BOOK REVIEWS

Biology Under Lysenko and Stalin

Lysenko and the Tragedy of Soviet Science. VALERY N. SOYFER. Rutgers University Press, New Brunswick, NJ, 1994. xxvi, 379 pp. + plates. $39.95. Translated from the Russian by Leo Gruliow and Rebecca Gruliow.

The 1948 meeting of the Soviet Agriculture Academy (VASKhNIL) that led to the total defeat of genetics in the Soviet Union remains one of the most enigmatic events in the history of 20th-century science. There have been different interpretations of this unprecedented interference by Soviet authorities in science. Many of those who have written on Soviet genetics and Lysenkoism have emphasized the incompatibility of genetic ideas with Bolshevik ideology and with the political practice of Stalinism. Others have explained Lysenko's domination in Soviet biology by the "pragmatic" motives of Soviet Party leaders fascinated with Lysenko's plans and promises for agriculture. Based on extensive Soviet sources and on research in Soviet archives, Valery Soyfer's book provides a valuable contribution to our understanding of Lysenko's relation to Soviet agriculture and agricultural science.

Trofim Denisovich Lysenko became widely known in the Soviet Union in the so-called "Great Break" years (1928–1932) when Russian agriculture was collectivized and there was general enthusiasm about the role of science in rapidly raising agricultural production. Lysenko's vernalization technique, which found some positive response among the scientific community, was advertised by him as a universal recipe for solving almost any agricultural problem. Vernalization was followed by Lysenko's other, more or less cranky, innovations in plant breeding. Initially it had been Nikolai Vavilov and the Soviet genetics community who profited most from the general atmosphere of "great expectations." Supported by the top Party officials, Vavilov created the Agriculture Academy and made it the world's leading research center in genetics and plant breeding. By the mid-1930s, however, it had become clear that genetics had not been able to produce any magic results for Soviet agriculture to satisfy the utopian hopes associated with science. As a result, Lysenko moved to the foreground, his views became openly Lamarckian, and at the VASKhNIL conference, held in December 1936, he started his crusade against genetics. Two years later he was nominated the VASKhNIL's president; this was followed by Vavilov's arrest in 1940 and his death in prison in 1943.

In analyzing the background of the Soviet genetics debate between 1936 and 1948, Soyfer emphasizes the role of political ideology and argues that there was some correspondence between Lysenko's biology and "the ideals of the system." The case certainly raises some larger questions about the nature and political functions of Soviet ideological doctrines. However, I do not agree with Soyfer's general point that there existed some universal ideological paradigm that underlay the Soviet treatment of genetics. Lysenko was perhaps more consistent than geneticists in his sociopolitical rhetoric, and he often portrayed his disagreements with them as a conflict of two class-based sciences: his own new, "Socialist" biology versus "bourgeois" genetics. But there is absolutely no evidence that his political language had any serious effect on the outcome of the debate. It is also significant that the system itself was not monolithic and uniform in its ideology and politics and that there existed a lot of different attitudes toward Lysenko and genetics among various political and bureaucratic groups. For example, Lysenko was invariably supported by agricultural bosses, but up to 1948 the officials responsible for science and education had been skeptical and often negative about his theories and practical plans. Moreover, the archival finds Soyfer presents and describes in his book demonstrate that Lysenko caused disagreements among the top Party leaders in the postwar years. On the one hand, geneticists got vigorous backing from Yurii Zhdanov, the head of Central Committee's science department and also the son of Andrei Zhdanov, the number two man in the Party. On the other hand, Stalin was enthusiastic about some of Lysenko's postwar promises in agriculture. This led to an open conflict between the Zhdanovs and Stalin, which triggered the anti-genetics campaign at the VASKhNIL meeting of 31 July to 7 August 1948.

In his biological views, Lysenko followed the Lamarckian paradigm of the inheritance of acquired characters. He also argued that the concept of nature as plastic was crucial for the theory and practice of Soviet agriculture. At the same time, he completely ignored the social implications of Lamarckism. In the late 1940s, Lysenko formulated a new theory of species formation. He asserted that many species of plants and animals could spontaneously transform, even under natural conditions, into other, quite different species. Soyfer remembers that Lysenko was enthusiastic in telling his students about warblers giving birth to cuckoos. But he was unwilling to accept the transmutation of species among the mammals. This gave rise to rumors both in Russia and in the West that by making this reservation Lysenko intended to prevent the birth of an ape baby in his own family.

After the 1948 meeting, Lysenko dominated Soviet biology and agricultural science for almost 18 years. This may create some problems for any interpretation of Soviet science politics as essentially "pragmatic." It is hard to understand, for example, why Lysenko's failures in agriculture went unrecognized by the system for so long a time. It has often been assumed that by manipulating information Lysenko and his allies in the Agriculture Ministry and the Party Central Committee completely blocked the negative signals from agriculture. It might also be that there was some general lack of feedback, as the situation in Soviet agriculture, which was backward and rather disorganized, depended more on rational agronomy than on transfer of scientific knowledge into practice. Unfortunately the evidence we have at present is insufficient to prove or disprove any of these possibilities and to clarify this crucial issue in the Lysenko story.

It is interesting, however, that from the early 1950s there was a growing opposition to Lysenko led by Stalin himself, who was perhaps dissatisfied with Lysenko's practical failures. The publication of two anti-Lysenko articles in a leading Soviet botanical journal in December 1952 might have signaled the beginning of a major anti-Lysenko campaign, and it is likely that only Stalin's death in March 1953 postponed Lysenko's fall from power, which did not occur until 1965, when the government and Party finally deprived him of their patronage.

