Wishful Science:
The Persistence of T. D. Lysenko's Agrobiology in the Politics of Science

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ABSTRACT

The suppression of genetics in Soviet Russia was the big scandal of twentieth-century science. It was also a test case for the role of scientists in a liberal democracy. The intellectual's perennial dilemma between scientific truthfulness and political loyalty was sharpened by acute ideological conflicts. The central topic of this essay is how the conflict was played out in Soviet agricultural and biological science in the 1930s and 1940s. The account is focused on the role of the then current Soviet science policy and its basic epistemic principles, the "unity of theory and practice" and the "practice criterion of truth."

INTRODUCTION

The Soviet alternative to international genetic science was called Lysenkoism after its leading figure, the agronomist Trofim Denisovich Lysenko. He called it agrobiology or Michurinism, after a gardener with ambitions to become a Russian Luther Burbank.[1] During the cold war ideological standoff, Lysenkoism was the central topic in the history of Soviet science. Stalinist tyranny combined with scientific ignorance, opportunism, and moral deviousness among the Lysenkoists, was depicted as primary causes of the tragedy. With the fall of the Soviet system well behind us, the time

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Abbreviations used in this article: ARAN—Archive of the Russian Academy of Sciences; *BV—Biulleten' VASKhNIL* (bulletin published by the Lenin Academy of Agricultural Science); SGS—Stenographic report from the conference on genetics and selection organized by the editorial board of the journal *Under the Banner of Marxism*, 7–14 Oct. 1939; *SRSKh—Sotsialisticheskaia Rekontruktsiia Sel'skogo Khoziaistva* (journal published jointly by the Lenin Academy of Agricultural Science and the Ministry of Agriculture); TsGANKh—Tsentral'nyi Gosudarstvenuyi Arkhiv Narodnogo Khoziaistva SSR (Central State Archive for the National Economy), Moscow; VL—(Vavilov Letters, 1929–40) *Nauchnoe nasledstvo*, vol. 10, *Nikolai Ivanovich Vavilov, iz epistoliarnogo naslediia 1929–1940 gg*. (Moscow, 1987).

[1] *Agrobiology* is a selection of Lysenko's works, first published in Russian in 1948, with a number of new editions and translations into foreign languages.

is ripe for a reconsideration of the nature and significance of Lysenkoism, paying more attention to the scientific and science policy issues.[2]

Science policy is the area in which science and politics overlap. It faces both ways and coordinates the two kinds of activity. The Soviet Union was the first country in the world to provide generous state support for science and technology. With the first five-year plan and the collectivization of agriculture, starting in 1928, a systematic and centralized science policy was formed. In natural and technological sciences, funds were increased, researchers recruited, and new institutions formed at breathtaking speed.[3] As late as the 1970s, the Soviet Union was far ahead of any other country in sheer numbers of scientists and engineers. This all-out effort laid the foundation of successes such as the Soviet thermonuclear bomb and the first Earth satellite. It also created the research system in which Lysenko made his career.

The Bolshevik enthusiasm for science as a motor of social progress appealed to liberal and left-wing scientists and intellectuals in the West. The British physical chemist and Communist J. D. Bernal set Soviet science and technology policy up as a paradigm for the rest of the world.[4] But there was also grave concern that such a centralized, state-governed organization of science would, in the long run, suppress essential intellectual freedom and undermine scientific as well as social progress. In 1940 the Oxford zoologist John Baker, together with the Hungarian-born physical chemist Michael Polanyi and the prominent Oxford ecological botanist Arthur Tansley, formed the Society for Freedom in Science in direct response to Bernal's ideas.[5]

Dividing resources between basic and applied research, between theoretical and practical science, is a persistent plight for the politics of science. One apparent escape is to say pragmatically that practical effect is what counts in the end and thus there is no important distinction to be made. This was the spirit of the slogan "unity of theory and practice," which dominated the politics of Soviet science in the 1920s and 1930s. The implication was that "pure" theoretical research is an abstract and useless activity, "cut off" from the working masses.[6] However, the dilemma between basic and applied science was nothing special to Soviet science. It still persists, as does the popularity of the pragmatic solution.[7]

The difference between theoretical and practical science points to a broader issue of cultural politics concerning the role of intellectuals. Is it important for rational politics to distinguish between, on the one hand, politically neutral theoretical science that is valid for all and serves common goals and, on the other, practical science engaged in solving specific social and technological problems and thus directly bound to certain political and economic agendas? Attempts at sharp separation would undoubtedly be harmful; the interaction between theory and practice has always been

[2] An attempt in this direction is Nils Roll-Hansen, *The Lysenko Effect: The Politics of Science* (Amherst, N.Y., 2005).

[3] A. G. Korol, *Soviet Research and Development: Its Organization, Personnel, and Funds* (Cambridge, Mass., 1965), 21.

[4] J. D. Bernal, *The Social Function of Science* (London, 1939).

[5] William McGucken, "On Freedom and Planning in Science: The Society for Freedom in Science, 1940–46," *Minerva* 16 (1978): 42–72.

[6] N. I. Bucharin et al., *Science at the Cross Roads*: *Papers Presented to the International Congress of the History of Science and Technology Held in London from June 29 to July 31, 1931, by the Delegates of the USSR,* 2nd ed. (1931; London, 1971).

[7] See, e.g., Michael Gibbons et al., *The New Production of Knowledge* (London, 1994); D. E. Stokes, *Pasteur's Quadrant: Basic Science and Technological Innovation* (Washington, D.C., 1997).

a major source of scientific progress. Nevertheless, isn't an understanding of the difference between theoretical understanding and practical action essential for proper organization of their interaction?

A corresponding distinction applies to scholarship and cultural activity in general. Throughout the twentieth century, liberal democrats tried to avoid radical politicization of cultural activities, seeing themselves as defending classical Enlightenment ideals against attacks from the Communist Left as well as the Fascist Right. In addition to Michael Polanyi, mentioned above, the Jewish-French philosopher Julien Benda, the American sociologist Robert Merton, and the Austrian-British philosopher Karl Popper are examples of well-known defenders of a nexus between scientific freedom and liberal democracy.

The Lenin Academy of Agricultural Science plays a central role in the following account because it epitomized the revolutionary Soviet science policy. Established in June 1929, it was to be "the academy of the general staff of the agricultural revolution"—the general staff being the Ministry of Agriculture.[8] Collectivization was conceived as a technoscientific as well as a social revolution. Working in tandem, a new science and a new social organization were to bring unprecedented progress in agriculture. Production would increase at the same time that excess labor would be transferred to industry. Agricultural science thus appeared as the key to solving two main problems: feeding the population and manning the new industries.

HISTORICAL INTERPRETATIONS OF LYSENKOISM

With the official ban on genetics in the Soviet Union following the August 1948 congress of the Lenin Academy, "Lysenko" and "Lysenkoism" became household words in the West. They referred to the paradigmic example of how the Soviet regime had betrayed scientific truth and intellectual freedom.

Julian Huxley, evolutionary biologist and the first general secretary of UNESCO, gave his diagnosis in 1949: "the major issue at stake was not the truth or falsity of Lysenko's claims, but the overriding of science by ideological and political authority."[9] In 1948–49, a flood of articles revealed the faults of Lysenkoism. Up to the 1960s, most of the literature on Lysenkoism was written by biologists and other natural scientists. In addition to Stalinist tyranny, Marxist theory of science and Lamarckian ideas of heredity were central factors of explanation for Lysenkoism.[10] The first comprehensive historical account of Lysenkoism, by Soviet biologist Zhores Medvedev, was published in 1969.[11] Written during the 1960s as part of the internal struggle to get rid of Lysenko, the account had circulated widely as samizdat before being translated into English and published in the United States. This balanced and insightful overview remains the most readable general account of the Lysenko story.

The publication in 1970 of *The Lysenko Affair* by the American historian of science

[8] *Sotsialisticheskoe zemledelie* [Socialist agriculture], 22 Jan. 1930, 2; article on the basic tasks of the Lenin Academy of Agricultural Science written by Nikolai Vavilov, its first president.

[9] Julian Huxley, *Soviet Genetics and World Science: Lysenko and the Meaning of Heredity* (London, 1949), ix.

[10] See, e.g., H. J. Muller, "The Destruction of Science in the USSR" and "Back to Barbarism—Scientifically," *Saturday Review of Literature*, 4 and 11 Dec. 1948; R. B. Goldschmidt, "Research and Politics," *Nature* 109 (4 March 1949): 219–27; Conway Zirkle, *Evolution, Marxian Biology, and the Social Scene* (Philadelphia, 1956).

[11] Zhores Medvedev, *The Rise and Fall of T. D. Lysenko* (New York, 1969).

David Joravsky marked a new epoch in the historiography of Lysenkoism. He started his research on the assumption that Lysenko had at least boosted farm yields—"why else would commissars of agriculture repeatedly say so?" But even this minimal expectation of rationality was disappointed. In the end, Joravsky saw no role for rational scientific or science policy arguments in Lysenkoism. He concluded that it was a "romantic" Western myth that Marxist theory in alliance with scientifically outdated Lamarckian theories of heredity were important causes of Lysenkoism. In his view, Lysenkoism "rebelled against science altogether. Farming was the problem, not theoretical ideology. Not only genetics but all the sciences that impinge on agriculture were tyrannically abused by quacks and time-servers for thirty-five years."[12] In spite of such romantically exaggerated conclusions, Joravsky's book remains a classic, a treasury of interesting facts and sharp analyses. Another leading American historian of Soviet science, Loren Graham, agreed that Lysenkoism had little to do with either Marxist theories about science or serious issues in biology. The "Lysenko episode was a chapter in the history of pseudoscience rather than the history of science."[13] I will argue that it is true that Lysenko ended up by rejecting sound science. But many leading Russian biologists approved of his early work. Its theoretical interest and potential practical importance were recognized internationally. The interesting problem is what took Lysenko and his followers into the sphere that Graham calls "pseudoscience."

