

Lysenko Unemployed: Soviet Genetics after the Aftermath

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Abstract: The history of the so-called Lysenko affair—the domination of Soviet biology by the agronomist Trofim Denisovich Lysenko and the state-sanctioned proscription of genetics from 1948 to 1965—has for decades generated widespread political and historical interest as a textbook example of political distortion of science. Building on archival sources from the United States and Russia, this essay resituates the “ending” of this episode within the context of the Soviet science system, arguing against the prevalent “rise and fall” narrative. This re-periodization of the Lysenko episode allows an inquiry into how history and historiography mutually construct each other, as well as highlighting the significance of *disestablishing* discarded scientific institutions along with theories.

Perhaps it is time to explore in more detail what the Lysenko affair is really an example of.

—Mark B. Adams (1972)

We ought to know Trofim Lysenko’s story by now.¹ For a good half century this notorious episode in the history of twentieth-century science served as a cautionary fable about the dangers of politicians interfering in science, as well as a self-congratulatory morality tale related in the Western bloc about the necessary correlation of democracy and science (and the necessary scientific inferiority, *ipso facto*, of the Soviet Union). Lysenko’s career was well documented by contemporaries, and their accounts have been exhaustively supplemented by historians, leaving the story of his biological doctrines as the central episode in the history of Soviet science. Indeed, the overlap between those contemporaries and the first generation of historians, who together set a durable framework for the controversy, is the central subject of this essay. His-

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¹ For the epigraph see Mark B. Adams, “Genetics and the Soviet Scientific Community, 1948–1965” (Ph.D. diss., Harvard Univ., 1972), p. 14.

torical and historiographical interventions blended easily at the moment that observers considered the episode to be “over,” converging on a “rise and fall” narrative that, despite much excellent scholarship during the past two decades, has largely persisted, with noticeable consequences for our understanding both of the late Soviet Union and of biology. Familiarity with the outlines of that narrative is essential for what follows.

First, the rise. Trofim Denisovich Lysenko (1898–1976) was born into a peasant family in the Poltava region of what is today Ukraine. Absent the revolution that transformed the Romanov Empire of his childhood, that is likely where he would have remained, but the extraordinary opportunities proffered by the Bolsheviks led to his education as an agronomist at the Kiev Agricultural Institute. While working at an agricultural station in Azerbaijan in the late 1920s he came to the attention of the Soviet press. Over the course of the 1930s he moved rapidly to the forefront of Soviet biology. This rise was propelled by his personal biography, by the appeal of the practical techniques (such as exposing seeds to cold) known as “vernalization” (*iarovizatsiia*), intended to improve the hardiness of crops in harsh growing conditions, and by the dialectical-materialist gloss that advisors such as I. I. Prezent attributed to his notions of heredity. His complex of ideas soon cohered into a neo-Lamarckian doctrine of the inheritance of acquired characteristics, “Michurinism,” named after the idiosyncratic Russian plant breeder I. V. Michurin (1855–1935). Although the Soviet Union had been, in the 1920s, one of the most important sites for international research in genetics (especially population genetics), by the end of the following decade there was a full-blown ideological war between “Michurinists” and classical geneticists, whom the former called, somewhat floridly, “Mendelist-Weismannist-Morganists.” This was a genuine struggle between well-matched adversaries, with sometimes the geneticists prevailing over the Michurinists, sometimes the reverse. The geneticists received a harsh blow when the leading seed geneticist and former president of the Lenin All-Union Academy of Agricultural Sciences (VASKhNIL), Nikolai I. Vavilov, was arrested in 1940 under the common allegation of “wrecking” and Lysenko took his place at the helm. (Vavilov died in a Saratov prison in 1943, from malnutrition.) On the other hand, during World War II Lysenko’s brother defected to the Germans, which boded ill for the agronomist.

In 1948 the balance tilted. At the conclusion of the infamous “August Session” of a VASKhNIL conference that year, Lysenko announced that his doctrine of the environmental pliability of heredity had been approved by the Central Committee of the Communist Party (that is, by Joseph Stalin), and classical genetics was proscribed as bourgeois pseudoscience. Lysenko was regnant, the geneticists were routed, and—so the story is told when following the wrecked careers of the geneticists—true biology withered in the Soviet Union for over fifteen years. Lysenko weathered the transition from Stalin to Nikita Khrushchev (who was partial to the agronomist), and genetics was officially banished from the life sciences until the latter was ousted as Soviet Premier in October 1964. Khrushchev’s fall carried Lysenko in its wake. In 1965 genetics was rehabilitated and Lysenko’s doctrine vanquished.

What to call that doctrine is a difficult matter. There are no “neutral” ways of discussing it. In the 1930s, Lysenko called his theories “Michurinism,” and this name persisted in the Soviet context throughout his career. After his fall from power, Michurin was incorporated into a framework consistent with classical genetics, and the name lost currency. When Lysenko was discussed after his death, his opponents sometimes used the term “*lysenkovshchina*,” attaching a Russian suffix used to characterize oppressive ideological regimes and thereby emphasizing structural aspects of Soviet science that promoted Lysenko’s rise. In the West, from very early on, the term “Lysenkoism” was common, collapsing the doctrines into a personality cult of its most prominent advocate, but the usage was rare in Russian until the 1990s. In this essay, I have adopted actors’ terminology whenever possible and for my own narrative have selected a less common and less problematic term to characterize Lysenko’s doctrines: “agrobiology.” This was the name he most

often used for his discipline in the 1960s, the focus of this essay, and it does not carry the same Cold War ideological baggage as the alternatives.²

The undoing of Lysenko and agrobiolgy was staged at the Academy of Sciences, the Olympus of the Soviet knowledge system, when a cohort of influential academicians—including, importantly, nuclear physicists such as Andrei Sakharov—opposed the election of a Lysenko crony, Nikolai Nuzhdin, to their ranks and then launched an investigation in 1964 into the books of the experimental farm in the Lenin Hills (Gorki Leninskie) outside of Moscow, where Lysenko was attempting to breed cows that produced milk with consistently higher butterfat.³ (Lysenko had expanded his work from plants to animals over the course of the Khrushchev years.) The commission that visited the farm, headed by A. I. Tulupnikov (a corresponding member of VASKhNIL, whose presidency Lysenko relinquished, for the second time, in 1962 to his confederate Mikhail Ol'shanskii), was tasked with examining only the farm's management practices, not the correctness of its director's biological theories, and so it headed for the ledgers. The results were damning: widespread fabrication of data in order to cover up the shockingly poor, even ruinous, results. Lysenko, naturally, protested both the procedures and the findings of the commission.⁴ A follow-up report in 1965 nonetheless concluded: "Unfortunately, in both his oral and his written comments academician T. D. Lysenko confines himself only to general arguments and unsubstantiated assertions and presents unfounded accusations addressed to the commission, but he does not provide in this case any arguments, proofs, facts. Thus academician T. D. Lysenko is unable to refute even one of the commission's statements."⁵ In the parlance adapted from the language of Renaissance courts, Lysenko "fell."

That is where the story usually ends. Histories of "the Lysenko affair" poured off the presses almost immediately, bringing a palpable sense of relief at the end of an era: Zhores Medvedev's *The Rise and Fall of T. D. Lysenko* (1969), David Joravsky's *The Lysenko Affair* (1970), Loren Graham's *Science and Philosophy in the Soviet Union* (1972), Mark B. Adams's comprehensive dissertation "Genetics and the Soviet Scientific Community, 1948–1965" (1972), and Dominique Lecourt's *Lyssenko* (1976).⁶ While this first wave lavished considerable attention on the drama of 1965, the scholarship since has tended to concentrate heavily on 1948: Why did the Soviets destroy a science? Subsequent research, grounded in the post-Soviet archives, has yielded a much more nuanced and accurate picture than the outline presented above. Since the em-

² See the helpful discussion in William deJong-Lambert and Nikolai Kremensov, "On Labels and Issues: The Lysenko Controversy and the Cold War," *Journal of the History of Biology*, 2012, 45:373–388. Actors' (geneticists' and historians') terminology will be used in direct quotations.

³ A brief account of these events is provided in Loren R. Graham, *Science and Philosophy in the Soviet Union* (New York: Knopf, 1972), pp. 248–249. For Sakharov's own account see Andrei Sakharov, *Memoirs*, trans. Richard Lourie (New York: Knopf, 1990), pp. 234–235.

⁴ T. D. Lysenko, "Zamechaniia akademika T. D. Lysenko po dokladu komissii AN SSSR o rezul'tatakh proverki khoziaistvenno-finansovoi deiatel'nosti eksperimental'nogo khoziaistva 'Gorki Leninskie,'" 3 May 1965, Archive of the Russian Academy of Sciences, Moscow (hereafter cited as ARAN), 1521/1/148/102–122. For a transcript of Lysenko's self-defense at the Academy of Sciences see K. O. Rossiianov, "T. D. Lysenko i Akademiia nauk (iz neopublikovannykh dokumentov)," in *Repressirovannaiia nauka*, ed. M. G. Iaroshevskii (Leningrad: Nauka, 1991), pp. 518–527.

⁵ A. I. Tulupnikov, "Zamechaniia komissii na otvet akademiku T. D. Lysenko po dokladu komissii, predstavlennoe k ob'edinennomu zasedaniiu," 31 Aug. 1965, ARAN 1521/1/148/92 (here and throughout this essay, all unattributed translations are my own).

⁶ Zhores A. Medvedev, *The Rise and Fall of T. D. Lysenko*, trans. I. Michael Lerner (New York: Columbia Univ. Press, 1969); David Joravsky, *The Lysenko Affair* (Chicago: Univ. Chicago Press, 1970); Graham, *Science and Philosophy in the Soviet Union* (cit. n. 3); Adams, "Genetics and the Soviet Scientific Community" (cit. n. 1); and Dominique Lecourt, *Lyssenko: Histoire réelle d'une "science prolétarienne"* (Paris: Maspero, 1976) (translated the following year as *Proletarian Science? The Case of Lysenko*, trans. Ben Brewster [London: NLB, 1977]).

phesis of these works is to understand Lysenko's triumph under Stalin in 1948, it is understandable that they do not go beyond 1965. Two of the most important contributions to this literature in recent decades—Nikolai Krementsov's *Stalinist Science* (1997) and Nils Roll-Hansen's *The Lysenko Effect* (2005)—although disagreeing strongly in interpretation, both focus almost exclusively on 1948 and barely mention 1965 (in the case of Roll-Hansen, there is a single sentence).⁷ With one exception, in every history of these events published to date the terminus of 1965 was set; the story was over.⁸

This periodization is reasonable, and it reflects both the perspective and the lived experience of the geneticists, who had been devastated in 1948 and found themselves largely frozen out until the early 1960s. Without doubt, many contemporary commentators among biologists and the popular press spoke publicly of “Lysenkoism” as completely finished. Theodosius Dobzhansky (1900–1975; coincidentally, he was also born in Ukraine), one of the most important biologists of the twentieth century and a 1927 émigré to the United States, gleefully declared in 1966 that “genetics in Russia has now been restored, and pseudo-genetics is no longer in power.” He was echoed by many who rejoiced that Lysenko was “finally ousted” and that “the last administrative rites” had been performed, that Lysenkoism now merited an “obituary,” and so on.⁹

There is a catch: Lysenko died on 20 November 1976. That is, he was alive for a full eleven years after his fall from power. This fact merits more reflection than it has yet received. He was still a full member of the Soviet Academy of Sciences, still director of his experimental farm; and, as documented in what follows, many of his activities were continuous with what came before. To political historians, such prolonged aftermaths are not surprising, whether in the form of survivals across revolutions (French and Russian, for example) or in the slow processes of reforming a political culture (as in de-Nazification or the Occupation of Japan). The Lysenko instance differs from these familiar examples in that the historical actors and the writers

⁷ The sentence is: “Lysenko was not stripped of his membership in the Academy of Sciences, however, and he kept his own research institute on the Lenin Heights in Moscow until he died in 1976.” Nils Roll-Hansen, *The Lysenko Effect: The Politics of Science* (Amherst, N.Y.: Humanity, 2005), pp. 275–276. See also Nikolai Krementsov, *Stalinist Science* (Princeton, N.J.: Princeton Univ. Press, 1997). Loren Graham has continued to investigate the history of agrobiolgy since 1972, over time de-emphasizing 1965 in favor of 1948. See, e.g., Loren Graham, *Science in Russia and the Soviet Union: A Short History* (Cambridge: Cambridge Univ. Press, 1993); and Graham, *What Have We Learned about Science and Technology from the Russian Experience?* (Stanford, Calif.: Stanford Univ. Press, 1998). Most recently, he has returned to the aftermath of Lysenko in Graham, *Lysenko's Ghost: Epigenetics and Russia* (Cambridge, Mass.: Harvard Univ. Press, 2016).

