Was There Ever a "Stalinist Science"?

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Alexei B. Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists*. 384 pp. London: Imperial College Press, 2004. ISBN 1860944205. $36.00.

Eduard Izrailevich Kolchinskii, *Biologiia Germanii i Rossii–SSSR v usloviiakh sotsial'no-politicheskikh krizisov pervoi poloviny XX veka (mezhdu liberalizmom, kommunizmom i natsional-sotsializmom)* [Biology of Germany and Russia–USSR amid the Sociopolitical Crises of the First Half of the 20th Century (among Liberalism, Communism, and National Socialism)]. 636 pp. St. Petersburg: Nestor-Istoriia, 2007. ISBN 5981871725.

Anastasiia Mikhailovna Korzukhina, *Ot prosveshcheniia k nauke: Fizika v Moskovskom i S.-Peterburgskom universitetakh vo vtoroi polovine XIX v.–nachale XX v.* [From Enlightenment to Science: Physics in Moscow and St. Petersburg Universities in the Second Half of the 19th and the Beginning of the 20th Centuries]. 261 pp. Dubna: Feniks+, 2006. ISBN 5927900666.

E. B. Muzrukova, ed., *Nauka i tekhnika v pervye desiatiletiia sovetskoi vlasti: Sotsiokul'turnoe izmerenie (1917–1940)* [Science and Technology in the First Decades of Soviet Power: The Sociocultural Dimension, 1917–40]. 495 pp. Moscow: Academia, 2007. ISBN 5874442537.

Ethan Pollock, *Stalin and the Soviet Science Wars*. 288 pp. Princeton, NJ: Princeton University Press, 2006. ISBN-13 978-0691124674, $35.00 (cloth); 978-0691138251, $24.95 (paper).

Susan Gross Solomon, ed., *Doing Medicine Together: Germany and Russia between the Wars*. xvii + 533 pp. Toronto: University of Toronto Press, 2006. ISBN 0802091717. $68.00.

Kritika: Explorations in Russian and Eurasian History 9, 3 (Summer 2008): 625–39.

Was there ever really such a creature as "Stalinist science"?[1] The term carries the perplexing heft of an oxymoron. If it is "Stalinist," then it is repressive, controlled, borderline irrational, and focused on power; if it is "science," then it is universal, disinterested, self-regulating, and focused on knowledge. There was obviously plenty of science during the period of Stalin's domination of the Soviet power structure (1927–53), but broad disagreement continues over whether (and if so, to what extent) we as scholars should call this science "Stalinist." Did it partake of the Soviet experience in anything other than temporal coincidence? If there was such a thing as "Stalinist science," then how does it change our understanding of the ostensibly irrational features of Stalinism? Or the rational features of science?

This is not merely an abstruse question that concerns the few historians and sociologists of science (and the odd philosopher) who investigate the Soviet epoch—such as the scholars involved in the six volumes under review. The issue of Stalinist science should be central for all historians of science, but even more so for historians of the Soviet Union. For the Soviet Union was, if anything, in large part a scientific and technological project—and every component of that project can be tied to Stalin and Stalinism. Stalin himself was instrumental in the three most transformative developments in science during the entire Soviet period: the Bolshevization of the Academy of Sciences (1928–32); the construction and detonation of the first Soviet atomic bomb (1949); and the criminalization of genetics and the granting to Trofim Denisovich Lysenko control over the life sciences in the communist world (1948).[2] Each formed not just a significant episode in the development of science in the Soviet Union; each also represented key transformations in Stalinist governance *tout court*—the set of institutions, practices, and,

[1] The term appears repeatedly in the scholarship on the history of Russian science, but perhaps never more blatantly than in the unsubtitled history of the Lysenko affair: Nikolai L. Krementsov, *Stalinist Science* (Princeton, NJ: Princeton University Press, 1997).

[2] Even the launch of Sputnik I (1957), that quintessential Soviet technological achievement, although it took place in the reign of Nikita Khrushchev, was both initiated by Stalin and made successful largely by the science policies pursued by his regime. See Asif A. Siddiqi, *Sputnik and the Soviet Space Challenge* (Gainesville: University Press of Florida, 2003). On the Bolshevization of the Academy of Sciences, see Loren R. Graham, *The Soviet Academy of Sciences and the Communist Party, 1927–1932* (Princeton, NJ: Princeton University Press, 1967); and Alexander Vucinich, *Empire of Knowledge: The Academy of Sciences of the USSR (1917–1970)* (Berkeley: University of California Press, 1984). The historiography on the Soviet atomic bomb has ballooned, especially in Russian, since David Holloway's *Stalin and the Bomb: The Soviet Union and Atomic Energy, 1939–1956* (New Haven: Yale University Press, 1994). See, for example, Vladimir Pavlovich Vizgin, *Istoriia sovetskogo atomnogo proekta: Dokumenty, vospominaniia, issledovaniia* (Moscow: Ianus-K, 1998–); E. A. Negin et al., *Sovetskii atomnyi proekt: Konets atomnoi monopolii. Kak eto bylo* (Nizhnii Novgorod: Izdatel'stvo "Nizhnii Novgorod," 1995); and Vladimir Gubarev, *Belyi arkhipelag Stalina: Dokumental'noe povestvovanie o sozdanii iadernoi bomby* (Moscow: Molodaia gvardiia, 2004).

crucially, *concepts* by which the regime exerted control over both its intellectuals and the rest of the population.