An alternative explanation, taking Marxist theory of science seriously, was sketched in the mid-1970s by supporters of the radical science movement. Richard Lewontin and Richard Levins saw Lysenkoism as "an attempt at scientific revolution"—a genuine attempt to transform science into a better instrument for social justice and progress. In their view, Lysenkoism raised important unsolved problems about the relationship of theoretical science to practical work. As practicing biologists, they also found Marxist philosophy to be fruitful in their own research. In other words, the issues of scientific method and science policy raised by Lysenkoism could not be so easily dismissed. Similar views were argued by left-wing intellectuals in France.[14]

This neo-Marxist "dialectical" perspective on Lysenkoism was not developed beyond the level of a sketch. But it serves to remind us that well beyond the Second World War the sociopolitical project of the Soviet Union had a broad appeal not based in repression and terror. Genuine enthusiasm also carried the project. For instance, the leading physicist and later dissident Andrei Sakharov wrote in a private letter about his grief at Stalin's death in 1953: "I am under the influence of this great man's death. I am thinking of his humanity."[15] Not until after Gorbachev made a last attempt to save the Soviet project did the vision completely fade away.

Valery Soyfer's *Lysenko and the Tragedy of Soviet Science* (1994) is valuable both for its wealth of detailed information and for the intense participant perspective.[16] As a student at the Timiriazev Agricultural Academy in the 1960s, and later faculty

[12] David Joravsky, *The Lysenko Affair* (Cambridge, Mass., 1970), ix.

[13] Loren Graham, *Science and Philosophy in the Soviet Union* (New York, 1972), 195.

[14] Richard Lewontin and Richard Levins, "The Problem of Lysenkoism," in *The Radicalisation of Science: Ideology of/in the Natural Sciences*, ed. Hilary Rose and Steven Rose (London, 1976), 32–64; Dominique Lecourt, *Proletarian Science? The Case of Lysenko* (London, 1977).

[15] Andrei Sakharov, *Memoirs* (New York, 1990), 164.

[16] Valery Soyfer, *Lysenko and the Tragedy of Soviet Science* (New Brunswick, N.J., 1994). Russian edition: Valerii Soyfer, *Vlast' i nauka: Istoriia razgroma genetikii v SSSR* [Power and science: The history of the rout of genetics in the USSR] (Tenafly, N.J., 1987).

member, he became involved in the last phase of the struggle against Lysenkoism—and its aftermath. The fall of Lysenko in 1964 did not bring immediate change in the governance of Soviet science. Soyfer's book was written in a dissident mood in the early 1980s, when the regime still suppressed historical investigations of Lysenkoism. His dissident late Soviet perspective still labors under the yoke of the cold war. The genuine dilemmas and hard choices that always face an uncertain science play a secondary role in his account. During the rise of Lysenko, from first national fame at the end of the 1920s to Lysenko's presidency of the Lenin Academy in 1938, the best road to fruitful development and sound application of agricultural biological science was not as clear as it appeared half a century later.

A new generation of Russian historians of science writing in the post-Soviet period have emphasized the normal social features of science under Stalin's regime. The complex integration of scientific and political establishments implied an intimate two-way relationship rather than simple subordination of science to politics.[17] Scientists were under political control but were also indispensable advisers to the government. Their political leverage could be used in competing with each other. This was nothing unique to Soviet science, although the centralized and brutal nature of the regime made stakes higher. In this institutional perspective, the Lysenko episode was due to a fashion that affected all of Soviet science. The social mechanisms driving "Stalinist science" are much the same in the traditional totalitarian and the new institutional account. Scientific issues play a minor role. The early success as well as the later fall of Lysenko is mainly described and explained in terms of ritualized public "discussions," lobbying political bosses, and so on. But strong moral condemnation has given way to a more relaxed and social relativist attitude.[18]

Alexei Kojevnikov's account of the 1948 debacle demonstrates the fruitfulness of the new institutional approach. Under mature Stalinism, the rules of intraparty democracy were extended to science, and scientific issues decided accordingly. Higher (political) authorities defined the problem and set the stage for a "free" discussion followed by a "vote." The result was binding and the losing side was obliged to make self-criticism (repent). Kojevnikov shows how Lysenko provoked the top leadership to set up such a game at a time when he was still in a key position to pick participants.[19] This explanatory model makes clearer why the fate of genuine science and scholarship could differ so much between disciplines. In 1948 a monopoly was given to Lysenko's obscure agrobiological alternative to genetics. In 1950 the followers of Nikolai Marr were denied a similar monopoly in linguistics. In 1949 plans for a corresponding "discussion" in physics was simply called off. The intraparty style of democratic centralism, as well as the strong confidence of the top political leaders

[17] See, e.g., Alexei Kojevnikov, "President of Stalin's Academy: The Mask and Responsibility of Sergei Vavilov," *Isis* 87 (1996): 18–50; Nikolai Krementsov, *Stalinist Science* (Princeton, N.J., 1997); Alexei Kojevnikov, "Dialogues about Knowledge and Power in Totalitarian Political Culture," *Historical Studies in the Physical and Biological Sciences* 30 (1999): 227–47; Kojevnikov, *Stalin's Great Science* (London, 2004); Nikolai Krementsov, *International Science between the World Wars: The Case of Genetics* (London, 2005).

[18] See, e.g., Krementsov, *Stalinist Science* (cit. n. 17), 58–60. This view was to a considerable extent anticipated by Mark Adams, who stressed the adaptability of scientific institutes to different political regimes and demands without much change in the substance their research. See, e.g., his "Science, Ideology, and Structure: The Kol'tsov Institute, 1900–1970," in *The Social Context of Soviet Science*, ed. L. L. Lubrano and S. G. Salomon (Boulder, Colo., 1980), 173–204.

[19] Kojevnikov, *Stalin's Great Science* (cit. n. 17), 207–14.

in their own scientific judgment, made for erratic results. However, broad consensus among the representatives of the scientific community was respected.[20]

VAVILOV AND LYSENKO

Nikolai Vavilov is usually described as the staunch defender of science who stood up against Lysenko's pseudoscience, was struck down by political intervention, and finally suffered martyrdom in the cause of genetics. He can also be described as a fellow traveler who understood too late the threatening nature of the science policy he was involved in. Before discussing this view, it is important to lay out a few facts about the economic setting and the two main actors.

Before the First World War, Russia was a big exporter of grain and other foodstuffs. With the civil war came food shortage and hunger. From then on, agricultural production and food supply proved a chronic problem for the Soviet government. Agriculture symbolized by grain production was at the center of public attention. Great efforts to improve the situation had little success. This painful economic failure is an ominous backdrop to the story of Lysenkoism.

Trofim Lysenko was born 1898 in a peasant family. He received poor and late basic education. His scientific training was extramural. Undoubtedly a gifted, intelligent, and inspiring person, he emerged as the leader of a school that rejected standard genetics. In 1938 he became president of the Lenin Academy of Agricultural Science, and he organized the 1948 congress that condemned classical genetics. Only in 1965 was he finally deposed from his dominant position in agricultural science and biology.

Nikolai Vavilov was ten years older than Lysenko and the son of a rich self-made merchant of peasant origin. He received thorough scientific training in Russia and abroad. From the early 1920s into the mid-1930s, he was the main entrepreneur of Soviet agricultural science and president of the Lenin Academy from 1929 to 1935. After first supporting and protecting Lysenko, he turned in the late 1930s to sharp opposition. Vavilov was arrested in 1940 and perished in prison three years later.

Vavilov was a liberal progressive, not a Marxist or a socialist. His view of science and its social role was similar to that of the British physicist and longtime editor of *Nature* Richard A. Gregory. Gregory's *Discovery, or the Spirit and Service of Science*, first published in 1916, was translated into Russian in 1923, edited and prefaced by Vavilov.[21] Gregory shared the demands of radicals such as Bernal, J. B. S. Haldane, and Joseph Needham that science must become more relevant and active in solving pressing social problems but did not share their socialist ideology. Vavilov similarly shared the utopian sentiments that motivated the policy of forced collectivization and did not heed warnings that collectivization would destroy the social structure of the agricultural community. His model was the United States, where more than 17 million people had left the farms between 1910 and 1920.[22] Lenin also revered the American agricultural revolution. He is said to have had *The New Earth*,[23] a glorification of the recent American agricultural revolution, as bedside reading.

[20] Ibid., 216.

[21] R. A. Gregory, *Otkrytiia, tseli i znachenie nauka* (Petrograd 1923).

[22] Mark Popovskii, *Delo akademika Vavilova* [The file of academician Vavilov] (Ann Arbor, Mich., 1983), 38–9.

[23] W. S. Harwood, *The New Earth* (New York, 1906). Russian translation: V. S. Garvud, *Obnovlenie zemlia* (Moscow, 1909).