⁸ See, e.g., Joel Kotek and Dan Kotek, *L'affaire Lyssenko* (Brussels: Editions Complexe, 1986); Stephen Fortescue, *The Communist Party and Soviet Science* (Baltimore: Johns Hopkins Univ. Press, 1986), p. 44; and Peter Pringle, *The Murder of Nikolai Vavilov: The Story of Stalin's Persecution of One of the Great Scientists of the Twentieth Century* (New York: Simon & Schuster, 2008), p. 301. In his recent textbook on the history of Cold War genetics, William deJong-Lambert includes no significant events after 1965 on his timeline: William deJong-Lambert, *The Cold War Politics of Genetic Research: An Introduction to the Lysenko Affair* (Dordrecht: Springer, 2012), p. xiv. The exception is Valery N. Soyfer, *Lysenko and the Tragedy of Soviet Science*, trans. Leo Gruliov and Rebecca Gruliov (New Brunswick, N.J.: Rutgers Univ. Press, 1994). For a survey and citations to the numerous articles and books on agrobiolgy's history see deJong-Lambert and Nikolai Krementsov, eds., *The Lysenko Controversy as a Global Phenomenon*, 2 vols. (New York: Palgrave Macmillan, 2017).

⁹ Theodosius Dobzhansky to Prof. F. G. Brieger, 31 Jan. 1966, I. Michael Lerner Papers, Ms.B.L563, American Philosophical Society Library, Philadelphia, Pennsylvania (hereafter cited as MLP). Folder: “Dobzhansky, Theodosius #7.” Further “rejoicings” come from David L. Lehman, “The Lysenko Affair: Implications for the Teaching of High School Biology,” *American Biology Teacher*, 1967, 29:721–726, on p. 721; David S. Greenberg, “Lysenko: Soviet Science Writes Fimis to Geneticist's Domination of Nation's Biological Research,” *Science*, 1965, 146:716–717, on p. 716; and Oliver J. Gillie, “An Obituary for Lysenkoism,” *Guardian*, 23 May 1967, p. 6. See also Theodosius Dobzhansky, “Revival of Genetics in the U.S.S.R.,” *Quarterly Review of Biology*, 1968, 43:56–59; Alexander Vucinich, rev. of Medvedev, *The Rise and Fall of T. D. Lysenko*, and Joravsky, *The Lysenko Affair*, *Russian Review*, 1972, 31:87–88; Garland E. Allen, rev. of Joravsky, *The Lysenko Affair*, *Amer. Biol. Teacher*, 1971, 33:502–505; and Walter Sullivan, “Soviet Genetics Reborn after Lysenko Period,” *New York Times*, 17 Oct. 1967, pp. 1, 26.

of the histories interacted so strongly at the moment of the narrative's creation and then solidified a picture that has proven very resilient.

In this essay, rather than treating 1965 as the straightforward aftermath of the catastrophe of 1948, I emphasize the years *after* the aftermath; and this change of perspective yields two primary arguments. First, by taking the documents produced by the agrobiologists seriously, I show that they understood their hold on power to have been rockier from 1948 to 1965 than the “rise” accounts suggest and therefore that they also perceived the “fall” as potentially only a temporary setback. The 1960s and 1970s, from the agrobiologists' perspective, resembled the back-and-forth of the 1930s. This is also visible through the sources produced by geneticists: in the infighting to establish control over the resurrected discipline of genetics, we observe a persistent fear that Lysenko (or at least his cronies) might reemerge, that the “fall” had not been final. The second argument is a historical explanation of this narrative's invisibility as a consequence of contemporaries—including historians of science writing in the early 1970s—framing the story in terms of Lysenko and then eclipsing him while he was still alive. This stress on personality rather than institutions (and then the dismissal of the personality) was in itself largely a contingent result, caused by the Soviet state's spasmodic persecution of the biologist and historian of agrobiolgy Zhores Medvedev. It is impossible to separate “historical events” from their “historiographical interpretation,” as contemporary historians' accounts repeatedly shaped how actors and politicians reacted to events.

DISESTABLISHING AGROBIOLOGY

The history of genetics is the most comprehensively treated aspect of Soviet science; this continues to be true for the period after 1965, where particular attention is focused on the issue of *establishment*: how does one build the discipline of genetics after a lengthy period of eclipse?¹⁰ There was, however, a complement to the erection of a new genetics establishment: the necessary and simultaneous *disestablishment* of agrobiolgy. (The historiography of science tends to be asymmetric on the treatment of establishment and disestablishment, a point I will return to in the conclusion.) For genetics to return, it was not enough simply to hire scientists and give them lab space; their counterparts in agrobiolgy, who had suppressed them for years, needed to be contained.

The revival of genetics is a happy story. The science rebounded with surprising speed after 1965: by 1970 there were twenty-five institutes centrally focused on aspects of genetics within the Soviet Academy of Sciences and the academies of the various republics, as well as forty institutes under the auspices of the Ministry of Agriculture (once an anti-genetics stronghold) and in various universities. There were new journals such as *Genetika*, which released its first issue in 1965, *Tsitologiia i genetika* (*Cytology and Genetics*), published by the Ukrainian Academy of Sciences, and *Ontogez*. *Genetika* was a particular success, publishing 374 original ar-

¹⁰ A. E. Gaisinovich, *Zarozhdenie i razvitie genetiki* (Moscow: Nauka, 1988); N. P. Bochkov, “Genetic Counseling in the USSR,” *Progress in Clinical and Biological Research*, 1979, 34:31–40; Bochkov, “Razvitie meditsinskoj genetiki v SSSR,” *Klinicheskaia meditsina*, 1980, 48:14–20; Mark B. Adams, “The Soviet Nature-Nurture Debate,” in *Science and the Soviet Social Order*, ed. Loren R. Graham (Cambridge, Mass.: Harvard Univ. Press, 1990), pp. 94–138; Susanne Bauer, “Mutations in Soviet Public Health Science: Post-Lysenko Medical Genetics, 1969–1991,” *Studies in History and Philosophy of Biological and Biomedical Sciences*, 2014, 30:1–10; and Bauer, “Virtual Geographies of Belonging: The Case of Soviet and Post-Soviet Human Genetic Diversity Research,” *Science, Technology, and Human Values*, 2014, 39:511–537. More attention is given to the persistence of Lysenko's followers in S. V. Shalimov, “Problemy sotsial'noi istorii otechestvennoi genetiki v ‘pozdnosovetskii’ period (1970-e–pervaia polovina 1980-kh gg.),” *Voprosy istorii estestvoznaniia i tekhniki*, 2015, 36(4):665–697; and Shalimov, “Razvitie genetiki v Novosibirskom nauchnom tsentre vo vtoroi polovine 1960-kh gg.: Sotsial'no-istoricheskii aspekt,” *Istoriko-biologicheskie issledovaniia*, 2013, 5(1):16–32.

ticles by contributors from a hundred different institutions in its first eighteen months. (Six institutions, however, generated almost 50 percent of the papers.)¹¹ Many of these pieces functioned as review articles of contemporary Western research. The pace was slower in ancillary disciplines; the Soviet journal *Molekuliarnaia biologiiia* appeared in 1967, eight years after the Western *Molecular Biology*—Adams judged in 1972 that “the growth of Soviet molecular biology as a discipline lagged behind its growth in the West by almost a decade.”¹²

There were, of course, significant institutional gaps that needed to be filled. As Melvin Green, a biologist from the University of California, Davis, observed during a visit to the Soviet Union in 1970: “After almost two weeks here, two facts are clear: genetic research is only so, so [sic] being hampered by lack of funds and equipment and development of new areas of genetic research e.g. behavioral genetics is almost nil.” Most saliently, a generation of geneticists was missing; the field was bimodal, with one cohort in their late fifties and another in their mid-twenties.¹³ Smoothing out that crater would have to start with new textbooks to replace agrobiological ones. The first proper Soviet textbook in genetics was M. E. Lobashev’s *Genetika*, which appeared with a print run of ten thousand in 1963 (significantly, before Lysenko’s fall) and was revised and updated with an impressive print run of sixty thousand a few years later.¹⁴

The speed of recovery is remarkable—though less surprising, as Adams has noted, when one takes into account where the research was coming from. Despite some high-profile arrests and even more dismissals, Lysenko’s power had never been absolute, and a significant complement of geneticists found shelter after 1948 in provincial institutions or under the protection of nuclear physicists (with their specialty now christened “radiation biology”).¹⁵ Every major center of genetics that prospered after 1965—with one important exception—predated Lysenko’s fall, and while the publication rate in the field rose after his ouster, it continued along the trajectory of its pre-1965 growth.¹⁶

The exception was the Institute of General Genetics of the Academy of Sciences, directed by Nikolai P. Dubinin (1907–1998), who was already widely known internationally before Lysenko denounced him in 1948.¹⁷ Dubinin’s institute was newly created in 1965 to replace the Institute of Genetics, which had been directed by Lysenko and was dissolved by the academy administration. Dubinin leveraged his position carefully, linking genetics to whatever the Soviet regime favored in a bid to cement the reestablishment of the field. His pedigree as a distinguished victim now yielded a series of rewards: a Lenin Prize in 1966 for his work on chromosomal theory and an Order of the October Revolution in 1975. Even the venerable Dobzhansky was well disposed upon his first post-Lysenko encounter with Dubinin at a meet-

¹¹ “Sostoianie i dal’neishee razvitiie genetiki v SSSR,” *Vestnik Akademii nauk SSSR*, 1970, no. 7, pp. 3–10, esp. pp. 3–4; Igor L. Kosin, “Soviet Genetics: Biography, History, Commentary,” *BioScience*, 1974, 24:583–589, esp. p. 588; Kosin, “A Decade in Soviet Genetics,” *Research Studies*, 1970, 38:251–257, esp. pp. 254–255; and Mark B. Adams, “Biology after Stalin: A Case Study,” *Survey: A Journal of East and West Studies*, 1977–1978, 23:53–80, esp. p. 64.

¹² Adams, “Genetics and the Soviet Scientific Community” (cit. n. 1), p. 148.

¹³ Mel Green to I. Michael Lerner, 13 Oct. 1970, Theodosius Dobzhansky Papers, Mss.B.D65, American Philosophical Society Library, Philadelphia, Pennsylvania (hereafter cited as TDP), Folder: “Lerner, I. Michael #3”; and B. L. Astaurov, “O genetike i ee istorii,” ed. V. V. Babkov, *VIET*, 1987, no. 1, pp. 79–88, on p. 82.

