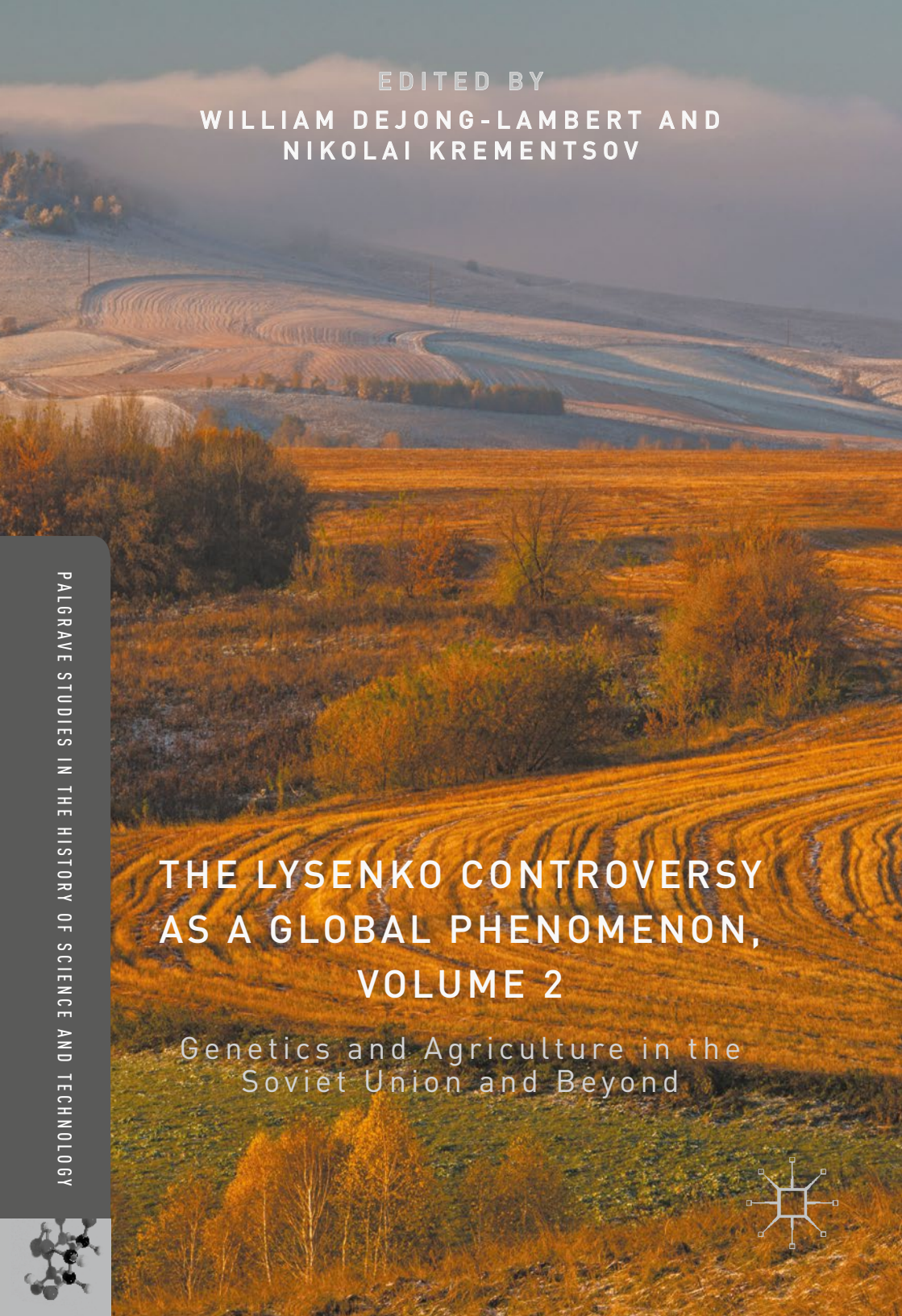
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PALGRAVE STUDIES IN THE HISTORY OF SCIENCE AND TECHNOLOGY
THE LYSENKO CONTROVERSY AS A GLOBAL PHENOMENON, VOLUME 2
Genetics and Agriculture in the Soviet Union and Beyond

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The Lysenko Controversy as a Global Phenomenon, Volume 2

Genetics and Agriculture in the Soviet Union and Beyond

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Editors
William deJong-Lambert
Bronx Community College
City University of New York
Bronx, New York, USA

Nikolai Krementsov
University of Toronto
Toronto, Ontario, Canada

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Preface, Vol. 2

In the second volume of *Lysenkoism as a Global Phenomenon* our authors focus on Lysenko's scientific and cultural impact outside of the Soviet Union. We begin in Hungary with "Opportunism and enforcement: Hungarian reception of Michurin biology in the Cold War period." The authors chronicle the rise and fall of T.D. Lysenko in Hungary according to how his theories were received prior to the Cold War, the reaction and counter-reaction of Hungarian biologists from 1948 to 1956, and how Lysenko ultimately undermined his own credibility among Hungarian biologists.

Next we examine what happens in Italy during these same years with "Lysenko in Bellagio: The 'Lysenko Controversy' and the struggle for authority of Italian geneticists, 1948–1956." Here the author shows how Italian geneticists instrumentalized the Lysenko controversy to consolidate control of biological research in Italy, following the Ninth International Genetics Congress in Bellagio, Italy, in 1953. We return to the Communist Bloc in "The National Pattern of Lysenkoism in Romania," where we learn of how the national affinity for French science and culture influenced Romanian geneticists to interpret Lysenko's theories as neo-Lamarckist, extending their currency far longer than was the case in other Soviet-allied states.

Our next chapter, "H.J. Muller and J.B.S. Haldane: Eugenics and Lysenkoism," examines the complicated relationship between eugenics and Lysenkoism in the USA and Great Britain, in terms of the relationship between two of the most important figures in history of the evolutionary synthesis of genetics and Darwinism. Muller was an advocate of eugenics and Haldane was a skeptic, and Muller's attempt to convince Stalin of his

eugenics plan helped smooth the way for Lysenko's ascent. Muller later emerged as one of Lysenko's most prominent critics, while Haldane was portrayed as his most important defender. The author analyzes the relationship between Muller and Haldane vis-à-vis eugenics and Lysenko to illustrate the challenges of geneticists negotiating the bio-political complexities from the interwar period into the Cold War.

This same theme appears in "Why did Japanese geneticists discuss Lysenko's biology scientifically?" where the author considers their thwarted interest in studying Lysenko's theories after World War II. Though Japanese biologists had many reasons to be interested in following up on Lysenko's claims, the US mandate in their defeated nation made such research impossible. Along similar lines, "Dialectics Denied: Muller, Lysenko, and the Fate of Chromosome Studies in Soviet Genetics," chronicles the study of chromosomal mutations as a heretofore unrecognized casualty of Lysenkoism. Though this was an area of research that thrived in both the USA and the USSR prior to the Cold War, the geopolitics of Lysenkoism abrogated progress, as biologists on both sides retreated to their respective foci on phenotype and genotype.

Our final two chapters, "Lessons from Lysenko" and "Current Attempts to Exonerate 'Lysenkoism' and Their Causes," look to the current status of Lysenko's legacy and ideas. The former argues that the recent rehabilitation of Lamarckism and continued desire to engineer life and steer the course of evolution reflect the aspirations launched by Lysenko decades ago. The opposite point of view appears in the latter chapter, where the author analyzes the current revival of Lysenko's reputation as a pernicious trend rooted in contemporary attitudes towards science and religion in Russia, a quarter century after the end of the Cold War.

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List of Contributors

Luis Campos is Associate Professor of History of Science and Senior Fellow at the Robert Wood Johnson Foundation Center for Health Policy at the University of New Mexico. His scholarship integrates archival discoveries with contemporary fieldwork at the intersection of genetics and society. He is the author of Radium and the Secret of Life (2015), a prehistory of radiation genetics. He is at work on his next book on the history of synthetic biology.

Francesco Cassata is Assistant Professor of Contemporary History at the University of Genoa. He has published several books on the history of eugenics and scientific racism in Italy, on the history of Lysenkoism in Italy, on the Italian geneticist Adriano Buzzati-Traverso and the International Laboratory of Genetics and Biophysics in Naples (1962–1969). He is working on several research projects: the history of EMBO-EMBC-EMBL; the anthropological studies of Dante's bones from 1865 to 1938; the radiogenetic and mutagenesis programme of durum wheat at the Nuclear Energy Research Centre Casaccia (Rome), from the 1950s to the 1980s.

William deJong-Lambert is an Associate Professor of History at the Bronx Community College, City University of New York, and associate faculty of the Center for Science and Society at Columbia University. His publications include, *The Cold War Politics of Genetic Research: An Introduction to the Lysenko Affair* (Springer 2012), a co-edited special issue on Lysenkoism of the *Journal of the History of Biology* (45/3, 2012), and several articles in *Journal of Cold War Studies, Historical Studies in the Natural Sciences, Studies in the History of Biology, East European Politics and Societies* and others, as well as chapters in edited collections. His research has been funded by the National Science Foundation, Fulbright, the American Council for Learned Societies, the American Philosophical Society and other organizations.

Eduard I. Kolchinsky is the director of the S.I. Vavilov Institute for the History of Science and Technology, St. Petersburg, Russia. He has published and edited numerous books on the history of Russian biology, philosophy, and evolutionary thought.

John Marks is Associate Professor in French Studies in the Department of French and Francophone Studies at the University of Nottingham, England. His main research area is contemporary thought and social theory. He is working on a study of discourses of life in post-war French thought on biology.

Miklós Müller Upon his retirement in 2005 as the head of the Laboratory of Biochemical Parasitology at the Rockefeller University, Prof. Müller turned his interests to the history of twentieth-century biology. His studies concern two topics: the life and works of Hungarian theoretical biologist Ervin Bauer (1890–1938) who successively worked in Hungary, Czechoslovakia, Germany and the Soviet Union, and the impact of Soviet distortions of biology (Lysenko and Lepeshinskaya) on biology in Eastern Europe in the 1940s and 1950s.

Cristiana Oghina-Pavie is an Assistant Professor in the Department of History of the University of Angers, France, and member of *Centre de Recherches Historiques de l'Ouest* CERHIO UMR CNRS 6258. Her research interests concern two main themes: history of international relations and life sciences. She studied French-Romanian acculturation of models of nation, state and politics. She works on relationships between horticulture and plant sciences in the nineteenth and twentieth centuries with focus on physiology and heredity. With epistemologists and biologists, she develops interdisciplinary approaches to the effects of breeding on the genetic diversity of cultivated roses.

Gábor Palló is a senior consultant at the Visual Learning Laboratory, Budapest University of Technology and Economics. His fields of research include history of chemistry and physics, twentieth-century history of natural sciences in Hungary, philosophy of science, history of migration of scientists, the relationship between science, politics, and philosophy. Some of his recent publications are *Zsenialitás és korszellem* (Genius and Zeitgeist), (Budapest: Áron Kiadó, 2004); "The Advantage and Disadvantage of Peripheral Ignorance: The Gas Adsorption Controversy," *Ambix* 57, no. 2 (2010), pp. 216–230; "Early impact of quantum physics on chemistry: George Hevesy's work on rare earth elements and Michael Polanyi's absorption theory," *Foundations of Chemistry* 13, no. 1 (2011), pp. 51–61.

Hirofumi Saito holds a PhD from the Tokyo Institute of Technology (Tokyo Tech) since 2008. From 2009 he is working as a research associate at the Tokyo Tech, focusing on the history of Soviet genetics. Since 2010, he has investigated documents of the Archive of the Russian Academy of Sciences. Recently he published two works, which reconsider the process of Lysenko's intervention into the Institute of Genetics in the late 1930s and Lysenko's personal state in his agrobiology school during World War II.

List of illustrations

PART I

The Lysenko Controversy: East and West

Opportunism and Enforcement: Hungarian Reception of Michurinist Biology in the Cold War Period

Gábor Palló and Miklós Müller

Introduction

In this chapter we outline the impact of Lysenkoism upon Hungarian biology according to three topics. First we show that though Lysenko's anti-genetics campaign had a negative impact upon the development of Hungarian genetics in the short term, it stimulated the institutionalization of biology, for the first time, as a scientific discipline. Next we outline five strategies of response—passive resistance, passive acceptance, passive opportunism, active opportunism, and sincere belief. As we show, the fourth option—best exemplified by Imre Törő, head of the Department of Histology and Embryology of the Medical University of Budapest—resulted in the most positive outcome in terms of the long-term development of Hungarian biology. Finally, we show that it was Lysenko

Authors deeply appreciate the help of Magdolna Baráth, Diana Háy and all coworkers of the Archives of the Hungarian Academy of Sciences, Éva Müller, Julia Thurau and Vadim Volubuev in library and archival searches.

G. Palló (✉)
Budapest University of Technology and Economics, Budapest, Hungary

M. Müller
Rockefeller University, New York, NY, USA

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himself—thanks to a January 23, 1960 performance at the Hungarian Academy of Sciences—who most contributed to the demise of Lysenkoism in Hungary. The Hungarian case thus complicates the "heroes v. villains" narrative typical of many earlier analyses of the Lysenko controversy: to show the salutary effects of Lysenkoism, that the "bad guys" sometimes did the most good, and that in some cases it was Lysenko himself, rather than his opponents, perceived and otherwise, who most contributed to his downfall.

We divide the timeline of the Hungarian response to Lysenkoism into three stages. During the first stage, occurring from the 1930s up until August 1948, Michurinism was investigated by Hungarian researchers as an interesting new development in plant breeding. The second phase occurred once Lysenko consolidated power over Soviet biology. In this phase Lysenko's theories were mandated as part of Hungary's transition into a Soviet ally, and biologists responded according to the five strategies outlined above. The third stage—counter-reception—began in 1953. This process did ultimately lead to the demise of Lysenkoism in Hungary, the process hastened along thanks to the appearance of its namesake.

Lysenkoism in Hungary

Michurinist biology first appeared in the Hungarian daily press on August 11, 1948, when the Communist Party newspaper, *Szabad Nép* (Free People) published an article entitled "The inheritance of acquired characteristics."[1] The unsigned article reported a major breakthrough in Soviet science, achieved by academician Trofim Denisovich Lysenko. The article was a quick response to Lysenko's report delivered on July 31 at a session of the All-Union Lenin Academy of Agricultural Sciences (VASKhNIL) under the title "On the Situation in the Science of Biology."[2]

The full text of Lysenko's report soon appeared in *Társadalmi Szemle* (Social Review), the theoretical journal of the Hungarian Communist Party, which discussed political and ideological topics in the framework of Marxist-Leninist-Stalinist theory. The translation of Lysenko's text was introduced by the editors, stressing that his report showed the victory of progressive, revolutionary science supported by the Communist Party over apolitical bourgeois science, the victory of dialectic materialism over idealism.[3] This made Michurinist biology a state-supported direction in science.

Support for certain biological theories by the political regime was not entirely unprecedented in Hungary. In previous decades, discrimination based on biological race, eugenics, and related topics of human genetics were enlisted as features of state ideology by the Hungarian government.[4] What was unique in this case is that the new political regime enforced the acceptance of a biological world view, which stood in direct opposition to the Nazi-inspired biological doctrine supported by the interwar government. This shift marked the Hungarian state's transition from being one of the numerous Nazi-oriented dictatorships in Eastern and Central Europe to a member of the Soviet-dominated Communist Bloc.

The Spontaneous Period

Michurinist biology attracted attention in Hungary long before the events of August 1948. Lysenko's ideas of vernalization were discussed in Hungary as early as 1933, when Rudolf Fleischmann, an experienced plant breeder, wrote a short article about it in an agricultural magazine.[5] Fleischmann is considered one of the founders of modern breeding, primarily of corn and wheat, in Hungary. He studied and worked in various countries, but his most important results were achieved in the seed breeding and trading station in Kompolt in northeastern Hungary, where he started to work in 1918. Fleischmann read the bulletin published by the Imperial Agricultural Bureau in Britain on Lysenko's vernalization.[6] He referred to this British publication and not to Lysenko's original papers, about which he probably did not know. His conclusion was that "If we find in it something that helps our domestic agriculture, the introduction of the method will follow by itself…We should add that similar experiments were performed in Hungary by Legány some years ago." Fleischmann was not unique in his interest. The next year, 1934, Ottó Bocskay published a longer report on Lysenko's theory of phasic development, the differentiation between growth and development and some results of vernalization and vegetative hybridization.[7]

Another article on Michurinist methods appeared in the Hungarian press in 1942, when Hungary fought on the side of Nazi Germany against the Soviet Union. It was written by Miklós Horn, also a highly regarded breeder, who produced new varieties of rye, wheat, oat, sunflower, corn, and potato. Unlike Fleischmann, Horn cited Russian sources that he probably read in the original. The enthusiastic title of his 1942 article, "Revolutionary Russian ideas in plant breeding," sounds as if it had been

published six years later, but his text reveals his strong reservations on vernalization and vegetative hybrids.[8] Though the journal that published these articles, *Köztelek* (Tilled Lands), was widely read, it is difficult to ascertain whether these early articles had any practical impact upon Hungarian agricultural research.

Lysenko's work was also described in a widely known textbook on plant breeding written in 1944 by Ödön Villax, a leader of a most important breeding center in Magyaróvár in northwestern Hungary.[9] While Villax included Michurin and Lysenko in this seminal text, he had his doubts and asked his young coworker, Árpád Kiss, to verify some experiments on vegetative hybridization and use of Michurinist mentors. The negative results obtained by Kiss were the first experimental refutation of Michurinist biology in Hungary.[10] Villax left Hungary for the West in 1948.

Evidence of further interest in Lysenko's work comes from Barna Györffy, a leading and pioneering geneticist in Hungary and head of the Research Institute of Genetics. In a report published in 1957, over a decade after the fact, Györffy mentioned that he had started experimental research on Lysenko's vernalization, and attempted to influence inheritance through metabolism in plants in 1945.[11] By the first half of 1948, the agricultural section of the Hungarian Society for Cultural Relations with the Soviet Union arranged a series of lectures on Soviet agronomy, including a lecture given by Ödön Villax on "Michurin and Lysenko."[12] These early examples show clearly that the new ideas of Michurinist biology were noticed by certain experts who did not require any coercion or government interference to take an interest in them. Scientists spotted new ideas in scientific literature, and they attempted to learn and employ them. Michurinist biology in this historical context—prior to the landmark VASKhNIL session of 1948—did not seem anything more than some new ideas related to practical agriculture.

1948

This pattern of reception[13] changed dramatically after August 1948. What had previously been a particular doctrine in agricultural science, of interest only to specialists, became a major political and ideological issue. Michurinist biology was now one of the vehicles of political changes in Hungary. The head of the Communist Party in Hungary, Mátyás Rákosi, dubbed 1948 the "year of turnaround." Historians consider this year as the beginning of real Sovietization, meaning the introduction of the Soviet model in all areas of public life:[14] the features of multiparty

democracy being replaced by one-party rule, and changes being made encompassing all aspects of politics—from central planning of the national economy, to nationalization of private property (including bank accounts and housing), and to central direction of cultural and scientific life. The most powerful leaders of this process—Mátyás Rákosi, Ernő Gerő, Mihály Farkas, and József Révai—spent a long period of time in the Soviet Union as emigrants: they and their close collaborators were called Muscovites, who were perceived to be trusted by the Soviets, including Stalin himself. Communists, who had worked in Hungary before and during the War as members of the prohibited Communist Party, seemed less trustworthy to the Soviet leaders.

The reorganization of science, as part of the governmental structure, began that same year. A new organization, the Hungarian Scientific Council, was set up on September 8 to build up a Soviet-type science system. This system was based on state-financed central planning, division of research and teaching, establishment of new research institutes, political control, screening of personnel, and dialectic materialist philosophy.[15] The Hungarian Scientific Council was led by a Party Collegium consisting of five members headed by Ernő Gerő, the second most important party politician next to Rákosi.[16] The Council had a Natural Science section. Its secretary, Sándor Rajki, was a 25-year-old scientist, who was and remained one of the most faithful advocates of Michurinist biology. The next year however, on Soviet initiative, the Scientific Council was disbanded and its function of directing academic science was transferred to the Hungarian Academy of Sciences.[17] Applied sciences, including agricultural research, were under the supervision of various ministries.

Acceptance of Michurinist Biology

After 1948, news of Michurinist biology spread fast in Hungary. The idea of Soviet science emphasized the move of scientists and science out of their ivory tower. A campaign was started to inform the Hungarian public of the great victories of Soviet science and its superiority to its bourgeois Western counterpart. Introduction of methods of Michurinist biology in agriculture was expected to improve harvests and enable the introduction of new crops, such as cotton, *kok-saghyz* or oranges in Hungary, which were regarded impossible before.[18]

The newspaper of the Party regularly reported on Michurinist biology, Soviet science and scientists, and the scientific relationships between Hungary and the Soviet Union in the context of political news, rather

than special sections covering science. The articles carried headlines such as "Sabanov: 'Hungarian science follows the same path as the Soviet science now'," [19] "Transformation of nature. Professor Glushenko's lecture on Michurin and the new roads of Soviet biology," [20] and "The situation of science radically changed." [21] Such articles also appeared in other newspapers and popular journals with great regularity. Films shown in theaters propagandized the enormous achievements of the Soviet economy, including agriculture. They showed happy workers on the fields lifting huge watermelons, working in cotton fields or holding meetings in rooms where Stalin's portrait was prominently displayed.

The impact was most evident in the realm of popular science. The highly respected *Természettudományi Társulat* (Society of Natural Sciences, established in 1841), which had consistently played a great role in popularizing science, gradually changed its work after 1945, in compliance with political requirements. In November 1948, just after the announcement of Lysenko's victory by the daily press, the Society published the translation of Lysenko's closing remarks at the VASKhNIL session in its popular monthly journal, *Természettudományi Közlöny* (Natural Sciences). [22] What had been an independent society was now transformed into a state-directed propaganda organ. By 1953 it would be drastically reorganized into a centrally controlled new society, *Természet- és Társadalomtudományi Ismeretterjesztő Társulat* (Society for the Dissemination of Scientific and Social Knowledge). [23] A report on popularization activity in this period states that the goal of the Society and its publications was to explain the decisions of the party and the government, to fight against bourgeois ideology, "to help the formation of dialectic materialist world view, and to provide agro-technical education." [24] The latter included Michurinist biology and it is notable that no other scientific area was expressly specified in the document.

The Society's main activity was to organize lectures—given by trained speakers following prescribed syllabuses and scripts, including still films for projection—all throughout the country. These lectures dealt not only with subjects such as strength of the Hungarian army, advantages of agricultural cooperation, Stalin's support for the peasantry, agricultural machines in the Soviet Union, Hungarian-Soviet friendship, and the international situation in Korea and China, but also with relatively less ideological subjects such as climate, sleep, contraception, or superstitions. Michurinist biology was presented in the context of international politics, the Marxist-Leninist-Stalinist world view, the transformation of nature and society, and practical issues of everyday life. The long existing popular science jour-

nal of the old society was discontinued by the end of 1948, and a new monthly, *Természet és Technika* (Nature and Technics), was initiated that reflected an editorial policy corresponding to the new regime. The Society also started a new weekly, *Élet és Tudomány* (Life and Science), which remains a most successful popular science journal.

A significant difficulty in the dissemination of Michurinist biology was the lack of experts in this area. In the beginning, relatively few articles that appeared in this literature were written by Hungarian authors. Many were translations of articles by Soviet authors. VOKS (All-Union Society for Cultural Relationships) dispatched a great number of Michurinist books to Hungary.[25]

A new professional journal on agronomy, *Agrártudomány* (Agrarian Sciences), was established by the Ministry of Agriculture in 1949. The first issue was introduced by a detailed summary of Michurinist biology.[26] Lysenko's *Agrobiology* was published chapter by chapter in this journal during 1949. The editors felt it necessary to add a special article to explain the terms used in Michurinist biology.[27] The whole book appeared in 1950.[28] By 1952 all major Russian works of Michurinist biology had been translated. Turbin's Michurinist *Genetics* became the approved text for agricultural schools.[29] A complete textbook of biology from Michurinist point of view was soon made available.[30]

As for education, by the early 1950s, Michurinist biology became part of the curriculum in elementary and middle schools. Textbooks appeared containing pictures showing Michurin as a kind, wise old man helping his people. Even elementary school textbooks explained the importance of plant breeding as giving tremendously increased yields and many new species.

By early 1949 emissaries of Lysenko began arriving in Hungary to give first-hand information on the new biology. The first agrobiologist to visit Hungary was Ivan Evdokimovich Glushchenko, a close coworker of Lysenko, who was a delegate to a meeting of the Society of Soviet-Hungarian Friendship. Glushchenko was the main traveling salesman of Michurin biology in many countries and at international congresses.[31] In Hungary, he visited many institutions and universities and gave numerous lectures on such topics as: "Michurin, the great transformer of Nature," "Struggle between idealism and materialism in biology," "Academician Lysenko as scientist and researcher." On his departure Glushchenko reported to the Hungarian authorities with some chagrin that he did not encounter anyone working along the lines of Michurinist biology during his visit.[32] Hungarians learned of his opinion from Béla Faludi's article

on "Tasks of Hungarian biology" that concluded: "The difficulties are due—as also stressed by professor Glushchenko—that our scientists did not delve deeply enough into materialist philosophy and did not master the use of the dialectical method."[33] Glushchenko suggested that in addition to translating more Soviet works on Michurinist biology it would be helpful to establish experimental laboratories in special farms (*Mintagazdaságok*). His report was evaluated by Ernő Gerő, the second in command of the party, who brought it to the attention of the other leaders of the party, Mátyás Rákosi and József Révai.[34]

Other visiting scientists, as the botanist Pavel Alexandrovich Baranov,[35] the parasitologist Konstantin Ivanovich Skryabin, and the geneticist Ivan Fedorovich Lyashchenko,[36] also significantly contributed to the dissemination of ideas of Michirinist biology by lectures, discussions and visits to scientific institutions. In the subsequent years, the connections between Hungarian and Soviet science became closer. The number of Soviet visiting scientists increased and in addition to shorter trips of Hungarians to the Union of Soviet Republics (USSR), a number of students were sent there for graduate training.[37] A visit of Hungarian farmers to the USSR, including Michurinsk, even became a significant part of a short piece in a leading Soviet literary magazine by the Michurinist writer, Gennadii Fish.[38]

Establishment of an Institutional Base

Among the positive outcomes of the reception of Lysenkoism in Hungary was the institutionalization of biology, a scientific discipline which till then lacked any significant institutional and infrastructural support. Prior to this period, biology had not been taught as a separate subject at Hungarian universities. Tivadar Huzella (1886–1950) taught biology as part of his lectures of anatomy in the medical school in Debrecen and later in Budapest.[39] At the Pázmány Péter University of Budapest and other universities in Hungary, botany and zoology had their own departments but no department of biology existed. Among the few examples of research institutes which existed in the interwar period is the Hungarian Biological Institute, a highly specialized research center in Tihany, at Lake Balaton. It consisted of two departments, one for the study of life in and around the lake and the other for experimental biological studies. Other minor biological institutes served the Nazi-type racist ideology and politics of the interwar ruling regime dealing with genetics, rece hygiene, and eugenics[40]: these included *Magyar Nemzetbiológiai Intézet* (Hungarian Institute of National

Biology), *Központi Öröklésbiológiai és Népesedéspolitikai Intézet* (Central Institute of Hereditary Biology and Population Policy) and *Embertani és Fajbiológiai Intézet* (Institute for Anthropology and Racial Biology). All of these institutions and with them the whole field of human genetics, disappeared completely after the defeat of Hungary in the War.

In the Pázmány Péter University of Budapest (renamed Eötvös Loránd University in 1950), a Department of General Biology was organized in 1948 to give an institutional basis for the accommodation of Michurinist biology. The physician Béla Faludi (1909–1984), who trained in France and worked in medical laboratories before the War, became its head. Faludi had little experience in biology; however, he became interested in biology after the War and became a fervent advocate of Michurinism for most of his life.[41] This Department of Biology became part of the newly established Faculty of Science, created in 1949 in all universities as part of a major reorganization starting in 1948.[42]

The faculties of medicine of the major universities (Budapest, Debrecen, Szeged and Pécs) were reorganized into separate medical universities in 1951. Biology, including some Michurinist biology, became part of their curriculum. Huzella's successor at Debrecen, Imre Törő (1900–1983), professor of anatomy and histology, taught biology for a while; then in 1950, he moved to Budapest to succeed Huzella as director of the newly named Institute of Histology and Embryology. The subject was then called medical biology to differentiate it from plant or agricultural biology that was organized around Michurinist biology. Medical biology dealt relatively little with Michurinist biology.[43]

The official places for training to become a Michurinist biologist were schools of agronomy. They were centralized into the *Magyar Agarártudományi Egyetem* (Hungarian University of Agrarian Sciences) established in 1945 and moved gradually to Gödöllö, a rural town about 30 km from Budapest. A famous professor of this university was Imre Nagy, head of the Department of Agricultural Politics, a Muscovite, and future hero of the 1956 revolution. In 1948, a department of biology was set up for teaching and research in genetics and plant breeding. Barna Györffy (1911–1960), an acclaimed expert on plant genetics in Magyaróvár, was appointed to head the department, and he taught there until 1951.[44] Győrffy, however, was one of the few experts who did not accept Michurinist biology, although he did not admit this openly at the beginning. His successor, Andor Bálint, another expert coming from Magyaróvár, was more accommodating.

Finding an adequate institutional forum for research in Michurinist biology was difficult in the new system of science which separated research from training, and basic research from applied research. Michurinist biology was related to agriculture that was generally considered more of an art than science. Agricultural research was directed by the Ministry of Agriculture, instead of the Academy of Sciences, which was otherwise responsible for the basic sciences. In 1949 the Ministry of Agriculture set up various institutions, including the *Mezőgazdasági Tudományos Központ* (Agricultural Science Center), *Agrobiológiai Intézet* (Institute of Agrobiology), *Erdészeti Tudományos Intézet* (Institute of Forestry), and others, to centralize research conducted in institutions scattered around the country and to introduce Soviet methods.[45] The newly established institutes included the *Központi Növénytermesztési és Növénynemesítési Kutatóintézet* (Central Institute of Plant Cultivation and Plant Breeding) in Budapest and *Agrobiológiai Intézet* (Institute of Agrobiology) in Martonvásár; these were merged into a *Növénytermelési Kutató Intézet* (Research Institute of Plant Production) in 1950 located in Martonvásár. This latter institute became the most important research center of plant breeding. Barna Győrffy already directed an institute that he wanted to name Institute of Genetics, but during the reorganizations in 1949, his institute was incorporated into the Institute of Agrobiology.

A big part of this new, rather complex group of institutes was subsequently transferred to the Academy of Sciences. Fields related to Michurinist biology (agronomy, biology, and medicine) were represented in different sections of the Academy of Sciences, reorganized in 1949 in conformity with the Soviet Academy of Sciences.[46] Their leading experts were elected to membership in the Academy of Sciences, providing significant privileges such as high remuneration, access to special reports, better healthcare, and use of official automobiles. Academy Members became directors of research institutes, heads of university departments, or editors of journals. In short, the new Hungarian regime bestowed power on them in their scientific fields as a reward for political and ideological loyalty.

Scientists' Reception Strategies

As mentioned, Michurinist biology arrived in Hungary along with many other aspects of Soviet rule, none of which could be strongly opposed since the country was effectively occupied by the Soviet army. Almost nothing

functioned as it did before the War, and almost no one could continue with his pre-War life. This was true for science and scientists as well. Scientists could not help but accept the Stalinization of their institutional system. Accepting Michurinist biology meant giving up a more or less unconscious empiricist approach to science and accepting a methodology that relied chiefly on citations of texts of the classics of Marxist-Leninist-Stalinist philosophy. Prior to this period, these texts had been virtually unknown to Hungarian scientists.

The new regime could build up a new institutional system, but the new institutions also needed people who would and could work in them. Glushchenko noticed this problem as he related his experiences in Hungary in his report to the Soviet Academy of Sciences: "Hungarian science, especially biology, is weak." He mentioned the hostile attitude of the Communist authorities to agronomists and scientists "who are regarded as almost reactionaries."[47]

The reaction of Hungarian biologists can be divided into five categories: Passive resistance, passive acceptance, passive opportunism, active opportunism, and sincere belief. Most experts functioned as plant breeders with practical knowledge but lacking in even the basics of genetics. Researchers who wanted to avoid working following the Michurinist line often used such ignorance as an excuse. However, some geneticists even found passive resistance to be an effective response to the new Soviet biology.

Barna Győrffy, for example, was a leading plant geneticist of the period. He received his scientific training in Germany in 1937 supported by a Hungarian fellowship intended to fund research serving the racist politics of the Hungarian dictator Miklós Horthy. Győrffy worked in the *Kaiser Wilhelm Institut für Biologie* directed by Fritz von Wettstein, an adherent of Russian geneticist Nikolai Vavilov.[48] Győrffy was held in very high esteem as a plant geneticist by his Hungarian colleagues, and he never praised Michurinist biology. In fact, when he was asked to speak or write about Lysenko or Michurinist biology, which could have enhanced his and his coworkers' positions, he consistently turned down the requests. Because of his authority as an expert, and a holder of a 1949 Kossuth prize, he was a member of every important scientific institution and body related to plant genetics. However, he was elected a member of the Hungarian Academy of Sciences only posthumously (1990).

In spite of all this, Győrffy was in charge of a large project organized by the Hungarian Academy of Sciences, with the title "Inheritance of Acquired Properties and Studies of Vegetative Hybridization."[49] Five institutions participated in this research project and several others conducted

research on subjects connected with it: Agrarian University (led by András Somos), Soronhorpács (Kurt Sedlmayr), Institute of Agrobiology in Martonvásár, Fertőd, Sopron (Rezső Bokor), Tihany, Institute of Botany at University of Debrecen, Botany Department of the Teachers College in Pécs, and others. Some institutions experimented with crossing distant species. Győrffy's students and coworkers were proud of their master who was considered an excellent scientist and a morally impeccable person.

Other biologists passively accepted the new political reality by publishing materials apparently supporting Michurinist biology. They delivered lectures and published articles with titles such as "Damage of Morganist Genetics to our Plant Breeding," "Hungarian breeders turned to methods of Michurin," "Michurinist Biology and Hungarian Plant Breeding," "The Spread and Results of Michurinist Biology in our Country," "Michurin's Impact upon Current Hungarian Husbandry," and so on.[50] These papers praised Soviet science, described its victories, and explained how much Hungary could learn from it. They emphasized the superiority of dialectical materialism, praised the great men behind its philosophical theories, and so on. Acceptance of the tenets of Michurinist biology in agriculture was seemingly widespread. Three years after 1948, the Hungarian Academy of Sciences already included in the program of its annual meeting a conference entitled "Results of the application of Michurinist Biology in our plant breeding and production of seed material."[51]

These publications, however, often revealed the authors' passive opportunism rather than their simple acceptance. A good example is Kurt Sedlmayr, agronomist of Austrian origin, who rented an estate in Sopronhorpács, near the Austrian border. His estate was nationalized in 1950 but he was kept on as director. He developed new polyploid varieties of beet that gave excellent yield and were more resistant to pests than other varieties available in the market. He cooperated with Barna Győrffy on polyploidy, a favorite topic of Gőrffy's. Sedlmayr was an excellent plant breeder who achieved remarkable results. He praised Michurinist biology and claimed that his success was due to applying its methods but he did not really rely on them in his work.[52] He was elected member of the Academy of Sciences in 1949, and twice received the Kossuth Prize, the highest distinction in Hungary, in 1950 and 1954. In 1956 he emigrated to Austria.[53]

This well-intentioned passive opportunism seems to apply the "veritas duplex" of the Middle Ages, separating from the truth of belief the truth of knowledge. While advertising politically proper ideological positions, passive opportunists relied on expert knowledge in their experimental studies. Such opportunism was tacitly supported by some Soviet visitors,

for example by Lyashchenko who visited Hungary in 1952–1953, and in his lectures he used the Michurinist notions of vernalization, vegetative hybrids, and creative Darwinism with ample citations from Engels, Stalin and Michurin, Lysenko and others.[54] But as a classical geneticist, an expert on wheat and sunflower biology, he was able to discuss with Hungarian plant breeders in technical terms. He suggested breeding plants, such as sunflower and rice, resistant to pets, but he found his Hungarian colleagues' theoretical knowledge and laboratory biology insufficient in contrast to descriptive botany and zoology.[55]

Another strategy, active opportunism, was presented by Imre Törő, member of the Hungarian Academy of Sciences and a knowledgeable medical biologist.[56] With the support of the Hungarian government, Törő had spent 1929–1930 in Berlin Dahlem in the Kaiser Wilhelm Institute of Biology studying developmental biology as a pupil of the future Nobel laureate Hans Spemann and his associate, Hilde Mangold. Subsequently, from 1936 until 1938, Törő worked as a Rockefeller fellow in the Anatomy Department at the Columbia University in New York City. In 1950, after moving to Budapest from Debrecen and becoming head of the Department of Histology and Embryology of the Medical University of Budapest, Törő reported on his own studies on the evolution of "non-cellular living substance," a topic that had very recently—in the summer of 1950—became a major chapter of Michurinist biology.[57] He claimed to have observed the formation of cell nuclei without the participation of pre-existing nuclei and called the phenomenon neokaryogenesis. As he wrote: "The essence of the process is the formation of a new cell nucleus in the protoplasm without participation of the nucleus of the mother cell, by chemical transformation of nucleic acids."

Törő interpreted this as formation of cells from non-nuclear intracellular living matter and stated that his experiments confirmed Olga Borisovna Lepeshinskaya's ideas on the development of cells from non-cellular living matter. Törő presented his claim in one single lecture and published it in detail.[58] In essence that was all he contributed to Michurinist biology. He never returned experimentally to this idea again, which was, however, deemed as important evidence in support of Michurinist biology by the Stalinist authorities who advertised it widely. Newspaper articles, radio reports, interviews and even school textbooks discussed Törő's breakthrough.[59] Törő immediately received the Kossuth prize in 1952 and gradually gained various high positions in directing Hungarian science, including head of different research institutes, laboratories, and medical section of the Hungarian Academy of Sciences. He also chaired scientific committees and

in turn became the dean and rector of the medical university. He was elected member of Parliament and other political bodies. His success proved providential for Hungarian biology. In all these positions, Törő could significantly help his colleagues' research and the overall progress of biology in Hungary. Such opportunism was rare but not without other examples.

Finally, there were some sincere Lysenkoists who promoted the reception of Michurinist biology. Next to Béla Faludi, pathologist-turned biologist and first professor of biology of the Eötvös Lórand University in Budapest, the secretary of the Hungarian Scientific Council (1948–1949), László Rajki,[60] was one of the most enthusiastic representative of Michurinist biology. Rajki studied agronomy, first in Kolozsvár (now in Romania) and subsequently in Budapest and Magyaróvár; he received a doctoral degree from the *Magyar Agrártudományi Egyetem* (Hungarian University of Agricultural Sciences) in 1948. During his studies he worked as assistant professor in Magyaróvár. He did graduate studies in the Soviet Union in 1949–1951 and again 1954–1955, further developing his positive views of Michurinist biology. These fellowships helped him much with his career. Subsequently, Rajki occupied positions in various state offices related to agriculture until 1954, when he was appointed director of the Agricultural Institute of the Hungarian Academy of Sciences in Martonvásár. In spite of his views on genetics, as director and breeder, Rajki was a pragmatic and proved to be quite successful. The varieties of wheat and corn produced by his institute were widely appreciated.[61]

While these reception strategies differed from each other, nobody risked to debate Michurinist biology openly, just as there were no public debates about Stalinism as a political and ideological doctrine. Under the strict control of the party, it would have been dangerous to voice criticism. Debates only started once the political circumstances altered.

Start of Counter-reception in the Early Post-Stalin Period

After Stalin's death in 1953, Hungarian politics changed significantly. The changes gradually led to a crisis of the whole regime which culminated in the 1956 revolution. The reception of Michurin biology, an element of the Stalinist structure, changed in parallel. In 1953 Rákosi was ousted and Imre Nagy, Győrffy's former colleague in Gödöllő, was appointed prime minister.[62] In 1950 as Minister of Food Production, he criticized Rákosi and the other leaders for their rigid and erroneous politics. Nagy aimed to improve living conditions, limit the activity of the secret police,

free political prisoners, and close labor camps. In short, he wanted to slow down communist restructuring without changing the regime. Given his interest and expertise in agrarian policies, he introduced some changes in the organization of agriculture. He stopped forcing peasants to work in Soviet-type collective farms and provided machines and material for developing their own small lots. This was politically relevant because in Hungary, a traditional agricultural country, about fifty percent of the population lived by farming. Thus Nagy's ideas had closer contact with the particular Hungarian conditions and traditions than Rákosi's program of industrialization.

Nagy's program also had a slightly nationalist flavor, an important element in a country where anti-Russian and anti-Soviet sentiments were strong. However, the changed agrarian politics did not yet signal loss of faith in Michurinist biology by the government yet. A 1953 report on research submitted to the secretariat of the Party stressed the importance of "organized study of dialectic materialism and Michurin biology by scientific workers in agronomy… The Academy of Sciences should organize a public debate on the most important…questions of Michurinist biology this fall…. Study of the material of the debate on Lysenko's theory on the origin of species currently developing in the Soviet Union should be used to broadly popularize Michurinist biology and to fight against the existing Morganist views."[63] In an editorial article in *Agrártudomány* (Science of Agriculture) published in 1953, Nagy still emphasized the role of Michurinist biology in achieving high yields.[64]

Despite the relative popularity of Nagy's approach, Rákosi and his group convinced the Soviet leaders of its faults. Nagy was deposed, expelled from the party and by 1955 the unpopular Rákosi had returned to power. By that time, however, the terrifying atmosphere of Stalinism had somewhat eased. Discontent could be more easily expressed, and Khrushchev's secret speech at the 20th congress of the Soviet Communist Party in February 1956 encouraged a wave of social, political and ideological criticism originating from various sources inside and outside the Hungarian Communist party.[65]

In parallel to these political developments, Michurinist biology gradually lost its political and scientific importance. Hungarian scientists soon noticed a somewhat weakened ideological pressure. A process started that appears like a counter-reception: almost as if the scientific community turned against Michurinist biology spontaneously as some of the most vociferous participants in its support perceived that the political winds had

shifted. The representatives of various reception types of Michurinist biology were able to express themselves more freely in the changing political, ideological atmosphere and open conflicts between them became evident and unavoidable.

The first reactions to the slight ideological relaxation soon appeared. The first papers[66] in a Soviet botanical journal criticizing Lysenko's theory of species formation, a late addition to Michurinist biology,[67] were immediately noted by Hungarian biologists. In February 1953, at a meeting of the Biological Section of The Hungarian Academy of Sciences, decision was reached to organize an open discussion of Lysenko's theory on speciation and have the relevant literature translated into Hungarian.[68] The Party secretariat, however, prohibited the debate: "The Academy of Sciences should not organize a debate on Lysenko's theory on the origin of species."[69]

Meanwhile, objections to Michurinist biology started appearing in the press. The Society for the Dissemination of Scientific and Social Knowledge reported in detail in its journal on the debate on Lysenko's views on species formation in the Soviet Union.[70] This topic remained in the focus of interest in Hungary as well.

For instance, Sándor Tóth, a philosopher, spoke at a conference organized by the Eötvös Lóránd University in 1955 to commemorate the tenth anniversary of the Soviet liberation of the country. In his talk "Philosophical aspects of the debate on species formation,"[71] based on dialectic materialism, Tóth explained that both the adherents of gradual evolution and of evolution by leaps are one-sided. Materialist dialectics teaches that quantitative (i.e., gradual) and qualitative (i.e., leaps) changes are intimately related to each other. He regarded Lysenko as a great biologist who occupied an outstanding place in the history of science but found his stand on this particular problem to be limited because of Lysenko's disregard for the quantitative element of evolution. Tóth noted, however, that some critical comments indicated a turn against Stalinism. Tóth wrote that according to some "Lysenko's teaching is fundamentally wrong. In the Soviet Union some views are forced to be accepted and their criticism is prevented by power. Lysenko's publication that appeared in 1948 … is false, its theoretical bases cannot be upheld. Lysenko's views are also false from the philosophical points of view. … Michurin's agrobiology does not help agricultural practice; it only serves the goals of ideological propaganda." While Tóth rejected these views, he did defend Lysenkoism with some reservations. He referred to the superiority of Soviet science over Western and the enormous advantage of dialectic materialist philosophy in

biological research. However, he did not deny that the objections to the Lysenkoist theory are not baseless; they pointed out "some weaknesses, contradictions in the hypotheses and some untenable points in them." Tóth still stressed, however, that the development of Hungarian biology depended on forming a creative relationship with Michurinist biology and that Hungarian science must establish good contact with Soviet science.

The Hungarian Academy of Sciences organized a major discussion on the problem of species formation on May 15, 1956. Senior botanist Rezső Soó, member of the Academy, discussed his views on species criticizing Lysenko's new ideas in his talk.[72] He was convinced of the existence of intra-species competition and gradual evolution, which Michurinist biology denies. Lectures were given by a number of prominent scientists.[73] Subjects discussed were the struggle for existence between individuals of the same species, how this struggle acts as mechanism of species formation, cooperation between individual organisms belonging to the same species, whether evolution is gradual or works by leaps and bounds, how characteristic Michurinist ideas represented by Lysenko can be applied in specific cases, and so on. The speakers advocated Lysenko and Michurinist biology, but they also raised objections and pointed out the need for further validation of certain Michurinist claims. A detailed report on this meeting was published by Soó in the official journal of the Society for Dissemination of Scientific and Social Knowledge.[74]

Shortly after this session the Division of biology of the Hungarian Academy of Sciences organized a discussion on June 1, 1956, covering "Hungarian Results in Researches on Genetics."[75] Barna Győrffy presented an overview of genetic research in Hungary[76] in which he expressed his doubts about the very existence of vegetative hybrids. The results of a research project organized by the Academy, under his leadership, "Inheritance of Acquired Properties and Studies of Vegetative Hybridization" enabled him to reach this conclusion. He was dissatisfied with the methods used and remained unconvinced by all the claims his colleagues made concerning their production of vegetative hybrids. In addition, Győrffy remarked that no real biologists had worked on the project since the participants were experts only in agronomy, horticulture, and forestry. Győrffy considered them scientifically naïve and that they did not understand the idea of species change. Győrffy had now turned from passive to active resistance.

In the debate, the Lysenkoist Faludi argued at length in support of the inheritance of acquired characters. He cited a number of non-Soviet

sources, including Western authors who published positive results. Andor Bálint, a passive opportunist now out openly, discussed the dilemma of breeders who wanted to achieve higher yields but did not wish to engage in theoretical problems. Two theories confront each other, he said, and everyone knew who advocated them. He himself supported Michurinist biology, but did not disregard empirical facts. With this confession, Bálint formulated the dilemma of scientists: should they uphold an empiricist philosophy of science or apply the philosophy propagated by the Stalinist political regime? Several participants, including Rezső Soó, pointed out that that classical genetics had significant traditions in Hungary. Miklós Fehér expressed that he was glad to have been invited although his field, human genetics, had become an outcast thanks to its connection with the racist Nazi eugenics line before the War. A number of comments followed discussing experiments of influencing plant development by physical factors, such as X-rays and ultrasonic radiation, or applying chemicals. Endre Papp, a Kossuth prize laureate famous for breeding corn hybrids in Martonvásár, went as far as to say that hybrids cannot be produced without classical Mendelian genetics.

Counter-reception

This debate on genetics at the Hungarian Academy of Sciences was already held in an atmosphere of "constructive criticism" of the regime. Hungarians had begun expressing their dissent with the official views dictated by the party, best exemplified by the organization in the summer of 1956, of Petőfi Circles as venues for uncensored debates on various subjects, such as education, press, history, medicine, and the economy. These groups consisted primarily of young professionals, many of whom were supporters of Imre Nagy. Michurinist biology, however, was barely brought up, even though agriculture was one of the frequent topics of discussions. Lysenko and Lepeshinskaya were mentioned only in a debate on education as examples of a new kind of nonsense science that existed without evidential foundation.[77] The speaker was the philosopher-mathematician Imre Lakatos,[78] who left Hungary in late 1956 and became one of the most influential philosophers of science of his time as professor at the London School of Economics.

The Hungarian revolution broke out in October 23, 1956. Gerő was dismissed and Nagy became prime minister again by the will of the marching masses. Revolutionaries now turned against Soviet dictatorship,

demanding the re-establishment of multiparty politics. On November 4, the Soviet army invaded Hungary, and made János Kádár general secretary of the Party. He imprisoned, and later executed, many leaders and participants of the revolution, including Prime Minister Imre Nagy (in 1958).

In spite of the bloodbath, a rigid Stalinist regime never returned. Hence, one of its features, Michurinist biology, disappeared gradually from science and education in Hungary, despite Lysenko's second blooming in the Soviet Union under Khrushchev.[79] In 1957, the central Soviet paper *Izvestiya* (News) published a major paper by Lysenko defending the tenets of Michurinist biology against recent developments in molecular biology.[80] Imre Törő reflected on this new development in a paper in the daily *Magyar Nemzet* (Hungarian Nation), a newspaper reputed to be more open to intellectual topics than the party's official newspaper, *Népszabadság* (People's Freedom), successor of *Szabad Nép* (Free people). Törő summarized the long scientific debate on the role of environment in inheritance. He regarded as one-sided both Weismann's thesis about the immutability of the germplasm and Michurin's and Lysenko's claim that adaptation to the environment can be inherited. The discovery of the genetic role of nucleic acids, DNA, proved that inheritance through metabolism is not possible, that is, vegetative hybrids cannot exist. All Hungarian experiments in this field brought negative results. Acquired characteristics cannot be inherited, but, he concluded new species could perhaps be produced in the future by changing the DNA. However, in some microorganisms inheritance might be influenced by other means also. According to Törő dialectic materialism is in good harmony with the genetic theory based on nucleic acids because DNA is a material entity, not an idealist assumption.[81] His main thrust was conciliatory, "We have to combine what is good in both doctrines." This approach was gaining popularity among Hungarian biologists. Törő radically changed his stand from active opportunism by embracing modern molecular biology.

Sándor Rajki, by that time director of the research center in Martonvásár, sincere Lysenkoist, retained his conviction and replied to Törő's article in the same newpaper.[82] He complained that starting from the meeting held in the Academy of Sciences on May 15, 1956, Michurinist biology has been silenced. According to Rajki, Törő called Lysenko's genetics dogmatist while evincing his own dogmatic views when it came to formalist genetics. According to Michurinist genetics, wrote Rajki, "inheritance is a property of living material, related to all its particles not only to the chromosomes."

This claim is proven by the existence of vegetative hybrids. Michurinist biology is based on the "the unity of organism and environment" that ensures the possibility of inheritance of acquired characteristics as it happens in the case of winter and spring wheat. Formalist geneticists, such as Törő, based their arguments on negative experimental results which, says Rajki, were complex and difficult to obtain.

Both Törő and Rajki used empiricist evidence, rather than relying on ideological, philosophical, and political reasoning. Although Törő explained that Western genetics does not contradict dialectic materialism and Rajki replied that though everything stated in dialectic materialism is true, it does not follow that everything a dialectic materialist states is true as well. The style of this discussion—as scientific debate—was typical in the Hungarian scientific-political context of 1958. Scientific arguments were used instead of citations from ideologists and the relevance of the debated issues was not linked to any political regime. The political relevance of Michurinist biology was definitely vanishing.

In the next stage of counter-reception, the participants discussed the political relevance of genetics in post-Stalinist political Hungary. On October 9, 1958, a Committee of Genetics of the Biology Division of the Hungarian Academy of Sciences held a debate on the political character of the Michurinist biology. Törő, now representing modern genetics, chaired the session and Rajki, sincere Lysenkoist, gave an extremely long introduction in defense of Michurinist biology.[83]

According to Rajki, there are Michurinists and non-Michurinists. While Michurinist biology had a monopolistic position until 1953–1954, Michurinists had now been excluded from important positions in Hungary due to Barna Győrffy's efforts. The field should be open both to Morganists and Michurinists, he argued. Both deserved a chance. He, as a breeder, was successful without thinking in genetic terms because breeding is a practical activity of crossing and selection. Rajki complained that he felt excluded. Faludi, another sincere Lysenkoist, agreed with Rajki in his feelings of exclusion. He thought Győrffy, denying the inheritance of acquired characters, persecuted Michurinist geneticists.

Another point was offered by Andor Bálint, a passive opportunist, who claimed that genetics is politics. Both Michurinist and non-Michurinist geneticists could be supporters or enemies of socialism. Hence, the real selective criteria of one's election to the membership of the Committee of Genetics at the Academy of Sciences should not be adherence to Morganism but faithfulness to the socialist political regime.

The meeting was also attended by Bruno F. Straub, a highly respected biochemist. Straub had been student and coworker of Albert Szent-Györgyi, the only Nobel laureate who received the prize while working in Hungary. Straub had high status in politics also. He would become the last President of Hungary before the collapse of the Soviet empire. At the meeting, Straub said that the Committee of Genetics was the most interesting committee at the Academy, a committee where real intellectual debates occur. However, genetics in Hungary was not sufficiently advanced to really contribute to the solution of fundamental problems of the discipline. He suggested that the committee should not try to decide the question of the validity of Morganist genetics, but should rather support the breeders who do the real job.

Törő, as chairman, concluded that there was a single genetics rather than two different genetics. In this science, as in other branches of science, experiments decide between rival theoretical statements. The existence of vegetative hybrids had not been experimentally verified; consequently they most probably do not exist.

This meeting showed that Barna Győrffy was now comfortable openly criticizing Lysenko in active resistance. In a report sent to the Academy of Sciences in 1959, Győrffy wrote that with his group he experimentally studied the growth of plants, its relations to metabolism, the possible regulation of metabolism, and inheritance of acquired characters, and they particularly focused on vegetative hybridization and in vitro embryo breeding. He concluded that "all our experiments on vegetative hybridization (one of my assistants started them in Magyaróvár) proved to be negative despite all efforts we exerted. These failures justify our skeptical attitude toward Lysenkoism."[84] Győrffy's motivation to reject Michurinist biology appears to be based on his empiricist conviction. He never argued with citations taken from Stalinist ideology, but only with his laboratory experiences.

The counter-reception continued with an unexpected event. The main character of Michurinist biology, Trofim Denisovich Lysenko appeared on the Hungarian stage in January 1960. This happened after Lysenko's comeback from his first fall, but before he was appointed again as head of the Lenin All-Union Academy of Agricultural Sciences in 1961 (See Fig. 1). Lysenko did not like to travel, yet that year (1960) he visited both Hungary and Czechoslovakia.[85] In Hungary, he was invited to a meeting on corn breeding and then delivered a public lecture at the Hungarian Academy of Sciences. His lecture or rather the session where he gave answers to 200 previously submitted questions figures among the best attended events

Fig. 1 Trofim Lysenko in Hungary
Source: Mrs. Diana Hay, Hungarian Academy of Sciences

in the history of the Academy of Sciences.[86] In his lengthy presentation, interrupted by a long break, Lysenko discussed the major topics of Michurin biology without any indication that he took notice of the advances in biology of the past decade. He reiterated his well-known views on the inheritance by the whole organism instead of specific genetic material such as DNA, to which he referred in a negative tone. He did not say anything about modern results in the study of DNA and RNA, which were a primary focus of research at that time. Lysenko said that "no organs are present in fertilized chicken egg, no beak, no leg, no feather. All these arise during development…If an organ can develop from another one, why should we assume that cells cannot arise from anything else, but from another cell?"[87] Lysenko could not give substantial answers to any question raised by the experts but simply repeated old Michurinist views, which most Hungarian scientists had rejected by that time. It was a great disappointment to the large audience and in the end result was that Lysenko personally contributed to the rejection of Michurinist biology in Hungary.[88]

Aftermath

From the 1960s onward not much remained of Michurinist biology in Hungary. Kádár's regime became gradually softer,[89] and by the late 1960s, with the advent of "Gulash communism," it was broadly accepted by the population. Although the Stalin era ended by the 1950s, some of its basic elements, as central planning, one-party government, Marxist-Leninist ideology, secret police surveillance, and others remained in Hungary until 1989. The uprising in 1956 prompted some caution in the behavior of both Soviet and Hungarian communist leaders. As a result, the regime lost its rigidity and grew into a formation often called "soft dictatorship," which aimed to keep the population quiet and politically passive by increasing the living standard.[90] Michurinist biology could not serve the goal of growing yields in agriculture, Soviets no longer forced its acceptance, and the large majority of experts refused it in the light of molecular genetics. Yet some remnants were still present.

While even Faludi turned away from Michurinist biology in his textbook of genetics in 1965,[91] sincere Lysenkoist Rajki still did not give up. In 1966 he had a dramatic clash with Barna Győrffy after submitting a thesis "Autumnization and its genetic interpretation."[92] to obtain the degree of Doctor of Science given by the Academy of Sciences (a system copied from the Soviet one). The dissertation defense lasted for two days, as his claims were strongly attacked.[93] Barna Győrffy was one of the opponents who commented on the dissertation and pointed out blow-by-blow all methodological and logical mistakes, improper usage of statistics, wrong assumptions, and the Michurinist approach. Rajki's replies were not accepted by his opponents nor by the audience. Finally, the chairman, a plant physiologist, Vilmos Frenyó, stopped the debate by fiat. In the secret ballot seven members of the thesis committee voted in favor of Rajki and only two against him.[94] Nobody knows who voted for and who against but the positive votes show that a Michurinist view did not automatically exclude someone from the scientific community just yet.

In the end Rajki felt compelled to face the advances of modern genetics. He turned away formally from Michurinist biology, accepted the basic role of DNA in heredity, but proposed a dramatic revision to the central dogma of modern genetics by hypothesizing a reverse transmission of information from protein to DNA, allegedly explaining the inheritance of acquired characters. He published a short book on this hypothesis in 1972.[95] He did not provide direct evidence for this hypothesis and only outlined his future experimental plans to test it. His hypothesis was not

accepted by the scientific community and received a harsh rejection in a review written not by a "reactionary" Western bourgeois scientist, but by a member of the Soviet Academy of Sciences, Nicolai Dubinin, director of the Institute of Genetics of the Soviet Academy. In his evaluation, Dubinin used expressions such as 'ridiculous', 'ignoring the development of genetics', 'scientifically insular views', and so on. Based on the basic tenets of contemporary genetics, Dubinin rebutted all claims made in the book.[96]

This debate was in essence an epilogue to the history of Michurinist biology in Hungary. By the 1970s its views, although not its old representatives, disappeared from science in Hungary.

Conclusions

The reception of Michurinist biology in Hungary had three phases.[97] In the first phase (1930s–early 1940s), it was regarded as representing new scientific results. Hungarian plant breeders and geneticists noticed new ideas in their field. Early experiments made by Barna Győrffy's group and others, however, led to suspicion concerning the validity of the new claims.

In the second phase, starting in the fall of 1948, before any discussion could have started concerning its validity, Michurinist biology changed its character from a scientific theory to a component of a totalitarian political regime, becoming part of the ideology of Stalinism acceptance of which was enforced by a military power that defeated and occupied Hungary in the Second World War. It functioned as a vehicle to reorganize science and intellectual life into the Soviet pattern and was an instrument of the government for political and ideological repression. Besides introducing an unfamiliar philosophical and ideological doctrine (dialectic materialism), a new institutional structure was also developed by the government. To accept Michurinist biology was more a question of political loyalty than of scientific conviction. The political circumstances of a defeated country left no space for resistance to the fundamental reorganization of every aspect of Hungarian life, including science.

Scientists developed various strategies to adapt to the new situation, the most effective of which was active opportunism. Passive resisters did not speak up, but merely silently distanced themselves from Michurinist biology. Passive accepters mouthed the slogans of Michurinism, produced articles and other types of publication in support, but otherwise ignored it. Passive opportunists propagated Michurinist biology, advertised its ideological merits, but hardly used it in their professional work, and took advantage of whatever opportunities supporting Soviet science offered

them. Active opportunists, on the other hand, contributed research results to Michurinist biology, based on their empiricist philosophy, while they might have regarded Michurinist biology as nonsense. Finally, there were sincere Lysenkoists who supported Michurinist biology with conviction.

In the third phase, after 1953, with de-Stalinization and decreasing external political pressure, the reception process turned in the opposite direction; a spontaneous counter-reception got underway. Michurinist biology gradually lost its status as a component of a political regime and became again a scientific doctrine that led to a number of open scientific debates. Michurinist biology was criticized on empirical basis, was considered science, bad science in fact, rather than pseudoscience, as astrology is.[98]

The dynamics of the reception of Michurin biology in Hungary was asymmetrical. Its reception and the formation of strategies in response occurred quickly, but its counter-reception resulted only in its slow gradual disappearance. The Hungarian revolution in 1956 was definitely not a turning point in the history of Michurinist biology in the country. Debates went on long after it.

That said, there was not much violence around Michurinist biology in Hungary. Vavilov and other suppressed Soviet geneticists hardly had any Hungarian counterparts. Geneticists were not imprisoned because of their anti-Michurinist views. But Michurinist biology probably induced excellent experts to leave Hungary for the West, such as Ödön Villax, Kurt Sedlmayr, Tibor Rajháthy, György Rédei, László Oláh, and others. Many other scientists and breeders had to abandon their ongoing research. Many were forced to speak against their convictions and to work in directions that they thought to be hopeless and nonsensical. They felt that they were forced to waste their precious time and energy due to a dreadful and outrageous political regime.

Michurinism as an intellectual endeavor and political symbol proved to be ephemeral in the field of biology but the institutional structure built for its conduct largely survived. The ironic outcome of Lysenkoism in Hungarian biology is that it—at long last—resulted in the establishment of biology as a scientific discipline in its own right, in Hungary.

Notes

1. “A szerzett tulajdonságok öröklődése” (The Inheritance of Acquired Characteristics), *Szabad Nép*, August 11, 1948.
2. Lysenko’s talk in English translation in: *The Situation in Biological Science*. Proceedings of the Lenin Academy of Agricultural Sciences of the

USSR. Session: July 31–August 7, 1948. Verbatim Report. Foreign Languages Publishing House, Moscow, 1949, pp. 11–50. Also available on the Internet. "The Situation in the Science of Biology," Address delivered by Academician Trofim Denisovich Lysenko, July 31–August 7, 1948. Downloaded December 6, 2013, http://www.marxists.org/reference/archive/lysenko/works/1940s/report.htm

3. Trofim Denisovich Liszenko, "A biológiai tudomány állásáról" (The Situation in Biological Science), *Társadalmi Szemle* 3 (1948), pp. 549–576. Also in T.D. Liszenko, *A biológiai tudomány állásáról* (Budapest: Szikra Kiadás, 1949), pp. 1–135.
4. See Marius Turda, "In Pursuit of Greater Hungary: Eugenic Ideas of Social and Biological Improvement, 1940–1941," *The Journal of Modern History* 85, no. 3 (2013), pp. 558–591; and idem, "'If Our Race did not Exist, It Would Have to be Created': Racial Science in Hungary, 1940–1944," in *Racial Science in Hitler's New Europe, 1938–1945*, eds. Anton Weiss-Wendt and Rory Yeomans (Lincoln: University of Nebraska Press, 2013), pp. 237–258.
5. Rudolf Fleischmann, "A buza 'jaworizációja (sic!)" (Jaworization (Sic!) of Wheat), *Köztelek* 43, no. 39–40 (1933), p. 351.
6. R.O. Whyte and P.S. Hudson, "Vernalization or Lysenko's Method for the Pretreatment of Seed," *Imperial Bureaus of Plant Genetics, Bulletin* (Aberystwych) (March 1933), p. 27.
7. Ottó Bocskay, "A tavaszi buza tenyésztési idejének megröviditése" (Shortening of the Vegetation Period of Spring Wheat), *Köztelek* 44, no. 7–8 (1934).
8. Miklós Horn, "Orosz forradalmi tanok a növénynemesités terén" (Revolutionary Russian Ideas in Plant Breeding), *Köztelek* 52, no. 21 (1942), pp. 506–508.
9. Ödön Villax, *Növénynemesítés I. Általanos Növénynemesités* (Plant Breeding I. General Plant Breeding) (Magyaróvár: Pátria Nyomda, 1944), pp. 1–354.
10. Interview with Árpád Kiss, In: Avar László, *Rögös vallomások, Ötven év a magyar agrárkutatás szolgálatában* (Rough Confessions. Fifty Years in Service of Hungarian Agricultural Research) (Martonvásár: Biological Research Institute of the Hungarian Academy of Sciences, Szeged, and Research Institute of Agronomy of the Hungarian Academy of Sciences, 1999), pp. 96–109.
11. Barna Győrffy, "A hazai genetikai kutatások eredményei" (Results of Genetic Research in Hungary), *A Magyar Tudományos Akadémia Biológiai Csoportjának közleményei* 1, no. 1 (1957), pp. 35–46.
12. "Report of the Society for the Period of January–May, 1949," Russian translation. GARF—State Archives of the Russian Federation, Moscow (Gosudarstvennyi Arkhiv Rossiiskoi Federatsii, hereafter—GARF), Fond 5289, Opis' 17, Delo 152, List 50.
13. We use a reception–counter-reception terminology, instead of the export, import terminology suggested by Krementsov. These latter metaphors are

borrowed from trade assuming two partners selling and buying goods according to their needs. We, however, aimed to point out the forced character of the reception process of Michurinist biology. Nikolai Krementsov, "Lysenkoism in Europe: Export-Import of the Soviet Model," in *Academia in Upheaval: Origins, Transfers, and Transformations of the Communist Academic Regime in Russia and East Central Europe*, eds. Michael David-Fox and Gyorgy Peteri (New York: Garland Publishing Group, 2000), pp. 179–202.

