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The practice criterion and the rise of Lysenkoism*

The idea that social practice determines thinking lies at the heart of the Marxist tradition in theory of knowledge. It was succinctly stated in Marx's theses on Feuerbach and later developed into a theory of primacy of the material, productive "basis" of society over its "superstructure" of ideas and institutions. In early Russian and Soviet Marxism science was usually placed in the superstructure, and a theory of two fundamentally different sciences, proletarian and bourgeois, was developed. Marxist literature of recent decades has pointed out how this "doctrine of two sciences" legitimated the kind of political intervention that proved so fatal in the Lysenko affair. (Cf. Frolov, 1988; Paul, 1981). To reduce this unfortunate theoretical tendency to subordinate science to politics Soviet Marxism has in the post Stalin era removed science from the superstructure and made it part of the basis as "a direct productive force" (Josephson, 1981).

No doubt this relocation of science from the superstructure to the material basis of society removes one source of legitimation of political intervention into science. The doctrine of two sciences has been abandoned. But the theoretical basis for scientific autonomy still appears unclear. The doctrine of science as a direct productive force emphasizes those aspects of the Marxist tradition that are common to a broader pragmatic tradition in the philosophy of science. There has been a tendency in this tradition to play down the role of theory and to integrate the scientific activity into a broad social practice. It is natural to assume that without a clear distinction between science and other activities scientific autonomy becomes difficult to conceive, and therefore to defend, when it is put under political pressure for one reason or another. This paper attempts to sketch the role of a broad and undifferentiated "practice criterion of truth" in creating a situation where the tradition of scientific autonomy could be set aside and valid results rejected in favour of wishful thinking.

For about thirty years, from the middle of the 1930's to the middle of the 1960's, Soviet biology was dominated by views which are now generally acknowledged to have been pseudoscientific[1]. The big question is: How could this happen, in the middle of the twentieth century, and under a regime which took special pride in its scientific approach to everything? I will try to show that this question is not merely of historical interest. The student revolution of 1968 inspired new interest in ideas similar to those that provided stepping stones for Lysenko's career.

The dominant ideology of the student revolution emphasized a wish for "unity of theory and practice" and a belief in radical social and class determination of science. The vision of science that inspired the student movement in the 1960's and 70's also became a source of inspiration for professional studies of science. In recent years a radical social relativism, comparable to that inherent in the doctrine of two sciences, has been upheld by, for instance, by Harry Collins (1985), Barry Barnes and David Bloor (1982), and Bruno Latour (1987). More moderate proponents of the "social construction" of scientific knowledge reject relativism while emphasizing the dependence of science on cultural, social and technological practices. Such views have been stated, for instance, by Simon Schaffer and Steven Shapin (1985), Norton Wise and Crosbie Smith (1986), and Timothy Lenoir (1988). I believe that a confrontation of these relativist and constructivist trends in science studies with historical situations where some of their contested ideas were actually used as principles of science policy, will be a useful exercise in the promotion of a general theory of science. For instance, the relation between basic and applied science has, in the aftermath of the student revolution acquired some of the same confused state that it had in Soviet science politics of the 1930's.

The argument of this paper has two main steps. First I show that an illusory belief in the practical usefulness of Lysenko's science was the basis for his external support. Then I analyze how this illusion was created through a science policy based on a Marxist theory of science that attempted a utopian unification of "theory and practice", albeit for praiseworthy political motives. It is my claim that the blurring of distinctions between science, technology and ideology undermined the critical function of science and prepared the way for Lysenkoism.

I want to stress that the special social, economic and political conditions of the Soviet Union are obviously an essential part of any historical explanation of the "Lysenko affair". No adequate understanding can be reached without keeping in mind the economic and social destruction wrought first by civil war and then by forced collectivization. It is not difficult to see that especially from 1936 onward the particular political system that Stalin headed, characterized by the "cult of personality", impinged directly on crucial decisions concerning the research system. The harshness with which the victims of scientific suppression were treated reflects the general brutal character of this system. In the best documented and most penetrating study that exists, *The Lysenko Affair* by David Joravsky (1970), the "self-deceiving arrogance among the political bosses" provides the central principle of explanation. "Stalin's hand" prepared the way for Lysenko and provided the decisive pushes which let him fulfill his lust for power.

However, political tyranny and terror does not necessarily lead to support of pseudoscience, and it cannot explain the direction and content of the pseudoscience that the Soviet regime supported. How did it come about that the leadership made a scientifically irrational choice against well-established tradition s of research? Why did it prefer Lysenko's research program for biology and agricultural science over alternatives which would have served the economy better? My analysis will focus on the views about science and its social role held by the various participants in the drama. I will try to show how the theory of science then dominant in the Soviet Union played a decisive role through the limitations and tendencies that it imposed on the thinking of bureaucrats and politicians, and to some extent on the scientists themselves.