¹⁴ Kosin, “Decade in Soviet Genetics” (cit. n. 11), p. 253; and “Lysenko’s Theories Persisting in Soviet,” *New York Times*, 8 Sept. 1965, p. 4.

¹⁵ Adams, “Biology after Stalin” (cit. n. 11), p. 55; and “Lysenko’s Downfall,” *New York Times*, 5 Feb. 1965, p. 24. Details of the research are presented in Adams, “Genetics and the Soviet Scientific Community” (cit. n. 1).

¹⁶ Adams, “Genetics and the Soviet Scientific Community,” pp. 335–336; Adams, rev. of Joravsky, *The Lysenko Affair*, *Isis*, 1971, 62:560–561, esp. p. 561; and Shalimov, “Razvitiie genetiki v Novosibirskom nauchnom tsentre vo vtoroi polovine 1960-kh gg” (cit. n. 10).

¹⁷ Trofim Lysenko, *The Science of Biology Today* (New York: International, 1948).

ing of the United Nations Radiation Committee in New York City: “I find him a most pleasant and likable person. It is easily understandable that he has the ambition of making good 20–25 years of his life which were destroyed by Lysenko.”¹⁸

The other leader of the revived Soviet genetics was Boris L. Astaurov (1904–1974), new director of the Institute of Developmental Biology, who was elected to the Academy of Sciences in 1966 and simultaneously became president of the Vavilov All-Union Society of Geneticists and Breeders; from that year until 1969 he was also a member of the Scientific Council of the Institute of General Genetics.¹⁹ Astaurov and Dubinin were at loggerheads almost immediately, maneuvering both publicly and privately to promote their contrasting visions of the future of genetics—a conflict watched with trepidation by Western colleagues. As the Berkeley biologist Michael Lerner (about whom much more in the next section) wrote to a colleague at Case Western in 1971: “The situation in Russia seems to be headed for some sort of showdown between Dubinin and Astaurov.”²⁰ Lerner continued in this vein in a letter to the Harvard evolutionary biologist Ernst Mayr concerning the latter’s impending visit to Moscow: “The current situation is that of a volcano about to erupt. . . . The issue is Dubinin’s all-out challenge to Astaurov for the number one position on the genetical totem pole. Dubinin has been behaving despicably in this matter, using tactics reminiscent of Lysenko, including not only an attack on human genetics, but actually forming at one stage an unholy alliance with Lysenko himself.” Lerner went on to observe that Dubinin “personally is a great disappointment to me. Until the revival of genetics he acted as a hero, and his work was, of course, first-rate. Since 1964 he has behaved abominably both as a scientist (a lot of his writing is plain plagiarism) and as an individual.”²¹ Dubinin’s subsequent reputation has not fared well. His self-congratulatory memoir, *Vechnoe dvizhenie* (*Perpetual Motion*), which appeared in 1973 and dealt a number of serious blows to the Astaurovites by tarnishing Astaurov’s mentor, the repressed Nikolai Kol’tsov (1872–1940), was widely mocked by colleagues as *Vechnoe samovydvizhenie* (*Perpetual Self-Promotion*).²²

One of the most serious divergences between Dubinin and Astaurov concerned what to do with the generation of agrobiologists that occupied biological institutions across the Soviet Union. Astaurov was for retribution. Agrobiologists had ousted geneticists from their posts and repopulated the discipline with talentless (or at best unprincipled) opportunists; the first order of business ought to be to cleanse the Augean stables. Dubinin, on the other hand, opted for forgiveness: “A person can change his views if he has understood his mistakes and sees his place in the future. Such a relationship to cadres had enormous significance—namely, it created the

¹⁸ Dobzhansky to Lerner, 15 June 1966, MLP, Folder: “Dobzhansky, Theodosius #7.” Regarding Dubinin’s facility in linking genetics to other fields see, e.g., his contribution to the bandwagon of space exploration: N. P. Dubinin, “Problemy kosmicheskoi genetiki,” *Izvestiia Akademii nauk SSSR: Serii biologicheskaiia*, 1967, no. 5, pp. 669–681. On the “rewards” see “K 80-letiiu Nikolaiia Petrovicha Dubinina,” *Genetika*, 1987, 23:189–191; and Paul R. Josephson, *New Atlantis Revisited: Akademgorodok, the Siberian City of Science* (Princeton, N.J.: Princeton Univ. Press, 1997), p. 93.

¹⁹ P. F. Rokitskii, R. I. Goriachevaia, and A. L. Tepenitsyna, *Boris Lvovich Astaurov* (Moscow: Nauka, 1972).

²⁰ Lerner to Arthur G. Steinberg, 18 Nov. 1971, MLP, Folder: “Medvedev, Z. A.—Materials 1971 #5.” See the detailed enumeration of the backers and institutional situations of both parties in Adams, “Soviet Nature-Nurture Debate” (cit. n. 10), p. 116.

²¹ Lerner to Ernst Mayr, 11 Nov. 1971, MLP, Folder: “Mayr, Ernst #2.” Three years later Lerner and Dobzhansky dissuaded Mayr from inviting Dubinin to a conference on the history of the “modern synthesis” of midcentury biology—in which Dubinin was the last surviving participant of the important Soviet contribution—because of his latter-day machinations. See Mayr to Dobzhansky, 7 Mar. 1974; and Lerner to Mayr, 14 Mar. 1974: MLP, Folder: “Mayr, Ernst #2.”

²² “Sud’by genetiki v nashem otechestve,” in *Surovaia drama naroda: Uchenye i publitsisty o prirode stalinizma*, ed. Iu. P. Senokosov (Moscow: Politizdat, 1989), pp. 401–445, on p. 443; and N. P. Dubinin, *Vechnoe dvizhenie* (Moscow: Izd. politicheskoi literatury, 1973). For more on the reaction to Dubinin’s memoir see Adams, “Soviet Nature-Nurture Debate” (cit. n. 10), p. 125.

conditions of an apprehension-free, thought-out and conscious distancing of a great many people from T. D. Lysenko's mistakes."²³ There would be but a few casualties: the journal *Agrobiologiia* was shuttered in the middle of 1965 and the Institute of Genetics dissolved.

Dubinin had a strong case from the administrative point of view. A large population of employed people could be retrained if only they laid down their agrobiological standards, and that would ameliorate the problem of cadres. It also checkmated the bellicose Astaurov: Lysenko supporters knew that if they opposed Dubinin a worse fate waited in the wings. The converts came readily, as Lerner documented obsessively:

V. Stoletov, Professor of Genetics at Moscow State University and RSFSR Minister of Higher Education: "He was a strong Lysenkoite, partly because of ignorance of biology and partly as a Communist hack official. In 1964 he switched his affiliations and the last time I saw him he was on the side of the angels."

N. V. Turbin: "A classical geneticist until 1948, he joined Lysenko then and was rewarded with the deanship of the Biology Faculty in Leningrad. By 1952 he felt strong enough to try to unseat Lysenko by accusing him of being soft on Weismannism-Morganism. As a result he found himself in Minsk (where I think he still is) and returned to the geneticist fold."²⁴

There were many others. Dobzhansky was pleased: "Of the several hundred quasi-geneticists who made their careers under Lysenko only one seems staunchly faithful to the 'master.' Others have reformed, and some of these, *mirabile dictum* [sic], are publishing tolerable good papers in the newly-founded journal *Genetika*!"²⁵

Dubinin's clemency extended even to Trofim Denisovich himself. He noted that Lysenko regularly attended meetings of the Biological Section of the Academy of Sciences, sat in the front row, and would defend his views with vigor: "As before he cannot critically evaluate his activity." Nonetheless, unlike his former associates, Dubinin did not demand that Lysenko recant, and as late as 1990 he dismissed those who called for a "rehabilitation" of Lysenko. "The very framing of such a question is inappropriate," he stated, "since Lysenko was never repressed by anyone, nor slandered." His view found supporters among other Soviet biologists. As V. P. Orlov wrote to A. A. Liubishchev (an early and prominent critic of Lysenko): "For example, you are for a trial of Lysenko. I am categorically opposed, and the isolation of him from science, but not from society by means of jail or loss of life, will be entirely sufficient."²⁶ (To what degree Lysenko actually ended up isolated is a separate question.)

As strange as the amnesty for agrobiologists might seem in hindsight, it constituted the main response of Dubinin and his allies to the perceived threat of the resurgence of Lysenko or his former protégés. Dubinin focused on personnel, leaving the structures largely unaltered (as indicated by the persistent complaints about his heavy-handedness noted above). The geneticists were by no means certain in the late 1960s that their restoration was an irreversible triumph,

²³ Dubinin, *Vechnoe dvizhenie*, p. 435.

²⁴ Adams, "Soviet Nature-Nurture Debate" (cit. n. 10), p. 114 (worse fate); and Lerner to Mayr, 11 Nov. 1971 (on Stoletov), 17 Apr. 1972 (on Turbin), MLP, Folder: "Mayr, Ernst #2."

²⁵ Theodosius Dobzhansky, "Russian Genetics: Emerging from Lysenko," *Science*, 1967, 158:577. He said roughly the same thing in his review of Medvedev's *The Rise and Fall of T. D. Lysenko*, *ibid.*, 1969, 164:1507–1509.

²⁶ Dubinin, *Vechnoe dvizhenie* (cit. n. 22), p. 437; N. P. Dubinin, *Genetika: Stranitsy istorii* (Kishniev: Shtiimitsa, 1990), pp. 327–328; and V. P. Orlov to A. A. Liubishchev, 30 Nov. 1966, reproduced in Liubishchev, *V zashchitu nauki: Stat'i i pis'ma, 1953–1972*, ed. R. G. Barantsev and N. A. Papchinskaia (Leningrad: Nauka, 1991), p. 146.

and anxious intimations of an agrobiological insurgency were common. In 1968, for example, during a discussion of whom to appoint to fill a vacancy as an academician of genetics, none other than Nikolai Nuzhdin—the very man whose failed candidacy had sparked the anti-Lysenko revolt at the academy—was floated. Astaurov and his colleagues proposed instead the controversial geneticist Nikolai Timofeev-Ressovskii, which triggered a vehement reaction, as noted by Zhores Medvedev (who will return in the following section): “The ‘Michurinists’ and T. D. Lysenko himself began active opposition to the election of Timofeev-Ressovskii to the Academy.”²⁷

The former agrobiologists succeeded in blocking Timofeev-Ressovskii, and the geneticists took note. Opponents of classical genetics continued to defend themselves in textbooks and still held positions at many agricultural institutes and at provincial universities (such as Tomsk). Frederick Seitz, then president of Rockefeller University, mused after reading a book about the Lysenko epoch: “One wonders, of course, if the circumstances which made the Lysenko situation possible are gone forever or whether they could reoccur.”²⁸ This uncertainty, so important in shaping the way that genetics was established and agrobiology disestablished, has left little trace in either disciplinary memory or the extant historiography. It was suddenly displaced by a related controversy that erupted in 1969, directing attention away from academician Lysenko and channeling Western and Soviet anxiety into a different track: dissidence.