14. The historical literature on the period is voluminous but the most often cited source is probably: Ignác Romsics, *Magyarország története a XX. században* (Budapest: Osiris, 1999), pp. 374–400. English translation: Ignác Romsics, *Hungary in the Twentieth Century* (Budapest: Corvina, 1999).
15. In a newspaper article, mathematician György Alexits, secretary of the Science Council wrote: A magyar mezőgazdaságtudomány középpontjába a micsurini-liszenkoi elméletet kell helyeznünk. (We Have to Make the Michurin-Lysenko Theory the Foundation of Hungarian Agriculture and its Theory), *Szabad Nép*, February 19, 1949, p. 5 (In Hungarian).
16. For the history of the Scientific Council see: Tibor Huszár, *A hatalom rejtett dimenziói. A Magyar Tudományos Tanács 1948–49* (The Hidden Dimensions of Power. The Hungarian Scientific Council 1948–49) (Budapest: Akadémiai Kiadó, 1995), and Sándor Kónya, *A Magyar Tudományos Tanács 1948–49* (The Hungarian Scientific Council 1948–49) (Budapest: A Magyar Tudományos Akadémia Könyvtárának Közleményei, 1998).
17. Gerő's memorandum about the reorganization of science. Decisions of the Party Collegium, June 18, 1949, AL Magyar Tudományos Tanács Iratai, Box 1, folder 7.
18. About the introduction to Hungary of new plant species in Michurinist spirit see: Zsuzsanna Borvendég and Mária Palasik, *Vadhajtások - A sztálini természetátalakitási terv átültetése Magyarországon 1948-1956.*(Untamed Seedlings: Hungary and Stalin's Plan for the Transformation of Nature), (Budapest: Napvilág Kiadó, 2015). English version in press. We are grateful to the authors for providing us with their manuscript.
19. "Sabanov: A Magyar tudomány már ugyanazt az utat járja, mint szovjet tudomány" (Shabanov: Hungarian Science Follows the Same Path as the Soviet Science Now), *Szabad Nép*, February 18, 1949, p. 2.
20. "A természet megváltoztatása Gluscsenko professzor előadása Micsurinról és a szovjet biológia új útjairól" (Transformation of Nature. Professor Glushchenko's Lecture on Michurin and the New Roads of Soviet Biology), *Szabad Nép*, February 17, 1949, p. 6.
21. "A tudomány helyzete nálunk gyökeresen megváltozott" (The Situation in Science Changed Radically), *Szabad Nép*, April 8, 1949, p. 3.
22. Trofim Deniszovics Liszenko, *A micsurini irányzat a biológiában* (Michurin Trend in Biology), *Természettudomány: A Magyar Természettudományi Társulat közlönye* 3, no. 11–12 (1948), pp. 301–311.

23. About the organization of the socialist public science society: Z. Karvalics László, *Egy értelmiségi tömegszervezet hétköznapjaiból: A TIT születése és első évei 1953–1964* (Days of a Mass Organization of Intellectuals. Birth and the First Years if TIT) (Szeged: JATEPress, 2010).
24. "Jelentés az ismeretterjesztő munkáról" (Report on Work in Popularization of Science). Hungarian State Archives, Budapest (Magyar Országos Levéltár, hereafter—MOL), 276 folio 89/372. ö.e., pp. 1–8. Unsigned and undated document that contains data and evaluation on the period of 1949–1953.
25. "Report on the propaganda actions of VOKS and foreign Societies of freindship [with the USSR] as related to the August session of the Lenin Academy," GARF, Fond 5283, Opis' 1, Delo 433, Listy 1–36. (In Russian).
26. Irén Klára Szabó, "A szovjet agrobiológiáról" (On Soviet Agrobiology), *Agrártudomány* (1949), pp. 2–15. Also as a small booklet printed in three editions: Irén Klára Szabó, *Az micsurini átöröklés elve és gyakorlati bizonyitékai* (Principles and Practical Proofs of Michurin's Genetics) (Budapest: Uj magyar könyvkiadó, 1949, 1949, 1950), pp. 1–39. Irén Klára Szabó, horticulturist, was trained as one of the first Hungarian-Soviet fellowship holders in Michurinsk, the town of Michurin in 1946–1947. See: István Nagy, *Szovjetösztöndijasok voltunk 1956-ban* (We were Soviet Fellowsip Holders in 1956) (Budapest: Press Publica Könyvkiadó, 2011), pp. 1–384.
27. "Magyarázó megjegyzések" (Explanatory Notes), *Agrártudomány* 1, no. 9 (1949), pp. 472–474.
28. Trofim Deniszovics Liszenko, *Agrobiológia* (Agrobiology) (Budapest: Mezőgazdasági Könyvkiadó, 1950), pp. 1–671.
29. N.V. Turbin, "Örökléstan és a Nemesités Alapjai" (Genetics and the Foundation of Breeding) (Budapest: Mezőgazdasági Kiadó, 1952), pp. 1–345.
30. T.V. Vinogradova, ed., *A micsurini biológia alapjai* (Principles of Michurinist Biology) (Budapest: Mezőgazdasági Kiadó, 1951), pp. 1–352.
31. Glushchenko's travelog *Countries, Meetings, Sceintist. Notes of a Biologist, Izdatel'stvo Akademii Nauk SSSR*, pp. 1–445 tells in detail of his many trips abroad, also of his repeated visits to Hungary, pp. 274–304. (In Russian). The leading sculptor of Hungary, Zsigmond Kisfaluy-Stroble created Glushchenko's bust (at present at unknow location—information by art historian László Kostyál). A photograph of the bust together with the artist and Glushchenko is in the archives of the Hungarian Press Agency (MTI).
32. Ivan Evdokimovich Glushchenko, "Rövid észrevételek a magyarországi növényélettani kutatás mai állásáról és néhány idevágó javaslat" (Brief Remarks on the Present State of Researh in Plant Physiology and Some Suggestions), MOL 276 folio, 65/331 ö.e.
33. Béla Faludi, "A magyar biológia feladatatai Micsurin és Liszenko tanitása nyomán" (Tasks of Hungarian Biology Based on the Doctrines of Michurin and Lysenko), *Szabad Nép*, June 12, 1949, p. 13.
34. See endnote 32.

35. "Baranov szovjet akadémikus botanikus látogatása Magyarországon" (Visit of Soviet Academician Botanist Baranov in Hungary). Full protocol of the visit in 1951 is at the Archives of the Hungarian Academy of Sciences, Budapest (A Magyar Tudományos Akadémia Levéltára) (hereafter—AL), VIII osztály iratatai, Box 1. folder 5. pp. 1–263.
36. Ivan Feodorovich Lyashchenko, Soviet geneticist gave a series of lectures on Michurinist biology in Hungary in the winter of 1952–1953. His handwritten lectures were translated into Hungarian by one of the authors of this paper (M.M.): I.F. Ljascsenko, *Előadások a micsurini biológia köréből* (Lectures on Michurinist Biology) (Budapest: Akadémiai Kiadó, 1953), pp. 1–251. (No Russian version exists). On Lyashchenko's real views about Michurinist biology see: E.P. Gus'kov, "Small Stories on Big Genetics," *Nauchnaya Mysl' Kavkaza*, no. 3, 1999, pp. 3–13 (In Russian).
37. Gábor Palló, "A magyar természettudomány a háború után" (Hungarian Science After the War), *Világosság*, no. 2–3, 1990, pp. 138–148.
38. Gennadij Semenovich Fish, "The Science of Life and Fighting Corpses (Two Camps in Biological Sciences)," *Znamya*, no. 8, 1949. Also available at GARF, Fond 17, Opis' 137, Delo 487, Listy 87–116. (In Russian).
39. He published an important biology textbook. Huzella Tivadar, *Általános biológia: az orvostudomány alapjai az élettudományban* (General Biology: Foundations of Medicine in Biological Sciences) (Budapest: Orvosi Könyvkiadó Társulat, 1933).
40. Turda, loc.cit.
41. Faludi was engaged in popularization of genetics earlier: Faludi Béla, *Származás és öröklés* (Origin and Inheritance) (Budapest: Népszava, a Szakszervezeti Tanács könyvkiadóvállalata, 1948), pp. 1–36. This brief work presented classical genetics and ended with a chapter on Michurinist biology. An even earlier pamphlet by Faludi, *Az átöröklés* (Inheritance) (Budapest: Radó Könyvkiadó, 1946), pp. 1–15 was entirely on classical genetics.
42. Szaniszlo Priszter, ed., *Az Eötvös Loránd Tudományegyetem Természettudományi Karának Története* (History of Faculty of Science of the Eotvos Lorand University) (Budapest: ELTE, 1991), p. 201.
43. Privatdocent György Kiszely taught the biology course. He described his experiences in 1989. György Kiszely, "Liszenko az orvosképzésben" (Lysenko in Medical Education). Manuscript. Semmelweis Orvostörténeti Muzeum Könyvtára. 89434. We are thankful to László Magyar, head of the Medical History Library, who drew our attention to this document.
44. László Heszky Andor Bálint, "A genetika és növénynemesítés tanszék története. 1920–1994" (History of the Department of Genetics and Plant Breeding). Szent István Egyetem, Mezőgazdaság- és Környezettudományi Kar, Gödöllő, Genetika és Biotechnológiai Intézet. http://mkk.szie.hu/dep/genetika/tanszek_tortenete.htm (Accessed January 5, 2014).
45. The reorganization of plant breeding institutions is reviewed in István Kollega Tarsoly, ed., *Magyarország a XX. században* (Hungary in the

twentieth Century) (Szekszárd: Babits Kiadó, 1996–2000). http://mek.oszk.hu/02100/02185/html/1071.html (Accessed January 3, 2014).

46. See endnote 16.
47. Ivan Evdokimovich Glushchenko, "Notes and Observations on Hungarian Scientific Life" (Moscow: Archive of the Russian Academy of Sciences) (Arkhiv Rossiiskoi Akademii Nauk, hereafter—ARAN), Fond 1751, Opis' 1, Delo 2, Listy 1–8 (In Russian).
48. Győrffy established close contacts with colleagues there. In fact, Hans Stubbe, with whom Győrffy remained the closest, worked in East Germany (German Democratic Republic-DDR) after the War as an intrepid anti-Lysenkoist. Győrffy's German contacts are detailed in: Julia Thiele, "Deutsch-Ungarische Beziehungen in der Pflanzengenetik—ein Fallbespiel," in Holger Fischer (herausg.), *Deutsch-ungarische Beziehungen in Naturwissenschaft und Technik nach dem Zweiten Weltkrieg* (München: Oldenbourg Verlag, 1999), pp. 413–436.
49. Barna Győrffy, "Összefoglalás a 'szerzett tulajdonságok öröklődése és vegetative hibridizációs vizsgálatok' c. akadémiai témára vonatkozó kutatásokról" (Summary of Research on the Programs of the Academy on Inheritance of Acquired Characters and Vegetative Hybridization), January 9, 1953, AL.
50. Andor Bálint, "A morganista genetika kártétele növénynemesítésünkben" (Damage of Morganist Genetics to Our Plant Breeding), *Agrártudomány* 4 (1951), pp. 338–345; Kurt Sedlmayr, "A magyar növénynemesítők magukévá tették a micsurinizmus módszereit" (Hungarian Breeders Turned to Methods of Michurin), *Agrártudomány* 5, no. 3 (1953), pp. 70–71; Kurt Sedlmayr, "A micsurini biológia és a magyar növénynemesités" (Michurinist Biology and Hungarian Plant Breeding), *Agrártudomány* 7, no. 10 (1955), pp. 443–448; András Somos, "A micsurini biológia elterjedése és eredményei hazánkban" (The Spread and Results of Michurinist Biology in our Country), *Agrártudomány* 7, no. 10 (1955), pp. 449–453; János Szigeti, "Micsurin hatása a jelenkori magyar állattenyésztésre" (Michurin's Impact upon Current Hungarian Husbandry), *Agrártudomány* 7, no. 10 (1955), pp. 453–457.
51. *A micsurini biológia alkalmazásásnak eredményei növénynemesitésünkben és vetőmagtermesztésünkben. A Magyar Tudományos Akadémia 1951. évi nagyűláse keretében tartott növénynemesitési kongresszus előadásai* (Results of Application of Michurin Biology in Our Plant Breeding and Production of Seed Material. Lectures at the Congress on Plant Breeding Organized as Part of the 1951 General Asembly of the Hungarian Academy of Sciences) (Budapest: Akadémiai Kiadó, 1952), pp. 1–225.
52. See endnote 50.
53. Ágnes Kenyeres, ed., *Magyar Életrajzi Lexikon* (Hungarian Biographical Lexicon) (Budapest: Akadémiai Kiadó, 1969).
54. See endnote 36.

55. The Biological Section of the Hungarian Academy of Sciences discussed Lyashchenko's observations: "Beszámoló a Biológiai Osztály munkájáról" (Report on the Activity of the Biological Section), AL Elnökségi Iratok, Box 2, folio 2–3, pp. 7–9 and 11.
56. For Törő's biography and evaluation by his close collaborators, see: György Csaba, "Törő Imre (1900–1993)," *Természettudományi Közlöny* 124 (1993), p. 547 and Pál Rölich, "Törő, Imre," *Acta Biologica Hungarica* 44, no. 4 (1993), pp. 317–319. See also the autobiography of György Csaba, *Aesculap a ladikon* (Aesculapius on the Boat) (Budapest: Medicina Könyvkiadó, 2011), pp. 1–215. See also Miklós Müller "A Kossuth prize in 1952 - The short term rule of dialectic Soviet cell biology in Hungary, *Orvostörténeti Közlemények*. 59 (2013), pp. 43–53.
57. H. Nachtsheim, "Biological Phantasies. New Developments in the Case of Lysenko," *Journal of Heredity* 42, no. 3 (1951), pp. 122–123; Anon, *Meeting on the Problem of Living Substance and the Development of Cells* (Moskva: Izdatel'stvo Akademii Nauk SSSR, 1951) (In Russian).
58. Imre Törő, "A sejtszaporodás mechanizmusának új formája" (A New Form of Mechanism of Cell Multiplication), *A Magyar Tudományos Akadémia Biológiai és Agrártudományi Osztály Közleményei* 3 (1952), pp. 47–60. In German: "Über eine neue Form des Zellentstehungsmechanismus," *Acta Morphologica Hungarica* 2 (1952), pp. 363–386.
59. Anon, "Uj Kossuth dijasok nyilatkoznak a kitüntetéseikről és terveikről" (Kossuth Prizewinners Talk About the Decoration and Their Plans), *Szabad Nép*, March 16, 1951, p. 5.
60. We rely on a website that collected biographical data on Hungarian scientists from many encyclopedias: http://www.tudosportal.hu/egy.php?id=4744
61. Bedő Zoltán, "Prof. Sándor Rajki," *Acta Agronomica Hungarica* 55, no. 3 (2007), pp. 393–396.
62. Two major biographies were published on Imre Nagy: Rainer M. János, *Nagy Imre: Politikai életrajz* (Political Biography of Imre Nagy) (Budapest: 1956-os Intézet, 1996–1999) and Méray Tibor, *Nagy Imre élete és halála* (Life and Death of Imre Nagy) (Budapest: Bibliotéka Könyvkiadó, 1989).
63. "Jelentés a Titkárság számára a mezőgazdasági tudományos munka helyzetéről" (Report to the Secretariat on the State of Scientific Research in Agriculture), OL M-KS folio 276, 54–239 ö.e., pp. 205–211. This document also contains the decisions of the Secretariat.
64. Imre Nagy, "Előre az idei termésért!" (Forward for this Year's Harvest!), *Agrártudomány* 5, no. 3 (1953), pp. 65–70.
65. See: Romsics, loc.cit., pp. 374–400.
66. N.V. Turbin, "Darwinism and the New Doctrine on Species," *Botanicheskii Zhurnal* 37, no. 6 (1952), pp. 798–818 and N.D. Ivanov, "Concerning T. D. Lysenko's New Doctrine on Species," *Botanicheskii Zhurnal* 37, no. 6 (1952), pp. 819–842 (both papers in Russian). David Joravsky says that "No

one could be sure in 1952 that the ship was sinking, and most Lysenkoites were not leaving. Turbin was farsighted." David Joravsky, *The Lysenko Affair* (Cambridge, MA: Harvard University Press, 1970), pp. 1–156.

67. Trofim Denisovich Lysenko, "*New Doctrine on the Species*." Many editions in Russian. See also: T.D. Liszenko, "A fajképződés kérdései" (Problems of the Origin of Species) and "A tavaszi és nem telelő fajták átalakitása őszi áttelelő fajtákká" (Transformation of Spring Crops and Not Wintering Crops in Wintering Forms). Translated from T.D. Lysenko, "Tasks of the All-Union Lenin Academy of Agriculture in the development of agriculture in the USSR in fullfillment of the decisions of the XIXth Congress of the Party". AL Biológiai Osztály Iratai, Box 8, folder 3, pp. 3 and 8. Lysenko used Lepeshinskaya's theory on the development of "noncellular living substance" as explanation for this process.
68. "Jegyzőkönyv a Biológiai Osztály 1953 február 13. üléséről" (Protocol of the February 13, 1953 Session of the Biology Division), Biológiai Osztály Iratai, Box 2, folio 7, p. 8. This document also praized the help received from Baranov, Skryabin and Lyashchenko, p. 11.
69. See endnote 63, p. 104.
70. "A faj és fajkeletkezés kérdéséről folyó vita néhány eredménye és további feladatai" (Some Results and Further Tasks of the Discussion on Species and the Formation of Species), *Természet és Társadalom* 114, no. 1 (1956), pp. 15–19.
71. Sándor Tóth, "Filozófiai szempontok a fajkeletkezési vitához" (Philosophical Aspects to the Debate on Species Formation), in *Az Eötvös Loránd Tudományegyetem évkönyve, 1955*, ed. Lajos Tamás (Budapest: Tankönyvkiadó, 1956), pp. 397–398.
72. Soó Rezső, "A faj és a fajkeletkezés kérdésének mai helyzete" (Current Situation of the Problem of Species and Species Formation), *Magyar Tudomány*, no. 4–5 (1956), pp. 173–179.
73. Rezső Soó, "Vita a fajkeletkezésről" (Debate on the Formation of Species), *Természet és Társadalom* 115, no. 9 (1956), pp. 527–529.
74. See endnote 73.
75. "Jegyzőkönyv készült a Biológiai csoport 1956. junius 1-én délelött tartott üléséről" (Protocol of the meeting of the Biology Group in the morning of June 1, 1956), AL Genetikai Intézet Iratai, Box 3, folder 5, pp. 1–42.
76. See endnote 11.
77. The texts of the Petőfi Circle debates were published in 1990s. András B. Hegedűs and János M. Rainer, *A Petőfi kör vitái hiteles jegyzőkönyvek alapján VI. Pedagógusvita* (Authentic Protocols of the Debates in the Petőfi Circle VI) (Budapest: Múzsák—1956-os Intézet, 1992), p. 36.
78. About Lakatos' period in Hungary see Lee Congdon, "Possessed: Imre Lakatos' Road to 1956," *Contemporary European History* 6 (November 1997), pp. 279–294. Jancis Long, "The Unforgiven: Imre Lakatos' Life in Hungary," in *Appraising Lakatos. Mathematics, Methodology, and the Man*,

eds. György Kampis, Ladislav Kvasz, and Michael Stöltzner (Dordrecht: Kluwer, 2002), pp. 263–302.

79. About Lysenko's return and his relationship to Khruschev see the epilogue of William deJong-Lambert, *The Cold War Politics of Genetic Research (Berlin, Springer, 2014)*, pp. 173–182.
80. T. Lysenko, "Theoretical Advances of Agronomic Biology," *Izvestiya*, December 8, 1957, pp. 3–5 (In Russian).
81. Imre Törő, "A genetikai tudomány mai állásáról Liszenko akadémikus cikke alapján" (On the Present State of Genetics Based on the Article by Academician Lysenko), *Magyar Nemzet*, January 28, 1958, pp. 4–5. Translated into Russian for Lysenko: ARAN, Fond 1521, Opis 1, Delo 265, Listy 1–20.
82. Sándor Rajki, "Néhány megjegyzés a genetikai tudomány mai állásáról" (Some Remarks on the Present State of Genetics), *Magyar Nemzet*, May 25, 1958, p. 7.
83. Our description is based on the protocol of the meeting: "Jegyzőkönyv a genetikai bizottságnak a Biológiai Csoport vezetőségének részvételével 1958. október 9-n megtartott üléséről" (Protocol of the October 9, 1958 Meeting of the Committee on Genetics with the Participation of the Executive Committee of the Biology Division), AL Biológiai Osztály Iratai, Box 72, folder 3, pp. 1–140. Rajki's talk alone comprises about 70 typewritten pages.
84. "A Magyar Tudományos Akadémia Genetikai Intézetének Működése, 1959 szeptember 4" (The Work of the Institute of Genetics of the Hungarian Academy of Sciences, September 4, 1959), AL Genetikai Intézet Iratai, Box 3, folder 2.
85. See Michal V. Simunek and Uwe Hossfeld, "Trofim D. Lysenko in Prague 1960: A Historical Note," *Studies in the History of Biology* 5, no. 2 (2013), pp. 84–88.
86. Miklós Müller, "Liszenko emlékezetes előadása a Magyar Tudományos Akadémián, 1960-ban" (A Memorable Presentation by T. D. Lysenko at the Hungarian Academy of Sciences in 1960), *Magyar Tudomány* 172, no. 11 (2011), pp. 1355–1359. For a contemporary laudatory report see: Imre Nóber, "Liszenko—Magyarországon" (Lysenko in Hungary), *Agrártudomány* 12, no. 1 (1960), pp. 1–9.
87. Typewritten transcript of Lysenko's lecture in Budapest: "T. D. Liszenko akadémkus előadása a Magyar Tudományos Akadémián 1960 január 25-én" (Presentation by Academician T. D. Lysenko at the Hungarian Academy of Sciences), AL Elnökségi Iratok, Box 226, folder 4, pp. 1–150. The Russian original: "Marxist-Leninist dialectics and … agrobiological science": ARAN, fond 1521, opis 1, delo 72, listy 1–164.
88. Present at the lecture, Sándor Igali, described his impressions later: "The audience listened uncomfortably to the well-known phraseology. Approximately 200 questions were submitted to the speaker who combined them into a few. When he could not give any meaningful answers, his failure

became obvious. The irony of the fate was that Lysenko himself disillusioned the Hungarian professionals, among them many of his own followers, in Lysenkoism." Sándor Igali, "A liszenkoizmus Magyarországon. Ideologiai-politikai diktatura a XX. század természettudományában." (Lysenkoism in Hungary. Ideological-Political Dictatorship in Natural Sciences in the twentieth Century), *Valóság* 45, no. 3 (2002), pp. 39–59. (in Hungarian)

89. Although the bloodiest period of retaliations lasted for no more than two years, there were executions until 1962. Some normalization and amnesties for political prisoners started in 1962.
90. Romsics Ignác, *Op.cit*., pp. 374–400.
91. Béla Faludi, *Örökléstan* (Genetics) (Budapest: Tankönyvkiadó, 1965), pp. 1–492.
92. A short version of the dissertation was published as a small book: Sándor Rajki, *Automnization and Its Genetic Interpretation* (Budapest: Akadémiai Kiadó, 1957), pp. 1–88. The paper Sándor Rajki, "Differences in Opinions Concerning Autumnization," *Acta Agronomica Academiae Sceintiarum Hungaricae* 18 (1969), pp. 458–462 also presents the different opinions voiced during the defense. This book was severely criticized in the West: Ralph Riley, "More Lysenkoism," *Nature* 217 (1968), pp. 291–292, and also in the Soviet Union: M.G. Agaev, "On the Problem of Transformation of Spring Plants Into Winter Plants and Vice Versa (Certain Considerations in Connection with the Investigations of Sandor Rajki on the 'Autumnization')," *Botanicheskii Zhurnal* 54, no. 9 (1969), pp. 1364–1378 (In Russian with brief abstract in English).
93. Evaluation of Rajki's thesis by Győrffy and Rajki's reply to the opponents, AL Győrffy Iratok, Box 9, folder 5, pp. 1–24 and 1–45. The protocol of the oral defense, AL Tudományos Minősitő Bizottság iratai, Box 80, folio 5, pp. 1–80. The whole protocol amounts to 200 typewritten pages.
94. "Doktori fokozat odaitélése megvédett értekezés alapján" (Doctoral Degree Awarded Based on Defense of a Thesis), AL Tudományos Minősitő Bizottság iratai, Box 80, folio 5.
95. Sándor Rajki, Márta Dévay, and Erna Rajki, *Anyagcsere és öröklékenység, avagy őszesítés mint mikroevolúció-Metabolism and Heredity, or, Autumnization as a Microevolution* (Martonvásár: A Magyar Tudományos Akadémia Mezőgazdasági Kutatóintézete, 1972). (In Hungarian and in English)
96. The long review was published in the discussion section instead of among the reviews. N.P. Dubinin, "Anyagcsere és örökítés, avagy őszesítés mint mikroevolúció" (Metabolism and Heredity, or, Autumnization as a Microevolution), *Biológia* 22 (1974), pp. 189–193.
97. See endnote 13.
98. Loren Graham considers Michurinst biology as pseudoscience in his seminal book, Loren R. Graham, *Science, Philosophy, and Human Behavior in the Soviet Union* (New York: Columbia University Press, 1987), p. 102. See also Michael D. Gordin, "How Lysenkoism Became Pseudoscience: Dobzhansky to Velikovsky," *Journal of the History of Biology* 45 (2012), pp. 443–468.

Lysenko in Bellagio: The Lysenko Controversy and the Struggle for Authority Over Italian Genetics (1948–1956)

Francesco Cassata

Introduction

The Ninth International Genetics Congress met in late August 1953 at Bellagio, on Lake Como. Nearly 900 geneticists from 39 countries attended. The scientific programme consisted of 27 keynote lectures, held in 7 plenary sessions,[1] and more than 300 papers, grouped in 15 sub-sessions.[2] As Richard B. Goldschmidt wrote in January 1954, perhaps no genetics congress had ever met "in a more scenic spot with such pleasant surroundings."[3] Four resorts (Bellagio, Menaggio, Tremezzo, Cadenabbia), scattered on opposite shores of Lake Como, and connected with free ferry services, hosted the guests. The entertainment was lavish: a full day's trip to Lake Lugano, a banquet at Villa Carlotta, a reception with the Duke Gallarati Scotti at Villa Melzi, a ball, a fête at the Grand Hotel Villa Serbelloni in Bellagio, tours of the countryside, and trips to the city of Como and some of the surrounding mountains provided "but too many temptations to enjoy oneself, both for the geneticists participating in the meetings and for their families."[4] In his December 1953 review in *Science*, I. Michael Lerner celebrated the Congress as "a memorable ten days."[5]

F. Cassata (✉)
University of Genoa, Genoa, Italy

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The Bellagio Congress was a defining moment not only for the participants, but also for the organizers, Claudio Barigozzi (1909–1996), Adriano Buzzati-Traverso (1913–1983) and Giuseppe Montalenti (1904–1990). Their work, between 1948 and 1953, coincided with the process of the institutionalization and discipline-building in Italian genetics. In 1944, even before the War had ended, Giuseppe Montalenti obtained the first chair in Italian genetics at Naples. Four years later, Adriano Buzzati-Traverso and Claudio Barigozzi were appointed to the newly created chairs of genetics, in Pavia and Milan respectively.[6]

Before leaving for Berkeley as a visiting professor of zoology in 1951, Buzzati-Traverso trained a group of bright young collaborators, which included Luigi Luca Cavalli-Sforza, Niccolò Visconti di Modrone and Luigi Silvestri in bacterial genetics; Giovanni Magni in radiogenetics and yeast genetics; and Renzo Scossiroli in plant genetics. During this same period in Naples, Montalenti—in collaboration with the Roman haematologists Ezio Silvestroni and Ida Bianco—inaugurated a vast research programme on the genetics of microcythaemia. This attracted the support of the Rockefeller Foundation Division of Medicine and Public Health (DMPH). In September 1953, Robert S. Morison, DMPH Associate Director, described this project as "an opportunity for the DMPH to participate in what could become a classical experiment in the prevention of a hereditary disorder."[7] It was in this context of increasing internationalization that, from 1948 to 1953, the three Italian geneticists became deeply involved in the organization of the Bellagio Congress.

The process of professionalization and internationalization of Italian genetics was significantly shaped by the Cold War, and the Lysenko controversy in particular.[8] Faced with the "Michurinist" campaign organized by the Italian Communist Party,[9] the reaction of academic geneticists was twofold. On the national level, they used the popular press to publicly denounce the political abuses and scientific flaws of "Lysenkoism." In November 1948, on the pages of the liberal-oriented magazine *L'Europeo*, Buzzati-Traverso ironically portrayed Lysenko as the "Soviet Paneroni," a mere "charlatan" and "pseudoscientist."[10] Ten days later, on the politically moderate pages of *Corriere d'informazione*, Montalenti interpreted "Lysenkoism" as a tragic example of the historic conflict between science and politics, on par with the persecution of Giordano Bruno and Galileo Galilei by the Roman Inquisition, and, more recently, the racial and anti-Semitic policies of Nazi Germany and Fascist Italy.[11]

Buzzati-Traverso and Barigozzi were engaged in the Cold War at the international level as well, contributing to the activities of the Congress for Cultural Freedom (CCF), a hegemonic institution designed to incorporate a wide non-Communist spectrum of intellectuals to solidify the US-European alliance.[12] In July 1953, Buzzati-Traverso and Barigozzi participated in the CCF's Hamburg Congress on "Science and Freedom." More importantly, the former was among the promoters of the CCF Committee on Science and Freedom,[13] while the latter organized the 1955 CCF conference in Milan, under the title "The Future of Freedom." Against this background, in this chapter I will explore the role of the Lysenko controversy in the organization of the Ninth International Genetics Congress, in terms of the parallel process of constructing Italian genetics as an academic, internationally recognized field.

At the inauguration of the Bellagio Congress in Villa Serbelloni, on 26 August 1953, the Italian academic geneticists claimed they had tried to contact all countries beyond the Iron Curtain, but "not even an answer"[14] had been obtained: "thus in the present Congress representatives from beyond the Iron Curtain are absent."[15] On the contrary, East German biologists, Hans Stubbe, Friedrich Mechelke and Hans Nachtsheim, had accepted the Italian invitation. But the impact of the Lysenko controversy was not just limited to the absence of attendees from Soviet-allied countries. It also provided an opportunity for Italian geneticists to build their own discipline at the academic level, reinforcing their ties with the international community.

As Nikolai Krementsov has persuasively demonstrated in his account of the Seventh International Genetics Congress, since 1930s "international genetics" (as represented by and at the congresses) became increasingly "academic" ("pure," "basic," "theoretical"), distancing itself from the "applied" research of its "foster parents," namely eugenics and agriculture.[16] The Ninth International Genetics Congress confirms this shift. The Italian academic geneticists used simultaneously the Bellagio Congress and the Lysenko controversy to foster the construction of their professional field, legitimizing themselves as expression of the "pure" and "international" genetics, while stigmatizing—with the adoption of the label "Lysenko"—their domestic rivals: respectively eugenicists, plant breeders and "medical" geneticists.

This chapter is divided into three sections, corresponding to the different fronts which characterized the struggle for authority over Italian genetics. First, I will examine the conflict between the Italian academic geneticists

and the demographer and statistician Corrado Gini (1884–1965). As president of the three most important Italian institutions in the field of population policy and eugenics (the National Institute of Statistics, ISTAT—from 1926 to 1932; the Italian Society of Genetics and Eugenics, SIGE—from 1924 to 1965 and the Italian Committee for the Study of Population, CISP—from 1928 to 1965), Gini was the leading figure in Italian eugenics from 1920s to 1960s and one of the most active eugenicists in the international arena.[17] In the case of the Bellagio Congress, Italian geneticists succeeded in shielding the scientific organization of the International Congress from Gini's influence, disengaging the control of the Congress from SIGE offices and obstructing the presentation of Gini's neo-Lamarckian paper at the Congress.

Second, I will explore how the three academic geneticists in charge of organizing the Congress rejected the proposal elaborated by a group of Italian agricultural researchers for a session dedicated to the topic of "applied genetics" in Italy. On this occasion, the figure of the horticulturalist Alberto Pirovano (1884–1973), with his non-academic career of a self-made plant breeder and his sympathetic reception of "Michurinism" catalysed the attacks of the Italian academic geneticists, eager to use the international arena of the Congress to criticize the lack of a Mendelian agenda in Italian plant breeding.

Finally, I will focus on the conflict between Italian academic geneticists and the zoologist Luisa Gianferrari (1890–1977) and physician Luigi Gedda (1902–2000). Luisa Gianferrari directed the Study Centre of Human Genetics (*Centro Studi di Genetica Umana*), inaugurated in December 1940 at the Institute of Biology and Zoology of the Faculty of Medicine in Milan. Gedda was president of the Italian Catholic Action from 1952 to 1959, and founder, in 1948, of the anti-Communist organization "Citizens' Committees" (*Comitati Civici*). From 1953 he headed the Gregor Mendel Institute of Medical Genetics and Twin Research (*Istituto di Genetica Medica e Gemellologia*) in Rome. Their activity in the field they labelled as "human" or "medical genetics" was characterized by a craft knowledge of human heredity, dominated by the notions of constitution, predisposition and diathesis rather than by statistical computations of Mendelian transmission ratios. Gianferrari's and Gedda's centres had profound institutional and ideological connections with the Vatican and the Italian Catholic milieu.

On each of these fronts, Barigozzi, Buzzati-Traverso, and Montalenti used the label "Lysenko" as a resource to be exploited for stigmatizing their

rivals (eugenicists, plant breeders and "medical" geneticists), undermining their credibility and visibility in the international arena. In this context, Corrado Gini's neo-Lamarckian eugenics, Alberto Pirovano's "electrogenetics" and Luigi Gedda's "Catholic constitutionalism," were targeted as three local variations of "Lysenkoism" to be criticized and rejected. In some cases, this association with "Lysenkoism" was motivated by factual connections: Gini and Pirovano had elaborated analogies between their own scientific contribution in the field of "applied genetics" (respectively, eugenics and plant breeding) and the development of "Michurinism" in the Soviet Union. In contrast, the portrayal of Luigi Gedda as the "Italian Lysenko" was purely metaphorical, and constructed with the precise aim to denigrate Gedda's profile as a geneticist by denouncing his influential domestic patrons, both in the Vatican and in the Christian Democrat government.

The strategy of discipline-building—that the Italian academic geneticists carried out against Gini's racial eugenics, plant breeders, agricultural researchers and Gianferrari's and Gedda's development of "medical genetics"—led to significant institutional fragmentation, redefining the social and political boundaries of the discipline. In 1948, SIGE was the only scientific association dealing with genetics in Italy. In the aftermath of the Bellagio Congress, in 1954, besides SIGE, three new associations included the word "genetics" in their acronyms: the Italian Association of Genetics (AGI), headed by Giuseppe Montalenti; the Italian Association of Medical Genetics (SIGEM), headed by the physiologist Carlo Foà (1880–1971); and the Italian Association of Plant Genetics (SIGA), headed by the Pavia geneticist Carlo Jucci (1897–1962). The Bellagio Congress and the Lysenko controversy, I will argue, served as boundary tools which shaped both discipline-building and the internationalization of Italian genetics.

Corrado Gini and neo-Lamarckian Eugenics

In their anti-Lysenko articles, Italian geneticists—in particular Buzzati-Traverso and Montalenti—frequently elaborated analogies between Fascist eugenics and Soviet Lysenkoism.[18] Between 1948 and 1953, these analogies reinforced the efforts of Italian geneticists to distance themselves, both scientifically and institutionally, from any possible connection with Fascist eugenics. The organization of the Bellagio Congress reflected, in particular, the conflict between Italian geneticists and the demographer and statistician Corrado Gini (1884–1965). President of the SIGE from 1924 to 1965, Gini was the leading figure in Italian eugenics from the 1920s through the

1960s. SIGE membership was dominated by demographers, statisticians, physicians and anthropologists. Despite the official name of the association, the role of genetics was marginal in terms of its research agendas and publications.[19] These were actually dominated by Gini's vision of "regenerative eugenics" (*eugenica rinnovatrice*) based on three major characteristics: the adoption of a Neo-Lamarckian theory of heredity, the rejection of the "Nordic," negative eugenics (sterilization, birth control, etc.) and the implementation of pro-natalist eugenic measures focused on environmental reform and biotypological control.[20]

In September 1938, the young Italian geneticists—Barigozzi, Buzzati-Traverso and Montalenti—participated to the third (and last) congress of SIGE, held in Bologna. Their contributions dealt respectively with cytogenetics, radiation genetics and sex determination, without any reference to eugenics. In contrast, the session on human genetics was comprised of eugenic contributions by Gini and his collaborators (mostly demographers and statisticians). In August 1948, Gini headed the Italian delegation to the Eighth International Congress of Genetics in Stockholm and accepted the invitation to host the next congress in Italy.[21] A few months after the Swedish Congress,[22] Gini immediately began work to re-establish the SIGE which had almost vanished during the War. On 31 December 1948, he sent a letter with five attachments to every SIGE member. The documents announced his intention to reactivate the organization after the wartime interruption in its activities, recognizing the increasing separation of genetics and eugenics. In anticipation of the first post-war assembly of SIGE, to be held on 15 January 1949, the attachments to the president's letter aimed at the rapid resolution of several organizational issues. First, the members were asked to approve a new statute with two new characteristics. The general frame of reference remained that of racial eugenics, as was made clear in Article I:

> The aim of the Italian Society of Genetics and Eugenics (SIGE) is to promote and support the studies, research and initiatives that seek to grow and perfect the knowledge of the laws of heredity and the improvement of the races, with particular attention to the human races.[23]

But, Article 2 sanctioned the establishment of two "special sections" to distinguish the spheres of genetics and eugenics within SIGE. The modified statute also introduced a markedly centralized "presidential" structure, in regard to the positions of leadership and organizational activities.

The statute was accompanied by a questionnaire, conceived as a sort of guideline for an internal referendum on several aspects of general relevance. The questionnaire asked for the approval of the separate two sections on genetics and eugenics (the latter including human genetics); the declaration of membership in one or the other, or both, sections; the nomination for a secretary to each section; and concrete proposals for the organization of the Ninth International Congress of Genetics in Italy (e.g. partners, contributions to expenses, etc.). A voting card followed for the approval of the president (Corrado Gini), vice-president (Ottavio Munerati, director of the sugar beet Experimental Station in Rovigo), and secretary-general (Carlo Jucci, head of the Institute of Zoology in Pavia) and for the nomination of three candidates for the presidency of genetic and eugenics sections separately.

Gini's attempts to reactivate SIGE along the lines of institutional and theoretical continuity with the past, as well as to place the organization of the Bellagio Congress under its control, aroused the opposition of the three main exponents of the Italian academic genetics. In a 1 January 1949 letter, addressed to all SIGE's members, Buzzati-Traverso and Barigozzi were explicit in their criticism. They demanded that the SIGE abandon any reference to eugenics. In their views, the designation of Italy as the seat of the next International Congress of Genetics placed SIGE in a position of responsibility to the international scientific community; and therefore did not allow a simple maintenance of the status quo:

> A very serious responsibility looms over Italian geneticists and the institution that has assumed the role of representing and coordinating them, for the obvious reasons of prestige and to demonstrate the level and dignity that the study and the discipline of genetics have reached even among us. This role must not be underestimated: transactions, compromises and accommodations that might be accepted—for lack of anything better—in our own home, could be severely criticised at an international level, and must therefore be avoided.[24]

According to Buzzati-Traverso and Barigozzi, there were two possibilities. First, the SIGE's structure would need to be transformed from "presidential" to "parliamentary," devolving the organization of the International Congress to the president of the genetics section. The second was to allow the SIGE to maintain its "presidential" character; however, the role of president could not be held by a demographer and statistician such as

Gini, but would be given "to a professional geneticist to represent Italian genetics on an international level."[25] Therefore, just a few days before SIGE's General Assembly, an internal fracture had occurred, as much scientific as it was ideological and political: on the one side, the statisticians and demographers with their past commitment to Fascist eugenics gathered around Gini and the university of Rome; on the other side, Buzzati-Traverso and Barigozzi, representing emerging academic genetics in the universities of Pavia and Milan, wanted to eliminate any reference to Italy's Fascist eugenics past. As Buzzati-Traverso and Barigozzi stated in a letter to their comrade-in-arms Montalenti:

> In its title, the society carries two names of Genetics and Eugenics; this has a historical justification, insofar as the foundation of the society dates back to times in which eugenics was the more widely used term and was appreciated in a way it is not today, while genetics—at least in Italy—had not yet reached the same broad significance with which it is used in various languages. It is highly doubtful that today the two names can be used side by side. It is above all certain that, while the term eugenics is falling into disuse, the term genetics corresponds, with unanimous consensus, to a dominion of experimental and exact research that is identified with the most vital and functional part of current biological thinking.
>
> There is little relevance in conserving a title for traditional reasons, if the structure and the style of the society become shaped by this situation. But, in the communication that we have received, there are several points which lead to the conclusion that new conditions have not been considered in the form planned for the functioning of the re-established society.[26]

A split, which seemed imminent, was avoided through the mediation of Giuseppe Montalenti, whose strategy was built upon maintaining the unity of SIGE under Gini's presidency, giving internal autonomy to the genetics section and wresting the organization of the future International Congress from Gini's control. Although Buzzati-Traverso refused to recognize the legitimacy of the voting, on 15 January 1949 SIGE's General Assembly supported Montalenti's moderate line.

After being elected president of SIGE genetics section, Montalenti successfully promoted the establishment of a Provisory Committee, presided over not by Gini, but by Alessandro Ghigi (1875–1970), professor of zoology at the University of Bologna and the Italian member of the International Organizing Committee for Genetic Congresses since

the 1920s. Ghigi's role was central to the organization of the Bellagio Congress. As former Rector of the University of Bologna between 1930 and 1943, Ghigi could ensure the academic, political and institutional resources that the young geneticists needed for organizing the Congress (in particular, funding from the Ministry of Public Education and the National Research Council). Ghigi was also, like Gini, part of an older generation who had served the Fascist regime. However, unlike Gini and his powerful cohort of demographers and statisticians, Ghigi represented, on the national and international level, the field of the natural sciences.

In November 1951, the Provisory Committee for the organization of the Congress nominated the Executive Committee (*Giunta Esecutiva*). The secretary-general was Barigozzi, vice-secretary was Cavalli-Sforza while Buzzati-Traverso and Montalenti were among the permanent members.[27] The Congress was, therefore, completely under the control of the three geneticists. However, this development only served to delay the break between Gini and the geneticists by a mere few months. Faced with the persistent hostility of Gini, particularly his refusal to recognize the appointment of Buzzati-Traverso as secretary of SIGE genetics section, Montalenti resigned his role as president of the section on 30 March 1950.[28]

Besides the institutional conflict within SIGE, another source of tension came from Gini's scientific contribution to the Bellagio Congress. In October 1952, Gini presented before the Executive Committee a paper on the "physical assimilation of the foreign settlements in Italy." This was based on anthropological and demographic enquiries between the 1930s and 1950s, organized by the CISP, to study Albanian settlements in three isolated villages in Calabria (Carfizzi, San Nicola dall'Alto and Caraffa: August–October 1938) and the Ligurian–Piedmontese settlements in two Sardinian villages (Carloforte, August 1939 and 1952; Calasetta, March–April 1940 and July 1953). CISP investigations had already been the subject of Gini's papers for the Second International Congress of the Latin Eugenics Societies (Bucharest, 1939—abandoned due to the outbreak of War), and at the Seventh and Eighth International Congresses of Genetics, held respectively in Edinburgh in 1939 and Stockholm in 1948.

The aim of the CISP expeditions was to shed light on the mechanisms of the "physical assimilation" of immigrant groups, providing data on the influences of the environment on changes in stature, pigmentation and 84 other anthropometric measures. According to Gini, the final results of the CISP enquiries demonstrated that the almost complete assimilation of Albanians in Calabria and of Ligurian and Piedmontese colonies in Sardinia was due, "at least for a substantial part," not "to the mingling of stocks,

but to the influence, direct or indirect, of the environment."[29] Gini's reference point for the CISP enquiries was Franz Boas' 1911 anthropological research on the changes in the physical characteristics of descendants of various European stocks which had immigrated to the USA.[30] In Gini's view, the "physical assimilation" of immigrants was based on a "neo-Lamarckian" theory of heredity, rejecting Weismann's demarcation between germ and soma, and explaining the transmission of hereditary characteristics in terms of induction, transmission of diatheses and "adaptive mutations."[31]

While Boas' study had intended to challenge American scientific racism, Gini's neo-Lamarckian vision supported an environmentalist form of racial eugenics. During the Fascist regime, the CISP data had provided the theoretical foundation for Gini's celebration of Fascist demographic and racial policies, aimed at the biological unification of different stocks that comprised the Italian nation while prohibiting intermixture with Jews and Africans.[32] In the post-war period, Gini adopted the same theoretical framework in order to reject the egalitarian approach of UNESCO's 1950–51 *Statements on Race*, while claiming a direct parallel between environmental and racial differences. In 1953, Gini published a review of the *Statements on Race* in *Genus*, the journal of CISP. Following his neo-Lamarckian vision, Gini rejected them as a political and ideological manifesto, contesting in particular the articles that pointed at culture, instead of heredity, as the major factor explaining intellectual and psychological differences among human groups. On the contrary, according to Gini, different environmental conditions (both natural and social) led to the biological differentiation of human "races" in physical and mental characteristics.[33]

The relevance of Gini's short review was twofold. On the one hand, it mirrored the scientific rift between Italian demographers and the new generation of academic geneticists over the meaning of race. While Buzzati-Traverso and Montalenti, in particular, advocated Theodosius Dobzhanky's biological notion of race as a Mendelian population, a group of interbreeding individuals,[34] Gini retained his belief in racial hierarchies and negative effects of crossbreeding between Whites and Blacks.[35] On the other hand, Gini's racist anti-egalitarianism was part of a broader strategy against UNESCO's democratic policies in the social and natural sciences. Between 1950 and 1963, as president of the International Institute of Sociology (IIS), the Italian demographer targeted the scientific legitimacy of the International Sociological Association (ISA), founded in 1949 by UNESCO with the aim to reform the social sciences after the end of

the War. In the 1950s and 1960s, the IIS counted among its members a number of social scientists with Nazi and Fascist backgrounds, including Friedrich Burgdörfer, Ilse Schwidetzky, Sabin Manuila, Karl Valentin Müller and Hans Freyer.[36]

Even as he criticized UNESCO's anti-racism, Gini was establishing an interesting connection between Soviet Michurinism and his own neo-Lamarckian theory of heredity. In 1952, he published a short review of the book *Soviet Genetics*, by left-wing British botanist Alan G. Morton, in *Genus*. Based on original Russian literature provided by the Science Section of the Society for Cultural Relations between the Peoples of the Commonwealth and the USSR, Morton's book described the "Michurinist theory" as "a positive attempt" to overcome "the difficulties of the gene concept" and "an important and serious contribution to biological thought"[37]:

Michurinism not only provides a theoretical basis to account for differentiation and development, which Mendelism with its unchanging genes is incapable of doing, but also indicates exactly the kind of experiments which are required to elucidate the complete course of individual development in any particular organism. A whole range of phenomena are opened to experimental attack on fruitful lines. (…) The ability of Michurinism to offer the basis for an epigenetic theory of development is the most powerful proof of its consistency with materialism and of its right to replace Mendelism as the working theory of genetics.[38]

In his review, Gini constructed a parallel between the theory of heredity of the "Soviet genetics," and his own neo-Lamarckian theory of "adaptive mutations." While criticizing Morton's "apologetic tone," Gini addressed the book as an example of how "a dispassionate examination and a rigorous discussion of the facts *could attenuate, if not eliminate, the contrast between the opposite views of Soviet and West geneticists.*"[39] Therefore, Gini's neo-Lamarckian notion of heredity, constituting the theoretical core of his eugenic vision, legitimized both his attack on UNESCO's anti-racism and his sympathetic approach to the "Michurinist" theory. Bearing in mind this connection, the opposition of Italian academic geneticists acquires a broader intellectual and political dimension, targeting at the same time eugenics and Lysenkoism, both on the local and on the international level. Rejecting Gini's paper on the "physical assimilation of immigrants" implied not only a complete dismissal of neo-Lamarckian interpretations of heredity, but also an implicit stance against both Fascist eugenics and Lysenkoism.

Buzzati-Traverso and Barigozzi voted against including Gini's paper in the Congress programme, while Montalenti suggested accepting it in order to prevent further tension. This moderate position once again prevailed, but another weapon remained in the arsenal of the academic geneticists: the schedule of the programme. At the end of July 1953, Claudio Barigozzi astutely set the date of Gini's presentation for August 28, knowing that Gini had to give, that same day, another talk at the annual conference of the International Economic Association (IEA) in Santa Margherita Ligure.[40] As a result, though Gini's paper on "physical assimilation" was published in the Proceedings of the Congress,[41] he did not present it in Bellagio. The plenary session on human genetics was dominated by Giuseppe Montalenti's paper on Rockefeller Foundation (RF)-funded research on the genetics of thalassemia.[42] Lionel S. Penrose,[43] Bentley Glass[44] and Torsten Sjögren[45] were other eminent speakers at the session. The reputation of fledgling Italian genetics on the international stage was secured.

The "Italian Michurin": Alberto Pirovano and Italian "Applied Genetics"

Plant breeding and agricultural research represented another front in the academic geneticists' struggle for control of the Bellagio Congress and the institutionalization of their discipline. Despite the reference to "genetics" in its title, the National Institute of Genetics and Cereal Research (*Istituto nazionale di genetica per la cerealicoltura*), the most important institution in the area of plant breeding, founded in 1919 by Italian agronomist Nazareno Strampelli,[46] did not contribute significantly to the development of a Mendelian agenda in interwar Italian agriculture.[47] During the Fascist "Battle of Wheat" (*Battaglia del Grano*), its main task had been implementing the commercial distribution of Strampelli's varieties through a network of state-funded associations and consortia.[48] The cultural and scientific weaknesses of Mendelism in Italian plant breeding was exacerbated by the institutional distance between the Faculties of Science, where chairs of genetics had been founded in the post-war period, and the Faculties of Agriculture and plant breeding stations of the Ministry of Agriculture and Forestry, where courses (or even notions) of genetics and statistics were often completely lacking.

In the early post-war period, these scientific and institutional conflicts reverberated in the international arena, affecting in particular the relationship between Italian academic geneticists and the Rockefeller

Foundation. In November 1950, for instance, when assistant director of the Rockefeller Foundation Natural Sciences Division, Gerard R. Pomerat, asked Giuseppe Montalenti if there were "men interested (and working) in plant breeding and the genetics of cereals and other crop plants," the reply was negative:

> M. replies that he doesn't know of any and then says that he has several times been asked to give lectures on genetics at the Faculty of Agriculture of the U. [University] of Naples at Portici, but that he has always declined because he cannot find the time. M. adds that the Faculty of Agriculture at Portici is one of the best in Italy but that its standard of research is very low. GRP then explains the RF system of training fellowships and says that if a suitably trained young geneticist or a genetically minded agronomist were to be assured a definite and suitably recompensed post at a place like the Fac. of Agric. [Faculty of Agriculture] at Portici, the RF would be very glad to study the possibility of giving him one or two years of advanced training in crop genetics and breeding.[49]

Pomerat's general impression—"there is no real work on plant breeding done in Italian universities and none of the geneticists now active in university posts seem to be interested in the genetics of economically important plants"[50]—were confirmed by talks with Buzzati-Traverso, that same month. During a lunch conversation in Milan, Buzzati-Traverso severely criticized the activities carried out by the National Institute of Genetics and Cereal Research, headed by the agronomist Ugo De Cillis and by the Corn Breeding Station in Bergamo, headed by Luigi Fenaroli:

> Of the Genetics Station in Rome B.-T. [Buzzati-Traverso] has no very high opinion and he calls De Cellis [De Cillis] "an idiot" who does not really know genetics. He is not much more optimistic about the Maize Culture Institute. Says Fenaroli has no training in genetics but was put in charge of preparing hybrid corn in Italy. Spent six months in the US and is now testing hybrid corn seeds which were sent here, but doesn't believe anything can be gained by crossing the best US hybrids with the better Italian varieties so obviously he's no very promising plant breeder.[51]

During an interview in Turin with the histologist Rodolfo Amprino, Pomerat received further dismissing comments regarding Luigi Fenaroli: "Note for what it is worth: GRP was later told by Amprino that Fenaroli doesn't believe in Morgan genetics!"[52]

In this polarized context, the Lysenko controversy, and in particular the sympathetic attitude of some Italian plant breeders and horticulturalists towards Soviet "agrobiology," deepened the rift between academic geneticists and agricultural researchers, by defining precise boundaries between "pure genetics" and "applied genetics." To illustrate this process of demarcation and its interplay with the organization of the Bellagio Congress, I will focus on the horticulturalist Alberto Pirovano (1884–1973). During the organization of the Congress in January 1953, Claudio Barigozzi received a letter from Alessandro Morettini, director of the Institute of Tree Crops in Florence, suggesting the organization of a special session dedicated to Italian "applied genetics." Writing immediately to Montalenti, Barigozzi clearly expressed his worries: "Morettini thinks to invite Crescini, Mancini, Gasparini, Bonvicini, De Cillis, *Pirovano*, Maliani, *Dionigi*. As you may see, there's an attempt to bring the worst names to the fore."[53] Buzzati-Traverso was harshly ironic about the intent of Italian plant breeders to figure as "applied geneticists" at the Bellagio Congress:

> There is no applied genetics in Italy, and there's nothing to be ashamed of for that. On the contrary, we should be ashamed of having approved a paper by any one of these guys, who knows as much genetics as my granddaughter Lalla: the one who is studying not natural science, but paints. Why not invite my granddaughter Lalla to give a presentation? At least she is quite a pretty girl. It seems to me that it would be better to have no contributions on this topic, rather than to have a presentation of that kind. The total absence could be justified as a surplus of hospitality and modesty, but a *pirovaneggiamento* would give us a label very difficult to eliminate.[54]

Two points of this quotation are notable. First, Buzzati-Traverso's stigmatization of the so-called applied genetics addressed not only horticulturalists, agronomists and plant breeders. It also implicitly targeted the zoologist Carlo Jucci, a pioneer of Italian genetics during the 1930s.[55] As a Rockefeller fellow in 1927, Jucci had supported the initial steps of Buzzati-Traverso's career in Pavia. In the early post-war years, particularly after the appointment of Buzzati-Traverso as full professor in Pavia in 1948, their personal and academic relationship worsened rapidly. The tension materialized in institutional opposition: Buzzati-Traverso was head of the Institute of Genetics, while Jucci established a Centre of Genetics within the Institute of Zoology, with funding from the Italian National Research Council (CNR). Research agendas as well began to diverge:

while Buzzati was increasingly engaged in radiation genetics and in genetics of natural populations of *Drosophila*, Jucci progressively moved towards the field of "applied" genetics. In 1946, he founded *Genetica agraria* (Plant Genetics), a journal of "genetics *applied* to agriculture," whose editorial board did not include the three Italian academic geneticists. In April 1950, Jucci, who was already SIGE's secretary-general, refused to join the SIGE's section of genetics, while accepting the presidency over a new SIGE section dedicated to "*applied* genetics," comprising SIGE plant breeders and agronomists.[56] Furthermore, in May 1952, Jucci, initially a member of the Executive Committee of the Congress, gave his resignation to protest against the choice of Bellagio—instead of Pavia—as the main site of the Congress.[57]

Another notable quote is the word *pirovaneggiamento.* The first half of the term was a reference to the horticulturalist Alberto Pirovano. The second part, *vaneggiamento*, means "delirium." Thus, *piro [Pirovano]—vaneggiamento* meant *Pirovano's delirium.* Born in Vaprio D'Adda (near Milan) in 1884, Pirovano took up the family business of horticulture, while studying botany and physics as autodidact. In the early twentieth century, Pirovano, together with his father Luigi, introduced over 500 cultivars of table grapes in Italy, including the well-known Italia grape (*uva Italia*), a white variety obtained in 1911 by crossing Bicane and Muscat Hamburg.[58]

In 1922, Alberto Pirovano published a 300-page volume, with more than 100 figures and plates, titled *The Electric Mutation of Botanical Species.*[59] By combining elements from Mendelism, Lamarckism, Weissmanism and Devriesian mutationism, Pirovano focused on the action of electromagnetic fields applied to pollen as a source for the induction of mutations and the "discipline of heredity in hybridization." According to Pirovano, the stability of the species could only be explained by the stability in the molecular structure of the germplasm. A direct action could thus bring disorder (and variation) in the atomic composition of the plasm. This action, which he called *jonolisi*, consisted in the application of a variable electromagnetic field directly to the pollen. The *jonolisi* was supposed to shake the stable arrangement of atoms, if the result was one of the few life-compatible, to give birth to a mutated plant.

Despite Pirovano's bombastic claims, the experimental part of his book was highly problematic. First, Pirovano combined different kinds of electrical treatment—radioactive materials, X-rays, UV rays, electrolytic action, magnetism and high-tension electric current—without specifying precisely

the alleged mutagen agent. The main portion of the experimental data was concerned with the application of an electromagnetic field to pollen. Pirovano recorded about 150 trials of this sort. Although the number was indicative, there was no attempt at a systematic control of the field strength, duration of treatment or cycle. In the first 19 cases, for example, the same plant was used throughout, but ten different field strengths, nine different periods of exposure and three different cycles were applied. As a result, there was no adequate basis for comparing one experiment with another. Pirovano's data and illustrations were therefore misleading since they failed to indicate the true range of variability associated with the use of normal pollen. Furthermore, the types of variation obtained—and attributed to the influence of experimental treatments—were of the sort ordinarily associated with hybridization, and there were no data on the changes in the frequency of these variations under experimental conditions.

Despite the limited results of his experiments, Pirovano did not hesitate to exalt electrogenetics as a revolutionary source of varietal innovation and horticultural improvement through the creation of new varieties. The extensive adoption of and experimentation with *jonolisi* could guarantee, in Pirovano's vision, the complete control over hybrid behaviour and the induction of stable mutations. By claiming the possibility, through the adoption of the *jonolisi* method, of converting a recessive trait into a dominant one, Pirovano exalted the potential revolutionary impact of electrogenetics in order "to completely subordinate the laws of heredity to the human will."[60]

Pirovano's electrogenetics was severely criticized by scientists. In 1925, for instance, the *Journal of Heredity* published a critical review of Pirovano's book by the US botanist Lewis H. Flint (Bureau of Plant Industry, Washington, DC), wherein he expressed serious doubts about the reliability of Pirovano's data:

> These methods are homogeneous in that they all involve the electrical treatment of pollen previous to pollination, yet they offer a lamentable diversity in the particular nature of the electrical treatment, since use is made of radioactive materials, x-rays, ultra-violet rays, electrolytic action, magnetism and high tension electric current, with diverse variations and combinations.[61]

Flint hoped that Pirovano's laboratory could produce more trustworthy data by following "a less pretentious and more carefully controlled program."[62]

Despite this criticism, Pirovano's electrogenetics gained the support of the Fascist regime, where his voluntaristic, vitalist approach was perfectly suited to the official ideology of economic and cultural autarky. In 1924, Pirovano became chief of the Laboratory of Electrogenetics in Belgirate, on the Lake Maggiore, and in 1927 he moved to Rome as the first director of the newly established Institute for Fruit Growing and Electrogenetics (*Istituto di frutticultura e di elettrogenetica*). In the early post-war period, the Institute continued to carry out its experiments in electrogenetics. In the name of the "plasticity of the species,"[63] Pirovano moved from the rhetoric of Fascist autarky to the celebration of Soviet "Michurinism," despite obvious political and ideological contradictions.

In 1948, Pirovano revised Mania Gordin's French translation of Michurin's "Selected Works" (*Oeuvres choisies*)[64] and gained the reputation of "Italian Michurin" in Communist and Socialist circles.[65] In 1949, in a long article published in the journal *Annali della sperimentazione agraria*, Pirovano contested the "revolutionary" character of Michurinism, considering Lysenko's theories as the latest expression of a long historical tradition of "transformism" in the life sciences, including his own electrogenetics as well.[66] In January 1952, Pirovano joined the Commission on Agrobiology, established by the Cultural Commission of the Association Italy-Soviet Union.[67] The Commission published the bulletin *Studi di Agrobiologia*, which aimed at popularizing the techniques of the "Lysenko-Michurin agrobiology" among Italian horticulturalists and plant breeders. The Italian Commission of Agrobiology followed the model of the *Association Française des Amis de Mitchourine* (AFAM), established in 1950 and headed by Ernest Kahane, professor at the Ecole Nationale d'Agriculture in Grignon, and Claude-Charles Mathon, *attaché de recherches* at the Centre National de la Recherche Scientifique (CNRS) in Paris.[68] In March 1952, an Italian delegation was sent to the First National Congress of the AFAM, publishing in *Studi di agrobiologia* extensive descriptions of the activities of the French association,[69] as well as essays by Mathon on "some aspects of Michurinism."[70]

Pirovano's "electrogenetics" and his sympathetic attitude towards Soviet Michurinism underpinned the decision of Italian academic geneticists to reject the participation of Italian horticulturalists and plant breeders to the Bellagio Congress. Pirovano's involvement in the Commission on Agrobiology and his reputation as the "Italian Michurin" provided the academic geneticists with a boundary tool which aimed at reconfiguring post-war Italian genetics not only as an internally coherent disciplinary field, but also as a scientifically and politically credible member

of the international field of genetics. Not surprisingly, therefore, Italian representatives were completely absent from the plenary session dedicated to "applied genetics." The session included lectures by Jay L. Lush, Otto H. Frankel, Friedrich Gustav Brieger and Sidney C. Harland,[71] essentially dealing more with "theoretical" issues of evolutionary and population genetics than with "applied" matters.