For almost 60 years, writing on Soviet science in general, and Stalinist science in particular, has been dominated by one major episode: the Lysenko affair. As has been described many times—Lysenkoism remains far and away the most comprehensively covered aspect of the history of Soviet science, ahead of even nuclear weapons and the space race—in the August 1948 session of the Lenin All-Union Academy of Agricultural Sciences (VASKhNIL), Academy President T. D. Lysenko, reading a speech we now know to have been edited by Stalin, declared genetics to be a "bourgeois science" and proscribed further research into the field, thereby officially abolishing genetics until after the mid-1960s and the removal of Nikita Sergeevich Khrushchev's continued support.[3] Despite its Khrushchevian end, Lysenkoism was a creature of Stalinism all the way through. Born into a Ukrainian peasant family in 1898, Lysenko benefited enormously from the expansion of education into the lower classes in the years following the Revolution, eventually embarking on research in plant physiology. His findings indicated, by his lights, that inheritance of acquired characteristics (often dubbed by his critics as Lamarckianism or, more accurately, "neo-Lamarckianism") seemed to be a prime force behind speciation in plants; and he claimed to be able to transform strains of summer wheat into winter wheat by subjecting the plants to repeated cold and stress treatments. This process of "vernalization" (*iarovizatsiia*) was supposed to "shatter" the heredity of the plant and render it susceptible to environmental influences.

For the most part, the vibrant Soviet genetics community ignored what it construed as a display of pseudo-science instead of confronting it head on. After 1931, Lysenko managed to find powerful patrons in the Communist Party, including the personal notice of Stalin, and when he dressed up his theories in the language of dialectical materialism under the title "Michurinism" (after the Luther Burbank-esque Russian figure of Ivan Vladimirovich Michurin), he gained greater and greater support through the 1930s. His influence declined during the war years, and right on the verge

[3] The scholarship here is vast. For some of the classic English-language treatments, see Krementsov, *Stalinist Science*; David Joravsky, *The Lysenko Affair* (Cambridge, MA: Harvard University Press, 1970); Loren R. Graham, *Science, Philosophy, and Human Behavior in the Soviet Union* (New York: Columbia University Press, 1987), 102–56; Nils Roll-Hansen, *The Lysenko Effect: The Politics of Science* (Amherst, NY: Humanity Books, 2004); Zhores A. Medvedev, *The Rise and Fall of T. D. Lysenko*, trans. I. Michael Lerner (New York: Columbia University Press, 1969); and Valery N. Soyfer, *Lysenko and the Tragedy of Soviet Science*, trans. Leo Gruliow and Rebecca Gruliow (New Brunswick, NJ: Rutgers University Press, 1994). Lysenko's speech was immediately released internationally and was widely available in English as Trofim Denisovich Lysenko, *The Science of Biology Today* (New York: International Publishers, [1948]). On Stalin's editing (and general toning-down) of the VASKhNIL speech, see Kirill Rossiianov, "Editing Nature: Joseph Stalin and the 'New' Soviet Biology," *Isis* 84, 4 (1993): 728–45.

of his complete eclipse, in 1948, he was catapulted to power. His downfall, spearheaded in part by untouchable Soviet nuclear physicists, uncovered fraud and corruption at almost every level. Lysenko remained a pariah until his death in 1976 (although he was never stripped of his membership in the Academy of Sciences).

It is to the great credit of all the volumes here that they attempt, in different ways, to remove Lysenkoism from the center of our accounts of Soviet science, dethroning it from the case that was most "characteristic" of Stalinist governance of science, to one of many such methods of governance, and an extreme and aberrant one at that. Of course, I am not helping these historians' quest to mitigate the overweening dominance of this figure—a goal to which I emphatically subscribe—by framing all these works around the question they seek to displace. But Lysenko is simply too large a problem not to be always looming in the background, and the success of these volumes hinges not just on their adding supplementary scientific episodes but on their ability to explain the Lysenko affair by "domesticating" it within a broader framework.