A QUESTION OF MADNESS

Michael Lerner’s name is usually absent from the histories of agrobiology, even though every individual interested in this episode has read a good deal of his prose. They read him in the form of a translated book, *The Rise and Fall of T. D. Lysenko*, written by Zhores A. Medvedev—a dissident author who, together with his twin brother Roy, became a mainstay of the expatriate anti-Soviet intelligentsia. Since the present essay seeks to uncover the history of those eleven years when Lysenko coexisted with the ostensible end of his doctrines, it is essential to pay particular attention to the histories written in those years; the historiography and the history are not independent. No work is more emblematic of the persistence of Lysenko’s status (real or imagined) after 1965 than Medvedev’s manuscript and its path to publication in English translation by Columbia University Press, and no event is more central to obscuring our understanding of that persistence than the Soviet state’s aggressive measures against Medvedev upon its publication and foreign reactions to them.

In 1962 Medvedev, a researcher in the physiology of aging, wrote a manuscript entitled “Biology and the Cult of Personality,” an account of the rise of Lysenko as dictator of Soviet biology compiled from interviews and documentary materials he had gathered.²⁹ Copies of the manuscript circulated in *samizdat* and formed an essential part of the case against Lysenko when he was disciplined in 1965.³⁰ Once Lysenko was displaced, Medvedev’s manuscript sud-

²⁷ Zh. A. Medvedev to Dobzhansky and Lerner, 4 Sept. 1969, TDP, Folder: “Medvedev, Zh. A. #3.” The case of Timofeev-Ressovskii, suspect because of his time in Third Reich Germany, was the most visible demonstration of institutional force mounted by those opposed to the geneticists; they successfully removed him from the Institute of Medical Radiobiology. For a brief Western treatment see Diane B. Paul and Costas B. Krimbas, “Nikolai V. Timoféeff-Ressovsky,” *Scientific American*, 1992, 266(2):86–92.

²⁸ Frederick Seitz to Dobzhansky, 28 May 1969, TDP, Folder: “Medvedev, Zh. A. #3.”

²⁹ Ironically, Medvedev had written to Lysenko in 1945 full of enthusiasm for his theories, was invited to meet him, and declared in a recent memoir that he was charmed by the occasion. See Zh. A. Medvedev, “Opasnaia professiia,” *Ist.-biol. issledovaniia*, 2011, 3(2):109–123.

³⁰ Many observers at the time believed that the other major *samizdat*/émigré attack on Lysenko, Mark Popovsky’s *The Vavilov Affair*, was heavily plagiarized from Medvedev’s original manuscript. See Mark Popovskii, *Delo akademika Vavilova* (Moscow:

denly acquired an ending. He revised the text into a book-length study entitled “History of Biological Discussions in the USSR (1929–1966)” and sought to publish it with Nauka, the publishing arm of the Academy of Sciences. Everything was done aboveboard: a fifteen-person commission, chaired by the Nobel Laureate in Chemistry Nikolai Semenov, was impaneled to vet the text, and each member wrote a separate, signed evaluation endorsing the manuscript’s publication.³¹ And then Nauka simply sat on the manuscript, neither printing it nor explaining its refusal to do so. Whether the censorship came from the Communist Party, the Academy of Sciences, or elsewhere is uncertain. (It seems unlikely that Lysenko was personally involved.)

Medvedev sent the manuscript to be published abroad, although he “clearly and explicitly informed the Soviet authorities that he was authorizing translation and publication of the final edition under the auspices of a group of Western scientists,” as the historian David Joravsky later explained to the book review editor at *Science*.³² There was reason for concern: in September 1965 and February 1966 the writers Andrei Siniavskii and Iulii Daniel’ were prosecuted in a show trial for publishing literary works overseas. When Medvedev first approached Lerner to translate and publish his book in November 1965, he cited the worrying example of the pirated foreign publication of Aleksandr Solzhenitsyn, which had resulted in political reprisals.³³

Medvedev was fortunate in choosing Lerner, who was assiduous even through a massive gastrointestinal hemorrhage (complicated by his rare blood type) that almost killed him in early 1969. Isadore Michael Lerner (who always went by his middle name) was born in 1910 in Harbin, Manchuria, and eventually found his way to the United States, graduating with a Ph.D. in genetics from the University of California, Berkeley, in 1936. He would remain at that institution, secure in his specialty of chicken genetics, for the rest of his career. He had long been an acute observer of the situation in Soviet genetics, and in 1968 his outlook was sunny but tentative: “The picture for the nonce is an optimistic one. But there are still many unredeemed Lysenkoites in high places and the effect of the lack of trained geneticists in the country will be felt for a long time to come.”³⁴ The following year, Lerner took up his pen at Medvedev’s request and helped transform the narrative of agrobiolgy. The Berkeley biologist slashed the manuscript, reordered sections, and punched up the style, thus creating what eventually appeared in English as *The Rise and Fall of T. D. Lysenko*.³⁵ Columbia University Press was en-

Kniga, 1991); Medvedev to Lerner, 16 Dec. 1966, MLP, Folder: “Medvedev, Z. A.—Materials 1966 #2”; and Liubishchev to M. A. Popovskii, 24 Nov. 1966, reproduced in Liubishchev, *V zashchitu nauki* (cit. n. 26), p. 273.

³¹ This history is recounted in several places, including Zhores A. Medvedev and Roy A. Medvedev, *A Question of Madness: Repression by Psychiatry in the Soviet Union*, trans. Ellen de Kadt (New York: Norton, 1971), pp. 30–31; and “Sud’by genetiki v nashem otechestve” (cit. n. 22), p. 442. Other members of the commission included Boris Astaurov, B. E. Bykhovskii, M. E. Lobashev, and Iu. I. Polianskii.

³² David Joravsky to Sylvia Eberhardt, 19 May 1969, TDP, Folder: “Medvedev, Zh. A. #3.” See also Dobzhansky to Lerner, 14 Jan. 1968, MLP, Folder: “Dobzhansky, Theodosius #8.”

³³ Medvedev to Lerner, 22 Nov. 1965, MLP, Folder: “Medvedev, Z. A.—Materials 1965.”

³⁴ I. Michael Lerner, *Heredity, Evolution, and Society* (San Francisco: Freeman, 1968), p. 282. In some sources Lerner’s first name is given as “Israel,” including his official biography for the National Academy of Sciences: R. W. Allard, *Israel Michael Lerner, 1910–1977* (Washington, D.C.: National Academies Press, 1996). Allard makes no mention of Lerner’s involvement in the reconstruction of genetics in the Soviet Union. His papers at the American Philosophical Society use “Isadore,” and that is what I have opted for here.

³⁵ The original Russian text has never appeared in full. Lerner received requests for the Russian manuscript from potential translators into German and Japanese, but he refused on the grounds that circulation of the Russian original might jeopardize Medvedev; he denied the same access to Radio Liberty. See Fujio Kanemitsu to Lerner, 20 Mar. 1970, MLP, Folder: “Medvedev, Z. A. Columbia Univ. Press #3”; and Lerner to John D. Coleman (Radio Liberty Committee), 17 Aug. 1970, MLP, Folder: “Medvedev, Z. A.—Materials 1970 #8.” The Russian edition—Zh. A. Medvedev, *Vzlet i padenie Lysenko: Istoriia biologicheskoi diskussii v SSSR (1929–1966)* (Moscow: Kniga, 1993), consists of Medvedev’s prose, but the structure is Lerner’s.

thusiastic, providing \$2,000 to assist Lerner with translation costs, and willing to adjust its advertising copy to accommodate concerns for Medvedev's safety in the Soviet Union.³⁶

Medvedev's forebodings about both piracy and reprisals proved prescient in short order. He was careful not to repeat the scenario of Siniavskii and Daniel', who had smuggled out manuscripts without first attempting publication in the Soviet Union. As such, they were accused of collaboration with NTS, "an émigré organization that is widely and reliably reputed to be a dependency of the CIA," which issued (among others) the Russian literary journal *Grani*, a frequent publisher of *samizdat* manuscripts.³⁷ Much to the surprise of Lerner, Dobzhansky, and then Medvedev, in 1969 *Grani* released a two-part series, under the byline of "Zh. D. Medvedev" (they got the patronymic wrong), entitled "Biological Science and the Cult of Personality"—evidently a version of Medvedev's first, intemperate, non-fact-checked manuscript.³⁸ Medvedev's supporters were furious.

"I understand that 'Grani' went ahead publishing a second installment," fumed Dobzhansky. "What bastards!" Joravsky concurred, using much the same language—"complete *svolochi*"—and saying that there was likely nothing that could stop *Grani* in its venture, since once they had the manuscript "it would take the CIA to persuade them against publication." Joravsky pointed to rumors that NTS "is supported by the CIA and also infiltrated by KGB agents." He worried that those alleged KGB moles "may be helping the Soviet prosecutor prepare a court case against Medvedev," for "it is a fact that the Soviet prosecutor considers association with *Grani* to be prima facie evidence of 'anti-state activity.'"³⁹ This was precisely the outcome that Medvedev had been trying to avoid.

Choosing his words carefully (under the reasonable assumption that the KGB would be reading his correspondence), Medvedev wrote to the editors after publication of the first installment in late May 1969: "With the present letter I want to express a decisive protest to the piratical actions of the sort of the journal 'Grani.'" He insisted that the journal was violating his rights and those of Columbia University Press, neither of which had been consulted. "I don't know on which 'black market' you obtained this manuscript which you are printing. But judging from the information I have received, it sharply differs even from that first unfinished variant, which I wrote back then, not however for publication." He demanded that *Grani* cancel the second installment and issue an apology. The editor responded a month later saying that she could not pulp the issue, that the NTS news magazine *Posev* had earlier announced the publication, and that attempts to contact Medvedev beforehand had been unsuccessful. *Grani* had been unaware, apparently, of the Columbia University Press wrinkle, although the editors likely knew there was no legal case for infringement. The press owned only the rights to the English translation, and since the Soviet Union was not yet part of the Universal Copyright

³⁶ "The important point that Medvedev expressed to me in a very strongly worded letter asked that the publicity does not anticipate the publication date by too long a period of time" in order to avert the "unbearable pressures" Solzhenitsyn had faced. Lerner to Robert J. Tilley (Assistant Director of Columbia University Press), 3 June 1968, MLP, Folder: "Medvedev, Z. A. Columbia Univ. Press #1." For publication details see Tilley to Lerner, 12 Jan. 1968, MLP, Folder: "Medvedev, Z. A. Columbia Univ. Press #1." The press conjured up the dramatic title after a lengthy debate that included such candidates as "The Zygote Is No Fool" and Other Examples of the Perversion of Science by T. D. Lysenko—Science in the Soviet Union, 1931–1966."

³⁷ Joravsky to Eberhardt, 19 May 1969, TDP, Folder: "Medvedev, Zh. A. #3." On *Grani*'s literary reputation see Arnold McMillan, "Exiled Russian Writers of the Third Wave and the Émigré Press," *Modern Language Review*, 1989, 84:406–413, on p. 407.

³⁸ Zh. D. Medvedev, "Biologicheskaiia nauka i kul't lichnosti," *Grani*, 1969, 24(70):127–172; and Medvedev, "Biologicheskaiia nauka i kul't lichnosti—Okonchanie," *ibid.*, 1969, 24(71):78–161.

³⁹ Dobzhansky to Lerner, 29 July 1969, MLP, Folder: "Dobzhansky, Theodosius #9"; Joravsky to Lerner, 1 May 1969, MLP, Folder: "Medvedev, Z. A.—Materials 1969 #2"; and Joravsky to Eberhardt, 19 May 1969, TDP, Folder: "Medvedev, Zh. A. #3."