Luigi Gedda, the "Catholic Lysenko"

The third front established by Italian academic geneticists during the organization of the Bellagio Congress was against the Centres of Human Genetics, headed by Luisa Gianferrari in Milan and Luigi Gedda in Rome. The conflict developed on three different levels: scientific, political and academic. On the one side, with the concrete political support of the Christian Democrats and the Vatican, Gianferrari and Gedda aimed to establish their own kind of "medical genetics," basically rooted in constitutional medicine and twin research, within the Faculties of Medicine. On the other side, in their effort to control the institutional development of the entire field of genetics in Italy, the three academic geneticists geared the organization of the Bellagio Congress towards ostracizing both Gianferrari and Gedda, while building and reinforcing their own disciplinary field—that of "genetics without adjectives," to quote Adriano Buzzati-Traverso. The adoption of the label "Lysenko"—especially with regard to Luigi Gedda, a key political figure of the Catholic right in post-war Italy—has to be understood in the context of this heated battle for hegemony over Italian genetics.

The Milan Study Centre of Human Genetics (*Centro Studi di Genetica Umana*) was inaugurated in December 1940, at the Institute of Biology and Zoology of the Faculty of Medicine. Supported by the majority of local political and academic authorities, the Centre was financed by both private and public institutions. The purpose of the Centre, according to article 1 of its statute, was to "collect data on the physiological and pathological human traits, with the aim of carrying out genetic studies, also with a focus on health and demographic problems."[72]

The first important initiative of the Milan Centre was the elaboration of a national genetic index of the transmission of hereditary traits. The index drew upon the archives of hospitals, surgeries, special schools, mental hospitals and many other institutes. Data collection was entrusted to volunteers recruited from among the students of the Faculty of Biology

and Surgery. Each volunteer was provided with data sheets by the Centre. In 1944, Gianferrari referred to 510 "student field–researchers."[73] By this time, the Centre had gathered almost 100,000 index cards by sifting through a number of relevant clinics, hospitals and institutes in Milan and Lombardy. Their data included over 1000 twins and approximately 1000 documented clinical pedigrees.

The general index, which identified "the branches that could be useful to study from the point of view of the hereditary transmission of several traits,"[74] formed the basis for the principal studies of the Centre during the 1940s. In its first research project, concerned with the identification and localization of "defective branches" in different zones of Lombardy, the field researchers discovered "original foci of various pathological hereditary forms," in particular dental malformations, tumours, schizophrenia and manic depression.[75] The second line of research dealt with the hereditary transmission of "talents," starting with "pictorial" ability.[76]

Between 1940 and 1945, the research activities of Gianferrari's centre were perfectly in line with the demographic, eugenic and racial policies of Fascist Italy, stressing in particular the dysgenic effects of consanguineous marriages in isolated Alpine rural settlements.[77] After the collapse of the Fascist regime, the Milan centre abandoned the project of racial improvement, shifting its attention to "eugenic counselling" for couples. In 1946, Gianferrari's centre provided the first such service in Italy. In 1948, another "municipal eugenic counselling" was founded at the Milan Policlinic, again entrusted to Gianferrari's centre.[78] The activities of the two consultancy centres were principally concerned with premarital counselling for prospective parents and counselling for maternal–foetal haematic group or blood group incompatibility.

The focus was on mental and nervous diseases, malformations, eye diseases and haemopathy (haemophilia). Methodologically, it was based on the construction of medical pedigrees through a combination of biographic narratives and anatomical and pathological observations. Medical pedigrees were used to construct the family as a collective patient, wherein a single pathological entity could be compared among a set of cases. In Gianferrari's vision, "eugenic counselling" was conceived as part of a more comprehensive system of preventive measures, which also included "direct action on environmental conditions."[79] This environmental component focused in particular on the dimensions of mother–infant relations (including housing and feeding) and education. At the Fourth International Congress of Catholic Physicians, held in Rome in September and October

1949, Gianferrari condemned any form of compulsory eugenic intervention and proposed that municipalities distribute a "sanitary booklet to inform those affected by morbid hereditary conditions or who come from defective pedigrees of the serious responsibility toward the offspring that marriage carries with it."[80]

For the Bellagio Congress, Gianferrari proposed a paper on the "genetics of leukaemia." This paper, which did not contain any reference to genes or mutations, was the result of a wide genealogical and statistical survey of 278 patients admitted to hospitals in Lombardy between 1945 and 1950. The collection of family cases did not provide any evidence concerning the pathological inheritance of leukaemia.[81] Gianferrari's presentation was not well-received and geneticists began manoeuvring against her during the pre-Congress discussion of the scientific programme. At the meeting of the Executive Committee, on 18 February 1953, Buzzati-Traverso voted against including Gianferrari's paper in the programme, while Montalenti suggested that Gianferrari present a paper on a different topic, such as the distribution of blood group frequencies in Italy. As in the case of Gini, Montalenti's moderate attitude finally prevailed, and Gianferrari's contribution was accepted. It was nevertheless relegated to a special session of "Human genetics," while the plenary session on human genetics included, as we have seen, Montalenti's account of the research on thalassemia, funded by the Rockefeller Foundation.

Political and academic opposition between "medical geneticists" and academic geneticists further aggravated the situation. In June 1953, just a couple of months before the Bellagio Congress, a controversy emerged with regard to the examination committee for the first Italian *libera docenza* in human genetics. In the 1950s, the *libera docenza* was an academic title for a university teaching qualification, and it was essential for the admission to the position of professor. At that time the Italian legislative procedure envisaged that the Minister of Public Education would consult the First Section of the High Council (*Sezione I del Consiglio Superiore*) of Public Education regarding the composition of the examination committee, but could also modify the final decision of the High Council. In the case of the *libera docenza* in human genetics, the High Council proposed the following names: as permanent members, Barigozzi (professor of genetics in Milan), Montalenti (professor of genetics in Naples) and Alfonso Giordano (professor of anatomy and pathological histology in Pavia); as substitute members, Buzzati-Traverso (professor of genetics in Pavia) and Umberto D'Ancona (professor of zoology in Padua), thus making the

examination committee a stronghold of academic geneticists who could control new appointments in the field of human genetics. Without taking this recommendation into account, on 15 June 1953 the Minister of Public Education, the Christian Democrat Antonio Segni, politically tied with Luigi Gedda, proposed a radical alternative: the three professors of genetics disappeared from the committee, and in their places, Segni nominated as permanent members Luigi Gedda, Luisa Gianferrari and Giovanni Di Guglielmo (professor of general clinical medicine and medical therapy in Rome); and as substitute members Alfonso Giordano and Giovanni Dall'Acqua (professor of medical pathology and clinical methodology in Bari).[82] The examination committee was therefore dominated by the most important exponents of the Catholic-oriented "medical genetics" (Gedda and Gianferrari).

Having petitioned in vain for the Ministry to reconsider its choice, Barigozzi, Buzzati-Traverso and Montalenti launched a frontal attack: the first two appealed, on 27 August, to the State Council (*Consiglio di Stato*),[83] while the third, in December, denounced Segni's decision directly to the President of the Italian Republic.[84] The three geneticists questioned the legitimacy of the ministerial decision: the Ministry had not only ignored the recommendation of the High Council, but also failed to give any justification for its interference. Finally, considering all sides of the controversy, the sixth session of the State Council, at its jurisdictional session on 7 April 1954, decided in favour of the geneticists and annulled Segni's decree of June 1953.[85]

The third aspect of this struggle between geneticists and physicians pertained to the institutional organization of the discipline. In 1951, Luigi Gedda inaugurated a new scientific association: the Italian Society of Medical Genetics (*Società italiana di Genetica Medica*), headed by Carlo Foà, professor of physiology at the University of Milan.[86] In January 1952, the first issue of the international quarterly *Acta Geneticae Medicae et Gemellologiae*, organ of the Italian Society of Medical Genetics, was published with Gedda as editor-in-chief. In the article opening the journal's first issue, Gedda described his journal's approach to genetics along the lines of a renewed holistic constitutionalism.[87] According to Gedda, the three different schools of constitutional medicine—morphological, functional and neuro-endocrinal—had tried to resolve the dichotomies between "Virchowian localism" and "Pasteurian esogenism," but with only limited success. Only genetics could allow a synthesis between "synchronic" (form and function in action) and "diachronic" (individual

anamnesis) studies of the phenotype and analyses of "family stock."[88] In Gedda's opinion, medicine had reached a "turning point" because, due to the decisive contribution of genetics, the focus of scientific and professional interest was shifting "from the recognition of the imprint of illness on the phenotype and from the knowledge of esogenic moments of illness," to the "endogenic moments, that is, to constitution."

Against this background, on 6 and 7 September 1953, just a week after the Bellagio Congress, the Italian Society of Medical Genetics held the First International Symposium of Medical Genetics (*Primum Symposium Internationale Geneticae Medicae*) in Rome, under the patronage of Pius XII. The Symposium deliberately coincided with the inauguration of the "Gregor Mendel Institute of Medical Genetics and Twin Research," in Rome, with headquarters in Piazza Galeno, directed by Gedda. The level of conflict between the Italian academic geneticists and Gedda's group is reflected in these few indignant lines Buzzati-Traverso wrote to Montalenti:

> And what do you think of those S.O.B.s (if you don't know what it means, ask the nearest American) Gedda and Gianferrari, who are putting together a symposium on medical genetics, without saying even one word to the organisers of the congress? With this, they also make us look stupid, in the eyes of those who would have been invited, who will conclude that usually in Italy, we gently lead each other to the gallows.[89]

At the inaugural ceremony of the Mendel Institute, Carlo Foà attacked the so-called pure geneticists directly, reasserting the right of medicine to engage in human genetics.[90] According to Foà, only medicine could provide geneticists with that "verification of the most subtle clinical symptoms," necessary for the investigation of human heredity.[91] In his inauguration speech as president of the Mendel Institute, Gedda stated his vision of the problems of human heredity, emphasizing in particular the connection between medical genetics and constitutionalism.[92] In his address to the participants of the Symposium, held at the papal summer residence of Castel Gandolfo on 8 September, and published the day after (in the original French) in *L'Osservatore Romano*, Pope Pius XII confirmed Gedda's programme. Pius XII, on the one hand, gave a summary of the basic and well-established facts and concepts of genetics, namely cell theory, fertilization, Mendel's laws, gene theory and mutations. On the other hand, with regards to the connections between heredity and evolution, the Pope considered evolution to be an unverified hypothesis.

In the final part of his discourse, Pius XII dealt with the issue of eugenics. According to his view, "the fundamental tendency of genetics and eugenics" was "to influence the transmission of hereditary factors in order to promote what is good and eliminate what is injurious." This, he argued, was "irreproachable from the moral viewpoint." Yet Pius XII strongly condemned "certain defensive measures in genetics and eugenics."[93] Sterilization, the "interdiction of marriage," the segregation of defectives and therapeutic abortion were all rejected in the name of respect for the dignity of the human person, according to Catholic teachings.[94]

The reply of the "pure" geneticists to Gedda's Symposium was swift. Following Theodosius Dobzhansky, who had criticized Pius XII's anti-evolutionism in *Science*,[95] Buzzati-Traverso published a vitriolic review of the Symposium proceedings, also in *Science*, denouncing the international isolation of the initiative:

> There are two types of international scientific meetings: those that are "international" by virtue of their being sponsored and organized by an internationally recognized agency, and those that receive this qualification from the person who decides to invite to his laboratory a number of colleagues from various countries. The *Primum Symposium Internationale Geneticae Medicae* belongs to this second type.[96]

Summarizing the scientific content of the Symposium, Buzzati-Traverso only referred positively to Ceppellini's paper on the genetics of aminoaciduria and to Franceschetti and Klein's paper on the screening of heterozygotes. He neglected to mention other contributions of the Symposium, such as those by Othmar von Verschuer, Hans Grebe and Wolfgang Lehmann, all geneticists and racial biologists deeply involved in Nazi medical research and rehabilitated after the War. Buzzati-Traverso criticized Gedda and Foà as well, suggesting that they at least learn the correct use of genetic terminology:

> Of course, no geneticist would deny the right of medical doctors to devote themselves to human genetics, and, in fact, important contributions to the science of heredity have been made by professional physicians. They, however, should at least take the trouble to learn the proper use of terms, in order not to confuse, for example, inbreeding with backcrossing (p. 13) or chromosome markers with closely linked genes (p. 448).[97]

Following the strategy of de-legitimatizing Gedda in the international context, vis-à-vis their patrons in particular, Italian academic geneticists did not hesitate to label him as the "Italian Lysenko" in their talks with Rockefeller Foundation officers. From this point of view, the diary of John Z. Maier, assistant director of the Biological and Medical Research Division of the Rockefeller Foundation, is revealing. After a meeting in Rome with Montalenti, in January 1954, to discuss a major grant in favour of his project on the genetics of thalassemia, Maier reported extensively on Montalenti's campaign against Gedda:

> On the way into town, M. detoured to show me a most interesting building at the Piazza Galeno on Viale Regina Margherita—a magnificent marble and bronze structure which has just recently been completed and, M. says, is as palatial within as without, and which houses a new Institute for Human Genetics, a dangling and free-floating sort of body which has no connection with the University or with anything else. It is directed by, and due to the efforts of, a Dr. Gedda, a practising physician in Rome with no teaching position, no research or scientific experience and no formal training in genetics. (…) *He would therefore appear to be the Lysenko of Italy*, and is of course causing all kinds of trouble for the classical Italian geneticists such as Montalenti and Buzzati-Traverso. It was he who organized the Rome symposium on medical genetics referred to above, as a result of a fit of pique because of what he considered to be the minor role allotted to him at the International Genetical Congress in Italy in the same month. Many of the delegates at the congress, all unwittingly, accepted his invitation to the symposium in Rome and were received by the Pope in formal audience. Ironically, his institute is named after Gregor Mendel.[98]

In November 1955, talking with Pomerat, Montalenti harshly criticized Gedda's journal:

> In Gedda's journal *Acta Geneticae medicae et Gemellologiae* for September 1955, there appeared an article on linkage between Rh and microcythemia authored by Dr. Nicoletta Vulpis of Chini's lab, which M describes as *absolute rubbish from a genetical standpoint and something he would have been ashamed to sponsor*.[99]

How was it possible to label Luigi Gedda, one of the most radical anti-communists in Italy, the "Italian Lysenko?" In the strategy of discipline-building elaborated by Italian academic geneticists, the reference to Lysenko was a negative category. The stigmatization in this case was moti-

vated by two arguments: first, Gedda's medical constitutionalism could not be considered genetics; second, Gedda's academic career was based not on scientific achievements, but on the political support from the Vatican and the Christian Democratic party. In the eyes of Italian academic geneticists, the correlation between craft knowledge and strong political backing was sufficient to turn Gedda into a local version of Lysenko.

Conclusions

The Bellagio Congress was a fundamental moment in the response to the Lysenko controversy in Italy. On the national scene, the Italian geneticists consolidated the disciplinary autonomy of their discipline by constructing intellectual, institutional and political boundaries that marginalized their competitors: SIGE eugenicists, plant breeders and "human"/"medical" geneticists affiliated with the centres of human genetics and the faculties of medicine. The implicit or explicit use of "Lysenko" by the Italian academic geneticists could be understood as a boundary tool in this struggle for disciplinary control. Between 1948 and 1956, Corrado Gini's "adaptive mutations," Alberto Pirovano's "electrogenetics" and Gedda's "Catholic constitutionalism" were all successfully portrayed as local variations of "Lysenkoism."

The Italian academic geneticists targeted both eugenics and "Lysenkoism" as similar forms of political interference in science. Furthermore, the neo-Lamarckian ("Latin") framework of Fascist eugenics, which the demographer Corrado Gini elaborated from 1920s to 1960s, contributed to reinforce the analogy with "Michurinist" biology. With regard to Alberto Pirovano, his reputation as the "Italian Michurin" was clearly a stigmatizing tool in the hands of the Italian academic geneticists: as Lysenko, Pirovano was considered a non-academic plant breeder without any proper training in genetics, and his "electrogenetics" was dismissed as pure "delirium." As for Gedda, the reference to Lysenko was completely metaphorical, stigmatizing in particular the intervention of the Italian government and the Vatican on behalf of a Catholic-oriented "medical genetics."

At the international level, the organization of the Bellagio Congress, coupled with the attacks on "local Lysenkos," enabled Italian academic geneticists to reinforce their connections with the Anglo-American scientific elites, legitimizing themselves as members of a transatlantic scientific community that not only shared a common view of "modern" genetics,

with its disciplinary consensus regarding research tools, methods, agendas and so on, but also a common liberal democratic interpretation of science and its values.

This process of discipline-building took three different forms. First, the elaboration of the Congress programme mirrored the conflict between the Italian academic genetics and other forms of "applied" genetic research, both in human genetics/eugenics and in agriculture. From among the Italian participants, only Buzzati-Traverso, Barigozzi and Montalenti presented their contributions at the plenary sessions of the Congress. The inclusion of Guido Pontecorvo, from the University of Glasgow, and Renato Dulbecco, from Caltech,[100] in these same sessions symbolically bridged the gap between the three Italian academic geneticists and the researchers forced to emigrate respectively to the UK and USA, for political reasons—as in the case of Pontecorvo, a refugee from Fascist Italy in the 1930s—or the lack of research opportunities—as in the case of Dulbecco who, after the War, first joined Salvador Luria and then Max Delbrück.

Second, the Bellagio Congress contributed to the formation of a liberal democratic network, culminating in the constitution, under the auspices of the CCF, of the *Committee on Science and Freedom*, a permanent organization with headquarters in Paris, established in July 1954 with the aim to carry on the exchange of ideas initiated at the CCF Hamburg Congress on "Science and Freedom" in July 1953.[101] Both Buzzati and Barigozzi participated to the Hamburg Congress, just a month before the Bellagio Congress. On this occasion, Buzzati pointed out the need to organize a more aggressive campaign against Lysenkoism, by setting up "a small group of active persons."[102] The Bellagio Congress not only gave the chance to discuss this proposal, but also facilitated concrete steps towards its implementation. During its closing business session, the Ninth International Congress of Genetics approved the following resolution: "The Congress asks the Permanent International Committee not to recommend that the next Congress be held in any country to which it may be expected that scientists would be refused permission to enter on grounds of race, nationality, religion, place of birth or political associations past or present."[103] The Bellagio Resolution, which clearly targeted the introduction of the US McCarran Act in 1952, reflected the challenge of the "Lysenko controversy" on the evaluation concerning the role and the position of science in democratic societies, both in Europe and the USA.[104]

On September 2, just a few days after the end of the Bellagio Congress, Buzzati-Traverso wrote to the Russian-born composer and CCF Secretary General Nikolai Nabokov suggesting that the Bellagio resolution be linked to the organization of the planned "Committee on Science and Freedom":

> It seems to me, and it is generally agreed among geneticists, that it would be very important to give very wide circulation on the press in every country to the above resolution, accompanied by explanations. At the Congress there were 873 participants, from 35 different countries. There were no representatives of countries behind the Iron Curtain, with the only exception of five geneticists from East Germany. It seems to me that it would be very much worthwhile if you would make an effort to make this known not only to the great press agencies but to small local papers in the various countries as well. If we had already the planned committee on "Science and Freedom" we could use it for this purpose![105]

It still took more than a year to organize the Committee, but the Bellagio Congress prompted a series of informal meetings between Nabokov and the Italian geneticists, namely Buzzati-Traverso and Barigozzi, which led to the CCF Conference on "The Future of Freedom," held in Milan in September 1955. As in Bellagio, Barigozzi was in charge of the organization of the CCF conference.[106]

Finally, the Bellagio Congress set an important precedent in proving the competence of the Italian academic geneticists in the organization of large-scale international congresses. The Second International Congress of Photobiology, held in Turin in 1957, offers a remarkable example of this important turning point. In fall 1956, Alexander Hollaender, head of the Biology Division of the United States Atomic Energy Commission (USAEC) Oak Ridge Laboratory and president of the Photobiology International Committee, became increasingly concerned with the proper organization of this Congress: the Turin radiologists in charge of it not only lacked competence in radiobiology, but were also completely unable to organize the Congress at an international level.[107]

Thanks to the mediation of the physicist Ugo Fano, based in Washington at the US National Bureau of Standards, Hollaender decided to set up an organizing committee to assist the Turin radiologists. Significantly, the new *ad hoc* committee included Buzzati-Traverso, Montalenti and Barigozzi—who was also appointed vice-president of the Congress. The initial contacts between Fano and Barigozzi present a telling example of the international impact of the 1953 Bellagio Congress. In October 1956,

Fano wrote to Hollaender: "I met today the local geneticist—Barigozzi, the man *who organized the Bellagio congress*. I don't know whether he is good as he sounds but he sounds quite good and eager to help. (…) He speaks *the same language as we do*."[108]

Four years had passed, but the memory of Bellagio was still alive and demonstrated that Italian genetics shared the common "language" of the Atlantic community, scientifically as well as politically.

Notes

1. Bases of heredity; Genetic mechanisms and mutations; Cytological mechanisms; Developmental mechanisms; Evolutionary mechanisms; Human genetics; and Applied genetics. For the scientific program, see G. Montalenti and A. Chiarugi, eds., *Atti del IX Congresso internazionale di genetica. Bellagio (Como), 24–31 agosto 1953* (Florence: Florentiae, 1954), Vol. 1, pp. 62–82 (hereafter, *Atti del IX Congresso internazionale di genetica. Bellagio*).
2. Evolution, Cytology, Human Genetics, Mutations, Animal genetics, Plant genetics, Applied genetics, Microbial genetics, Quantitative inheritance, Blood groups, Cell physiology, Phaenogenetics, Disease resistance genetics, Sex, Biochemical genetics.
3. R.J. Goldschmidt, "IX International Genetics Congress," *AIBS Bulletin* 4, no. 1 (January 1954), p. 26.
4. I.M. Lerner, "The Ninth International Congress of Genetics," *Science* 118, no. 3076 (December 1953), p. 709.
5. *Ibid*.
6. Formal and developmental genetics of pseudotumours in *Drosophila* and cancer cytology were the main research focus of Barigozzi's group in Milan.
7. R.S. Morison officer's diary, "Rome, Italy, September 23–25, 1953," p. 109, Record Group (RG) 12.1, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York (hereafter RAC).
8. W. deJong-Lambert and N. Krementsov, "On Labels and Issues. The Lysenko Controversy and the Cold War," *Journal of the History of Biology* 45, no. 3 (2012), pp. 373–388.
9. F. Cassata, "The Italian Communist Party and the 'Lysenko Affair' (1948–1955)," *Journal of the History of Biology* 45, no. 3 (2012), pp. 469–498. On the Lysenko Controversy in Italy, see F. Cassata, *Le due scienze. Il "caso Lysenko in ItaliaE"* (Turin: Bollati Boringhieri, 2008).
10. A. Buzzati-Traverso, "In Russia si nasce secondo la legge di Lyssenko," *L'Europeo* IV, 46, November 8–14, 1948, p. 9. Born in Rudiano, Giovanni

Paneroni (1871–1950) was an ice-cream maker and a popular "astronomer." During the 1920s and 1930s, he became famous for his Pataphysic theories and his Tolemaic campaign against the Italian "Galilean professors." His motto was: "Beasts, the Earth doesn't revolve around the Sun!" (*La terra non gira, o bestie!*).

11. G. Montalenti, "Scienza e Libertà: il caso Lysenko," *Corriere d'informazione* IV, 275, November 23–24, 1948, p. 3.
12. On the history of the CCF, see: P. Coleman, *The Liberal Conspiracy. The Congress for Cultural Freedom and the Struggle for Mind of Postwar Europe* (New York: Free Press, 1989); P. Grémion, *Intelligence de l'anticommunisme. Le Congrès pour la liberté de la culture à Paris, 1950–1975* (Paris: Fayard, 1995); M. Hochgeschwinder, *Freiheit in der Offensive? Der Kongress für kulturelle Freiheit und die Deutschen* (München: Oldenburg, 1998); G. Scott-Smith, *The Politics of Apolitical Culture. The Congress for Cultural Freedom, the CIA and post-war American hegemony* (London and New York: Routledge, 2002); G. Scott-Smith and H. Krabbendam, *The Cultural Cold War in Western Europe, 1945–1960* (London: Frank Cass, 2003).
13. The Committee was headed by the renowned physicist Michael Polanyi. See, W. Taussig Scott and M.X. Moleski, *Michael Polanyi: Scientist and Philosopher* (Oxford: Oxford University Press, 2005); M.T. Mitchell, *Michael Polanyi: The Art of Knowing* (Wilmington: ISI Books, 2006); M.J. Nye, *Michael Polanyi and His generation. Origins of the Social Construction of Science* (Chicago: The University of Chicago Press, 2011).
14. *Atti del IX Congresso internazionale di genetica*, p. 18.
15. *Ibid*.
16. N. Krementsov, *International Science Between the World Wars. The Case of Genetics* (London and New York: Routledge, 2005), pp. 131–136.
17. For a comprehensive history of Italian eugenics, with a specific focus on Gini's role, see in particular: F. Cassata, *Building the New Man. Eugenics, Racial Science and Genetics in Twentieth-Century Italy* (New York and Budapest: Central European University Press, 2011).
18. A. Buzzati-Traverso, "In Russia si nasce secondo la legge di Lyssenko," *L'Europeo* 4, no. 46, November 8–14, 1948, p. 9; G. Montalenti, "Scienza e Libertà: il caso Lysenko. Una polemica scientifica si trasforma in tragedia," *Corriere d'informazione* 4, no. 275, November 23–24, 1948, p. 3.
19. See: C. Barigozzi, "I nuovi orizzonti della citogenetica," *Genus* 3, no. 3–4 (June 1939), pp. 35–72; A. Buzzati-Traverso, "I nuovi orizzonti della radiogenetica," *ivi*, pp. 73–130; G. Montalenti, "I recenti studi sul problema della determinazione del sesso e dei caratteri sessuali secondari negli animali," *ivi*, pp. 193–214.
20. Cassata, *Building the New Man*, pp. 147–192.

21. G. Montalenti, "L'VIII Congresso internazionale di Genetica (Stoccolma, 7–14 luglio 1948)," *La Ricerca Scientifica* 19 (1949), pp. 130–131.
22. Gini resumed his academic position as Dean of the Faculty of Statistical, Demographic and Actuarial Sciences at the University of Rome, after being nearly suspended during the post-fascist purging. On the purge trial, see: F. Cassata, "Cronaca di un'epurazione mancata (luglio 1944–dicembre 1945)," *Popolazione e Storia* 2 (2004), pp. 89–119.
23. Gini to members, December 31, 1948, AM, b. 24, f. 2, sf. 8.
24. Buzzati-Traverso and Barigozzi to Montalenti, 1 January 1949, Giuseppe Montalenti Papers, Università di Roma "La Sapienza," Sezione di Storia della Medicina (hereafter MP), b. 24, f. 2, sf. 8. The letter was sent to Montalenti for comments.
25. *Ibid*.
26. *Ibid*.
27. Minutes of the meeting of the Provisory Committee for the 9th International Congress of Genetics, November, 3, 1951, MP, b. 28, f. 9. Other permanent members—Carlo Jucci, professor of zoology in Pavia, Silvio Ranzi, professor of zoology, and Sergio Tonzig, professor of botany, both at the University of Milan—were connected with both Buzzati and Barigozzi.
28. For more details, see Cassata, *Building the New Man*, pp. 295–308.
29. C. Gini, "The Physical Assimilation of the Foreign Settlements in Italy," in *Atti del IX Congresso internazionale di genetica*: 246–253, on p. 252.
30. See F. Boas, *Changes in the Bodily Form of Descendants of Immigrants* (Washington: Senate Document 208, 1911).
31. Gini explicitly adopted the Italian term *sub–Lamarckiana*, which I translated as "neo-Lamarckian": see C. Gini, "Cause e carattere adattativo dell'evoluzione delle forme viventi," *Genus* 17, no. 1–4 (1961), pp. 1–42. See also C. Gini, "Mutazioni casuali o mutazioni adattative?" *Genus* 4, no. 3–4 (1940), pp. 77–84.
32. See Cassata, *Building the New Man*, pp. 147–191.
33. C. Gini, Review of A. Montagu, *Statement on Race* (New York: Schuman, 1951), *Genus* 10, no. 1–4 (1953–1954), pp. 192–194.
34. See in particular: A. Buzzati-Traverso, "Nonsenso biologico del razzismo," *La Rassegna d'Italia* 1, no. 1, January 1946, pp. 1–8. Significantly, in January 1951, UNESCO, following Julian Huxley's suggestions, invited Buzzati-Traverso to help draft the second *Statement on Race*: see, on this topic: C. Pogliano, *L'ossessione della razza. Antropologia e genetica nel XX secolo* (Pisa: Edizioni della Normale, 2005), p. 166.
35. For example, C. Gini, "The Testing of Negro Intelligence," *The Mankind Quarterly* 1, 2 (October–December 1960), pp. 120–125. On the relationship between Gini and the International Association for the

Advancement of Ethnology and Eugenics (IAAEE) and its publication, *The Mankind Quarterly*, see Cassata, *Building the New Man*, pp. 354–379.

36. On the IIS in the 1950s and 1960s, see F. Cassata, *Il fascismo razionale. Corrado Gini tra scienza e politica* (Rome: Carocci, 2006), pp. 194–213, and Id., "Un'Internazionale di destra: l'Institut International de Sociologie (1950–1970)," *Studi storici* 2 (2005), pp. 407–435.
37. A.G. Morton, *Soviet Genetics* (London: Lawrence & Wishart, 1951), p. 5.
38. *Ibid*., p. 158.
39. C. Gini, "Review of A. Morton, *Soviet Genetics*," *Genus* 10, no. 1–4 (1953–1954), p. 187; italics added.
40. Gini to Barigozzi, 10 July 1953 and 17 July 1953, Barigozzi Papers (BP); Barigozzi to Gini, 15 July 1953 and 21 July 1953, BP. I wasn't allowed access to Barigozzi Papers. I just received from Prof. Gianpiero Sironi one single file of correspondence related to the organization of the Bellagio Congress. I thank Prof. Sironi for this opportunity.
41. C. Gini, "The Physical Assimilation of the Foreign Settlements in Italy," *Atti del IX Congresso internazionale di genetica*, pp. 246–253.
42. G. Montalenti, "The Genetics of Microcythemia," in *Atti del IX Congresso internazionale di genetica*, pp. 554–588.
43. L.S. Penrose, "Some Recent Trends in Human Genetics," in *Atti del IX Congresso internazionale di genetica*, pp. 521–530.
44. B. Glass, "The Investigation of Factors Which Modify the Frequencies of Alleles in Human Populations," in *Atti del IX Congresso internazionale di genetica*, pp. 551–553.
45. T. Sjögren, "Recent Progress in Psychiatric and Neurologic Genetics," in *Atti del IX Congresso internazionale di genetica*, pp. 531–550.
46. Nazareno Strampelli (1866–1942) was the most famous of the Italian wheat geneticists. Mussolini's "Battle of Wheat" was largely based on Strampelli's hybridization program. The main reference for Strampelli's works is Istituto Nazionale di Genetica per la Cerealicoltura, ed., *Origini, Sviluppi, Lavori e Risultati* (Milan: Alfieri & Delacroix, 1932). On Strampelli during the Fascist period, see in particular: T. Saraiva, "Fascist Labscapes: Geneticists, Wheat, and the Landscapes of Fascism in Italy and Portugal," *Historical Studies in the Natural Sciences* 40, no. 4 (2010), pp. 457–498; R. Lorenzetti, *La scienza del grano. Nazareno Strampelli e la granicoltura italiana dal periodo giolittiano al secondo dopoguerra* (Rome: Ministero per i Beni e le Attività Culturali, 2000).
47. On this, see in particular L. Iori, *Agricultural Genetics and Plant Breeding in Early Twentieth-Century Italy*, PhD Dissertation, University of Bologna and University of Exeter, 2013.
48. *Ibid*.

49. Officers' Diaries, G.R. Pomerat's Diary 1950, Box 68, "5 November 1950, Sunday (Naples and Rome)," pp. 375–376, RAC, RG 12.2, Series 905 POM 1950.
50. Officers' Diaries, G.R. Pomerat's Diary 1950, Box 68, "14 November 1950, (Padova)," p. 395, RAC, RG 12.2, Series 905 POM 1950.
51. Officers' Diaries, G.R. Pomerat's Diary 1950, Box 68, "19 November 1950, (Milano)," p. 409, RAC, RG 12.2, Series 905 POM 1950.
52. Officers' Diaries, G.R. Pomerat's Diary 1950, Box 68, "14 November 1950, (Padova)," p. 395, RAC, RG 12.2, Series 905 POM 1950.
53. Barigozzi to Montalenti, 20 January 1953, MP, b. 28, f. 9. Emphasis in the original.
54. Buzzati-Traverso to Montalenti, 2 February 1953, MP, b. 28, f. 9.
55. On Carlo Jucci, see in particolar: P. Bernardini Mosconi, ed., *Carlo Jucci nel centenario della nascita. Testimonianze e documenti* (Milano: Cisalpino, 2000); A. Volpone, *Gli inizi della genetica in Italia* (Bari: Cacucci, 2008), pp. 128–141.
56. Letter by C. Gini to SIGE's members, 31 May 1950, MP, b. 24, f. 2.
57. Barigozzi to Montalenti, 16 May 1952, MP, b. 28, f. 9.
58. On Alberto Pirovano, see in particular A. Volpone, *Gli inizi della genetica in Italia*, pp. 74–84.
59. A. Pirovano, *La mutazione elettrica nelle specie botaniche e la disciplina dell'eredità nell'ibridazione* (Milano: Hoepli, 1922). On Pirovano's electrogenetics, see L. Iori, "Electrical Hybrids," in *Electricity and Life. Episodes in the History of Hybrid Objects*, ed. G. Pancaldi (Bologna: CIS, 2011), pp. 65–92.
60. A. Pirovano, *La mutazione elettrica nelle specie botaniche*, p. 206.
61. L.H. Flint, "Electrogenetics. A Review," *Journal of Heredity* 16, no. 6 (June 1925), p. 215.
62. *Ibid*., p. 216.
63. A. Pirovano, "Revisione del materialismo biologico," *Annali della Sperimentazione Agraria—Supplemento* III, 3 (1949), pp. 19–53.
64. I.V. Mitchourine, *Oeuvres choisies* (Moscow: Editions en Langues Etrangères, 1949). Pirovano wrote about his collaboration with the French translator in: "Revisione del materialismo biologico," p. 19.
65. O.T. Rotini, *Taccuino sovietico* (Pisa: Giardini, 1955), p. 74. Professor of agrarian chemistry, Orfeo Turno Rotini (1903–1990) was a member of the Italian Commission of Agrobiology.
66. A. Pirovano, "Revisione del materialismo biologico," p. 19.
67. The Commission included also Orfeo Turno Rotini, head on the Institute of agrarian chemistry at the University of Pisa and the agronomist Felice Lanza (Station of agrarian chemistry, Rome).

68. The AFAM counted about 100 members, 1000 correspondents, 14 "friendly" associations (associatins amicales) in France and ten groups in the Paris zone: Archives du Parti Communiste Français (APCF), Archives départementales Seine—Saint-Denis, Paris, Fonds F. Cohen, note by F. Cohen, 15 July 1951.
69. R. Duperon, "Fisiologia vegetale. A proposito della reversibilità dei processi di vernalizzazione," *ivi* I, no. 4, May 1952, pp. 20–22; P. Chouard, "Dibattito sulla vernalizzazione," *ivi*, pp. 23–24.
70. C.-C. Mathon, "Alcuni aspetti del miciurinismo," *Studi di Agrobiologia* I, no. 1 (January 1952), pp. 4–24.
71. J.L. Lush, "Rates of Genetic Changes in Populations of Farm Animals," in *Atti del IX Congresso internazionale di genetica*, pp. 589–599; O.H. Frankel, "Invasion and Evolutions of Plants in Australia and New Zealand," *Ibid*., pp. 600–620; F.G. Brieger, "Centers of Diversification of Wild and Cultivated Plants"; S.C. Harland, "Genetical Aspects on Verticillium Resistance in Cotton" (Brieger's and Harland's lectures were not published in the Proceedings).
72. L. Gianferrari, "Il contributo dell'Università al Centro di studi di genetica umana," *Gli Annali della Università d'Italia* 3, no. 1 (October 29, 1941), pp. 24–28.
73. L. Gianferrari, "Sull'organizzazione e sull'attività svolta dal Centro di studi di genetica umana nel primo quadriennio dalla sua fondazione," *Natura* 35 (1944), pp. 112–116, on p. 114.
74. *Ibid*., p. 113.
75. *Ibid*., p. 115.
76. *Ibid*., p. 116.
77. L. Gianferrari, "Importanza, urgenza di ricerche genetiche in popolazioni endogame," *Atti e memorie della Società Lombarda di Medicina* 5, no. 8, 1937, pp. 581–584.
78. L. Gianferrari, "Il Centro di Studi di Genetica umana dell'Università di Milano ed i Consultori di genetica umana dell'Università e del Comune di Milano," *Natura* 41 (1950), pp. 75–81, on p. 76.
79. L. Gianferrari, "Introduzione alla profilassi delle malattie ereditarie," *Acta Geneticae Medicae et Gemellologiae* 2 (May 1952), pp. 113–117, on p. 117.
80. Id., "Genetica umana," in *Atti del IV Congresso internazionale dei medici cattolici (Roma, 24 settembre–2 ottobre 1949)* (Roma: Orizzonte Medico, 1950), p. 129.
81. Id., "Il problema genetico delle leucemie," in *Atti del IX Congresso internazionale di genetica*, pp. 390–434.
82. Ministerial decree by Antonio Segni, June 15, 1953, Archivio Centrale dello Stato, Ministero della Pubblica Istruzione, Direzione Generale Istruzione Superiore (hereafter ACS, MPI, DGIS), Divisione I, Commissione libere docenze 1938–1953, b. 74, f. 1052.

83. Barigozzi and Buzzati-Traverso appeal to the State Council, August 27, 1953, ACS, MPI, DGIS, Divisione I, Commissione libere docenze 1938–1953, b. 74, f. 1052.
84. Appeal by Montalenti to the Head of State, December 14, 1953, in ACS, MPI, DGIS, Divisione I, Commissione libere docenze 1938–1953, b. 74, f. 1052.
85. In July 1954, the Ministry of Public Education appointed a new examination committee, which included, as permanent members, Barigozzi, Gedda and Gianferrari, and, as substitute members, Montalenti and Giordano. In November, at the end of the examination, the title of *libero docente* in human genetics was assigned to Angelo Cresseri and Giuseppe Morganti (Gianferrari's assistants in Milan), Ruggero Ceppellini (Barigozzi's assistant in Milan) and Amleto Maltarello (Gedda's *aiuto* at the Mendel Institute in Rome). See ACS, MPI, DGIS, Divisione I, Commissione libere docenze 1938–1953, b. 74, f. 1052.
86. A Fascist since 1924, Foà converted to Catholicism in 1938. After the promulgation of the racial laws, he emigrated to Brazil, subsequently returning to Italy in 1945.
87. The "Italian School of constitutional medicine" had its roots in mainstream clinical medicine, namely in the Medical Clinics of the Universities of Padua and Bologna. Its main exponents were respectively Achille De Giovanni (1838–1916) and Giacinto Viola (1870–1943). Italian constitutionalism shared a number of general principles with other European versions of medical constitutionalism—the primacy of the clinic, an individualized conception of illness, the celebration of bedside diagnostic skill, the denunciation of mechanization (standardization, specialization). Yet, a distinctive trait of the Italian school was probably the focus on the role of anthropometry and statistical biometrics in clinical medicine and medical pathology. On the "Italian School of constitutional medicine," G. Cosmacini, "Medicina, ideologie, filosofie nel pensiero dei clinici tra Ottocento e Novecento," in *Storia d'Italia*. *Annali*, Vol. 4, *Intellettuali e potere*, ed. C. Vivanti (Turin: Einaudi, 1981), pp. 1159–1194; Id., "Scienza e ideologia nella medicina del Novecento: dalla scienza egemone alla scienza ancillare," in *Storia d'Italia*. *Annali*, Vol. 7, *Malattia e medicina*, ed. F. Della Peruta (Turin: Einaudi, 1984), pp. 1223–1267.
88. L. Gedda, "Genetica, medicina e costituzione," *Acta geneticae medicae et gemellologiae* 1 (January 1952), pp. 1–6, on p. 5.
89. Buzzati-Traverso to Montalenti, MP, February 2, 1953, b. 28, f. 9.
90. C. Foà, "Discorso pronunciato nella cerimonia inaugurale dell'Istituto G. Mendel il 6 settembre 1953," in *Genetica Medica*, ed. Gedda, pp. 447–448, on p. 447.
91. Foà, "Discorso pronunciato," pp. 447–448.

92. L. Gedda, "Profilo scientifico della genetica medica," in *Genetica Medica*, ed. Gedda, pp. 3–18, on pp. 13–14.
93. "Address of His Holiness Pope Pius XII to those attending the "Primum symposium internationale geneticae medicae," in *Genetica Medica*, ed. Gedda, p. 429.
94. "Address of His Holiness Pope Pius XII," p. 429.
95. T. Dobzhansky, "Comment on the Discussion of Genetics by His Holiness, Pius XII," *Science* 118, no. 3071 (November 1953), pp. 561–563. In October 1953, Dobzhansky, recently returned from Italy, warned Pomerat against Gedda: "Without going into details here, it seems that Gedda is Head of the Catholic Action group in Italy and out of sympathy with the rest of Italian geneticists" (Officers Diaries', G.R. Pomerat's Diary 1953, Box 69, Series 905 POM 1953, "23 October 1953, Professor Theodosius Dobzhansky, Columbia University," p. 270, RAC, RG 12.2).
96. A. Buzzati-Traverso, "review of L. Gedda, ed., *Genetica Medica. Primum Symposium Internationale Geneticae Medicae Roma 67 settembre 1953* (Rome: Edizioni dell'Istituto Gregorio Mendel, 1953)," *Science* 122, no. 3161 (July 1955), p. 206.
97. Buzzati-Traverso, "review of L. Gedda," p. 206.
98. Officers' Diaries, John Maier Diary, Box 83, Folder 199, "1954, January 10 to 14, Rome; professor G. Montalenti," p. 11, RAC, RG 12.3; italics added.
99. Officers' Diaries, G.R. Pomerat's Diary 1955, Box 70, "28 November 1955 (Naples)," p. 376, RAC, RG 12.2, Series 905 POM 1955; italics added.
100. G. Pontecorvo, "Mitotic Recombination in the Genetic Systems of Filamentous Fungi," in *Atti del IX Congresso internazionale di genetica*, pp. 192–200; R. Dulbecco, "Recent Developments of Virus Genetics," in *Atti del IX Congresso internazionale di genetica*, pp. 182–191.
101. In November 1954, the Committee on Science and Freedom included the following members: Michael Polanyi (Chairman), J.R. Baker, E. Boeri, A. Buzzati-Traverso, C.D. Darlington, T. Dobzhansky, W. Gerlach, P. Gillis, S. Hook, H. Janne, T. Komai, D. Legache, H. Plessner, E. Rabinowitch, E. Shils.
102. "Il Congresso per la libertà della scienza," in *Libertà della cultura* 13, August 1953, p. 10. See also Buzzati-Traverso to N. Nabokov, September 2, 1953, CCF/IACF Archive, Regenstein Library, University of Chicago, Series II. Box 48, Folder 10.
103. *Atti del IX Congresso internazionale di genetica*, p. 21.
104. On this issue, see in particular: R. Selya, "Defending Scientific Freedom and Democracy: The Genetics Society of America's Response to Lysenko," *Journal of the History of Biology* 45, no. 3 (2012), pp. 415–442.

105. Buzzati-Traverso to N. Nabokov, 2 September 1953, CCF/IACF Archive, Regenstein Library, University of Chicago, Series II. Box 48, Folder 10.
106. See the correspondence between Barigozzi and Nabokov, CCF/IACF Papers, Series II, Box 42, Folder 12. On the 1955 Milan Conference: G. Scott-Smith, "The Congress for Cultural Freedom, the End of Ideology and the 1955 Milan Conference: 'Defining the Parameters of Discourse'," *Journal of Contemporary History* 37, no. 3 (July 2002), pp. 437–455.
107. Hollaender to W. Burckhardt, 1 October 1956, Alexander Hollaender Papers Supplement (University of Tennessee Libraries, Knoxville, Special Collections), 1951–1972, MS.1105, Box 1, Folder 6: C.I.P.—Turin 1957.
108. Fano to Hollaender, October 19, 1956, Alexander Hollaender Papers Supplement, 1951–1972, MS.1105, Box 1, Folder 6: C.I.P.—Turin 1957.

The National Pattern of Lysenkoism in Romania

Cristiana Oghina-Pavie

Introduction

The Sovietization of Romania's science resulted just as much from political decree as it did from the voluntary submission of Romanian scientists: "The Romanian scholars were brought to heel, summoned in their turn to place themselves under the yoke of the Soviet state science."[1] This statement is taken from a 1949 report sent to the French Ministry of Foreign Affairs by the cultural attaché of the Legation of France in Bucharest, the young semiotician Roland Barthes. He had a unique insight into Lysenko's impact behind the recently, metaphorically, drawn "Iron Curtain," and from his point of view, Lysenkoism was a product of the relentless spread of the influence of the Soviet Union in Eastern Europe.

Dissemination of Lysenkoism in the satellite states was a more complex process[2] than the docile submission to the Soviet yoke, emphasized by Barthes in his report. Romanian historiography has widely studied Sovietization and its victims, but no exhaustive study has so far been devoted to Romanian Lysenkoism. The main purpose of this chapter is to investigate the factors within science that played a crucial role in the national pattern of Lysenkoism in Romania. This does not mean ignoring

C. Oghina-Pavie (✉)
University of Angers, Angers, France

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the political pressure exerted by the USSR and the Romanian authorities on science. Rather I will focus on the epistemological factors inside science, still understudied by historians.

Two major aspects of the Romanian case will be covered in this chapter. First is the behavior of Romanian scientists who mediated the adoption of Lysenkoism under the auspices of the Romanian Association for Strengthening Relations with the Soviet Union (ARLUS).[3] I will focus on the role of Traian Savulescu (1889–1963), a biologist, renowned for his work on plant pathology and systematics before World War II. In the postwar period, Savulescu became actively involved in Romanian politics and was the most influential advocate of Lysenkoism in biology and agronomy.

The second major aspect which will be studied here is the strong French influence on Romanian culture and science. The traditional link between France and Romania played a contradictory role in the history of Lysenkoism. On the one hand, French influence was severely attacked by pro-Soviet propaganda against the West. At the same time, the preexisting neo-Lamarckian thought in Romanian biology was conducive to the adoption of Lysenkoism. These influences underpinned the reception and development of Romanian Lysenkoism.

The import of Lysenkoism in 1948–1954 was followed by a long phase where the Soviet model was intertwined with French neo-Lamarckism. This original trend in Romanian biology helped sustain Lysenkoism and extended its impact far longer than in other Eastern Bloc countries. Elements of genetics were integrated in the beginning of the 1960s, continuing until the official introduction of Western genetics in 1965. However, in the 1960s, the abandonment of Lysenko's thesis did not entail the simultaneous rejection of Michurinism. Romanian biologists and horticulturists continued to cite Michurin's work until the early 1970.

My hypothesis is that neo-Lamarckism played a crucial role in maintaining the continuity of Romanian science, despite the brief interference of Lysenkoism. Due to the resemblance between Lysenkoist postulates and neo-Lamarckism, Romanian biologists were able to preserve Romanian science from Soviet domination. They used neo-Lamarckism to align their research with their predecessors before the Second World War and forge a uniquely Romanian national scientific identity. The idea of continuity will therefore be the main theme of this chapter. I will focus on the role of Romanian scientists in the introduction of Lysenkoism, on the persistence of neo-Lamarckian concepts and on the historical reconstruction of a continuous national science, in order to argue that the importation of Lysenkoism was an acculturation of the Soviet model to the Romanian national pattern.

The Mediators of Lysenkoist Propaganda

Between the end of World War II and the beginning of the Cold War, Romania was a field of confrontation between Western and Soviet camps. ARLUS was established in a general meeting at the Faculty of Science of the University of Bucharest on November 12, 1944.[4] The location was not chosen randomly: half of the 20 founding members were university professors. Constantin I. Parhon (1874–1969), professor of neurology and psychiatry and a renowned specialist in endocrinology, was the first president. Well known as a leftist pacificist, Parhon used his prominence in the medical community to attract intellectuals from other fields. ARLUS was organized into sections covering economics, science, literature, philosophy, applied sciences, social sciences, military, transport and communications, education, media, propaganda, art, sport and tourism.

In the first issue of its weekly paper, *Veac Nou* (New Century), ARLUS emphasized freedom of expression in newly liberated Romania. However, the organization quickly revealed its real mission by sending a congratulatory telegram to Stalin on his birthday, and organizing a reception for the president of the All-Union Society for Cultural Relations Abroad (VOKS), Vladimir Kemenov.[5] ARLUS quickly developed branches in all educational institutions, as well as in factories and collective farms. It was organized into clubs on various specific topics. Under the tutelage of VOKS, ARLUS became, like all the official friendship societies, the main distribution channel of Soviet propaganda in all fields, including science.[6]

The presence of scientists in ARLUS was significant. Whether they were communists, socialists, pacifists, anti-Fascists or simply opportunists, the first members of ARLUS were part of the inner circle close to C.I. Parhon.[7] Some, including the president, were also, in 1944, active members of other associations, such as *Societatea Amicii Statelor Unite* (Society of Friends of the USA) or *Societatea Amicilor Frantei* (Society of the Friends of France). This suggests a sincere commitment to asserting the autonomy of Romanian culture and maintaining ties with the West. As serious researchers in their respective fields, they provided an intellectual bond within the organization, an undisputed prestige among new members and, as professors in higher education, the ability to influence young students.

Many biologists and physicians served as presidents or vice-presidents of scientific sections, as well as sections on propaganda and education.[8] President of the Section of Applied Sciences, Traian Savulescu, was a specialist in plant biology who played a central role in the importation of Lysenkoism. Born in 1889, Savulescu obtained a doctorate in Natural

Sciences at the University of Bucharest in 1916, specializing in botany and plant systematics. In 1929, he was appointed Director of the Section on Plant Pathology in the Romanian Institute of Agricultural Research (ICAR), where he became a member of numerous international networks on floristry, plant systematics, pathology and mycology. He traveled extensively in France, Germany, Austria, Switzerland and Italy, for scientific exchanges, conferences and meetings of scholarly societies and represented Romania at the International Institute of Agriculture in Rome.[9] In 1938, he married his second wife, Alice Savulescu (born Aronescu) (1905–1970), herself a biologist, who had received her PhD in microbiology and pathophysiology at Columbia University in New York in 1934. However, Traian and Alice Savulescu were dismissed from their teaching positions for a short period at the beginning of the War (1940–1941) because Alice was Jewish.. Savulescu returned to teaching in 1941 under the Antonescu government.[10]

Savulescu's motivation for becoming a founding member of ARLUS in November 1944, as well as the origins of his friendship with C.I. Parhon, are unclear. As an internationally known scientist who had published dozens of studies on mycology, systematics and plant pathology, Savulescu was well positioned, as president of the Applied Science section of ARLUS, to promote issues of biology and agronomy. He joined the Romanian Workers' Party (*Partidul Muncitoresc Român*- PMR) in February 1945 and, from that date until his death in 1963, his activity for political and social change in Romania was continuous. Traian Savulescu was one of the 60 university professors who, adhering to the National Democratic Front in 1945, supported the new left-wing government.[11] Savulescu participated in the reception of delegations of Soviet scholars and traveled frequently to Moscow.[12]

From 1945 to 1948 his scientific publications focused on his specialty: classification of bacteria and fungi, annual reports of the pest status in Romania and a university course in plant pathology.[13] His political activity does not appear to have influenced his scientific work; however, Savulescu became extremely active in propagating communist ideas. He regularly published articles in *Veac Nou* and in the general press where he wrote enthusiastically on Soviet agriculture. Savulescu also prefaced several works of Russian and Soviet biologists translated into Romanian.[14] He worked actively to apply the Soviet model in Romania.[15]

This commitment to the values of the new regime gave momentum to his political career. In 1946 he was appointed Deputy Secretary of

State and served as Minister of Agriculture from 1946 to 1948. These governmental functions placed Savulescu at the forefront of agrarian reform.[16] A law passed on March 23, 1945, mandated the nationalization of property exceeding 50 ha and the total expropriation of landholdings belonging to war criminals and former collaborators with the Nazi regime. Savulescu managed the implementation of the reform, as well as a severe famine in the eastern part of Romania following a drought in 1946–1947.

After the abdication of King Michael on December 30, 1947, and the proclamation of the People's Republic of Romania (RPR), the new regime nationalized industry, banks and insurance companies. The agrarian reform completed in 1948 was immediately followed, in March 1949, by a new project for the "socialist transformation of agriculture." This period was characterized by purges affecting all levels of society, including leaders of the new regime.[17] As for Parhon and Savulescu, they continued to occupy important positions in the state. On April 13, 1948 Parhon became President of the Presidium of the Grand National Assembly—the highest official office, where he served till 1952—as well as honorary president of the Academy of the People's Republic of Romania. Savulescu, meanwhile, became coordinator of the Ministries of Agriculture and Forestry and the second Vice-President of the Council of Ministers in the Groza government, a position he left in 1949.

Savulescu's main task was reorganizing scientific institutions, meaning the subordination of the Romanian Academy to the communist state. This process involved the collaboration of the most influential, loyal and opportunistic Academy members.[18] In May 1948, a session discussed the new statutes. Some scholars feared the complete nationalization of the institution and openly criticized the project.

On May 29, Savulescu presented the report of the Science section, advocating that the academy be transformed into a state institution. His arguments concerned the Academy's mission to improve people's lives. Only the state, according to Savulescu, could ensure the durability of the institution. Savulescu traced the history of the Academy since the nineteenth century, showing that though it always responded to national needs, infighting and lack of funding from the bourgeois state slowed its development. An academy attached to the Council of Ministers, he argued, was the only guarantee to fulfilling the scientific needs of the nation, while maintaining intellectual independence:

> The Romanian Academy will cease to be an institution of patronage and consecration, and forgive me the comparison, a club of retired gentlemen, nostalgic for the past, who enjoy weekly gatherings.[19]

By the summer of 1948, several decrees established the status, duties and composition of the new RPR, following the Soviet model.[20] Nationalization offered the opportunity to purge some members and select new ones based upon political criteria.[21] At the first meeting in October, Savulescu declared that science should serve the state and transform Nature to benefit the new social and political order.[22] Science must be applied and useful—as opposed to fundamental science which was deemed "sterile." Therefore, the full import of Lysenkoism could rely on all the mechanisms of the academic system with, on the inside, strong support from important scientists and, on the outside, the political, bureaucratic and repressive apparatus of the state.

Import of Lysenkoism

The import of Lysenkoism was based on agronomic practice, theoretical positions on heredity and evolution and opposition to Western science. In a general climate of censorship and severe repression of all forms of opposition,[23] these three aspects of Lysenkoism, as practice, theory and ideology, were introduced as a system.

On January 17–22, 1949, a session of the ICAR was dedicated to the achievements of Soviet science in the field of agriculture. The session was attended by 300 participants: researchers and directors of agricultural stations, research institutes on agriculture, animal husbandry and forestry, technicians from the Ministry of Agriculture and agronomy students. The reports were submitted by teachers of the Faculty of Agronomy of Bucharest and ICAR researchers,[24] who were primarily concerned with two topics—the Dokuchaev-Kostachev-Williams system on soil science and the Michurinist conception of biology. Both had been presented as revolutionary and opposed to Western theories and practices, the first by denying physicochemical approaches to the soil, the second by challenging Mendelian-Morganian genetics.

Both also raised, in different but complementary ways, the question of evolution. For Lysenkoism, the link with the evolution was more obvious because heredity was central to its dogma. For Williams, this link was more metaphorical. Williams considered soil as a historical and natural body subject to change, in what the Soviet literature called "genetic science of the soil."

Williams and Lysenko both invoked dialectical materialism to explain the transformation and mastery of Nature as a factor of production. Using arguments drawn from their predecessors (Dokuchaev and Kostachev for Williams and Michurin for Lysenko), they deduced a set of practices that interfered with the cultivation of plants, animal husbandry and soil cultivation. Concrete measures, immediately applicable in all experimental stations and state farms, were mandated at the January 1949 session: fall and spring plowing to conserve water in the soil, perennial grasslands, soil enrichment, cropping forage, plantation forests and forest belts, improved livestock by the effects of nutrition and improved varieties of vegetables and fruits.

As director of ICAR, Traian Savulescu defined the research program for the coming years in strict compliance with the Soviet approach.[25] The expected result of these measures was increasing agricultural production. Resisting or contesting the validity of Lysenkoism meant opposing the agrarian policy of the Romanian Workers' Party. Lysenkoism and agrarian policy were closely linked to collectivization with the creation of state farms (GOSTAT) following a March 2, 1949,[26] decree and an PMR Central Committee Plenum March 3–5, 1949.[27] Any act to undermine collectivization was defined as sabotage and punishable by 5 to15 years in prison.[28] Scientific debate, already muzzled by purges the previous year, was impossible because it exposed scientists to accusation of being enemies of the people.

On March 29–31, 1949, the importance of Lysenkoism outside the narrow circle of specialists in agriculture was affirmed at the Conference for Peace and Culture of Romanian Intellectuals, a local version of the World Congress of Intellectuals for Peace, held in Wrocław, Poland, the previous year. However, the work of the Bucharest Congress was not limited to the defense of peace. Traian Savulescu, as president of the RPR Academy, presented a long report in which he listed the characteristics of the new Romanian science: it should be a mass science, progressive, collective, planned and useful. In direct reference to the session of August 1948 of VASKhNIL, Savulescu announced the official position of the RPR Academy on issues of evolution, heredity and transformation of living beings under the influence of the environment, as well as his full commitment to Lysenko's theses. He particularly emphasized the ideological implications of Michurinist biology versus genetics. For him, Western genetics was a "distortion" of science, specific to an idealistic and bourgeois world view. In arguing that inheritance was immutable, genetics justified social and political inequality. Soviet Michurinist biology, "based on a realistic and dialectical representation of nature," was an expression of confidence in the transformative power of man over Nature and society.

In a formal meeting of the RPR Academy on March 30, Traian Savulescu presented a long report, "From the practical plant breeding to principles of general biology: Reflections on the report of T.D. Lysenko," officially announcing the introduction of Lysenkoism into Romanian agriculture.[29] Without debate, the Academy adopted a motion to support Lysenko.[30]

Once the practical and theoretical status of Lysenkoism was affirmed, the third aspect—ideological—was announced in June 1949 by the medical section and the Presidium of the RPR Academy, which adopted a resolution "against anti-scientific, anti-national and anti-patriotic tendencies" in Romanian science. This position was motivated by a discussion of the *Ophthalmological Review* to which three objections were raised: it published in English and French, it ignored the achievements of Soviet scientists, and it prided itself with being well appreciated by an American scientific journal. Savulescu repeated the themes[31] of struggle against "universal, bourgeois and imperialist" science. According to him, the political split between the two antagonistic blocs must necessarily also have been found in the scientific field. Western imperialism was accused of using "universal" and "abstract" science as an instrument of domination over small nations. Soviet science should serve as an example, because it was the only way to improve knowledge and understanding, and enable mankind to transform Nature for the benefit of society. Quoting Western scientific literature or publishing in Western journals was evidence of enslavement to capitalist imperialism. Again, Michurinism was raised as a bulwark against cosmopolitanism.

> I.V. Michurin and his disciples raised biology to a superior level of knowledge, understanding, interpretation and application; Soviet biology has a decisive superiority, verified in practice, over Western science, imbued with reactionary ideas cut off from life. The struggle waged by T. D. Lysenko and the Michurinist School against idealist biology aroused interest not only in the USSR, but also all over the world. The enemy, camp which distorted scientific truth was exposed.[32]

Savulescu criticized the tendency of Romanian scientists to emulate their Western colleagues, particularly the French: "Why did you insist on being little Frenchmen, instead of aspiring to be yourselves?"[33]

It was this report that Roland Barthes was reacting to in the quote at the beginning of this chapter. He went on to refer to Savulescu directly, saying that "this academic, who owes all his training to French science, did not hesitate to deliver an indictment against Western scholars as unworthy as the

coarse flattery endlessly addressed to Soviet science.[34] Barthes highlighted that in Romania the main target of the anti-Western campaign was France. The reason for this was the French influence on the Romanian language, nationalism, ideology, literature, art, manners and tastes of the high society, symbolizing Romania's connection to Europe. Before World War II, 80 % of Romanian university professors had been educated in France, and 60 % of foreign scientific books purchased in Romania were French. This is a common estimation; these numbers were widely discussed in the press, the diplomatic documents and the memoirs of the intellectuals of that time. Obviously, a large number of Romanian scientists studied in France.

Though the harmonious relationship between French and Romanian culture was interrupted by the Second World War, it quickly resumed once it ended.[35] However, from the perspective of the new regime, the predilection of Romanian intellectuals for France made them less receptive to Soviet cultural policy. The professors of the University of Iasi handed to the director of the French Institute a "Call to France" in December 1947. Fearing the decline of French culture and increase of Soviet influence, they asked books and reviews:

> The spiritual life we will find there will relight the extinguished torch. Its light and heat are necessary for the youth. This is a great work of public salvation to be undertaken. It would be worthy of France.[36]

Thus French cultural diplomacy and ARLUS engaged in a competition that gradually transformed into open conflict.[37] The year 1948 marked a turning point in French-Romanian cultural relations with Romania's formal renunciation of a 1939 Cultural Agreement and the abolition of the French Institute, created in 1924.[38] French cultural diplomacy was gradually smothered by Sovietization.[39] However, these short-term political maneuvers could not nullify the long-term effects of French influence.

The French Influence: Neo-Lamarckism in Romanian Biology

Despite the attack against France, it is precisely the French neo-Lamarckian tradition that had created favorable conditions for the reception of Lysenkoism. The biologists and agronomists trained in French schools and universities before and after the First World War were exposed to Lamarck's theories. This approach to evolution was preserved in their

research and teaching after they returned to Romania. Among those who did so was the most esteemed figure in interwar Romanian biology, Emil Racovita (1868–1947).

Born into a family of wealthy intellectuals in Iasi, Racovita studied law in Paris and also took courses in natural science. He received a law degree in 1889, a natural sciences degree in 1891, and obtained the title of Doctor of Science with a thesis on the morphology of annelids in 1896. He then worked in the French marine biology stations in Banyuls and Roscoff with Henri Lacaze-Duthiers. Between 1897 and 1899, he participated in a scientific expedition to Antarctica on the *Belgica*, became interested in cave fauna and, with René Jeannel, founded a review *Biospéleologica*.

In 1919, the Romanian government offered Racovita a professorship at the newly created University of Dacia Superior in Cluj, to give the institution prestige on an international level. Racovita had little interest in teaching. By this time, he had taken a French wife and inherited a considerable fortune, freeing him from the need to work, so he initially declined this proposal. A few months later the government reiterated the invitation, giving him full control in organizing a research institute, and he agreed. The Romanian parliament passed a law founding the Institute of Speleology in Cluj in December, 1919.[40] Racovita was appointed director for life and given free rein on deciding a budget, facilities, materials, staff and, of course, the scientific direction. Racovita paid himself a salary equivalent to a professor of the University of Cluj, but to his collaborator, René Jeannel, he gave a five-year contract with a salary equivalent to twice what he had been paid in France—about 10 times higher than a typical Romanian professor. The government agreed because its purpose was to convince Racovita, at any price, to return home.

Racovita introduced a strong international, interdisciplinary approach at the Institute. He continued his research on cave fauna and mobilized zoologists, geologists and archaeologists from France and other foreign countries for conferences, exploratory trips and publications. He became Rector of the University and played a leading role at the national level as a member, and then president, of the Romanian Academy from 1927 to 1929. René Jeannel returned to France in 1927, but he continued to support Racovita's work.