There are a few ways of going about this de-Lysenkoization of the historiography of Soviet science, and various ones are employed (sometimes in combination) to quite impressive effect in each of the volumes under review. First, one might remove the focus from biology to another scientific discipline—say, physics—which not only did not undergo ideological purging in the Stalin years but rather experienced its most sustained, vigorous, and exciting growth. This is the path pursued by Alexei Kojevnikov in *Stalin's Great Science*, his collection of essays (almost all previously published, but often in history of science journals that Soviet historians tend not to consult on a regular basis). Kojevnikov's work deserves to be taken very seriously indeed. His arguments are highly original, even when broaching well-trodden territory; and his collected interpretations amount to a substantial revision of the picture of physics in the Stalin period, a revision which is not only overdue but is almost certainly correct.

Physics—especially theoretical physics—poses a real problem for the Lysenko-centric picture of Stalinist science, what Kojevnikov calls its "main paradox" (xii). Privileging Lysenko (or genetics) in an account of science in the 1930s and 1940s generates a tone very much in the minor key: a tale of ideological interference and the perversion of a once vibrant science into a tragic self-mockery. Theoretical physics, by contrast, experienced its greatest triumphs in Russia in precisely these years, emerging from a neglected field in tsarist times (compared to, say, chemistry or physiology) to the breeding ground of such figures as Lev Davidovich Landau, Igor′ Evgen'evich Tamm, Petr Leonidovich Kapitsa, and a great many others.[4] Lysenko-obsessed

[4] Kojevnikov's analysis of the growth of this community in the 1920s and 1930s complements earlier studies by Paul R. Josephson, *Physics and Politics in Revolutionary Russia* (Berkeley:

Sovietologists once used to dismiss this efflorescence of one of the most demanding and prestigious of the sciences (at least a decade before the atomic bomb made such a coterie of talent necessary for national security) as either a sideshow or a miraculous survivor of Stalin's ignorant nihilism.

Kojevnikov's argument, stated baldly, is that Soviet physics did not so much persist and thrive *despite* Stalinism but *because* of it. It is difficult to get a full sense of the variety of careers for physicists in the Stalinist context in the space of a brief review, precisely because, as Kojevnikov documents, those careers were so varied. Soviet physics was not grown autarkically, as the time abroad in both Landau's and Kapitsa's cases demonstrates (the former supported by Rockefeller Foundation money, the latter by an extended stay in Ernest Rutherford's Cavendish Laboratory in Cambridge) (85). It was not "shielded" by nuclear weapons, because the growth of the science preceded Soviet proliferation efforts (and, in fact, made the rapid development of a Soviet plutonium bomb possible). These physicists, moreover, were not hostile or immune as a group to Marxist dialectical materialism. Far from it. The most intriguing set of essays in this book concerns the work by Tamm, Iakov Frenkel′, and others concerning "collective excitations"—a physical technique whereby microphysical explanations are replaced by a description of the whole body being modeled. The terminology is patently Soviet, and Kojevnikov makes a case that the concept behind it was born out of the furnace of Soviet debates on the nature of free will and individual action. The quest for a replacement of the problematic band theory of solids eventually proved fruitful in ways earlier unimagined: "After several years of use, the notion of 'collectivized electrons' was no longer just a figurative metaphor for Frenkel, but the appropriate description of the complex reality within metals and the basis of his understanding of the physics of solids" (63). Not only was the philosophy not "window-dressing," it provided a critical resource for the development of a novel (and still indispensable) physical interpretation that finds application in virtually every domain of solid-state physics.[5]

Kojevnikov is highly eclectic in his search for models to explain the heterogeneity of his cases. For example, in his compelling portrait of the physicist Sergei Ivanovich Vavilov, president of the Academy of Sciences under Stalin and the brother of Nikolai Vavilov—a geneticist and the most prominent victim of Lysenko's rise (Nikolai died of malnutrition shortly after being released from prison)—he relies on traditional methods of biography to

University of California Press, 1991); and Karl Hall, "Purely Practical Revolutionaries: A History of Stalinist Theoretical Physics" (Ph.D. diss., Harvard University, 1999).

[5] The classic study of how dialectical materialism served on occasion as a productive resource for Soviet science, and not merely as ornament or blight, is Graham, *Science, Philosophy, and Human Behavior*. On the heterogeneity of dialectical materialism in the Soviet context, see David Joravsky, *Soviet Marxism and Natural Science, 1917–1932* (New York: Columbia University Press, 1961).

avoid casting Sergei exclusively in the light of his brother.[6] In the later chapters of the book, in an effort to disqualify Lysenko's triumph as an inevitable corollary of Stalinist culture, he turns to "anthropology":