Even after 1965, Lysenkoites continued to occupy certain key positions not only at VASKhNIL and institutions of higher learning but also in the Party. The publications carrying criticisms of Lysenko and Lysenkoism were regarded by the authorities as "revanchist" and were almost totally banned under Brezhnev. In 1988, Soyfer broke the silence by publishing a piece on Lysenkoism in the leading Soviet *pere-*

Vignettes: The Editorial Process

The Peer Review System. Some like it! Some dislike it! Some believe it is unfair! Some suspect it is ambiguous! . . .

Since authors are themselves peer reviewers and vice versa, they may take on a sort of split personality. In their bifunctionality each should be fair to the other for there is no other way to self-respect and self-control.

—*H. Nöth*, in the foreword to H.-D. Daniel's *Guardians of Science: Fairness and Reliability of Peer Review* (VCH Verlagsgesellschaft)

High scientific quality and readability are the editor's responsibility; he is the one who must effect the unnatural but necessary symbiosis between hard economic facts and soft scientific virtues that characterizes scientific publishing.

—*Magne Nylenna* and *Povl Riis*, as quoted in *Principles of Health Care Ethics* (Raanan Gillon, Ed.; Wiley)

stroika magazine *Ogonyok*. Although *glasnost* and *perestroika* were under way, the article caused a furious reaction on the part of Yegor Ligachev, a Politburo member and the official in charge of Soviet ideology, science, and education. At his demand, Soyfer and his family were deprived of Soviet citizenship before they were allowed to go to the West. Quite unexpectedly, Soyfer was also attacked on the other front: his publication was criticized in the press by certain geneticists who were obviously concerned about its harmful effects on the compromise with the former Lysenko camp and on the stability of the Soviet biology community.

Soyfer's approach to the problem of Lysenkoism is based on the dichotomy of "science" and "power" in the history of Soviet biology, and his tone is very emotional when he writes about the destruction of genetics and perversion of science by Lysenko and Stalin. At the same time, his emphasis on the psychological and personal dimension of the Lysenko story produces a rather paradoxical image of Lysenkoism "with a human face." It is interesting, for example, that there were certain positive traits in Lysenko's personality. Lysenko sincerely believed in his messianic role in science and was nothing of the careerist-opportunist he has often been portrayed as in both scientific and historical literature; he was not ungenerous and helped his followers and younger colleagues; in addition, he was not anti-Semitic, and there is some evidence that in the conditions of official anti-Semitism of the late Stalinist years he gave vigorous support to certain Jewish scientists from his camp. Soyfer's study also contains a great deal of information and a lot of personal details about the lives of other scientists involved in the Lysenko story, especially those whose role was hidden or lied about, about the public image of Lysenko and Lysenkoist biology as it was shaped by Soviet newspapers and mass media, and about the interiorization of political rhetoric, of various cultural and ideological stereotypes, by Soviet science. This makes his book not only a pioneering study in the history of Soviet biology and of science politics under Stalin but also an important contribution to our understanding of the everyday life of the Soviet science community.

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Embryogenesis

Developmental Biology of Ascidians. NORIYUKI SATOH. Cambridge University Press, New York, 1994. xx, 234 pp., illus. $64.95 or £30. Developmental and Cell Biology Series.

A revolution in biology in the late 19th century provided the first insights into how embryos develop. Using the gametes of aquatic organisms, the biologists of this era first described in exquisite detail the cell lineages of development and then asked whether these normal lineages were inviolate or could be perturbed. These studies, done on embryos of ascidians, mollusks, worms, sea urchins, and amphibians, laid the groundwork for our views of how embryos work.

The current, late-20th-century perspective is that there are several ground plans, which exist as a continuum between different organisms, for laying down the body plan in the early embryo. In some organisms, such as mammals, there is no rigid map of the future embryo in the oocyte, and embryos depend on cell-cell interactions to set up the body plan. Other organisms, such as ascidians and fruit flies, possess a rigid map set into the oocyte (or generated shortly after fertilization in the zygote) that determines the body plan; cell-cell interactions are then important for later specification of the embryo parts.

The ascidian embryo figured importantly in the early work, being used by Chabry in 1887 in the first experiments in which parts of embryos were removed. This French scientist found that the embryos could not compensate for a lost part and that the blastomeres were in fact a mosaic of the future larva; his work led to the similar experiments of Roux on amphibians and Driesch and Boveri on sea urchins.

Although the bulk of developmental work today is done on flies, mice, sea urchins, and worms, there has been a resurgence of interest in the ascidians, with many groups in Japan, the United States, and Italy studying these next-of-kin to the vertebrates. The reasons for this renewed fascination become apparent as one reads Satoh's book, the first comprehensive monograph ever on the development of this group of animals.

First, there is the odd reproductive biology of these sessile filter-feeders, involving the production of eggs surrounded by a chorion and unique extra-ovarian cells. Sperm pass through this chorion but in the passage leave their mitochondria behind. Then there is the still-not-understood block to self-fertilization seen in many species of these hermaphroditic organisms, which T. H. Morgan first studied in 1904. And then there is the remarkable reorganization of the egg at fertilization, also first described at the turn of the century by Conklin, but now better understood in terms of calcium rises and calcium oscillations, movements of cell motors, and emplacements of specific cytoplasms in relation to the cytoskeleton. And again referring to Conklin and his original cell lineage, we now have an exquisite cell lineage worked out by Satoh and his collaborators along with identified molecular markers so that the nature of this important oocyte reorganization is on the verge of being understood. And then there is the elegant evidence for cytoplasmic determinants, which have been so nicely demonstrated by Whittaker and his colleagues in their description of differentiation in the absence of cleavage.

The ascidians also provide a fascinating