The controversy between the neo-Darwinian and neo-Lamarckian views on heredity obviously existed in Romania, but Racovita's scientific reputation brought unparalleled power to Neo-Lamarckism. Racovita was personally involved in the theoretical debate and mobilized his research to support his vision of evolution. As he announced in a public lecture in 1927,

> The neo-Darwinians have come to completely deny the possibility of the external environment to produce large variations and to transmit it to posterity; … We must say, however: all changes are caused by variations of the external environment ….[41]

From 1931 onward, however, government subsidies decreased and the Institute ended up in a precarious financial situation. It was heavily damaged during World War II and the equipment and collections were destroyed. In the aftermath, Racovita lost his influence and fortune, and spent years trying to revive the Institute, until his death in 1947.

Though Racovita was dead by the time Lysenkoism was introduced into Romanian biology, his ideas played an important role. For example, in 1947 Stefan Popescu (1888–1961), of the Faculty of Agronomy of Iasi, published what was probably the last book on plant breeding and genetics before the arrival of Lysenkoism.[42] The theoretical part of Popescu's book addressed the issue of the relationship between living organisms and the environment in neo-Lamarckian terms, similar to Racovita's. His approach was based on a conception of life as a continuous and dynamic interaction between the organism and its environment, which had a lasting influence.

> …Organisms cannot be conceived outside of their external environment. We can say that in reality they inherit, to a greater or lesser extent, the ability to respond to life factors: both internal and external.[43]

According to Popescu, a change in the life conditions of plants and animals during the early stage of their development led to changes and deviations in the offspring. Citing Lamarck, Darwin, Weismann, Mendel, Bateson and Morgan, Popescu considered genetics as a bridge linking the concepts of the physiology of variation and heredity. Popescu understood the environment as a synthesis between the internal, consisting of genes and hormones, and external, consisting of the living conditions of the organism. His analyses on neo-Darwinism did not deny the contribution of this school, but emphasized its "experimental limits" and the difficulty of basing plant-breeding practice on neo-Darwinian concepts. Concerning the debates of his time, Popescu mentioned the position of the "Russian school" (he did not use the word "Soviet"), which attached great importance to the environment and offered an original experimental approach. However, Popescu was not willing to fully convert to this model either.[44] His ideas were based on those developed by Racovita and Jeannel between 1930 and 1940.[45]

Did the fact that neo-Lamarckism was so central to Romanian biology make Romanian biologists more amenable to Lysenkoism? This is a complex question. Analyzing the theoretical positions of biologists affected by political purges between 1946 and 1949 could shed light on this issue. Were they neo-Darwinian or rather neo-Lamarckian? An answer is not possible given the current state of research on the victims of communist repression in Romania; however, if we limit ourselves to the most prominent researchers in biology and agronomy it appears that repression was motivated by political, not scientific, reasons.

Among the full members of the Romanian Academy who were not appointed to the new RPR Academy in 1948 were a biologist and an agronomist. The first, Constantin Motas (1891–1980) was a Doctor of the University of Grenoble, a zoologist and hydrobiologist. He was arrested in May 1949 for being close to the leader of the Social Democratic Party, Titel Petrescu and sentenced to 20 years in prison. The second, Gheorghe Ionescu-Sisesti (1885–1967), studied agronomy at Stuttgart and Jena and founded the Romanian Institute for Agronomical Research, which he directed, in 1924. He served as Minister of Agriculture four times, between 1931 and 1940. In 1948, he was removed from the Academy by the new political regime. Meanwhile, two doctors, Mihai Ciuca (1883–1969) and Constantin Ionescu-Mihaesti (1883–1962), and a biologist, Dimitrie Voinov (1867–1951), former full members, were demoted to honorary status.[46]

What about Savulescu? Was he a neo-Lamarckian? The answer is not simple. There is not enough space in this chapter to provide a comprehensive summary of his work; so I will limit myself to the concept of species, a central feature in his publications before and after World War II. Savulescu was not a product of the French biological school. He did not have close relations with Racovita. In 1912, his Romanian professor Voinov recommended Savulescu to Racovita to do his doctoral thesis at the Museum of Natural History in Paris; however Savulescu was unable to obtain a scholarship and had to do his thesis in Romania.[47] Between 1918 and 1924, inspired by the experimental transformism of the French neo-Lamarckian Gaston Bonnier, Savulescu undertook research on experimental taxonomy on *Campanula*, namely classifying plants according to the morphological modifications caused by a deliberate change in their environment. This approach respected neo-Lamarckian theoretical principles and methodology. However, his research was soon to follow a different path.[48]

In 1944 Savulescu wrote an article with a more theoretical approach, "The genetic basis of the evolution of living beings."[49] This focused on the contribution of genetics to the theory of evolution. For him, the definition of species should be established on genetic research, based on the rediscovery of Mendel's laws. This meant a synthetic approach to cytology, bio-geography and systematics applied to the study of plant populations. Savulescu clearly distinguished between phenotype and genotype, emphasizing the non-environmental factors influencing the development of the latter. He made extensive references to Morgan, and illustrated his paper with Vavilov's crop genes distribution map, affirming his belief that evolution must be studied genetically.

Was Savulescu able to reconcile these views with Lysenkoism? His article "From the practice of domestication of plants to the principles of general biology. Reflections on the report of T.D. Lysenko" helps answer this question.[50] The purpose of the text was to present the position taken by Lysenko in August 1948. Savulescu framed the confrontation between Lysenko and Western genetics as an authentic scientific controversy about the plasticity of organisms under the influence of the environment. The article is 70 pages long—and 50 pages are Lysenkoist propaganda. From this one would assume it is basically a typical "Soviet-style" document, with requisite references to Lysenko, Michurin, Marx, Engels, Lenin et al. Savulescu employed here numerous Lysenkoist arguments: Michurin's vegetative hybridization of fruits and vegetables; the acclimatization of rice and wheat by vernalization and so on. However, the remaining 20 pages are of much interest. He mobilized here three kinds of arguments. Firstly, he quoted Western authors (P. Brien, Lucien Daniel, Lucien Cuénot, Maurice Caullery, J.B.S. Haldane, Marcel Prenant, Georges Teissier, etc.), as well neo-Lamarckians and neo-Darwinians and interpreted their writings in order to support Lysenko's thesis.[51] Savulescu's second argument is theoretical. He said Lysenko's proposals were accurate because he approached the living world experimentally and dialectically. Savulescu criticized theories that considered the species as an isolated construction of random genes. He said evolution was based upon three factors: "phylogeny (kinship), ontogeny (development) and heredity (the transmission of inherited and acquired characters)." And finally, the most interesting argument is a series of examples of Savulescu's own works:

> I tried to bring, according to my capacity, a modest personal contribution regarding the essential principles of biology in relation to heredity, adaptation and evolution of living beings, as was suggested by the report of the Soviet agrobiologist Lysenko and the grandiose work of genius Michurin.[52]

He thus presents his researches on parasitic plants and his neo-Lamarckian study on experimental taxonomy on *Campanula*, carried out in 1918–1924, as original evidences justifying Lysenko's thesis. In this way, Savulescu engaged himself, as a biologist, in defense of Lysenkoism. He strongly argued for Lysenko and his theories. He presented himself as a serious biologist ready to adopt and apply them in Romania. According to Savulescu, Lysenko's work was a continuation of neo-Lamarckism familiar to Romanian biologists. Though this was not explicitly stated in Savulescu's article, it was implied by references to the French neo-Lamarckian emphasis on the plasticity of living organisms and experimental transformism. This link would play a crucial role in Romanian Lysenkoism in the following decades.

Romanian biologists were not asked to make original contributions but, following the pattern elsewhere, simply affirm and apply Lysenkoism.[53] They wrote only propaganda articles and advice on agronomic techniques. Apart from this, between 1949 and 1953, almost all publications on scientific subjects representing a theoretical or an ideological issue were translations of Russian and Soviet biologists, namely works of Lysenko, Michurin, Glushchenko, Turbin, Sacharov, Zavadski and so on. Articles translated from Russian were published mainly in the series on biology, zoology, geography or medicine of the *Analele Romano-Sovietice* (Romanian-Soviet Annals) and *Probleme de filozofie* (Problems of Philosophy). The only major projects undertaken by the RPR Academy focused on areas that seemed safe from interference from Party philosophers, such as comprehensive works on Romanian fauna and flora. It was only during the thaw after Stalin's death that the neo-Lamarckian influence showed its real impact, and Romanian Lysenkoism revealed its originality.

Michurin Without Lysenko

The period following Stalin's death was, as in other East Bloc countries, a period of transition. This occurred in the context marked by a speech by the first secretary of the Romanian Workers' Party, Gheorghe Gheorghiu-Dej, in front of the Central Committee of the PMR on March 23–25, 1956.

He repeated the ideas of Khrushchev's speech of February 14, 1956, admitting some mistakes made in the past by Stalinists and improper methods derived from the cult of personality, as a breach of internal party democracy and the principle of collective leadership.[54] Moreover, de-Stalinization had a relatively muted impact in Romania, compared with places like Poland and Hungary. After the withdrawal of the Soviet troops in 1958, the Romanian ruling class adopted a strategy of "nationalization" of the Soviet model, accompanied by a new phase of enhanced repression with the aim of maintaining its power.[55]

Between 1955 and 1956, some political prisoners and former dignitaries were released from prison. A few scientists and professors returned to their positions in research institutes and universities, a process concurrent with Vavilov's rehabilitation and the beginnings of the critique against Lysenko in the USSR. At the RPR Academy, Traian Savulescu was re-elected President, and proceeded to repair what he described as the "abuse of the Stalinist period." High-ranking scientists, including Savulescu, retained their positions and, Savulescu's image was even enhanced by the return of these academics who had been excluded from Academy and Universities in 1948. His professor, Gh. Ionescu-Sisesti, returned as full member of the Academy and, in 1955, became president of the biology and agronomy section. Constantin Motas was released from prison in 1956, was immediately invited to a private discussion with Gheorghe Gheorghiu-Dej and, a month later, appointed director of the Institute of Speleology. Many biologists of the older generation regained the right to sign articles and speak to students. Some of them renewed contacts, interrupted in 1949, with scientific institutions in France, Belgium, Great Britain or other countries. These initiatives were not only personal, but also part of a policy of the RPR Academy to exchange publications with Western scientific institutions and participate in international scientific events.

Did this thaw mean a full and abrupt abandonment of Lysenkoism? This was not the case in Romania—at least not in terms of any open criticism.[56] Rather what we see is a version, in science, of what occurred in other areas of Romanian society and politics, where the Soviet model was not rejected, but amended in a manner that reinforced national identity. Savulescu claimed in 1956: "there is no break between the old academy and the RPR Academy."[57]

Original publications by Romanian biologists and agronomists, published from 1955 to 1965, demonstrate the persistence of Lysenkoist themes mixed with neo-Lamarckian concepts. In the precise moment

when the ideological cover of Lysenkoism was being removed, Romanian neo-Lamarckism re-emerged. Still unsure to what extent Lysenko could be criticized, biologists did not publicly denounce him. They relied on the conceptual system that was familiar to them—neo-Lamarckism—embellished with references to Soviet science, to maintain the continuity of Romanian science. To illustrate this, I will focus on two areas in which the synthesis operated: plant breeding and the history of biology.

Several textbooks on agricultural experimental techniques had been published by universities (Bucharest, Timisoara, Iasi, and Cluj) since 1949. The first great original book on agricultural techniques were published in 1956–1958,[58] and of plant breeding in 1960.[59] The authors were practitioners of plant breeding in experimental stations and professors of agronomy.[60] The presentation of breeding practices was preceded by the theoretical foundations of the methods employed. These works refer to Michurin, Glushchenko and Turbin in order to argue the unity between the organism and its environment as the condition for the formation of the characteristics and of their inheritance. Practical advice was founded on principles that considered heredity as a consequence of environmental changes on the physiology of plants.

The list of methods was Lysenkoist: sexual hybridization, mentor method, education of seedlings to cause adaptation, accumulation of the effects of disturbed heredity on several generations, distant hybridization and vegetative hybridization. However, in the chapters of these books providing detailed descriptions of field techniques, these methods do not all receive the same attention. The presentation of sexual hybridization is supported by numerous precise examples of agricultural or horticultural plant varieties obtained in experimental stations in Romania. It also describes intense breeding activity in various experimental stations. Even for fruit species, the area of expertise for Michurin's gardening practice, sexual hybridization was always detailed and recommended, with amendments deriving from the principle of the education of young plants (abundant or poor food in the early stages of development).

This approach, mixing heredity and developmental physiology, is quite similar to that used in the USSR between 1920 and 1930, before the radicalization of Lysenkoism.[61] However, for the most problematic methods, and especially for vegetative hybridization, the examples were drawn from the Soviet writings. The only indigenous examples were those provided by the "very promising" work of Rudolf Palocsay in Cluj on the distant hybridization of cherry and the vegetative hybridization of peaches and vegetables.[62]

Despite the rhetoric of those books, the area of plant breeding seems to contain two distinct parts: one theoretical and one practical.[63] The first paid tribute to Lysenko with numerous references to Soviet agronomic works; the second focused exclusively on conventional plant breeding techniques dating from the previous century. This dichotomy raises the issue of the differences distinguishing the import of Lysenkoist practice and theory. This discrepancy continued throughout the period of the thaw. It had the effect of allowing effective plant breeding while avoiding open criticism of Lysenko. Thus, the national principle of continuity was highlighted in the practical aspects.

My second example focuses on historical biological works and the historical introductory chapters in scientific biological books, many of which were published in the late 1950s and early 1960s. These included monographs of Romanian scientists working in the nineteenth and first half of the twentieth centuries or republications of their selected works.[64] These books, written by biologists, provided an opportunity to present the history and tradition of their discipline as they preferred. The monographic works mentioned above were marked by a desire to find a national continuity of science before and after World War II, and an epistemological identity for Romanian biology. Nicolae Botnariuc, in his 1961 *Din istoria biologiei generale* (History of General biology) clearly explains that his book will address "current problems" of biology.[65] History, he wrote, enables us to locate the "correct" orientation of biological science.

> Contemporary biology is tormented by numerous difficult and controversial issues, which have many economic, philosophical and political implications. Among these are: the relationship between the organism and the environment, the problems of heredity and variability, the question of the relationship between ontogeny and phylogeny. I firmly believe that the correct approach to these problems cannot be found without knowledge of the past, of their history.[66]

It was therefore a matter of finding in the history of scientific commentary on the origin, organization and evolution of living beings some answers to the controversy between East and West in the field of biology. More specifically, the goal was to select arguments in favor of the position taken by Romanian biologists. Botnariuc, like other authors, tried to find in the writings of his predecessors all the elements of similarity with their own biological thought. Lysenko himself uses the same pattern of exploitation

of the past. He selected isolated aspects from the works of Lamarck and Darwin as arguments for his "creative Darwinism," and used the work of Michurin to underpin his theoretical and ideological dogmas. Similarly, Romanian biologists searched for a conceptual continuity between the neo-Lamarckism of the past, and the Lysenkoism of the present, to define their position in the long-term history of biology. According to them, the import of Lysenkoism was not a break from tradition, an effect of political change, or a distortion of science by ideology, but a step in the progress of knowledge. This use of history to justify the present[67] had significant consequences. It extended the impact of Lysenkoism and made its eventual rejection more difficult. After 1961, the writings of Romanian biologists no longer referred to Lysenko. However, Michurin was regularly referred to for several more years (Fig. 2).

Accounts of the history of biology also anchored science in Romanian nationalism. This nationalist trend became more powerful, in all areas of culture, after 1960, and especially after 1964, when Gheorghe Gheorghiu-Dej and his successor, Nicolae Ceausescu, showed a strong tendency toward political autonomy in the Eastern bloc. The instruments of the Sovietization, implemented in 1945–1948, were dismantled in 1963–1964: closing the publishing house "Russian Book," the reorganization of ARLUS and the dislocation of the Pedagogical Institute Gorky.[68] Gheorghiu-Dej and Ceausescu promoted a relative (and temporary) decline in the influence of the state on science.[69]

This distancing from the Soviet model was reflected in biology by the return of genetics. Was this "returning" as radical as it might seem? After

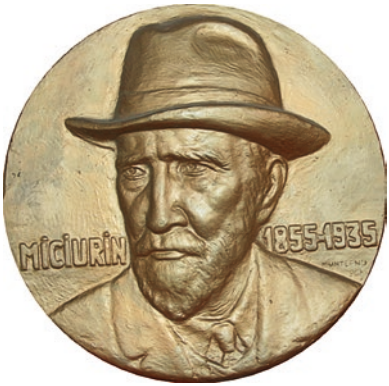

Fig. 2 Gheorghe Munteanu, Miciurin/Michurin, 1963 (bas-relief gilded plaster). © Didona Oghina

1962, many works bearing the title *Genetica* (Genetics) were published. Petre Raicu, author of one of the first books on genetics, introduced it with a *Short History of the Emergence and Development of Genetics*.[70] Lysenko was, as expected, completely absent from this book. However, Michurin's works were described on several pages. Raicu qualified Michurin as "one of the most remarkable Darwinists, who discovered the most important laws of general biology, which made possible the directed transformation of heredity of organisms." Meanwhile Weismann's theory was described as "mystical and idealistic," and Mendel's laws were presented as a simple special case of the segregation of traits in peas.

This historical overview, with its endorsement of Michurin over Weismann and Mendel, was a stark contrast to the content found in the rest of the book, which was based on genetics. This can be interpreted in two ways. The first is that the theoretical foundations of neo-Lamarckism were fully assimilated by Romanian scientists. Michurin's work, which in no way contradicted neo-Lamarckism, was naturally defended in the post-Lysenko period. Their initial readings of Western authors were through the lens of neo-Lamarckism, which allowed them to reconcile Michurinism and genetics. The second is that they simply continued the practice, as did authors writing introductions to books across numerous fields, of making requisite reference to Soviet authorities. They were familiar to epistemological contradiction between the introduction and other chapters of their books.

In 1964, the first symposium of genetics was organized in Bucharest.[71] The program consisted of 17 reports on general issues of heredity and 55 papers grouped into sections on cytogenetics, radio-genetics, sexual hybridization and heterosis and hereditary pathology. The introductory report was presented by Petre Raicu, Stefan Milcu[72] and Nicolae Giosan.[73] This was a significant disciplinary configuration: genetics, medicine and agronomy were equally represented. But it was also a significant political configuration because alongside a biologist (Raicu), were the vice-president of the RPR Academy (Milcu) and the vice-president of the Superior Council of Agriculture (Giosan). This indicates that the symposium was approved by state authorities. The introductory report, "Orientations of Current research in Genetics," opened with a critical presentation of Morgan's works and an appreciative description of Michurin's. As regards Romanian genetics, the authors emphasized the tremendous support that the communist state had provided to works on genetics in Romania since 1944! This rhetorical endorsement of genetics

was not entirely new, as some authors had participated in the International Congress of Genetics in Holland the previous year, as well as other scientific meetings in Western countries.

Later editions of Raicu's book, published in 1964, 1966 and 1980, were purged of references to Michurin. However, the return to genetics after 1965 required annual revision of university courses. Between 1965 and 1980, there were more than 70 books on genetics and, among them, more than 50 university textbooks. But the ambiguity of the first works in genetics suggests a real difficulty in making the new scientific thought coherent. This difficulty may have several explanations. A lot had happened in Western biology in the intervening years which Romanian authors had to assimilate quickly. Furthermore, the coexistence of several generations of biologists trained before and after World War II made a patchwork of views on heredity and evolution. Dissimilar epistemological paradigms co-existed.

Also, abandoning Lysenkoism did not mean rejecting Marxist ideology. Biologists continued their endeavor to make the scientific content of their work compatible with the principles of dialectical materialism, to defend the very concepts that the theory had only recently been used to oppose. These difficulties and inconsistencies were generally confined to the introductory chapters. But their presence indicates that the context of the production of scientific knowledge has remained the same.

As for Michurinism, its longer term legacy is reflected in a 1975 publication by Emil Pop and Radu Codreanu, *Istoria stiintelor in Romania—Biologia* (History of Sciences in Romania—Biology).[74] The authors tried, in the new scientific and political context, to find corresponding arguments in science policy. Concerning the development of biology after World War II, Pop and Codreanu noticed that in this period the new regime introduced the collective planned and coordinated work in all fields, in agriculture as well as in science. Even at this late period, Michurinism still received mild support:

> It should be emphasized that the development of genetics was hampered for some time by adopting Michurinism unreservedly. This adoption caused damaging exaggerations and provoked a crisis in genetic research in our country. It should nevertheless be noted that adopting this Michurinist approach did not have only negative effects. First, this Michurinist trend attracted the attention of Romanian researchers to the importance of environmental conditions on the modification of the heredity. Second, it allowed some practical applications, useful in agriculture and breeding.[75]

The authors were biologists. How they wrote the history of genetics and Michurinism, reflects their scientific priorities. They were familiar with neo-Darwinism and neo-Lamarckism, as interpreted by Racovita, confronted with the political and ideological turn after the War, and involved in the national synthesis of these epistemologies. They defended the continuity of Romanian science in general terms, while also focusing on their own particular interests.

The two themes the authors tried to highlight were the value of national achievements and the international recognition of Romanian scientists. By the mid-1970s, the Ceausescu regime was seeking both national identity and international openness without, however, denying the national past, both recent or more distant. The official position in the writing of national history was to focus on continuity and to minimize break times. The Lysenkoist period in the Romanian history of biology was treated in the same way. It was not openly and fully criticized. According to the Romanian historians of science, in the 1970s, although some "damaging exaggerations" was to be deplored during the Stalinist period, it was important to recognize the "practical achievements" made by the Michurinism. The central idea was to give an overall positive vision of national science.

Conclusion

The abandonment of Lysenkoism followed the major dates of the chronology in other satellite countries. However, in Romania, Michurinism was preserved and never truly denied. Why? The answer to this question highlights the acculturation of Lysenkoism in Romania.

This can be attributed to the overlapping generations of scientists active before, during and after Lysenko's theories were introduced in Romanian biology. Students of neo-Lamarckian professors in France or Romania drew upon a multitude of references. Lysenkoism reminded Romanian biologists of their lessons in neo-Lamarckism and the heredity of acquired characters, stadial development, and the influence of the environment on the adaptation of organisms. It would be mistaken to say that they simply mixed neo-Lamarckism with Lysenkoism, and thus deny any political, repressive and ideological scope to the latter. The fact is that they made an original synthesis, which became operative in the thaw period, when Michurin replaced Lysenko as the authority to be cited. Once freed from ideological dogma, Michurinism (without Lysenko) became acceptable in

the neo-Lamarckian epistemological system. Its application in Romania was thus integrated into the national history of biology and agronomy.

This allegiance to the past benefited scientists who were key players in the Sovietization of Romanian biology. Traian Savulescu's image is not tainted by his association with Lysenkoism.[76] The history of science, as part of the nationalist narrative under Ceausescu, had a crucial impact on the legacy of Lysenkoism. Homage events, such as Racovita's commemoration in 1968,[77] were an opportunity to reaffirm the position of Romanian biologists in the tradition of their "great predecessors." To understand the nature of the Romanian Lysenkoism, it is necessary to take into account the Romanian obsession for consistency in all areas of Romanian culture. This is related to a cultural, rather than a scientific, trend: the prevalence of historical continuity in moments of rupture.

Notes

1. "Une mise au pas, toute politique, celle des savants roumains, sommés à leur tour de se placer sous le joug de la science d'État soviétique." Letter from Roland Barthes, cultural attache to the Legation of France in Romania, entitled "Politicization of Science in Romania," July 21, 1949, no. 665, addressed to CMB (Direction générale des relations culturelles), MAE (Ministère des affaires étrangères), CMB, 1948–1955, Education, 155. Cf. Annie Guénard, "De la reconstruction à l'éviction. Entre 1944 et 1949, une politique culturelle française en Europe centrale et orientale confrontée à l'organisation du Bloc communiste," in *Matériaux pour l'histoire de notre temps* (Paris, 1994), N° 36, pp. 21–27.
2. William deJong-Lambert and Nikolai Krementsov, "On Labels and Issues: The Lysenko Controversy and the Cold War," *Journal of the History of Biology* 45 (2012), pp. 373–388, Springer, 2011, p. 379.
3. ARLUS was founded in 1944. The acronym stands for *Asociatia Română pentru strângerea legăturilor cu Uniunea Sovietica*.
4. ARLUS, *Activitatea în cifre si imagini* (Bucuresti: Imprimeriile Independenta, 1945). Cf. Adrian Cioroianu, *Pe umerii lui Marx. O introducere in istoria comunismului românesc* (Bucuresti: Editia a II-a, 2007), pp. 107–148.
5. Michael David-Fox, "From Illusory 'Society' to Intellectual 'Public': VOKS, International Travel and Party: Intelligentsia Relations in the Interwar Period," *Contemporary European History* 11, no. 1, Special Issue: "Patronage, Personal Networks and the Party-State: Everyday Life in the Cultural Sphere in Communist Russia and East Central Europe," February 2002, pp. 7–32. Nikolai Krementsov, "A 'Second Front' in Soviet Genetics: The International Dimension of the Lysenko Controversy, 1944–1947," *Journal of the History of Biology* 29, no. 2 (Summer 1996), Springer, pp. 229–250.

6. In 1953, ARLUS pretend to have 4.900.000 members and 20.000 sections. Cf. F.F. (sic), "Soviet Cultural Collaboration: The Role of the Friendship Societies in the Satellite States," *The World Today* 10, no. 5 (May 1954), pp. 197–209. Published by: Royal Institute of International Affairs, Article Stable URL: http://www.jstor.org/stable/40392730
7. The Romanian historian Adrian Cioroianu proposed a four-category classification of Romanian intellectuals who had actively contributed to the Sovietization of the country: the Communists by vocation, the Social Democratic supporters, the intellectuals who were afraid and those who were not politically engaged but showing political naivety or weakness. Adrian Cioroianu, *op.cit.*, p. 277.
8. Two vice-presidents were doctors who had only recently become involved in politics. The first, Daniel Danielopolu (1884–1955), was permanent secretary of the Romanian Academy of Medicine, who became Minister of Health from December 1944 to March 1945. The second was Communist Party member Dimitrie Bagdasar (1893–1946), a neurosurgeon who served as Minister of Health in the Petru Groza government from 1945 to 1946. The general secretary of ARLUS, Simion Oeriu (1902–1976), was a biochemist who became Secretary of State and General Commissioner in the Commission of the Armistice under the Groza regime as well. The president of the ARLUS' sub-section on higher education was a socialist sympathizer, Constantin Motas (1891–1980), a zoologist and Doctor of the University of Grenoble, France.
9. According Valeria Iuga-Raica, author of a Savulescu biography published in 1971, he kept in his private archives a rich correspondence with 300 foreign scientists. *Traian Savulescu (1889–1961)* (Bucuresti: Editura stiintifica, 1971), p. 139.
10. That same year he received an award acknowledging his 25 years of service as an educator. His biographies mention his participation in the supervisory committee of a German journal, the *Phytopathologische Zeitschrift*, in 1942. Traian Savulescu, *Bio-bibliografie*, Cuvant inainte de academician Alice Savulescu, Bucuresti, Universitatea Bucuresti, Biblioteca centrala universitara, Bucuresti, 1968.
11. Dumitru Sandru, *Comunizarea societatii romanesti in anii 1944–1947* (Bucuresti: Editura Enciclopedica, 2007), p. 62.
12. *Moscova vazuta de Mihail Sadoveanu, Prof. Mitita Constantinescu, Prof. Traian Savulescu* (Bucuresti: Cartea Rusa, 1945).
13. Academia Républicii Populare Romane, *Omagiul lui Traian Savulescu cu prilejul implinirii a 70 de ani* (Bucuresti: Editura Academiei Republicii Populare Romane, 1959), pp. xv–xxx.
14. K.A. Timireazev, *Metoda istorica in biologie. Cu o prezentare a autorului de Traian Savulescu* (Bucuresti: Universul, 1946).

15. Traian Savulescu, *Academia de stiinte in URSS in diferite timpuri*, Coll. ARLUS, n° 1, 1945.
16. Dumitru Şandru, *Reforma agrară din 1945 în România* (Bucuresti: Institutul National pentru Studiul Totalitarismului, 2000).
17. Lucretiu Patrascanu, founder member of the Communist Party in 1922, Minister of Justice since 1944 was removed under pressure of Ana Pauker and Gheorghe Gheorghiu-Dej in february 1948. He was accused of having fallen under the influence of the bourgeoisie and being a Titoist-fascist agent and a spy in the service of Western intelligence services. He was arrested and imprisoned until his execution in 1954. About Patrascanu and more generally about violence, repression and victims of this period, see Comisia prezidentiala pentru analiza dictaturii communiste din România, *Raport final*, 2006 (Raportul Tismaneanu), Bucuresti, 2006.
18. Norman Naimark, "The Sovietization of the Eastern Europe," in *The Cambidge History of the Cold War*, Vol. 1, *Origins*, eds. Melvyn P. Leffler and Odd Arne Westad (Cambridge: Cambridge University Press, 2010), p. 195.
19. *Analele Academiei Române*, seria II, Vol. LXVII (Bucuresti: Academia Româna, 1948–1949), pp. 353–354.
20. Nikolai Krementsov, "Lysenkoism in Europe. Export-Import of the Soviet Model," *Academia Upheaval: Origins, Transfers and Transformations of the Communist Academic Regime in Russia and East-Central Europe*, eds. Michael David-Fox and Gyorgy Peteri (New York: Garland Publishing Group, 2008), pp. 179–202.
21. See articles under the title: "Anul 1948—instiututionalizarea comunismului," *Analele Sighet,* no. 6, Fundatia Academia Civica, 1998 and the chapter "Academia Prizoniera," in *Istoria Academiei Române (1866–2006)*, ed. Dan Berindei (Bucuresti: Editura Academiei, 2006).
22. Traian Savulescu, *Stiinta, literatura, arta si slujitorii lor in Republica Populara Romana, Raport prezentat in ziua de 18 Octomvrie 1948, cu prilejul deschideruii celeui dintai Sesiuni generale a Academiei Republicii Populare Romane,* Monitorul Oficial, 1948.
23. Eugen Denize and Cezar Mâta, *România comunista. Statul si propaganda* (Târgoviste: Editura Cetatea de Scaun, 2005), pp. 32–34.
24. E. Coiciu, C. Ilchievici, N. Constantinescu and C. Perpeliuc.
25. The papers were grouped under the title "Cuceririle stiintei sovitice pe tarâmul agriculturii în desbateri la Institutul de cercetari agronomice" / The achievements of Soviet science in the field of agriculture in the debate of the Institute for Agricultural Research, in *Studii. Revista de stiinta si filozofie*, An II, n° 2 (aprilie–iunie 1949), pp. 64–132.
26. Decretul nr.83 din 2 martie 1949 pentru completarea unor dispoziţiuni din legea nr.187 din 1945, *Buletinul Oficial* nr. 1/2 mar. 1949.
27. Vladimir Tismaneanu, *Stalinism for All Seasons: A Political History of Romanian Communism* (University of California Press, 2003), pp. 108–110.

28. The revision of the Penal Code on January 19, 1949 also toughened penalties for crimes of treason, conspiracy against the social order, spying, etc.
29. Traian Savulescu, "De la practica domesticirii plantelor la principii biologice generale. Reflexiuni pe margineau raportului lui T.D. Lyssenko," in *Analele Academiei R.P.R. Sectia de Stiinte biologice*, Seria A, Tom 2 (Bucuresti: Academia RPR, 1949), 70 p.
30. One should note the coincidental with the conference which took place in Poland on March 30, 1949 on the new biology and the speech of the President of the Polish Academy of Sciences, Jan. Dembowski, himself director of the Institute of Experimental Biology. *Cf.* William deJong-Lambert, "Lyssenkoism in Poland," *Journal of History of Biology* 45 (2012), p. 502.
31. Nikolai Krementsov, *Stalinist Science* (Princeton, NJ, 1997).
32. *For a Proper Orientation of Scientific Activity in the Rumanian People's Republic*. Resolution of the Presidium of the Academy of the R.P.R. June 28, 1949. Report of the Section of Medical Science of the Academy of the R.P.R., Report by Professor Traian Savulescu, President of the Academy of the R.P.R. s.d.
33. *Ibid*. This question had been addressed to Romanian artists by the Soviet sculptor Vera Mukhina three years earlier when she visited Bucharest.
34. "La campagne très ample menée contre la revue d'Ophtalmologie et le *cosmopolitisme* atteint ou va atteindre bien de savants roumains (…). Mais le cas de ces savants est à tout prendre moins tragique que celui des intellectuels roumains qui s'asservissent totalement aux impératifs soviétiques et renchérissent sur les mots d'ordre imposés. Tel est le cas de l'auteur de ce second rapport, le Président de l'Académie RPR, Trajan Savulescu: cet universitaire, qui doit toute sa formation à la science française, n'a pas hésité à prononcer contre les savants occidentaux un réquisitoire tout aussi indigne que les flagorneries grossières adressées sans fin à la science soviétique," Roland Barthes, *loc.cit.*, p. 21. On the question of the formation of Savulescu, R Barthes was misinformed because he never did study abroad.
35. Ministère des Affaires étrangères (France), Centre des archives diplomatiques de Nantes (CADN), Bucarest Ambassade, 343, *Rapport sur l'activité de l'Institut français et de la Mission universitaire française en Roumanie pendant l'année universitaire 1944–1945.* Le 8 avril 1945.
36. CADN, Bucarest Ambassade, 343, Le directeur de l'Institut français de Hautes Etudes en Roumanie au ministre de France, le 4 décembre 1947.
37. CADN, Bucarest Ambassade, 343. *Directeur de l'institut Français de Bucarest à la Direction générale des relations culturelles.* Le 18 novembre 1947.
38. André Godin, *Une passion roumaine. Histoire de l'Institut Français de Hautes Etudes en Roumanie (1924–1948)* (Paris: L'Harmattan, 1998).

39. François Chaubet and Laurent Martin, *Histoire des relations culturelles dans le monde contemporain* (Paris: Armand Colin, 2011), p. 119.
40. Universitatea din Cluj, Insitutul de speologie, Legea de infiintare promulgata in 26 aprilie 1920, Contractul din 18 decembrie 1919, memoriul din 14 decembrie 1919. *Cf.* http://www.emil-racovita.ro/speologie
41. Emil G. Racovita, *Evolutia si problemele ei* (Cluj, Editat de subsectia Eugenica si biopolitica a "ASTREI," 1929), pp. 54–55.
42. Graduated of the Herastrau (Bucharest) School of Agriculture, Stefan Popescu taught in Kishinev (1934–1941), Timisoara (1941) and Iasi (1941–1947). In 1947 he was appointed director of the plant breeding station of Iasi and professor at the Faculty of Agronomy. Member of the National Peasant Party (NTP) before the war, he enrolled in the Romanian Workers' Party (PMR) in 1947 but he was excluded from the party "at the first verification" of his personal file. He was forced to retire in 1949, despite numerous letters addressed to the Dean of the faculty in which he asked to be reinstated in his teaching duties. Arhivele Statului filiala Iasi, Institutul Agronomic Iasi, 2168 /Professors/ Nominatives files/ S. Popescu/1916.
43. Stefan Popescu, *Curs de ameliorarea plantelor si genetica* (Iasi: Editura Politechnicei "Gh. Asachi," Facultatea de Agronomie, 1947), p. 26.
44. Stefan Popescu, *Curs de ameliorarea plantelor si genetica* (Iasi: Editura Politechnicei "Gh. Asachi," Facultatea de Agronomie, 1947), pp. 9–26.
45. Loison Laurent, *Qu'est-ce que le néolamarckisme? Les biologistes français et la question de l'évolution des espèces* (Paris: Vuibert, 2011), and private communication.
46. Dimitrie Voinov had studied in Paris, in 1899–1893, with Lacaze-Duthiers, Delage and Pruvot. Professor of Chair of animal morphology at the Faculty of Science of Bucharest, he published articles on the work of Weismann in 1893–1894. He made neo-Lamarckian analyses about the environment as a factor for species transformation on abyss fauna. His works were published in Paris in "Annales de zoologie expérimentale et générale," "Comptes rendus hebdomadaires des séances de la Société de Biologie." He was elected as a member of the Romanian Academy in 1927. His eviction from the RPR Academy in 1948 was motivated by his advanced age (81 years). It is relevant to emphasis that Voinov was Savulescu's professor and his main protector at the beginning of his career.
47. Romanian students, whose families cannot afford to pay for their stay in France, demanded a scholarship from Romanian or French government. Savulescu was probably in the latter case. Racovita already had an established reputation in Parisian scientific circles and numerous scientists, not just biologists, testify to the valuable help he could give them, supporting their candidacy for the French scholarship. Savulescu met Racovita in Paris and kept resentment about the failure of his application.

48. Traian Savulescu, "Origine de quelques espèces de *Campanula* des Carpathes, des Montagnes de la Péninsule Balkanique et de l'Asie Mineure," Extrait de *Bulletin de la section scientifique de l'Académie Roumaine* (Bucarest: Academia Româna, Cartea româneasca, 1924), 15 p.
49. Traian Savulescu, "Baza genetica a evolutiei fiintelor vii," in *Materia si viata* (Bucuresti: Imprimeria nationala, 1944).
50. Traian Savulescu, "De la practica domesticirii plantelor la principii biologice generale. Reflexiuni pe margineau raportului lui T.D. Lyssenko," in *Analele Academiei R.P.R. Sectia de Stiinte biologice*, Seria A, Tom 2 (Bucuresti: Academia RPR, 1949), 70 p.
51. He quoted N.Vavilov again, not to criticize him but to defend the Soviet biology, proof of his very approximate knowledge of the situation in the Soviet science.
52. *Op.cit*., p. 26.
53. Mandatory courses were taught in all research and education institutions, even at the RPR Academy, on Marxism-Leninism. The Romanian poet and philosopher Lucian Blaga described how he felt after 11-hour or 12-hour days, of being obliged to listen to lectures on Marxism-Leninism as a member of the faculty at the University of Cluj: "I finally understand that professors must suspend their own brain activity and attempt the impossible: to think with the brain of the 'classics of Marxism'. The change amounts to a grafting operation. We have to shave the gray matter of our brains as we shave the hair at the hairdresser. To accept a standard brain, as a transplant." *Cf.* Eugen Denize and Cezar Mâta, *Romania comunista. Statul si propaganda (1948–1953)* (Targoviste: Editura Cetatea de Scaun, 2005), p. 108.
54. Stefan Bosomitu and Mihai Burcea (coord.), *Spectrele lui Dej. Incursiuni in biografia si regimul unui dictator* (Institutul de investigare a crimelor comnismului si memoria exilului romanesc) (Iasi: Editura Polirom, 2012), pp. 25, 192.
55. Comisia prezidentiala pentru analiza dictaturii communiste din România, *Raport final* (Bucuresti, 2006), p. 346.
56. As was the case in Poland, William deJong-Lambert, "Lyssenkoism in Poland," *loc.cit*., pp. 512–515.
57. Traian Savulescu, *90 de ani de viata academica in tara noastra, Lucrarile sesiunii stiintifice a Academiei RPR, 2–6 iunie 1956* (Bucuresti: Editura Academiei RPR, 1956), p. 69.
58. N. Zamfirescu, V. Velican, and Gh. Valuta, *Fitotehnia*, Vol. I–II (Bucuresti: Editura Agro-Silvica de Stat, 1956–1958).
59. M.I. Neagu, *Selectia plantelor horti-viticole* (Bucuresti: Ministerul Agriculturii, Editura Agro-Silvica de Stat, 1960). N. Ceapoiu and A.S. Potlog, *Ameliorarea plantelor agricole*, 2 Vol. (Bucuresti: Ministerul Agriculturii, Editura Agro-Silvica de Stat, 1960).

60. Nichifor Ceapoiu (1911–1994), one of the authors, was, from 1938, a researcher at the Plant Breeding Station in Cluj and Textile Plants Station in Bucharest. After 1949 he worked in the Romanian Institute of Agronomic Research (ICAR), while teaching at the Faculty of Agronomy of Bucharest. He was a specialist in hemp studies and selection. Before 1960, he carried out an active selection work of the hemp, but also studies on the hybrids, phenomenon of heterosis and sexual bipotentiality of the hemp. Stelian Oprescu, *Inaintasi ai geneticii in Romania* (Bucuresti: Editura CERES, 1983), pp. 48–53.
61. Nils Ross-Hansen, *The Lyssenko Effect. The Politics of Science* (New York: Humanity Books, 2005), pp. 23–28.
62. Rudolf Palocsay, *Experientele mele. Noi realizari in aplicarea miciurinismului* (Bucuresti: Editura Agro-Silvica de Stat, 1955).
63. Nils Roll-Hansen, "Wishful Science: The Persistence of T.D. Lysenko's Agrobiology in the Politics of Science," *Osiris* 23, no. 1, "Intelligentsia Science: The Russian Century 1860–1960" (2008), pp. 166–188.
64. Some examples: *Victor Babes, volum omagial* (Bucuresti: Editura de Stat, 1949); Paula Albu, *Emil Racovta si conceptia sa biologica* (Bucuresti: Editura Agro-Silvica de Stat, 1955). Stefan Milcu, *Viata si opéra lui Emil Racovita* (Bucuresti: Biblioteca Academiei RPR, séria de biobibliografii, 1956); Liviu Floda et ali, *Gheorghe Marinescu* (Editura Tineretului, 1958), Gheorghe V. Radu, *Viata si opera stiintifica a lui Dimitrie Voino* (Bucuresti: Biblioteca Academiei RPR, séria de biobibliografii, 1956); Victor Babes, *Opere alese*, Vol. I–II (Bucuresti: Editura Academiei RPR, 1954–1950).
65. Nicolae Botnariuc, *Din istoria biologiei generale*. Capitolul *Din istoria biologiei romanesti* a fost scris de S. Ghita (Bucuresti: Editura Stiintifica, 1961). Nicolae Botnariuc (1915–2005) studied natural sciences in Bucharest. Zoologist and taxonomist, he was Professor of General Biology at the University of Bucharest (1948–1983) and Director of the Laboratory of Systematic and evolution of animals (1949–1973). He was received at the Romanian Academy after the fall of the communist regime in 1990.
66. Botnariuc, *op.cit.*, 1961, pp. 7–8.
67. The Romanian historian Lucian Boia, questioning the use of the past by the communist regime employed the expression: "imaginary historical roots." According to him, the past, reduced to the role of myth, was multifunctional. It can be used, as needed, to argue anything and everything. Hence the search for precursors, unique characters or isolated incidents that are used to demonstrate everything and its opposite. Lucian Boia, *La mythologie scientifique du communisme*, Nouvelle édition revue et augmentée (Paris: Les Belles Lettres, 2000), p. 98.
68. *Cf.* Irina Gridan, "Du communisme national au national-communisme. Réactions à la soviétisation dans la Roumanie des années 1960," *Vingtième siècle. Revue d'histoire*, no. 109 (2011/1), pp. 113–127.

69. Dan Catanus, "The Illusion of Normality; The Romanian Academy at the Beginning of Nicolae Ceausescu's Regime," in *Totalitarianism Archives* (1–2/2012), pp. 109–112.
70. Petre Raicu, *Metode noi în genetică* (Bucuresti: Editura didactica si pedagogica, 1962). Petre Raicu (1929–1988), professor at the University of Bucharest, was also a researcher at the Institute of Biology of the RPR Academy. Raicu had obtained his doctorate in 1955 and from 1961 to 1962 he followed a specialization course in Paris.
71. Ministerul Invăţământului, *Cercetări de genetică: lucrările primului Simpozion Naţional de Genetică (18–20 iunie 1964)* (Bucuresti: Editura Didactică si Pedagogică, 1965).
72. Stefan Milcu (1903–1997), endocrinologist, was a professor at the Faculty of Medicine of Bucharest. He was appointed member of the RPR Academy in 1948. In 1964, he was director of the Institute of Endocrinology in Bucharest and vice-president of the Academy RSR.
73. Nicolae Giosan (1921–1990) obtained his doctorate in Agronomy in 1952 in Moscow. He was a researcher and professor at the Agronomic Institute of Bucharest (1953–1974). In 1964 he was Vice-President of the Superior Council of Agriculture. Between 1974 and 1989 he was President of the Grand National Assembly. He was imprisoned during the Ceausescu's fall in December 1989 and he died in prison few months later.
74. Emil Pop and Radu Codreanu, eds., *Istoria stiintelor din Romania: Biologia* (Bucuresti: Editura Academiei RSR, 1975). Emil Pop (1897–1974), the lead author, was a biologist. After attending university in Cluj, he taught plant physiology and plant geography at the same university. In the 1930s he conducted study expeditions in Austria, Switzerland, France, Belgium, England, as well as Germany in 1943. After World War II, he was temporarily removed from the university and appointed director of the botanical garden, but he quickly returned to education. In 1950 the professors of Marxism at the University of Cluj criticized his course on plant physiology before a meeting of the teaching staff. They accused him of lacking knowledge of scientific Marxism, making little use of Soviet scientific literature and well appreciating too many references to Western scientists. Despite this incident, his international reputation earned him appointment to the Romanian Academy in 1955, where for many years he directed the Biological Sciences Section. Radu Codreanu (1904–1987), the second author, was a biologist too. After studying in Bucharest, he became, in 1924, the assistant of Dimitrie Voinov at the University of Bucharest until 1930, when he was recruited at the Institute of Speleology in Cluj. He thus became one of Racovita's closer collaborators. He obtained a scholarship from the Romanian Academy and a French scholarship to do a State doctorate thesis in France (1939) on invertebrate parasitism under the direction of Maurice Caullery in the Laboratory of evolution

in Paris. In 1940, he was appointed substitute professor and, in 1942 full professor of the chair of General Biology from the University of Cluj (in refuge in Timisoara), where he took over from Racovita. Supported by Savulescu, he became professor at the University of Bucharest in 1945 and focused on issues of evolution and systematic in zoology. He was one of the instigators of commemorative events in the honor of Racovita: the bust of the Romanian biologist was offered by the Romanian Government to the Arago Laboratory in Banyuls sur Mer in France in 1965, and a major international conference was organized in his memory in Bucharest in 1968 under the auspices of UNESCO. He was elected as correspondent member of the Academy of the Socialis Republic of Romania in 1974 *Cf.* Alexandru Marinescu, "Profesorul Radu Codreanu (1904–1987)," *Travaux du Muséum d'Histoire Naturelle "Grigore Antipa,"* 29 (1987), pp. 367–370.

75. *Ibid*., p. 203.
76. His name was given in the 1960' to the Center of biological research in Bucharest and to two agricultural schools. The "Traian Savulescu prize" is always awarded nowadays by the Romanian Academy to reward works in biology and agronomy. Biographies and articles concerning Savulescu emphasize his scientific merits his role in the creation of the Romanian school of Plant Pathology and the organization of agricultural research. Some biographers focus on his high position in the Academy and in the state. According to them, this position allowed Savulescu to protect his pupils and collaborators at the Romanian Institute of Agricultural Research from purges. "Tr. Savulescu—a famous phytopathologist and botanist concerned with genetic problems" in Stelian Oprescu, *op.cit.,* pp. 63–67; Radu Iftimovici, *Creatie româneasca in biologia universala* (Bucuresti: Albatros, 1977), pp. 360–362.
77. Orghidan Traian (sous la dir.), *Livre du centenaire Emil Racovitza,* (1868–1947) (Bucarest: Editions de l'Académie de la République Socialiste de Roumanie, 1970).

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H.J. Muller and J.B.S. Haldane: Eugenics and Lysenkoism

William deJong-Lambert

On December 30, 1948 J.B.S. Haldane participated in a radio program broadcast by the BBC on what had become a cause célèbre of Cold War science—the Lysenko controversy. The other three participants—S.C. Harland, C.D. Darlington and R.A. Fisher—were also all respected geneticists. The difference between Harland, Darlington, Fisher and Haldane was that while the first three were eager to weigh in by denouncing T.D. Lysenko as a fraud, Haldane was not. By this time two things were clear. One, the anti-genetics campaign launched by Lysenko had replaced eugenics—especially its German variant, *Rassenhygiene*, implemented in Nazi Germany during the 1930s—as a focus of concern among activist biologists in the USA and Great Britain.[1] Two, Haldane was apparently the only geneticist unwilling to outright condemn Lysenko as a charlatan who had nothing to offer to contemporary studies in evolution and heredity.[2] Lysenko's power, so the story went, was purely the product of state interference in science, an outcome portrayed as inevitable in a country where the official doctrine—Marxism—presumed science as a central

W. deJong-Lambert (✉)
Bronx Community College, CUNY, Bronx, NY, USA

Columbia University, New York, NY, USA

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component of state ideology, and thus necessarily subject to the dictates of the central governing authority, the Communist Party.

Yet behind this story lies a far more nuanced chronology, showing how Haldane was inadvertently led into the role of Lysenko martyr by Hermann J. Muller, perhaps his generation of geneticists' most fervent advocate of the notion that a better world could be bred to order through human selective breeding. Muller's fantasies for this project were mapped out along the lines of other would-be technocrats of his generation, such as Julian Huxley: a society composed of genetically superior individuals who, in reproducing their kind, would raise civilization to a whole new level.[3] The question of how geneticists should treat eugenics was a central issue among geneticists planning the Seventh International Congress of Genetics, planned for Moscow in 1937, but ultimately held in Edinburgh in 1939. At the Eighth International Congress of Genetics, the disagreement over Lysenkoism was already apparent between Muller and Haldane.[4] What happened two weeks later at the VASKhNIL session would make Lysenkoism replace eugenics as the problem among biologists.

In this chapter I analyze this history in terms of Haldane's relationship with Muller, the Nobel Prize-winning geneticist who had sought Haldane's—and Joseph Stalin's—support for his socialist eugenics plan in the previous decade. As I will show, the most curious aspect of these events is the speed with which Muller went from actively courting Haldane's endorsement for his eugenics program, to deliberately deceiving him about his role in Lysenko's consolidation of control over the Lenin All-Union Academy of Agricultural Sciences (VASKhNIL), following a meeting on December 19–27, 1936. My purpose is twofold: one, to chart the course whereby Lysenkoism replaced eugenics; two, to show how Muller successfully steered Haldane into the role he himself would have otherwise occupied—most celebrated geneticist famously duped by the promise of Marxist science.

This chapter is divided into four parts. In the first part, I describe Haldane's views on eugenics as chronicled in sources ranging from his boyhood diary at Eton, to numerous articles and essays in the decades that followed. Among the most obvious themes recurrent in these writings is that Haldane, like Muller, viewed the USA as—next to Nazi Germany—the most nefarious player in the game of trying to use biology as a prop for supposedly scientific beliefs, in fact rooted in racism and class prejudice.

In the second part, I describe Muller's views on eugenics, first expressed in a student essay from his undergraduate years, that evolved into his

notorious address at the 1932 Third International Eugenics Congress in New York City and his 1935 manifesto, *Out of the Night*, as well as his opinions expressed in private correspondence. What is most remarkable is the extent to which, despite the fact that Haldane's beliefs were clearly so different from his own, Muller continually courted the latter as a like-mind and publicly portrayed him as such.

In the third, I chronicle the pivot point—Muller's hasty exit from the Soviet Union in 1937—as he sought to cover up his role in inadvertently speeding up the demise of Soviet genetics, and how he deliberately hid any knowledge of this from Haldane. Though Muller can certainly not be blamed for the cancellation of the Seventh International Congress of Genetics, his correspondence with Julian S. Huxley, another leading light of British evolutionary thought and an ardent supporter of eugenics, clearly indicates that he was mulling over how his actions had contributed to the outcome. This raises the question of why—having kept Haldane in the loop on the fate of the congress up to that point—he now wanted to keep him in the dark, directly instructing Huxley that Haldane must now "not be informed."

In the fourth, I cover the landmark moment in Haldane's role in the Lysenko controversy, the December 1948 BBC broadcast which was interpreted by geneticists, as well as many members of the public at large, as an unqualified defense of Lysenko's theories. The transcript of the debate was subsequently published as a book edited by a journalist, John Langdon-Davies, titled *Russia Puts the Clock Back*. Haldane was presented as the sole defender of Lysenko in the subtitle—"Langdon-Davies Versus Haldane"—which in turn was featured above a quote from a prominent member of the British scientific establishment, Sir Henry Dale, who had recently resigned from the Russian Academy of Sciences in protest: "A terrible indictment" it read. Exactly who, or what, was being "indict[ed]" was left unclear.[5]

I argue that this episode, wherein Haldane found himself set up as the only advocate of an apparent communist tyrant in biology, must be considered in context with his relationship with Muller. As I will show, in the previous decade Muller had routinely used Haldane by presenting the latter's views on eugenics as similar to his own, despite the fact that they were anything but. I will also argue that Haldane's position on Lysenko was not nearly so simple as it was portrayed to be by Muller and others. Though most geneticists had resisted criticizing Lysenko before the August 1948 VASKhNIL session, out of concern for how this

might negatively affect their Soviet colleagues, they were now pressured to loudly denounce Lysenko as a charlatan. Meanwhile Haldane had no good reason to not follow his normal inclination to challenge the majority by keeping his mind open to all options, and join in the nearly uniform, public condemnation of Lysenko among his geneticist colleagues, because Muller had not told him what was going on. Thus we are left to wonder how Haldane's response would have been interpreted in an environment where scientists were not immediately required to unreservedly condemn Lysenko, as well as how he would have reacted had Muller told him what he had told Huxley.

Part I: J.B.S. Haldane and Eugenics

Among the more telling anecdotes in J.B.S. Haldane's family history is that his sister Naomi claimed he joined the British Communist Party because he felt guilty for being born upper class.[6] Haldane was the descendent of a distinguished line that included artists, aristocrats, politicians and celebrated scientists such as his father, John Scott Haldane, known within the family as "Uffer." Uffer's research focused on the physiology of respiration, and he frequently conducted experiments upon himself in a home laboratory, breathing poisonous gases to determine better methods for protecting miners of coal and tin. Uffer also used his son, known within the family as "Boy," as a test subject in his experiments, and during the First World War they collaborated on designing the gas mask used by British troops.

Haldane became good friends with another descendant of scientific royalty, Julian Huxley, grandson of Darwin's "bulldog" T.H. Huxley, when the Huxleys relocated to Oxford in 1913. Huxley and Haldane would remain good friends, rivals and colleagues their entire life. What is notable, however, in terms of the narrative which follows is that despite their friendship Huxley would, following the instructions of his other close friend, Hermann J. Muller, deliberately keep Haldane in the dark about events in Soviet biology by the late 1930s.

Haldane was first introduced to genetics when Uffer took him to a lecture on Mendelism by A.D. Darbishire at the Oxford University Junior Scientific Club.[7] He would go on to be one of three figures, including Sewall Wright and R.A. Fischer, who would found the field of population genetics and provide the mathematical evidence that Darwin's theory of natural selection could explain evolution in nature. Despite becoming one of the most important biologists in what Huxley termed the "Modern

Synthesis" of genetics and Darwinism, Haldane never received a scientific degree. Rather he studied Greats, a field devoted exclusively to the study of classical literature. Haldane's fluent knowledge of the classics would provide him the narratives he would use to compose some of his most compelling works as a popular science author, including *Daedalus: Or a Science of the Future*, a dark satire of eugenics that would inspire Aldous Huxley's *Brave New World*.

Haldane's early interest in eugenics is evident from his boyhood diary at Eton, wherein he wrote: "Eugenics will be pretty awful for the unfit, though really no worse than the way we treat them now."[8] This precocious view of what the desire to breed better humans would mean for the progress of human civilization and the equality of mankind, would develop into the conviction that, however desirable in principle, elimination of the "unfit" was a pointless project which could not, given current knowledge of genetics and heredity, have a positive impact upon human evolution.

Haldane's skepticism toward eugenics is most clearly evident in the above-mentioned *Daedalus*, published in 1923.[9] By this time Haldane was a celebrated war hero, famous for manufacturing his own bombs and so fearless of death that he put his own men in danger.[10] It is important to highlight the extent to which Haldane's experiences in the trenches fostered unconventional views—such as the relative safety of gas warfare—outlined in his next controversial opus, *Callinicus: A Defence of Chemical Warfare*, published two years later.[11] Both works reflect a recurrent theme in his writing, relevant both to eugenics and his later position on Lysenko: Popular skepticism or suspicion of scientific advances resulted from the fact that the public was relatively ignorant of how science worked and affected their lives. They also underpinned the increasingly popular image of Haldane as a man with a mind so open he was willing to give even the most apparently outlandish claims due consideration, best expressed in his belief that "the universe is not only queerer than we suppose, but queerer than we *can* suppose."[12]

As for what can be found on the pages of *Daedalus*, most evident is the extent to which the recently ended Great War left him with a dim view that science is necessarily a positive force in human culture. He begins the book with a grim memory of battle, wherein the combatants appear "irrelevant" amid the "great black and yellow masses of smoke" that "[tear] up the surface of the earth with almost visible hatred."[13] Haldane spends the first half of the book considering every aspect of human endeavor—art, industry and each science except biology—in terms of two questions:

"Has mankind," in pursuing scientific progress, "released from the womb of matter a Demogorgon which is already beginning to turn against him," or have we become "a mere parasite of machinery, an appendage to the reproductive system of huge and complicated engines which will successively usurp [our] activities, and end by ousting [us] from the mastery of this planet?"[14] In other words, is science something that will ultimately result in self-extinction, or simply remove humanity from our central place in nature, reducing us to a class enslaved by our own inventions?

Once Haldane addresses these questions in terms of reproduction and our relationship with nature, he warns that "biological invention … tends to begin as a perversion and end as a ritual supported by unquestioned beliefs and prejudices."[15] He frames his prediction of what will initially seem perverse yet soon be taken for granted in terms of an essay to be published by an Oxford undergraduate 150 years hence, which describes the invention in 1951 of ectogenesis—the possibility of developing an embryo outside the womb.[16] By the time Haldane's fictitious essay is produced, ectogenesis has become "universal" in Great Britain.[17] Haldane goes on to predict the workings of a society wherein a small population of "undoubtedly superior" individuals are selected for reproduction and the resultant impact upon politics, public health, psychology and religion.[18] He warns that we must "beware … him in whom reason has become the greatest and most terrible of the passions," and concludes that "[t]he scientific worker of the future will more and more resemble the lonely figure of Daedalus as he becomes conscious of his ghastly mission and proud of it."[19]

Haldane's grim, satirical account was a huge sensation, selling nearly 15,000 copies in its first year despite its relatively expensive price.[20] The book also, along with numerous articles and essays Haldane would publish in popular magazines and the press, launched him on a career as a popular science writer. One collection of these writings, *Possible Worlds: A Scientist Looks at Science*, published five years after *Daedalus*, contains a more lucid account of Haldane's opinion of eugenics.

"Eugenics and Social Reform," like *Daedalus*, begins with the shadow of Haldane's experiences during the First World War. In this case he compares the fact that mustard gas, "the most humane weapon ever invented," has been banned, while "the growing science of heredity is being used in this country to support the political opinions of the extreme right, and in America by some of the most ferocious enemies of human liberty."[21]

Haldane defines the two major issues as "hereditary abnormalities or tendencies to disease" and "the inheritance of intelligence and the different

birth rate in different social classes." He addresses both in terms of economic inequality, claiming not only that it would take hundreds of generations to "eliminate hereditary feeblemindedness," but that a "feeble-minded girl" would be "quite as likely to harm her contemporaries by transmitting venereal disease, and her children by negligence, as to be responsible for the idiocy of future generations." The chief problem in Haldane's view was that any legislation adopted would "probably be applied unjustly to the poor," increasing their suffering, which would keep them in poverty, which in turn exacerbates the problem the policy was intended to address—the differential birth rate.[22]

At this point Haldane enlists a familiar trope in his writing, which is to say something he knows will sound shocking to grab his reader's attention: improve human heredity by promoting social equality.

> It therefore follows that any measures which tend to disseminate heritable property among the poor, such as the breaking up of large estates, are eugenically desirable. So are drastic improvements in our elementary education and in our scholarship system. … If I attached the importance to eugenics which certain people claim to do, I should, I think, be bound to advocate the complete abolition of hereditary property, and the free and compulsory attendance of all children at State schools.[23]

The points Haldane addressed in his brief essay received book-length treatment in his 1938 publication, *Heredity and Politics.* Haldane begins by stating that though the two countries in the world where eugenics has had the greatest influence are Germany and the USA, he will focus on the latter in order to emphasize that "such abuses may occur under a legal system based on English law, and carried out under the criticism of a press somewhat freer than our own."[24] He proceeds to discuss what he calls five "doctrines" of the US eugenics system: (1) the "unfit should be sterilized"; (2) the question of whether the "unfit" "continue their kind"; (3) class inequality and the differential birthrate; (4) "that certain races are congenitally superior to others"; (5) "crossing between different races is harmful."[25]

Haldane's characteristic wit is evident throughout the text. For example, when describing the classifications listed under the "American Model Sterilization Law," he notes that the population of "blind," "deaf," and "homeless, tramps, and paupers," would mean sterilizing Milton, Beethoven and Jesus, respectively.[26] When quoting a 1910 Report of the

Committee of the Eugenics Society which states that those undeserving of Poor Law relief include individuals "born without any independence," he comments: "In my own experience the majority of new-born infants are devoid of this quality."[27] With regard to the fourth doctrine, racial superiority, he points out that, since it first appears in the Book of Genesis, it could be considered "originally a Jewish doctrine."[28] In terms of the differential birth rate, he notes that clearly—in Darwinian terms—the wealthy are less "fit" since they are less likely to reproduce, and concludes: "If the rich are infertile because they are rich, they might become less so if they were made less rich."[29]

Haldane also addresses the practical problem of whether or not our knowledge of genetics is sufficient to develop a science of eugenics. His answer, based upon his mathematical calculations, is that it could not. Even worse, not only would "the premature application of our rather scanty knowledge … yield little result," it would "merely serve to discredit the branch of science in which I am working [i.e., genetics-W.dJL]."[30]

Toward the end of the text he specifically addresses Muller's plan, as outlined in *Out of the Night*, and concludes: "Once again I am inclined to regard such a proposal as possibly premature in view of our very slight knowledge of the genetical basis of those characters which are found in the 'great men' whom we regard as admirable."[31]

It is with all of the above in mind that we must consider why Muller continually solicited Haldane's support for his eugenic views, cited his writings on eugenics as evidence for his own, but then chose to exclude him from any knowledge of his own role in the cancellation of the Seventh International Congress of Genetics and the rise of Lysenko. Had Haldane known, he certainly would not have stumbled into the position he found himself in once Lysenko seized control of Soviet biology.

Part II: H.J. Muller, Eugenics and Lysenkoism

Hermann J. Muller's background could not have been more different than Haldane's. Unlike Haldane, who was essentially bred from birth to become a scientist, Muller grew up in a working-class family in German-Irish Harlem, and was only admitted to Columbia University thanks to his high score on a citywide exam after graduating from public high school in the Bronx. Muller soon became a member of Thomas Hunt Morgan's famous "fly room" at Columbia, so named thanks to the focus of Morgan and his students' research, the domestic fruit fly *Drosophila melanogas-*

ter. The relationships between Morgan and his students, including Alfred Sturtevant and Calvin Bridges, as well as their relationships among one another, were notoriously fraught.[32] Jealousy over who contributed what, as well as accusations (coming almost exclusively from Muller) over misplaced credit for various discoveries and ideas, were rampant. Muller's reputation for having a "priority complex," that is, being overwhelmingly concerned with guarding his reputation and legacy, is a relevant detail to take into account when analyzing his behavior toward Haldane.[33]

Muller's closest friend in this period was Edgar J. Altenburg, who joined Muller at Columbia after studying for a year at City College to the north. According to Altenburg, he and Muller "traded in the three R's for the three S's—science, sex, and socialism," during their undergraduate years at Columbia.[34] The combination of these underpinned Muller's notion of how eugenics could be developed.

A talk delivered at a meeting of the campus Peithologian Society in 1910 evinces Muller's convictions and ambitions for his nascent eugenic plan. "[T]he way to eliminate the unfit is to keep them from being born," he stated, adding that "[w]ith knowledge of the laws of nature comes power to manipulate them, and knowledge of life thus means the perfection of man."[35] Muller would maintain these beliefs, albeit according to different ideas of how they could be achieved, for the rest of his life.