> Understanding the logic of a different culture—Stalinist culture in this case—asks for an anthropological approach. A regularity, indeed, can be found, but on the level of formal rules and rites of public behavior rather than in the contents and results of disputes. The analysis ... will reveal the rules of the Communist games of *diskussiia* (disputation) and *kritika i samokritika* (criticism and self-criticism). An inquiry into the rituals of Stalinist political culture and its special domain called "intraparty democracy" will then be needed to understand both the ascribed functions of these games and the possible motivations of politicians who proffered them to scholars as methods for handling scientific disputes. Provoked from above, scholars engaged in a variety of academic conflicts while pursuing their own agendas and inventively using available cultural resources in dialogues with politicians. An important feature of these games was that, in theory and often also in practice, their outcomes were not predetermined but depended upon the play. How scholars interpreted and exploited this particular feature will be shown by a new analysis of the Lysenko case of 1948.[7] (191)

This framework comes with strengths and weaknesses. We gain in seeing patterns that were earlier obscured, but we lose in causal explanation. In a sense, Kojevnikov commits the opposite sin from his cold-warrior bugbears: while they read Lysenko as typical of Stalinist science and therefore treat physics as an anomaly, he sees physics as typical and then chooses to show Lysenkoism as an anomalous product of a usually functional culture. If one has to choose between these interpretations, Kojevnikov's is most probably the better. But such a choice is not necessary. Both the Sovietological reading and Kojevnikov's stem from the idea that "science" under Stalin was in some important sense a unitary object, and the explanation of one discipline's (or subdiscipline's, or scientist's) fate must be linked to the fate of the whole. Such a proposition is to be proven, not assumed.

The most serious flaw in Kojevnikov's excellent account is his treatment of physics (and science in general) before 1917. Even here, however, he breaks older taboos. Histories of Soviet science have often ignored the implications of the fact that (Western elite) science has been a presence in Russia since at least the founding of the Academy of Sciences by Tsar Peter the Great in

[6] The approach taken, for example, in David Joravsky, "The Vavilov Brothers," *Slavic Review* 24, 3 (1965): 381–94.

[7] These arguments may be more familiar to historians in their original form: Alexei Kojevnikov, "Rituals of Stalinist Culture at Work: Science and the Games of Intraparty Democracy circa 1948," *Russian Review* 57, 1 (1998): 25–52.

1725. As a result, they are astonished by the vibrancy of the Russian scientific community in the 1920s and interpret this as a (usually positive) reaction to the Bolshevik Revolution. As Kojevnikov recognizes, this picture is nonsense. In his first two chapters, he deftly traces the tsarist state's mobilization of science and technology to fight World War I. After hostilities erupted with Germany, certain crucial components of the war effort, such as optical glass, which used to be imported from the Reich, needed to be replaced with domestic efforts. Systematic exploitation and organization of Russian science (the most prominent example is KEPS, the Commission for the Study of the Natural Productive Forces of Russia, proposed by Vladimir Ivanovich Vernadskii and established in 1915) led to a tremendous integration of science directed toward applied ends in state-funded institutes.[8] Kojevnikov also rightly interprets this as the seed of the massive proliferation of these specialized institutes under the Bolshevized Academy of Sciences in the 1930s.[9]

The problem in Kojevnikov's account is that he moves the absolute caesura from 1917 to 1914. According to him, in tsarist Russia, "industry offered practically no career opportunities for [scientists], and the only available jobs for scientists in Russia—with very few exceptions—existed at universities and other teaching institutions" (1). To demonstrate this claim that the dominant ethic was "science for science's sake" in prewar Russia, he cites the complaints of Dmitrii Ivanovich Mendeleev and Vladimir Vladimirovich Markovnikov (both chemists) about this attitude. Unacknowledged is the deliberate polemical exaggeration in these quotations as a bid to get the state to support their specific applied proposals *even more* than it already did. Aside from tremendous scientific involvement in the Baku oil industry after 1863, pharmaceuticals, metallurgy, mining, and textile production all employed significant numbers of Russian scientists. The reason for the myopia on Kojevnikov's part stems from a touch of polemical exaggeration but also from his focus on physics—truly underdeveloped in the imperial period—and not enough attention to medically useful physiology or to chemistry, the most professionalized and marketable science in the empire.[10] That criticism aside, Kojevnikov's volume needs to be taken as a major milestone in our understanding not just of how science flourished under Stalin but also of how Stalinism as a whole operated in domains outside of science.

[8] On Vernadskii, see Kendall E. Bailes, *Science and Russian Culture in an Age of Revolutions: V. I. Vernadsky and His Scientific School, 1863–1945* (Bloomington: Indiana University Press, 1990).

[9] The reference point, again, is the work of Loren R. Graham, "The Formation of Soviet Research Institutes: A Combination of Revolutionary Innovation and International Borrowing," *Social Studies of Science* 5, 3 (1975): 303–29.