The world collection of plants was Vavilov's big project. Cultivated plants and their wild relatives were collected from all over the world to serve as material for breeding. This was the world's first large-scale gene bank, a grand and foresighted idea. But as the country became caught up in a crash program of modernization, there was diminishing understanding of and support for such long-term scientific investments.

Lysenko was a junior researcher in the already vast system of Soviet agricultural research when he first attracted public attention through a presentation in *Pravda* in 1927: a young "barefoot professor" at the experimental station of Gandzha in Azerbaidzhan had found a way to extend the period of growth and "make the fields green in winter," quite independently of academic science.[24] The impressed journalist did not consider how much milder winters were south of the Caucasus.

Lysenko's research at Gandzha was published in a small monograph, *Effects of the Thermal Factor on the Duration of Phases in the Development of Plants* (1928).[25] The central idea is that a certain "sum of heat" measured as "degree-days" is needed for a plant to pass through each of its developmental phases, from germination to flowering and ripe fruit. Eventually, this idea developed into Lysenko's theory of stages in the development of plants. He built on earlier work by the leading Soviet cotton specialist Gavril Zaitsev, a friend and colleague of Vavilov's working in Tashkent.[26]

The developmental physiology of plants, in particular the effects of light and temperature, was a new area in research. The intense interest was due not least to potential agricultural applications. Theoretical thinking and experimental methods in Lysenko's work had weak aspects and received deserved criticism, but its methods and problems were typical of the state of the art.[27]

In January 1929 Lysenko presented a paper on the effects of cold treatment on germinating wheat at a big national conference on biological agricultural science in Leningrad.[28] The leading Soviet expert in this field was Nikolai Maksimov, head of the plant physiology lab in Vavilov's Institute for Plant Cultivation. Maksimov had been critical of Lysenko, not least of his unwillingness to listen and learn, and he wanted to reject Lysenko's paper to the congress. But Vavilov found the paper sufficiently original and promising.[29] Maksimov attended Lysenko's lecture with critical remarks, but he also gave Lysenko's ideas ample room in the paper that summed up this section of the congress.[30]

[24] Vitaly Fyodorovich, "Polia zimoi" [The fields in winter], *Pravda*, 7 Aug. 1927, 6.

[25] T. D. Lysenko, *Vliianie termicheskoge faktora na prodolzhitel'nost' faz razvitiia rastenii*; *opyt co zlakami i khlopchatnikom* [Effects of the thermal factor on the duration of phases in the development of plants: Experiments with grasses and cotton], *Trudy Azerbaidzhanskoi tsentral'noi opytnoselktsionnoi stantsii, im .tov. Ordzhonikidze v gandzhe* [Works of the Central Experimental Station of Azerbaidzhan named Comrade Ordzhonikidze] (Baku), 1928, no. 3.

[26] See S. Reznik, *Zaveshchanie Gavrila Zaitseva* [The testament of Gavril Zaitsev] (Moscow, 1983).

[27] See, e.g., Roll-Hansen, *The Lysenko Effect* (cit. n. 2), 58–64.

[28] While normal annual plants germinate and produce seeds within the same year, winter annual plants germinate in the late autumn, pass the winter as small seedlings, and then set flower and seed the following spring or summer. If a winter annual plant is sown in the spring, it will not flower, or will flower poorly, during that same year. Winter rye and winter wheat are examples of winter annual plants. Spring rye and spring wheat are normal annual plants completing a whole life cycle within the same year (growth season).

[29] Semion Reznik, *Nikolai Vavilov* (Moscow, 1968), 268.

[30] N. A. Maksimov and M. A. Krotkina, "Issledovaniia nad posledeistviem ponizhionnoi temperatury na dlinu vegetatsionnogo perioda," *Trudy po prikladnoi botanike, genetike i selktsii* [Works on applied botany, genetics, and selection] 23, no. 2 (1929/30): 427–78.

VERNALIZATION—PROMISES AND FAILURES

The success of Lysenko's vernalization research had a serious economic background. Grain was the key agricultural product, and harsh winters that killed the seedlings of winter sowings became a critical problem. This happened extensively in both 1927 and 1928, on the eve of the first five-year plan.

The ambitious young Lysenko was not quite satisfied with the attention he had received at the Leningrad conference and decided to prove the importance of his work with a striking public demonstration. He instructed his peasant father to soak some sacks of winter wheat seed grain, bury them in a snowdrift, and then sow at the ordinary time for spring wheat. As summer came, the field of Lysenko's father stood out as a wonder to the peasant community, according to local and national newspapers. After sensational presentations in the press and on-site inspections from agricultural specialists and government authorities, Lysenko was given a new job at the Ukrainian Institute for Selection and Genetics at Odessa. He became head of a new laboratory for *iarovizatsiia*. It was well known that winter grain could be sown in the spring and produce a normal harvest if slightly germinated seed had been subjected to a period of low temperature. The task of Lysenko's group was to further investigate this phenomenon and see whether useful practical methods could be developed.

A school of followers grew around Lysenko and his theory of stages in the development of plants. Their work soon attracted international attention. From 1929 on, the two Imperial Bureaux of Plant Genetics, British institutions established in 1928 to serve communication of new knowledge in plant breeding and applied plant physiology, was instrumental in making Lysenko's work known.[31] A scientist of the bureaux introduced the term "vernalization," a Latinized version of the Russian iarovizatsiia, which remains the standard scientific term today. As late as the 1970s, Lysenko's name was also routinely in textbook accounts of vernalization, as a token of his status as a founding father in this field.

In the 1920s and 1930s, the physiology of plant development was internationally perceived as an unexplored field with high potential for practical applications—in other words, a promising field, one vulnerable to wishful thinking. When the world's leading cotton specialist met Lysenko in 1933 and characterized him as "a biological circle squarer," Vavilov answered liberally that an "angry species" such as Lysenko who "walked by faith and not by sight" might make some useful discovery, perhaps even "how to grow bananas in Moscow." It did no harm and might do some good to let him go on working.[32]

It soon turned out that the vernalization of winter seed grain was not practical, and Lysenko shifted to vernalization of spring grain. He claimed that this would counteract troublesome summer droughts by speeding up ripening. The method was introduced on a mass scale without much testing. When leading agricultural experts asked

[31] Roll-Hansen, *The Lysenko Effect* (cit. n. 2), 64–8, 142–8. The system of Imperial Agricultural Bureaux was expanded in 1928. Among the new bureaus established was an Imperial Bureau for Plant Genetics in Cambridge and another one in Aberystwyth, for "crops other than herbage plants" and for "herbage plants," respectively. See W. R. Black, "Imperial Agricultural Bureaux," *Journal of the Ministry of Agriculture*, 1929, 461–7, on 465.

[32] S. C. Harland on "The Lysenko Controversy," *The Listener*, 9 Dec. 1948, 873.

for more precise experimental trials,[33] Lysenko angrily suggested that his opponents were "wreckers" trying to sabotage the agricultural revolution.[34]

Vavilov was reviewer for the Commissariat of Agriculture. As late as July 1935, he was vigorously defending Lysenko's vernalization of seed grain as "a great achievement" in general agrotechnology.[35] But after rapid expansion in the mid-1930s,[36] the procedure was quietly reduced to empty ceremony and disappeared.[37] Only Lysenko's reputation for practical achievements remained due to the positive propaganda of the mass media and the suppression of public scientific criticism.

VERNALIZATION OF THE WORLD COLLECTION

The main reason for Vavilov's enthusiastic view of vernalization was its applications to plant breeding. An impatient government decree of July 1931 demanded that new varieties of grain be produced in four to five years instead of the former ten to twelve years. Vernalization was a promising tool in responding to this pressure by making effective use of the world collection of plants. Many foreign plant varieties would not develop normally in the Russian climate but could be made to do so with the help of vernalization. This was essential both to investigate their hereditary properties and for manipulating flowering to make hybridization possible.

In March 1932 Vavilov approached the Odessa institute and Lysenko for cooperation on vernalization of the world collection.[38] At the Sixth International Congress of Genetics, held in Ithaca, New York, in August 1932 he praised the "remarkable discoveries" of Lysenko and the "enormous new possibilities" they opened to plant breeders. [39] In a lecture at the Leningrad House of Scholars in April 1933, Vavilov told how plant breeding "last year unexpectedly received help from physiology."[40]

At this time Vavilov's Institute of Plant Industry (Vsesoiuzny Institut Rastenievodstva, VIR) in Leningrad was seething with political trouble. Graduate students wanted revolution in science. During Vavilov's half-year travel to the United States, a number of his leading collaborators were arrested or forced to leave the institute. Without experienced older scientists, the research would not be effective, Vavilov tried to explain to the Young Turks in January 1934. In particular, he worried that they were not sufficiently interested in vernalization. In May, he once more asked Lysenko for help.[41]

[33] P. N. Konstantinov, P. I. Lisitsyn, and D. Kostov, "Neskol'ko slov o rabotakh Odesskogo instituta selektsii i genetiki" [Some words on the works of the Odessa institute of selection and genetics], *SRSKh*, 1936, no. 11:121–30.

[34] T. D. Lysenko, "Otvet na statiu 'Neskol'ko slov o rabotakh odesskogo instituta selektsii i genetiki' akad: Konstantinova P. N., akad. Lisitsyna P. I., i Doncho Kostova" [Reply to the article "Some words on . . ."], *SRSKh*, 1936, no. 11:131–8.