After completing his degree, Muller joined Huxley in the biology department of the newly founded William Marsh Rice Institute (Rice University) in 1914. Though Huxley soon returned to England as a result of the First World War, the bond formed in Houston was enduring. In 1920 Muller later relocated to the University of Texas at Austin where he continued to develop his ideas on eugenics, and also continued his research on X-rays as a potential source of genetic mutations. He conducted the experiment that would win him the Nobel Prize in 1926, and in 1930, he was joined in his lab by two aspiring Soviet geneticists, funded by the Rockefeller Foundation, Israel J. Agol and Solomon Levit, who were interested in Muller's work in genetics and eugenics. He also continued to become increasingly attracted to socialism, and by 1932 had become involved with a socialist student organization that was illegal on campus. Muller's activities soon attracted the attention of the FBI who informed campus authorities.[36] Fortunately for all concerned, Muller was offered a Guggenheim Fellowship that same year to work at the Kaiser Wilhelm Institute for Brain Research in Berlin, from where he would soon relocate to the Soviet Union.

Before departing for Europe, Muller delivered an extraordinarily controversial address at the Third International Eugenics Congress at the American Museum of Natural History in New York City. Muller's presentation is notable for several reasons, not the least of which is that the program of the congress essentially chronicled the deteriorating relationship between geneticists and eugenicists. A decade earlier, at the Second International Eugenics Congress, held in the same venue, geneticists and eugenicists, united by mutual interest in human heredity, presented their research alongside one another. The composition of the third congress was different in that virtually no geneticists showed up. Though the organizers attempted to arrange a luncheon with the geneticists, who would be passing through town on their way to the Sixth International Congress of Genetics in Ithaca, it seems the former were far more interested in getting together than the latter.[37] Muller's address also stands out in that if it were up to the man in charge, Charles Davenport, Muller would probably have never presented at all.

When Muller sent Davenport a copy of his paper, Davenport was appalled. Muller's talk was an anti-capitalist tirade wherein he essentially argued that capitalism was dysgenic. Davenport initially reduced the amount of time allotted to Muller from one hour to fifteen minutes, and from there cut it down to ten. Muller, realizing he was being censored, sent copies of his presentation to other geneticists who he presumed would be sympathetic.[38] One of those he wrote was to Haldane, who replied,

> Your article on eugenics appears to me to be rather moderate, provided you are a convinced socialist. I am an unconvinced one (i.e. I vote socialist on probability). But I should be inclined to go further than you, and say that capitalism was almost bound to be dysgenic.[39]

Despite Haldane's opinion that Muller's paper was "moderate," it hit like bombshell. Headlines in newspapers the next day like—"HOLDS CAPITALISM BARS EUGENIC GOAL—Prof. Muller of Texas Asserts Profit Motive is Inimical to Welfare of Race—DENIES SUCCESS YARDSTICK—Leader of Finance or Politics Mentally Akin to Gangster, He Tells World Congress—AGAINST BIRTH SUBSIDIES—Not Charity, but Society Organized to Assure Economic Plenty to All, is Needed, Paper Says"—essentially summed up what he had said.[40]

Among the geneticists in town for the Ithaca meeting, who would not be present at the Museum of Natural History, was Haldane. However

expressing his disapproval by simply not giving a paper could not be enough for a voluble personality like Haldane. It was his first time in the USA and he wanted to be noticed. Haldane's opinions on eugenics appeared in the New York *Tribune* on the last day of the congress, that is, one day before stories on Muller's address. Haldane was quoted saying,

> The eugenicists … talk loosely about the fit and the unfit, but the fittest in the Darwinian sense are people on the verge of being feebleminded. One-half of 1 percent of the people couldn't fit into the social system under this classification. I see the problem differently—to try to make a society into which as many people as possible can be fitted. It is easier to alter the social organization than to alter human beings.[41]

At this point it is worth pausing to note that though Haldane's letter to Muller does not necessarily read as though the author was opposed to eugenics, his statements in the newspaper most certainly do.[42] They also reflect the skepticism evident in *Daedalus*, not to mention what he had expressed in his article, "Eugenics and Social Reform." Though *Heredity and Politics* would not appear for six more years—after Muller had departed the USSR—it is fair to ask if Muller could really have been under the impression that he and Haldane were of the same mind when it came to the practicality of human breeding.

As Muller arrived in Europe, he wrote to Altenburg that the celebrated Russian geneticist Nikolai Vavilov was eager for him to visit the Soviet Union, and asked if news of his eugenics address had reached Austin.

> Vavilov told me I'd get all my expenses paid if I visited Russia, but I don't want to do that for 6 or 8 months yet at least. If you visit Austin … pretend you know nothing, and pick up what gossip you can about my status there. By the way, did news of my eugenics talk get into the Texas papers, and if so, what sort?[43]

Altenburg was reading a draft of *Out of the Night*, and Muller wrote to him of the challenges he anticipated making his plan acceptable to the broader public. He predicted that the "social mechanism," that is, the practical program to be implemented, would be developed over time, just like other aspects of socialist construction, such as state control of industry. In the short term, he predicted that "the selective process may get a start partly, at least, through the action, and example set by, a relatively few of the more capable women."[44]

What is most striking in Muller's comments to Altenburg is the extent to which he was willing to initially deceive people, in order to achieve what he viewed as a vital long-term goal. The "capable women," referred to above, would ultimately need to accept the idea that their role in the reproductive process would be controlled by the government. Though the plan could initially be presented as offering "more free choice," and women would be told they would be acting "voluntarily," "the word 'voluntary' is …, elastic."

> It will, at the present stage, repel too many people, to tell them that women's reproduction will be controlled socially by forces outside of them; better lay emphasis on the better opportunity women will have to have better children, and to choose their sperm well … and then say there will be agencies evolved to give them better and better guidance (or advice, or something equivalent) in this matter.[45]

The passage quoted above is notable for two reasons: One, it reflects precisely the conviction Haldane expressed in *Daedalus* that biological advances which start as shocking eventually seem normal; two, the extent to which Muller felt absolutely no qualms about misleading people ("'voluntary' is, however, elastic") and hiding his motives.

It is ironic that Muller referred to Haldane directly in his next letter, and his comment is revealing. After recommending Altenburg read Haldane's recently published magnum opus, *The Causes of Evolution*, he wrote that among the points he disagreed with is that "man is more childlike: I think Haldane must have had a long childhood himself."[46] Muller's joke shows that as much as he appreciated Haldane's scientific work, he seems to have also, on a certain level, viewed him with condescension.

Muller's willingness to shade the truth in order to get what he wanted became more evident once he arrived in the Soviet Union and laid the groundwork to get Stalin to support his eugenic plan. Two concurrent trends became central to the achievement of Muller's goals. The first was the growing influence of eugenics in Nazi Germany, the second was the rising prominence of Lysenko, who would increasingly identify genetics and eugenics—in its most troubling aspects—as indistinguishable. These two factors came to a head most explicitly in the context of planning for the Seventh International Congress of Genetics.

The details surrounding the cancellation of the Genetics Congress have been well-documented elsewhere.[47] Though research indicates concerns that the organizers were unprepared, as well as the burgeoning Great

Terror were the primary reasons for the meeting's cancellation, Muller's role—particularly his perception of how his actions affected the congress and his career—is important in context with his behavior toward Haldane. In May 1936, seeking to take advantage of the Academy Presidium's decision to allow eugenics to be addressed in the proceedings, Muller sent Stalin a copy of his manifesto, *Out of the Night*.[48]

In his book, Muller not only cited Haldane's *Daedalus* in such a way as to imply that Haldane endorsed—as opposed to satirized—ectogenesis, but he even sought Haldane's endorsement for the cover.[49] There is no record of whether or not Stalin read Haldane's brief summary of Muller's book, but if he had it probably would not have mitigated his negative reaction. If anything, it would have exacerbated it. The text read as follows,

> You may regard it as a revelation, or, quite as likely, throw it into the fire. But do not dismiss it as a mere phantasy. The author is one of the world's leading biologists, and his proposals, whether or not they are desirable, are entirely practicable. If they are adopted, the results will be as important as those of the industrial revolution.

Muller clearly did not pause to wonder whether suggesting readers throw your book into the fire, or propose that the ideas outlined therein might be un-"desirable," really counted as an endorsement, because he followed up by asking Haldane to review it in *Nature*. Muller told Haldane that, "I feel that a word from you would play a very decisive part in the manner of reception of the ideas, not only in England but everywhere."[50]

However Muller's book might have been received elsewhere, it did not go over well with Stalin. Muller wrote that by inseminating women with the seed of the "most transcendentally superior individuals," it would be possible to make a "considerable step" in improving the population of the Soviet Union within a single generation. He went on that in 20 years—"if … capitalism still exists beyond our borders"—this "vital wealth in our youthful cadres" "could not fail to be of considerable advantage to our side."[51]

Muller soon learned that his book did not receive the reception he envisioned. As he later wrote to Huxley, Muller heard that Stalin was "displeased" and had "ordered an attack against it."[52] In the meanwhile, plans for holding the Seventh International Congress of Genetics in Moscow had begun to falter—in part due to the question of how, or if, eugenics would be treated in the program.[53] Several US geneticists wished to use the occasion to denounce Nazi theories on race; however, Muller would soon use the December VASKhNIL meeting to pronounce his socialist-eugenic

alternative. For whatever reason—and despite the fact that eugenics had been effectively banned in the Soviet Union for nearly a decade—Muller could not accept that this line would be totally unacceptable.[54]

On November 13, Solomon Levit—who was becoming increasingly suspect for his interests in eugenics—was publicly denounced as an advocate of Nazi doctrines. The next day the Politburo issued a secret decision to cancel the congress.[55] On December 4, Muller wrote to Haldane breaking the bad news. "I am writing this only unofficially," he confided, "as my understanding is that the Program and Organization Committees no longer exist."[56] Ten days later the story, possibly at Muller's instigation, appeared in the *New York Times*, in an article highlighting Muller's involvement. As the reporter wrote, "Americans had a special interest in the congress because Professor Herman (*sic*) J. Muller of the University of Texas, who during a four-year leave here has attracted worldwide attention with his experiments on mutations of the fruit fly, was chairman of the program committee."[57]

The *Times* followed up with an editorial three days later, in which they implied that the cancellation was due to the desire of dictators to control science.

> …Geneticists concede that their science is young, that it is not yet possible to control genes (units of heredity) so rigorously that human beings of the right social type can be created to order. Yet the dictators of communist and totalitarian States rush in where these cautious men still feel their way.[58]

Obviously the *Times* editorialist had not read Muller's letter to Stalin. If he had, he would have realized that, in fact, the story was exactly the opposite. It was the scientist—not the dictator—who had believed human types could be "created to order," and that this was, in fact, part of why the congress was not going forward.

The controversy soon boiled over in a VASKhNIL session held during December 19–27, pitting Muller, Vavilov and several Soviet geneticists against Lysenko and his followers. This session may be regarded as a precursor to the notorious July 31 to August 7 session twelve years later, in that genetics was essentially being put on trial. Among the more shocking facts is that, despite the fact that Israel Agol, another of Muller's former students from Texas, was arrested on the first day of the meeting, Muller still went ahead and discussed his plan for a socialist eugenics. This proved unfortunate. According to what Muller later wrote to Huxley,

> At the end of my anti-Lamarckist, anti-Lyssenko (*sic*) address, I called attention to the pro-Fascist race and class implications of Lamarckism, since if true it would imply the genetic inferiority, at present, of people and classes that had lived under conditions giving less opportunity for mental and physical development. The audience applauded wildly, but there was a terrific storm higher up and I was forced to make a public apology, while the statement I had made was omitted from the published address.[59]

On the last day of the meeting Muller attempted to save the situation by invoking the name of a geneticist who he believed could give greater credibility to his side: Haldane. It sounds, according to a transcript of the proceedings, that he staged the thing ahead of time with Vavilov. Muller announced from the stage that he had received a letter from Haldane, who was on his way to help fight Fascism in the Spanish Civil War. Muller referred to Haldane as "the greatest geneticist in England," and emphasized: "We did invite him to be one of the major speakers to the international congress that is planned to take place next year."[60] The fact that Muller's last ditch attempt to defend his position by using Haldane to demonstrate that not only were geneticists anti-Fascist, but they were actively engaged in fighting Fascism in Spain, is ironic for reasons that will become increasingly clear.

Part III: "Haldane, Especially, Must Not Be Informed…"

Muller realized he would be better off leaving the Soviet Union—at least for a while—to repair his image and salvage his work in genetics. As he wrote in a letter to Huxley, "problems arising out of the questions disputed at the Dec. meeting," had taken over the research agenda at his institute, and would occupy his time for at least a year or two.[61] Once again, as Muller told Huxley, he had sought out Haldane's advice and decided to go to Spain.[62] Just over a week later Muller wrote to Haldane to confirm his plans, which he requested the latter keep confidential.[63]

Muller departed the USSR and took a train to France from where he would enter Spain. The letter he wrote to Huxley on his way is a fascinating document in that everything about it—that it goes on for eight pages, includes numerous asterisk-marked digressions, writing up and down both sides of the paper to fill in details of various episodes as they occurred to him later—evinces Muller's anxiety at this juncture. He went over all the

details of what had forced him to flee; however, his greatest concern was how these events would be interpreted. It was on this point—particularly the notion that his eugenic ambitions would be seen as a source of folly—that he seemed most insecure.

> … I am not willing to retreat completely in the latter idea. Nor can I sit by quietly to see it connected, in the minds of the social progressives and radicals, with the reactionary social doctrines and movements that they are fighting against.

Muller worried that to "retreat" now would allow his opponents in the USSR to label his ideas "bourgeois" and "counterrevolutionary," which in turn would influence "an important section of progressive thought" in the West against them as well. Even worse, he would be seen "as just another 'bourgeois idealist' who failed to accept the realities of the revolution, and expected a paradise and then, in reaction, become 'anti-Soviet.'"[64]

Though what Muller described is, more or less, what ended up happening the process would take many years. It would also be his good fortune that the Cold War would begin in the meanwhile, making his "retreat" seem, in retrospect, prescient. He would be seen as a man who—unlike the likes of Haldane—had learned from experience.

In the meanwhile, the only way to prevent what had happened from being "misinterpreted," would be to make sure it was only discussed in a context he could control. He told Huxley he did "not want to become an agent of anti-Soviet propaganda," adding: "What I have told you are only facts, they cannot be appraised without taking them in connection with the favorable facts concerning the U.S.S.R. and its system." The problem was that the "mass of people" could "hardly see two facts at a time and so these facts might have a dangerous effect upon them." "When they are finally given out," Muller said, "it must be in just the right setting."[65]

By "just the right setting" we can presume Muller meant conditions in which he would not seem to have been wrong. Muller would soon receive a letter from one of his old rivals in the fly room, Calvin Bridges, which would force him to recognize that—at least in certain circles—his worst fears had been realized. On the train to France, however, Muller seems to have decided that his greatest threat was Haldane. He wrote to Huxley,

> Haldane, especially, must <u>not</u> be informed—not now, anyway—for I judge from the tone and content of his letters to me that he is at present having

> his political opinions impressed upon him with a rubber stamp (greatly as I admire his intellect and person), and would be influenced in the reverse direction from that which I intended. He would think I had gone over the conservative or fascist camp, which is the very impression I am trying to disprove.

"Haldane, especially, must not be informed." At this point we are justified in asking why Muller did not want to tell Haldane. After all, Muller had sought Haldane's backing on his 1932 eugenics address in New York City, requested his endorsement and review for *Out of the Night*, confided in him about the cancellation of the genetics congress in Moscow, invoked his name at a key point in his showdown with Lysenko, and then asked for his "confidential" advice on going to Spain. So why couldn't he tell him what was going on now? Did not Haldane deserve the truth? What was Muller afraid of?

A week later Muller wrote to Huxley again, repeating his plea to keep silent about what had transpired. It sounds as though Muller's fear of humiliation continued to weigh on him and had sunk deeper.

> What a rotten business it is when one's mouth is gagged—what misunderstandings, minor and major, it leads to! Again, this all-or-none reaction working. But it isn't I who started this business of giving only one side of the facts, and my associates expect me to follow their method, otherwise I'm quite "out" with them at once, and those things which I'm trying to further with them will acquire a bad odor for them and for all those who are under their influence to some degree.

The "facts," according to Muller, were the real problem. He was not yet ready to tell all, but if he revealed "a part of the facts now" it would "make people anti-Soviet, which would in turn help the Fascists." The time would come to tell the facts "fully," "so that they are seen in proper relation and perspective," "but that cannot be now…" Therefore, Muller concluded, "until I am ready to discuss the matter openly all-round … it is indispensable that I should not be quoted. Such statements should not be made as that I am 'disillusioned' etc."[66]

It is worth noting that the "major fact," in Muller's mind, was that "those things" which he was "trying to further"—that is, eugenics—would "acquire a bad odor."[67] What upset him most was the idea that people would say Muller realized he had been foolish to emigrate to the Soviet Union. That, more than anything, is what really seemed to disturb him.

Muller's first hint that the proverbial cat was out of the bag must have been after he arrived in Spain, when he received the above-mentioned letter from Bridges. Muller's intense dislike for Bridges would have made the letter even more upsetting.[68] Bridges said, given that just the summer before Muller had expressed concern that war would interrupt the progress of science in the USSR, it seemed strange that he had taken time away from his work to go fight Franco and the Fascists. "I interpret it as meaning," Bridges wrote,

> …that you were already in difficulties with the science end? On account of the crisis between the environmental and genetical points of view precipitated by Lysenko? You of course are the most prominent of the genetics "chess players" and I would expect you to be a target for attack. … I heard that you lost your party card in the controversy.[69]

Now Muller knew for sure that—at least in certain circles—the debacle was being discussed.

But just because Bridges knew did not mean Haldane did. In this case Muller would have little to fear. Communications across the Atlantic were not what they are today, information circulated more slowly. For example, the man who would finally offer Muller a job, Francis Crew at Edinburgh University, was so unaware of how Muller's sojourn in the Soviet Union had affected his prospects, that he highlighted these experiences when endorsing Muller to US employers. Crew even went so far—much to Muller's frustration—as to recommend they get in touch with the University of Texas, from where Muller had resigned in disgrace.[70]

In the meanwhile, Muller continued to enlist Haldane in support of various causes. Five months after he had left the Soviet Union for good, Muller wrote Vavilov that though the Seventh International Genetics Congress was not going to be held in Moscow, he had convinced Haldane and L.C. Dunn that it should be.[71] And yet, on the subject of Lysenko, Muller continued to keep quiet. According to Haldane's first wife, Charlotte, Muller visited her and J.B.S. on his way to Edinburgh to start work with Crew in the fall of 1937. In her autobiography, *Truth Will Out*, Charlotte wrote that Muller "expressed acute disillusionment and dissatisfaction with the changing scientific line in the Soviet Union, and described to us, although with a certain degree of reticence, the peculiar developments in Soviet genetics; the gradual unfolding of the Party attack

on Vavilov, and the rise to power in genetical circles of one, I. Present." "I do not now recollect," she added, "whether at that time he mentioned the name of Lysenko."[72]

Charlotte's mention of Muller's "reticence" and lack of clarity on whether he mentioned Lysenko is notable because by the time she published the book, 1950, she and Haldane were bitterly divorced and she was actively seeking Muller's support for her work.[73] Charlotte had every motivation to remember the episode differently because her divorce was—at least in her account—partly the result of Haldane's refusal to believe the terrible things she told him about what was happening in the USSR after she visited in 1941. Haldane's willful ignorance of the truth would have seemed even more damning if he had also ignored Muller, but he did not. There was nothing to ignore because Muller was not "inform[ing]" Haldane of the reasons he departed the USSR.

And yet Muller continued to seek out Haldane on other matters. Less than a year after his visit in London, Muller got in touch with Haldane again. Muller had received word that Robert Merriman, the leader of the Abraham Lincoln Battalion, had been captured in Spain. Muller telegrammed Haldane for help organizing an appeal to have Merriman released.[74]

Of wider significance was what became known as the "geneticists' manifesto," written by Muller. The story of how Muller ended up with the happy task of composing the "party line" in genetics begins with another "blunder" on Crew's part, related to the fact that Muller was not telling him any more than he was telling Haldane. In a letter dated July 26, 1939, Vavilov wrote to Muller relating the unfortunate news that neither he, nor any of his colleagues, would be attending the congress in Edinburgh.[75] Crew, meanwhile, received an official letter from Vavilov, dated the same day, that Crew and Muller were convinced Vavilov had not written, but merely been forced to sign. While the tone of Muller's letter was frank and dejected, in the missive to Crew "Vavilov" essentially blamed the cancellation of the Moscow congress on Crew and the rest of the organizing committee.

Crew sent a copy of both letters, along with a working draft of an official reply to the letter they assumed Vavilov had not actually written, to Haldane.[76] It is clear Crew did not ask Muller's advice or permission when he did so, because a week later Haldane received a letter from Muller where he wrote thus:

> …It is fortunate that Crew now realizes he should not send any such letter to Vavilov as that which he proposed, and which he sent you and the other recorders a copy of. It's a misfortune it ever was sent out even to recorders, as he now realizes, but at least there's now no question of sending it further. The job of heading off similar adverse criticism from other ostensibly friendly quarters will not be an easy one though, hence the more solidly the more progressive elements among the geneticist can get together at this juncture—when the real fascists would be only too glad to try to exaggerate or widen any breach in our ranks the better.[77]

What Muller left out was that a "breach in our ranks" was already present, and it was between himself and Haldane. It would only grow wider.

The Second World War temporarily interrupted Lysenko's anti-genetics campaign; however, it would soon be renewed in the altered context of the Cold War. If Muller's eugenic ideas had been controversial before the War, afterward they were unthinkable. Muller finally returned to the USA without a job about one year after Edinburgh congress. Muller was finally hired by Amherst College where he stayed for a few unhappy years until his pre-War radicalism caught up with him and he was forced to leave.[78] Muller accepted an offer at Indiana University in 1945 where he would remain—transformed into a "better dead than red anti-communist"—for the rest of his career.[79] Once Muller was awarded the Nobel Prize in 1946, his reputation among his colleagues—despite past dalliances with dictators and totalitarian fantasies—seemed secure.

The war years were also difficult for Haldane, albeit for reasons far different from Muller's. While Muller found the teaching emphasis at Amherst stifling, Haldane actively engaged in a series of dangerous experiments testing conditions on submarines for the British Navy. This work is important because not only did it severely damage his health, but his mental state was deeply affected as well, thanks to the fact that he seems to have pressured his coworkers into adopting his practice of self-experimentation.[80] This is a topic that requires further investigation, but the upshot seems to be that Haldane was both physically and emotionally fragile thereafter.[81]

Though accounts varied as to Lysenko's current status and influence after the War, the issue was now addressed seriously in terms of a concerted effort to translate the work of Soviet geneticists into English, and investigate Lysenko's claims. The first comprehensive study of the latter, undertaken by P.S. Hudson and R.H. Richens at Cambridge University,

essentially concluded that until he published his results in such a way that they could be replicated, it was impossible to evaluate Lysenko's experiments.[82]

The antipode of Hudson and Richens was a translation of Lysenko's *Heredity and Its Variability*, organized by Theodosius Dobzhansky and L.C. Dunn at Columbia University. Whereas Hudson and Richens undertook a serious assessment of Lysenko's ideas, Dobzhansky and Dunn set out to expose him as a charlatan. While Dobzhansky and Dunn's project—because it was based upon the false assumption that simply reading a book by Lysenko would be enough to convince anyone that he was wrong—was a failure, it was also the first source of Lysenko-related tension between Haldane and his colleagues. As Dobzhansky finished up the translation in the summer of 1945, he and Dunn wrote to geneticists like Muller requesting them to write negative reviews. It is obvious that these were only intended to be "reviews" in the sense that their subject was Lysenko's book. In fact, Dunn wrote his before he even read it.[83]

Unfortunately for Haldane, he had a few more scruples. When Muller asked him to write a review, Haldane responded,

> I regret that I have not read Lysenko's book, and am therefore clearly not in a position to do anything about the matter. You will agree that publication in the U.S.A. does not mean availability in this country.[84]

Haldane's response might sound disingenuous, were it not for the fact that the distance between the USA and Great Britain was indeed a significant factor determining what got published and reviewed where during the period.[85] Nevertheless, Muller seized the opportunity to question Haldane's loyalty by writing to Milislav Demerec that he had "expected something like this."[86] The most astonishing thing about Muller's remark is that Muller himself did not publish a review.[87] This seems very odd. Why would Muller not take the opportunity to skewer Lysenko? It would seem likely the answer is related to the same one he had given Dunn two years earlier, when Dunn asked him to join the American-Soviet Science Society. The last thing Muller wished to do at this juncture was draw attention to his relationship with the Soviet Union.[88] As Muller had written to Huxley many years before, he wished to "retire from all such discussions" until he was "ready to discuss the matter openly all-round."[89] Such a moment would not come soon enough for Haldane.

Despite the efforts of US and British geneticists, Lysenko was finally able to declare victory at the July 31 to August 7, 1948, VASKhNIL session. The event was quickly seized upon in the West as an example of Soviet tyranny in the sciences, and the position of geneticists was closely scrutinized. Muller immediately resigned from the Russian Academy of Sciences, a gesture interpreted as a damning judgment of the current situation in Soviet biology. Muller's public image had now shifted from communist firebrand to cold warrior. If there was one biologist whose stance on Lysenko was as at least as much a focus as Muller's, it was Haldane's.

Part IV: The BBC "Debate"

Perhaps the most important sentences in the transcript of the BBC broadcast are Haldane's first two: "I find this discussion very odd for several reasons. The first is that I do not know what the other speakers have said."[90] Indeed it was not a debate. The four speakers were recorded separately. According to John Langdon-Davies, who placed the discussion at the center of his anti-Lysenko expose, *Russia Puts the Clock Back: Langdon-Davies versus Haldane*, "The B.B.C. took special precautions against possible murder by having each of the four conspirators record contributions under circumstances which insured against their meeting on the stairs."

Two points before proceeding. First, John Langdon-Davies was a journalist, not a geneticist. The idea that he would present himself as the direct opponent of Haldane on the Lysenko issue smacks of self-promotion. Second, the notion that Haldane, Harland, Darlington and Fisher—colleagues who were presumably quite aware of one another's professional qualifications—would have "murder[ed]" one another over Lysenko, is clearly just hype to sell books. Unfortunately, Langdon-Davies' hatchet job was the filter through which many would interpret Haldane's behavior.

Besides, Haldane's image among the scientific community and general public was already well-established by this time. He had joined the British Communist Party (BCP), in part to offset the scandal of his ex-wife, Charlotte, having quit. He became a high-profile member of the Politburo, playing the role of the left-wing radical, unafraid to criticize the USA on its own soil.[91] The same year Dobzhansky and Dunn's version of *Heredity and Its Variability* appeared, Haldane visited the USA on behalf of the BCP. Dobzhansky's account of Haldane's behavior, in a letter to Dunn, accurately portrays his image as an unreformed radical.

> Here is something to amuse you. I was invited … for a reception organized by the journal "Science and Society" to "meet Prof. J.B.S. Haldane." … The old man was in fine shape and his words were lapped up by those present. … To me it was indescribably funny to hear him say that "dialectics" has influenced "unconsciously" a number of workers in USA, especially Sewall Wright, whose approach to evolution is "almost Leninist." … Not so funny was to hear J.B.S. mildly defend Lysenko, and of course all present will from now on say that Lysenko is a great man because another great man says so![92]

Haldane's performance on another visit, one year later, makes this even clearer. He was immediately quoted in the US press that he would judge US capitalism by its ability to provide him 18 ½-inch collared shirts, and complained that the fruit fly samples he had brought for Dobzhansky would probably be detained on Ellis Island.[93] Dobzhansky, once again, was highly amused.

> The New York Times carried a long interview with Haldane where he says that his purpose of coming to U.S.A. is to find 18 ½ collar shirts which are not available in England, and that he will judge American civilization by its ability or inability to supply 18 ½ shirts! … However, after this foolish start Haldane has settled down…[94]

It is worth contrasting Dobzhansky's portrayals of Haldane with how he had now come to view Muller. Though Dobzhansky and Muller would continue to disagree on numerous issues—mostly related to eugenics—the latter had, in terms of his politics, clearly redeemed himself in the eyes of the former. In the same letter where we find the above account of Haldane, Dobzhansky also described how Muller had come across at conferences they had both attended recently.

> Incidentally, Muller was in splendid form, both at Boston and at Princeton. I never saw him so placid and self-possessed without being self-conscious.[95]

In other words, though to a certain extent Dobzhansky would always think of Muller as the "fire-eating communist" who had denounced his own country and set off for the "promising land" (*sic*), that is, the USSR, Dobzhansky could tell he was finally content, having won the Nobel Prize. It was now Haldane's role to play the radical.[96]

Thus that Haldane would be asked to weigh in on Lysenko is unsurprising. In correspondence dating as far back as 1946 till the BBC broadcast,

Haldane was regularly asked about his opinion on Lysenko's work and the fate of Vavilov.[97] On the latter point he remained silent; however, he was not at all unwilling to criticize Lysenko. For example, in a letter to a Czech correspondent Mikuláš Teich sent the month before the radio program, Haldane said Lysenko had "gone too far" in his criticism of Soviet geneticists and "thrown the baby out with the bathwater."[98] He was also impatient with Lysenko sympathizers in the British biology community who asked him to help organize a united defense of Lysenko.[99] He was more interested in learning more and discussing the strengths and weaknesses of Lysenko's claims than in establishing a party line.

Had the November 30, 1948 broadcast actually been a discussion between Haldane, Harland, Darlington and Fisher, then he would have gotten the chance to do so. However, as mentioned above, all four were recorded separately. As for how listeners responded to the broadcast, as opposed to the response portrayed in *Russia Puts the Clock Back*, the archival evidence indicates Haldane received numerous letters of support. Among these was one sent the next day by his sister, Naomi, and her comments are revealing.

> The broadcast last night was very interesting though it would have been much more worthwhile if you could all have seen one another's scripts. … Harland was convincing on the background and seemed to be trying to be fair. What he gave the impression of saying was that Lysenko is a second-rate man and how can he be expected to have first class ideas. And I remember you saying much the same about Lysenko yourself at one time. … the thing that worries people is whether or not Lysenko has or hasn't stopped other people who are doing good and useful work from continuing, and that must be answered sooner or later.[100]

Unfortunately for Haldane, the question of whether Lysenko "ha[d] or ha[d]n't stopped other people who are doing good and useful work from continuing" had long been answered, but Muller had not told him. At this point it is worth considering some reasons why. The most obvious reason would seem to be Haldane's above-mentioned volubility. He was, along with Huxley, a celebrity scientist in Great Britain. As Muller's train rolled through Europe on its way to Spain, he was mulling gloomily over how he would be judged outside the USSR for his role in the cancellation of the genetics congress and Lysenko's growing influence. He was also considering how, and where, he could continue his work, and he soon reached the

conclusion, if he hadn't already, that it would have to be somewhere other than the Soviet Union. The basic necessity of finding a new job made the issue of discretion all the more urgent.

An alternative explanation lies in the rest of the sentence after Muller stated Haldane must not be told of his misadventures. He said that Haldane was "having his political opinions impressed upon him with a rubber stamp," adding: "He would think I had gone over the conservative or fascist camp, which is the very impression I am trying to disprove." In other words, Haldane was a malleable intellect who was being so badly brainwashed by the liberal Left that he would ignore what Muller said and conclude he was a Fascist.

This is hard to accept. Had not Haldane stated as far back as 1932 that the "test of devotion" to science in the Soviet Union would come when they acknowledged "innate human inequality?"[101] It seems more likely Muller was concerned that Haldane would think about Muller's analysis of the situation in Soviet biology to that point, and conclude he had badly miscalculated. This would certainly not help Muller in his quest for redemption.

If we accept that Muller believed Haldane could not be trusted to keep quiet about anything Muller told him, we must also ask if he was correct. A relevant factor in this question is Muller's above-mentioned "priority complex." Any historian who spends time going through Muller's correspondence, or consulting other sources where he speaks about his research and that of his colleagues, would need to explain why they *don't* come away with the impression that Muller believed he was routinely robbed of proper credit for his ideas.[102] It is easy to see how this concern could translate into a fear of being maligned for notions he regretted. If secrecy and silence were his priorities after December 1936, the desire not to inform Haldane seems logical.

Was Muller right? Would Haldane have immediately denounced Lysenko and sounded the alarm about the imminent threat to Soviet genetics? Possibly—and if he did it would have been Muller, with regard to eugenics, rather than Haldane, with regard to Lysenko, who was put in the awkward position of having to explain his behavior. According to this formula Haldane's famous 1932 statement—quoted twice, for maximum effect, in *Russia Puts the Clock Back*—that the "test of devotion" to science in the Soviet Union would come when they acknowledged "innate human inequality," would be interpreted as prescient, rather than embarrassing.[103]

Conclusion

In this chapter I addressed the process by which Lysenkoism replaced eugenics as a controversial issue among geneticists from the early 1930s to the onset of the Cold War. To do so I analyzed the relationship between two geneticists at the center of both controversies—H.J. Muller and J.B.S. Haldane. I argued that though Haldane was useful to Muller in terms of promoting his eugenics program, once it was clear this plan was not to be realized, Muller grew concerned about the extent to which his attempts to pursue it had contributed to the cancellation of the Moscow genetics congress, his attitude toward Haldane shifted radically. At this point Muller, motivated by his fear that Haldane was a loose cannon who could not, like Huxley, be trusted to portray events exactly as Muller wished them described, chose to keep him in the dark.

The inglorious end to Muller's eugenic ambitions in the USSR coincided with the rise of T.D. Lysenko. By the end of World War II, eugenics, which had been such a pressing issue for geneticists in terms of how, or if, it should be treated, was rendered irrelevant by the evidence revealed behind the gates of liberated Nazi camps throughout East and Central Europe. This image in turn contributed to Lysenko's renewed influence, and his ability to portray genetics as eugenics by a different name. Once the Cold War got underway, Lysenko's anti-genetics campaign became the new focus among biologists concerned with the fate and reputation of their science at home and abroad. By now Muller and Haldane were barely on speaking terms, and the latter was happy to let the former follow his usual pattern of wishing to keep an open mind on every topic, thus hanging himself on the rope of Lysenko. My point is not that Haldane was right or wrong to defend Lysenko, only that Muller made sure he was ignorant, and this ignorance led Haldane to become a scapegoat for his contemporary's fascination with Soviet science.

Notes

1. For useful definition of the term "activist scientist" see Melinda Gormley, "Scientific Discrimination and the Activist Scientist: L.C. Dunn and Professionalization of Genetics and Human Genetics in the United States," *Journal of the History of Biology* 42, no. 1 (2009), pp. 33–72.
2. Columbia University geneticist Leslie Clarence Dunn also attempted to keep a relatively open mind about the merits of Lysenko's claims, however this is only apparent in his private correspondence on the subject with his

colleague Theodosius Dobzhansky. See for example, Theodosius Dobzhansky to L.C. Dunn, October 2, 1946. L.C. Dunn Papers, The American Philosophical Society (hereafter, APS).

3. Julian Huxley, *If I Were Dictator* (New York and London: Harper Brothers, 1934).
4. See p. 5 notes of Dr. S.N. Das Gupta, Counselor for Agricultural Sciences in the Natural Sciences Section of UNESCO, July 3, 1948, Box 409, Unesco Archives, Unesco, Paris. Gupta was sent in place of Huxley to attend the Stockholm Congress. Gupta writes: "The next item was the address by Prof. Muller the President of the Genetics Section: He spoke on "Genetics in the Scheme of Things." It was an able address covering a wide topic/field in the course of which he bitterly criticized both the Nazi and the present attitude of Russia with regards to genetics and the treatment meted out to Soviet scientists holding orthodox views on genetics, to which a somewhat sarcastic reference was made by Prof. J.B.S. Haldane later in the day after he had delivered his lecture on Mutation in Man."
5. John Langdon-Davies, *Russia Puts the Clock Back: Langdon-Davies Versus Haldane* (London: Victor Gollancz Ltd., 1949).
6. Naomi Mitchison, *You May Well Ask: A Memoir 1920–1940* (London: Victor Gollancz Ltd., 1979), p. 191.
7. Clark, *JBS*, p. 28.
8. ACC. 10306/8. John Burdon Sanderson Haldane. Writings, 1910–56, diary 1910–11. Haldane Papers, National Library of Scotland (hereafter, NLS), p. 5.
9. J.B.S. Haldane, *Daedalus: Or Science and the Future* (London: Keagan Paul, 1923).
10. J.B.S. Haldane to L.K.H. April 14, 1915. MS-374-12, Mf. Sec. MSS, 2103. MS 20665, Haldane Papers, NLS.
11. J.B.S. Haldane, *Callinicus: A Defence of Chemical Warfare* (New York: E.P. Dutton and Co., 1925).
12. J.B.S. Haldane, *Possible Worlds: A Scientist Looks at Science* (New York and London: Harper and Brothers, 1928), p. 208. Italics in the original; See also Aldous Huxley's portrayal of Haldane as the character Shearwater in his 1923 novel, *Antic Hay* (London: Chatto and Windus).
13. Haldane, *Daedalus*, p. 1. The passage is developed from a sonnet he wrote to Naomi during World War I. J.B.S. Haldane to Naomi Mitchison, January 22, 1916. Accc. 10753/1. Naomi Mitchison, 1915–17, 1967–70. Haldane Papers, NLS; See also "Destruction of Richebourg L'Avoué," MS 20579. J.B.S. Haldane Papers, NLS.
14. Haldane, *Daedalus*, p. 4.
15. *Ibid*., p. 49.
16. *Ibid*., p. 63.

17. *Ibid*., p. 65.
18. *Ibid*., pp. 63–77.
19. *Ibid*., pp. 78, 92–93.
20. Krishna A. Dronamraju ed., *Haldane's* Daedalus *Revisited*, (Oxford: Oxford University Press, 1995), p. 1; Judith Adamson, *Charlotte Haldane: Woman Writer in a Man's World* (London: Macmillan Press, 1998), p. 38; Julian Huxley, *Memories I* (New York: Harper and Row, 1970), p. 131.
21. Haldane, *Possible Worlds*, p. 200.
22. *Ibid*., p. 201.
23. *Ibid*., p. 204; It is important to point out now that though Muller was also keenly interested in the plight of the working class, the notion of raising living standards across the board, rather than selecting the seed of those he deemed superior, is nowhere present in his eugenic prescriptions.
24. J.B.S. Haldane, *Heredity and Politics* (New York: W.W. Norton and Co., 1938), pp. 7–8.
25. *Ibid*., pp. 15–24.
26. *Ibid*., p. 84 ft.
27. *Ibid*., p. 19 ft.
28. *Ibid*., p. 21.
29. *Ibid*., p. 134.
30. *Ibid*., pp. 8–9.
31. *Ibid*., p. 133.
32. Much of this is documented in Robert Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago, IL: University of Chicago Press, 1998). Other good sources are Elof Axel Carlson and James Schwartz's biographies of Muller.
33. For examples of the extent to which Muller was convinced he was not given fair credit for his ideas, see H.J. Muller to Edgar Altenburg, January 6, 1933, October 24, 1935, March 30, 1936, October 20, 1939. Muller MSS, Lilly Library, Indiana University (hereafter, LLIU), as well as Nobel Laureates on Scientific Research. Oral History Interview. Harriett Zuckerman and Hermann J. Muller, Bloomington, Indiana, December 12, 1963. Oral History Library Columbia University, pp. 6–23, 30–31, 41–52.
34. Elof Axel Carlson, *Genes, Radiation and Society: The Life and Work of H.J. Muller* (Ithaca: Cornell University Press, 1981), p. 33.
35. *Ibid*., pp. 34–35.
36. FOIPA No. 1024867-000. June 29, 2006. Federal Bureau of Investigation. U.S. Department of Justice. Washington, DC; See also Carlson, chapter 12.
37. See William F. Gregory to H.H. Laughlin, June 28, 1932; Correspondence, William F. Gregory to H.H. Laughlin, July 5, 1932; Correspondence, H.H. Laughlin to William F. Gregory, July 11, 1932.William F. Gregory (?) to A.F. Blakeslee, July 12, 1932. American Museum of Natural History,

Box 737. Folder, "International Congress of Genetics 1932." 1267. AMNH Library Central Archives.

38. Carlson, pp. 178–179; Jay Lush to Hermann J. Muller, June 25, 1932. Muller Correspondence, 1931–1932. Muller MSS, Series I Box I, LMC 1899, LLIU.
39. J.B.S. Haldane to H.J. Muller, July 29, 1932. Haldane, 1932–1964. Muller MSS, Series I Box 21, Lilly Library, Indiana University. See also J.B.S. Haldane, *The Inequality of Man and Other Essays* (London: Chatto and Windus, 1932).
40. "Holds Capitalism Bars Eugenic Goal," *New York Times*, August 24, 1932; see also, Muller Correspondence, 1931–1932. Muller MSS, Series I Box 1, LMC1899, LLIU.
41. AMNH Library Central Archives, Box 737. Folder, "International Congress of Genetics 1932." 1267.
42. This is certainly how the editors viewed it, as evident in their editorial published the same day. An original copy is available in AMNH Library Central Archives, Box 737. Folder, "International Congress of Genetics 1932." 1267.
43. H.J. Muller to Edgar J. Altenburg, September 10, 1932. Muller MSS, LLIU.
44. H.J. Muller to Edgar J. Altenburg, November 10, 1932. Muller MSS, LLIU.
45. H.J. Muller to Edgar J. Altenburg, November 10, 1932. Muller MSS, LLIU.
46. H.J. Muller to Edgar J. Altenburg, January 6, 1933. Muller MSS, LLIU. Muller was probably referring to the following passage from *Causes of Evolution*: "If human evolution is to continue along the same lines as in the past, it will probably involve a still greater prolongation of childhood and retardation of maturity. Some of the characters distinguishing adult man will be lost. It was not an embryologist or palaeontologist (*sic*) who said, 'Except ye … become as little children, ye shall not enter into the kingdom of heaven." J.B.S. Haldane, *The Causes of Evolution* (Princeton, NJ: Princeton University Press, 1990), p. 81.
47. See Nikolai Krementsov, "Eugenics, *Rassenhygiene*, and Human Genetics in the late 1930s: The Case of the Seventh International Genetics Congress," in *Doing Medicine Together: Germany and Russia between the Wars*, ed. Susan G. Solomon (Toronto: University of Toronto Press, 2006), pp. 369–404., as well as Nikolai Krementsov, *International Science Between the Wars: The Case of Genetics* (New York and London: Springer, 2005), particularly chapters 2, 4 and 5.
48. See Krementsov, *International Science Between the Wars*, pp. 92–93.

49. See H.J. Muller, *Out of the Night* (London: Victor Gollancz Ltd., 1936), p. 51, ft. 1; p. 91.
50. H.J. Muller to J.B.S. Haldane, April 29, 1936. Haldane Box 16. Scientific Correspondence Abroad (Other than America) 1947. J.B.S. Haldane Papers, UCL Library Services, Special Collections (hereafter, UCL); Haldane did not review it in *Nature*, however if he had his assessment would probably have reflected what he wrote a few years later in *Heredity and Politics*, i.e., that Muller's ideas were "premature in view of our very slight knowledge of the genetical basis of those characters which are found in the 'great men' whom we regard as admirable." J.B.S. Haldane, *Heredity and Politics* (New York: W.W. Norton and Co., 1938), p. 133.
51. Hermann J. Muller to Joseph Stalin. Subseries: Writings by Muller, Box 1. Muller MSS, LLIU.
52. H.J. Muller to Julian Huxley, March 9, 1937. Huxley, J.S. 1933–1937. Muller MSS, Series I Box 23, LLIU.
53. See Mark B. Adams, ed., *The Wellborn Science: Eugenics in Germany, France, Brazil and Russia* (Oxford: Oxford University Press, 1990), pp. 195–196.
54. See Adams, ed., *The Wellborn Science*; Nikolai Krementsov, "From 'Beastly Philosophy' to Medical Genetics: Eugenics in Russia and the Soviet Union," *Annals of Science* 68, no. 1 (2011), pp. 61–92.
55. Krementsov, *International Science Between the Wars*, pp. 45–46.
56. H.J. Muller to J.B.S. Haldane, December 4, 1936. Haldane, 1932–1964. Muller MSS, Series I Box 21, LLIU.
57. "Moscow Cancels Genetics Parley," *New York Times*, 1936.
58. *New York Times*, December 17, 1936.
59. Muller to Huxley, March 9, 1937.
60. *Trudy Vsesoyuznoy akademii S.-Kh. Nauk, im. V.I. Lenina Spornye voprosy genetitsi i selektsii* (Debatable problems of genetics and selection), Raboty IV sessii Akademii9-27 dekabrya 1936 goda. Otvetsvennyi redactor O.M. Targul'yan Isdatel'stvo Vsesoyuznoy akedemii S.-Kh. Nauk, im. V.I. Lenina (Moskva 1937 Leningrad), p. 437. Many thanks to Miklos Muller for the translation.
61. Correspondence, H.J. Muller to Julian Huxley, February 24, 1937. Huxley, J.S. 1933–1937. Muller MSS, Series I Box 23, LLIU.
62. *Ibid*.
63. H.J. Muller to J.B.S. Haldane, March 7, 1937. Haldane, 1932–1964. Muller MSS, Series I Box 21, LLIU.
64. *Ibid*.
65. *Ibid*.
66. H.J. Muller to Julian Huxley, March 13, 1937, Huxley, J.S. 1933–1937. Muller MSS, Series I Box 23, Lilly Library, Indiana University.

67. According to Muller's second wife, Thea, Muller held onto his eugenic views his entire life. In the folder in the Lilly archives containing Muller's letter to Stalin there is a note from her which reads: "This 'Eugenic Proposal' had been kept in the safety deposit box in the bank since we came to Bloomington. H.J.M. did not want others to read it although he still held the same ideas on eugenics. He felt bad about the style of the article. This article was the one sent to Stalin to interest him in eugenics. (This was before the purges!) I am sure H.J.M. would not want anything quoted from this article." Subseries: Writings by Muller, Box I, Muller Papers, LLIU.
68. The extent of Muller's antipathy for Bridges is evident in a passage from James Schwartz's biography, *In Pursuit of the Gene: From Darwin to DNA*, where the author recounts an episode in which Bridges may have taken credit for Muller's ideas. Schwartz writes that Muller's response was "a slap on the hand of the incorrigible Bridges, who was incapable of restraining himself when it came to borrowing other men's ideas" (Schwartz, p. 262). Regardless of whether or not Bridges was guilty or innocent in this episode, the fact that Schwartz describes it in these terms is indicative of how Muller's feelings about Bridges were most likely transmitted to Schwartz through Muller's son David.
69. Calvin Bridges to H.J. Muller, 1937. Correspondence 1937, November–December. Muller MSS, Series I Box 1, LMC 1899, LLIU.
70. Hermann J. Muller to Edgar J. Altenburg, October 20, 1939. H.J. Muller Papers, LLIU.
71. H.J. Muller to Nikolai Vavilov, February 21, 1938. Vavilov, 1938–39, 1941. Muller MSS, Series I Box 30, LLIU; See also, H.J. Muller to L.C. Dunn, June 17, 1937. Hermann J. Muller, 1928–1937, Box 23, APS.
72. Charlotte Haldane, *Truth Will Out* (Great Britain: Peel, Ashford and Hart Ltd., 1950), p. 318.
73. Well over a dozen letters exchanged between Charlotte Haldane and Muller from 1949 to 1950, discussing J.B.S. Haldane's response to Lysenko, can be found in the Muller collection at the Lilly Library, Indiana University. In one letter Muller wrote, "I have often wished that Professor Haldane had lived for a considerable time in the Soviet Union as it is hard for me to conceive that he would have continued along the same line in that case." Muller's lament is of course ironic given that he chose not to inform Haldane about what was going on in the USSR in 1937, as he did Huxley. See H.J. Muller to Charlotte Haldane, March 16, 1949. Haldane, 1932–1964. Muller MSS, Series I Box 21, LLIU.
74. Notes for telegram, H.J. Muller to J.B.S. Haldane, June 8, 1938. Haldane, 1932–1964. Muller MSS, Series I Box 21, LLIU.

75. Nikolai Vavilov to H.J. Muller, July 26, 1939. Vavilov, 1938–39, 1941. Muller MSS, Series I Box 30, LLIU.
76. Frances Crew to J.B.S. Haldane, August 4, 1939. Haldane Box 35, 271, UCL.
77. Hermann J. Muller to J.B.S. Haldane, August 10, 1939. Haldane Box 35, 269, UCL.
78. Carlson, pp. 284–285.
79. Elof Axel Carlson. Lecture. New York, NY, June 10, 2005.
80. See, Dorothy Sheridan, ed., *Among You Taking Notes…: The Wartime Diary of Naomi Mitchison 1939–1945* (London: Victor Gollancz Ltd., 1985), pp. 87, 188; Clark, pp. 154–161.
81. See Ivor Montague, *The Youngest Son: Autobiographical Sketches* (London: Lawrence and Wishart, 1970), pp. 234–238.
82. P.S. Hudson and R.H. Richens, *The New Genetics in the Soviet Union* (Cambridge, UK: Imperial Bureau of Plant Breeding and Genetics, 1945 [it was published in December 1946!]).
83. Theodosius Dobzhansky to L.C. Dunn, July 24, 1945. Leslie Clarence Dunn Papers, APS; Dunn did not read Russian but he wrote his review before Dobzhansky finished the translation. This also seems to have been the case with Alfred H. Sturtevant. See Business Reply Card, Kings Crown Press. Alfred H. Sturtevant Papers. California Institute of Technology. Box 2.14. Dobzhansky Correspondence. California Institute of Technology.
84. J.B.S. Haldane to H.J. Muller, May 15, 1946. Haldane, 1932–1964. Muller MSS, Series I Box 21, LLIU.
85. For example, Muller wanted to publish a review of Charlotte Haldane's book in *The Saturday Review of Literature*, however the editors were not interested because their policy was to only publish reviews of books published in the U.S. See Raymond J. Walters to H.J. Muller, March 10, 1950. Haldane, 1932–1964. Muller MSS, Series I Box 21, Lilly Library, Indiana University.
86. H.J. Muller to Milislav Demerec, June 5, 1946. Milislav Demerec Papers, APS.
87. Reminiscences of L.C. Dunn, p. 773.
88. H.J. Muller to L.C. Dunn, May 7, 1944. L.C. Dunn Papers, APS.
89. Muller to Huxley, March 13, 1937.
90. "The Lysenko Controversy," *The Listener*, December 9, 1948, p. 875.
91. Haldane's resentment of US power and influence is evident across a variety of publications and public statements. An early example is his 1926 article, "Nationality and Research," published in *Forum* (pp. 718–723) wherein he declared, after discussing the abundant resources provided to US scientists by private industry and philanthropy, "In spite of these facts

and the undoubted genius of many Americans, I am inclined to think that in pure (though not perhaps applied) science, American produces less than either Britain or Germany. The probable reason is that great men are more important to science than great laboratories." A later example is his claim that his reason for relocating to India in 1957 was that Great Britain was occupied by American troops. See Ronald Clark, *J.B.S.: The Life and Work of J.B.S. Haldane* (New York: Coward-McCann, 1968), pp. 229–243. Other examples are chronicled below.

92. Theodosius Dobzhansky to L.C. Dunn, January 10, 1946. L.C. Dunn Papers, APS.
93. "Haldane in Search of an 18½ Inch Collar," *New York Times*, December 22, 1946.
94. Theodosius Dobzhansky to L.C. Dunn, January 8, 1947. L.C. Dunn Papers, APS.
95. *Ibid*.
96. Reminiscences of Theodosius Grigorievich Dobzhansky. Oral History Library. Columbia University, p. 306.
97. See Haldane Box 15 (b). Lysenko Controversy, 1947–1950; Box 18. Scientific Correspondence 1938–53, G-H; Haldane Box 20 R-T. Scientific Correspondence 1936–1954 S-T, UCL.
98. Mikuláš Teich to J.B.S. Haldane, October 3, 1948, as well as Haldane's undated response in the same folder. Haldane Box 15 (b). Lysenko Controversy, 1947–1950, UCL. See also Mikuláš Teich, "Haldane and Lysenko Revisited," *Journal of the History of Biology* 40 (2007), pp. 557–563. In his article Teich calls for a more nuanced examination of Haldane's position on Lysenko.
99. See A.G. Bateman to J.B.S. Haldane, August 29, 1948. Haldane Box 15 (b). Lysenko Controversy, 1947–1950, UCL.
100. Naomi Mitchison to J.B.S. Haldane, undated. Haldane Box 15 (b). Lysenko Controversy, 1947–1950, UCL.
101. Haldane, *The Inequality of Man and Other Essays*, p. 137.
102. See endnote above.
103. See Langdon-Davies, title page and p. 60.

Why Did Japanese Geneticists Take a Scientific Interest in Lysenko's Theories?

Hirofumi Saito

Introduction

Theories of genetics proposed by Trofim Denisovich Lysenko (1898–1976) and his followers in the Soviet Union elicited powerful critical reactions from scholars and scientists in Western intellectual communities. In 1946, when Lysenko's short pamphlet entitled *Heredity and Its Variability* was published in English,[1] critics were quick to point out Lysenko's ignorance and misconceptions about modern genetics. In particular, British and American geneticists noted a series of defects in his experiments in their book reviews.[2] Among those critical reviews, Theodosius Dobzhansky (1946) stated, "The progress of science would be seriously disorganized if all scientists interrupt their work every time somebody publishes a dubious claim."[3] As this attitude shows, Western geneticists rejected the theories of the Lysenko school outright. They concluded that Lysenko's experimental results did not deserve a second glance. Following the infamous August session of VASKhNIL, in which Lysenko, with the approval of the Communist Party, virtually declared the abolition of genetics studies in

H. Saito (✉)
Tokyo Institute of Technology, Tokyo, Japan

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the Soviet Union, Western geneticists did not leave room for the purely scientific arguments of Lysenko's theories anymore. Instead, they discussed a series of events in relation to Cold War science, politics, and ideology.[4]

From the very beginning of their discussion, Western geneticists adopted the position that Lysenko's theories were scientifically worthless. From today's perspective, there is no doubt that Lysenko's theories exerted no positive intellectual influence on the development of genetics knowledge. Because of this, historians of biology tend to ignore some cases in which purely scientific arguments on Lysenko's theories developed among professional geneticists in some countries. In fact, many studies on the history of Soviet genetics adopt political, economic, cultural, and social viewpoints—that is to say an *external* approach to the history of sciences—to explain the acceptance of Lysenko's theories and the development of the Lysenko school.[5] On the other hand, an *internal* approach, such as the explanation based on particular intellectual trends of geneticists in those days, was not mainstreamed until now.[6]

Unlike Western geneticists, during the end of the 1940s and the beginning of the 1950s, orthodox[7] Japanese geneticists showed scientific interest in the theories and experiments of the Lysenko school. In Japan, even after the August session of VASKhNIL, scientific argument on Lysenko's theories did not cease. Thus, for the comparative study of each country's response to Lysenko, Japan will provide an interesting case in which geneticists' internal motives, not external such as political and ideological, played a large part in the development of the controversy. What kind of internal motives existed behind Japanese geneticists' scientific concern with Lysenko's theories? I will try to answer this question in this chapter.

The Scope of this Chapter

As a beginning, I would like to limit the analysis of the Japanese case to the non-political and non-cultural aspects.[8] Thus, with regard to the political motives with which some geneticists participated in the controversy, I will give a brief explanation here. As restrictions on political activities began to be lifted after the Pacific War, leftist scientists began to embrace socialist ideology, slogans, and thought. In particular, the doctrine of "unity of theory and practice" was adopted as a criterion of whether one was a real scientist; any failure to conform to this ideology was considered a serious defect on the part of a scientist. Some leftist biologists regarded Lysenko as an embodiment of this attitude and approved Lysenko's theories and agricultural proposals explicitly in their writings; such a manner served

for them both to demonstrate their academic legitimacy to ordinary people and to denounce biologists who opposed Lysenko's theories as the separation from practice.[9] However, independent of those who approached Lysenko's theories with political motives were some geneticists with purely scientific motives. Teiri Nakamura, who was actually involved in the Japanese Lysenko controversy, somewhat explains these motives in his book[10] as follows: There were geneticists who recognized the limitations of the current chromosomal theory.[11] Some of them said they observed the phenomena of heredity that could not be explained solely by chromosomal behavior like crossover, and others doubted whether geographic variation could be always explained only by the gene mutation. Instead, those geneticists emphasized the role of environmental effects and began to accept Lysenko's theories positively.

In agreement with Nakamura's account above, I will propose my own view as to why Japanese geneticists had a scientific interest in Lysenko's theories. First, for the purpose of supplementing Nakamura, I will reconsider the question of the conditions under which Lysenko's theories were introduced in Japan after the War. My view is that Japanese geneticists' first response to Lysenko's theories was formed while research conditions were extremely poor in Japan immediately after the Pacific War, and their attitude was very different from that of Western geneticists in the same period. Second, in explaining Japanese geneticists' strong interest in Lysenko's theories, I will deviate from Nakamura's interpretation of the state of Mendel-Morgan (orthodox) genetics around 1950. Nakamura explains that one critical aspect of orthodox genetics as seen from biologists sympathetic to the Lysenko school was that orthodox genetics could not elucidate the process by which some traits were expressed epigenetically. Geneticists with intermediate viewpoints between the orthodox one and Lysenko's also criticized that a phenotypic trait was not reducible to a one-to-one stable correspondence of genes because the function of genes was greatly influenced by cytoplasm or other external conditions. However, orthodox geneticists began to understand that the environmental effect could not be negligible in the course of phenotypic expression. Moreover, molecular genetics and embryological genetics were developing studies for grasping this process.[12]

It is true that interest in Lysenko's theories resulted in large part from complaints about the stagnation of Mendelism. With the advance of genetics, as Nakamura describes, Mendelism had by the end of the 1940s sloughed off the old concept of the gene as particles separated from other

substances in a cell. When the development of modern genetics disciplines was planned on the basic line of Mendelism around 1950, criticism against Mendelism from the Lysenko school seemed to have lost its ground almost completely. However, this account does not explain the fact that scientific interest in Lysenko's theories was seen among geneticists as late as the mid-1950s. The present author's understanding is that scientific interest in Lysenko's theories had an aspect other than mere complaints about Mendelism. That is to say, in the course of the development of Mendelism, rather than in conflict with Mendelism, interest in Lysenko's theories was generated and increased by the rise of a particular genetics subject and discipline, as described in detail later.

The purpose of this paper is to show that the Japanese Lysenko controversy overlapped the recovery of genetics in the postwar period, and the attitude of Japanese geneticists to Lysenko's theories was determined in this recovery period. I will propose two types of recovery: (1) normalization of the conditions of research, and (2) a trend toward reforming genetics disciplines. In part 2, I will examine recovery of the first type to illustrate how Lysenko's theories were introduced in Japan. Then, in part 3, I will take up recovery of the second type in order to examine Japanese geneticists' internal concern during the end of the 1940s and the beginning of the 1950s.

Introduction of Lysenko's Theories in the Normalization Process of Genetics Research Conditions in Japan

For this part, it is helpful to begin by previewing the early history of Japanese genetics.[13] Especially, I will highlight the contribution of German geneticist Richard Goldschmidt (1878–1958), who taught at the agricultural department of Tokyo Imperial University from the fall 1924 to the fall 1926.

As early as the first decade of the twentieth century, there appeared in Japan prominent pioneering geneticists, especially in the field of animal genetics. Among them was Kametarō Toyama, assistant professor at Tokyo Imperial University, who was counted as one of the earliest Mendelian researchers because of his study (1906) demonstrating that Mendel's law is applicable to the heredity of the silkworm. Toyama's work was principally connected with the improvement of the silkworm, which

was requested in practical terms of sericulture that had grown to become the chief industry of Japan by that time. Meanwhile, genetics research in Japan firmed its institutional system in 1920 when the Genetics Society of Japan was established by succeeding the activity of the Japanese Society of Breeding (founded in 1915).[14] In 1921, the Genetics Society started to issue annually the *Journal of Genetics*, which almost constantly contained some papers written in English and abstracts of the latest foreign works.

In September 1924, Richard Goldschmidt came to Japan in order to teach genetics at Tokyo Imperial University.[15] Since Toyama had died in 1918, the chair of genetics had remained vacant. Chiyomatsu Ishikawa, a professor of zoology who had been trained under August Weismann in Freiburg and who had become, like Toyama, a grand pioneer of Japanese animal genetics, was very worried with this personnel matter. At that time, Ishikawa was recommended to retire according to the mandatory retirement system newly introduced into the University. On condition that he agreed with his retirement, Ishikawa nominated Goldschmidt, who he had become acquainted with in 1914 when Goldschmidt visited Japan for the first time, for the chair.[16] For Goldschmidt, there was great merit in conducting his research in Japan. As Goldschmidt observed in 1914, the intersexuality of the gypsy moth (*Lymantria*) displays rich local differentiation in Japan. Therefore, Japanese varieties were indispensable for verification of his original theory about intersexuality change. Ultimately, Goldschmidt accepted the invitation.

Goldschmidt held basic lectures on transmission genetics in English for students of the agricultural department and sometimes the medical department, and gave a special lecture connected with his own research in German, mainly for graduate students. One of his best pupils, Kiyoshi Masui, contributed to the study of sex determination in fowl and later became president of the Genetics Society of Japan in 1946. Goldschmidt was respected by Japanese geneticists not only for his established reputation in physiological genetics (precisely, it is not true that all Japanese geneticists accepted his unique, controversial works uncritically), but also for his broad knowledge and understanding of Japanese culture, art, and lifestyle. After Goldschmidt left Japan, he kept in touch with Japanese genetics throughout his life. For instance, he contributed his personal library on genetics, which amounted to about 50,000 works, to the National Institute of Genetics, newly established in Mishima City in 1949 as the first genetics research center in Japan, independent of laboratories in universities.

Goldschmidt played an important role in introducing to Japan the physiological approach, which characterized the research style of German genetics.[17] On the other hand, *fruit fly* genetics, which occupied the interests and tasks of American geneticists in the interwar period, spread from Kyoto University after Taku Komai, an animal geneticist who had studied under T. H. Morgan for two years from 1923, came back to Japan with drosophila samples in 1925. From the middle of the 1920s to the late 1930s, genetics studies in Japan developed by keeping equal attention on two major approaches. However, I would like to point out that dependence on drosophila was not so remarkable in Japan, compared with America. Instead, the silkworm constantly supported Japanese genetics in the early age since Toyama, not only as the traditional research subject itself but also as a model organism applicable to studies both on heredity and on physiology in general. Hideo Kikkawa, who tackled physiological problems of the silkworm from the standpoint of biochemical genetics, wrote in 1943, "Even in wartime, Japan was superior to other countries in the stock of experimental samples thanks to, in addition to fruit fly samples, rich samples of various mutants of silkworm."[18] To sum up, at least until the late 1930s, the state of Japanese genetics research had been in a relatively normal condition; the Genetics Society held general meetings every year, and geneticists were familiar with advances in world genetics by tracing foreign publications while publishing original works.

Japan's defeat in the Pacific War in August 1945 caused serious damage to the genetics community, as well as to the intellectual community in general. As Japanese geneticists became aware of the disparities in knowledge between Japan and the West, they increased their efforts to close the gap between them. One of the primary tasks of recovery was the normalization of access to foreign literature. Very few foreign publications were available during the War due to the breakdown of scientific interchange. In January 1947, the American Library Association contributed a number of books and journals published during and immediately following the War.[19] They were distributed to only a few major institutes, such as the library of the University of Tokyo. According to the list of journals brought to Japan,[20] distribution included the 1946 edition of the *Journal of Heredity*, which contained Dobzhansky's highly critical review of Lysenko's book (1946). In the spring of 1948, one of the biggest Japanese book companies, Maruzen, resumed its import of certain foreign academic journals. However, during the initial postwar period, it was unlikely that the majority of Japanese geneticists had ready access to

Western scientific literature. In such a condition, Lysenko's theories were hardly known at all among geneticists in Japan.

