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Der Aufstieg des Lyssenkoismus

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My main project was to write a book in English about the rise of Lysenkoism and the suppression of classical genetics in the Soviet Union in the 1930s. I had collected material through a number of years and also written two papers as well as a book in Norwegian on this topic. Besides this I had planned to finish a couple of papers and to do some work on Scandinavian eugenics in cooperation with Gunnar Broberg, also a fellow in 1987/88. I also looked forward to stimulating discussions in the *Schwerpunktgruppe* "Culture of science" on "social construction" of scientific knowledge and related topics.

The suppression of genetics in the Soviet Union is the most striking example of political intervention into science in this century. Starting around 1936 it reached a climax in 1948 and an end point only in 1964. Classical genetics was suppressed because it expressed the aims and ideals of the bourgeoisie and the capitalist system and would be an obstacle to the development of socialism. Lysenko's so-called "agrobiology" was taken to represent the new kind of science needed by a socialist society. The harshness of the suppression reflected the totalitarian Stalinist system. David Joraysky in his classic account, *The Lysenko Affair,* has described vividly and intensively how Stalinist terror and "bossism" stifled real science and made pseudoscience thrive.

However, there is also an internal scientific side to the story. The political intervention built on two preconditions which were established in the first half of the 1930s. Firstly, Lysenko received wide support and acceptance within the scientific community. Secondly, a view of science that made strong political intervention legitimate became generally ac-

cepted in science policy debates. An important aspect of the story is that genuine scientists voluntarily cooperated with Lysenko. For instance his early career was supported by his later main opponent Nikolai Vavilov. This complicity of science in its own destruction needs to be studied. The good intention was to change science in such a way that it would more efficiently serve the building of a new and better society. But the theory of science that guided this attempt at a methodological reform or revolution was inadequate. My analysis concentrates on the role played by the pragmatic view of science expressed in the so-called "practice criterion" of the truth of scientific theories.

I have completed the historical narrative from 1927 to 1937 which I will take as the final year, adding a brief epilogue about later events. My analysis and explanation of the historical events have developed considerably during my year at the Wissenschaftskolleg, stimulated by sharp discussions in the Culture of science group. In particular discussions on "social construction" versus "realism" in the theory of science have served to develop and make more precise my unterstanding of the Soviet "practice criterion" of truth. What is now missing in a full first version of the manuscript on the rise of Lysenkoism is mainly a concluding chapter.

The work on Scandinavian eugenics concentrated on the sterilization laws introduced in the 1930s and on a comparison between Scandinavia and Germany. (See report from work-shop on "The comparative study of eugenics — Germany and Scandinavia" organized together with Gunnar Broberg.) Besides my collaboration with Gunnar Broberg I had interesting and useful contacts with Prof. Gerhard Baader and Dr. Michael Hubenstorf at the Institut für Geschichte der Medizin of the Freie Universität, Berlin. A paper on "The progress of eugenics: ideological and scientific factors" with a general international perspective was finished during my period at the Wissenschaftskolleg.

I was also able to finish a paper presenting a case study from the history of early 20th century genetics, "The Crucial Experiment of Wilhelm Johannsen". While interest in the interaction of science and politics is common to my studies of Lysenkoism and eugenics, the case study of Johannsen's selection within pure lines is focused on the problem of objectivity, trying to demonstrate the ability of experiment in some cases to provide a basis for definitive choices between alternative theories. It is part of my contention that such choices are highly independent of differences in social and cultural context. In this sense the *outcome* of crucial experiments is decided not by social context but by the nature of the object under investigation.

Discussions in the Culture of science group indicated considerable differences in the epistemological and philosophical intuitions of biologists

and physicists. The former tend to find a realistic conception of theoretical entities, as well as strong claims to objectivity, more natural and even necessary to a healthy science. (It goes without saying that many theoretical entities are best interpreted as thought instruments and that many scientific claims are only meant hypothetically.) Both sides agreed that this difference reflected differences in the dominant activities of the two fields, physics being highly experimental and technological while biology is still much concerned with the natural world. But the difference nevertheless poses questions about the primacy of the fundamental perspectives. Is science based on experimental manipulation or on natural historical description and explanation?

It is hardly by accident that studies of physics lead more often to a pragmatic view of science than studies of biology. The proponent of natural history might like to suggest that the pragmatic physicist sees the world from the perspective of the laboratory while the realist biologist sees the laboratory as merely a help to investigate more precisely the objects found in nature. Such speculations suggest that case studies of the presuppositions and effects of publicly sponsored research projects in ecology (environmental science) could give interesting insights into the relation between science and politics. While experimental science tends to have an instrumental effect, providing the means to solve problems set through political debates, ecology often supplies the agenda for political debate. In ecology it is less a question of how certain effects can be obtained, and the world changed, and more a question of what it is like.

Though communication in the Culture of science group partly became frustrated and broke down, I found the discussions and confrontations both interesting and useful for my own research. And I suspect that my appreciation of the positive sides of these discussions will increase as time goes by. At least I have developed a much clearer understanding of the content and differences between the various views designated as "social construction of science", and I hope this will be helpful in focusing future discussions on the most important issues and avoide side-tracks.