[35] F. 8390, op. 1, e. 604, ll. 85–9, TsGANKh.

[36] R. O. Whyte, "History of Research in Vernalization," in *Vernalization and Photoperiodism*, ed. A. E. Murneek and R. O. Whyte (Waltham, Mass., 1948), 1–38, 9.

[37] See Eric Ashby, *Scientist in Russia* (London, 1947), 115.

[38] Vavilov to F. S. Stepanenko and T. D. Lysenko, 29 March 1932, VL, 165.

[39] Nikolai Vavilov, "The Process of Evolution in Cultivated Plants," in *Proceedings of the Sixth International Congress of Genetics, Ithaca, New York, 1932*, vol. 1 (New York, 1933), 331–42, 340.

[40] N. I. Vavilov, "Problema selektsii v SSSR" [Problems of selection in the USSR] (manuscript of lecture to be given at Doma Uchionykh in Leningrad, 28 April 1933), f. 8390, op. 1, ed. khr. 284, ll. 48ff., TsGANKh.

[41] See various letters from Vavilov to Russian colleagues, including Lysenko, in the period August 1932 to May 1934, VL.

RESEARCH PLANNING

Planning became a fashion in Soviet science in the early 1930s, under the leadership of Nikolai Bukharin. And Vavilov, president of the Lenin Academy, was an active participant.[42]

Bukharin's general view of science was strikingly presented at the 1931 International Congress for History of Science in London. A delegation of prominent Soviet scientists arrived spectacularly in a special airplane at the last minute. Bukharin, head of the delegation, was the most scholarly and intellectual of the top Soviet leaders; among the participants was Nikolai Vavilov. Bukharin explained the scientific revolution happening in the Soviet Union: "the rupture between intellectual and physical labour" was being eliminated and scientific research was rising to a new level of efficiency. His prime example was plant breeding.[43]

In spite of the great scientific efforts, agricultural production declined, and there was widespread hunger in the early 1930s. A government decree of July 1934 stated that the Lenin Academy "had not fulfilled the basic task to which it had been assigned." Inadequate organization and narrow specialization were among the basic faults. Among the few bright spots were Lysenko's vernalization and Vavilov's world collection of plants.[44]

In June 1935, the Lenin Academy was radically reorganized. Lysenko was one of the fifty new academicians (members) appointed. Many were bureaucrats rather than scientists, some without any scientific training. An old Bolshevik and former vice minister of agriculture, A. I. Muralov, took over as president. Vavilov became a vice president.

LYSENKO MOVES INTO GENETICS: NEW WHEAT VARIETIES IN RECORD TIME

Vavilov's interest in vernalization of the world collection stimulated Lysenko's shift to plant breeding and genetics. But although many established experts received his early research in plant physiology well, the reactions to his ideas on breeding and genetics were generally negative. That environmental conditions could somehow influence heredity in a direct and adaptive manner represented an interesting possibility that could not be discarded out of hand. Such neo-Lamarckian ideas were, in fact, pursued extensively by one of Lysenko's staunchest opponents, the plant geneticist Anton Zhebrak. But Lysenko's experiments and arguments indicated scientific incompetence rather than originality.

Lysenko and his co-workers had a holistic approach to plant development, seeking a unified theory of development and heredity. They stressed the interaction of environmental and hereditary, internal and external, factors. Heredity was a property of the organism as a whole. The chromosomes had no special role. Thus the theory of stages in the development of plants was extended to include heredity.[45] This biological ho-

[42] *Trudy vsesoiyznoi konferentsii po planirovaniio genetiko-selektsionnykh issledovanii* [All-union conference on the planning of genetics and breeding research] (Leningrad, 1933).

[43] N. I. Bucharin, "Theory and Practice from the Standpoint of Dialectical Materialism," in Bucharin et al., *Science at the Cross Roads* (cit. n. 6), 11–33, 15–6.

[44] *Izvestiia,* 20 July 1934, 1.

[45] A. Favorov, "Theoretical and Practical Significance of Lyssenko's Research on the Vernalization of Agricultural Plants," *Herbage Reviews* 1 (1933): 9–14.

lism provided Lysenko with a bridge from developmental physiology to genetics. Lysenko presented his ideas in a small book coauthored with his ideological and political adviser, I. I. Prezent.[46] As proof of the truth of his statements, Lysenko announced the creation of new and superior early ripening spring wheat varieties in only two and a half years. In early June 1935, members of the Lenin Academy reviewed the work of Lysenko's team in Odessa.

Two weeks later, the presidium of the Lenin Academy met in Moscow. Lysenko was not present. G. K. Meister, a prominent plant breeder and party member who had just been appointed as one of the vice presidents, argued that Lysenko simply did not understand what genetics was about, and even Muralov wondered whether Lysenko, in his laudable boldness, had not crossed the border to unhealthy extremism. But Vavilov defended him passionately. Why had the critics not spoken up in Odessa? Of course the new strains had not yet been fully tested, said Vavilov, but "[w]e were singing the praise of the method," not of an "accidental new variety." If Meister and some of the other speakers had been at Odessa, they would have understood how hard it was to find a way to confront youthful enthusiasm and "impatience" with tactful corrections. Vavilov also reminded the meeting that a worldwide critical revision of the principles of genetics was taking place—for example, the Morgan school of Drosophila genetics, founders of the chromosome theory of heredity, had been criticized for lack of "dialectical depth" by another prominent Soviet breeder and geneticist, A. A. Sapegin.[47]

THE INTERNATIONAL CONGRESS AND THE "TWO DIRECTIONS IN GENETICS"

In 1932, Vavilov asked the Soviet government for permission to invite the Seventh International Congress of Genetics, scheduled for 1937, to convene in Moscow. But the ministry was cold about the idea. In fact, it only allowed a couple of scientists to go to the Sixth Congress, held in New York State, in 1932. Vavilov urged Lysenko to go, but without success. Sweden was chosen as the place for the next congress. But when the Swedes withdrew their offer in the summer of 1935, Vavilov seized the opportunity and was able to obtain permission from the government as well as assent from the Permanent International Committee on Genetics to hold the next congress in Moscow.

Vavilov acknowledged, in his letter to the chairman of the International Committee, Norwegian medical doctor and geneticist Otto Mohr, that Soviet genetics was young and inexperienced, but he counted on help from people such as American geneticist H. J. Muller, then working in the Soviet Union. (Yet Muller, in a letter to his friend Mohr, had just expressed doubt that Soviet genetics was ready for the demanding task of holding a congress and worried that it would divert energy from research.) By the end of 1935 a local organizing committee had been set up with Muralov as president. Among the members were Vavilov, Lysenko, the botanical ecologists Boris Keller and Vladimir Komarov, and the experimental biologist Nikolai Kol'tsov.

[46] T. D. Lysenko and I. I. Prezent, *Selektsiia i teoriia stadiinogo razvitiia rasteniia* [Selection and the theory of the development of plants through stages] (Moscow, 1935). Prezent was a political activist with some training in law. He played a central role in developing a theory of "creative Darwinism" for education and other popularization of science in the 1930s. After Vavilov had turned down Prezent's offer of advisory services, he joined Lysenko in 1932.

[47] F. 8390, op. 1, ed. khr. 604, ll. 85–93, TsGANKh.

The human geneticist Solomon Levit was secretary, and Muller was in charge of the program. Both Keller and Komarov had Lamarckian sympathies and were skeptical of classical genetics. Keller was an active player in Soviet science politics and a supporter of Lysenkoist ideas. Komarov was to become president of the Academy of Science in the following year, 1936.

Through 1935–36, the Lenin Academy and the Ministry of Agriculture staged a broad debate on dialectical method in the science of biological heredity. A primary purpose was to sort out differences between classical genetics and Lysenkoist agrobiology. There were sharp disagreements, for instance, on the handling of seed production in highbred plants, and the ministry demanded a resolution. Growing political tension on the eve of the Great Terror (1936–38) made open scientific debate difficult, especially on ideologically sensitive topics such as dialectical method. Nevertheless, a number of leading biologists and agricultural scientists contributed. Vavilov, however, remained conspicuously silent. To conclude the debate, the Lenin Academy organized a conference on "the two directions in genetics" in December 1936. At the opening, Muralov proclaimed that based on "the Marxist-Leninist-Stalinist worldview" and a rejection of "fascist 'theories' of race," a comprehensive examination of different genetic theories was to "provide unity of method" for practical breeding work.[48] This tall order expressed bureaucratic voluntarism rather than scientific rationality.

The coming international congress was an underlying issue for the conference delegates. Vavilov and the geneticists saw such a congress as a possible way to stem the Lysenkoist tide and promote genuine genetic science. Not surprisingly, Lysenko objected to the congress, which threatened to undermine his scientific standing. The tense and ominous situation was underscored by the arrest of Levit before the December conference. As an active party member Levit was vulnerable. He had also spent a year in Muller's lab in Texas on Rockefeller money and was director of the Institute for Medical Genetics, an institution suspected of promoting eugenic ideas.[49]

Hard-hitting scientific criticism of Lysenko's genetic ideas from Muller and the leading Soviet geneticist, A. S. Serebrovskii, could not prevent the congress from turning into a public relations catastrophe for genetics. The defiant foreigner Muller, himself a proponent of eugenics on socialist premises, made the fateful step of arguing that Lamarckism rather than Mendelism was the eugenically obnoxious theory. This unleashed a violent campaign tarnishing Serebrovskii and other geneticists for earlier eugenic utterances. Vavilov kept a low profile, primarily defending his own Institute of Plant Industry. Lysenko launched a frontal attack on classical genetics. Geneticists and plant breeders were ranged against him, but a number of other influential leaders in biological science supported many of his views. In the Soviet mass media, genetics came off as a theory full of bourgeois metaphysical prejudice, one holding up the practical advance of agriculture and harboring dubious sympathies with eugenics.