Next, I will illustrate the process by which Lysenko's theories were brought to Japan while Japanese academia was still isolated from the world. Through three cases, Ryūichi Yasugi, Hitoshi Kihara, and Jūhei Satō, with different motives for the introduction of Lysenko's theories, readers will be able to understand what Japanese geneticists' thirst for foreign knowledge, including Lysenko's theories, was like.

The simplest approach to obtaining knowledge about Lysenko's theories was, needless to say, to read Russian literature. However, from the end of the War until the beginning of the 1950s, Japanese geneticists had limited access to the original Russian language literature of the Lysenko school. Until an official exchange of scientific literature resumed between the two countries in 1951,[21] Russian literature was available only to a limited few, who obtained it through private means. In addition, because Lysenko's theories were published in Russian, few Japanese scientists could read and evaluate his work. Theodosius Dobzhansky introduced Lysenko's theories in America,[22] while in Japan these theories were not introduced by a professional geneticist at first. In the summer of 1947, Ryūichi Yasugi (1911–1997), a historian of Darwinism, wrote a short but relatively detailed review of Lysenko's theories based on information from a few Russian publications.[23] Many learned Lysenko's theories for the first time from this article. In the same year, Yasugi briefly introduced the Lysenko school's experiments on vegetative hybridization based on a single volume of the Russian journal *Yarovizatsiya*, edited by Lysenko.[24] Yasugi could not evaluate Lysenko's theories adequately and therefore strongly expected that his introduction would help Japanese geneticists re-examine Lysenko's theories and that, based on its results, they would develop independent criticisms of Lysenko's theories. Before August 1948, when much information on Soviet genetics flowed together, Japanese biologists mainly depended on Yasugi's introductory articles.

Hitoshi Kihara (1893–1986) was one of the most prominent Japanese geneticists and was known in the academic world for his discovery of the ancestor of common wheat, namely bread wheat (*Triticum aestivum*), in 1944.[25] It was natural that Kihara was greatly interested in the work of distinguished Soviet botanist Nikolai Ivanovich Vavilov (1887–1943), because the vast collection of seeds of cultivated plants at the Institute of Applied Botany (from 1929 the All-Union Institute of Plant Industry) proved very useful for Kihara's research. In 1929, when Vavilov planned

to visit Japan to collect seeds of local species, Kihara took care of all the formalities for his trip. In October, Kihara met Vavilov for the first time in Kyoto and arranged for him to deliver a lecture entitled "Origin of Cultivated Plants" at Kyoto Imperial University. After that, he kept in touch with Vavilov.[26]

Kihara had established his research credentials by the 1940s. He attended the Eighth International Congress of Genetics in Stockholm in July 1948 as the first Japanese scientist to attend an academic conference abroad after the Pacific War.[27] There, he interacted with many Western geneticists, such as C. D. Darlington and Goldschmidt.[28] After the congress, he traveled to Sweden, Denmark, and North and South America for two months. In Boston, Kihara met for the first time with American geneticist Karl Sax, who could have been Kihara's potential competitor in the field of wheat genetics. This time, however, they appeared to have agreed to join forces to criticize Lysenko's genetics, for they co-published an article later.

After Kihara came back to Japan on October 2, 1948, he published a report of his travels in 1949. In the preface of the report he writes:

> As long as I observed [the 8th International Congress of Genetics], I felt that there was not a remarkable degree of advance in genetics research abroad. However, the more I read the foreign literature I had carried back, the more I recognized that the gap in the genetics research levels between Japan and the West had increased substantially during the war.[29]

Kihara's remark as the leading geneticist, especially because of its candor, indicates the unfavorable state of Japanese genetics at that time. The bibliography attached to the report mainly comprised Western literature collected by Kihara during his travels. In this bibliography, literature on Lysenko's genetics previously unavailable in Japan, such as *Scientist in Russia* by Eric Ashby (1947) and *Heredity and its Variability* by Lysenko (1946), were included. Later, Kihara prepared his own criticism of Lysenko's theories on the basis of those books.

Kihara majored in wheat genetics, and among Japanese geneticists, he was the most suited for commenting on Lysenko's experiments on professional grounds. Kihara started his independent criticism of Lysenko's genetics in 1950. First, he contributed two articles to *Shizen* [*Nature*].[30] In the first article, Kihara summarized Lysenko's genetics based on *New Genetics in the Soviet Union*[31] by Hudson and Richens (1946) and especially took up the case of the graft hybrid that Lysenko posited as powerful

evidence of his genetics. Kihara suggested some alternative possibilities to interpret the graft hybrid from the viewpoint of orthodox genetics. He contributed another article in 1950 to *Iden* [*Heredity*], which became his first independent criticism.[32] In the article, he provided a table contrasting orthodox genetics and Lysenko's genetics along with his own interpretation. Of the items in the table, he focused on the problem of environmental impact on the heredity of plants. Kihara criticized Lysenko's claim that he could freely convert a wheat species into any other species using adequate physiological treatments. He pointed out that each species of wheat had an individual number of chromosomes and insisted that it was impossible to change the number of chromosomes of wheat by merely tweaking environmental conditions through vernalization.

Among Western geneticists, Karl Sax, who examined Lysenko's genetics from experimental agricultural and botanical viewpoints, also had the same view as Kihara,[33] and in 1953, the two men co-wrote and published an article in the *Journal of Heredity*.[34] In his criticism, Kihara seemed to maintain a balanced viewpoint and sometimes adopted a partly sympathetic attitude to Lysenko's approach as follows: Unlike chromosomes, cytoplasm is not stable when exposed to the environment. If we demonstrate that change in cytoplasm can cause a mutation in chromosomes, we will recognize the inheritance of acquired traits in the broad sense.[35] Kihara predicted that if orthodox geneticists focused more on the role substances outside the nucleus, such as cytoplasm, played in the process of heredity, it would further the development of genetics.[36] To sum up, Kihara could play an original role in the introduction of Lysenko's theories owing to his international activity, which made him an exceptional figure in Japan in the early postwar period.

Some Japanese geneticists, who belonged to the group *Neo Mendel Kai* [*Neo Mendelian Society*],[37] industriously addressed the classification of approximately 12,000 foreign biological papers and books after the War. In November 1948, they published a catalogue of foreign genetics literature.[38] Each chapter of the catalogue was entitled with a particular theme, like "Structure of a Chromosome," and consisted of commentary articles and references written by a qualified member of *Neo Mendel Kai*. Those themes selected were important problems as seen from the field of biology at that time. Hence, we can say that they highlighted the main concerns of geneticists during that period. One particular chapter is entitled "Genetics Theory of the Lysenko School."[39] In the efforts made to recover genetics knowledge, the introduction of foreign knowledge was not selective. Therefore, Lysenko's theories were introduced as *new* knowledge along with other genetics topics.

According to Nakamura (1997), the Japanese Lysenko controversy began at the 19th convention of the Genetics Society of Japan held in Matsumoto City in October 1947; since then, arguments among geneticists intensified incrementally.[40] However, in many cases those geneticists still could not discuss Lysenko's own writings directly. At least until the spring of 1948, no noticeable reference to Lysenko's literature appeared in orthodox geneticists' writings. In March 1948, a group discussion was held in Tokyo by seven senior geneticists over Lysenko's theories.[41] However, the discussion did not extend beyond the book review of Lysenko's *Heredity and Its Variability* by Richard Goldschmidt (1946), which turned out to be the only document the participants had consulted before the discussion. Some of them did not even read Yasugi's introductory paper mentioned above. A member of *Neo Mendel Kai*, Jūhei Satō (1911–1996), upon hearing the low quality of this discussion, decided to improve the condition of the argument. Soon after, he obtained Lysenko's book through his own personal means, and a copy of the book came to be circulated among Japanese geneticists after May 1948.[42]

Up to that time, many of Japan's leading geneticists had released their initial comments on Lysenko's theories. In general, these comments were highly critical. In July 1948, many of these comments were included in a collection of articles edited by *Neo Mendel Kai*.[43] In the preface to this collection, the editor expressed a strong desire by saying that, "I hope that this book is published as soon as possible, if not a day sooner, and that geneticists will undertake a complete re-examination of Lysenko's theories by scientific criticism, test, and practice of them."[44] Ironically, immediately following publication of the collection, the August session of VASKhNIL took place, which had a strong impact on the Japanese scientific community. By that time, Japan had recovered almost normal access to Western literature. Thus, the severe attack on the Soviet government by Western geneticists in their criticism of this event and their negative view of Lysenko's theories received rapid translation into Japanese.[45] Some influential senior geneticists followed those arguments by Western authorities, and their own criticism gradually took on a political tone. Nonetheless, alongside the increase in political arguments, scientific arguments persisted in Japan.

In addition to the discussion above, here I refer to geneticists' scientific interest in Lysenko's *experiments*, the result of which the Lysenko school regarded as evidence of acquired traits, examining the period when information on Lysenko's experiments became available in Japan, Britain, and

America. In 1946, both the theoretical and experimental sections of the Lysenko school were introduced together in Britain and America. Besides the English translation of Lysenko's book *Heredity and Its Variability*, which consisted mainly of the theoretical contents, the other important publication in 1946 was *The New Genetics in the Soviet Union*. The authors of the book, P. S. Hudson and R. H. Richens, who were based at the Imperial Bureau of Plant Breeding and Genetics at Cambridge, surveyed a large number of experimental records of the Lysenko school in this book. Soon after publication, the book became an important resource for British and American geneticists to evaluate the experimental results of the Lysenko school. In fact, many geneticists in those countries found basic defects in Lysenko's experiments including the lack of a control variable and inadequate statistical processing. As a result, they concluded that Lysenko's theories, which were supported by those inadequate experiments, were, as they had thought, not reliable at all.

Meanwhile, Japan had access to the theoretical aspects of Lysenko's work in the late 1940s, prior to the experimental details. It was not until the early 1950s that Japanese geneticists sufficiently knew the procedure of Lysenko's experiments from the original Russian literature.[46] Japanese geneticists could not judge for themselves the explicit defects of Lysenko's experiments, as British and American geneticists could. Instead, they heard a rumor of Lysenko's fantastic experimental results, such as vernalization and, especially, vegetative hybridization that was, for its own sake, an interesting phenomenon. Some of them were inspired to conduct an experiment in vegetative hybridization in their own way and in fact attempted it.[47] In Japan, the defects of the original experiments of the Lysenko school did not prevent them from independently conducting a serious test.[48]

The Internal Motives Behind Geneticists' Scientific Interest in Lysenko's Theories

As seen above, Japanese geneticists' responses to Lysenko's theories can be regarded to some degree as a manifestation of intellectual curiosity and thirst for foreign knowledge, which grew during the recovery process following their limited access to foreign literature. Apart from this explanation, however, this part will demonstrate that, for some geneticists, internal motives also attracted them to approach Lysenko's theories. These geneticists' research interests directly determined their positive or, at least, tolerant attitude to Lysenko's theories.

Behind the scientific interest in Lysenko's theories, we can find a particular subject with which geneticists were primarily concerned. In Japanese geneticists' articles published from the end of the 1940s to the beginning of the 1950s, references to Lysenko's environmental theory often coincided with the hotly debated subject of the mechanism of phenotypic expression. By that time, George W. Beadle's pioneering experiment (started in 1935), the purpose of which was to specify a chain of biochemical reactions that determine drosophila's eye color, had inspired many questions about the mechanism of phenotypic expression. Parts of this field remained largely unstudied so that it attracted geneticists' serious attention. For example, an authoritative physiological geneticist Yoshichirō Umeya writes:

> The relation between gene and cytoplasm [environment in the broad sense] should be basically neither too close nor too remote, but when we investigate the mechanism of phenotypic expression embryologically, we must pay careful attention to the close relation between the two. The correct course for experimental genetics in Japan lies in this direction.[49]

As expressed in this remark, not only Umeya but also each Japanese geneticist had individual concepts about the direction genetics study should take in the postwar period. Among genetics disciplines, physiological genetics was expected to play an important part in the study of this mechanism as mentioned below.

> Mendelism goes beyond chromosomal genetics or statistical genetics, which was condemned in terms of Lysenko's theories for the idea that each trait is described directly as the correspondence to genes. Then studies on intermediate relations between genes and traits, that is to say studies of physiological genetics, began to gain attention.[50]

In general, geneticists hoped traditional themes of Mendelism, such as the regulation of gene separation and the clarification of a gene's correspondence to traits, would be necessarily replaced by studies of the mechanism of phenotypic expression as the new trend. For this purpose, some Japanese geneticists suggested the need for establishing cooperation between disciplines of genetics to study the theme in various ways, using, for example, physiological, epigenetic, cytological, and embryological approaches.[51]

The priority that physiological genetics should take in the study of the mechanism of phenotypic expression was stated enthusiastically by geneticist Yoshito Shinotō, a professor at the University of Tokyo.[52] Shinotō proposed a helpful, original concept for the study of phenotypic

expression. He named the materials that work in the cytoplasm and rule the series of reactions during the course of phenotypic expression "*Hatarakite* [*Factors*]."[53] By this term, Shinotō meant not only material bases such as enzymes, or hormones, but also broader concepts such as events, including the changing environmental conditions that directly or indirectly intermediate between genes and traits. With the introduction of the concept of *Hatarakite*, Shinotō divided the course of phenotypic expression into two different phases: (1) gene to *Hatarakite* and (2) *Hatarakite* to the finally expressed (observable for everyone) trait. Physiologists had already studied the second phase extensively.[54] Shinotō regarded the first phase as a problem left largely unexamined (by Morgan's group), as he writes, "The relation between genes and *Hatarakite* is the important, ultimate theme which is left us."[55] Regardless of whether he was certain that the first phase contained the stage of what is called today in molecular biology "gene expression" at its starting point, which possesses much more complex (non-visible) reactions between series of genes and *Hatarakite*, he certainly understood that the research of the first phase was essential to the complete elucidation of the mechanism of expression of every trait, which was the goal of physiological genetics.[56]

As mentioned above, the development of Japanese physiological genetics owes much to Richard Goldschmidt's role as a *teacher* in the middle of the 1920s. Nevertheless, I cannot easily conclude that more than twenty years after his stay in Japan, Goldschmidt's effort had finally borne fruit in the emergence of positive prospects for physiological genetics, as opposed to Morganist transmission genetics.[57] This is because the shift of geneticists' interest from the chromosome to physiological, embryological, or other types of problems was a natural result of the development of interdisciplinary approaches by the late 1940s; in this meaning, the shift itself was not peculiar to Japan as being inherited from Goldschmidt. Thus, instead of emphasizing Goldschmidt's individual role too much, I would like to return to the early tradition of Japanese genetics that started with the silkworm as a model organism, useful for investigating animal physiology. In this tradition, the favorable condition for accumulating physiological studies and for the acceptance of physiological approaches did exist previously in Japan. Thanks to the conditions provided, Goldschmidt could successfully perform his role of accelerating the further prosperity of this field in Japan. Therefore, my explanation is that the traditional preference for physiological studies, which was increased by Goldschmidt's educational role, helped to push geneticists' shift to physiological problems, such as the mechanism of

phenotypic expression, especially in Japan. With regard to the cause of the shift, however, I do not intend to hurry a conclusion that at this moment would not be beyond assumption. On the contrary, regarding the result of the shift, I want to insist a little more confidently: The active debate about Lysenko's theories emerged from the shift of Japanese geneticists' interest to a physiological approach.

This intense interest in the mechanism of phenotypic expression and the disciplinal trend of physiological genetics inevitably drew geneticists' attention to the role of the environment. Therefore, when some physiological geneticists met with Lysenko's environmental approach, they honestly sought in it a hint for the further study of the relation between environment and gene expression. Those physiological geneticists adopted a *neutral* attitude toward both orthodox genetics and the Lysenko school, although they agreed with the basic concepts of orthodox genetics. It would be more precise to say that the increasing concern of geneticists with the mechanism of phenotypic expression called attention to Lysenko's idea as a sort of environmental theory, rather than that Lysenko's environmental theory originally increased the concern of geneticists.

The appropriate explanations that could effectively dispel the Lysenko school's misconception over the relation between gene and environment certainly existed at this time. They can be summarized as follows: (1) until a trait is finally expressed, each gene related to this trait receives an adequate control of expression from particular environmental conditions; (2) what is inherited from parents is the way each gene responds to the particular environmental condition during the course of phenotypic expression, not merely the genotype itself. Seen from the point of view of certain neutral physiological geneticists, those explanations met no conflict with the basic line of Mendelism and could be regarded as a compromise.

It was likely that some physiological geneticists believed that the final expressed trait was determined by a *function* between two literally independent variables: gene and environment. In this sense, the role of environment was as important as the gene. Not only that, but it was also possible to explain that gene expression is regulated in response to particular environmental conditions. In this sense, the role of the gene might be inferior to that of environment and could even be disregarded. It was long before the regulation of gene expression in response to environment was studied concretely in the early 1960s that geneticists, primarily physiological geneticists, had a scientific interest in Lysenko's *invalid* theories. Therefore, the affinity some neutral geneticists felt with Lysenko's theories could be understood according to the state of knowledge at that time.

In 1949, Yoshio Nagatsuka, who specialized in veterinary medicine, presented an article approving of Lysenko's approach that did not treat heredity, metabolism, and environment as separate from each other in experiments.[58] Nagatsuka wrote that if the previous achievements of physiological genetics, ecology, and experimental embryology were to be examined from the viewpoint of Lysenko's school, the results of the experiments would contribute to a close cooperation between Mendelism and Lysenko's theories. Physiological geneticists positioned themselves to solve the environmental condition regarding the expression of particular traits, and ultimately, they hoped to synthesize numerous environmental theories within the gene theory.[59] From the viewpoint of Mendelism, doing so would lead to the decline of arguments on Lysenko's theories. The existence of neutral geneticists like Nagatsuka represents serious scientific interest in Lysenko's theories in Japan. At the same time, such an interest demonstrates a certain element of naiveté in Japanese genetics knowledge in the postwar period.[60]

Conclusion

After the Pacific War, Japanese geneticists found incentives for engaging in scientific argument over Lysenko's theories as they participated in the recovery process of Japanese genetics. Japanese geneticists' first acceptance of Lysenko's theories was not selective; the introduction was in a way parallel to other types of genetics researches, due to geneticists' thirst for new foreign knowledge. Japanese geneticists were allowed to develop independent arguments without preconceived ideas about Lysenko's theories influenced by severe Western views.

After the VASKhNIL session, scientific arguments about Lysenko's theories persisted until the middle of the 1950s. This strange situation (longtime debate), as seen from other Western case, can be explained by Japanese geneticists' deep concern with the mechanism of phenotypic expression that met the rise of physiological genetics as the primary discipline studying this mechanism at that time. By explaining the role that environment plays in phenotypic expression in terms of gene theory, physiological geneticists hoped to settle arguments over Lysenko's theories. However, physiological geneticists' emphasis on the environmental role sometimes expressed a certain affinity with Lysenko's theories. In this sense, scientific interest in Lysenko's theories reflects the naiveté of genetics studies in postwar Japan.

Finally, I would like to add a few remarks on what we can learn from Japan's case. Today, we know Lysenko's theories had no positive influence on genetics knowledge. However, essentially, whether an idea is of significant interest to scientists and worthy of serious discussion or examination is determined in accordance with the state of knowledge in a specific place during a specific period. Keeping this in mind, we can understand that in Western countries in general, the state of knowledge was mature enough to develop intensely negative views of Lysenko's theories. Thus, in hindsight, it appears that Lysenko's theories were dismissed from the time of their presentation by the majority of scientists around the globe. However, Japan's postwar geneticists are an exceptional case of professional geneticists holding a scientific interest in Lysenko's theories. If Japan's case can present a lesson for scholars of the history of science, it may be that it is impossible to examine a scientific argument fairly unless he or she fully understands the state of knowledge that existed at the time the argument was proposed.

Notes

1. T. D. Lysenko, *Heredity and Its Variability* (New York: King's Crown Press, 1946). The translator of this pamphlet was the outstanding geneticist, Th. Dobzhansky, who emigrated from Russia to America in 1927. The plan for this publication was prepared in collaboration with American geneticist L. C. Dunn during World War II. They expected that leading Western geneticists would understand what Lysenko's theory really was through this translation and would write their critical reviews publically. The hidden goal of the translation was to raise criticism against Lysenko's theories and to convince the Soviet government that Lysenko's ideas were scientifically barren. For details, see N. Krementsov, "A 'Second Front' in Soviet Genetics: The International Dimension of the Lysenko Controversy, 1944–1947," *Journal of the History of Biology* 29 (1996), pp. 229–250 and O. S. Harman, "C. D. Darlington and the British and American Reaction to Lysenko and the Soviet Conception of Science," *Journal of the History of Biology* 36 (2003), pp. 309–352.
2. For example, see R. Goldschmidt, "Heredity and Its Variability," *Physiological Zoology* 19 (1946), pp. 332–334, L. C. Dunn, "Heredity and its Variability. T. D. Lysenko (Book Reviews)," *Science* 103 (1946), pp. 180–181, and Th. Dobzhansky, "Lysenko's Genetics," *Journal of Heredity* 37 (1946), pp. 5–9.
3. Dobzhansky, ibid., p. 9.

4. British and American geneticists had personal—that is, not scientific—reasons for writing about Lysenko, because not a few of them had a close connection with Soviet geneticists purged; especially, Hermann J. Muller, who worked in cooperation with Nikolai Vavilov in Moscow from 1933 to 1937, showed indignant reactions to Lysenko in his two articles: "The Destruction of Science in the USSR," and "Back to Barbarism-Scientifically," printed in the *Saturday Review of Literature* of December 1948.
5. There are many studies on the history of Lysenkoism to which I owe my study. Especially, studies of the Soviet Union case are abundant. For example, Z. A. Medvedev, *The Rise and Fall of T. D. Lysenko* (New York: Columbia University Press, 1969), D. Joravsky, *The Lysenko Affair* (Cambridge, MA: Harvard University Press, 1970), L. R. Graham, *Science in Russia and the Soviet Union: A Short History* (Cambridge: Cambridge University Press 1993), N. Krementsov, *Stalinist Science* (Princeton: Princeton University Press, 1997), E. I. Kolchinskii, *V Poiskakh Sovetskogo Soyuza Filosofii i Biologii: Diskussii i Repressii v 20-kh nachale 30-kh gg.* (Saint Petersburg: Dmitrii Bulanin, 1999), V. Soifer, *Vlast' i Nauka -Razgrom Kommunistami Genetiki v SSSR-* (Moscow: CheRo, 2002), N. Roll-Hansen, *The Lysenko Effect: The Politics of Science* (Amherst, New York: Humanity Books, 2005).
6. Among a few previous studies that adopt an internal approach, Roll-Hansen (2005) has paid careful attention to the discipline of plant physiology in the early 1930s in order to explain Lysenko's emergence and promotion.
7. The word *orthodox* here refers to those holding positions against the Lysenko school. From here forward, the word "geneticists" alone always refers to orthodox types, not to geneticists of the Lysenko school.
8. For the cultural aspects of the Japanese case, especially the role of Japanese Marxist philosophers and evolutionists in the controversy, see T. Fujioka, "The Japanese Lysenkoism and its Historical Background," *Istoriko-Biologicheskie Issledovaniya* 5, no. 1 (2013), pp. 7–15.
9. For instance, agronomist Yōichi Fukushima, who served on the Japanese Science Council for a long period from 1949, strongly promoted Lysenko's agricultural proposals. He supervised the Japanese translation of Lysenko's main work *Agrobiologiia* (See H. Ōtake and N. Kitagaki, *Ruisenko to Sono Gakusetsu -Shutyo Nougyōseibutsugaku no Kaisetsu-* [*Lysenko's Theory, Explanation of His Main Work Agrobiologiia*], supervised by Y. Fukushima, (Tokyo: Nauka, 1951)). In his commentary of this translation, Fukushima criticizes his opponents as follows: The reason why reactionary scholars like Hitoshi Kihara criticized the defects of Lysenko's theories is ascribed to their ignorance of the ABCs of science, that science has the methodology that works effectively with the correct world view, and also to their ignorance of the fact that in the Soviet Union, politics itself are based on a correct world view and then practiced by the correct methodology…In this perspective,

we can easily detect his confusion between scientific methodology that should be universal and one's own worldview.

10. The title of this book is *Nihon no Ruisenko Ronsō* [*The Lysenko Controversy in Japan*]. The first edition was published in 1967. This book covers the comments, attitudes, and behaviors of important figures in the controversy. It has been regarded as the most comprehensive study on the subject.
11. See T. Nakamura, *Nihon no Ruisenko Ronsō* [*The Lysenko Controversy in Japan*] (Tokyo: Misuzu Shobō, 1997), pp. 30–34.
12. *Ibid*., pp. 117–119.
13. For this subject, an introductory article written in English exists. See T. Komai, "Genetics of Japan, Past and Present," *Science* 123 (1956), pp. 823–826.
14. 1920 was also the year when the Genetics Society of the UK was established.
15. Description here is based on T. Tatebe, *Goldschmidt: Nippon to Kankei no Fukakatta Idenseirigaku no Pioneer* [*Goldschmidt: Pioneer of Physiological Genetics Who Had Close Relations to Japan*] (Osaka: Kansaitosho-shuppan, 1984).
16. The fact that Toyama started his research career under the guidance of Ishikawa partly explains the reason why Ishikawa would not compromise with the matter unless an authoritative figure like Goldschmidt became his prominent pupil's successor.
17. For both detail of the research style and disciplinary trend of German genetics and a comparison between research in Germany and in America, see J. Harwood, *Styles of Scientific Thought—The German Genetics Community 1900–1933* (Chicago and London: The University of Chicago Press, 1993).
18. H. Kikkawa, "Konchū no Tryptophan Taisha wo meguru Shomondai [Problems on the Tryptophan Metabolism of Insects]," *Kagaku* [*Science*] 13 (1943), p. 14.
19. T. Hiroshige, *Sengo Nihon no Kagaku Undō* [*The Scientific Movement in Postwar Japan*] (Tokyo: Thūōkōron-sha, 1960), pp. 49–50.
20. A. Yuasa, "Shintyaku Zassi kara Mita Gaikoku no Saibougaku [The Foreign Cytology Seen from the Newly-arrived Journals]," *Iden* [*Heredity*] 1 (1947), pp. 27–30.
21. I summarize here the recovery process of information exchange between Japan and the Soviet Union based on related articles from the (Japanese) National Diet Library's monthly informational report *Biblos*. In July 1951, the Science Council of Japan accepted the offer to resume a book exchange with the library of the Academy of Sciences USSR in Leningrad. Though the kinds of books were at first limited to official documents such as government publications, in April 1953, the exchange began to cover broader varieties of books. In 1956, when the pace of exchange became regular, a lot of Soviet newspapers and

periodicals became available in Japan. At that time, some Michurinist works published by VASKhNIL were included in the exchanged items.

22. See note 1.
23. R. Yasugi, "Ruisenko Gakusetsu ni tsuite [About Lysenko's Theories]," *Shizenkagaku* [*The Natural Science*] 8 (1947), pp. 17–27.
24. In order to seek the experimental data of the Lysenko school, Yasugi visited agricultural institutes, but only managed to find a single copy of *Yarovizatsiya* (No. 1, 1941). See R. Yasugi, "(Saiho) Ruisenko Gakusetsu ni tsuite [Again about Lysenko's Theories]," *Shizenkagaku* 12 (1947), pp. 2–24.
25. H. Kihara, *Komugi no Sosen* [*The Ancestor of Wheat*] (Osaka: Sōgensha, 1947).
26. In the archival documents of Nikolai Vavilov, there is a letter from Kihara dated December 3, 1929, in which he requested that Vavilov send the seeds of some cereal species. The Saint Petersburg Branch of Archives of Russian Academy of Sciences, f. 803, op. 3, d. 85/ l. 1.
27. The difficulty of obtaining permission from SCAP (the Supreme Commander for the Allied Powers) to attend was described in K. Iida, "Practice and Politics in Japanese Science: Hitoshi Kihara and the Formation of a Genetics Discipline," *Journal of the History of Biology* 43 (2010), pp. 529–570.
28. Darlington had close relations with Kihara. Darlington researched at Kihara's laboratory in Kyoto for about 40 days around 1933.
29. H. Kihara, *Kagakusha no Mita Sengo no Oubei* [Postwar America and Europe as Seen by a Scientist] (Tokyo: Mainichi Sinbun-sha, 1949), p. 1.
30. The two articles are included in H. Kihara, *Komugi no Gōsei- Kihara Hitoshi Zuihitsu-shū* [*Synthesis of Wheat—Collected Papers of Hitoshi Kihara*] (Tokyo: Kōdansha, 1973), pp. 64–126.
31. P. S. Hudson and R. H. Richens, *The New Genetics in the Soviet Union* (Cambridge: Imperial Bureau of Plant Breeding and Genetics, 1946).
32. H. Kihara *op. cit.* [note 30], pp. 127–144.
33. K. Sax, "Genetics and Agriculture," *Bulletin of the Atomic Scientists* 5 (1949), pp. 143, 146.
34. H. Kihara and K. Sax, "Genetics in the U.S.S.R.," *Journal of Heredity* 44 (1953), pp. 132, 158.
35. H. Kihara, "Kaigai ni Okeru Idengaku no Shinpo [The Advance of Genetics in Abroad]," *Iden* 3, no. 4 (1949), p. 1.
36. H. Kihara *op. cit.* [note 30], p. 126.
37. The basic purpose of this group, when organized, was to spread the knowledge of genetics among not only specialists but also ordinary people. In fact, good genetics textbooks were published by the group, like Neo Mendel Kai, ed., *Gendai Iden Gakusetsu* [*Modern Genetics Theories*] (Tokyo: Hokuryū-kan, 1949).

38. Neo Mendel Kai, ed., *Idengaku-Saibōgaku Bunken Sōsetsu 1940–1946* [*Reviews of Literature of Genetics and Cytology 1940–1946*] (Tokyo: Hokuryū-kan, 1948).
39. The author of this chapter was a professor at the University of Tokyo, Jūhei Satō, who did not have special preference for Lysenko and his theories.
40. Nakamura, *op. cit.* [note 11], p. 48.
41. A record of the discussion was published in the Journal *Iden* 2, no. 6, pp. 14–20, entitled "Group Discussion: The Criticism of New Genetics."
42. For the circumstances surrounding Lysenko's *Heredity and Its Variability* in Japan, See J. Satō, "Ruisenko Gakusetsu Ronsō no Keika [The Development of Discussion on Lysenko's Theories] (1948)," included in Neo Mendel Kai, ed., *Ruisenko Gakusetsu* [*Collection of Articles on Lysenko's Theories*] (Tokyo: Hokuryū-kan, 1949), pp. 1–14.
43. Neo Mendel Kai, ed., *ibid*.
44. *Ibid*., preface.
45. Other than Goldschmidt's review (1946) and Dobzhansky's review (1946), a Japanese translation of a documentary on the August session: C. Zirkle, *Death of a Science in Russia* (Pennsylvania University Press, 1949), was published in 1952.
46. As written previously, Kihara was able to obtain *The New Genetics in the Soviet Union*, and based on this book, he examined Lysenko's experiments. However, this was an exceptional case. The Japanese translation of the collective works of the Lysenko school of *Agrobiology* was first published in 1951 by the Russian book agency *Nauka-sha*. In addition, with regard to the proceedings of the August session of VASKhNIL in 1948, an incomplete volume of the transcript was published in 1949 under the edition of Ryūichi Yasugi and botanist Yōichi Takanashi. The complete volume was published much later in 1954 by *Nauka-sha*. In both America and Britain, a complete English translation was published by December 1948. This gap explains Japan's delay in accepting information on Soviet genetics.
47. Zenji Suzuki, who was a young biologist in the early 1950s, talked about his memories of his own experiments in vegetative hybridization at the symposium held in Tokyo in February 2012. His account made it clear for me that serious scientific concern with Lysenko's experiments was not rare in Japan in those days. See Z. Suzuki, "Nihon ni okeru Michūrin Undō no aru Sokumen- Hitori no Seibutsugaku-Nōgaku Gakuto niyoru Dokyumento [An Aspect of the Michurin Movement in Japan: Memoirs of a then Young Biologist and Agronomist]," *Seibutsugakushi-kenkyū* 88 (2013), pp. 5–13.
48. From the early to mid-1950s in some rural districts of Japan, not biologists but ordinary farmers tested vernalization in practice on a large scale. This movement is known as *Yarobi Undō* [Vernalization Movement].

49. Y. Umeya, *Keishitsu to Kankyō* [*Trait and Environment*] (Tokyo: Iwanami-shoten, 1951), p. 448.
50. Quotation from the preface of the *Gendai Iden Gakusetsu* [note 37].
51. Satō points out that the more problems for genetics to treat emerging in the differentiation of disciplines in the postwar period, the more specialized the methodology of genetics became. Thus, the cooperation between genetics disciplines would be more needed than ever. J. Satō, "Kagaku no Bunka to Kyōryoku [Differentiation of Scientific Disciplines and Cooperation Between Them]," *Iden* 4, no. 3 (1950), p. 1.
52. Although Shinotō never manifested approval to the Lysenko school in his writings, he explicitly showed longtime interest in the phenomenon of vegetative hybridization. Since he had gained, according to his insistence, a vegetative hybridization between tomato and eggplant in October 1944, he made several attempts to gain other types of vegetative hybridization of *Solanaceae* plants. Those experiments can be regarded today as the serious test of Lysenko's theories by a professional geneticist.
53. As for gene, Shinotō calls it *Yōso* [*Elements*].
54. Lysenko's early work in the field of plant physiology, which is known as vernalization, seemed to relate to this second phase.
55. Y. Shinotō, *Idengaku-shi-kou* [*Lecture on the History of Genetics*], 4th ed. (Tokyo: Chikara-shobō, 1949), p. 160.
56. This might be regarded as a common goal of all geneticists in those days. In a short article entitled "Dreams of Geneticists after 100 Years," Jūhei Satō predicted that a series of reactions during the course of phenotypic expression would be completely investigated with regard to all traits by every living organism. *Iden* 3, no. 3 (1949), p. 24.
57. The fact that Goldschmidt felt ressentiment to Morgan's group, which disregarded his physiological approach in the interwar period, tempts us to adopt the conclusion. See, Harwood, *op. cit.* [note 17], pp. 49–50.
58. Y. Nagatsuka, "Ruisenko Gakusetsu to Mendelism wo Sōgō Suru Sagyō-Kasetsu no Teian [A New Working Hypothesis Synthesizing Lysenko's Theory and Mendelism]," *Seibutsu-kagaku* [*Biological Sciences*] 1, no. 3 (1949), pp. 129–136 [Esp. pp. 131–132].
59. Positions of this kind can be found in several articles, for example, see K. Tanaka, "Iden Ryūshi-setsu to Zensei-setsu [The Particulate Theory and the Preformation Theory]," *Kagaku* 19, no. 9 (1949), pp. 406–409.
60. Another indicator of the naiveté in Japanese genetics knowledge is that the substance and structure of a gene had not yet been identified perfectly, although much information on a gene's character, by that time made clear owing to radiobiological or biochemical approaches, was revealed by the end of the 1940s in Japan. See M. Iijima, "Senshokutai no Kōzō [Structure of a Chromosome]," in Neo Mendel Kai, ed. *op. cit.* [note 38], pp. 97–106.

PART II

The Lysenko Effects: Biology and Philosophy

Dialectics Denied: Muller, Lysenkoism, and the Fate of Chromosomal Mutation

Luis Campos

> "It should be noted that this admission of the existence of chromosomes is the only common element between Lysenko and the neo-Mendelians."
>
> – J. Huxley, *Soviet Genetics and World Science: Lysenko and the Meaning of Heredity* (1949), 99.

Chromosomal mutations were a compelling new field of research for both American and Soviet investigators in the 1920s and early 1930s. Investigators on both sides of the Atlantic, strongly influenced by Hugo de Vries' "mutation theory" of 1900 (which called for sudden emergence of new species in the space of one generation) eagerly sought to account for the emergence of many experimentally produced new species and varieties in terms of the number, arrangement, and interconnections of chromosomes resulting from polyploidy, trisomy, and reciprocal translocation. Correlating the appearance of potentially important phenotypic changes in plants with their chromosomal components, investigators also sought to explain the possibility of experimental evolution by appeal to "chromosomal mutations" that operated at a level that was neither solely organismic (as de Vries had held), nor solely genic (as these transformations occurred

L. Campos (✉)
University of New Mexico, Albuquerque, NM, USA

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completely independently of any changes known to have occurred in genes). This category of "chromosomal mutation"—although it appears almost nowhere in the secondary scholarly literature on the history of genetics—was in fact widespread in the primary genetics literature of the early twentieth century.[1]

And yet, although widely established in the 1920s and early 1930s in both the US and the USSR (two countries that investigated the phenomenon most intensively, although similar research had, of course, been done in Germany, England, and elsewhere), chromosomal mutations rapidly began to disappear as objects of serious scientific consideration in both the US and the USSR at roughly the same time—within the space of only a few years starting in the mid-1930s. Shockingly, much of the early important and pathbreaking work on natural, radiation-, and chemical-induced chromosomal mutation that had been done in this early period rapidly came to be overlooked and its existence even forgotten.

Many reasons surely exist for the decline of this level of chromosomal mutations as an object of scientific investigation—H. J. Muller's epochal 1927 discovery of the "artificial transmutation of the gene" by means of X-rays being arguably chief among them. In subsequent years, as Muller's studies of the genetic loads of mutation in fruit flies changed mutants into monsters, mutation of any kind attained a largely negative valence. With the dawn of the atomic age, fears of the genetic dangers of ionizing radiation—concerns first thoroughly investigated and popularized by Muller—meant that mutation was most frequently characterized and studied at the genic level. Accordingly, chromosomal mutations were increasingly referred to as "chromosomal aberrations," and no longer seen to be central to the evolutionary process.

Muller's 1927 discovery was interpreted rather differently in the US and the USSR, however— in the US, it was the production of fortuitous genic mutations in an otherwise "hard" heredity not subject to environmental influences, while some in the USSR held the exact opposite, that it was clear evidence of the influence of the environment upon heredity. Muller's winning of the Nobel Prize in 1946 and his endless crusading for concerns of radiogenic safety in a new nuclear age, while fascinating, can thus be only one part of the story in the constriction of the meaning of mutation to the genic level.

In fact, the very coincidence of the disappearance, or at least dramatic downsizing, of the very category of "chromosomal mutation" in the history of genetics in both the US and in the USSR, and at nearly the same

time, suggests that there is another previously unacknowledged factor at work. The death of "chromosomal mutation" may be a heretofore unremarked casualty of Lysenkoism.

To use the vocabulary of cytogenetics, Lysenkoism was a chiasma bringing together two traditions, reworking them, and ultimately transforming them, each by the other. The geopolitical implications for genetics occasioned by the rise of Lysenkoism meant that Lysenkoism polarized the available options to geneticists—in the US, active visions of a biologically engineered Promethean future were replaced by fears of mutation, making it increasingly tricky to research or refer to chromosomal-level phenomena as anything other than aberrations or signs of artificially induced damage. For American geneticists living in the wake of Muller's epochal discovery, any acknowledgment that heredity might also be based in some level of heredity other than the genes led to fear of association with Lysenkoism. In the Soviet Union, on the other hand, Lysenko's and his supporters' attacks on promising Soviet chromosome studies—attacks that were carried out for many of the same sorts of reasons as the attacks against belief in genes—led to the sidelining of an important homegrown tradition of research. So closely associated were chromosomes with ideologically suspect "genes," that it became as dangerous to study chromosome-level phenomena as it was to study gene-level phenomena.[2] Even claims to have engineered chromosomes for productive agricultural benefit became problematic for some Soviet geneticists.

Lysenkoism thus appears to be a heretofore unremarked agent in the ultimate fate of "chromosomal mutation," denying the dialectical level of the chromosome midway between gene and environment, leading to polarized conceptions of heredity, and causing a previously unidentified but important epistemological constriction in the meaning of mutation for both Soviet and American geneticists. That such a result could obtain from traditions of genetic research that at first shared so much in common is a remarkable twist of history. How did this happen?

Prometheanism

Lysenko's claims to remake nature to suit the needs of man—often quoting Ivan Michurin—are readily familiar: "We must not wait for favors from nature; our task is to wrest them from her" or "It is possible, with man's intervention, to *force* any form of animal or plant to *change more quickly and in a direction desirable to man*. There opens before man a broad field

of activity most useful for him."[3] Or again: "Man can and must create new breeds of plants better than Nature."[4] Popular articles with titles like "With the Power of Mankind, We Will Seize the Key to Variability of Plant Forms from Nature" appeared in Soviet sources. Much has been made in the secondary literature of the challenge the eminent Soviet geneticist and naturalist Nikolai Vavilov raised to Lysenko: "You can refashion heredity?" to which Lysenko is said to have replied, "Yes, heredity!" Such a quote is often included in historical accounts with an implication that this—as well as the other statements—verge on ridiculous overstatement. "The notion that nature could be reshaped at will to suit communism," historian Valery Soyfer has noted, "was an easy corollary from Lysenko's claim that there was no such science as genetics."[5]

Such Promethean claims would not have been out of place even before Lysenko, however, as the 1920s and 1930s witnessed a period of a great efflorescence of "biological engineering." Indeed, Promethean claims for evolutionary engineering and the benefits of induced mutation were once common currency in both the US and the USSR in the decades before Lysenko's rise to power.[6]

The respected geneticist Nikolai Vavilov himself had been making statements of just this sort for years. In 1931 Vavilov declared that he wanted to "direc[t] the evolution of cultivated plants and domestic animals according to our will," and in an introductory lecture at the Faculty of Agronomy in Saratov he promised students of plant breeding: "In the near future man will be able to synthesize forms completely unimaginable in nature." Elsewhere, Vavilov remarked:

> By knowledge of the past, by studying the elements from which agriculture had developed, by collecting cultivated plants and domestic animals in the ancient centers of agriculture, we seek to master the historical process. We wish to know how to modify cultivated plants and domesticated animals according to the requirements of the day. We are but slightly interested in the wheat and barley found in graves of Pharaohs of the earliest dynasties. To us, constructive questions—problems which interest the engineer—are more urgent.[7]

Such grand promissory claims for the future of genetic research—remaking nature to suit the needs of man—were as common in the US as they were in the Soviet Union. The famed American horticulturalist Luther Burbank, for example, had decades earlier been claiming that man could "go nature one better": "man, by perseverance, patience, watchfulness, study, care and love, may aid immeasurably

in the processes of… evolution. He may accomplish in ten years what nature takes ten centuries to do. For more than half a century I have had one definite object—the improvement of the vegetable kingdom for the benefit of man."[8] Newspaper descriptions of other new institutions for genetical research drew on similar language:

> The dream of Bacon, who saw in the *New Atlantis* gardens, devoted to the modification and improvement of animals and plants at man's will, is being more than realized by the Carnegie Institution at its new 'Station for Experimental Evolution' at Cold Spring Harbor, Long Island. In this ten-acre plot man—long content with his part as caretaker and subjugator of living species—is now learning the new role of creator.[9]

And the director of the Station himself, Charles Davenport, claimed that:

> A knowledge of the principles of evolution… shows how organisms may be best modified to meet our requirements of beauty, food, materials and power… by using the already known principles of evolution great practical advances have been made in the past. We are consequently justified in expecting that an extension of evolutionary principle will result in further advances in the future.[10]

Both American and Soviet geneticists were strongly influenced to consider a new engineering approach to heredity by the work of the Dutch botanist, Hugo de Vries, who published his "mutation theory" in 1901. De Vries held that new species could emerge in the space of one generation, in an abrupt leap, and thought he had found experimental proof in the evening primroses growing in his garden. His work influenced scores of researchers and his aim—seeking to make evolution an experimental science—essentially set the research agenda for the Department of Genetics at the Station for some time to come. (De Vries was invited to give the inaugural address at the Station in 1904, where he suggested the use of radium and X-rays as possible means of getting experimental control over evolution.)

De Vries' theories proved immensely influential. Among de Vries' greatest admirers in the USA was Albert F. Blakeslee, the second director of Cold Spring Harbor, who likewise searched for a species for his own investigation that had entered into what he hoped was a de Vriesian "mutating period." Studying the jimsonweed *Datura stramonium*, Blakeslee ascertained that the number, arrangement, and interconnections of chromosomes—completely independently of any changes known to

have occurred in genes—were correlated with phenotypic changes in his plants. These changes arose from polyploidy, trisomy, and "segmental interchange" (reciprocal translocation), among other mechanisms. Blakeslee concluded that these phenomena constituted an intermediate realm of variation and mutation that he called "chromosomal mutation" that operated at neither the organismic nor the genic level. This was a rather significant departure from the famed approach of Thomas H. Morgan and his associates, whose studies on the fruit fly *Drosophila* had first led to the development of the "chromosome theory of heredity"—the belief that the genes were like pearls on the string of the chromosome. While Morgan's "chromosome theory of heredity" had, oddly, little to do with chromosomes except to treat them as bearers of hereditary units, Blakeslee's model proposed that chromosomes acted in addition to genes as hereditary entities themselves, and that their differing combinations could be sources for phenotypic variation.

Morgan's reliance on *Drosophila* as a model genetic system obscured some alternative genetic mechanisms (such as polyploidy) that were much more common among plants.[11] And, intriguingly, it was Blakeslee's work involving chromosomal mutations in the 1930s, not Morgan's, that contains the earliest references to anything explicitly termed "genetic engineering." Mixing and matching chromosomes could lead to what Blakeslee described as "synthetic new species" and he and many other commentators envisioned a future where evolution could be "made up to order." The claims that were made for the social utility of such scientific discoveries were clear in the American context:

> The ability to induce chromosome doubling, therefore, is of importance to practical as well as to theoretical genetics. With increasing knowledge of the constitution of chromosomes and of methods whereby their structure and behavior may be altered, there arises an opportunity for the genetics engineer who will apply knowledge of chromosomes to building up to specifications forms of plants adapted to the surroundings in which they are to grow and suited to specific economic needs.[12]

Similarly, an American volume from 1935 dedicated to Charles Davenport reads: "It is not uncommon procedure these days for the geneticist to make a new variety of plant to order. In animals the procedure is slower and more difficult, but the improved breeds of live stock, pets, and poultry owe their present-day perfection to man's increasing knowledge of the

principles of genetics. And the end is not yet."[13] Such goals were strikingly similar to some of the advertised goals of later Lysenkoism. Therefore, when even as late as 1937 Lysenko was still declaring that "the conversion of one species into another takes place by a leap," this was something that American geneticists had at one time readily believed and worked with, even if by the 1930s they were no longer as enamored of de Vries' mutation theory as they had been in 1910.

But Soviet investigators, too, drew intellectual lines of filiation to de Vries, and to his vision for developing ever finer experimental control over evolution. (Vavilov, for one, who was known to dream of the mutation theory, had met de Vries in 1922 and wrote to him as late as 1928 to say that de Vries' picture hung in every Russian genetics laboratory.)[14] Soviet and American investigators alike thus drew on a shared discourse of mutation coming from a common mentor with Promethean aims.

The Soviet Context

New methods were developed to make such aims reality. One of Blakeslee's greatest successes in inducing mutation in plants had been by using the chemical colchicine, whose retarding action led to polyploidy, or multiple copies of entire sets of chromosomes instead of the usual diploid ($2n$) number.[15] Immediately after reports of Blakeslee's work, Soviet investigators likewise began to use colchicine to "control evolution" according to their own lights. In fact, the Soviet Union had a remarkably strong tradition of its own investigating chromosomal mutations.

The study of polyploidy had been "a strong field in Soviet science until 1948, thanks to Karpechenko, Zhebrak, Sakharov, and others," according to Soyfer.[16] Indeed, in 1946 Eric Ashby wrote that there were schools of old genetics in the Soviet Union that were "setting the pace on world standards in such fields as population genetics and the use of colchicine-induced polyploids in plant breeding."[17]

One ardent researcher of artificial amphidiploidy and "[o]ne of the most gifted geneticists of the twentieth century," was Georgii Dmitrievich Karpechenko.[18] His work on the production of allopolyploids—"hybrids" or "mutants" that were neither genic nor organismal in mode of origin—led most famously to the production of *Raphanobrassica*, a cross made from plants belonging to two different genera. Theodosius Dobzhansky called this hybrid of cabbage and radish a key moment in "formation of

new hitherto non-existent species" that had been "made fertile by doubling the chromosome number." Karpechenko had indeed succeeded in producing "a new synthetic artificial species" with an effort that to some seemed only slightly less strange than Lysenko's own ridiculous and oft-repeated claims, "that it is possible to produce a camel from a cotton seed and a baobab tree from a hen's egg."[19]

Anton Zhebrak, who had once worked in Morgan's lab, reported to *Science* in 1945 that:

> Important results have been achieved in the field of polyploidy, and many new plant forms have been developed. This research is little known abroad, although some of my work on the development of new varieties of [allo-polyploid] wheat has been mentioned in *Science* and *Nature*. Sakharov and Lutkov have developed new varieties of buckwheat; Navashin—kok-sagyz (the Russian dandelion, as it is called in America); Lutkov—flax; Rybin—hemp, etc. The work of these scholars offers splendid prospects for the future selection of the crops concerned.[20] / It therefore follows that experimentally by hybridizing remote forms and by means of chemical treatment with agents that exert a specific influence on nuclear material we obtained new types of wheat hitherto unknown in nature. / I am of the opinion that these data on experimental polyploidy in cultivated and wild plants are a valid proof of the soundness of the contemporary chromosome theory of heredity.[21]

Intriguingly, Zhebrak interpreted his studies of colchicine-induced polyploidy not only as proof of the chromosome theory of heredity, but also as refutations of the Weismannian "autogeneticists":

> In my opinion experiments with amphidiploids, and with tetraploids of buckwheat, millet, rye and a number of other cultivated plants are the most original in the Soviet Union. Their value is not only of a practical kind, it is of considerable theoretical interest as well. In the experimental induction of polyploids in cultivated plants it is important to note that they were obtained by the action on the hereditary material of such an external agent as colchicine. This completely shatters the theoretical premises of autogeneticists who claim that the germ plasm is isolated from external factors and stable in respect to external influences.[22]

Zhebrak was proud of having "obtained new types of wheat hitherto unknown in nature,"[23] and saw these experiments as proof of Marx's eleventh thesis on Feuerbach:

> Experiments with polyploids confirm the dialectical interdependence between the nature of the plant and its environment; they likewise demonstrate that the hereditary nature of a plant which is dependent upon its nuclear complement may be controlled by man. These experiments better than any other work in the field of genetics confirm the justness of the aphorism of Marx, that up to the present philosophers have been trying to interpret the world, whereas the problem is to change it. / Contemporary experimental genetics has mastered the means of remodeling the hereditary basis of plants and reconstructing the vegetable world.[24]

Prior to his ouster by Lysenko, Nikolai Vavilov had also been all in favor of this chromosomal engineering, and reported on a Soviet conference on polyploidy that had taken place in 1938 in Moscow:

> It has been definitely established that chromosome doubling in sterile hybrids between distant species is a phenomenon of comparatively frequent occurrence… The possibilities opened up by the artificial induction of amphidiploidy, i.e., of chromosome doubling in hybrids, are immense. Genetics is entering a new era of extensive application of distant hybridization, at least in the case of plants.

"Distant hybridization" might also work in some cases in animals, Vavilov thought, but he was more interested in reporting on novel "physical and chemical methods for obtaining polyploids and amphidiploids," including "the application of high temperature during the period of flowering," and in particular on "new methods of obtaining polyploids by the aid of colchicine (Dr. Blakeslee) and acenapthene…" Reporting on the findings of Karpechenko and others on barley and flax, and others who obtained polyploids in tobacco, lettuce, petunia, rye, hemp, poppies, and various medicinal plants, and reporting as well on Zhebrak's work on unusually successful distant wheat crosses, Vavilov concluded that "the general impression of the 1938 conference was that genetics and plant breeding are entering a new era of great possibilities for the radical transformation of species and varieties and of the extensive use of distant hybrids for practical purposes, inasmuch as sterility may be overcome by the method of artificial amphidiploidy."[25]

Chromosomal mutation on both sides of the Atlantic frequently centered in its experimental phase around the chemical colchicine. This is not to suggest a one-way flow from Blakeslee's early use of colchicine to the Soviet use of it. Indeed, the study of chromosomal variability and mutation by means of chemicals had been a significant field of research in the

Soviet Union, and J. A. Rapoport "was the first in the world to experiment extensively with chemical mutagenesis."[26] Polyploidy by colchicine was simply an obvious extension of earlier Soviet efforts to produce polyploid varieties by other methods.

American geneticists had likewise readily accepted the colchicine technique. While in the US such efforts fell under the new label of "genetic engineering," in the USSR they fell under claims to "transform nature." As V. S. Nemchinov noted, "by understanding the effect of tampering with and manipulating chromosomes, we can transform nature and by using the change in the generations of plants and animal organisms we can alter their form. I think that the chromosome theory of heredity is one of the great discoveries in biology." (While applause was initially recorded to have been the response to such a statement, this was relabeled "commotion" in the published edition of his remarks).[27]

Just what the "chromosome theory of heredity" *meant* appears to have differed by national context—in the US, it meant Morgan-style mapping of genes onto chromosomes, and the chromosomes as the material bearers of heredity, but with the focus of the study of heredity primarily on the hereditary qualities of the gene. In the Soviet Union, at least for those like Nemchinov, the chromosome theory of heredity was interpreted in what was more directly related to the number and properties of chromosomes as determined by experiments in allopolyploidy—a Soviet parallel to the cytogenetic work being done by Blakeslee.

Rapoport also made strong claims for the use of colchicine to improve Soviet science and to refute the dangerous Weismannism:

> Mutation is an immense achievement of Soviet science in that it revealed the powerful operation of external physical factors and the operation of chemical factors. … These achievements are that we Soviet geneticists have found chemical agents which enable us to obtain at will hereditary variations many thousands of times more often than was the case before. … As a result of this work we can say that we have utterly refuted Weismann's proposition that the germ cell is contained in a separate case. There is no such case, for the germ cells change with the same frequency as the body cells.
>
> ...Nor do we make adequate use of the artificial polyploidy method, which we vulgarly call colchicine treatment, but by the aid of which we obtain a double unit of heredity. We can see kok-saghyz, tau-saghyz, sunflower, hemp and other plants the size of which is twice that of the initial diploid plants. Hundreds of similar examples can be quoted in the case of decorative plants. Nevertheless, there is no sign of that perseverance which is necessary

> to squeeze the utmost out of the polyploidy method. This method is important because of its practical potentialities, but its theoretical significance is also great. It shows that it is possible to produce by human hand species which took an immense length of time to create in nature (tobacco, plums).
>
> ...We must not simply ape others, but it is our duty critically and creatively, as V. I. Lenin taught us, to assimilate all that has been done abroad. We must carefully tend the shoots of what is new, and train new cadres who will be able to push science further forward.[28]

But from similar claims and similar methods in the US and the USSR came different reasons to eschew such conceptions of chromosomal mutation. While for Americans, such work was at most likely to contribute to goals of genetic engineering, the focus on genes as the elements of heredity far outshone the work done with chromosomal arrangements. The Soviet story is equally complicated, albeit in a different direction.

For starters, Lysenko was suspicious of colchicine. Although Lysenko himself had claimed on occasion to have induced chromosome duplication—the transformation of durum wheat (28 chromosomes) into soft vulgare wheat (42 chromosomes)—he valued such work not for its presumed cytogenetic value but because it established that *"the conversion of one species into another takes place by a leap."* And yet, he noted, "it is not the numbers of chromosomes that determines the quality of a strain." Polyploidy alone could never be sufficient.[29] In short, Lysenko was suspicious of attributing too much to the effects of colchicine on chromosomes, even though he himself had claimed on occasion to have induced chromosome duplication.

Although he did not mean to "deny the biological role and significance of chromosomes in the development of the cells and the organism,"[30] Lysenko also noted that heredity "is inherent not only in the chromosomes, but in every particle of the living body."[31] Lysenko claimed not to be standing in the way of Soviet cytogenetic work or to its association with "practical agriculture in our country," calling such a claim "calumny," but nevertheless criticized those who conducted such work on the grounds of their poor productivity:

> I know that many institutes have been engaged and are engaged in this sort of—in my view—scarcely productive activity. More, the Ministry of Agriculture set up a special institution, headed by A. R. Zhebrak, to study questions of polyploidy. I think that this institution, though it has for some years done nothing besides its work on polyploidy, has produced literally nothing of practical value.[32]

Lysenko remained skeptical of the use of chemical mutagens in a science of heredity. In *The Science of Biology Today*, he wrote:

> We do not deny the action of substances which produce mutations. But we insist that such action, which penetrates the organism not in the course of its development, not through the process of assimilation and disassimilation, can only rarely and only *fortuitously* lead to results useful for agriculture. It is not the road of systematic selection, nor the road of progressive science.[33]

Fortuitous results alone did not a science make, for Lysenko:

> Mendelism-Morganism is built entirely on chance; this 'science' therefore denies the existence of necessary relationships in living nature and condemns practical workers to fruitless waiting. There is no effectiveness in such science. With such a science it is impossible to plan, to work toward a definite goal; it rules out scientific foresight.

Although Lysenko did not deny that such mutants were possible, and that they exhibited large changes all at once—he thought evolution took place through just such kinds of changes—he viewed such induced changes as merely fortuitous, unreliable, and the Mendel-Morganist explanation offered for their production ideological.

Clearly, then, Lysenko did not deny that chemicals can cause effects in producing mutations; he agreed in defining mutations as large-scale shifts (such as what Blakeslee and many of the Soviet geneticists were seeing); but he felt that these could not be produced at will because they were caused by chance and not by law and therefore were not the proper base for a science of heredity for the people. Moreover, he viewed it as a problem that such chemicals would act only at one moment rather than over the lifespan of the organism (as his understanding of heredity necessitated).

Lysenko was thus opposed to "chromosomal mutations" as the engine of evolution, even as he knew that such techniques could be fortuitously productive. This was an uneasy balancing act: just because colchicine could work did not mean Lysenko was in favor of its continued use in a program of agricultural improvement. Lysenko even at one point came to describe colchicine as "one of the strongest poisons," saying that it "deforms plants. Cells cease to divide normally, and something like a cancerous tumor develops."[34] In 1940, Lysenko was reported to have said: "By treating plants with a very powerful poison, colchicine, and other torturing applications they [the neo-Mendelians] mutilate plants.' He also added that colchicine treatment was of not practical value."[35]

Others agreed; G. A. Babajanyan noted in 1948: "We are told that this chemical substance is already causing a large number of mutations. But what does it really amount to? It would be better if this 'large number' did not exist, because the organisms obtained in this way are all trash, freaks!"[36] But Andrei Zhdanov found such "a bouquet of epithets… far from appropriate for encouraging a new task. It is strange to hear an innovator say that a new plant form is abnormal. But I tell you: We don't give a damn about normal or abnormal, the important thing is to get bigger crops, a higher harvest!"[37]

Despite Lysenko's suspicion of chemical means of inducing mutation, a few decades later, in December of 1962 and February of 1963, Lysenko referred to *radiation* mutagenesis as an achievement for Soviet science. In referring to the discovery by Astaurov of "a process for regulating the sex of silkworms by irradiation and otherwise influencing their cell nuclei, chromosomes, and genes," Soyfer has noted, Lysenko reported an increase in yield of 25%–30%. "Who now… seriously doubts the possibility of literally molding, literally creating, winter-hardy winter plants out of entirely non-winter-hardy plants, such as spring wheat or barley," Lysenko wrote in *Pravda*, "by utilizing the conditions of the nonliving external environments?"[38] Why radiation mutagenesis merited praise while chemical mutagenesis was suspect for Lysenko remains curious.

Meanwhile, for their part, anti-Lysenkoist "Mendel-Morganian" geneticists castigated Lysenko for making polyploidy into a "fetish" rather than a "tool… a very sharp and valuable tool." The implication of their criticism of Lysenko—"Those who use it intelligently keep track of the ploidy of their strains as they go along"—was that although Lysenko might like the idea of polyploidy strains, he simply did not understand them.[39]

The American Context

Some version of the genetic engineering ideal was thus common to the study of heredity in both the US and the USSR. Scientists on both sides of the Atlantic were fascinated by mutations, and impatient with having to wait for them to occur. They wanted to take control of evolution into their own hands. H. J. Muller, for example, was frustrated with having to wait for mutations—studying mutation frequency was like hoping to find a dollar bill on a sidewalk, he once said—and he wanted to come up with a method instead to detect the *frequency* of mutations, even those mutations not readily apparent (he then invented a particular *ClB* strain of fruit flies with lethal mutations to study this phenomenon). He also

wanted to find a way to increase the mutation frequency—using radium and X-rays—and began looking for such ways to control mutation from very early on.[40] What is most intriguing is that for all his metaphysical genism—viewing the gene as the fundamental particle of life and of variation—Muller shared with many Soviet scientists (active both prior to and during the reign of Lysenkoism), many of the same sorts of Promethean goals of transforming nature..

Even as early as 1916, in an unpublished manuscript entitled "Applications and Prospects," Muller noted that: "Stockard, reporting that alcohol may induce heritable changes in nervous system; not acquired characters—do not appear in animals that are treated, but in progeny and are handed down." And Muller concludes: "These findings, if confirmed, will constitute the first known cases of induced mutations."[41] Muller spoke here of the inheritance of characters to be acquired in the following generation, not quite the inheritance of acquired characters as Lysenko would have it. But shortly following his famous 1927 experiment, Muller noted in a lecture that we can envision a new kind of artificial evolution by

> simply concentrating the processes of nature, defeating time, and accordingly involving ourselves in a greater outlay all at once, but getting correspondingly greater returns, and in a far shorter period.… If you ask me, what new forms of plants and animals can we hope eventually to produce through such artificial evolution, I must say, the answer will vary with such species in question, with our needs and with its possibilities, about which we can make few or no specific assertions in advance… Human imagination is too shortsighted to see more than a few steps ahead… we may after all make some headway on this sector of the biological battlefront: we cannot make life—far from it; we probably can, however, *remake* it.[42]

Despite coming from the most ardently genic of classical geneticists, these Promethean claims are remarkably resonant with those of many others with differing views of heredity. Indeed, some of Muller's descriptions sound just as stridently optimistic as any of the standard Lysenkoist tropes: "For although we cannot make l[iving] things—far from it—we can remake them, and we will not forever let this knowledge go unused."[43] Muller was after the productive use of mutations for improving human understanding and human betterment—in various ways—and for him *the gene* was the way to do so. But Muller did not stop at plants and animals:

> [W]hat of man himself? Must we always remain aloof from the challenge thrown to us by our own genes? …It is humiliating to think that all mankind is entirely at the mercy of this little mite of material; that he is unable in any way to alter or control anything that may happen to it, no matter what consequences for himself such uncontrolled changes in his genes may have. We must not be satisfied until we can turn the tables on these genes, so that instead of admitting that 'the individual is only the gene's way of producing more genes' we can truthfully make the statement that 'the gene is only the individual's way of producing more and better individuals.'[44]

Years later, in 1947 to an audience of 4000 at Indiana University celebrating his winning the Nobel Prize, Muller remarked: "Included in these possibilities, we now see, are not only the physical ones of remaking the earth, and we can now be sure, adventuring upon other planets, but the biological ones of remoulding the life forms around us, and in the end, even our own inner nature, so as to make us more godlike."[45]

Muller's colorful Promethean statements are fully the match of the promissory language of Lysenko and his followers. "If it is true that we can produce various qualitative changes inside the gene artificially by X-rays," Muller once noted in a lecture at Ithaca, "then eventually we should be able to produce desirable changes in species."[46] The title of the *Times* article reporting the lecture ("Evolution Process Is Aided by X-Rays") resonates familiarly with similar-sounding Soviet titles, like "With the Power of Mankind, We Will Seize the Key to Variability of Plant Forms from Nature."

Such resonances—such structural and discursive similarities—are not to say that Muller's claims were essentially the same as those of Lysenko and his supporters. Far from it. Muller's wanting to control mutations, to get them at will, and to produce new species was of a piece with claims made by Blakeslee, but Muller focused on a much smaller level and argued that such large changes will not take place all at once (indeed, he actively fought against this notion, considered such changes to be fortuitous—and yet somehow still thought they might someday soon come under human control). Timing was important for Muller—affecting the germ cell at just the right moment—and he paid less attention to the larger dimensions of the life of the organism. His Prometheanism is therefore hardly the same as Lysenkoist ideology by most reckoning—although in some contexts it was possible for it to be understood as bearing similar implications.