Convention there was no protection for the Russian original. A lackluster explanatory note was printed in a later issue of *Grani*.⁴⁰

Meanwhile, the English translation appeared. It is worth revisiting the arguments in its conclusion. Rather than presenting an abrupt fall of Lysenko from power, Medvedev chronicled Lysenko's decline and even the 1965 reorganization of genetics at the Academy of Sciences as gradual affairs. Throughout all of these transformations, he noted:

It is remarkable that neither Lysenko nor his associates, who had previously reacted so tempestuously to even indirect criticism, wrote a single reply to the great number of exposés and critical articles published. It might be thought that they were refused publication, but this was not the case. There were just no replies. I have especially checked with a number of editorial boards, and it is clear that the staff of Lysenko's army kept silent. These were sensible tactics. The fish was on the hook or, more precisely, in the net, and it waited to see whether or not it would be pulled ashore. Or, possibly, it might be transferred to a smaller pond and allowed a peaceful existence.

Medvedev observed what was evident to all: that Dubinin's amnesty was widely accepted and that many hard-line agrobiologists had recanted and retained their positions in the new genetics establishment. The penultimate section of the book was entitled "Has Lysenkoism Been Liquidated?" and concluded that it had been "weakened" but that liquidation was not in the cards. "A cleanup is going on, but by legal means of competition and recertification," Medvedev noted. "It is a *gradual* selective process." Lysenko had achieved power "by a coup. Today these methods are inapplicable; hence the reverse process is proceeding at a much slower pace."⁴¹

At this point, the case took a dramatic turn. On Friday, 29 May 1970, as Zhores Medvedev reported in a letter to Lerner, he received a call from the local clinic in Obninsk saying that they had urgent information about his son's health. Medvedev knew his son was camping and likely fine but suggested that his wife go to the clinic. The caller insisted that it had to be him. He responded that he had plans to go to Moscow and hung up. Fifteen minutes later there was a knock at the door, and through the window he saw an ambulance with two policemen beside it. Then "they forced the door," and the person who had been on the phone, another person in plainclothes, and three policemen barged in. (Medvedev's wife summoned neighbors as witnesses.) The plainclothes person identified himself as A. E. Lifshits, chief psychiatrist of the Kaluga hospital. He said that the head of the town council, N. Antonenko, had made a request and "they want to investigate my mental health." Medvedev was asked questions about his Lysenko book and about a manuscript in press on international cooperation among scientists. He was grabbed by the arms and taken to the Kaluga psychiatric hospital, from where he was writing to Lerner: "I now 4th day enjoy the society of schisophrenics, psychopats and manjiaks and alcogolisc and have the privilege to read as much as I like and to meet my wife and friends three times a week. Not so bad."⁴²

⁴⁰ Medvedev to Editors of the Journal "GRANI," 21 May 1969, MLP, Folder: "Medvedev, Z. A. Columbia Univ. Press #2"; N. Tarasova to Medvedev, 20 June 1969, MLP, Folder: "Medvedev, Z. A. Columbia Univ. Press #2"; Tilley to Lerner, 19 May 1969, MLP, Folder: "Medvedev, Z. A.—Materials 1969 #2" (copyright issues); and "Ot redaktsii," *Grani*, 1969, 24(72):232.

⁴¹ Medvedev, *Rise and Fall of T. D. Lysenko* (cit. n. 6), pp. 230, 239 (agrobiologists recanting and retaining their positions), 241 (emphasis in original). Early reviews picked up on some of these issues. See Robert Olby, rev. of Medvedev, *The Rise and Fall of T. D. Lysenko*, *Soviet Studies*, 1970, 21:517–520. Joravsky's own review, "Cracked Wheat," *New York Review of Books*, 29 Jan. 1970, drew heavily from the comments he made in correspondence behind the scenes.

⁴² Medvedev to Lerner, 2 June 1970, TDP, Folder: "Lerner, I. Michael #1" (all misspellings in the original). A similar account was later published in Medvedev and Medvedev, *Question of Madness* (cit. n. 31), p. 30. The manuscript on international co-

Zhores's twin brother Roy immediately mobilized support from a network of influential scientists and rushed to the hospital to confront Lifshits. "I read his manuscript *Biology and the Cult of Personality*," Lifshits told Roy—the title indicating that he had seen the *Grani* publication and not the more measured American edition—"and began to have doubts about the sanity of the person who had written it." Roy's rejoinder was characteristically sarcastic: "Moreover, if one reads Lysenko's works today or knows about his past activity, one is bound to have doubts about his mental stability as well. So why does Lysenko still attend meetings at the Academy of Sciences instead of going to a mental hospital for a psychiatric examination?" Lifshits's response: "Perhaps Lysenko is abnormal, but he doesn't live in the Kaluga region."⁴³

The outcry about the commitment of a scholar to a mental asylum because of his writings—an increasingly common tactic during the Brezhnev years—grew deafening.⁴⁴ Major geneticists in the academy, such as Vladimir Engel'gardt and Astaurov (but not, significantly, Dubinin, who squelched attempts at protest), joined by the influential physicists Petr Kapitza and Andrei Sakharov, pressured the Soviet government. In the United States, scientists lobbied both professional societies and the National Academy of Sciences to issue an official demarche. (The latter did not happen, but Philip Handler, president of the National Academy of Sciences, wrote a strongly worded private letter to his Soviet counterpart M. V. Keldysh.)⁴⁵ In a towering irony, *Posev*—the sister publication of *Grani*—loudly decried "psychiatric terror."⁴⁶

Clearly something worked, for Medvedev was quickly released; he was, however, not long for the Soviet Union. He had lost his position at the Timiriazev Agricultural Academy in Moscow back in 1963 because of his research into the Lysenko case, and his subsequent employer in Obninsk soon fired him as well.⁴⁷ He continued to publish abroad, and in 1973 the Soviet government granted him an exit visa so he could accept an invitation to work on the science of aging as a research fellow in the United Kingdom. His family (minus his older son) accompanied him, and he promised not to comment on politics, declaring his intention to return to the Soviet Union. The Kremlin had different plans. In September his Soviet citizenship was revoked, and he remained in exile in Britain—although with tense relations to the rest of the émigré community, which resented his commitment to Marxism.⁴⁸ (Medvedev is now ninety-two and resides in Cornwall.)

This episode substantially changed the narrative. *The Rise and Fall of T. D. Lysenko* could have been interpreted as a call for vigilance about the fragile status of Soviet genetics, or the persistence of Lysenko acolytes in high positions, or even the possibility of the continuing authority of Lysenko himself in administrative circles—all of these are clearly articulated in the

operation among scientists was soon published abroad and in English as Zhores Medvedev, *The Medvedev Papers*, trans. Vera Rich (London: Macmillan, 1971).

⁴³ Medvedev and Medvedev, *Question of Madness*, pp. 43, 45.

⁴⁴ On psychiatry as a tool for controlling dissent in the Brezhnev era see Sidney Block and Peter Reddaway, *Psychiatric Terror: How Soviet Psychiatry Is Used to Suppress Dissent* (New York: Basic, 1977); Benjamin Nathans, "The Dictatorship of Reason: Aleksandr Vol'pin and the Idea of Rights under 'Developed Socialism,'" *Slavic Review*, 2007, 66:630–663; and Mark G. Field, "Commitment for Commitment or Conviction for Conviction: The Medicalization and Criminalization of Soviet Dissidence, 1960–1990," *Soviet and Post-Soviet Review*, 1995, 22:275–289.

⁴⁵ Medvedev to Lerner, 2 June 1970, TDP, Folder: "Lerner, I. Michael #1"; and Philip Handler to M. V. Keldysh, 8 June 1970, MLP, Folder: "Medvedev, Z. A. Arrest #1." See also Sakharov, *Memoirs* (cit. n. 3), pp. 310–311. For the lobbying efforts of the Genetics Society of America see Steinberg to Lerner, 1 June 1970, MLP, Folder: "Medvedev, Z. A. Arrest #1."

⁴⁶ "Psikhiatricheskii terror: Vystupleniia A. S. Esenina-Vol'pina i K. V. Boldyreva pered komissiei senata SShA," *Posev*, 1973, 29(1):9–11.

⁴⁷ His petition to be reinstated at Obninsk is reported in Antoine Elens to Lerner, 25 Sept. 1970, TDP, Folder: "Lerner, I. Michael #3."

⁴⁸ "S kommentariiami i bez nikh," *Posev*, 1973, 29(2):21–23; "S kommentariiami i bez nikh," *ibid.*, 1973, 29(9):7–9; and Viktor Sparre, "U Zh. Medvedeva inaiia tsel' . . .," *ibid.*, 1974, 30(11):62–63.

text. That subtle story might have forced a reevaluation of Dubinin's policies among both Westerners and Soviets. Instead, "the Medvedev affair"—owing in no small part to vigorous publicity by the Medvedev brothers—was shoehorned *in the West* into an established narrative of "dissidence" patterned on the case of Solzhenitsyn.⁴⁹ The Western press referred to the arrest as "straight out of a Kafka novel" and worried that "even Sakharov himself might eventually prove vulnerable" (as indeed he was).⁵⁰ The dissidents returned the compliment, and the mobilization against Lysenko became a central part of their automythology, turning *The Rise and Fall of T. D. Lysenko* into a text of protest against the Soviet state instead of a commentary on contemporary Soviet genetics.⁵¹ The furor over the use of psychiatry switched the train onto a different track and precipitated amnesia about the ongoing politics surrounding Lysenko after 1965.

THE SORROWS OF OLD TROFIM

The title of this essay notwithstanding, Lysenko was never unemployed.⁵² Certain aspects of what has been labeled "the Lysenko affair" arguably persisted—such as Lysenko himself, his biological doctrines, and the science system that enabled the rise of both in the first place. I will defer discussion of the persistence of agrobiological doctrines to the next section, turning first to the persistence of Lysenko the man. During the eleven years after his public defrocking, Lysenko conducted himself largely as before. He was covered, more or less, by the same general amnesty that Dubinin extended to his followers. Of course, he lost his position as the director of the Institute of Genetics—that had been abolished and replaced by Dubinin's Institute of General Genetics—but he retained control of his farm in the Lenin Hills, the very same farm that had brought about his woes after Khrushchev's ouster. At the farm (which remained in operation until 1986) he supervised over a hundred researchers at the faculty level and about a hundred additional employees, and he continued to attend academy events, sit in the front row, and debate.⁵³ As Medvedev told the historian of biology Horace Freeland Judson in a 1976 interview: "He's still alive, and still chairman of this station, still academician—and so he enjoys all privileges as a scientist who has reached the top position; but he doesn't enjoy the popularity and fame now."⁵⁴

Though unpopular, Lysenko could still lobby for what he believed was his due. He repeatedly petitioned the academy leadership to provide resources to his farm to keep the cows in good health, and he battled furiously to retain his title as "scientific director," which he felt

⁴⁹ On the methodological and historiographical difficulties of characterizing all resistance to Communist regimes as dissidence see, e.g., the careful analysis in Jonathan Bolton, *Worlds of Dissent: Charter 77, the Plastic People of the Universe, and Czech Culture under Communism* (Cambridge, Mass.: Harvard Univ. Press, 2012).

⁵⁰ These quotations are from "Scientific Breakdown," *Newsweek*, 15 June 1970, pp. 44–45, but similar rhetoric and implications can be found in many other places. See, e.g., "Lysenko Rides Again," *Nature*, 1970, 226:887–888; "The Dangers of Speaking Out," *ibid.*, pp. 989–990; and "Protesting Spiritual Murder," *Time*, 29 June 1970, p. 38. The same was true in France, where Denis Buican's *L'éternel retour de Lyssenko* (Paris: Copernic, 1978) was obsessed with the case of Medvedev (pp. 173–174).

⁵¹ Vladislav Zubok, *Zhivago's Children: The Last Russian Intelligentsia* (Cambridge, Mass.: Belknap, 2009), pp. 136–137.