In the meantime, a special commission had reported to the Soviet government that preparations for the congress were lagging and that on the preliminary program there

[48] A. I. Muralov, "Zadachi dekabr'skoi sessii" [Tasks of the December session], *BV*, 1936, no. 12:1–3, on 2.

[49] V. V. Babkov, "Medical Genetics in the Soviet Union," *Herald of the Russian Academy of Sciences* 71 (2001): 553–61.

were many "Fascists."[50] On November 17, the Council of Ministers (the Soviet cabinet) decided to cancel the congress for 1937.[51]

But only gradually through rumor, unreliable mass media messages, and private communication did information about the canceling of the congress reach the Permanent International Committee on Genetics. On December 14, 1936, the *New York Times* reported that Vavilov had been arrested, that other Soviet geneticists were being threatened with arrest, and that the 1937 congress in Moscow had been canceled. A week later, an editorial in *Izvestiia* reacted to this "slander": Vavilov had not been arrested, and the congress had only been postponed at the request of Soviet geneticists. Furthermore: "Real freedom for research work, real intellectual freedom exists only in the USSR," where science works for the benefit of the people and not a small group of capitalists, claimed the editorial. As was later revealed, Stalin himself had heavily edited the article.[52]

On January 7, 1937, Mohr wrote to Muralov, with copies to Vavilov and Levit: "it is urgently needed that you send me immediately detailed information on the situation." On February 13, Muralov and Vavilov sent an answer with the brief explanation that many scientists and institutions had wanted a postponement, adding that the government had now permitted the international congress to be held in 1937.[53] Included, however, were bulletins from the December 1936 genetics conference. Mohr and others could now read for themselves the arguments used against classical genetics.

For Mohr and most other members of the international committee, the clandestine political interventions to control the program of the congress were irreconcilable with traditional liberal ideals of science. Increasing political terror and the beginning of Moscow prosecutions soon made it clear that the Soviet Union was no longer a suitable place. A congress in Moscow would threaten to undermine genuine science by obscuring the differences between science and politics. Through the spring of 1937, Mohr maneuvered diplomatically to gain time. He needed to form for himself, and communicate to the members of the international committee, a trustworthy picture of the situation. By July 1937, he had become convinced that holding the congress in Moscow would be a grave mistake and sent a new memorandum to the members of the international committee recommending that the congress be moved from Moscow to Edinburgh, to be held in 1939.[54] Some left-leaning Western geneticists, such as Muller, J. B. S. Haldane, and the American representative to the committee, Leslie Dunn, continued to support a congress in Moscow. It would be a betrayal of Soviet genetics to move it, they argued. To Mohr it was a relief when Muller admitted in November 1937 that Mohr had been right and that now even the Russian geneticists agreed.[55]

[50] Memo dated 8 Oct. 1936, f. 201, op. 3, d. 10, ll. 1–3, ARAN.

[51] A more detailed version of the following account of the cancellation of the Moscow genetics congress is found in Roll-Hansen, *The Lysenko Effect* (cit. n. 2), 230–43. A different interpretation with detailed descriptions of the workings of Soviet institutions is found in Krementsov, *International Science* (cit. n. 17), 42–72.

[52] For a detailed account, see Krementsov, *International Science* (cit. n. 17), 45–52.

[53] Mohr to Muralov, 7 Jan. 1937. Muralov and Vavilov to Mohr, 13 Feb. 1937. Copies of these letters were later sent to all members of the International Committee of Genetics together with a memorandum in which Mohr briefly described what had happened, expressed grave concern for free scientific debate, and stated that he was investigating alternative countries for the congress. See, e.g., Federley Papers, University Library, University of Helsinki, Helsinki.

[54] Federley Papers, University Library, University of Helsinki.

[55] Mohr to Muller, 26 Nov. 1937, Muller Archives, Lilly Library, Indiana University, Bloomington.

SUPPRESSING FREEDOM OF SPEECH

As the international committee struggled to get reliable information about the state of affairs in the USSR, events moved quickly and dramatically in Soviet genetics. On January 7, 1937, at a meeting for authors and staff of the state publishing house for agricultural literature, the newly elected president of the Academy of Sciences, Komarov, complained that knowledge of Darwin was scandalously low.[56] And the minister of agriculture, I. A. Iakovlev, explained in a crassly polemical speech how classical Mendelian genetics was incompatible with Darwinism.[57]

In an atmosphere full of fear, one prominent biologist dared to challenge the political control over science. Although Nikolai Kol'tsov had actively opposed the October coup of the Communists in 1917, he had been able to preserve and expand his school of research. By 1939 he was the grand old man of Russian experimental biology. He was the one who read out Muller's speech to the December 1936 conference, and he saw clearly how fatal the image of genetics communicated by the mass media could become to the future of genetics in the USSR. Therefore he wrote to Muralov asking for the publication in *Pravda* and *Izvestiia* of "extensive articles written by genuine geneticists in defense of their science." But in the presidium of the Lenin Academy, the scientists—Vavilov, M. M. Zavadovskii, and G. K. Meister—feared a political confrontation and chose not to support Kol'tsov.[58]

Toward the end of March 1937, the Lenin Academy summoned a special session for all staff and members to discuss its own troubled affairs in the light of the new Soviet constitution, officially declared to be "the most democratic in the world." The curiously distorted debates at this meeting, called an aktiv, vividly demonstrate how political conformism, fear for one's own career, and narrow administrative logic can cooperate to suppress scientific freedom and autonomy.

Muralov accused Kol'tsov of eugenic views unacceptable under the new democratic constitution. But Kol'tsov defiantly repeated that the academy had failed to correct misleading and highly tendentious press reports. An open and democratic discussion in the spirit of the new constitution was just what he wanted. His former statements on eugenics had been quoted out of context and misinterpreted. For their time and context, his claims were legitimate and scientifically well founded. Kol'tsov found no reason to take back a word of what he had said.[59] His opponents were infuriated by this unwillingness to repent and perform self-criticism. How could this man, Kol'tsov, dare to pose proudly as a modern Galileo in defense of science? asked the scientific secretary.[60] The scientists were silent.

The difficult balance between political-bureaucratic and scientific authority was the central topic of the aktiv. Vavilov reminded the audience that the 1935 reorganization had been aimed at getting rid of an inefficient bureaucratic system, but all Muralov's selfless efforts had just made the situation worse.[61] D. N. Prianishnikov,

[56] V. L. Komarov, "Izdavat' Timiriazeva i Darvina" [Publishing Timiriazev and Darwin], *SRSKh*, 1937, no. 4:27–9.

[57] Ia. A. Iakovlev, "O darvinisme i nekotorykh antidarvinistakh" [On Darwinism and some anti-Darwinians], *SRSKh*, 1937, no.4:17–26, 24–5.

[58] A. E. Gaissinovich and K. O. Rossianov, "'Ia gluboko ubezhdion chto ia prav . . .'" [I am deeply convinced that I am right . . .], *Priroda,* 1989, no. 5:86–95; no. 6:95–103.

[59] F. 8390, op. 1, ed. khr. 954, ll. 82–4, TsGANKh.

[60] Ibid., ll. 37–9.

[61] F. 8390, op. 1, ed. khr. 956, ll. 42–8, TsGANKh.

an expert on fertilization and a staunch liberal defender of genuine science, pointed to the central bureaucracy's lack of contact with active scientific research as a problem. The reorganized Lenin Academy of Agricultural Science's preoccupation with short-term practical results made the academy fail in its primary task of planning and organizing scientific research.[62]

In his closing speech, Muralov challenged Vavilov on his specialty of rust-resistant wheat. Muralov had actively directed such research through instructions to the research stations. "Should I just keep away from this?" he asked rhetorically. "Absolutely," replied Vavilov from his seat. "Is it not the task of the presidium to organize research so that new varieties can be produced as quickly as possible?" retorted Muralov. "[Y]ou call this bureaucratic interventionism, but we call it organization of research." Vavilov answered, "You should take advice from the best specialists," giving a couple of names. "But you did not mention Lysenko and Tsitsin,"[63] continued Muralov. Vavilov then held his tongue. "I will fight to produce new varieties as quickly as possible," concluded Muralov.[64]

VICTORY AND CONSOLIDATION OF LYSENKOISM

Soon after this aktiv, the leadership of the Lenin Academy was hit by the terror. As elsewhere in Russia, active Communist Party members were particularly at risk. During the summer of 1937, Muralov and the scientific secretary were arrested. Meister, also a party member, became president, but a few months later he was arrested. For a period in the autumn and winter of 1937–38, Vavilov acted as academy president. Thus the Great Terror had the unintended effect of removing the last hurdles to Lysenko's ascent. On February 28, 1938, he was appointed president of the Lenin Academy, the top position in Soviet agricultural science.