[10] On these two sciences and their positions in tsarist Russia, see, respectively, Daniel P. Todes, *Pavlov's Physiology Factory: Experiment, Interpretation, Laboratory Enterprise* (Baltimore: Johns Hopkins University Press, 2002); and Michael D. Gordin, *A Well-Ordered Thing: Dmitrii Mendeleev and the Shadow of the Periodic Table* (New York: Basic Books, 2004).

It might be difficult for the historian not acquainted with the development of physics in the Russian context to truly grasp the dramatic surge of quality, quantity, and prominence of physics and physicists in the Stalin period. Under the tsars, physics was poorly developed in basically every sense. A. M. Korzukhina's *From Enlightenment to Science* traces the history of Russian physics from the University Statute of 1863—whose liberalizing reforms she takes as setting the conditions for the development of a professionalized scientific discipline—to 1917. Her approach is a conventional (and for Western historians of science decidedly old-fashioned) disciplinary analysis, but her research is careful and provides a coherent account of the growth of professorships, journals, and scientific organizations. The reader learns very little about either the content of the physics (what kinds of research were they engaged in?) and the context (why physics? why then?), and in this sense Korzukhina falls considerably short of the rich texture of Kojevnikov's book. The most important point in the book is her stress (22–23) on the inversion of academic hierarchies in mid-19th-century Russia: from the Academy of Sciences as the center of physics (and other disciplines) to the partial decentralization of the scholarly vanguard to the universities, especially in Moscow and St. Petersburg. As she puts it, with only minimal exaggeration: "Almost all events in Russian physics at the end of the 19th and beginning of the 20th centuries directly or indirectly were related to Moscow or St. Petersburg" (125). Korzukhina fills in some of the background to Kojevnikov's revaluation of Stalinist science, but she does not draw out the implications of university autonomy, self-organization, and international contacts from the late tsarist period to the contrast presented by the fate of genetics in 1948. This narrative is in the background of any Russian reader's mind, however, and by implication she reinforces the model of how science works "normally" (in the classic Western sense), and thus one where Lysenkoism is an aberration attributable to the Soviet period.

There are other ways to reposition Stalinist science than by shifting focus to a different discipline. A second tactic places the emphasis in the affair not on *biology*, but on Soviet science policy more broadly, seeking to put the 1948 proscription of genetics into a pattern that reveals more clearly in just which ways Lysenkoism was typical or atypical of Stalin's governance. Ethan Pollock's *Stalin and the Soviet Science Wars* is the most sustained effort to date in this direction and contributes, of all these volumes, the most thorough exploitation of Soviet archives. The typical historians' tack in seeking to explain what happened at the 1948 VASKhNIL meeting has been to engage in diachronic explication, tracing back the roots of Lysenkoism to earlier periods. Pollock's account is unabashedly synchronic. Homing in on the epoch of "high Stalinism," he examines the VASKhNIL biology episode in juxtaposition with the five other postwar conferences to establish order and

coherence in Soviet scholarship across a diversity of fields: philosophy, linguistics, physiology, and political economy, as well as a scheduled conference on theoretical physics, which was canceled for reasons that remain obscure and the subject of much controversy (among both historians and Russian physicists). These conferences were clearly conceived in a series, but they all bore idiosyncratic markings, and it would be an error to take any of them as normative and see the others as deviations from it. Pollock's brief is to explore these conferences and to take seriously Stalin's personal intellectual involvement in each case, thus demonstrating how broader patterns show us the rationality behind the seeming chaos of Lysenkoism. In reference to the Alan Sokal–inspired "science wars" of the late 1990s, which raised accusations of charlatanism and shoddy scholarship in historical and sociological studies of science, Pollock insists that Stalin's science wars were deeply important to the conception of Stalinism as a way of life (2–3).[11]

Stalin's support of Lysenko was thus no aberration. In essence, Stalin sought to use these postwar "discussions" to reverse a series of relaxations of Soviet control of science dating from World War II, thus returning to the *status quo ante* of the 1932 restructuring of the Academy of Sciences. During the war, scientists involved in war research were allowed to work largely without party oversight, were able to participate in a global anti-fascist coalition of scientists, and were exposed—both via recruited German scientists or espionage material—to new technologies (radar, atomic weapons, jet propulsion, rockets).[12] Stalin wanted to reassert control, and he did so not just through coercion (the traditional Sovietological model) but by insisting that dialectical materialist ideology ought to be the uniform basis of the practice of Soviet science. The postwar clampdown on scientific autonomy was a disciplinary technique, a Cold-War propaganda move, and—Pollock's original contribution—a sincere expression of intellectual commitment on the part of Iosif Stalin.