⁵² The title is a reference to the Polish writer Aleksander Wat's wonderful short story about an unemployed Satan who wandered Europe after the Great War looking for work in a world that had fallen so far that it no longer had any use for him: Aleksander Wat, *Lucifer Unemployed*, trans. Lillian Vallee (Evanston, Ill.: Northwestern Univ. Press, 1990).

⁵³ Soyfer, *Lysenko and the Tragedy of Soviet Science* (cit. n. 8), p. 288. The number of employees is cited by Soyfer.

⁵⁴ Horace Freeland Judson, interview with Zhores Medvedev, 16 Feb. 1976, at the Mill Hill laboratory of the Medical Research Council, London, 3:00, Horace Freeland Judson Collection, Mss.B.J92, American Philosophical Society Library, Philadelphia, Pennsylvania, p. 778. Extracts from this interview were published in Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology* (1996), commemorative ed. (Cold Spring Harbor, N.Y.: Cold Spring Harbor Laboratory Press, 2013), pp. 450–451. Medvedev said much the same in a letter to Kanemitsu, 11 May 1970, MLP, Folder: "Medvedev, Z. A. Columbia Univ. Press #3."

was part of the settlement accompanying the dissolution of the Institute of Genetics. He expected that his demands would be ignored. As he wrote in one such document in 1972, “I won’t be offended” at silence from the administration of the Biology Section, “since I am already used to the fact that after 1965 not one of my requests has been approved by the leadership of the Academy and the Section.” Thirty workers at Lenin Hills wrote a letter to General Secretary of the Communist Party Leonid Brezhnev, demanding that he

confirm academician T. D. Lysenko as the scientific director of the Experimental Scientific-Research Base “Lenin Hills”; give academician T. D. Lysenko the opportunity to work on the basis of those scientific concepts which were figured out by him and confirmed in practice; allow the publication of reports and the printing of scientific articles in the same manner as permitted in our country to all scientific-research institutions and to scientists; call to order individuals who spread rumors to all corners of the guilt and participation of acad. T. D. Lysenko in the death of N. I. Vavilov and other scientists and, it goes without saying, preserve the entirety of the Experimental Scientific-Research Base “Lenin Hills” of the AS of the USSR for the works of academician T. D. Lysenko.⁵⁵

The workers also explicitly indicted Medvedev’s book.

Historians of Soviet science have pored over Lysenko’s personnel file in the archives of the Academy of Sciences numerous times in seeking to uncover the history of his ascent to power, but there has been very little commentary on the contents of this file through to the academician’s death. These later documents make for fascinating reading, highlighting the death by a thousand paper cuts beloved of vindictive administrators. Lysenko was the scientific director of an outpost of the academy in the Lenin Hills, and that meant that he was required to report regularly on his research progress and future plans. Upon sustained prodding he would submit such reports, and then they would promptly be returned as inadequate. This had the predictable effect of enraging Lysenko. “I am convinced that the Presidium of the AS USSR will not consent to a single one of my scientific theoretical statements. But it by no means follows from this that in science it is not necessary and is even forbidden to work on those theoretical questions with which these or those scientists do not agree, even if they are members or even the chairs of the Presidium of the AS USSR,” he wrote in 1972. “For example, I fundamentally disagree with so-called molecular genetics, but because of this I am neither hot nor cold to anyone. Therefore I consider it normal that those academicians, members of the Presidium, physicists, chemists, mathematicians, who not only share these hypotheses of so-called molecular genetics, but who are simply bewitched by these hypotheses, cannot agree with my theoretical biological conceptions.” Lysenko fully appreciated the catch-22: “It is clear such a state of affairs obtained as to demand from academician Lysenko the unrealizable: to give such a plan with which the opponents of academician Lysenko’s biological conception, i.e. the opponents of the Michurinist biological conception, would agree.”⁵⁶

This was 1972, six years after agrobiolgy was ostensibly discarded, and Lysenko was still articulating the same doctrines of the inheritance of acquired characters, vernalization, and the noncentrality of genes that had characterized his views four decades earlier. Such instances

⁵⁵ T. D. Lysenko to M. D. Millionshchikov, 24 Mar. 1972, ARAN 1521/1/152/17–23, on p. 23 (see also the letters reproduced in N. V. Ovchinnikov, *Akademik Trofim Denisovich Lysenko* [Moscow: Luch, 2010], pp. 58, 178); and Lenin Hills Workers’ petition to Leonid Brezhnev, 30 Jan. 1970, ARAN 1521/1/150/125–134, on p. 133.

⁵⁶ T. D. Lysenko, “Zapis’ besedy s vitse-prezidentom Akademii nauk SSSR M. D. Millionshchikovym,” 16 May 1972, ARAN 1521/1/152/1–16, on pp. 6–7, 11.

can be multiplied many times, but a single additional example will suffice. After none other than Nikolai Dubinin returned to Lysenko a report that he felt devoted insufficient attention to how Lysenko's future research would engage with molecular genetics, Trofim Denisovich barked back:

The results of researches in my area of biological knowledge on impeccability show the erroneusness of the very foundations of Weismannist genetic conceptions in all their variations. This concerns also the present, in our country very fashionable, variant of the Weismannist conception—MOLECULAR GENETICS. Therefore in all our researches we not only do not involve “the ideas and methods of molecular genetics,” but on the basis of very convincing results of our experiments flatly REJECT WEISMANNIST GENETICS AND THE MOLECULEAR GENETICS THAT EMERGES FROM IT. The conception of molecular genetics has no relation to living nature. It is not by chance that the creators of this conception are not biologists but a small group of foreign physicists and chemists, the scientific notions of whom in no degree describe the essence of the living. In our country also a small group again of such, as a rule, non-biologists, but academician mathematicians, mechanical physicists, chemists, and physicists, are fervid supporters of molecular genetics and violent opponents of my biological conception.

The comments on genetics here could have been composed in 1937; those about foreign scientists resonate with the antic cosmopolitanism of 1948. The key point is that nothing seems to have changed to suit the circumstances of the 1970s. It is of course impossible to determine a person's sincerity, but these documents seem to bear out the supposition of contemporaries like Dobzhansky that “Lysenko himself was not a deliberate faker—he was a fanatic who deluded himself as well as others.”⁵⁷

Perhaps the most surprising aspect of the file is that not only did Lysenko know what his fellow academicians were saying about him; he was also fully aware of the historiography about his eponymous “affair” that was then cohering overseas. In one of his meetings with M. D. Millionshchikov, a turbulence specialist who served on the presidium of the Academy of Sciences, Lysenko referenced what he considered slanderous works appearing abroad and tarnishing his reputation. Millionshchikov did not miss a beat: “You are talking about ‘*The Lysenko Affair*.’ I’ve read it”; and then—one can clearly detect mischief in his tone—he continued: “Surely you, Trofim Denisovich, are not in agreement with all the statements put forward in [David] Joravsky’s book?” In his letter of complaint to the academy leadership about Millionshchikov’s behavior, Lysenko wrote: “This question so surprised me that I, as they say, simply goggled my eyes and did not choose my words, and answered him: What do you think, that I have lost my mind? As though I could agree with even one single word of D. Joravsky’s slanderous book.” It was not the only work he complained about. Unable to read foreign languages, Lysenko had *Rise and Fall* translated back into Russian and “bitterly resented” it (in the words of Roy Medvedev), and he likewise considered the widely reviled autobiography by Dubinin to be “pseudobiology.”⁵⁸

⁵⁷ Lysenko to N. P. Dubinin, 25 Sept. 1974, ARAN 1521/1/273/248–251, on pp. 248–249; and Dobzhansky, rev. of Medvedev, *The Rise and Fall of T. D. Lysenko* (cit. n. 25), p. 1509. See also Iu. Polianskii, in “Sud’by genetiki v nashem otechestve” (cit. n. 22), p. 408.

⁵⁸ Quoted in Lysenko, “Zapis’ besedy s vitse-prezidentom Akademii nauk SSSR M. D. Millionshchikovym,” 16 May 1972, ARAN 1521/1/152/4–5. Regarding Lysenko’s views on *The Rise and Fall of T. D. Lysenko* see Medvedev and Medvedev, *Question of Madness* (cit. n. 31), p. 51; for his appraisal of Dubinin’s autobiography see T. D. Lysenko, “Skol’ko pakhnotnoi zemli po N. P. Dubininu prikhoditsia na cheloveka?” 5 Oct. 1973, ARAN 1521/1/273/316–320, on p. 316.

Lysenko stuck around the academy, which meant that he was hard to ignore. It was in this atmosphere that the historian of science Loren Graham entered the palatial dining hall of the Academy of Sciences late in 1971. Graham, then about to publish his history of dialectical materialism, *Science and Philosophy in the Soviet Union*—which included a substantial chapter about genetics—had made efforts to interview Lysenko for years but had never been able to gain access.⁵⁹ Looking for a place to sit, Graham saw “a gaunt and homely man” sitting alone at a table and “immediately recognized Lysenko.”⁶⁰ He sat next to him uninvited (a common enough practice in Moscow). Not only did Graham recognize Lysenko; Lysenko recognized Graham—and defended himself against Graham’s writings in which the American had attributed responsibility for N. I. Vavilov’s death to the agronomist. After all, he—Lysenko—had never even been a member of the Communist Party! (This point, at least, was true.) Graham pushed back, and Lysenko stormed off.

A few moments later, however, he returned and switched from attack to defense: “And, once again, I am now an outsider. Why do you think I was sitting alone here at this table when you came up? No one will sit with me. All the other scientists have ostracized me.” Lysenko continued: “I sympathize with the Jewish refuseniks. They are scientists who have been ostracized by the Soviet establishment because they applied to emigrate to Israel, were turned down by the Soviet authorities, and now they have no jobs and no place to turn. They are alone like me.” Graham, understandably, considered this apologia “grotesque,” but he was not the only one to hear it. Lysenko publicly approached refuseniks like Veniamin G. Levich, and he even ran up to none other than Andrei Sakharov after the latter had been censured by the academy leadership in 1973. As Sakharov later recalled: “Lysenko apparently wanted to show that we were both being persecuted, he and I.”⁶¹

In short, Lysenko felt sorry for himself. “I am certain that in the history of the biological sciences of all time,” he wrote to the government, “no single scientist has been subjected to such persecution, lambasting, and defamation as I am subjected to, because I cannot, I do not have the right to defend myself to the smallest degree.”⁶² From Lysenko’s point of view, he was the victim. He had served the Soviet state to the best of his capacities—much like Sakharov, who had designed the hydrogen bomb for Stalin—and now the state had turned on him because of fashionable new doctrines and the scheming of careerists.

AGROBIOLOGY’S SHADOW

Lysenko’s self-presentation hinged on his being without allies, an obvious exaggeration of the case. After all, plenty of his supporters kept their positions after their patron fell. Even Dubinin, architect of the policy of clemency, noted the persistence of Lysenko’s ideas, if not his personal authority: “However, his influence and that of his theories on the consciousness of many peo-

⁵⁹ “Lysenko himself was described as being in semiretirement and refused to grant interviews to visiting delegations and reporters”: Graham, *Science and Philosophy in the Soviet Union* (cit. n. 3), p. 251. David Joravsky, whose classic history of these events appeared in 1970, managed to interview Lysenko’s ideological aide-de-camp I. I. Prezent but not Lysenko himself: Joravsky, *Lysenko Affair* (cit. n. 6), p. xi. Prezent died on 15 Sept. 1969.