In his new position, Lysenko promoted agrobiology and harassed genetics. The official government policy, however, still supported open competition between the two directions in genetics to allow the theory that proved most successful in "practice" to triumph. Keeping Vavilov and Zavadovskii as vice presidents of the academy was an expression of this balancing policy.

In March 1939, the Eighteenth Congress of the Communist Party marked the end of the Great Terror, and it again became possible to challenge the hegemony of Lysenkoism. A group of Leningrad biologists wrote to Andrei Zhdanov, the party secretary responsible for science, about the lack of open public debate on genetics. Free competition between the two directions in genetics was suppressed by administrative power, they claimed. On Zhdanov's initiative, a second conference "On the Controversy in Genetics and Breeding" was held in October 1939 under the auspices of the party's theoretical journal, *Under the Banner of Marxism*.

The philosophical staging consolidated Lysenkoism. While the 1936 conference had been summarized and evaluated by a genuine geneticist and breeder, Meister, the 1939 conference was summarized and evaluated by a philosopher, Mark Mitin. He mildly criticized Lysenko for not taking seriously enough chromosomes and other facts produced by classical genetics and his supporters for occasionally failing to

[62] F. 8390, op. 1, ed. khr. 954, ll. 97–100, TsGANKh.
[63] A leading plant breeder who gradually turned away from Lysenko in the 1940s and 1950s.
[64] F. 8390, op. 1, ed. khr. 956, ll. 84–6, TsGANKh.

observe rules of proper scholarly conduct. But, Mitin stated, Lysenko's practical achievements were beyond doubt: "Against this nobody has said, or could say anything from the rostrum of our meeting, because these things have been introduced into practice, into life, and have been widely disseminated."[65] Mitin disregarded Vavilov's sharp criticism that the Ministry of Agriculture had introduced new methods on Lysenko's recommendation with no support from "experimental data whatever."[66] And when Zhebrak asked for a commission to evaluate Lysenko's new, useless varieties of wheat, he was quickly put down by Chairman Mitin.[67] Yet Mitin's praise of Lysenko was not without a kernel of truth: the practical usefulness of the vernalization of seed grain was taken for granted. Vavilov did not criticize Lysenko on this point, and the two most knowledgeable experts, P. I. Lisitsyn and P. N. Konstantinov, who had raised the issue in 1936, were not present.

A certain redressing of the balance between Lysenkoism and classical genetics followed. Some of Lysenko's attempts to interfere in internal affairs of Vavilov's Institute of Plant Industry and the academy's Institute of Genetics were stopped. But the general effect was to strengthen the ideology of "unity of theory and practice" that had helped Lysenko all along. Many Western left-wing scientists continued to accept the official Soviet claim to an open scientific debate subject to the practice criterion of truth. The theories of both "the schools of Lysenko and Vavilov alike are subjected to the acid test of practice," the British embryologist Joseph Needham had written in 1938.[68]

But in the tense international situation the balance was precarious. The Hitler-Stalin pact of 1939 obscured the difference between enemies and friends. Vavilov had been under surveillance by the secret police since the early 1930s. By 1940, there was a large file on him, including numerous denunciations for political disloyalty, betrayal, and sabotage; in August 1940, he was arrested and charged with spying for the British. He was indeed working with his British contacts to achieve the publication of *Theoretical Basis of Plant Breeding*[69] in English.[70] This was a three-volume handbook, edited by Vavilov, summing up the main results of Soviet plant-breeding research. Ironically, it contained a broad and very positive presentation of Lysenko's contributions.

A regular rout of classical genetics followed Vavilov's arrest. A number of his colleagues in Leningrad were also arrested. A Lysenko sympathizer took over the Institute of Plant Industry, and Lysenko himself became director of the academy's Institute of Genetics, in Moscow. Kol'tsov was removed as director of the Institute for Experimental Biology and died soon after. A Lysenkoist became his successor. In spite of this dramatic setback, the research and teaching of classical genetics did survive in a number of institutes and university departments. But most important, the events of 1939 and 1940, symbolized by Vavilov's fate, made it clear to the scientific

[65] Mark B. Mitin, "Za peredovuiu sovetskuiu nauku" [For a leading Soviet science], *Pod znaeniem Marksxizma* [Under the banner of Marxism] 10 (1939): 149–76, on 150.

[66] "Genetics in the Soviet Union: Three Speeches from the 1939 Conference on Genetics and Selection," *Science and Society: A Marxian Quarterly* 4 (1939): 183–233, on 187–8.

[67] SGS, vol. 1, 221–2.

[68] Joseph Needham, "Genetics in the USSR," *Modern Quarterly* 1 (1938): 370–4. Needham used the pseudonym "Helix and Helianthus."

[69] Nikolai Vavilov, ed., *Teoreticheskie osnovy selektsii rastenii* [Theoretical basis for the selection of plants], 3 vols. (Moscow, 1935–37).

[70] See Darlington Papers, Bodleian Library, University of Oxford, Oxford, C 114.

community in the Soviet Union that no constructive compromise or free scientific competition with Lysenkoist agrobiology was possible. From now on it was war—within the constraints set by the institutional culture of Stalinist science.[71]

THE 1948 GENETICS CONFERENCE

After Nazi Germany attacked the Soviet Union in June 1941, ideological orthodoxy was toned down. Western democracies and the Soviet Union were now comrades in arms. By the end of World War II, there was growing hope for classical genetics. Criticism of Lysenko's theories as well as his leadership in the Lenin Academy was reviving. Anton Zhebrak, as head of a section in the Central Committee secretariat for a period in 1945–46, worked actively to establish new contacts with Western geneticists. But the emerging cold war, with its campaigns against internationalism and subservience to Western capitalism, soon provided Lysenko with a platform for counterattack.[72]

The dramatic events around the August 1948 conference of the Lenin Academy were precipitated by a young official in the Central Committee, Iurii Zhdanov. He was trained as a chemist and belonged to the inner circle of the regime. He was the son of Andrei Zhdanov, the most influential leader on cultural questions after Stalin. Iurii Zhdanov had even discussed principles of science policy personally with Stalin. In April 1948, he spoke to an audience of party officials on "controversial questions of contemporary Darwinism."

Following the official policy line of open and fair scientific debate and competition, young Zhdanov evaluated the two schools of genetics. He repeated the standard criticism of classical genetics: it lacked practical results, was obsessed with fruit flies, believed in unchanging genes, and generally suffered from a divorce of theory from practice. Zhdanov praised Lysenko for his great practical achievements but then expanded on the criticism of Lysenkoism. Lysenko had neglected recent discoveries about polyploidy of chromosomes,[73] rejected the use of hybrid corn,[74] not fulfilled the promise of new useful varieties of cereals in two to three years, and so on. I. Zhdanov also warned against illegitimate ways of suppressing other schools of thought and against the way philosophers had intervened in favor of Lysenko from the 1930s on.[75]

Iurii Zhdanov's lecture indicated the growing influence of genuine genetic science in the central party apparatus. But Lysenko adroitly used the budding criticism to force a showdown while he still had the upper hand. He used his political connections and administrative powers as president of the Lenin Academy to organize a special session of the academy with a majority biased in favor of agrobiology. A few geneticists spoke courageously for their science, but others just sat quietly, expecting the worst. At the end of the session, Lysenko dealt a final blow to his opponents. He announced that his keynote address on "The Situation in Biological Science" had

[71] Vividly described in publications by Krementsov and Kojevnikov (cit. n. 17).

[72] These developments have been described in detail by Krementsov, *Stalinist Science* (cit. n. 17).

[73] Methods for multiplication and manipulations of the number and composition of chromosomes were seen internationally as important techniques for creating genetic variation and progress in breeding.

[74] By 1948, this was a great agricultural success in the United States, often seen as the first big practical result of classical genetics.

[75] Iu. Zhdanov, "Vo mgle protivorechii" [In the darkness of contradiction], *Voprosy filosofii* 7 (1993): 65–92.

already been approved by the Politburo. In fact, it had been carefully edited by Stalin himself.[76] Those who had spoken against him had the choice between public repentance and leaving their jobs. A ban on teaching and research in classical genetics was soon confirmed by the Ministry of Education and the Academy of Sciences.

The August 1948 congress and the official Soviet ban on genuine genetic science was a striking example of self-defeat by wishful thinking—or shooting oneself in the foot. Translations of the verbatim report of the proceedings were distributed internationally. All over the world, scientists could read in detail the pseudoscientific Lysenkoist argument. The result was a major defeat for the Soviet Union on the intellectual front of the cold war.

There was strong opposition to Lysenko in the central party organs in 1948, and without Stalin's personal support he would most likely have been deposed. A number of idiosyncrasies made him sympathetic. Like many left-leaning politicians and intellectuals, Stalin had a soft spot for Lamarckian ideas about the malleability of heredity under environmental influence.[77] The mechanistic and somewhat inhumane deterministic taste of classical genetics did not suit his romantic and holistic tendencies. Stalin was also a passionate hobby gardener who felt he, not unlike Lysenko, had an intimate practical knowledge of plants. Branched wheat was a characteristic Stalin hang-up. Many spikes on each straw suggest a manifold increase in yields. In December 1946, Stalin had presented Lysenko with a sample of branched wheat from his homeland, Georgia. Lysenko dutifully started a project, although branched varieties of wheat had long been known to breeders and repeated attempts to make practical use of this property had failed. But, of course, Stalin's utopian hope was not completely wild. Sometimes new attempts succeed where many have failed before.