In fact, Muller's legendary 1927 artificial transmutation of the gene was interpreted in the Soviet Union not simply in the context of Mendelian genetics, as Muller understood it and as Lysenko would have framed it to his own advantage. It was, rather, understood by some as being supremely relevant to the question of Lamarckism—even as Muller and others steadfastly argued against such an interpretation.[47]

Clearly, just what counted as Lamarckism at this period was vigorously contested (not only in evolutionary studies but also in studies of symbiosis and antibody formation).[48] The widespread variety of attempts to induce mutations artificially by the time of Lysenko's rise to power—the induction of mutations by means of radiation, chemicals, moisture, temperature, age, and so forth—appears to have so far destabilized the somagerm distinction for some Soviet geneticists that what drosophilists and many classical geneticists considered to be one of the ultimate successes of their genically oriented, non-Lamarckian research program in classical genetics—Muller's artificial transmutation of the gene in 1927—opened up the possibility for a total transformation of hereditarian thought in the Soviet Union *against* the interpretations of classical genetics. As historian Mark Adams has noted:

> For some Soviet Marxists, Muller's discovery redeemed Mendelian genetics by demonstrating that, far from being eternally fixed, genetic traits could be changed by environmental influences and might eventually be deliberately manipulated and controlled. Serebrovsky, for example, emphasized the scientific and ideological significance of Muller's discovery in an article in the 11 September 1927 issue of *Pravda* entitled 'Four Pages That Shook the Scientific World.'[49]

While Lysenko had narrated "fortuitous" changes caused by colchicine as hardly worthy of consideration, Muller's X-ray radiation mutagenesis was interpreted by other Soviets as proof of the effect of *environmental* influences on heredity—even as for Muller it stood for precisely the opposite, that is, for the significance of the individual generally imperturbable gene in calculations of mutation frequency.

In short, both Soviet and American geneticists could see what they wanted to see in Muller's work. X-ray mutagenesis was not unequivocal proof of genic mutation, but could in fact just as easily be adopted into Lysenkoist environmentalism—just as easily as colchicine had at first been adopted, and then been excluded, from sanctioned Lysenkoist practices.

Such radical underdetermination of theory by evidence requires that we pay close attention to local narratives.

And so, despite the fact that one might otherwise have expected chromosomal mutations to remain of intense interest in the Soviet Union for being a dialectical level above the gene, chromosomal mutations increasingly came under fire in the Soviet Union. Already facing challenges in the West following claims for the death of de Vries' mutation theory and Muller's remarkable 1927 successes, Lysenko's discourse and political considerations (following Muller's own politicking) actively militated against the use of other mutagens besides temperature and moisture that were by then common in the West and that were of increasing interest to Karpechenko and others. Retaining a strong emphasis on breeding and analysis of phenotype, the works of these scientists became most endangered when they endeavored to explain their chromosomal mutant hybrids using the language of modern genetics.

Meanwhile, in the US, the fact that so much important research on chromosomal mutations had been conducted by Soviet scientists (even scientists accused by Lysenko and his supporters of being Mendel-Morganist), appears to have combined with an intensely compelling focus on the gene so as to lead to a further evacuation of attention from the chromosomes as sources of evolutionary novelty in their own right. For American geneticists, chromosomes could be affected by external mutagens, and could even produce new species—but did so by means revealing "damage" or "aberrant" behavior. Chromosomes could not be given the same level of authority as genes as fundamental units of evolution, and could certainly not in the face of a Lysenkoist and Communist threat deliberately bent on attacking the very existence of genes as ideological. This relentless genetical focus on the gene dovetailed with selectionist explanations in the constriction of the evolutionary synthesis during the 1940s, serving also to preclude more structural, chromosomal mechanisms of evolution beyond the gene.[50]

Lysenkoism thus came to be an important factor in the ultimate fate of "chromosomal mutation" in both the US and the USSR. In fact, Lysenkoism brought about effects not only on claims of promissory genetics among American geneticists, but shaped the very content of hereditary knowledge itself, leading to polarized conceptions of heredity and contributed to a constriction of the meaning of "mutation."

Dialectics Denied

Pluralistic and complex views of heredity and evolution above and beyond the merely genic seem to have largely disappeared from much of American genetics during the 1940s and 1950s. This pattern maps well on to the rise of Lysenkoism. I have attempted to suggest here that, in addition to more proximate causes, there is perhaps a larger global or external context at play for these shifts in the meaning of "mutation." After Muller, heredity in American genetics became increasingly genic and selectionist, rather than pluralistically physiological, chromosomal, organismal, and affected by development, drift, and other factors. As the wagons were circled, heredity in the West was increasingly *defined, refined, and constrained* in opposition to Lysenkoist interpretations.

The historically contingent constriction of the meaning of mutation—that chromosomal mutations were not mutations proper—tied in with seeing mutations as damage rather than promissory; to be promissory was to be Soviet, and to study chromosomal mutations as a form of environmentally induced mutation was to run the risk of being associated with Lysenkoist or Lamarckist ideas. In the American context, even the fact that research on chromosomal mutations had been conducted by Soviet scientists—even scientists accused of being Mendel-Morganists—may have combined with a post-Muller tendency to reduce the evolutionary significance of complex hereditary systems to the level of the gene. Talk of such variation—and especially of its artificial induction by carefully chosen environmental mutagens like radiation or colchicine—could unintentionally but unwisely sound like Lamarckist Lysenkoism.

Something in the geopolitical context had clearly changed, causing the chromosome to wither away in the US. What that something was, was clear to everyone. One scientist commented in September 1948 on the different response he had received as a result of changing political circumstances:

> It is striking that while no adverse comment was made when I directed attention to the interesting nature of Lysenko's work seven years ago, it is now alleged that I do so 'to confuse and mislead the public' on political grounds. In those seven years the political situation has altered. The facts concerning inheritance have not.[51]

Or, more pointedly, as a colleague wrote to the cytogeneticist C. D. Darlington in 1949:

> Once you get to the stage of having the plasma-genes in the cytoplasm modified by external agencies and even taking subversive control of the chromosome mechanism, it is not far removed, if not from Lamarckism, then at least from something very deep underlying the superficial tripe that Lysenko has been getting off his chest.[52]

Even J. B. S. Haldane, referring to consequences attending discoveries of "important genetical phenomena which have nothing to do with genes," warned in 1956 that "today, it is not safe to discover such phenomena in certain countries"—mentioning both the US and the USSR. In the 1930s Blakeslee had given a series of broadcast radio addresses speaking of "evolution made to order," and Muller had likewise made many such similar claims. But after the emergence of Lysenkoism, Muller explicitly stated that the kind of promissory hype of the genetic engineering of the 1930s must be *downplayed* to avoid any potential confusion with the legacy of Lysenkoism. Indeed, by 1952, Muller had changed his tone noticeably, was all too keenly aware of how his work could be seen as allied to Lysenkoism. He left his Promethean popularizations behind:

> Furthermore, given effects with so-called ever-sporting genes cannot yet be regarded as pointing the way to control over ordinary processes of gene mutation… And any such differences in mutational spectrum as have been found on comparison of different agents, and of different genetic backgrounds, are entirely too far removed from the production of pre-determined mutations to allow the public to think that we are yet even on the track of positive results in this most important field. So important indeed is this field, theoretically as well as practically, and also so closely connected with the whole Lysenko controversy (inasmuch as the Lysenkoists of course have long claimed to be able freely to produce mutations to order) that any popular treatment which misleads the public in regard to this field is to be avoided for this reason alone.[53]

Muller was well aware by 1952 of how his gene-centered work could be appropriated and reinterpreted by Lysenkoists—after all, even his 1927 X-ray work had been reinterpreted under Lysenko as a contribution to Michurinist principles. Muller therefore only very cautiously discussed the potentials of genetic engineering, fully aware that thoughts of directed mutation could mistakenly be seen to resonate with Lysenkoist views. The control of evolution, in other words, received a bad rap (the history of eugenics is undoubtedly a contributing factor in this story as well).

Moreover, the idea of the "control" of evolution—a dominant Promethean theme of American efforts at experimental evolution since the founding of the Cold Spring Harbor Station for Experimental Evolution—had been eclipsed by a discourse of risk, damage, and fear. A positive message became a negative one; Muller's own concerns over Cold War fallout and his work on genetic loads fits in this story as well. But perhaps there is *also* room here in this revised narrative of the shifting meaning of "mutation" for the effects of fears of Lysenkoist ideology on visions of promissory genetics. After all, it took until the late 1960s for "genetic engineering" to re-emerge as both a term and a goal of American biology—only once the shadow of Lysenkoism had lightened. And it was only in the 1970s that there were first glimpses of a re-emergence of chromosomal cytogenetic evolutionary studies.

The fate of chromosomal mutation in both the Soviet Union and in the USA was thus a case of "dialectics denied"—denied in the Soviet Union, ironically, because although chromosomes existed at a level qualitatively different from genes and presented new phenomena themselves distinct from the genic level, they were seen to be insufficiently materialistic and were too closely associated with genes and "Mendel-Morganism." And chromosomal mutations were denied in the US and elsewhere because talk of such variation, and especially of its artificial induction by carefully chosen environmental mutagens, in not being sufficiently focused on the gene resonated a little too closely with Lamarckist Lysenkoism.

Through a series of historically contingent geopolitical circumstances surrounding Lysenkoism, the study of chromosomal mutation got wrapped up in the Soviet Union in arguments against Morgan's "chromosome theory of heredity." Tragically, what had been a premier field of Soviet genetic research was targeted for being too abstract, theoretical, and ideological, and too close to "Mendel-Morganism." Chromosomal mutations were somehow still too fortuitous or idealistic for Lysenkoists. (As Zhukovsky replied to Lysenko: "You refuse to admit that these mutations are caused by changes in the chromosome. That is where we disagree. It has gone so far that the very mention of the word 'mutation' or 'chromosome' frightens many people.") One terrible consequence was that many geneticists of the Soviet Union, for whom chromosomal mutations were a significant and understudied level of hereditary and evolutionary significance, were soon deposed, exiled, or killed.[54]

On the other hand, American efforts to artificially induce mutations using environmental mutagens—a decades-long set of research programs that sometimes claimed to find stimulating effects (such as radiation horme-

sis) and that actually succeeded in producing recognized new species—were rapidly sidelined as chromosomal mutations were increasingly interpreted as *damage*, as *defect*, as things to be avoided, and as genetic variability was established as being fundamentally about gene-level changes, and that only these were to be called "mutations." The specter of a Lamarckian interpretation of these efforts in radiation genetics and chemical-induced mutagenesis also contributed to the gradual abandonment of these chromosomal efforts. This specter evidently did not haunt Europe alone.

Lysenkoists focused on the phenotype; American geneticists focused on the genotype. Little attention was paid to the evolutionary and potentially controllable space between, the karyotype, the chromosomes, and their mutations. The one geneticist who might be thought of as a key link between Russia and the West—Hermann Hermanovich, as he liked to be called—was caught between his ardent support of Russian geneticists and his even more ardent support of the gene. Even during the period that historians have pointed to as the heart of the modern synthesis, then, the genic "thesis" of Muller and the environmental "antithesis" of Lysenko served to direct attention away almost entirely from the question of chromosomal mutations, and their induction. Geneticists would have to wait decades for their next synthesis. The story of how mutation came to mean a change in a gene is thus in no small measure an example of Lysenkoism not only affecting the geopolitical *context* of heredity science, but even helping to shape its very *content*. Meanwhile, on both sides of the ocean, the solid reality of chromosomal mutation melted away.

Notes

1. As I have argued elsewhere, classical genetics was in fact much more pluralistic in approaches and understandings than we might imagine from our vantage point today. Luis Campos, "Genetics Without Genes: Blakeslee, *Datura*, and 'Chromosomal Mutations,'" in *A Cultural History of Heredity*, Vol. 343 (Berlin: Max Planck Institute for the History of Science, 2008).
2. In this I echo the structural argument of Nils Roll-Hansen, who has argued that "The Lysenko affair stimulated a polarization where Mendelian neo-Darwinism and Lamarckism appeared as the alternatives. When the government chose Lamarckism as in the Soviet Union the effects were disastrous for genetics. But even in the West, where neo-Darwinism prevailed, it was sometimes hard to get support and credit for work that did not fit the dualism." Nils Roll-Hansen, "Lamarckism and Lysenkoism Revisited," in *Transformations of Lamarckism: From Subtle Fluids to Molecular Biology*, eds. Snait Gissis and Eva Jablonka (MIT Press, 2011), pp. 77–88, on 79.

3. Lysenko quoting I. V. Michurin, in Lenin Academy of Agricultural Sciences of the USSR, *The Situation in Biological Science* (Moscow: Foreign Languages Publishing House, 1949), p. 34. Hereafter, *Situation*.
4. *Situation*, p. 86.
5. Soyfer, *Lysenko and the Tragedy of Soviet Science* (Rutgers University Press, 1994), p. xix.
6. Indeed, Promethean claims are far from limited to genetics, or to the US and the USSR—such claims are common features of many experimental sciences.
7. Vavilov, "The Problem of the Origin of the World's Agriculture in the Light of the Latest Investigations," in *Science at the Crossroads*, eds. Nikolai Bukharin et al. (1931), pp. 95–106, on 106, 97.
8. Luther Burbank, "My Greatest Discovery in Fifty Years," *Popular Science*, April 1923.
9. John Elfreth Watkins, "Man as Creator, Wonders of New Station for Experimental Evolution," Illustrated Weekly Magazine, *Los Angeles Times*, February 24, 1907, p. 11.
10. *Carnegie Institution of Washington Year-Book* 5 (1906), p. 92.
11. Even though major improvements in cytogenetics came through careful work on model plants by the 1920s, much of the most successful work in this "lost discipline karyology" has yet to find its historians. (I am indebted to Joe Cain for this phrasing, from comments delivered at the 2007 meeting of the International Society for the History, Philosophy, and Social Studies of Biology held in Exeter.)
12. Blakeslee and Amos G. Avery, "Methods of Inducing Doubling of Chromosomes in Plants, by Treatment with Colchicine," CIW Supplementary Publications No. 34, reprinted from *J Heredity* XXVIII, no. 12 (December 1937), p. 20.
13. Laurence H. Snyder, *The Principles of Heredity* (Boston: DC Heath, 1935), p. 2.
14. Vavilov to de Vries, February 10, 1928. Hugo de Vries Papers, University of Amsterdam, 587:831.
15. For more on the history of colchicine, see Jordan Goodman, in *Molecularizing Biology and Medicine*, eds. Soraya Chadarevian and Harmke Kammiga (1998).
16. Soyfer, p. 236.
17. Eric Ashby, "Genetics in the USSR," *Nature* 158 (August 31, 1946), pp. 285–287, on 287.
18. Mark Popovsky, *The Vavilov Affair* (Hamden, CT: Archon Books, 1984), p. 148.
19. 170; Popovksy 52, quoted from Yuri A. Dolgushin, *At the Source of a New Biology* (Moscow: Goskultporsvetizdat, 1949), pp. 10–11.

20. Anton Zhebrak, "Soviet Biology," *Science* 102 (October 5, 1945), p. 357.
21. "Speech by A. R. Zhebrak," Eighth Sitting, *Situation*, p. 470.
22. *Situation*, p. 475.
23. *Situation*, pp. 468–469.
24. *Situation*, p. 476. Julian Huxley pronounced Zhebrak and his colleague's work to have produced "some extremely interesting results." Julian Huxley, *Heredity East and West: Lysenko and World Science* (1969), reprint of *Soviet Genetics and World Science: Lysenko and the Meaning of Heredity* (1949), pp. 44–45.
25. N. I. Vavilov, "Genetics in the USSR," *Chronica Botanica* 5, no. 1 (1939), pp. 14–15.
26. Soyfer, *Lysenko and the Tragedy of Soviet Science*, p. 204.
27. Ethan Pollock, *Stalin and the Soviet Science Wars* (Princeton University Press, 2008), p. 66, fn81.
28. *Situation*, pp. 157–159.
29. Lysenko, *The Science of Biology Today* (1948), p. 56. Others agreed with Lysenko's characterization: elsewhere, F.K. Teterev: "None of the attempts of Darlington, Lawrence, and our collaborator, Viktorovsky, and of many others to double the number of chromosomes with the help of colchicine, acenaphthene, by rooting of leaves and the like have yielded positive results."
30. Even while saying, "But it is not at all the role which the Morganists attribute to the chromosomes," p. 22.
31. *Situation*, p. 606.
32. *Situation*, p. 31.
33. *Ibid*., p. 54.
34. Soyfer, p. 170.
35. "However, this is how Lysenko refers to the method (*Biol. Rasvitia Rastens*, 1940, p. 287)." Huxley, p. 104.
36. *Situation*, p. 162.
37. Zhdanov, "Controversial Questions of Contemporary Darwinism" lecture, quoted in Roll-Hansen, p. 170.
38. Soyfer, pp. 271, 275.
39. Cook, "Lysenko's Marxist Genetics: Science or Religion," *Journal of Heredity* 49, no. 7 (July 1949), pp. 169–202.
40. For more on Muller, see Campos, *Radium and the Secret of Life*, Chapter 5.
41. "Applications and Prospects," mss. 1916 Rice. Hermann J. Muller Papers, Lilly Library, Indiana University.
42. "ca. 1927/28, untitled lecture?" ca. 22 pages, 20–21. Hermann J. Muller Papers, Lilly Library, Indiana University.
43. *Ibid*., p. 21.
44. *Ibid*.

45. H. J. Muller, "Changing Genes, and their Effects on Evolution," Indiana University convocation, January 1947, celebration of Nobel Prize. Hermann J. Muller Papers, Lilly Library, Indiana University.
46. "Evolution Process Is Aided by X-Rays," *New York Times*, August 27, 1932.
47. What is deeply ironic here, as Mark Adams has pointed out, is that Muller had gone out of his way to attack "the Lysenkoists by triumphantly declaring that a belief in the inheritance of acquired characteristics was not only unscientific, but also played into the hands of fascism since it would mean that peoples oppressed for eons would thereby have become genetically inferior. As many of the listeners must have known, this as the exact argument that had gotten Filipchenko into trouble in 1925." Mark Adams, "Eugenics in Russia, 1900–1940," in *The Wellborn Science: Eugenics in Germany, France, Brazil, and Russia* (Oxford University Press, 1990), pp. 153–216, on 196.
48. See for example, Jan Sapp, *Evolution by Association. A History of Symbiosis* (New York: Oxford University Press, 1994); and Sapp, *Beyond the Gene* (New York: Oxford University Press, 1987).
49. Adams, "Eugenics in Russia, 1900–1940," *The Wellborn Science*, p. 179.
50. Stephen Jay Gould, "Hardening of the Synthesis," in *Dimensions of Darwinism*, ed. Marjorie Grene (1983), pp. 71–93.
51. Kaplan, *Science* 108, 48.
52. W. T. Astbury to Darlington, July 19, 1949. Cyril Dean Darlington Papers, J.3, Bodleian Library, Oxford University.
53. Muller to W. R. Singleton, Biology Dept., Brookhaven National Laboratory, April 29, 1952. Hermann J. Muller Papers, Lilly Library, Indiana University.
54. *Situation*, p. 464.

Lessons from Lysenko

John Marks

Introduction

Received wisdom tells us that we can, and should, have learnt lessons from Lysenkoism. But what can we take from this episode in order to inform our understanding of science today? In very broad terms, two strands of interpretation have emerged in the extensive work that has been undertaken on the phenomenon of Lysenkoism. The first strand focuses on the irrational despotism of the Stalinist era and on the way in which erroneous science was promoted in the Soviet Union for non-scientific reasons. The Soviet authorities favoured Lysenkoism because it corresponded with official ideology, and in turn this openness to ideological distortion was exploited by a ruthlessly ambitious *apparatchik* in the shape of Trofim Lysenko. According to this analysis, Lysenkoism stands as a test-case example of the danger of science and proper scientific values being abandoned in favour of ideology. David Joravsky, for example, argues strongly that Lysenkoism demonstrated very little serious engagement with scientific issues or even with Marxist theory. He emphasises instead the fact that Lysenko's promotion as

J. Marks (✉)
University of Nottingham, Nottingham, UK

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a "practical" agronomist corresponded to Stalin's "operative ideology" of creating a new, repressive hierarchy by elevating workers and peasants into powerful positions in order to undermine established elites.[1]

This first strand of interpretation feeds directly into the idea that Lysenkoism stands as a cautionary tale of what happens when the integrity of scientific enquiry is contaminated by extra-scientific considerations. This reading of Lysenkoism has, to a certain extent, become common currency and the invocation of neo-Lysenkoism is now an established rhetorical move on the part of those who feel that their work or position on scientific matters is being challenged or even silenced for non-scientific, ideological reasons. In the post-Cold War era, this accusation has frequently been made on the grounds that the pursuit of scientific truth and objectivity is being restricted in order to protect and bolster political consensus. In this way, Lysenkoism today often functions as a shorthand term for the perceived totalitarian drive to silence scientific findings or perspectives that are incompatible with officially sanctioned frameworks of reference. In short, Lysenkoism is equated with the dead hand of state bureaucracy and "politically correct" science policy. A typical example would be the way in which Bernard D. Davis invoked Lysenkoism in 1983 at the height of the sociobiology debate in the USA, arguing that research in this area of "behavioral genetics" was being marginalized and suppressed for ideological reasons:

> In effect, we see here Lysenkoism risen again: an effort to outlaw a field of science because it conflicts with a political dogma. To be sure, the new version is more limited in scope, and it does not use the punitive powers of a totalitarian state, as Trofim Lysenko did in the Soviet Union to suppress all of the genetics between 1935 and 1965. But that is not necessary in our system: A chilling atmosphere is quite sufficient to prevent funding agencies and graduate students from exploring a taboo area. And such Neo-Lysenkoist politicization of science, from both the left and the right, is likely to grow, as biology increasingly affects our lives—probing the secrets of our genes and our brain, reshaping our image of our origins and our nature, and adding new dimensions to our understanding of social behavior. When ideologically committed scientists try to suppress this knowledge they jeopardize a great deal, for without the ideal of objectivity science loses its strength.[2]

By extension, Lysenkoism is also associated with the closing down of scientific dialogue. The lesson, we are told, is that competing scientific perspectives should be allowed expression, and evaluated in strictly scientific terms. Along these lines, Stephen Ceci and Wendy Williams refer to the "spectre" of Lysenkoism with regard to the issue of IQ and biological

differences, a subject that has remained controversial in the wake of sociobiology debate. Although they, as they put it, hold the "acceptable" view that racial and gender differences in IQ are not innate, they argue that the silencing of alternative views undermines the healthy dialectic of challenge and proper scientific debate:

> Acts of censure edge close to Lysenkoism. They also do a disservice to science. When dissenters' positions are prevented exposure in high-impact journals and excluded from conferences, the dominant side goes unchallenged, and eventually its rationale is forgotten, forestalling the evolution of crucial ideas.[3]

Alongside this first strand of interpretation and the implicit and explicit lessons that it entails, a slightly different—although not altogether unrelated—approach attempts to analyse Lysenkoism in terms of complex relationships between science, politics, society and philosophy. The general ethos of this second strand is summarised neatly in Louis Althusser's argument that Lysenkoism cannot be reduced to "a theoretical folly involving questions of biology, a folly abetted by State intervention."[4] This second approach is more open to the idea that Lysenkoism shares some features with "normal" forms of science, and it looks at the issue of ideology in a slightly different way by focusing on the fact that Lysenko's emphasis on a practical approach to science and agriculture fitted with Bukharin's interpretation of Marxist-Leninism's view that the primary aim of a socialist regime was to change the world.[5]

The two strands of interpretation are clearly closely related, and the differences between the two are often a matter of nuance. However, in general terms the second approach seeks to show how Lysenkoism achieved a certain plausibility and legitimacy under specific historical conditions. What is more, as Roll-Hansen argues, this approach to Lysenkoism can be applied more generally:

> The rise of Lysenkoism can no longer be seen as simply the result of illegitimate political interference. Lysenkoism appears rather as an extreme form of tendencies that are inherent to any modern science that is closely bound up with practical political and economic purposes.[6]

By drawing primarily on this second strand of analysis and interpretation the aim here is to suggest that revisiting Lysenkoism as a discursive construction might help to gain some purchase on current issues

regarding biology, politics and economics. The assessment of Lysenkoism and what we might learn from it undertaken by Richard Lewontin and Richard Levins is instructive in this regard.[7] They propose a dialectical approach to Lysenkoism as an alternative to the emphasis on Stalinism and bureaucratic excesses. They formulate the principles of this dialectical approach—"lessons" as it were—as a series of analytical principles. We should remember that history "may leave an important trace", that "being and becoming are dual aspects of nature", that "conditions change", and that "qualitative effects of context and interaction may be lost when phenomena are isolated".[8] They show primarily the various ways in which Lysenkoism itself failed to absorb the lessons of a genuinely self-reflexive dialectical approach, and in doing so they aim to inspire the elaboration of a Marxist philosophy that might usefully inform scientific practice.

For the purposes of the argument here, these principles suggest ways in which an understanding of Lysenkoism might aid the analysis of contemporary science. Science and the systems of thought—the ideological and philosophical frameworks within which science occurs—are not isolated phenomena: instead, they form a dynamic dialectical whole. Furthermore, it is important to recognise that the discourse that surrounds and supports science is never judged solely on strictly scientific or philosophical grounds, but rather must provide a convincing popular narrative.

The polemical construction of an opposition between two types of biology ("proletarian" and "bourgeois"—for which read "socialist" and "capitalist") is in many ways the surface effect of a more complex set of relations between ideology and science that have continued to play themselves out in the intervening years. A number of areas will be highlighted which require problematization in this respect. First, the Cold War opposition between Lysenkoism and genetics has tended to define Lysenkoism—and by association Lamarckism—*against* genetics as a purely ideological construction. This view tends to reduce the complexity of Lysenkoism as a phenomenon and assumes a polemical opposition that does not always aid analysis. Recent years have, for example, witnessed something of a revival of Lamarckian perspectives on heredity, development and evolution: in short, "soft inheritance" is back on the agenda. Second, Lysenkoism cannot be directly transcribed as a set of manoeuvres onto contemporary science and science policy. For one thing, political and ideological influence is today more likely to be indirect. Third, Lysenkoism shows us in many ways that science can never be separated from politics. As Lewontin and Levins point out, it is necessary to distinguish between the "minimal theoretical structure" of a

particular field of science and semi-autonomous ideological superstructure. Of particular interest in this respect are the links between genetics and economic and political models. Lysenkoism portrayed Mendelian genetics as an expression of capitalist ideology and, although this claim was inflated, it is clearly the case that the development of genetics took place in the context of a broadly ideological context. Conversely, and with some degree of justification, Lysenkoism was, and continues to be, equated with the problems of a command economy and social engineering. Loren Graham, for example, sees parallels between the rejection of genetics and the Soviet refusal to accept what he regards as the "natural" economic reality of the market.[9] Crucially, contemporary biology is arguably even more closely embedded in issues of economy, politics and society. In short, Lysenkoism reminds us that science is never value-free.

In this chapter I will look at some relatively recent references to Lysenkoism in public discourse and consider, drawing on Michel Foucault, the problematic nature of polemics when dealing with issues around science. These contemporary allusions to Lysenkoism tend to replay in various forms the Cold War ideological opposition between state socialism and capitalism. It is necessary to look beyond these rather loaded allusions in order to identify ways in which those making accusations of Lysenkoism actually reproduce some of its "errors". For example, the Lysenkoist opposition between Lysenko's "down-to-earth", practical background approach and out-of-touch, overly abstract approach of established elites finds certain echoes in contemporary debates.

In the remainder of the chapter I will look at Lysenkoism as a significant episode in the rise to prominence of biology in political, economic and scientific terms, along with the ways in which contemporary biological science is increasingly framed by neoliberal ideology. I will pay particular attention to the way in which the claims made around biotechnology are associated with ideological discourses. Ironically, it seems, as will be suggested here, that the principle that life can be engineered—initially associated with Lysenkoism—has undergone a series of transformations, and has reappeared as an axiom of a new capitalist ontology. We are now in an era of *speculation*—both in economic and scientific terms—and *flexibility*, in which, in a rather different form, the manipulation of life has in many ways become both a promise and a reality. I will argue that we can learn lessons from Lysenkoism, but that they should not be limited to the polemical appeals to the history of scientific marginalization that are the most common framing of Lysenkoism today. These lessons *are* related to the

ideological inflections of science in two distinctive ways. First, it should not be forgotten that, however flawed and ideologically expedient the claims of Lysenkoism were, the dynamics of the ideological battleground between Soviet communism and Western capitalism meant that the counter claims of the molecular genetics paradigm inevitably became overly rigid and monolithic. Current challenges to this reductionist paradigm, marking something of a tentative return to Lamarckism today, feed into a new capitalist ontology of "plasticity" and life enhancement. The promises held out by contemporary genetics, biotechnology and neuroscience ironically echo Lysenkoism's promise of abundance, but now in the form of a radically transformed *neoliberal* ideological framing. My argument in this respect is influenced by the Marxist analysis of Hilary and Steven Rose in their recent *Genes, Cells and Brains.* Here, they argue that the contemporary "molecularised life sciences", as they call them, have retreated to a simple, pervasive biological narrative: "Their discourses are at once essentialist and Promethean; they see human nature as fixed, while at the same time offering to transform human life through the real and imagined power of the biotechsciences."[10]

Whilst the intention here is not to claim that Lysenko was scientifically "right" in any sense, it is important to acknowledge there was some sort of minimal scientific rationale that gave coherence and credibility to Lysenkoism, and also to show that ideology operates by constructing plausible narratives that situate science and its technological applications in relation to current common-sense assumptions relating to economics and social order. Biology is an inherently worldly field of science that proposes both an ontology of the living world and at the same time a programme for the ways in which we might manage and allocate material resources. Rather than thinking of Lysenkoism as a cautionary tale of ideological contamination, it is more fruitful to consider the precise nature of the ideological framework within which science has a social existence. The danger that such an argument runs is that of constructing a false equivalence between Lysenkoism and contemporary genetics, biotechnology and neuroscience. Obviously, such an equivalence would be untenable. Lysenkoism *was* fraudulent science in a way that contemporary biology is not. However, the *promises* held out by fields such as neuroscience are often deeply problematic in political and philosophical terms. In an entirely different context, Lysenkoism offered similar promises of biological abundance and viewed biological processes as a confirmation of a very particular ideological worldview.

Contemporary References to Lysenkoism

Contemporary accusations of Lysenkoism are certainly not uncommon, coming unsurprisingly from those who feel that their claims to scientific truth are being marginalised for non-scientific reasons. We will look here briefly at two relatively recent examples. First, Henry I. Miller, writing in the mid-1990s, accuses the Clinton administration of adopting a philosophical orientation that is holding back the development of biotechnology in the USA.[11] Just as Lysenkoism evoked the "old myth" of "Lamarckian and Communist doctrine", so policy on biotechnology is driven by the myth of a pure state of nature that should be protected from biological tampering. Lysenko's "political correctness" appealed to the Soviet authorities, he made extravagant promises, and he was ruthless is denouncing and marginalizing his enemies. Miller identifies vice-president Al Gore and Chief Domestic Policy Advisor Greg C. Simon as the "heirs-apparent" to Lysenko. Neither Gore nor Simon, according to Miller, has significant scientific training, and their positions are motivated by political and quasi-philosophical considerations. Miller is particularly dismissive of what he sees as Al Gore's New Age philosophising, accusing him of neither trusting nor respecting science.[12]

More recently, the controversy around global warming and climate change has been marked by extensive references to Lysenkoism. In a widely quoted article originally written in 2009, Cliff Ollier accuses the Intergovernmental Panel on Climate Change (IPCC) of exerting a powerful ideological influence—enhanced by the use of a highly efficient propaganda machine—in order to promote the view that global warming is caused by "anthropogenic" carbon dioxide emissions.[13] Ollier draws parallels between Stalinism and what he sees as the "ideology" of environmentalism and claims that this dominant ideology draws on an extensive propaganda machine. Furthermore, he claims that environmentalism as an ideology underpins a bureaucracy and series of vested interests that depend upon the maintenance of this "ruling concept". He takes issue with the received wisdom that the science of global warming is "settled": all new, potentially contradictory, evidence is dismissed, and any opposing voices are demonized as "Deniers" (evoking the spectre of Holocaust denial) and victimized.

These contemporary invocations of Lysenkoism clearly draw on, and repeat, the dominant framing of the Lysenko affair as a distinctive and defining episode in the history of *polemics*. This is the "two camps" model as Lewontin and Levins describe it:

> The confrontation between socialist science and bourgeois science was seen in the military metaphor as an implacable battle ending with victory or defeat. There was no sense of interaction. Enemy scientific writings consisted of the outrageous or of admissions.[14]

Michel Foucault's analysis of polemics as "a parasitic figure on discussion and an obstacle to the search for truth" provides a useful context here.[15] He identifies three components of contemporary polemics: religious, judiciary and political. In a religious sense, polemics identifies the point of weakness—an unacknowledged, illegitimate interest or dark desire—that lies behind dogma. Polemics as a quasi-judiciary practice concentrates on building a case, collecting proof of guilt and listing infractions. As far as politics is concerned, polemics seeks to build alliances and to construct a coherent and readily identifiable enemy.

All three models can be discerned in Ollier's argument. The dogma of anthropogenic global warming is underpinned by the ideology of environmentalism, which in turn supports an extensive network of vested interests, effectively controlled by the IPCC. The juridical case is built around accusations of scientific dishonesty and the victimization of opponents, and the political case rests upon the evidence that the ideological construct of global warming is itself a deeply politicized set of alliances supported by a propaganda machine and a self-justifying bureaucracy. In a general sense, as Foucault emphasizes, polemics closes down discussion, pushing opponents back into entrenched positions and preventing the possibility of genuine intellectual exchange which might facilitate the production of scientific "truth". Polemical appropriations tend to assume that, in a relatively mechanistic sense, scientific truth and method is distorted by a framing discourse that is both radically non-scientific and also impoverished and compromised by ideological thinking. In this sense polemics is, in terms of the argument set out by Lewontin and Levins, undialectical. In this way, polemics reproduces the ideological battleground of the Cold War, and in doing so runs the risk of not acknowledging the tendency of polemics to polarize debate and undermine the nuance and subtlety of argument.

This polarization remains a relatively unacknowledged consequence of Lysenkoism: the starkly drawn contrast between a "scientific" Western Mendelian neo-Darwinism and Lysenko's opportunistic, ideologically inflected Lamarckism inevitably had long-term effects on the development of biological science, exerting what now looks like a distorting effect on

Western biology. The modern synthesis of Mendelian genetics and natural selection was reinforced as a reaction to, and rejection of, Lysenko's Lamarckism, and this in turn fed into the development of a strongly determinist molecular paradigm in the post-war era. As the middle ground between Lamarckism and Mendelian neo-Darwinism was evacuated, the scientists in the West whose work was deemed to bear any traces of the latter approach found it difficult to gain credence for their work.[16] Consequently, there was undoubtedly a pressure on biologists in the West to line up in support of Mendelism. Audra Wolfe, for example, has recently shown that, despite the feeling expressed by a number of members of the Genetics Society of America (GSA) that they might stray into the realms of dogma, the Society was inevitably drawn into a broad statement of support for the relatively monolithic concept of Mendelism.[17] Fields such as embryology and epigenetics, which emphasize the role of environment, were particularly affected.[18]

Foucault's rejection of polemics stems in part from the fact that he is wary of the idea that it is possible to arrive—by means of critique—at a definitive political prescription for a given problem. Instead, Foucault reflects on his own work as a series of reflections on the way in which science, politics and ethics interact in order to form scientific domains, political structures or moral practices. In short, it is a question of identifying the underlying problematic of a given set of practices which, in the case of Lysenkoism, have political, economic, technological and ideological dimensions. If we are to learn any valuable lessons from Lysenkoism it is necessary to find an alternative to polemics, and Foucault's alternative of *problematization* is valuable in this respect.

A problematizing approach of this kind attempts to look beyond the surface effects of polemics in order to identify the way in which a particular object of discourse emerges as being worthy of attention in terms of the stakes of what is "true" and "false". Looking from this perspective at the way in which Ollier and Miller construct their arguments, it is clear that they both invoke Lysenkoism not simply as a paradigmatic example of the ideological distortion of science, but also in order to suggest that contemporary science policy has similar political motivations to Lysenkoism. The analogies they draw are frequently more than *formal*: contemporary accusations of Lysenkoism draw parallels—often implicitly—between the Soviet socialist conviction that the living world can be moulded in a technocratic fashion by state intervention and the wrong-headed interventionism of similarly socialistic contemporary political forces. Al Gore is reproached

by Miller not only for the fact that he sacrifices science to myth, but also for his self-righteous, arrogant assumption that the role of government is to *regulate* the field of biotechnology. Gore's unscientific attachment to the "childlike" myth of a natural world of purity and innocence is equated with his drive to regulate the activity of markets. In short, Miller clearly sees parallels between Gore's suspicion of biotechnology and his antipathy towards the principle of free markets. The accusation of Lysenkoism implies that Gore is not only out of touch in scientific terms, but also that he subscribes to an outmoded model of state-led intervention.

It is also important to recognize that polemics depends upon the construction of narratives that can be readily understood by a popular audience. Once again, it is worth revisiting Lysenkoism as an example of the way in which such popular discourses emerge, and Loren Graham's paper on the eugenics movement in Russia and Germany remains a useful source in this respect. As Graham emphasises, although the Soviet rejection of eugenics may now appear self-evident, it was by no means inevitable that this would be the case. Eugenic ideas had not yet forged strong links with either end of the political spectrum, and they were frequently symptomatic of a generalized post-First World War pessimism in both countries. In Russia, debate about the relation of eugenics to socialism and socialist theory was slow to develop: the concept of eugenics was relatively new and unknown, and it was initially received by some within the Soviet authorities as a field that might have potential as a scientific means of collective improvement.

Significantly, Lamarckism was seen as scientifically flawed by a good number of Marxist scientists and intellectuals. Graham identifies Vasilii Slepkov's 1925 article, "Human Heredity and Selection: On the Major Theoretical Premises of Eugenics", as one of the first comprehensive critical analyses of eugenics that identified the emphasis on biological determinism as reactionary. Slepkov explicitly drew on the Lamarckian principle of the inheritance of acquired characteristics as a scientific view that would be more in line with the Marxist idea that material social conditions determine consciousness. As the debate gathered pace and ideological fault lines were in the process of being drawn, the eugenicist Yuri Filipchenko intervened and pointed out that, according to a strictly Lamarckian model, the proletariat would be so hampered by the dysgenic effects of generations of poor living conditions that they would not be able to administer the Soviet state. Mendelian genetics, on the other hand, held out the promise of unexploited genetic potential in the proletariat that could flourish

under the right environmental conditions. Ultimately, Graham argues that Filipchenko's relatively sophisticated argument lost out to a generalized popular belief that Lamarckism and acquired characteristics opened up "limitless possibilities" for collective human improvement.[19] This analysis of the debate around eugenics and acquired characteristics leads Graham to a particularly interesting analysis of the genealogy of Lysenkoism. In response to the fact that the result of the debate amongst scientists and theorists on the issue was essentially a "draw," Lysenkoism emerged as a relatively "safe" sphere in which to work out a more definitive answer:

> The terms of the *de facto* intellectual truce were pragmatic: let the partisans of classical genetics and acquired characteristics resolve the issue in practice, in the applied science of agronomy—not in the dangerous field of human heredity where the Germans in the 1930s were winning such notoriety, but in agriculture where the Soviet Union needed help.[20]

There are contemporary parallels with the development of genetics, biotechnology and the neurosciences. These areas are not immediately linked in public consciousness with the threat, for example, of mass eugenics. Instead they appear to promise a series of eminently practical interventions that will potentially enhance the lives of individuals.

The Return of Lamarckism: Plasticity

In order, then, to learn lessons from Lysenkoism, it is necessary to understand the underlying ontological assumptions that frame contemporary biology within the ideology of neoliberalism. The answer lies in the broad concept of "plasticity", which has emerged as the opposition between Lamarckism and neo-Darwinism has begun to look much less stark and definitive. The polemical inheritance of Lysenkoism—and the fact that Lysenkoism ultimately failed as a challenge to a gene-based biology—has had the effect, until recently, of marginalizing any alternatives to neo-Darwinism. Although it clearly does not look like the basic concept of the gene will be radically superseded, the absolute rejection of Lamarckism that characterized the reaction to Lysenkoism has now been widely challenged in scientific terms, and there has been a renewed interest in evolutionary models that stand outside of the rigid neo-Darwinism that accompanied the rise of molecular biology. Both neo-Darwinists and neo-Lamarckists acknowledge the phenomenon of phenotypic plasticity, but tend to place a greater emphasis on evolutionary or ecological timescales

(Haig 2011). This "return" of Lamarckism is also linked to what looks increasingly like a significant paradigm shift in the deterministic discourse of genetics that emerges from the ideological battle with Lysenkoism.

Although phenotypic plasticity was marginalized by the rise of molecular biology, a number of commentators have pointed to a return of plasticity into a number of fields in biology in recent times, including epigenetics, embryology and evolutionary developmental biology.[21] Plasticity here refers to a series of related areas of emerging scientific consensus. "Developmental plasticity" refers to the capacity of a particular genotype to produce a variety of features (morphological, physiological and behavioural) in response to different environmental conditions.[22] In a more general sense, the idea of plasticity conveys a growing confidence in the capacity to manipulate and engineer life. In this way, plasticity has also emerged as a general principle informing our contemporary biotechnological view of life, as Hannah Landecker claims.[23] The importance of the cell was minimized by the development of genetics and molecular biology in the first half of the twentieth century, but the cell has now returned to prominence as an object that demonstrates the inherently plastic quality of living matter. Biotechnology depends upon the possibility of exploiting "a certain plasticity of organisms". Living systems can continue to carry on "living" even when they or their environment are radically altered:

> Where would biotechnology be if after being spliced or frozen or fused or extracted from its original environment, the cell or organism just up and died? In my view, the history of biotechnology from 1900 to now may be described as the increasing realization and exploration of the plasticity of living matter.[24]

What lessons can we take from this renewed interest in Lamarckism and the emergence of plasticity as a key concept? First, as Gilbert and Epel point out, we should recognize that the exaggerated and politically inflected claims that Lysenko made for his work started off within the realms of what might be recognized as legitimate scientific thought.[25] There could have been no Lysenkoism without some—as Roll-Hansen puts it—"minimal scientific plausibility". In a broader sense, the inheritance of Lysenkoism should alert us to the ideological dimensions of plasticity. The claims of biotechnology, genetics and neuroscience are often presented in what appear to be purely technical terms. However, these are not neutral: they convey a particular narrative of what life is, and of how it should be managed.

Biology and Contemporary Capitalism

Lysenkoism reminds us, then, that science and scientific theorizing have complex links with politics and, more particularly, economic models. These links are mutually reinforcing: biological models validate economic thinking and vice versa. What is more, the exchanges between the two fields are multifaceted, encompassing ideological constructions of biology as an ontology that validates forms of natural political economy and also material practices. A number of commentators have analysed the ways in which various versions of a Darwinian evolution have underpinned a political economy that favours competition, free markets and possessive individualism. Hilary and Steven Rose, for example, propose a Marxist-feminist critique of the way in which this interpretation of Darwinism—a "mutant" Darwinism as they refer to it—has emerged as a dominant cultural paradigm that promotes a very particular and value-laden model of evolution. The Modern Synthesis of the 1930s, and subsequently the combination of molecular genetic determinism and sociobiology that developed in the 1970s, have naturalized the values of both liberal and contemporary neoliberal competitive capitalism.[26]

Similarly, Donna Haraway perceives ideological analogies between the informational, cybernetic transformation of biology in the era of molecular genetics and a broadly post-Taylorist model of what she calls "investment capitalism". She points to the fact that sociobiology conceives of nature as a market in which the basic unit of investment is the gene or, to use Richard Dawkins' formulation, the "replicator". Genes employ bodies and societies as strategies for maximising "reproductive profit".

> The genes must make stable mediating devices: that is, they must produce machines *embodying* evolutionary stable categories, just as capital requires capitalist institutions. Without mechanisms for transmission and replication, the genes are like hoarded money. The market demands a technology of production consistent with its own imperatives. Here we leave the realm of competition and exchange and enter the factories of life.[27]

However, with the development of biotechnology, the economic dimensions of biology are arguably intensified and, crucially, genetics emerges for the first time as a practical, technological field. Whereas the Industrial Revolution relied on the non-biological technological uses of fire, minerals and chemistry, advanced economies have important biological dimensions: "Now, however, our economy appears to be changing rapidly, incorporating and relying upon

new organisms whose genomes have been modified through the application of human effort and ingenuity."[28] In their recent *Genes, Cells and Brains,* Hilary and Steven Rose have argued that the "Promethean" promises of contemporary biology are profoundly inflected by neoliberal ideology and the drive to break with the ideals of welfare based on solidarity that were constructed in the post-war era in Europe and North America. The fields of genomics, genetics and neuroscience, hold out new possibilities of life enhancement—life as plenitude and abundance—but only for a rich elite.

More recently, a number of commentators have explored the links between the biotech era and the distinctive dynamics of contemporary neoliberal free-market capitalism. Notable in this respect are Melinda Cooper's *Life and Surplus: Biotechnology and Capitalism in the Neoliberal Era* (2008) and Kaushik Sunder Rajan's *Biocapital: The Constitution of Postgenomic Life* (2006). Both Cooper and Rajan trace the links between the development of biotechnology and the rise of neoliberalism in the USA since the late 1970s. Cooper, in particular, analyses the various ways in which the collapse of the Fordist model and its replacement by a neoliberal framework of reference entails a significant set of shifts in the way that life and its relation to production and consumption is conceptualized. Crucially, neoliberalism has consistently questioned and undermined the implicit contract between individuals and the welfare state. The welfare state is, in this sense, an "economy of life" that seeks to protect the life of the population as a whole by redistributing wealth and providing guarantees of income and healthcare. In return, individuals are required in some sense to give their life to society. So, the welfare state seeks to maintain a distinction between the sphere of reproduction from that of production through a series of mutual obligations. In contrast, neoliberalism no longer aims to protect life from the laws of the market and the speculative dynamics of capital.

Strikingly, Cooper points to the parallels between the development of neoliberalism and the challenge to the Weismannian-Mendelian paradigm posed by biotechnology, which is inaugurated by the invention of recombinant DNA in 1973. As Cooper puts it, life as a concept is now "destandardized". The generalization from bacteria of the principle of transversal—rather than vertical transfer—of DNA corresponds to a post-Fordist model of production. Biotechnology emerges as a key feature of what Cooper labels "post-Fordist bioproduction".[29] This new regime of accumulation entails a fundamental shift in economic rationale, moving away from the drive to commodify and reproduce in standard form that underpins Fordism to a model of "promise". In line with the focus on

financialization and speculative investment that has been at the heart of neoliberal economic model, so post-Fordist bioproduction seeks to own and make profits on *potential*. Biological patents permit the ownership of an organism's "principle of generation" rather than ownership of a particular organism with a necessarily limited lifespan:

> In the age of postmechanical reproduction the point is no longer to reproduce the standardized Ford-T model in nature, but to generate and capture production itself, in all its emergent possibilities. Its success is dependent on the constant transformation of (re)production, the rapid emergence and obsolescence of new life forms, the novel recombination of DNA rather than the mass monoculture of standardized germplasm.[30]

Cooper points to a general association that has been established in the contemporary neoliberal era between the idea of free markets as self-organizing entities and scientific thinking on complexity theory. We see the emergence of, as she puts it, a kind of "free-market vitalism" from the 1990s onwards.[31] She draws directly on Marx's thinking on the way in which capitalism conceives of limits and growth in terms of both crises and surplus. This Marxian reading of contemporary bioeconomics highlights the importance of capitalism's periodic flight into speculative "delirium". Marx highlights the disjunction between the "plenitude" of the future that is promised in this speculative mode and the construction of the present as a moment of depletion and devastation. This contrast between the promise of the future and an impoverished present is particularly stark in the case of contemporary neoliberal speculation.

The neoliberal response to what was widely perceived as a burgeoning crisis of resources and pollution—initially expressed in the Club of Rome's world futures report in 1972—was to call for a paradigm shift away from an economic model of Fordist mass production to a post-Fordist innovation-based economy. Neoliberalism responds to falling production by blaming the welfare state and the redistribution of wealth and life chances. The state can no longer provide collective insurance against risk, which now becomes individualized.[32] As Rajan sees it, there is an implosion of the economic logic of neoliberalism and the epistemology of what he terms "postgenomic" life: "[T]he very grammars of the life sciences and of capital are co-constituted; life becomes a business plan."[33] Rajan identifies the particular importance of the concept of "innovation". It is no longer a question of generating resources from what already exists, but rather of generating and creating new resources.[34] In this way, the economy of biotechnology depends upon

a distinctively post-Fordist conception of "debt". The rise of the USA as a debtor-imperialist force is an expression of capitalist "delirium". This delirium is caught up in a dynamic of ever-more extravagant promises based on a dream of regeneration and the creation of new life.

What we see here is the emergence of a distinctive ideological framing of genetics and biotechnology. Keeping in mind the dialectical principles suggested by Lewontin and Levins, we can see that the opposition between genetic determinism and a voluntaristic approach has been transformed over time. Whilst the principle of a genetic inheritance and the importance of genetic determinism remain, the practical development of biotechnologies has opened new possibilities as far as the manipulation of life is concerned.

Brain Plasticity: A New Political Battleground

Evidence of the fact that the concept of plasticity has an ideological dimension can be seen in current debates on brain plasticity, otherwise known as neuroplasticity. This refers to the capacity of the brain to modify both structure and function in response to environmental factors. It seems that the brain can make new cells (neurons), construct new synaptic connections between these cells, and modify established neuronal connections.[35] At the same time, there has been a growing recognition of the plasticity of human brain functions. These "plastic" dynamics are associated with environmental factors such as stress levels and injury, and more positively with learning new skills throughout life.[36]

Brain plasticity can be interpreted in very different ways, and these interpretations tell us a good deal about the way in which our existence as biological beings is now, more than ever, deeply political. The lesson to be applied from Lysenkoism is not that these interpretations can in any way be defined as right or wrong in scientific terms, but rather that they are loaded with ideological significance. Also, along similar lines to Graham's analysis of Lysenkoism and eugenics referred to earlier, neuroscience appears to offer a relatively "safe" way of working through the social and political significance of neuroplasticity. As Pitts-Taylor observes, a number of commentators have interpreted this phenomenon as a challenge to the conservative tendencies of biological reductionism and determinism: plasticity in this sense appears to offer a route for intellectuals out of this biological determinism.[37]

Along these lines, French philosopher Catherine Malabou has focused on what she sees as the philosophical possibilities for freedom and self-construction that arise from recognition that the brain can be "rewired" by shocks.[38] She revisits the classic philosophical question of the relation-

ship between mind and body in order to construct a progressive politics of self-transformation at an individual and societal level. Neuroscience has, she suggests, converged with the anti-Cartesian philosophical assertion that the brain is not a central command centre that has sovereignty over the body. Instead, the brain is a decentred system that cannot be separated from the body or the environment. This does not mean, however, that we should think of consciousness as a purely mechanical product of this radically materialist view of mind and body. The brain is capable of "self-reformation":

> Is this not the best possible definition of plasticity: the relation that an individual entertains with what, on the one hand, attaches him originally to himself, to his proper form, and with what, on the other hand, allows him to launch himself into the void of all identity, to abandon all rigid and fixed determination.[39]

On the other hand, neuroplasticity also opens up the possibility of the construction of a "flexible" self-policing subject that corresponds to the demands of neoliberal ideology. Along these lines, Malabou points to ways in which brain plasticity is seen as being in tune with the broad post-Fordist shift to a *network* model of capitalism. As relatively rigid, top-down modes of organization have been replaced with more mobile, decentred networks, so the capacity to be flexible and constantly available to new connections fosters a new, paradoxically hyperactive form of docility. Similarly, Pitts-Taylor points to the fact that political rationality of neoliberalism seeks to promote individual responsibility for health over state provision. In this sense, plasticity encapsulates the paradoxical neoliberal injunction to deploy subjective freedom in order to make oneself a flexible, pliable and disciplined subject. Individuals are expected to improve and optimize their own health, and to take reasonable preventive measures in the face of disease and ageing. They, in short, are required to invest their biological capital wisely, and to a certain extent to improve their biological inheritance. Significantly, Pitts-Taylor finds in her literature survey that the plastic brain is widely referred to as a relatively underexploited *resource*, and she refers here directly to Rajan's analysis of biocapital in terms of hype and speculation around potential.

Conclusion

What, then, are the lessons that we can take from Lysenkoism? First, contemporary invocations of Lysenkoism are often narrowly polemical and also tend to replay the ideological oppositions of the past. The sugges-

tion here has been that Lysenkoism provides a useful starting point for a consideration of the complex and heterogeneous circumstances in which biological science establishes itself and operates as a discursive field which is increasingly embedded in politics. Lysenkoism also reminds us that science has, at any given time, a practical, operational dimension, and the Soviet authorities placed great emphasis on the practical orientation of Lysenko as an agronomist. Further to this, the ideological dimensions of science and science policy have what we might broadly term philosophical and populist components. In short, rather than rehearsing the polemical to-and-fro of Lysenkoism, it is more useful to look at the way in which the episode functions as a pivotal moment in the development of these ways of conceptualizing biology and its relation to ideas about life and political economy.

As far as the philosophical framing of biology is concerned, Lysenkoism marks the point where a version of Darwinism, in the shape of the Modern Synthesis, wins out over the Soviet synthesis of dialectical materialism and Lamarckism. These are more than theories of biology, in that they make claims to general epistemological and ontological significance. This neo-Darwinism is bolstered by the development of molecular biology, which is framed within a rigidly reductionist paradigm, and the subsequent emergence of social biology and evolutionary psychology. However, as we have seen, there is a further ideological supplement to this commitment to a biologically determined and relatively fixed human "nature". The technological success of biotechnology and the potential suggested by genetics and neuroscience give rise to an emerging paradigm of plasticity which associates the commercial exploitation of life with the pervasive ideology of neoliberal economics. So, although philosophically the revival of some aspects of Lamarckism has challenged neo-Darwinism, at the popular level the idea of plasticity appears to resonate with dominant contemporary ideas in political economy. Ironically, it might be claimed that it is Lysenkoism that anticipates the biotechnological capacity to engineer life. Lysenkoism in this sense was an early skirmish in a struggle to define the productive possibilities of life. With the collapse of state socialism at the end of the twentieth century, these productive possibilities are now developed and promoted within the context of advanced capitalism.

Notes

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3. Stephen Ceci and Wendy M. Williams, "Should Scientists Study Race and IQ?" *Nature* 457 (February 12, 2009), p. 788.
4. Louis Althusser, "A Critical Viewpoint on Rectification of Errors: Lysenko: Unfinished History," trans. Grahame Lock, *Marxism Today* (February 1977), p. 56.
5. See Roll-Hansen, *The Lysenko Effect*, pp. 143–144.
6. *Ibid*., p. 17.
7. Richard Lewontin and Richard Levins, "The Problem of Lysenkoism," in *The Radicalisation of Science*, eds. Hilary Rose and Steven Rose (London: Macmillan, 1976), pp. 32–64.
8. *Ibid*., p. 60.
9. Loren R. Graham, *What Have We Learned About Science and Technology from the Russian Experience?* (Stanford, CA: Stanford University Press, 1998), p. 31.
10. Hilary Rose and Steven Rose, *Genes, Cells and Brains: The Promothean Promises of the New Biology* (London: Verso, 2013).
11. See Henry I. Miller, "USA Biotechnology Policy: The Ghost of Lysenko?," *Current Opinion in Biotechnology* 6 (1995), pp. 255–260; Henry I. Miller, "When Politics Drives Science: Lysenko, Gore and U.S. Biotechnology Policy," *Social Philosophy and Policy* 13, issue 2 (1996), pp. 96–112.
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13. Ollier, "Lysenkoism and 'Global Warming'," *Energy and Environment* 20, nos. 1&2 (2009), pp. 197–200.
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15. Michel Foucault, *Ethics: Subjectivity and Truth: Essential Works of Michel Foucault 1954–1984, trans. Robert Hurley and others* (London: Penguin, 2000), p. 112.
16. Roll-Hansen, "Lamarckism and Lysenkoism Revisited," in *Transformations of Lamarckism: From Subtle Fluids to Molecular Biology*, eds. Snait B. Gissis and Eva Jablonka (Cambridge, MA: MIT Press, 2011), p. 79.
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19. Loren R. Graham, "Science and Values: The Eugenics Movement in Germany and Russia in the 1920s," *The American Historical Review* 82, no. 5 (December 1977), p. 1153.
20. *Ibid*., p. 1157.
21. See Scott F. Gilbert and David Epel, *Ecological Developmental Biology: Integrating Epigenetics, Medicine and Evolution* (Sunderland, MA: Sinauer

Associates, Inc., 2009); Mary Jane West-Eberhard, *Developmental Plasticity and Evolution* (Oxford University Press, 2003).

22. See Eva Jablonka, "Cellular Epigentic Inheritance in the Twenty-First Century," in *Transformations of Lamarckism: From Subtle Fluids to Molecular Biology*, eds. Snait B. Gissis and Eva Jablonka (Cambridge, MA: MIT Press, 2011), p. 145; Sonia E. Sultan, "Evolutionary Implications of Individual Plasticity," in *Transformations of Lamarckism: From Subtle Fluids to Molecular Biology*, eds. Snait B. Gissis and Eva Jablonka (Cambridge, MA: MIT Press, 2011), p. 193.
23. See Hannah Landecker, *Culturing Life: How Cells Became Technologies* (Harvard University Press, 2007).
24. *Ibid*., p. 232.
25. Gilbert and Epel, *Ecological Developmental Biology*, p. 423.
26. See Hilary Rose and Steven Rose, "Darwin and After," *New Left Review*, no. 63 (May/June 2010), pp. 91–113.
27. Donna Haraway, *Simians, Cyborgs and Women: The Reinvention of Nature* (New York: Routledge, 1991), p. 62.
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29. Melinda Cooper, *Life and Surplus: Biotechnology and Capitalism in the Neoliberal Era* (Seattle: University of Washington Press, 2008), p. 23.
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31. *Ibid*., p. 18.
32. *Ibid*., p. 62.
33. Kaushik Sunder Rajan, *Biocapital: The Constitution of Postgenomic Life* (Durham and London: Duke University Press, 2006), p. 283.
34. *Ibid*., pp. 113–114.
35. See Bryan Kolb and Ian Q. Wishaw, "Brain Plastcity and Behavior," *Annual Review of Psychology* 49 (1998), pp. 43–64.
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39. *Ibid*., p. 80.

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Current Attempts at Exonerating 'Lysenkoism' and Their Causes

Eduard I. Kolchinsky

The phenomenon typically referred to as "Lysenkoism" has long been a feature of Soviet scientific historiography. Less examined is the problem of *Lysenkovshchina*, which, as I will show, is far more important in that it is the strategy by which the former is being revived today.[1] I define Lysenkoism as a set of concepts and theories proposed by a scientist named Lysenko, as opposed to *Lysenkovshchina*, which was a social practice consisting of scientists competing for influence in the party-state administration. This distinction is essential to understanding the contemporary treatment of Lysenko's work in Russia, and attempts to restore his reputation.

From the late 1940s, US observers and historians considered Lysenko's activities within the general context of party-state policy.[2] A Russian historiography on Lysenkoism first appeared thanks to Zhores Medvedev, whose work intersected with Joravsky and Graham's. Abba Gaissinovich followed up by, like Medvedev, also describing events from the perspective of those who participated directly in the biological debates beginning during the 1930s, and lasting to the mid-1960s.[3]

E.I. Kolchinsky (✉)
St. Petersburg Branch of the Institute for the History of Science and Technology, Russian Academy of Sciences, St. Petersburg, Russia

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Despite their differing approaches, both US and Russian historians established a trend of portraying events in black-and-white, giving praise and passing judgment on those they regarded as protagonists and antagonists. In this framework the academic community of Soviet biologists, as a rule, were depicted as victims of Lysenkoism. Vladimir Dudintsev's best-seller *White Robes* (1988) was very important in establishing the image of Lysenko's opponents as disinterested truth seekers. Simon Snoll's book *Heroes and Villains of Russian Science* also corresponded to this tradition.[4]

In the 1990s, the opening of previously classified archival collections created new opportunities for research on Lysenkoism, and the tragic fate of Soviet genetics.[5] These studies contributed to a better understanding of the events that have been interpreted as a struggle within the academic community for maintaining international research standards.[6] At the same time, participants in those events have been often portrayed as people who competed for funding, the attention of party-state authorities, domination of their own research schools, as well as leading positions in social networks, that is, those interested in building "scientific empires."[7] However, historians have put too much emphasis on social and political factors, while neglecting the role of scientific practice and the content of research.

Recent academic debates that take place outside Russia focus, as a rule, on the Cold War context of Lysenkoism as a global phenomenon: they analyze socio-political and scientific factors that caused the proliferation of Lysenkoism in the post-war decades, not only in the countries of the Soviet Bloc, but in other national contexts as well, particularly Italy, France and Japan.[8] This research was central at a few recent symposiums on Lysenkoism in New York (2009), Vienna (2012) and Tokyo (2012). Some of the materials presented at these events have been published as special issues of *The Journal for the History of Biology* and *Studies in the History of Biology*.[9]

In the Russian Federation, however, the situation is quite different. Here we observe a wave of publications that claim to provide a new perspective on the opposition between Lysenkoists and geneticists. Some of these works examine the history of Lysenkoism worldwide.[10] Others focus on the conflict between N.I. Vavilov and T.D. Lysenko. Attempts to overcome the narrative of "heroes and villains of Russian science," and analyze the conflict between Vavilov and Lysenko within a broader socio-political context of the Stalinist period, led to an increase in publications that position themselves as a "pragmatic" interpretation of the conflict.[11] As their authors suggest, it was "Vavilov's fault" that he supported a "young

agronomist" Lysenko and ensured his rapid career advancement, while their subsequent confrontation was nothing but a trivial disagreement between two rival scientific schools, competing for funding preferences in the Stalinist environment.

Dozens of publications produced by established scholars also glorify Lysenko's "achievements" in applied biology and blame Vavilov for his alleged failure to focus on "real" problems of agriculture. Too frequently Vavilov is accused of wasting state funding on useless expeditions and international exchanges. His critics also emphasize Vavilov's supposed lack of patriotism. Sometimes these accusations go so far as to suggest that geneticists were "lucky" to become victims of Stalinist repressions. Their victimhood allegedly earned them fame, while Lysenko—as the argument goes—suffered for his patriotism. His struggle against eugenics, achievements in plant selection and animal breeding and alleged opposition to Khruchshev's risky experiments with agriculture are all presented as unacknowledged virtues.

My objectives here are threefold. First, I will consider some examples of recent attempts to exonerate Lysenko in Russia. Second, I will identify the main arguments advanced by Lysenko's apologists. Third, I will clarify the social, political and intellectual context of this process. As mentioned above, in this chapter I define Lysenkoism as a set of various quasi-scientific concepts that were a contradictory mixture of agronomic techniques and elements, borrowed from different evolutionary and genetic theories. *Lysenkovshchina*, on the other hand, is a social practice of fighting against competing research teams by appealing to the party-state administration. This practice takes root in an environment where research funding is distributed without a thorough academic peer review of proposed projects, or with no consideration of international trends in science. *Lysenkovshchina*, as an extreme form of "ideologically correct science," leads to a situation where the state eagerly supports unrealistic research projects that contradict the basic laws of science.