⁶⁰ Unless otherwise indicated, quotations in this and the following paragraph—including those of Lysenko—are from Loren R. Graham, *Moscow Stories* (Bloomington: Indiana Univ. Press, 2006), pp. 121–124.

⁶¹ Andrei Sakharov, quoted in Soyfer, *Lysenko and the Tragedy of Soviet Science* (cit. n. 8), p. 289. The connection might go deeper. Loren Graham reports that some Soviet scientists told him that they had opposed firing Lysenko out of a desire to protect Sakharov (private communication, 28 Feb. 2015). Inviolability of the “academician” status was an important precedent to maintain.

⁶² Lysenko to the government, undated [early 1970s], ARAN 1521/1/128, reproduced in Ovchinnikov, *Akademik Trofim Denisovich Lysenko* (cit. n. 55), p. 181. In the same letter Lysenko declares contemporary research on heredity “pseudoscientific” (*lzhе-nauchnyе*) (p. 182).

ple, especially from the *nomenklatura* strata of society, was great. The roots of the *lysenkovshchina* were deep; they stretch down to our times.” Astaurov, who hated the policy, concurred, noting in his posthumously published reminiscences that “Lysenko’s omnipotence has ended, although the dominance of the *lysenkovshchina* continued for a long time.”⁶³ Both use the terminological latecomer “*lysenkovshchina*” to stress the systemic features of Soviet science, tying those features to the symbolism of Lysenko himself, and this proved a rhetorically effective way to displace discussion of “agrobiology” among geneticists. The term was never deployed publicly while Lysenko was alive; it is, in itself, a marker of a later phase of the story. Both comments, however, point to the lingering influence of agrobiologists after the amnesty.

For example, Lysenko continued to be shielded from criticism until well after his death. Although in 1965 the academy was free to criticize Lysenko in its private deliberations, those deliberations were not printed. The closest thing to criticism one can find is the publication, in the literary journal *Novyi mir*, of a defense of dialectical materialism in the context of a rehabilitation of genetics by Bonifatii M. Kedrov, a distinguished philosopher and historian of science; along the way he taught his readers a fair amount of classical genetics. The first section of Kedrov’s piece was entitled “Things Are Beginning to Be Called by Their Names.”⁶⁴ This did not last long. Archival documents have revealed that in 1967 Vladimir Stepakov, head of the Ideology Department of the Central Committee of the Party, forbade the publication of any anti-Lysenko materials. A case in point is I. T. Frolov’s *Genetics and Dialectics*, published in 1968. Frolov’s brief was to reclaim genetics, long attacked as intrinsically incompatible with dialectical materialism, for the official philosophy of science and in the process condemn the philosophical pronouncements of Lysenko and his “followers” and “supporters.” In May 1969 Frolov’s book was publicly criticized as written “from a one-sided position” and withdrawn from general circulation.⁶⁵

Open criticism of the legacy of agrobiology was not permitted until 1987, well into Mikhail Gorbachev’s *glasnost’*. The first breaches against the silence came through literature. In 1984 Vladimir Dudintsev—widely known for his novel *Not By Bread Alone* (1956), an indictment of bureaucratic inefficiency that fit seamlessly into the public discourse of Nikita Khrushchev’s Thaw—serialized in the literary journal *Neva* his novel *White Robes* (*Belye odezhdyy*), a lengthy narrative chronicling the disillusionment of an honest agrobiologist in the wake of Lysenko’s seizure of power. (Dudintsev had visited Medvedev in the mental asylum.) In 1986 the journal *Iunost’* published V. I. Amlinskii’s memoir of his father, a historian of biology who had been repressed under Lysenko, entitled *Opravdan budet kazhdyi chas: Povest’ ob ottse* (*Each Hour Will be Redeemed: A Tale of My Father*). And in 1987 *Novyi mir* serialized D. A. Granin’s novel *Zubr*, a sympathetic portrait of the persecuted geneticist Nikolai Timofeev-Ressovskii.⁶⁶

⁶³ N. P. Dubinin, *Istoriia i tragediia sovetskoi genetiki* (Moscow: Nauka, 1992), p. 3; and V. I. Ivanov, “Svetlaia pamiat’—svetlomu cheloveku,” in Boris L’vovich Astaurov: *Ocherki, vospominaniia, pis’ma, materialy*, ed. O. G. Stroeva (Moscow: Nauka, 2005), p. 287. See also Maxim W. Mikulak, “Darwinism, Soviet Genetics, and Marxism-Leninism,” *Journal of the History of Ideas*, 1970, 31:359–376, esp. p. 374; V. Ia. Aleksandrov, *Trudnye gody sovetskoi biologii: Zapiski sovremennika* (St. Petersburg: Nauka, 1992), pp. 221–222; and Adams, “Biology after Stalin” (cit. n. 11), p. 78.

⁶⁴ B. Kedrov, “Puti poznaniia istiny: Razdum’ia o sud’bakh estestvoznaniia,” *Novyi mir*, 1965, 41(1):213–235.

⁶⁵ I. T. Frolov, *Genetika i dialektika* (Moscow: Nauka, 1968). For the ban on publication of anti-Lysenko materials see Soyfer, *Lysenko and the Tragedy of Soviet Science* (cit. n. 8), p. 290; and “Sud’by genetiki v nashem otechestve” (cit. n. 22), p. 443 (including the criticism of Frolov’s book).

⁶⁶ Vladimir Dudintsev, *Belye odezhdyy: Roman* (Moscow: Sovetskii pisatel’, 1988); Vladimir Amlinskii, *Opravdan budet kazhdyi chas: Roman i povest’* (Moscow: Moskovskii rabochii, 1986); and Daniil Granin, *Zubr: Povest’* (Moscow: Sovetskii pisatel’, 1987). Only the last has been translated into English: *The Bison: A Novel about the Scientist Who Defied Stalin*, trans. Antonina W. Bouis (New York: Doubleday, 1989).

A public roundtable in Moscow on 3 June 1987 used these three works to launch a detailed exposition of the history of what they termed the *lysenkovshchina*. At the roundtable, Iu. I. Polianskii, a corresponding member of the Academy of Sciences who had served on the commission that recommended publication of Medvedev's book in the mid-1960s, lamented that Lysenko's views had, "unfortunately, penetrated rather deeply into the mass of Soviet people." With some colleagues, he undertook to publish a "General Biology" textbook for lower schools and edited the journal *Biologiia v shkole* (*Biology in the Schools*) for several years. "All this reconstruction (*perestroika*) was unbelievably hard. But it continues to this day, since again there appear brochures (an entire series has already appeared)"—referring to the pamphlets by B. G. Ioganzen and E. D. Logachev, published out of the Kemerovskii region's division of the popular-science journal *Znanie* from 1985 to 1987—"that, in essence, directly propagandize for *lysenkovshchina*."⁶⁷ Even under Gorbachev, many politically savvy biologists working in the Soviet Union feared that agrobiological might stage a comeback.

The timing was not what they had feared, and surely it would be too much of an exaggeration to call it a comeback, but this constant drumbeat of worry about the lurking danger of Lysenko's followers provides some perspective on the surprising resurgence of agrobiological rhetoric in Vladimir Putin's Russia (and not only there). Starting shortly after the turn of the millennium, articles and books began to appear that either lamented Lysenko's comeback or proclaimed it with full-throated glee (often combined with denials of Stalin's engineered famine in Ukraine and other staples of extremist Russian-nationalist discourse). At first, such endorsements emerged in the context of neo-Stalinist polemics and masked their lack of biological heft with *ad hominem* attacks, mischaracterizations of the historical record, and vitriol (as pointed out by numerous critics).⁶⁸ This relatively marginal discourse received an unexpected boost with the emergence of epigenetic research in biology.

Briefly, epigenetics explores changes in the development of the cell traceable to influences outside of chromosomal DNA (e.g., how changes in nutrition, physiological environment, and so on can shape or transform the expression of genes) and—in its most controversial variants—how these changes in gene expression could be inherited by later generations. Read in a certain light, this can sound like claiming that environmental influences can be inherited and perhaps that classical genetics propounds a misleading picture of heredity. That is, it can be cast to resonate with agrobiological.⁶⁹

⁶⁷ Iu. A. Polianskii, in "Sud'by genetiki v nashem otechestve" (cit. n. 22), pp. 405–406.

⁶⁸ For a fuller discussion of this debate see esp. Graham, *Lysenko's Ghost* (cit. n. 7), Ch. 8. For the original sources see, e.g., Mikhail Anokhin, "Akademik Lysenko i bednaia ovechka Dolli," *Literaturnaia gazeta*, 18–24 Mar. 2009, no. 11 (6215), p. 12; "Vavilov i Lysenko," *Novyi Peterburg*, 19 Oct. 2015, no. 41; Iurii Mukhin, *Prodazhmaia devka Genetika* (Moscow: Bystrov, 2006); Sigizmund Mironin, *Delo genetikov* (Moscow: Algoritm, 2008); Ovchinnikov, *Akademik Trofim Denisovich Lysenko* (cit. n. 55); V. I. Pyzhenkov, *Nikolai Ivanovich Vavilov—botanik, akademik, grazhdanin mira* (Moscow: Samoobrazovanie, 2009); and Mikhail Anokhin, "Nakormivshie lozh'iu," *Lit. gaz.*, 4 Feb. 2015, no. 5 (6495). For critiques see V. A. Dragavtsev, "O fal'sifikatsii istorii sovet'skogo rastenievodstva: Pis'mo v 'Literaturnuiu gazet'u,'" *Politicheskaia kontseptologiya*, 2015, no. 1, pp. 247–249; E. I. Kolchinskii, "Publichnyi donos kak sposob 'nauchnykh diskussii,'" *ibid.*, pp. 237–246; Leonid Korochin, "Vo vlasti nevezhestva: Neolysenkovshchina v rossiiskom soznanii," *Lit. gaz.*, 6–12 Mar. 2002, no. 9 (5869); and I. A. Zakharov-Gezekhus, "Popytki reabilitatsii lysenkovshchiny (o publikatsiakh P. F. Kononkova, N. V. Ovchinnikova, V. I. Pyzhenkova)," *Ist.-biol. issledovaniia*, 2011, 3(2):124–129.

⁶⁹ Florian Maderspacher, "Lysenko Rising," *Current Biology*, 2010, 20:R835–R837. Maderspacher tends to define all exploration of environmental regulation of genes as "Lysenkoism," eliding a complex scientific and political history. See Jan Sapp, *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics* (New York: Oxford Univ. Press, 1987); and Maurizio Meloni, *Political Biology: Science and Social Values in Human Heredity from Eugenics to Epigenetics* (London: Palgrave Macmillan, 2016).

This wave of argumentation was accelerated by a 2011 article in the *Journal of Biosciences* by three Chinese scholars who argued for a separation of (good) Michurinism from (bad) Lysenko.⁷⁰ The critique of the classical gene, in this variant, is lauded, while the repression of geneticists, endorsement by the state, and especially the Marxist dress of neo-Lamarckian biology are condemned.⁷¹ Within Russia, however, such discourse may be more than appropriation; indeed, it might be a continuation of the rocky process of resisting the disestablishment of agrobiology. The most striking example is *Unknown Lysenko*, a short book published in 2014 by Lev Anatol'evich Zhivotovskii. In structure, this work resembles the polemical books: classical Mendelian genetics presents an oversimplified picture of heredity; practical agronomic manipulation can produce better crop yields; there is little direct evidence of Lysenko's involvement in the worst of the repressions. Different are the more measured tone and the status of the author. Zhivotovskii is a doctor of biological sciences, winner of a state prize from the Russian Federation, the Shmal'gauzen prize in evolutionary biology, and a prize from the British medical journal the *Lancet*; moreover, part of the book was written while he was on research leave at Stanford University. His stated goal was to "return T. D. Lysenko's scientific name to its appropriate place given the level of his scientific discoveries, without being silent about the negative sides of his activity." The work was, of course, met with vigorous refutation from a host of biologists and historians, but it remains the case that Zhivotovskii is one of the leading geneticists in Russia today.⁷² In fact, he directs a lab at the Vavilov Institute of General Genetics at the Russian Academy of Sciences—which makes him, after a fashion, Nikolai Dubinin's successor.