THE PREDICAMENT OF INTELLECTUALS

What most shocked the international scientific community in 1948 was the undisguised suppression of intellectual freedom.

From the 1930s, liberal intellectuals had cooperated with Communists in the fight against Fascism. In a critical political situation, this appeared the only viable alternative in spite of the authoritarian and antiliberal tendencies of Communist ideology. With the common victory over Nazi Germany, the Soviet Union emerged in a positive light, and hopes were strong that freedom and social equality could now be realized together in the West and the East.

At the end of World War II, it was a widespread view among Western biologists that both Lysenko and Vavilov were genuine scientists with important contributions. There were rumors but no reliable knowledge about Vavilov's death. A thoroughly respectful overview of the new genetics in the Soviet Union by two scientists at the Commonwealth Bureau of Plant Breeding and Genetics concluded that Lysenko's genetic ideas were highly problematic. But they took for granted that his practical and theoretical achievements in vernalization merited a general scientific standing comparable to that of Vavilov.[78] In a review of their book, the plant physiologist Eric Ashby noted that it

[76] Kirill Rossianov, "Editing Nature: Joseph Stalin and the 'New' Soviet Biology," *Isis* 84 (1993): 728–45.

[77] See, e.g., Stalin's letter to Lysenko, 7 Oct. 1947, published by Iurii Vavilov in "Avgust 1948: Predistoriia" [August 1948: The prehistory], *Chelovek*, 1998, no. 4:104–11, 109–10.

[78] P. S. Hudson and R. H. Richens, *The New Genetics in the Soviet Union* (Cambridge, UK, 1946), 4.

was "almost orientally apologetic" in its efforts to be fair to Lysenko. But Ashby did not remark on its respect for Lysenko's achievements in vernalization.[79] Ashby's own report from his period as a diplomat in Moscow during the war described Lysenko as an honest and sincere scientist, no charlatan or showman, in spite of his unfounded ideas about genetics.[80] At this point, the prospects looked good for genetics in the Soviet Union. Western geneticists believed that it would soon flourish[81] and organized a campaign to support the efforts of their Soviet colleagues to get rid of the Lysenkoist yoke. They were careful, however, to keep to purely scientific criticism and avoid political issues that could make the situation difficult for their Soviet colleagues.[82]

Other intellectuals were more explicit about the cultural threat from the Soviet Union. Karl Popper's *The Open Society and Its Enemies* targeted the sociology of knowledge inspired by Marxism as a main source of the revolt against reason that was threatening liberal democracy after the Second World War.[83] For Michael Polanyi and other leaders of the Society for Freedom in Science, the standard example of what Bernalism might lead to was the suppression of genetics in the Soviet Union.[84] When the society was established in 1940, its ideas met with little approval, but by the end of the war this had changed. There was broad acclaim of its ideals in the journal *Nature* by once skeptical scientists, and even the left-leaning British Association of Scientific Workers found the ideas germane; the society's membership soared.[85] The French watchdog of intellectual freedom Julien Benda, who had joined the Popular Front in the 1930s with a commitment that brought him close to membership in the Communist Party, also expressed concern about the threat of the Soviet system to intellectual freedom.[86] In a new preface for the 1947 edition of *La trahison des clercs* (The treason of the intellectuals), he stressed traditional scientific ideals of adherence to truth and neutrality in political matters as a counterforce to totalitarianism whether from right or left, which was not unlike Robert Merton's ethos of science with universal validity and disinterestedness as central ideals.[87]

In 1948 there was still widespread sympathy in the West both for the ideal of a science that would truly serve the people and for some of Lysenko's general biological ideas. The ban on genuine genetic science, however, undermined remaining sympathy. It dawned on many left-wing intellectuals that Communism and freedom were more at odds than they had hoped. The valiant defense of Lysenko's Lamarckism and

[79] Eric Ashby, "Genetics in the U.S.S.R.," *Nature* 158 (31 Aug. 1946): 286–7.

[80] Ashby, *Scientist in Russia* (cit. n. 37), 116.

[81] See, e.g., L. C. Dunn, "Science in the U.S.S.R.: Soviet Biology," *Science* 99 (28 Jan. 1944): 65–7; Julian Huxley, "Science in the U.S.S.R.: Evolutionary Biology and Related Subjects," *Nature* 156 (1 Sept. 1945): 254–6.

[82] Nikolai Krementsov, "A 'Second Front' in Soviet Genetics: The International Dimension of the Lysenko Controversy, 1944–1947," *Journal of the History of Biology* 29 (1996): 229–50.

[83] Karl Popper, *The Open Society and Its Enemies* (Princeton, N.J., 1950); see also Popper, *The Poverty of Historicism* (London, 1957).

[84] Michael Polanyi, "The Autonomy of Science," *Memoirs and Proceedings of the Manchester Literary and Philosophical Society* 85 (1943): 19–38.

[85] McGucken, "On Freedom and Planning in Science" (cit. n. 5).

[86] David Schalk, *The Spectrum of Engagement: Mounier, Benda, Nizan, Brasillach, Sartre* (Princeton, N.J., 1979), 43.

[87] Julien Benda, *La trahison des clercs* (Paris, 1947), 83–92. The book was first published in Paris in 1927; the first English translation was published in New York in 1928. R. K. Merton, "The Normative Structure of Science," in *The Sociology of Science* (Chicago, 1970), 267–78. Originally published as "Science and Technology in a Democratic Order," *Journal of Legal and Political Sociology* 1 (1942): 115–26.

science for the people by a cultural icon such as George Bernard Shaw simply highlighted its futility.[88]

In Britain, the brilliant population geneticist and scientific popularizer J. B. S. Haldane had argued insistently that some claims of Lysenko's might well be right and that his agrobiology deserved support as a competing research program. But after it was proclaimed as the truth in 1948, Haldane quietly faded out of the Communist movement.[89] Nevertheless Haldane retained a predilection for unorthodox genetic ideas,[90] and in his auto-obituary he coyly suggested that if he had had Lysenko's power over science, he would probably have committed similar mistakes.[91] The physicist Bernal, however, continued his defense of Lysenko well beyond 1948. In the first edition of *Science in History* (1954), Bernal still presented agrobiology as a valid exemplar of socialist science. But in the second edition of 1957, he admitted that political loyalty to the Soviet Union had led him "to pass over the inadmissible way in which the controversy was conducted in the Soviet Union."[92] Yet the ideals of science that fueled Lysenkoism retained their attractiveness into the 1970s. How to separate the scientific mistakes of Lysenkoism from its valid sociopolitical and epistemic principles was an important topic for the radical (left-wing) science movement.[93]

The influence of Lysenkoism was stronger and more lasting in France than in the Anglo-American cultural sphere, reflecting the cultural basis of a strong Communist Party. In French agricultural botany, there was a lasting tradition, organized as the Friends of Michurin, pursuing ecological studies in the spirit of Lysenko and Vavilov.[94] Writings of Nobel Prize geneticists Jacques Monod and André Lwoff witness how important this struggle was in the French scientific community. Dialectical materialism was the main target of Monod's slim 1970 volume of essays in the philosophy of biology, *Chance and Necessity*. Lysenko was quite right and Soviet geneticists mistaken, declared Monod; the theory of a gene that is stable through generations is completely irreconcilable with the principles of dialectics.[95] Monod's own epistemic alternative was "the ethics of knowledge," in harmony with the existentialist mood of his youth, respecting objectivity and avoiding the traps of vitalism and animism. This ethics expresses a code of behavior for scientific intellectuals similar to the ideals of Benda, Merton, and Popper. For Monod, the ethics of knowledge was "the only attitude at the same time rational and deliberately idealist on which a true socialism can perhaps be built."[96]

Lwoff in the preface of a new 1975 edition of *La trahison des clercs* vigorously defended the nonconformist Benda against common misinterpretations. For instance, Lwoff said, the Great Larousse encyclopedia described Benda's book as "a pamphlet against the intellectuals." To the contrary, Lwoff stated, the book exhorted truth, justice, and reason as the three universal values that intellectuals should pursue and

[88] George Bernard Shaw, "The Lysenko Muddle," *Labour Monthly*, Jan. 1949, 18–20.

[89] A sympathetic and informative account and analysis is given by Diane Paul, "A War on Two Fronts: J. B. S. Haldane and the Response to Lysenkoism in Britain," *J. Hist. Biol.* 16 (1983): 1–37.

[90] J. B. S. Haldane, *Biochemistry of Genetics* (London, 1954).

[91] Soyfer, *Lysenko and the Tragedy* (cit. n. 16), 282.

[92] J. D. Bernal, *Science in History*, 2nd ed. (London, 1957), 827.

[93] Lewontin and Levins, "The Problem of Lysenkoism"; Lecourt, *Proletarian Science?* (Both cit. n. 14.)

[94] A central person was Claude-Charles Mathon, working for many years at the University of Poitiers.

[95] Jacques Monod, *Le hasard et la nécessité* (Paris, 1970), 52.