In Soviet discussions about Lysenko's views and methods in the 1930's, i.e., in the period when he rose to become a major star in Soviet science, appeals for practical success played a major role. The Lysenkoites constantly referred to successful "practice" as a main argument. Their opponents seldom disputed these claims and tried feebly to present the public with equally successful applications of their own theories.

The practic criterion

The practice criterion for truth of scientific theories says roughly that the truth of a theory can best be judged by its success in practice. This is an idea with intuitive appeal to those who value empirical studies and practical usefulness, and are sceptical of loose speculation in science. But as we shall see, without distinctions that can separate relevant from irrelevant practice, the principle may subvert rather than support such values. That a theoretical idea can be shown to work in a scientific experiment does not mean that introducing the new technique into practical production will increase the output. And the ability of an idea to create social cohesion and purpose in a group guarantees neither its scientific truth nor the economic usefulness of its associated techniques. We have in other words to distinguish between scientific, technological and social "practice" criteria.

Roughly I will try to distinguish three levels or kinds of practice criteria:

1) *Scientific.* Having to do with the "practice" of scientific experimentation. It establishes the existence of entities and causes and the general possibility of producing certain kinds of phenomena. For instance, in Lysenko's case the vernalization (cold treatment) of seed grain to speed up development was established as a really exciting phenomenon.
2) *Technological.* Linked to the questions of applied

science and technology. It establishes the technical/economic efficiency of a certain production procedure or process relatively to available alternatives. For instance, in Lysenko's case the vernalization of seed grain was *claimed* to be economically profitable.

3) *Social.* This includes different criteria for a broad evaluation of scientific ideas. One important aspect is the ideological evaluation. From the point of view of general political practice the harmony or conflict of a certain scientific idea with a preferred ideology can be important. For instance, Mendelian genetics was thought to be responsible for racism. Metacriteria for the two first levels of practice criteria also belong here. For instance, one could hold that the technological criterion should dominate the scientific. Or that the ability of a theory to produce social cohesion and unity of purpose in a group should be part of an evaluation of science and technology. It was typical of Soviet science policy in the 1930's to let various criteria on the third level influence judgments on the two other levels.

In the biological debates in the Soviet Union in the 1930's and 40's the practice criterion worked within a specific theoretical framework. Two elements of this framework which tended to undermine and blur the distinctions that I just mentioned were the principles of *partiinost'* and "unity of theory and practice".

The demand for *partiinost'* derived from Marxist sociology of knowledge: All products of intellectual activity are marked by class interests and it is the duty of a Marxist to make certain that his products serve the interests of the proletariat. During the period of cultural revolution, 1928 to 1932, this obligation was extended to all intellectual work. Class interest became a legitimate and powerful argument in scholarly debates.

John Barber (1981) in his book, *Soviet Historians in Crisis 1928—1932,* has analysed the transition in historiography. He finds the decisive point to be when Stalin intervened in the debate in October 1931 with a letter to the journal *Proletarskaia revolutsiia.* This put a definitive end to the relatively relaxed and pluralist cultural atmosphere of the NEP era. By the end of 1932 "Intellectual neutrality and academic autonomy had ceased to be options" (p. 5). The central figure of Barber's account is Mikhail Pokrovsky who headed the Communist Academy from 1922 and the Institute of Red Professors from 1921 until his death in 1932. Though the autonomy of natural science was not as easily wiped out as that of historical scholarship, Barber's analysis does demonstrate the great change in intellectual climate that took place around 1930: intellectual struggles as well as personal rivalries from now on "took place in a context of consensus over basic principles [with] absolute rejection of non-marxist scholarship, the need to make historical study politically relevant, the desirability of close contact between the intellectual and political spheres ..." (144)

The demand for unity of theory and practice has its roots in Marxist critique of idealism in bourgeois philosophy and science. A classical locus is Marx's second thesis on Feuerbach:

> The question of whether objective truth can be attributed to human thinking is not a question of theory but is a practical question. In practice man must prove the truth, that is, the reality and power, the this-sideness of his thinking. The dispute over the reality or non-reality of thinking which is isolated from practice is a purely scholastic question. (Quoted from Wetter, 1958: 509.)

That "theoretical science is divorced from practice" or that "theory lags behind practice" became standard complaints in the 1930's, and a main aim of science policy was to make theory catch up and become more useful by forging a closer unity between the two.