There is no question that Pollock has a point about the coherence of these meetings and the value of looking at them together (something Kojevnikov also does through an interesting anthropological investigation of these sessions as "games" in his chapter 8). Distinctive in Pollock's telling are the evidence and the conclusion. The evidence is simply staggering: reams of

[11] On the all-too-recent "science wars," see Keith Parsons, ed., *The Science Wars: Debating Scientific Knowledge and Technology* (Amherst, NY: Prometheus Books, 2003); Keith M. Ashman and Philip S. Baringer, eds., *After the Science Wars* (London: Routledge, 2001); and the *locus classicus*, Andrew Ross, ed., *Science Wars* (Durham, NC: Duke University Press, 1996).

[12] N. S. Simonov, *Voenno-promyshlennyi kompleks SSSR v 1920–1950-e gody: Tempy ekonomicheskogo rosta, struktura, organizatsiia proizvodstva i upravlenie* (Moscow: ROSSPEN, 1996); and Christoph Mick, *Forschen für Stalin: Deutsche Fachleute in der sowjetischen Rüstungsindustrie, 1945–1958* (Munich: R. Oldenbourg, 2000).

archival correspondence and minutes concerning each meeting from inception through planning and execution. (In particular, in the case of political economy, historians have not even begun to reckon with the quantity of material at hand.) Through this kind of attention to primary material from all levels of elite science policy, Pollock can demonstrate both how much truth there is in traditional interpretations of these conferences as bureaucratic infighting and the limits of that kind of interest-group explanation. For there was something more at stake in these matters: Stalin actually *cared* about who was right in these disputes and in each case, overtly and covertly, put his hands on the scales out of what Pollock can only conclude was genuine conviction. Even though Pollock takes pains not to make his story exclusively about Lysenko, it is from his analysis of this episode in chapter 3 that one can see his case for intellectual content most explicitly:

> Stalin's editorial comments and written exchanges with Lysenko suggest that one of the most important reasons the leader threw the full weight of the Party behind "Michurinist biology" had to do with ideas, not politics. As odd as it seems to those of us who now accept the practical rewards of modern genetics, Stalin apparently believed that Lysenko was fundamentally correct about the inheritance of acquired characteristics and man's ability to use that knowledge to control nature. It is a sign of remarkable intellectual arrogance that Stalin would take it upon himself to tell scientists which ideas in their field were correct and which were not. Just the same, he also recognized that scientific theories had to at least appear to emerge victorious from a process that included the exchange of opinions, or discussions. This is the most logical way to explain his reluctance to simply pass a decree supporting Lysenko. "Correct," "objective" ideas could receive the Party's support, but they could not appear to have been dictated. (70)

So not only did Stalin have views about power politics, about dialectical materialism, or even about genetics, but he also had strong views about how "science" was supposed to operate. Lysenko did not triumph by claiming that his science was "proletarian"; Stalin changed the focus by declaring Michurinist biology ("creative Soviet Darwinism") to be *objective*. Western genetics was in error because *it* was contaminated by class content. Pollock's approach thus gives new content to the term "Stalinist science": science that operated according to the rules and parameters set by Stalin from epistemological grounds. As Pollock points out, the Lysenko affair may have been bad for genetics, but it was very good for the prestige of "science" as a domain of endeavor in the Soviet Union—Stalin made ideas and intellectual work the forefront of Soviet civilization.

Pollock's synchronic history displaces Lysenkoism's peculiarity as an issue in biology and makes it more of a *Soviet* espisode. A third approach

to de-centering Lysenko attacks the elephant in the room head on: it discusses the process of Lysenkoization of Soviet biology directly but juxtaposes it with the politicization of genetics in other nation-state contexts (such as Anglo-American eugenics or National Socialist racial hygiene). In this light, Lysenko appears as one manifestation of a more general process taking place globally (or at least in Germany and the Soviet Union) in the 1930s and 1940s. Eduard Kolchinskii's *Biology of Germany and Russia–USSR* is a superlative example of the benefits to be gained by taking a long-term diachronic and comparative look at how biology was politicized in the first half of the 20th century. Kolchinskii's volume is the product of over 30 years of research, and it shows: it is hefty, rich in detail, and comprehensive in citations to primary and secondary literature in Russian, German, and English. (The profusion of foreign-language secondary literature in the footnotes is particularly noteworthy; Kolchinskii is almost unique among practicing historians of science in Russia in his careful attention to and use of innovations in Western historiography in dealing with these matters.) Mostly this is a work of synthesis, pulling together strands from two rather separate (though parallel) historiographies, rather than a work of archival mining like Pollock's.[13] But what Kolchinskii is putting together here is nothing less than the imbrications of Russian and German life sciences in general—*not* just genetics but also evolutionary theory, anthropology, medicine—with communism and National Socialism, respectively.

