it's always getting postponed.[352]

But, although there is no way of knowing what the outcome will be, the intentions of the advocates are clear.

Proponents of EBM are trying to accomplish several things. First, they are trying to redefine the relationship between biomedicine and the state. As I argued in chapter one, Soviet biomedicine was an integral part of the Soviet state, with the pinnacle of the profession integrated into state structures at the highest levels. While with the collapse of the Soviet Union the state was very quick to relinquish its support for the system of health protection, it did not relinquish its control. Reasserting professional authority through EBM rhetoric is an important strategy in the process of loosening state control and integrating Russian biomedicine into the fledgling market economy.

Second, EBM advocates seek to redefine biomedical epistemology—to alter the practices of knowledge production by taking those practices out of therapeutic medicine and abstracting out the subjectivities of both clinicians and patients. This is an important strategy in the process of repositioning Russian biomedicine within the global biomedical

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[352] NI, personal interview, July 2006.

enterprise. As I have argued in chapter one, Soviet biomedical practitioners envisioned an important role for themselves on the global stage as the purveyors of a superior methodology of healthcare provision. With the collapse of the state structures that supported their role on the global stage, Russian biomedical practitioners suddenly found themselves relegated to the role of recipients of both material and methodological aid. As I have demonstrated in chapters two and three, this goal represents a stark departure from Soviet biomedical epistemology, which insisted on the hybrid nature of medical science and reserved an important role for individual clinicians and patients for producing and applying biomedical knowledge.

These features of Soviet biomedical epistemology may seem incongruent with the collectivist ideology espoused by the Soviet propaganda machine and the existence of a centralized state bureaucracy (also espoused by the propaganda machine). And indeed they are incongruent. But rather than merely noting the paradox, I want to argue that its presence indicates that we need to reconsider our assumptions about what the Soviet Union was like, especially in the period of late socialism. Both ideology and hierarchy were declining in importance throughout this period, and centralized state bureaucracy of planning and control was simply not working as intended.

The third goal of EBM advocates is to redefine the professional identity of the biomedical practitioner. Soviet deontology defined biomedicine as a calling, not just a profession. Being a physician was something one *was,* not something one *did*, and in taking the Hippocratic oath the Soviet physician was (at least in principle) committing to a life long project of continuous self-improvement. EBM advocates are attempting to

redefine these normative structures and recast biomedicine as a profession guided not by a concept of duty but by a commitment to methodology.

This redefinition of normative structures has important implications for knowledge production practices. In the Soviet epistemic tradition the validity of knowledge was guaranteed by the integrity of the individual practitioner, which meant that while knowledge could not be produced by anyone, it could be produced just about anywhere. Biomedical researchers were empowered to alter their methods to accommodate their conditions of practice without fear that doing so would automatically render their data illegitimate.

For EBM advocates, valid biomedical knowledge is knowledge produced through the implementation of an appropriate methodology, and, although there are multiple methodological possibilities, they are not equal—some are better then others. The best method is the one that most completely purges the subjectivity of the individual from the process of knowledge production. A consequence of this is that while valid knowledge can in principle be produced by anyone, it can't be produced just anywhere—knowledge production requires access to the various technologies of mediation that can be relied upon to achieve objectivity.

Both EBM and the Soviet epistemic culture that it seeks to replace is, each in its own way, an adaptation to the conditions of practice prevalent in a particular period. That is, each epistemic culture is a response to the political economy. But neither biomedical system *is* the political economy. What keeps biomedical epistemology from collapsing into political economy is culture. Cultural norms and practices are just as important to the constitution of biomedical epistemology as they are for political and economic

institutional structures, and they endow the individual with the power and the responsibility to get out of the rubber cage.

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