The period of cultural revolution was also the period of the first five-year plan, with rapid industrialization and forced collectivization in agriculture, as well as the period when Lysenko made the first important steps in his career as a manager of Soviet science.

The illusion of practical effectiveness

As I have mentioned, a firm belief that Lysenko's methods had in fact led to increased yields in agriculture dominated the Soviet debate in the 1930's and 1940's. In particular this pertained to his vernalization of grain. Vernalization was a cold treatment of seed which had just started germination. It speeded up the maturation of the plants and could in theory be used in various ways to overcome climatic problems. Lysenko's first proposal was to sow winter grain in the spring where harsh winters would damage autumn sowings. To the popular imagination this appeared a brilliantly useful discovery. Through vernalization one could avoid the problem of sprouts perishing during the winter by sowing in the spring instead of the autumn. However, this proposal was quickly abandoned because the traditional alternative turned out to be preferable, namely to use spring grain which did not need cold treatment to develop in one season. Lysenko turned instead to a quite different agricultural problem. He proposed vernalization of spring grain as a method to combat the effects of summer drought. If you could speed up the maturation of the grain by a week, for instance, the effects of summer drought would be lessened. This was the method that was introduced on a large scale in Soviet agriculture in the early 1930's and which made Lysenko famous.

Belief in the practical usefulness of Lysenko's vernalization of grain was also widespread in the West. Even after the pseudoscientific character of his work in genetics had been revealed it was widely thought that at least his early proposals of vernalization had been a genuine improvement. Joravsky (1970: x), for instance, started his study of the Lysenko affair with the assumption that some of Lysenko's methods had really increased farm yields. This would provide a minimum of initial rationality to Lysenkoism. Its development could then be explained as sound science gone astray through ideological and political interference. But when Joravsky dug into the historical material, he found no evidence for increased yields, only failures and unscientific experiments and arguments. Zhores Medvedev (1969) in the *The Rise and Fall of T. D. Lysenko* likewise concluded that Lysenko's claims to practical success were illusory.

Despite the analysis and documentation of Medvedev and Joravsky some Western writers have continued to believe that Lysenkoism had a basis in useful agricultural techniques. For instance, the French Marxist philosopher Dominique Lecourt (1977) has assumed that the rise of Lysenko did have a "real material basis". Some of the early methods, like vernalization of wheat, must have increased the yields. Lecourt's argument against Medvedev and Joravsky does not stand up, however. One serious weakness of his analysis is a lack of knowledge of the biology involved. For instance, he appears to assume that it was vernalization of winter wheat that was applied on a large scale (p. 64). He seems unaware that Lysenko quickly changed his proposal to vernalization of spring wheat. The evaluation of new methods in agriculture depends on thorough comparison with the best available alternatives. A clear conception of the method that is under evaluation is thus essential.

A second flaw in Lecourt's argument is that he does not clearly distinguish the effectiveness of a technique in a scientific experiment from the economic effectiveness of the same technique when it is applied on a large scale under conditions of production. Lecourt uses as a main argument for increased yields the scientific fact that winter grain can be made to ripen in one season through vernalization. You cannot have it both ways, he argues, "either the technique works or it does not" (pp. 62—63). In this case it worked as a scientific experiment, but it did not increase agricultural yields.

Another analysis of Lysenkoism, by the American biologists Lewontin and Levins (1976), is also superficial on the crucial question of the practical effectiveness of Lysenko's early methods, in particular the vernalization of wheat. They avoid commitment to the belief that his methods did increase the yields. But they do dispute that Lysenkoist errors were "responsible for a disruption in the progress of agricultural production". They maintain that no evidence has been presented which demonstrates that Lysenkoism led to decreased agricultural yields in the Soviet Union; "we might as easily postulate that they would have been lower except for Lysenkoism" (p. 58). This claim is substantiated by an interesting table comparing wheat yields in the United States and the Soviet Union through the years 1926 to 1970. Lewontin and Levins point out that the two countries had a parallel increase in yields through this period with the Soviet Union making a marginally larger gain than the United States. Though the yield per unit area is twice as high in the United States, one has to agree that by themselves these data give no evidence for a negative effect of Lysenkoism.

However, by presenting this argument and not attempting a closer analysis of the agricultural techniques, Lewontin and Levins cover up the fundamental question of the effectiveness of Lysenko's methods. It also appears that they are unaware of Lysenko's early shift from vernalization of winter grain to vernalization of spring grain. They choose the comparison of wheat yields because "the vernalization of winter wheat was the first Lysenkoist recommendation and one with which the movement came to be identified" (p. 58).