In a sense, Kolchinskii's narrative is closest to the older Sovietological line: the story of Lysenko (and Nazi *Rassenhygiene*) is a tale of top-down politics in a totalitarian environment shaping and eventually almost ruining scientific endeavor. Kolchinskii is far more subtle than this crude formulation, however, and the account he gives shows how the sheer disruptiveness of the scientific milieu in these periods—two world wars, revolutions, civil war, and Stalin's Great Break for Russia; wars, depression, hyperinflation, and Hitlerism for Germany—*alongside* tremendous conceptual transformations in the life sciences over the same years, created conditions that were ripe for abuse. Scientists who were not willing to go along with the state's programs had no equivalent base on which they could draw to fight back. His explicit goal is to provide a full-scale study in biology of the influence of external factors on the conceptual content of science in the manner suggested in Paul Forman's classic account of acausal physics in Weimar Germany.[14]

[13] Similarly interesting comparative approaches to science under Stalin are Loren R. Graham, *Between Science and Values* (New York: Columbia University Press, 1981); Graham, *What Have We Learned about Science and Technology from the Russian Experience?* (Stanford, CA: Stanford University Press, 1998); and Paul R. Josephson, *Totalitarian Science and Technology* (Atlantic Highlands, NJ: Humanities Press, 1996).

[14] Paul Forman, "Weimar Culture, Causality, and Quantum Theory: Adaptation by German Physicists and Mathematicians to a Hostile Environment," *Historical Studies in the Physical*

For Kolchinskii, the key factor was war, which generated a climate of resource scarcity, which in turn forced scientists to compete for official favor with regimes less than fully committed to the scientific project. "Ethical and political compromises," he writes, "were inevitable" (43). His argument, as it develops over the following several hundred pages, is doggedly comparative, to good effect. An example:

> The form of organization of science in the Soviet Union for a long period guaranteed the support of parity in the arms race. Along with this the "iron curtain" and the ideology of "particularism" [*osobost'*] of Soviet science facilitated constant interference of party-government organs in the organization and functioning of the scientific community, which led to the domination of pseudo-scientific creations in separate areas of natural sciences, above all in biology....
>
> The ideologization of biology developed differently in Germany. German scientists already before World War I occupied a strictly conservative position, actively participating in the formation of imperialist nationalism. Professors not only supported the policy of the government, but they facilitated its "scientific" foundation, proving the necessity of the partitioning of the world and the civilizing role of Germany in the East. (45)

If anything, Soviet science comes off as the less "distorted" variant in this juxtaposition, since the coercion of Soviet scientists was less voluntary, their submission to the total domination of elites was of shorter duration, and the end result produced a dissident movement internal to itself (symbolized by Andrei Dmitrievich Sakharov, among others) who worked to overturn the oppressive context.

Kolchinskii's verdict on Stalinist science is thus rather restrained: the Bolsheviks were slow to corrupt science, and they did so to a degree analogous to (and possibly less severe than) what the Nazis did in the West. Stalinist science (a.k.a. Lysenkoism) was a product of the conditions of Stalinism and can tell us little in reflection about the culture of Stalin's order that we did not already know. This is basically a structuralist analysis, and the specific examples in such a framework are always less informative than the general conditions. We are left with a less judgmental and more international narrative than the traditional Sovietological one, but one that still treats the case of science as *sui generis*. Kolchinskii hesitates to draw larger lessons from his juxtaposition: "The experience of biology under

Sciences 3 (1971): 1–115. This highly influential piece has come under extensive attack and refutation over the decades but remains the primary reference for so-called "externalist" historians of science: those more interested in social factors in the development of science as opposed to the "internal" philosophical and cognitive aspects.

Stalin and Hitler, however, does not demonstrate that dictatorship destroys science. Although many scientists suffered, and in the USSR biology was dealt an essential devastating blow, in both countries it survived" (582). This is an optimistic story, and for the most part a true one, but it does little to tell us about the features of Stalinist science other than that it was science, and it happened under Stalin.

Finally, one can subvert the chronology altogether, and focus on the 1920s, before Soviet science was ever infected with the virus of Lysenkoism. Two recent edited volumes emphasize the development of varied communities of scientists, technicians, and physicians within the Soviet Union in the decade before Iosif Stalin assumed control and began (among other things) reformatting the country's scientific infrastructure. Susan Gross Solomon's edited volume, *Doing Medicine Together*, an outgrowth of a University of Toronto conference in 2000 on German–Soviet medical relations between the wars, brings together a series of generally excellent essays that explore medical cooperation between Weimar Germany and NEP-era Russia along a series of fronts: organizational, institutional, disciplinary, and biographical. The biographical is the most prominent. Five of the essays (almost half the volume) deal almost exclusively and in great detail with the mercurial German physician Heinz Zeiss (1889–1949), a politically right-wing scientist who spent the better part of the 1920s and early 1930s traveling around the Soviet Union in an attempt to develop his racialized theories of "geomedicine." The portrait is fascinating and brings into relief many of the ironies of cooperation: the strange political bedfellows; the significance of medicine as a ground of rapprochement in the spirit of Rapallo; the importance of financial backing for scientific developments; and the eventual erasure of this period of partial mutual understanding with the rise of Nazism and Stalinism on both sides of the divide.