[96] Ibid., 193.

chastised those who did not. Like much other contemporary philosophy Marxism, said Lwoff, drawing on Benda, was a philosophy of action that "does not admit of stable truths but only of truths determined by the moment." Lwoff parodied the "logic of contradiction" inherent in dialectical materialism as the right to contradict truth. His testimony was the suppression of genetics in the Soviet Union, starting with Lysenko's announcement of a new genetics in 1935. In 1975, there still had been no sanctions against the scientists and politicians responsible. Instead a scientist who published a book exposing the affaire ended up in a psychiatric hospital, wrote Lwoff.[97]

Monod and Lwoff were defending scientific autonomy in a new period of political radicalization. They felt that in the 1970s Marxist ideas about the social nature of science had once more become popular and threatening in universities and cultural life.

THE PERSISTENCE OF LYSENKOISM IN THE SOVIET UNION

After the climax of 1948, Lysenko enjoyed a few years of supreme power in Russian biology, experiencing a cult of personality only a step below that of Stalin himself. But accumulating failures of his practical advice gradually undermined Lysenko's authority. One spectacular failure was cluster planting of trees as part of the grand Stalin plan for the transformation of nature.[98] Assuming altruism and mutual sacrifice rather than competition between individuals of the same species, Lysenko advised the planting of clusters, rather than isolated individuals, to increase survival under harsh conditions. After a few years, there could be no denying that cluster planting in the steppe had been a great waste of resources. The final showdown between Lysenko and his academic critics in 1965 also focused on questions of practical economic results. A commission of the Academy of Sciences found that claims by Lysenko about the milk yields of his cows at the Lenin Hills Farm were at least unfounded, if not based on directly falsified data.[99]

Although practical failure was the most effective political argument, inconsistency with established biological theory was the basis of the scientists' campaign. Even Stalin was aware that science must observe standards of objectivity valid across cultural and political divides. In editing Lysenko's 1948 speech, Stalin had deleted references to "bourgeois biology" and claims that "all science is class science."[100] In a famous 1950 pamphlet on *Marxism and the Problems of Linguistics*,[101] Stalin rejected the monopolistic ambitions of certain schools, insisting that Soviet science must be open to criticism and observe international standards of objectivity.

Opposition to Lysenko grew after Stalin's death in 1953. He had to resign from the presidency of the Lenin Academy in 1956. But with support of party general secretary Nikita Khrushchev—a peasant son with a background from Ukrainian agricultural politics—Lysenko fought back. In 1958, a new editorial board took charge of

[97] André Lwoff, introduction to Julien Benda, *La trahison des clercs* (Paris, 1975), 9–40, 17–9. He probably had Zhores Medvedev and his book *The Rise and Fall of T. D. Lysenko* (cit. n. 11) in mind.

[98] Described in the government resolution as a "Plan for Erosion-Control, Forest Planting, Introduction of Grassland Crop Rotation, and Building of Ponds and Reservoirs to Guarantee Large and Dependable Harvests in the Steppe and Forest-Steppe Regions of the European USSR." Quoted from Soyfer, *Lysenko and the Tragedy* (cit. n. 16), 205.

[99] Ibid., 283–8.

[100] Rossianov, "Editing Nature" (cit. n. 76), 732.

[101] J. V. Stalin, *Markzism i voprosy iazykoznaniia* [Marxism and the problems of linguistics] (Moscow, 1950).

the oppositional *Botanical Journal*, and in 1961, Lysenko returned to his position as president of the Lenin Academy. But by now the Academy of Sciences, led by physical scientists, was heavily behind the drive to oust Lysenko.

In 1963, a new crisis emerged as the magazine *Neva* described the fateful 1948 session as a result of authoritarian Stalinism. This was sharply denounced by high officials, and the editors publicly regretted their "gross error." Soon thereafter, the election of new members to the Academy of Sciences precipitated a confrontation between it and Khrushchev. His candidates were voted down, and Khrushchev was furious. He considered a radical reorganization of the academy but did not have time to retaliate as his career stranded on broad failures in agricultural policy. For the first time in its history, the Soviet Union had had to import grain. In October 1964, Khrushchev was removed from his commanding position and replaced by Leonid Brezhnev, who was an engineer by training, and Lysenko quickly lost his important administrative posts.[102]

By 1965, the direct political suppression of genetics in the Soviet Union was finally at an end. But the new liberty was not matched by openness about the causes of repression. The general ideas and attitudes governing science policy persisted with little change. In 1967, Mark Popovskii's articles about the fate of Nikolai Vavilov were sharply attacked. Such emotionally inflammatory writings would spread uncertainty and stimulate revengefulness among the public and could not be permitted. Party ideologues worried that persons responsible for the mass media had become "less demanding of skilled treatment of modern problems of science and practice."[103]

A typical example of this continuity is the career of agricultural economist and bureaucrat Pavel Lobanov. He chaired the 1948 session and became president of the Lenin Academy when Lysenko had to step down in the period 1956 to 1961. With Lysenko's final retreat in 1965 Lobanov once more took over as president of the Lenin Academy of Agricultural Science—until 1978.

CONCLUSION

My account is focused on the politics of science—the arena in which science and politics overlap and interact—because I believe that provides the best explanatory perspective on the development of Lysenkoism, its rise and fall as well as its protracted aftermath. And I have pointed to the Lenin Academy of Agricultural Science as the key institution at which the pragmatic principles of Soviet science policy, expressed in the practice criterion of truth and the unity of theory and practice, were played out with more consequence than in other Soviet institutions of science policy.

The rise of Lysenkoism from 1929 to 1939 is described in detail, and the rest of the story, up to the 1980s, is only sketched. There are two main reasons for this. First, the early period has so far received less historiographical attention. The scientific content and contemporary standing of Lysenko's early work in plant physiology, in particular, has mostly been superficially treated. Second, this is the period when an ideologically distorted view of the relation between science and politics precipitated an irrational institutional and cultural system of science politics.

This perspective throws new light on the role of Nikolai Vavilov. He was not only

[102] For a detailed account, see Soyfer, *Lysenko and the Tragedy* (cit. n. 16), 251–83.
[103] Ibid., 291.

the martyr of genetics but also a main entrepreneur of the research system in which Lysenko made his career. He supported Lysenko's career in its early stages and did not publicly turn against him until the late 1930s. As Polanyi pointed out, it was with some reason that Lysenko turned Vavilov's own pronouncements from 1932 against him at the October 1939 conference: "The divorce of genetics from practical selection" that characterizes the West "must be resolutely removed from genetics-selection work in the U.S.S.R."[104] In the debates over the "two directions in genetics" in 1935–36, it was others who defended the value of genetics as a theo-retical science. To argue that disregard for the different character of theoretical and applied science was a major source of the political appeal of Lysenkoist misconceptions[105] was not popular at that stage. When Kol'tsov stood up for scientific and intellectual freedom in 1936–37, Vavilov gave him no public support. Such outspokenness was, of course, dangerous under the Great Terror. But Vavilov proved his courage later. So perhaps he was too involved with current government science policy to clearly see the threats that Kol'tsov pointed to.

The vagueness and ambiguity of the classical ideal for the political role of the intellectual, proclaimed by Benda and others, has worried the British social anthropologist Ernest Gellner. He has pointed to paradoxical and treacherous consequences of a naturalistic and pragmatic interpretation of the ideal. Jean-Paul Sartre's softness on Stalinism was an example of how easily it happens that "he who preaches against the treason of the clerics commits it in the very same sermon." The task of "*not* committing the treason is far, far more difficult" than most intellectuals have assumed, claimed Gellner.[106] It is not surprising that Nikolai Vavilov, as a scientific technocrat, had difficulty in breaking free from the role of a loyal fellow traveler.[107]

Why did biology and not, for instance, physics, chemistry, or geology, succumb to a primitive and retarded pseudoscience? The answers lie in the important differences that existed in the status of current knowledge and the institutional situation of biology versus those that existed for other sciences. The biological knowledge at stake—in plant physiology as well as genetics—was new, still to a large extent uncertain, and disputed. The Lenin Academy of Agricultural Science was set up in 1929 in the spirit of a socialist revolution in science. This was a spirit with international appeal, as can be seen from the enthusiastic reaction of the British scientific Left to Bukharin at the London 1931 International Congress of the History of Science.[108]

Considering its incompatibility with international biology and its accumulating practical failures, Lysenko's agrobiology could not last. But why did it take so long to disprove it and why was the Soviet government so slow to give it up? The continuity of science policy doctrines, institutions, and personnel prolonged the life of Lysenkoism almost to the end of the Soviet Union. There was no clear abandonment of the practice criterion of truth or the unity of theory and practice, only a slow and partial recognition of their inadequacy.

[104] Polanyi, "The Autonomy of Science" (cit. n. 84), 34.

[105] M. M. Zavadovskii, "Protiv zagibov v napadakh na gentiky" [Against deviations in the attacks on genetics], *SRSKh*, 1936, no. 8:84–96, on 95–6.

[106] Ernest Gellner, "La trahison de la trahison des clercs," in *The Political Responsibility of Intellectuals*, ed. Ian Maclean, Alan Montefiore, and Peter Winch (Cambridge, UK, 1990), 17–27.

[107] For the history of the term, see David Caute, *The Fellow-Travellers: A Postscript to the Enlightenment* (London, 1973), 1–7.

[108] See, e.g., G. Werskey, "Introduction: On the Reception of *Science at the Cross Roads* in England," in Bucharin et al., *Science at the Cross Roads* (cit. n. 6), xi–xxix.