Campaign to Exonerate T.D. Lysenko

Since the mid-1990s, some Russian and foreign geneticists have interpreted recent discoveries in molecular biology that are related to research on cytoplasmic and epigenetic inheritance and prions, as proof that acquired traits can be inherited—a Lamarckian idea that was central to Lysenko's views.[12] For example, M.D. Golubovsky, a geneticist and historian of science who in the last few decades has been living in the USA, wrote in

Priroda, a respected Russian popular science journal thus: "Discoveries in the field of mobile genetics demonstrate that a cell, as a holistic system, can adaptively rebuild its genome in the process of selection. Instead of passively waiting for an accidental mutation that would ensure its survival, the cell is capable of responding to an environmental challenge by an active genetic search." Concluding his account of non-standard types of inheritance, Golubovsky assures his readers: "unwittingly, many basic principles of classical genetics that were rejected by Lysenko achieved in this way the status of an almost undisputed truth. When a serious scholar found something that apparently confirmed Lysenko's views, he was afraid to make his discovery public, because he was afraid of being ostracized by the academic community. Even if his work was published, it was done with many reservations and occupied a marginal position."[13]

In fact, current debates in molecular genetics and embryology have nothing to do with Lysenko's speculations about "shattering heredity," "modifying organisms by mentoring," or his denial of DNA as the basis of inheritance. Nevertheless, a Czech biologist, Jaroslav Fregr, tried to find some rational ideas in Lysenko's work.[14] He agrees that Lysenkoism was just a set of absurd theories based on anecdotal observations or poorly conducted experiments without the corresponding control plots and statistical evaluation of the results. However, in Fregr's opinion, early in his career Lysenko received some facts that "might inspire modern biologists to construct testable hypotheses and suggest experiments that could extend our scientific knowledge."[15]

Fregr attempted to explain some of these phenomena (vegetative hybridization, wobbled heritability, heritability of environmentally induced adaptive modifications and effects of intra-varietal hybridization of self-fertilizing cultivars) in terms of modern biology. Meanwhile, a Chinese biologist, Yongsheng Liu, also tried "to distinguish between Lysenko's rightness and wrongness, and make a concrete analysis of his contributions to biology and his tragedies, thus reconsidering him from a comprehensive and objective viewpoint."[16] In Liu's opinion, Lysenko made important contributions to plant physiology, genetics, evolutionary theory and agrobiology. Essentially, he found just one fault with Lysenko: "Sometimes Lysenko forgot to mention the names of his predecessors in his publications."[17]

These statements provide a kind of a "scientific foundation" for a large-scale campaign to exonerate T.D. Lysenko in Russia. This campaign acquired a nationalistic flavor from the start, coinciding with the rise to power of a new ruling elite led by Vladimir Putin. Political and ideological arguments were in the foreground of these attempts. In the words of

geneticist Iuriĭ Ivanov, "so-called *Lysenkovshchina* has never existed.[18] There was anti-Lysenkovshchina, as a stage in *perestroika*, which destroyed the Soviet Union." Historian and journalist Iurii Mukhin also claims to expose "myths about the great scientist Vavilov and the adventurist Lysenko," while economist Iurii Bobylev assures his readers that the main objective of genetics, and the human genome project in particular, was creating genetic weapons against the white race, which makes accordingly 90 % of the Russian population.[19]

These publications are part of a well-coordinated campaign, as evidenced by the fact that in 2006, several publishing houses simultaneously released books that glorified Lysenko as a great scientist and patriot who had been slandered by the enemies of Russia.[20] These books are written by people of different professions and with different life experience. Some of these works are pulp literature; however, some have scientific pretensions.

Iu. Mukhin, the author of *Genetics, a Harlot: Cognition of the World or a Sinecure (kormushka)* is known as a die-hard Stalinist and staunch opponent of liberalism.[21] Up to the mid-1990s he was the head of a ferro-alloy plant in Kazakhstan where he published dozens of papers in technical journals and patented a few inventions in metallurgy. Later he became a public activist and publisher, funding communist newspapers such as *Zavtra* (Tomorrow) and *Den* (Day), while also publishing *Duel*—a nationalist, anti-Semitic newspaper. He is publisher of the popular book series, *Russkaia Pravda* (Russian truth), and also produced films that "unmask" anyone who questions Stalin's and Beria's administrative talents, and attacks the Russian intelligentsia for its alleged lack of professionalism and disengagement from practice.

In *Genetics, a Harlot*, Mukhin claims to be concerned with genetics; however, he gives it only passing treatment in the first section. At first glance, the book almost seems to be a joke. Yet Mukhin is quite serious when he tells his readers about prosperous Stalinist collective farms, or Lysenko's alleged opposition to Khrushchev's virgin land campaign and the promotion of corn cultivation.[22] He denies the famines of 1932–33 and 1947, which caused millions of deaths in various regions of Russia. Yet he is most eloquent about Jews and Americans who "skillfully manipulated" Soviet geneticists, facilitating the collapse of the Soviet Union. Mukhin citations are the protocols of custodial questioning of Vavilov, as well as Vavilov's involuntary confessions. Mukhin claims that the "subversive activities" (*sic*) of Vavilov and his colleagues were conclusively proven in the course of Vavilov's trial, and interprets the fact that the accused pleaded guilty as evidence of their "crimes."[23]

Mukin's book reads like something from the 1940s in that the narrative echoes the style of that period. This feature is particularly attractive to millions of Russian citizens who feel nostalgic for the Soviet past. The conclusion of the book is predictable: Vavilov was not a victim of the Stalinist regime—he was justly punished for betraying his fatherland and his scientific reputation is vastly exaggerated. Lysenko, on the other hand, was a brilliant scientist with an impeccable class background. In 2008, the Savelovskii district court in Moscow found Mukhin guilty of making public statements calling for extremist activities and nationalist propaganda, and prohibited him from occupying editorial positions. Nevertheless, he is now publishing a newspaper *K barieru* ("To the Barricades"), in which he continues to spread hatred toward scientists in general, and geneticists in particular.

Iurii Bobylov, who authored the book, *Genetic bomb: Secret scenarios of bioterrorism*, is a recognized expert on war and terrorism. He also relies on concepts advanced by the lieutenant-general Vorob'ev, who had previously worked for a secret "Biopreparat" (Biochemical) research institute and the KGB (Komitet Gosudarstvennoi Bezopasnosti -The Committee of State Security), and thus is presumably well informed about the use of genetics in the USSR for the production of biological weapons. In the first, 2006, edition, Bobylov himself characterized his book as "intellectual pornography" that was meant to scare, mystify, excite and seduce its readers with tales of dangerous intrigues masterminded by "biologists, the military and secret services of a few developed countries of the world, including Russia."[24]

Two years later Bobylov put more emphasis on the "biological aspects" by using racist interpretations of human biological diversity. He claimed that the main objective of genetics in general, and the human genome project in particular, is the development of genetic weapons aimed at the white race. For him, genetic research is part of a "rising global evil" and "diabolization" of contemporary life. Bobylov predicts "genocidal wars" among different races will become more common, and therefore calls for developing ethnic and racial weapons.[25] The author claims that cheap American chicken legs, which flooded Russian supermarkets in the 1990s, were in fact a deadly "genetic bomb," on par with the development of transgenic biotechnologies. In order to ensure Russia's "bio-survival," the author calls for strengthening "defensive" secret services and reforming academic research. It is worth noting that his book was published by Belye Al'fy, a publishing house allegedly controlled by the military, police and secret service, which specializes in the production of racist and nationalist literature.

A trained geneticist, Vladimir Pyzhenkov, authored the book, *Vavilov, a botanist, academician, and a citizen of the world*, which was published by Samoobrazovanie publishing house in the "Stalin's epoch" book series.[26] Pyzhenkov spent his academic career at the All-Union Institute of Plant Breeding (VIR), created by N.I. Vavilov, in the years after Lysenko's hegemony, when genetics had been restored as a legitimate field of research. He completed his doctoral studies in 1969, and for a few years chaired its research unit for the study of vegetables. Since 1992, Pyzhenkov has been working in the St. Petersburg State Agricultural University, where he chairs the department of genetics, selection and seed breeding. Under the aegis of the leading agricultural school of Russia, Pyzhenkov published a series of books and pamphlets in which he assures his readers that Vavilov produced no original research, made no contribution to solving the problems of agriculture, but simply traveled a lot, wasting money and making empty promises.[27] Puzhenkov's arguments are almost a verbatim restatement of accusations made against N.I. Vavilov in the late 1920s and 1930s by A.V. Al'benskii, A.K. Kol', I.I. Prezent, G.I.Shlykov and others who would turn into ardent Lysenkoists.[28]

Since he is denying Vavilov's contribution to science, Pyzhenkov is desperate to attribute Vavilov's law of homologous series in hereditary variation, and his concept of the centers of origin of cultivated plants, to a number of predecessors—both real and imagined—including I.W. Goethe, A. Humboldt and A. de Candolle. Pyzhenkov does not distinguish between a casual remark made by Darwin on parallel variability, and a well-articulated concept of homologous series proposed by Vavilov, and confirmed by current research on genomes of various organisms.[29] Pyzhenkov's claim about Vavilov's collection of seeds of cultivated plants being useless for practical purposes is not supported by the Food and Agricultural Organization (FAO)—an international agency, which ranked it as the fourth most important collection on a global scale in terms of its role for mobilizing genetic resources of plants.[30] By comparing Vavilov's expeditions with the work of Foreign Plant Introduction Section at the US Department of Agriculture, Pyzhenkov portrays them as amateurish enterprises, which resulted in a random collection of materials.

Unlike Mukhin, Pyzhenkov positions his publications as academic research in the history of science. Yet he does not seek out new archival materials. Instead he is content with interpreting certain isolated statements made by D.N. Borodin and N.I. Vavilov in their correspondence, Vavilov's secret police investigation file, the Council of People's

Commissars' resolution that authorized the establishment of VIR and so on. The lack of standard references to archival and published documents prevents serious scholars from verifying Pyzhenkov's interpretation. This lack of evidence is perhaps explained by the fact that agricultural scientist Piotr F. Kononkov, the editor of Pyzhenkov's book, received his doctorate from the Institute of Genetics of the USSR Academy of Sciences at a time when Lysenko was the head of the institute.

Though Kononkov is an "honored worker of science of the Russian Federation," laureate of the Russian State Prize (2003), and laureate of the Russian Government Prize (2013), he is also particularly active in glorifying Lysenko.[31] He edited a multi-authored book *Trofim Denisovich Lysenko: a Soviet agronomist, plant breeder, biologist*, which portrayed Lysenko as a great scholar and patriot.[32] Among the contributors are a doctor of agricultural sciences and plant breeder, I.V. Dragina, who taught Michinurin-style genetics at Moscow State University in the 1960s, as well as N.V. Ovchinnikov and A. Chichkin, whose academic backgrounds I was unable to trace. The volume seeks to demonstrate that Lysenko's group of researchers pursued "the line set by Stalin's leadership of the USSR on the question of accelerating the application of agricultural science to practice, which was a critical issue on the national agenda in the 1930s to 1940s, the struggle with eugenics in the 1930s and launching the patriotic movement in the USSR in the late 1940s to early 1950s."[33] Kononkov published a booklet, *Lysenko's contribution to the victory in the Great Patriotic War*, which argues that Lysenko's "innovations" were meant to solve the food problem in the periods of famine when the country lacked adequate academic, material, financial and human resources.[34]

That same year, Ovchinnikov published a book, *Academician Trofim Denisovich Lysenko: Michurinist biology*. It was also edited by P.F. Kononkov, and reviewed by V.P. Petrov—a doctor of agricultural sciences, professor of the department of genetics, plant breeding and plant protection at the School of Agriculture of the Russian University of Peoples' Friendship and a member of the editorial board of the journal *Rastenievodstvo i zhivotnovodstvo* (Plant and animal breeding). In the section, "Scientific and practical problems," Ovchinnikov lists Lysenko's major "achievements." Among them he names vernalization of grain crops, vegetative hybridization, nest method of forest planting and breeding high-fat dairy cows. As a supplement, the book contains memoirs left by M.V. Alekseeva and I.V. Dragina about "persecution" they were subjected to by geneticists after 1964, and a translated article by Liu Yongsheng. Characteristically, the author makes

numerous references to works by authors such as outstanding biologist J.B.S. Haldane, modern historians of biology Uwe Hossfeld and Nils Roll-Hansen and cites them as though they confirm Lysenko was right. Haldane's judgments about Lysenko and the practical value of his ideas were, in fact, controversial.[35] However, Haldane estimated the possibility for the direction of change in multicellular organisms as unlikely and rejected the idea of inheritance of acquired characteristics: "I will never accept Lysenko's views on genetics."[36] Hossfeld and Roll-Hansen deny as well any practical significance to Lysenko's work.[37] The paper also refers to the arguments made by staunch advocates of Lysenko, V.N. Remeslo and D.A. Dolgushin, about Michurinist biology being allegedly efficient for breeding high-yield winter wheat varieties, although these arguments have not been substantiated by data on genetic diversity of original spring varieties, sowing time, statistics on their survival rate and so on.

As for awards and honorary titles bestowed on Lysenko by the Soviet government, the context in which they occurred, that is, the Stalin era, is ignored. Some of Lysenko's opponents awarded then were subjected to repression afterward. Moreover, Ovchinnikov borrowed all of his information about Lysenko's achievements from the news media, circa 1932 to the early 1960s. This information was repeatedly refuted later on. At that time, everything published in the press was propaganda and, as such, glorified the achievements of Soviet science with Lysenko as its best representative. Even Darwin's jubilees were essentially carried out with the same purpose. For example, in 1937 at the 60th anniversary of Darwin's death, the eminent soil scientist and agronomist Vasiliĭ Vil'iams published an article devoted not to Darwin, but to Lysenko as Darwin's successor. The author stated: "the Doctrine of Lysenko wins, because it's correct, dialectical, historical and evolutionary."[38]

The section on the socio-political context of the conflict between geneticists and Lysenko is based on research by Krementsov, but supplemented with a new thesis about Stalin who—as the argument goes—never pretended to have any scientific opinions. Apparently Stalin simply punished geneticists for their commitment to Trotskyism and propagandizing the eugenic ideas of Nazis and American imperialists.[39] In fact, neither Vavilov, nor his colleagues who died with him, ever took part in the intra-party debates or supported Trotsky. The primary ideologist of Michurinist biology, I.I. Prezent, and his wife, were Trotskyites at a point in their careers, and were even arrested during the Great Terror of 1936–1938. This fact, however, did not prevent them from occupying leading positions at the

departments of genetics and Darwinism at Leningrad State University in the early 1940s. Ovchinnikov also refers to Vavilov and Timofeev-Ressovsky's pre-trial investigation and court trial documents that contain their guilty pleas, without noting that these "confessions" were the result of psychological pressure and torture.[40]

In order to prove his thesis that Lysenko was persecuted, Ovchinnikov refers to certain documents from the archive of the Academy of Sciences. These are mainly Lysenko's letters to the Presidium of the USSR Academy of Sciences, the Soviet Government and the Central Committee of Communist Party (1973–1975).[41] However these documents testify only to the fact that Lysenko complained to authorities about criticism he faced till the end of his life. He listed many grievances against "some people who are malicious enemies not only of our progressive science of biology but also the enemies of the Soviet regime," and who were subjecting him to "persecution, defamation and slander."[42] Using political denunciations as his sole argument, Lysenko continually portrayed his critics as foreign agents.

Ovchinnikov never denies that "the Michurinist trend in biology was a kind of deviation from the main line of the disciplinary development," which was achievable "only within the framework of the Stalinist regime and only with the direct support of Stalin."[43] Paradoxically, he considers Lysenko's rejection of modern biology, especially molecular, as his main merit. Ovchinnikov, with great pleasure, quotes Lysenko's letter addressed to N.P. Dubinin, written on September 25, 1974, to demonstrate that Lysenko's stance on molecular biology remained unchanged: "once again I declare that we have never used and are not going to use any ideas and methods of molecular biology. I would like to advise all biologists, plant and animal breeders and students in the Soviet Union against adopting these methods, as they only hinder our understanding of the essential, that is advancement of theoretical biology."[44] This statement would cause no objections if it were not for the fact that this deviation from the main line of the disciplinary development resulted in a great number of pseudobiologists with academic degrees, titles, awards and prizes, many of whom still prosper and reproduce their research schools by mentoring the next generation of scientists and persecuting their opponents.

Piotr Kononkov recently published a book of his own, titled *Two Worlds, Two Ideologies*.[45] This book, sponsored by the Federal Agency of Press and Mass Communications, is plainly Stalinist and Lysenkoist. His Lysenko is a great scholar and patriotic humanist, while geneticists are depicted as pseudoscientists and charlatans, performing tasks assigned to them by

globalist structures, hostile to Russia. Opponents of Lysenko are called "traitors of the nation" (*national-predateli*). This is also the label Putin used for critics of his policy in Ukraine. The editor of this book, German V. Smirnov, educated as an engineer, is known for his anti-Zionist positions. As Smirmov wrote, Zionism was the main player against Lysenko not only in Russia, but all over the world.

In the last few years, an author who uses the pen name Sigizmund Mironin has produced a number of books. His works, published in the series, "The Mystery of 1937," vilify genetics and glorify Lysenko. The titles of these books—*Stalin's Order, Famine in Russia, The Geneticists' Affair, Stalin's Murder: The Greatest Conspiracy of the 20th Century, Pseudo-science—genetics. Plague XX century*—basically reflect their contents.[46] In terms of their objectives, style and argument, these books are similar to Mukhin's works, even though Mironin positions himself as a professional biologist (he sometimes states he has a doctoral degree in medicine). According to the author, the main objectives of his book on the history of genetics, as well as his lengthy internet publication, *Why Stalin Defended Lysenko*, are exposing the "greatest mystification of the 20th century"—that is, the idea that genetics was a serious science, while Lysenko was an ignorant charlatan.[47] The author assures his readers that an unbiased analysis of the confrontation between geneticists and Lysenkoists, and a comparison of their concepts with the current state of research in molecular genetics, would demonstrate an almost complete compatibility of Lysenko's ideas with recent work on these problems. Or to use Mironin's own words, "Lysenko was closer to the truth than formal geneticists of his days."[48]

Mironin's work on the history of genetics contains a long list of cited literature (354 entries, of which 197 are in English), yet most of these publications have nothing to do with genetics. The book is full of verbose arguments about the golden age of science under Stalin, the Trotskyites who attacked the honest scientist Lysenko and denounced him to the Central Committee of the Communist Party of the Soviet Union (CPSU), the struggle against cosmopolitanism, and the so-called Leningrad affair.[49] The arguments advanced by the author are simple: Everything that has been said about Lysenko is a lie because Lysenko supported Stalin, who, as everyone knows, never made mistakes. Meanwhile, the author exaggerates the importance of the fact that in the aftermath of World War II geneticists were the first to launch an attack against Lysenko, and the latter had to defend himself by appealing to Stalin. It is perhaps not irrelevant that Mironin's publisher, Algorithm, is notorious: in 2013 it issued a Russian translation of Joseph Goebbels' novel, *Michael*.

Russian-language websites are full of books and articles calling for the restoration of Lysenko's academic credibility and refuting "myths" created by geneticists. For example, Iurii Chekalin, who edits an Orthodox magazine, *Feofil*, placed his detailed response to criticisms against Lysenko on a website run by deacon Andrei Kuraev, a popular public speaker on issues of Orthodoxy.[50] Chekalin is certain that the 1948 VASKhNIL session resulted in no damage to genetics in the Soviet Union. On the contrary, it "stimulated its development, as numerous research personnel were finally forced to do real work with results of a practical significance."

Recent apologists for Lysenko reproduce the arguments and style of their idol. These texts are almost indistinguishable: factual mistakes and deliberate misrepresentations can be found on every page, references to sources are given in a way that makes them difficult to check, and declarations concerning achieved successes are mingled with unsubstantiated accusations against their opponents. Though it is tempting to regard these works as trivial, in a Russia undergoing a spiritual and intellectual crisis of rising clericalism and anti-scientism, this type of literature is penetrating into the mass media and reaching broad strata of society. Thus, an article titled "Seed Wars," which has been published in the newspaper *Novyi Petersburg*, claims that Russia is not ready to step into the contemporary market of cultivated plant seeds because of the "deadening grip" of Vavilov's concepts, and his students who "illegitimately aspire to a place among world moderators of agrobiodiversity."[51]

The article, "The academician Lysenko and poor lamb Dolly," generated a wide response as well. It was published in *Literaturnaia gazeta*—a weekly cultural and political newspaper.[52] In the early 1960s, *Literaturnaia gazeta* criticized Lysenko on many occasions. But not this time. The author, Mikhail I. Anokhin—a doctor of medicine and the chief pediatrician for functional diagnostics in Moscow—assures that successful experiments in cloning mammals confirm Lysenko's theories, while Vavilov "made virtually no contribution to science and practice, yet he …travelled a lot, established connections in the West, had bank accounts there and …squandered state funding." The conclusion leaves no doubt of the author's position: "With the course of time experts had to acknowledge that Lysenko made a greater contribution to science, and to practice in particular, than Ivan P. Pavlov […] Dolly the sheep died but the genetics that Lysenko criticized with a perhaps exaggerated passion died even earlier."[53] Anokhin considers Lysenko's achievements as equal to the making of the first Soviet atomic bomb by Igor' Kurchatov, and the construction

of the intercontinental missiles by Sergei Korolev, that launched the first artificial earth satellites to space orbits. The paper is manifestly provocative in its treatment of scientists whose names mark the advances of Soviet science in the 1950s–1960s.

Finally, a jubilee issue of *Izvestiia Timiryazevskoi sel'skokhoziastvennoi akademii* (Proceedings of the Timiryazev Agricultural Academy—the school that Vavilov graduated from) published a paper titled "After the fight (jubilee notes)."[54] Its author, who is a historian of medieval Russia, argues for an objective approach to the conflict between Vavilov and Lysenko. He calls for acknowledging that both figures were right from a certain perspective, if judged in the socio-political and scientific context of the twentieth century. He writes that readers must leave aside "their personal sympathies and aversions, labels indiscriminately applied by both sides, and learn to discriminate between facts and their interpretations."[55] Yet the text makes clear that all these calls are addressed first and foremost to Vavilov's apologists (S. Reznik, M. Popovskii, Zh. Medvedev, V. Soyfer and Iu. Vavilov). In Zhuravlev's opinion, it was Vavilov's alleged failure to solve the objective set by Stalin—"to renew completely the seed pool within the shortest possible time span and in this way to increase radically the yields"—that was the reason for Vavilov's punishment, while denunciations made by Shlykov and Kol' in the early 1930s, or "Lysenko and his supporters' intrigues," played no role at all.[56] In the end, Zhuravlev's "objective approach" leads to the same conclusion: Vavilov defended genetics—the science that made no contribution to real improvement of agriculture, but merely created genetically modified organisms "with sterilization effects."[57] Genetics, therefore, is presented as part of a global conspiracy orchestrated by the US ruling elites, who aim to reduce the human population by 700–900 million people, thus making Russia into a "vast source of raw materials… void of redundant population."[58]

In the past two years we can see significant changes in the circles that aim to re-evaluate the historical and scientific role of Lysenkoism. Some professional geneticists joined these circles to produce the new literature in "re-thinking," or even fully rehabilitating Lysenkoism. One of them is a geneticist from the Vavilov Institute of General Genetics, Lev Zhivotovskii, a "honored worker of science of the Russian Federation" and a laureate of the Russian State Prize. In late 2014, Zhivotovskii published his controversial book, *Unknown Lysenko*, as a new attempt at "re-thinking" the role of Lysenko. He claims that Lysenko is one of the founders of the developmental plant biology. Zhivotovskii is not apologetic about Lysenko; his objective is rather

to restore the "unbiased" view on the Lysenko affair, placing Lysenkoists and geneticists on the same level. That major problem with this "detached objectivity" is that it is in no way warranted by serious historical scholarship. Neither Zhivotovskii nor his associates, such as well-known geneticist Stanislav Maletskii of the Institute of Genetics and Cytology in Novosibirsk, provided any new arguments for re-thinking Lysenkoism.[59]

Response of the Russian Academic Community

Nowadays some scientists and journalists again portray geneticists and plant breeders as "enemy agents," and the latter have to defend themselves. Prominent embryologist and geneticist Leonid Korochkin was probably the first biologist who warned about the threat of Lysenkoism's revival. In 2002, he published an article, "Neo-Lysenkoism in the Russian consciousness," in *Literaturnaia gazeta*. This article, in a revised form, was reprinted in the third issue of the *Bulleten' v zashchitu nauki* (Bulletin in Defence of Science) after Korochkin's sudden death.[60] Korochkin considered various attempts to create new variants of "genetics," or even to revive Lepeshchinskaia's doctrine of "living matter," and concluded that the dramatic growth of ignorance and popular "theosophy" are the main threats to Russian biology.

The other early statement against the revision of the "Vavilov and Lysenko affair," was made by a professor doctor of biology who works for the VIR, Ernst Truskinov. In his paper, "N.I. Vavilov. A life and death drama," published in *Zvezda*, a literary magazine popular among Russian liberals, as well as in his internet publication, "Shadows of the past and present: current attempts at discrediting academician N.I. Vavilov's scientific and civic legacy," on the VIR official site, he considers Mukhin, Iurkin and Pyzhenkov's work in terms of their cynicism and biases.[61]

Anokhin's 2009 article in *Literaturnaia gazeta* generated more than 600 responses. The opinions expressed ranged from the most positive to the most negative. A liberal publication, *Novaia gazeta*, was the first to react. One article, "Lysenko in a lamb skin," cited criticism by leading Russian geneticists and academics, including Vladimir Gvozdev, Evgenii Sverdlov and Garry Abel.[62] All of them refuted Anokhin's claims that Barbara McClintock's research on sheep cloning confirmed Lysenko's ideas. In his statement, Abel explicitly attributed Anokhin's publication to a crisis in Russian science. As he gloomily predicted, further developments "could be more serious than Lysenko-style ignorance. The attempts to reanimate

Lysenko have been facilitated by the neglect of fundamental research, a phenomenon coinciding with the rise of a poorly understood market economy… in virtually all spheres of life, with the unscrupulous pursuit of an easy and fast profit." The editorial comments said that these interviews raise the question of the social and political origins of Lysenko's recent advocacy.

The newspaper itself did not answer this question. Answer was not received from the Russian Academy of Sciences, which only posted these materials on the official website of the Presidium. Meanwhile, it is clear from published interviews of leaders of Soviet molecular biology, which show that the basis of neo-Lysenkoism is Stalinism, and the isolation of Russian science from the rest of the world, as well as the representation of geneticists as "agents of the West," who are responsible for the USSR's collapse. It is also obvious that the modern attempt to exonerate Lysenkoism is one result of a crisis in Russian science.

A popular writer and scientific journalist, laureate of the Russian State Prize, Vladimir Gubarev, asked the same question in his article, "Devil from the past": "Why drag academician Lysenko out again onto the historical stage?"[63] He also did not give an answer, although he pointed out that some other prominent Russian scientists, like Nobel Prize laureates, had also been slandered recently. Gubarev accused the editorial board of *Literaturnaia gazeta* of betraying its own liberal traditions because, in the Khrushchev years, the newspaper took part in the struggle against Lysenko. Gubarev said Anokhin was lying when he claimed that Nikolai P. Dubinin had allegedly acknowledged Lysenko's achievements. Gubarev was even prepared to go to court, if Anokhin wished to sue him for libel. Yet Anokhin refused to defend himself in this way.

Literaturnaia gazeta ignored the stream of protests against Anokhin's article. The only letter that spoke against Anokhin which was published in the newspaper was written by corresponding members of the Academy of Sciences, Nikolai Yanokovsky (the head of Institute of Genetics) and Il'ia Zakharov-Gezekhus (the chair of the Commission for research and preservation of N.I. Vavilov's academic heritage). The editorial board of *Literaturnaia gazeta* manifestly ignored a letter of protest written by the VIR Academic Council. Instead, they published a letter by P. F. Kononkov (Pyzhenkov's editor), who claimed that Pyzhenkov was correct in his assessment that the futility of Vavilov's research in plant breeding had been proven as early as 1939. The newspaper also published Anokhin's reply, in which he did not deny his poor understanding of genetics, but claimed the more important fact was that geneticists supported liberal views and market ideology.

Further reaction of Russian biologists to attempts to reconsider the "Vavilov affair" was far from unanimous. Some scientists continued to voice protest, yet their critical responses remain unpublished. Others consider these publications as a ridiculous episode, which does not deserve to be discussed by serious scientists. To some it is simply an expression of the freedom of speech. Yet in general the anti-Vavilov and pro-Lysenko campaign has consequences. The number of those who believe that geneticists and Lysenkoists were equally responsible for the tragedy of Russian biology is on the rise, even among biologists and historians of science. To my knowledge, among leading Russian geneticists, only I.A. Zakharov-Gezekhus and Valerii Glazko persist in publicly opposing what they call an "exhumation of Lysenkoism."[64] To their ranks we can also add the name of a Ukrainian biologist, Tat'iana I. Sokolova, who considered I.V. Michurin's research from the perspective of modern genetics.[65]

Zhivotovskii's book met strong critiques.[66] My critique of this book, from the point of view of the historian, was published by Commission against Pseudoscience on the portal of the presidium of the Russian Academy of Sciences.[67] Well-known biochemist Vladimir Muromets, professor of enzymology at the Moscow State University, published a critical review under the telling name "The timely, but wrong book about Lysenko" in the newspaper *Troitskii variant* (a newspaper very popular among liberal scholars).[68] He utterly disagreed with attempts to place Lysenkoists to the same category as geneticists. To support his position, Muromets uses some material from his private archive, including a 1956 letter from Ivan I. Schmalhausen to Nikita Khrushchev. In this letter Schmalhauzen, one of greatest evolutionists of the twentieth century, complains that he could not publish his work on evolution and his criticism of Lysenko for almost ten years.[69]

The molecular geneticist and historian of science, Valerii Soyfer, who left the USSR for the USA 25 years ago, was especially harsh in his assessment of Zhivotovskii's book. In his letter to the newspaper *Troitskii variant*, he reminded that from the very beginning, biologists and agronomists all around the world tested Lysenko's ideas and hypothesis, then re-tested and tested again. Every experiment proved the same thing: Lysenko's hypotheses were wrong. Every practical recommendation of Lysenko's proved to be fruitless as well. According to Soyfer, Zhivotovskii's book is not a scientific publication.[70]

Certain Russian biologists increasingly seek to avoid discussing the conflict between Vavilov and Lysenko. It is very telling that the recent

president of the Russian Academy of Agricultural Sciences, Gennadii A. Romanenko, requested placing Lysenko's portrait in a conference hall where the Presidium of the Academy has its meetings. No one except for a geneticist, Aleksandr Zhuchenko, spoke against this decision. In 2009, the VIR directorship passed to Nikolai I. Dziubenko, a former student of agronomist and seed breeder academician Vasilii I. Remeslo, who was one of Lysenko's closest supporters.[71]

Some Reasons for the Revival of Lysenkoism

The social and political processes stimulating the revival of Lysenkoism are fairly obvious. First and foremost is Russia's so called "sovereign democracy" (*suverennaia demokratiia*)," that is, politics of authoritarian rule, with some elements of democracy, but without most of its main principles. The opponents of genetics play upon the nationalist feelings of both the ruling elites and a certain section of Russian society, by exploiting their nostalgia for the days of Stalinism. But of far greater importance are factors that are internal to science: first of all, owing to the collapse of Russian science in the 1990s, and the isolationist tendencies of recent times.[72] They facilitate not only the revival of *Lysenkovshchina*, but also the spread of some elements of Lysenkoism.

The restoration of Lysenko's credibility could be seen as revenge of the part of those in the Russian academic community who work in the field of agronomy at the Russian Academy of Agricultural Sciences (RASKhN)—an institutional stronghold of Lysenkoists. It is quite indicative that the name index of the Proceedings of an international conference, "Vavilov's ideas in the contemporary world" (November 6–9, 2012, St. Petersburg), organized by RASKhN, contains no reference to Lysenko, even though there were a number of presentations on the historical context of Vavilov's work. In contrast, most papers by geneticists and historians of science that were published in the jubilee issue of *Vavilov's journal of genetics and selection*, placed emphasis on Lysenko's assault on genetics in the 1930s.

In VIR, Vavilov's famous collection of genetic plant resources has been reduced by 4 %within the last few years, while American, Chinese and Indian collections have expanded by about 50 %, downgrading the VIR collection to fourth place among the world depositories. Moreover, the VIR collection is at serious risk, as the institute lost several experimental stations and farms that are essential for its maintenance.[73] As a result, Vavilov's entire collection is under threat. Continuous attempts are

made to deprive the VIR of its experimental fields in the vicinity of St. Petersburg, because the land is very attractive to developers who plan to build high-priced private housing.[74]

It is important to realize that Lysenkoism has actually never disappeared from Russian science. Even after 1965, when Lysenko lost his influence, many of his disciples and supporters retained their positions as heads of departments, laboratories and research institutes within the system of agronomic research and higher education. For decades, millions of people studied the basics of biology from textbooks that reproduced Lysenko's ideas. No wonder that contemporary critics of evolutionary theory in Russia often make statements resembling Lysenko's attacks on genetics and neo-Darwinism. In their works we find the same old declarations that science leaves no room for chance, about the unity of a living organism and its environment, teleology and vitalism as essential attributes of life, inheritance as a characteristic of the entire cell and so on.

These basic Lysenkoist precepts have been revived under conditions of intensive clericalism of Russian society. Lysenko's advocates and Orthodox fundamentalists are often united. The editor of the Kononkov's book, G. Smirnov, saw in Lysenko's views the "influence of Orthodox theology," while geneticists, in his opinion, were all atheists.[75] According to an opinion poll carried out by the All-Russian Center for Public Opinion Research (VTsIOM) in 2009, in 140 settlements in forty-two regions of the Russian Federation, many citizens trust information publicized by creationists.[76] Since most Russians are not biologists, they are unable to notice numerous factual mistakes (or rather deliberate misrepresentations), while the authors of these texts shamelessly exploit public ignorance in order to boost their own sense of self-importance. In the last few years teleological, and even theological ideas, have been expressed by historians of biology Vadim I. Nazarov and Iurii V. Chaikovskii in *Vestnik RAN* (Herald of the Russian Academy of Sciences)—one of the leading academic journals—where they have been presented as the most recent concepts of evolution.[77]

In my view, attempts to restore the academic credibility of Lysenkoism are primarily related to the spread of *Lysenkovshchina* as a social practice. A revival of popular mysticism and superstitions, mass emigration of scientists who worked in the cutting-edge branches of natural sciences—especially in molecular biology and genetics—as well as the degradation of secondary and higher education—have all led to the dissolution of the boundary between science and pseudoscience. A growing gap between modern science and its popular understanding resulted in "academic"

publications in premier Russian scientific journals which reject contemporary biology, and call for a return to the Bible in the fields of ecology, nature protection or taxonomy. Some of these publications are written even by members of the Russian Academy of Sciences.[78]

Attempts to introduce creationism in middle school are becoming more and more persistent. In 2013, the Trinity Lavra of St. Sergius published the third edition of Sergei Vert'ianov's textbook *General Biology* for high schools.[79] It interprets contemporary biology from the Biblical perspective.[80] The Trinity Lavra of St. Sergius promotes the textbook as a model for "Orthodox biology." Vert'ianov claims that in the foreword he used some statements of Russian Academy of Science member and ex-director of the Institute of Genetics Iurii P. Altukhov (1936–2006). The hierarchy of the Russian Orthodox Church tries to influence genetic research and its practical applications. On December 25–26, 2013, the Holy Synod of the Russian Orthodox Church declared surrogate motherhood to be "against nature," and morally unacceptable, as it "contradicts the Creator's will."[81] Patriarch Kirill announced this decision on one of the state TV channels.

In the past few years, government agencies, when considering different kinds of projects, do not care to consult scientific experts. As a result, federal agencies, ministries and local administration lavishly fund suspicious projects, such as torsion fields, perpetual motion generators and so on.[82] Hundreds of millions of dollars were wasted on "top-secret projects" that promised massive advantages in armaments, communication technology and high energy. Pseudoscience reigns in agricultural and medical research, where it offers various nostrums such as "energy information therapy," "passive torsion generators," "resonance bio-correctors," and "quantum radiators" for treating plants, animals and humans.[83]

A "Clear Water" project is a particularly notorious case of pseudoscience that has gained massive support among the Russian political elites. This project promised to provide 100 % water purification from any contamination. It was proposed by Viktor I. Petrik—a man with no scientific background, who was convicted in 1984 for robbery, fraud and blackmail.[84] The chairman of the State Duma and the leader of United Russia, the current ruling party, Boris V. Gryzlov, became Petrik's co-author of Clear Water Project. Gryzlov labeled those who opposed him in his hopes to secure 500 billion dollars to fund this project "obscurantists." Ultimately this project was recognized as absurd, and contrary to the physics and chemistry laws. Gryzlov was forced to resign from both posts (chairman of the State Duma and party leader) because of this project. Yet many other influential supporters kept their positions, including Sergei Kirienko,

head of Rosatom State Nuclear Energy Corporation (and a former chairman of Russia's Council of Ministers), and Sergei M. Aldoshin, a vice president of the Russian Academy of Sciences. The Clear Water affair demonstrates that pseudoscience is still supported in Russia at the highest level, including leaders of the Russian Academy of Sciences.

Pseudoscientific ideas in Russia flourish in many institutes, such as the Kurchatov Institute that used to be the leading physics research institute in Russia.[85] Recent speeches by its current director, Mikhail Koval'chuk, who calls for the dissolution of all disciplinary boundaries, for a new science based on the unity of nanoinformation, bioinformation and cognitive technologies, are reminiscent of the Lysenkoist mantra concerning the transformation of nature under human influence.[86] The Kurchatov Institute receives ample funding and purchases expensive cutting-edge equipment. Yet its principal research output is "deciphering the human genome of the Russian nation," which received a lot of attention from the mass media in 2010–2012.[87] The absurdity of this idea is clear to every geneticist. The campaign that leading national mass media, including *Rossiiskaia gazeta* (the official newspaper of the Russian government) launched in support of this project (22.01.2010) resembled reports about Lysenko's achievements in the 1930s–1940s.

Koval'chuk failed to be elected to the Academy of Sciences in 2007; he also failed to be re-elected to the position of the head of the Institute of Crystallography in 2013. Yet the same year he was appointed by Putin as a secretary of the Presidential Council for Science. With Putin's support, Koval'chuk built a "scientific empire" that seems to be equal to the one ruled by Lysenko at the peak of his career. Apart from the Kurchatov Center, which incorporates the best institutes in nuclear physics, Koval'chuk is still the head of the Institute of Crystallography, the chairs of physics at the Moscow Physics and Technology Institute, and the Saint Petersburg State University, and the department of nanosystems at the Moscow State University. In 2013, when a bill authorizing the liquidation of the Russian Academy of Sciences was discussed, many members of the Academy compared Koval'chuk with Lysenko.[88] Like Lysenko before him, Koval'chuk makes promises to develop hybrid anthropomorphous systems (e.g., a soldier for future wars), genetic cards for every Russian citizen and so on.

In December 2014, one of the most respected Russian physicists, Vladimir Zakharov, named M. Koval'chuk among the main destroyers of Russian science, who as incompetent leaders control financial flows and make decisions. "The most dangerous among them are people with some scientific background, but who did not receive academic recognition and

are driven by their inferiority complex, which is compensated by 'overvalued' ideas distributed with genuine eloquence. That is, for example, the notorious M. Koval'chuk."[89] As far as I know, this opinion is supported by the majority of Russian scientists, but the government does not want to listen to them.

On September 20, 2013, Vladimir V. Putin signed a decree authorizing the reorganization of the Russian Academy of Sciences. The document provoked vocal protests among members of the Russian academic community.[90] Once again, the Russian state authorities demonstrated that they retain the supreme authority in all academic questions. Once again, Lysenkovshchina has been supported at the governmental level.

Conclusion

This overview of the recent attempts to exonerate Lysenkoism and to restore Lysenko's academic credibility in Russia examines the main arguments advanced by Lysenko's apologists, and illuminates the social, political and intellectual contexts of their efforts. Nationalist sentiments shared by the ruling elite, as well as a portion of Russian society, are particularly important in facilitating the revival of Lysenkoism in post-Communist Russia. However, the principal reason for the Lysenkoist revival lays in contemporary public attitudes toward science in Russia, especially the rise of anti-scientific sentiments in society and among the ruling elites, combined with the growing influence of religious fundamentalism. To some extent, the revival of Lysenkoism can also be explained by the academic traditions of Russian biologists, many of whom learned biology from textbooks produced by the advocates of Lysenko. Many of the most active and capable geneticists emigrated. A deeply entrenched confrontation between those Russian biologists who work at the institutions of the Academy of Sciences, and those who specialize in agricultural research—a field still dominated by Lysenko's advocates—is also a factor contributing to the revival of Lysenkoism.

At the same time, *Lysenkovshchina*, as a social practice of fighting against competing research teams by appealing to the party-state administration, was formed before Lysenko's ascent to the upper echelons of science. By placing recent attempts to revive Lysenkoism in Russia in the historical perspective, and by carefully reconstructing the events that took place in the late 1920s and early 1930s, we can produce a new understanding of the opposition between the Lysenkoists and geneticists in the Soviet Union, and re-examine the history of Lysenkoism on a global scale.

Notes

1. A complete bibliography of research on Lysenkoism would include hundreds of items ranging from seminal monographs to short notes in periodicals. Early works include Conway Zirkle, *Death of Science in Russia* (Philadelphia: University of Pennsylvania Press, 1949); Theodosius Dobzhansky, "The Suppression of a Science," *Bulletin of the Atomic Scientists* 5 (1949), pp. 144–146; Robert Cook, "Lysenko's Marxist Genetics: Science or Religion?" *Journal of Heredity* 40 (1949), pp. 169–202; David Joravsky, *The Lysenko Affair* (Cambridge: Harvard University Press, 1970); Zhores Medvedev, *The Rise and Fall of T. D. Lysenko* (New York: Columbia University Press, 1969); Loren Graham, *Science and Philosophy in the Soviet Union* (New York: Knopf, 1972); Valerii Soyfer, *T.D. Lysenko and the Tragedy of Soviet Science* (New Brunswick, NJ: Rutgers University Press, 1994); Elena Levina, *Vavilov, Lysenko, Timofeev-Ressovskii… Biologiia v SSSR: istoriia i istoriografiia* (Moscow: Airo, 1995); Nikolai Krementsov, "Printsip konkurentnogo iskliucheniia," *Na perelome* 1 (1997a), pp. 107–164; Nikolai Krementsov, *Stalinist Science* (Princeton: Princeton University Press, 1997b); later historical assessments include Nikolai Krementsov, "Sovetskaia nauka i Kholodnaia voina," in *Nauka i krizisy*, ed. Eduard I. Kolchinsky (St. Petersburg: Dmitriĭ Bulanin, 2003), pp. 830–906; Nils Roll-Hansen, *The Lysenko Effect. The Politics of Science* (Amherst, NY: Humanity Books, 2005); Eduard Kolchinsky, *Biologiia Germanii i Rossii-SSSR v usloviiakh sotsial'no-politicheskikh krizisov pervoi poloviny XX veka (mezhdu liberalizmom, kommunizmom i natsional-sotsializmom)* (St. Petersburg: Nestor-Istoriia, 2007); A bibliographic reference book on N.I. Vavilov, whose name is inextricably linked to his struggle against Lysenkoism, includes more than 1300 works in Russian and more than 100 works in foreign languages that are available in Russian libraries [Konstantin Petrov, Tat'iana Osiaeva, and Elena Ivoilova, *Nikolai Ivanovich Vavilov: k 125-letiiu so dnia rozhdeniia. Bibliograficheskii ukazatel'* (Saratov: Nauka, 2012)]. Ninety-four works on Vavilov and his collaborators were published just within four years (2008–2011). More than 60 works were published in 2012—the year of his 125 anniversary.
2. Theodosius G. Dobzhansky, who was by birth and education a Russian biologist, was particularly troubled by the plight of his colleagues, geneticists, in the Soviet Union. Living in the US, he tried to help them by all possible means. He was instrumental for the opening of a kind of a "second front" against the Lysenkoists (Nikolai Krementsov, "The 'Second Front' in Soviet Genetics. The International Dimension of the Lysenko Controversy," *Journal of the History of Biology*s 29 (1996), pp. 229–250.

3. Abba Gaisinovich, "The Origin of Soviet Genetics and the Struggle with Lamarckism (1922–1929)," *Journal of the History of Biology* 13, no. 1 (1980), pp. 1–51.
4. Simon Snoll', *Geroi i zlodei rossiiskoi nauki* (Moscow: Kron-Press, 1997); In subsequent editions Snoll' [Simon Snoll', *Geroi, zlodei i konformisty rossiiskoĭ nauki* (Moscow: Liblok, 2001)] considerably modified the title acknowledging in this way that the situation had been much more complex than a simplistic binary opposition.
5. Iakov Rokitianskii, Iurii Vavilov, and Vladimir Goncharov, eds., *Sud palacha. Nikolai Vavilov v zastenkakh NKVD. Biograficheskii ocherk. Dokumenty* (Moscow: Academia, 1999); Iurii Vavilov, *V dolgom poiske. Kniga o brat'iakh Nikolae i Sergee Vavilovykh* (Moscow: FIAN, 2004); Vladimir Esakov, *Nikolai Ivanovich Vavilov* (Moscow: Nauka, 2008).
6. Peter Pringle, *The Murder of Nikolai Vavilov. The Story of Stalin's Persecution of One of Great Scientists of the Twentieth Century* (New York: Simon & Schuster, 2008); Gary Nabhan, *Where Our Food Comes From: Retracing Nikolai Vavilov's Quest to End Famine* (Washington: Island Press; Shearwater Books, 2009).
7. Krementsov, 1996, 1997a, 1997b.
8. Nikolai Krementsov, "Lysenkoism in Europe: Export–Import of the Soviet Model," in *Academia in Upheaval. Origins, Transfer and Transformations of the Communist Academic Regime in Russia and the East Central Europe*, eds. Michael David-Fox and György Péteri (London and Westport, CT: Bergin & Garvey, 2000), pp. 179–202; Ekkerhard Höxtermann, "'Klassenbiologen' und 'Formalgenetiker' Zur Rezeption Lyssenkos unter den Biologen in der DDR," *Acta Historica Leopoldina* 36 (2000), pp. 273–300; Uwe Hossfeld and Lenart Olson, "From the Modern Synthesis to Lysenkoism, and Back?," *Science* 297, no. 5578 (2002), pp. 55–56; Laurence Schneider, *Biology and Revolution in Twentieth Century China* (Landham, MD: Rowman & Littlefeld, 2003); Audra Wolfe, "What Does it Mean to Go Public? The American Response to Lysenkoism Reconsidered," *Historical Studies in the Natural Sciences* 40 (2010), pp. 48–78; Tsyuoshi Fudjoka, *Ruisenko shugi wa naze shutsugen shitaka: seibutsugaku no benshōhōka no seika to zasetsu [Why did Lysenkoism Arise?: The Achievement and Failure of an Attempt to Dialecticize Biology]* (Tokyo: Gakujutsushuppankai, 2010); William deJong-Lambert, *The Cold War Politics of Genetic Research: An Introduction to the Lysenko Affair* (New York: Springer, 2012).
9. Francesco Cassata, "The Italian Communist Party and the 'Lysenko Affair' (1948–1955)," *Journal of the History of Biology* 45 (2012), pp. 469–498; Michael Gordin, "How Lysenkoism Became Pseudoscience: Dobzhansky to Velikovsky," *Journal of the History of Biology* 45 (2012), pp. 444–468;

William deJong-Lambert and Nikolai Krementsov, "On Labels and Issues: Controversy and the Cold War," *Journal of the History of Biology* 45 (2012), pp. 373–388; Rena Selya, "Defending Scientific Freedom and Democracy: The Genetics Society of America's Response to Lysenko," *Journal of the History of Biology* 45 (2012), pp. 415–442; Arturo and Quetzal Argueta, "Vavilov, a Soviet Darwinist in Mexico," *Studies in the History of Biology* 3, no. 2 (2011), pp. 66–82; Piotr Köhler, "Botany and Lysenkosm in Poland," *Studies in the History of Biology* 3, no. 2 (2011), pp. 32–53; Aglika Edreva, "The Destroying of an Eminent Genetics?" *Studies in the History of Biology* 5, no. 1 (2013), pp. 54–62; Tsuyoshi Fujioka, "The Japanese Lysenkoism and the Historical Backgrounds," *Studies in the History of Biology* 5, no. 1 (2013), pp. 7–17.

10. Aleksei Kouprianov, "The 'Soviet Creative Darwinism' (1930s–1950s). From the Selective Readings of Darwin's Works," *Studies in the History of Biology* 3, no. 2 (2011), pp. 8–31.
11. Eduard Kolchinsky, "Nesostoyavshiisia 'soiuz' filosofii i biologii (20-e-30-e gg)," in *Repressirovannaia nauka*, ed. Mikhail G. Iaroshevskii (Leningrad: Nauka, 1991), pp. 34–70; Eduard Kolchinsky, *V poiskakh 'sovetskogo soiuza' biologii i filosofii (diskissii i repressii v kontse 20-kh—nachale 30-kh gg* (St. Petersburg: Dmitrii Bulanin, 1999); Krementsov, 1997a.
12. Otto Landman, "The Inheritance of Acquired Characteristics," *Annual Reviews of Genetics* 25 (1991), pp. 1–20; Eva Jablonka, Michael Lachmann, and Marion J. Lamb, "Evidence, Mechanisms and Models for the Inheritance of Acquired Characters," *Journal of Theoretical Biology* 158 (1992), pp. 245–268; Eva Jablonka and Marion J. Lamb, *Epigenetic Inheritance and Evolution: Lamarckian Dimension* (Oxford: Oxford University Press, 1999); Mihkail Golubovsky, *Vek genetiki: evolutiutsiia idei i poniatii* (SPb.: SPbF IIET RAN, 2000a); Mihkail Golubovsky, "Nekanonicheskie nasledstvennye izmeneniia," *Priroda* 9 (2000b), pp. 3–9; Lev Zhivotovskii, "Nasledovanie priobretennykh priznakov: Lamark byl prav," *Khimiia i zhizn* 4 (2003), pp. 22–26; Lev Zhivotovskii, *Neizvestnyi Lysenko* (Moscow: KMK Press, 2014);Stanislav Maletskii, "Razvitie rastenii i vzgliady N.I. Vavilova v kontekste sovremennykh predstavlenii ob epigeneticheskoi nasledstvennoi izmenchivosti," in *Sovremennoe sostoianie i prioritetnye napravleniia razvitiia genetiki, epegenetiki, selektsii i semenovodstva sel'skokhoziastvennykh kul'tur*, ed. Ivan E. Likhenko (Novosibirsk: RASKHN, 2013), pp. 128–139.
13. Golubovsky, 2000b.
14. Jaroslav Flegr, "Was Lysenko (partly) Right? Michurinist Biology in the View of Modern Plant Physiology and Genetics," *Rivista di Biologia/ Biology Forum* 95 (2002), pp. 259–272.
15. *Ibid*., p. 258.

16. Yongsheng Liu, "Lysenko's Contributions to Biology and His Tragedies," *Rivista di Biologia/Biology Forum* 97 (2004), pp. 483–498 (michurin.narod.ru/liu2004.htm).
17. *Ibid.*, p. 496.
18. Iurii Ivanov, *Issledovaniia plodovitosti v sviazi s teoriiami biogeneza* (Novosibirsk, 2009).
19. Iurii Mukhin, *Genetika—prodazhnaia devka. Poznanie mira ili kormushka* (Moscow: Bystrov, 2006); Iurii Bobylov, *Geneticheskaia bomba. Tainyi stsenarii bioterrorisma* (Moscow: Belye al'fy, 2006).
20. All these books had gigantic print-runs by the standards of scientific and popular science literature in Russia. They range from 3000 to 10,000 copies. It testifies to a well-coordinated campaign that coincided in time with the first attempts made by the Russian Ministry of Education and Science (chaired by Andrei A. Fursenko—Putin's personal friend) to liquidate the Russian Academy of Sciences. Another campaign that was launched about the same time in Russia was the campaign for restoring a positive image of Stalinism and its punitive institutions. The Presidential Decree of May 15, 2009, no. 549 established a Presidential Commission to Counter Attempts to Falsify history to the Detriment of Russia's Interests (http://ria.ru/society/20090519/171517015.htm; http://ru.wikipedia.org/wiki). The Commission was chaired by the present Chairman of the State Duma Sergei E. Naryshkin, while the Ministry of Education and Science was charged with the task of assisting with its operation. The Commission was to compile the lists of "history falsifiers" who were meant to be punished in various ways, including criminal charges. Fortunately, these plans did not materialize. The Commission existed less than three years and was closed down by the Presidential Decree of February 2012, no. 183.
21. Mukhin, 2006.
22. In reality Lysenko supported Khrushchev's policies on agriculture and even published a small book *Some Most Important Aspects of Agriculture in the Virgin Land Regions* (Lysenko, 1960).
23. Rokitianskii et al., 1999.
24. Bobylov, 2006, p. 6.
25. Bobylov does not explicitly refer to the conflict between the Soviet geneticists and Lysenko, however, his book has been actively exploited by Lysenko's supporters, as the book argues that genetics is inherently hostile to the humankind and the Russian nation in particular. In the author's opinion, the progress of genetics might reduce the Russian nation to 1.170 thousand people within the next three hundred years (Bobylov, 2008, p. 125).
26. Vladimir Pyzhenkov, *Vavilov—botanik, akademik, grazhdanin mira* (Moscow: Samoobrazovanie, 2006; 2010).

27. Vladimir Pyzhenkov, *Nikolai Ivanivich Vavilov i ego 'zakon gomologicheskikh riadov v nasledstvennoi izmenchivost'* (St. Petersburg: SPbGAU, 2006); Vladimir Pyzhenkov, *N.I. Vavilov i N'iu-Iorkskoe otdelenie Biuro prikladnoi botaniki* (St. Petersburg: SPbGAU, 2007); Vladimir Pyzhenkov, *N.I. Vavilov, ego 'tsentry proiskhozdeniia kulturnykh rastenii' i introduktsiia* (St. Petersburg: SPbGAU, 2008).
28. Eduard Kolchinsky, "Kul'turnaia revoliutsiia v SSSR i pervye ataki na shkolu N.I. Vavilova," *Vavilovskii zhurnal genetiki i selektsii* 16, no. 3 (2012), pp. 502–538.
29. Valerii Glazko and Valentin Chesko, *Avgust 48. Uroki proshlogo* (Moskow: MSHkA, 2009).
30. FAO, *The Second Report on State of the World's plan genetics resources for food and agriculture* (Rome: Commission on Genetic Resources for Food and Agriculture, 2010).
31. An honorary title given by the Government to eminent scientists.
32. Piotr F. Kononkov, ed., *Trofim Denisovich Lysenko—sovetskii agronom, selektsionist i biolog* (Moscow: Samoobrazovanie, 2008).
33. *Ibid*., p. 5.
34. Piotr F. Kononkov, *Vklad Trofima Denisovicha Lysenko v pobedy v Velikoi Otechestvennoi voine* (Moscow: Samoobrazovanie, 2010).
35. Diane Paul, "A War on Two Fronts: J. B. S. Haldane and the Response to Lysenkoism in Britain," *Journal of the History of Biology* 16, no. 1 (1983), pp. 1–37; Oren Harman, "C. D. Darlington and the British and American reaction to Lysenko and the Soviet Conception of Science," *Journal of the History of Biology* 36, no. 2 (2003), pp. 309–352; Mikuláč Teich, "Haldane and Lysenko Revisited," *Journal of the History of Biology* 40, no. 3 (2007), pp. 557–563.
36. J.B.S. Haldane, "Nonsense about Lysenko," *Daily Worker*, 9 November, 1949, p. 2.
37. Uwe Hossfeld, "Po tu storonu sinteticheskoi teorii evoliutsii .Georg Schneider kak evoliutsionnyi biolog," in *Russko-nemetsskie sviazi v biologii i meditsine*, ed. Eduard I. Kolchinsky (St. Petersburg: Boreij Art, 2001), pp. 170–176; Nils Roll-Hansen, *The Lysenko Effect. The Politics of Science* (Amherst, NY: Humanity Books, 2005).
38. Vasilii Vil'iams "Prodolzhatel' dela Darwina," *Pravda* 120, no. 7086 (1937), p. 4.
39. Krementsov, 1997b.
40. Rokitianskii, Vavilov, and Goncharov, 1999; Rokitianskii, Goncharov, and Nekhotin, 2003.
41. Lysenko letters to the Department of General Biology of the Academy of Sciences dated April 10 and May 18, 1973; May 6, 1975 (Archive of Russian Academy of Science, fond 1521, opis. 1, delo. 128); the draft of

Lysenko letter to Soviet government or to the Central Committee of the communist Party, presumably the early 1970s .

42. Nikolai Ovchinikov, *Akademik Trofim Denisovich Lysenko: Michurinskaia biologiia* (Moscow: Luch, 2010), pp. 181–182.
43. *Ibid*., p. 188.
44. *Ibid*.
45. Piotr Kononkov, *Dva mira, dve ideologii. O polozhenii in biologicheskihk naukahk in Rossii v sovetskii i postsovetskii period* (Moscow: Luch, 2014).
46. Sigizmund Mironin, *Zagadka 37 goda Stalinskii poriadok* (Moscow: Algoritm, 2007); Sigizmund Mironin, *Zagadka 37 goda. "Gologomor na Rusi"* (Moscow: Algoritm, 2008a); Sigizmund Mironin, *Zagadka 37 goda. Delo genetikov* (Moscow: Algoritm, 2008b); Sigizmund Mironin, *Lzhenauka genetika. Chuma XX veka* (2010), http://lysvav.narod.ru/genetika_lzhenauka_miron.pdf; Sigizmund Mironin, *Zagadka 37 goda. Ubiistvo Stalina. Krupneishii zagovor XX veka* (Moscow: Eksmo, Algoritm, 2011a); Sigizmund Mironin, *Pochemu Stalin zashchishchal Lysenko* (Moscow: Algoritm, 2011b), http://www.rusproject.org/pages/analysis/analysis_10/genetika_lzhenauka_miron.pdf (October 15, 2015).
47. Mironin 2008a, 2010, 2011b, http://www.rusproject.org/sites/default/files/files/books/m/stalin_i_lysenko.pdf (June 20, 2011).
48. Mironin, 2011b, p. 4.
49. The "Leningrad Affair" was a series of court trials against members of the party and state leadership of the Russian Soviet Federal Socialist Republic that took place in the late 1940s—early 1950s. The trials targeted the Leningrad party leadership and many party and state activists who had been promoted from their positions in Leningrad to higher offices in Moscow after the war. The victims were primarily those people who were close to Andrei A. Zhdanov, the second man in the Stalinist hierarchy in the early post-war period, who was evidently sympathetic to geneticists (Graham, 1972). According to Nikolai L. Krementsov (2003, pp. 862–864), it was the public speech made by Iurii A. Zhdanov (Andrei Zhdanov's son and Stalin's son in law) at the Polytechnic Museum on April 10, 1948 that provoked Stalin's intervention into the conflict between geneticists and Lysenkoists and ultimately led to the 1948 VASKhNIL session. In the speech Iurii Zhdanov sharply criticized Lysenko.
50. At http://kuraev.ru/smf/index.php?topic=288407.0http://kuraev.ru/smf/index.php?topic=288407.0 (August 10, 2009).
51. Anatolii Iurkin, "Semiannye voiny," *Novyi Peterburg* 50 (2005), p. 8.
52. Mikhail Anokhin, "Akademik Lysenko i bednaia ovechka Dolli," *Literaturnaia gazeta*, no. 11 (6215), p. 12 (March 18–24, 2009). It was the favorite newspaper of freethinking intelligentsia before the collapse of the Soviet Union.

53. *Ibid*.
54. Aleksandr Zhuravlev, "'Posle draki' posleiubileinye zametki," *Izvestiia Timiriazevskoi sel'skohkoziastvennoi akademii* 4 (2012), pp. 164–174.
55. *Ibid*., p. 165.
56. *Ibid*., p. 168.
57. *Ibid*, p. 165.
58. *Ibid*., p. 173.
59. Maletskii.
60. Leonid Korochkin, "Neolysenkovshchina in rossiiskoĭ biologii," *Bulleten' v zashchitu nauki* 3 (2008), pp. 62–66.
61. Ernst Truskinov, "N.I. Vavilov. Drama zhizni i smerti," *Zvezda* 10 (2007), pp. 188–199, http://www.vir.nw.ru/books/Tru1.pdf
62. Kim Smirnov, "Lysenko v ovech'ei shkure," *Novaia gazeta* 33, no. 22 (2009).
63. Vladimir Gubarev, "D'iavol iz proshlogo. Komu ponadobilos' vytaskivat' na avanstsenu akademika Lysenko," *Delovoi vtornik* 13 (2009), p. 1.
64. Iliia Zakharov-Gezekhus, "Popytki reabilitatsii lysenkovshchiny," *Studies in the History of Biology* 3, no. 2 (2011), pp. 124–129; Glazko and Chesko.
65. Tat'iana Sokolova, *Nasledie I.V. Michurina: mify i real'nosti* (Lugansk: Maksim, 2010).
66. Zhivotovskii, 2014.
67. Professor E.I. Kolchinsky o knige *Neizvestnyi Lysenko* (iz perepiski s kollegami) at http://klnran.ru/2014/11/kolchinsky-o-knige-neizvestny-lysenko (November 2, 2014).
68. Vladimir Muromets, "Svoevremennaia nepravil'naia kniga o Lysenko," *Troitskii variant* 24, no. 168 (2014).
69. *Ibid*., p. 13.
70. Valerii Soyfer, "Pis'mo v redaktsiiu," *Troitskii variant* 25, no. 169 (2014), p. 5.
71. Kirill O. Rossiianov, "T.D. Lysenko i Akademiia nauk (iz neopublikovannykh dokumentov)," in *Repressirovanaia nauka*, ed. Mikhail G. Iaroshevskii (Leningrad: Nauka, 1991), pp. 518–527, 523. In his public statements, N.I. Dziubenko more than once called Remeslo an outstanding geneticist and plant breeder. He seems to be very proud of that fact that Remeslo supervised his master thesis (a private communication of VIR former director Viktor A. Dragavtsev).
72. Loren Graham, *What Have We Learned About Science and Technology from the Russian Experience?* (Stanford: Stanford University Press, 1998).
73. Victor Dragavtsev, "Nikolai Ivanovich Vavilov—odin iz 100 velikikh liudei planety Zemlya," *Gatchinskaia Pravda* 123, October 25, 2011, p. 6.
74. At http://www.fontanka.ru/2010/04/15/116

75. German Smirnov, "Konflikt v sovetskoi biologii," in Piotr F. Kononkov. *Dva mira, dve ideologii. O polozhenii v biologicheskikh naukakh v Rossii v sovetskii i postsovetskii period* (Moscow: Luch, 2014), pp. 6–43, 14.
76. Press release VTsIOM No. 1372, November 27, 2009 at http://wciom.ru/novosti/press-vypuski/press-vypusk/single/12813.html (March 17, 2010).
77. Vadim Nazarov, "Sovremennaia nauka za novuiu teoriiu evoliutsii," *Vestnik RAN* 77, no. 4 (2007), pp. 316–322; Iurii Chaikovskii, "Immunogenetika i evolutsiia: ne vpast' by v druguiu krainost," *Vestnik RAN* 73, no. 3 (2003).
78. Vladimir K. Zhirov, "Chelovek i biologicheskoe raznoobrazie: pravoslavnyi vzgliad na problemu vzaimootnoshenii," *Vestnik Murmanskogo gosudarstvennogo tekhnicheskogo universiteta* 11 (2008), pp. 609–626.
79. The most important Russian monastery and the 'spiritual' center of the Russian Orthodox Church.
80. Nadezhda Markina, "Adam i Eva byli pitekantropami," *Gazeta.ru*, March 28, 2013, http://www.gazeta.ru/science/2013/03/28_a_5118993.shtml
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