That erasure is highly significant for deepening historians' understanding of this troubled period, and for the later developments under Stalin. For the picture of Soviet science that both Stalin (in part through his proxy Lysenko) and Sovietological historians of science present is of Soviet science as fundamentally autarkic, cleaved off the "normal" development of Western science by brute force and crude ideology. No one can read *Doing Medicine Together* and retain this misapprehension. It is too much of a stretch to call science in this period (perhaps in *any* period) "international" in essence: national borders do matter in each of the cases, particularly when money or institutions are at stake, but they represent resistances rather than absolute barriers. Keeping this two-way transfer in mind provides a useful corrective to Kolchinskii, who in his juxtaposition of these two countries often stresses their apartness more than their conjunctions.

E. B. Muzrukova's *Science and Technology in the First Decades of Soviet Power* is a more uneven and less coherent volume. The result seems to

intensify many of the presumptions that undergird the classic history of Lysenkoism: state influence on science is deleterious (with the exception of V. L. Gvozdetskii and O. D. Simonenko's interesting article on Lenin's electrification program, GOELRO); Russian scientists tend to operate exclusively in the context of Russia and Russian-language science; and the dominant story is one of partial success up to the point when World War II begins and the state starts to direct its attention to these scientists within its borders. Most of the articles in this volume (E. I. Kolchinskii's contribution on "proletarian biology" is an exception) cite almost no Western secondary literature, even when it is clearly germane.[15] The net result gives a feel, in many of the articles, of special pleading (Russian scientists did this first!) or an obsession with establishing intellectual genealogies of teacher–student in "schools" of various disciplines.

The phenomenon of the "school" (*shkola*) has permeated Russian-language history of science since at least the Stalin years. The goal was to write a history where teachers train a whole series of students to address a particular set of questions within a particular discipline and using a particular collection of tools and methods. The end result—usually praised—is a kind of discipline-within-a-discipline, fiefdoms of science that radiate from the "Pavlov school" or the "Ioffe school" or whatever school you think of.[16] Even Kojevnikov sometimes slips into this mode, but he (correctly, to my mind) points out that the *shkoly* functioned as real institutions and were products of the Stalinist organization of science itself, a kind of internal clan structure exemplified by Landau's students (265). In the case of many of the essays in Muzrukova, the concept is both anachronistic and without explanatory function—it invokes the coherence of a school as a causative factor in scientific development when that very coherence is what needs to be accounted for. Western historiography used to have this genealogical mania as well, but it has largely been discarded or heavily modified, for the good of history-writing as a whole, and *shkola*-discourse in this volume would clearly have benefited from more critical reflection. The preface declares that the "articles in this collection are made primarily in the genre of the social history of science. This direction of research is oriented to the explanation of the course of development of science, and not simply to its description. Experience has shown that this is achieved if cognitive aspects of science are examined together and in interaction with social aspects" (7). In this volume, resorting to some older Soviet-era crutches leaves the reader with the unnerving inference that there was a functioning scientific community that continued with an

[15] The GOELRO piece neglects the most comprehensive history of the program: Jonathan Coopersmith, *The Electrification of Russia, 1880–1926* (Ithaca, NY: Cornell University Press, 1992).

[16] Such as the generally excellent Iurii Ivanovich Solov'ev, *Istoriia khimii v Rossii: Nauchnye tsentry i osnovnye napravleniia issledovanii* (Moscow: Nauka, 1985).

unbroken line through the Stalin years. If so, how do you explain Lysenko? Or Lysenko's fall?

Taken together, these books do more to disassemble a flawed picture of what "Stalinist science" was than to create any new portrait. Given the tenacity and ubiquity of that classic picture of Stalinist science as the Lysenko affair writ large, the historians' acts of creative destruction are long overdue and greatly appreciated. Lysenkoism was clearly a central episode in the history of 20th-century science, and its effects on Soviet and post-Soviet biology are still being felt, but it does not serve as a microcosm of the vagaries of Soviet science as a whole, or even science in postwar high-Stalinist culture. It was not a symbol or stand-in for *anything*; it was merely a significant historical episode like many others (the "anti-cosmopolitan" campaign, the Leningrad affair, etc.) that reveal much about themselves and little about anything outside themselves. The persistence of the traditional Lysenko narrative tells us more about our expectations about the way science "should be" than about how science was in fact practiced in the Soviet Union.

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