CONCLUSION

The account provided in this essay of the unsettled history of agrobiology in the last decade of Trofim Lysenko's life (and beyond) sits uneasily with the conventional history in the form of the classic "rise and fall" narrative. Although significantly deepened by subsequent scholarship, the general tendency has remained to see a more-or-less evenly matched battle between Michurinists and geneticists in the 1930s up to the mid-1940s, followed by an absolute victory for Lysenko (and absolute defeat for genetics) in 1948, and then the reverse in 1965. Yet, as we have seen, plenty of geneticists carried on under Lysenko's regime, albeit under difficult conditions; why would one assume anything different for the agrobiologists after 1965?

Seen this way, Lysenko's "triumph" was in some ways a continuation of the pitched battles of the 1930s, in which sometimes he would emerge on top and sometimes he would receive a slap from the higher-ups. Criticism of agrobiology was allowed to appear in the *Botanicheskii zhurnal* in 1952—quickly rebuffed, but nonetheless a blow—and Lysenko's star was in eclipse from Stalin's death in 1953 until a few years later, when Nikita Khrushchev threw his support behind the agronomist.⁷³ Nikolai Vavilov had been rehabilitated by the state on 20 August

⁷⁰ Yongsheng Liu, Guangyin Wang, and Xiuju Li, "Michurin's Legacy to Biological Science," *Journal of Biosciences*, 2011, 36:13–16. See also Jaroslav Flegr, "Was Lysenko (Partly) Right? Michurinist Biology in the View of Modern Plant Physiology and Genetics," *Rivista di Biologia/Biology Forum*, 2002, 95:259–272.

⁷¹ The only pro-Marxist account I have seen is "Lysenko, Michurin, and Epigenetics," <http://www.socialistphalanx.com/t787-lysenko-michurin-and-epigenetics> (accessed 20 Sept. 2014).

⁷² Lev Zhivotovskii, *Neizvestnyi Lysenko* (Moscow: T-vo nauchnykh izdaniy KMK, 2014), p. 7. See also Zhivotovskii's website: <http://en.vigg.ru/institute/subdivisions/independent-divisions/laboratory-of-genetic-basis-of-identification/> (accessed 14 Feb. 2015). A selection of Russian reviews, as well as links to Zhivotovskii's defense, is available at <http://vigg.ru/news/news-single/article/vne-nauki/> (accessed 28 June 2017).

⁷³ Aleksandrov, *Trudnye gody sovetskoi biologii* (cit. n. 63), p. 150; E. I. Kolchinskii and M. B. Konashev, "Kak i pochemu 'Pravda' uchila 'Botanicheskii zhurnal'?" *VIET*, 2003, no. 4, pp. 49–74; and Raissa L. Berg, *Acquired Traits: Memoirs of a Geneticist from the Soviet Union*, trans. David Lowe (New York: Viking, 1988), pp. 213–214.

1955, and the reorganizations of the Academy of Sciences in 1957 and 1963 were both interpreted at the time as attempts to isolate genetics from Lysenko's domination. Indeed, in 1961 Michael Lerner, watching from his perch at Berkeley, wrote: "In the area of genetics and evolution, the 1948 party line is no longer binding and Mao's hundred flowers are apparently encouraged to blossom." Lysenko lost control of VASKhNIL, from which he had ousted Vavilov decades earlier, for the *second* time in April 1962, having regained temporary leadership of it only in August 1961.⁷⁴ The fall was a process—or, perhaps more accurately, one could see Lysenko as experiencing a series of "mini-falls," and then recoveries, over and over again. It makes sense that he and some of his supporters would interpret 1965 as another move in the extended chess game that was his career. As the *Washington Post* commented in 1965: "He is down again, but it would be premature to count him altogether out. Once before, Lysenko seemed to be in utter disfavor."⁷⁵

Premature it would have been; but although there were severe constraints on the extent to which geneticists could attack Lysenko and agrobiology during the 1970s, a similar silence was also imposed on his supporters. Lysenko's name never appeared in *Pravda* or *Izvestiia*, the official newspapers, from the time of his censure by the academy until late November 1976. At that moment a brief suspension of the cloak of silence was permitted, and—on the final page of the broadsheet—a small, black-bordered box announcing his death appeared. (See Figure 1.)

The occasion of Lysenko's death prompted another round of condemnation from Western commentators, who had seemingly forgotten all about his aggravating existence in the wake of "the Medvedev affair." Not only could these obituaries proclaim again the folly of injecting "ideology" into science—something that ostensibly happened only in the darkest depths of totalitarianism—but they occasionally revised even the chronology of the fall to push it further away from the present: "With Stalin's death [i.e., 1953], Professor Lysenko crashed from the Communist heights with an echo heard in far countries." (To be fair, even this piece acknowledged that Lysenko was never fired.) There is a feeling of a class reunion about some of these publications, as stalwart anti-Lysenkoists from the 1940s, such as the venerable British biologist C. D. Darlington, emerged from retirement to excoriate their old foe once more.⁷⁶ But not all of them. Theodosius Dobzhansky had passed away the previous year, and although I. Michael Lerner outlived Lysenko by six months, he refrained from comment.

A reexamination of Lysenko's rise and fall—indeed, even the terms "rise" and "fall" themselves—requires restructuring our account of the single most influential episode of the clash between science and politics, ideology and knowledge. It is difficult to overstate how significant the traditional understanding of "the Lysenko affair" has been to science policy debates around the globe from the 1940s to the present, and his name has been ballyhooed in controversies over anthropogenic climate change and vaccination and in other contemporary clashes over

⁷⁴ I. Michael Lerner, "Blossoms of 100 Flowers in Soviet Genetics," *Science*, 1961, 134:1974. On the interpretations of the academy reorganizations see Adams, "Biology after Stalin" (cit. n. 11), p. 73; and Konstantin Ivanov, "Science after Stalin: Forging a New Image of Soviet Science," *Science in Context*, 2002, 15:317–338. Regarding Lysenko's loss of control over VASKhNIL see Gaisinovich, *Zarozhdenie i razvitie genetiki* (cit. n. 10), p. 326; and John Turkevich, "Soviet Science Appraised," *Foreign Affairs*, 1966, 44:489–500, esp. p. 497.

⁷⁵ "Lysenko's Fall," *Washington Post*, 13 Feb. 1965, p. A12.

⁷⁶ Harrison E. Salisbury, "Symbol of Stalinist Science," *New York Times*, 24 Nov. 1976, p. 31; and C. D. Darlington, "Obituary: T. D. Lysenko," *Nature*, 1977, 266:287–288. For other obituaries see Robert C. Toth, "Lysenko, Soviet Charlatan, Dies," *Los Angeles Times*, 24 Nov. 1976, pp. 1, 8; "Genetics 'Quack' Lysenko Dies," *Guardian*, 24 Nov. 1976, p. 3; "Soviet Biologist Lysenko, Denounced as Fraud, Dies," *Chicago Tribune*, 24 Nov. 1976, p. 15; and "Trofim Lysenko, Tried to Link Biology, Marxism," *Boston Globe*, 24 Nov. 1976, p. 31.



Figure 1. Lysenko’s obituary in *Izvestiia*, the only mention of him since his loss of power. The text reads: “The Presidium of the Academy of Sciences of the USSR, Section of General Biology of the AS USSR, and the experimental scientific-research base ‘Lenin Hills’ of the AS USSR announce that on 20 November 1976 in the 79th year of life there died Hero of Socialist Labor, laureate of State prizes academician Trofim Denisovich Lysenko, and they express sympathy with the family of the deceased.” Source: *Izvestiia*, 23 November 1976, no. 276 (18424), p. 4.

political regulation of scientific claims. By these measures, Lysenkoism was *the* catastrophe of modern science, in particular in its “value-free” incarnation, a threat that lurks for anyone foolish enough to attempt to distort truth according to political preferences.⁷⁷ The nature of this catastrophe has been strongly shaped by a general sense that we are living in the aftermath of it and therefore can draw lessons from it with objectivity and dispassion. This somewhat self-satisfied rendering is enhanced by the fact that all the canonical histories of this episode were created within a few years of each other, within a few years of his fall, and in full view of their primary villain: Trofim Denisovich Lysenko.

This episode raises two major points. First, there is the persistent tacking between historical events and historiographical analysis. In an idealized picture, these two aspects of any historical inquiry are separated: one is supposed to establish, as far as the evidence allows, what the historical events were and how they unfolded; and then, in an analytically distinct process, the historian situates the result within interpretations of related episodes. In this essay I have continually muddled these two aspects because they were not indeed distinct. The most significant book-length accounts of the history of agrobiolgy, which still form the touchstones of scholarship, were published in the period in which the historical events of Medvedev’s imprisonment and Lysenko’s tangles with the Academy of Sciences took place, and *all* subsequent his-

⁷⁷ The theme of “value-free science” has, of course, a long history. For good discussions of its roots and persistence see Robert N. Proctor, *Value-Free Science? Purity and Power in Modern Knowledge* (Cambridge, Mass.: Harvard Univ. Press, 1991); and Sandra Harding, *Objectivity and Diversity: Another Logic of Scientific Research* (Chicago: Univ. Chicago Press, 2015).

stories use either Medvedev's original manuscript or Lerner's translation as a crucial source. Lysenko and his opponents read this historiography and reacted to it, and the historians intervened in the history—witness David Joravsky, the author of the most widely cited Anglophone history of agrobiolgy's rise and fall, protesting against the treatment of Medvedev, the most prominent Soviet critic. Historians are also historical actors; this is not new. Less widely appreciated is the ubiquity of the historiographical reframing of historical debates in the sciences—and that this reframing requires scrambling the idealized picture of historiography. In the specific case of Lysenko, we see that such an approach can uncover new historical causalities: the politics of dissidence and the struggles within the Soviet genetics establishment served to “freeze” some hallmarks of the narrative, especially its periodization and rise–fall structure, long after the documentary record began to reveal a more complex picture. The blurring of boundaries between history and historiography is more visible because of the recentness of the story and its high political stakes, to be sure, but the blurring itself is much more common than historians usually remark.

The second point concerns an asymmetry in how historians treat the losing side in scientific controversies. Although historians of science broadly agree that it takes time for a scientific field to become established, far less attention has been paid to how a field now deemed antiquated, misguided, or downright dishonest becomes *disestablished*. That asymmetry is drawn from the actors themselves, who see the real story as lying with the science now deemed correct, not that which has been discarded, and so less attention is paid to the institutions that had buttressed the now anathematized doctrines. The Lysenko story serves as a reminder that even in this case once considered the hallmark of the discredited and fraudulent, there is a long and persistent echo, and that echo is sustained by *institutions*, not personalities like Lysenko who drew most of the infamy. It is not simply the case, as Thomas Kuhn famously quoted Max Planck, that “a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die,” for Lysenko's death was far from the end.⁷⁸ Understanding the complexity of how discredited sciences lose their backing is just as salient an issue as the converse.

⁷⁸ Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (1962; Chicago: Univ. Chicago Press, 1970), p. 150.