Some people may think that I judge Lysenko's methods to have been ineffective without sufficient evidence. As I find Joravsky and Medvedev to have presented quite convincing arguments, I will not go into an extensive discussion, but merely present briefly some main reasons. The most striking evidence is perhaps Lysenko's own writings where he presents the investigations that allegedly document an increase in yields. They are a travesty of scientific method.

For instance, in 1932 vernalized wheat, mainly spring wheat, was sown on a total of 43,000 hectars distributed over a large number of collective and state farms in the Ukraine. A set of three questionnaires were distributed to all participating farms. The first was to register the time from sowing to the emergence of sprouts above ground. The second would register the formation of spikes, and the third the size of the yields. By 27 August questionnaires representing close to half the area had been returned. But the great majority of farms had only returned the first one. There were 722 replies to the first, 77 to the second and 59 to the third questionnaire. On this material Lysenko (1932) based a preliminary report printed in the second issue of his new journal *Biulleten' Jarovizatsii*. Lysenko found that vernalized plants were on an average 3—4 days earlier in emerging above ground and in producing spikes. With respect to yields Lysenko was cautious in his claims due to the small number of returns. But he did use considerable space to describe some of the most successful cases, apparently to demonstrate what the method could yield when properly applied. No further report of this trial was published.

This fragment was the closest to a scientific evaluation of the method that Lysenko ever published. By 1935 he had landed on a yield increase of 10%. This apparently was based on trials reported by questionnaires. But no account is given of the percentage of returns, or of the way these trials were organized, with respect to controls, etc. (Rodionov and Filatov, 1935). The early issues of Lysenko's journal were filled with reports of successful trials at individual collective farms, with no serious attempt to weigh the positive against the negative results in order to reach more general conclusions as to the effectiveness of the method. Another strong indication that the effectiveness of Lysenko's methods was illusory is his inability to reply when public criticism was raised. In the autumn of 1936 three prominent agricultural plant scientists, Konstantinov, Lisitsyn and Kostov, published a paper where they pointed out that the results of vernalization were very variable, and that no proper testing had been carried out to clarify under what conditions the method was beneficial. Results varied according to weather, local conditions and varieties of grain. It was necessary to find the right varieties and adapt methods to the various local conditions before vernalization of seed grain could be usefully applied in practice. In 1936 vernalized wheat had, on the average, given smaller yields than non-vernalized wheats. In his reply, Lysenko (1937), produced little else than invectives against his opponents and descriptions of individual successful examples to support his claims.

Despite threats from Lysenko that researchers who did not respect the results reached by application in practical production could be "swept away from scientific activity," the criticism was repeated on a number of occasions in 1936 and 1937. And it appears to have made its mark. According to official Soviet sources, 7 million hectares were vernalized in 1936 and 10 million in 1937, but for later years no figures were published (Whyte, 1948: 8—9). Various sources concur in the judgment that the method was quickly abandoned. It is also important to notice that the method that was defended as "vernalization" was further changed and ended up having little to do with the original cold treatment. It degenerated into something very similar to the old peasant method of heating the seed in the sun before sowing[2].

Vavilov's support for Lysenko

We can now narrow down our general question about how Lysenko's take-over of Soviet biology could happen. We want to ask more specifically: How was the myth of the practical usefulness of Lysenko's ideas created? Why was scientific criticism so ineffective? I believe the disregard for traditional scientific norms of behavior and standards of method was a main source. We will approach this theme by looking more closely at Lysenko's relationship to the most influential Soviet agricultural scientist in this period, Nikolai Ivanovich Vavilov[3].

Vavilov was born in 1887, eleven years before Lysenko, as the son of a rich Moscow merchant. He received a thorough Russian training in biology and agricultural science finished off by a year abroad, the larger part of which was spent studying plant breeding and genetics with William Bateson and other British scientists. In the early 1920's Vavilov became a leading figure in the development of Soviet agricultural science. When the Lenin Academy of Agricultural Science was established in 1929, Vavilov was the obvious president. A young, enthusiastic and very energetic proponent of the new society to be created after the revolution, Vavilov was a characteristic representative of the positive collaboration between non-communist scientific specialists and the Soviet government.

Though Vavilov of late is frequently hailed as a great geneticist, his first special interest was plant pathology, and his most influential scientific contributions were in the study of the origin of cultivated plants. His special fields of research were systematics and plant geography. But above all he was a great scientific administrator. His central project was the World Collection of cultivated plants which was to serve as the basis for breeding new varieties for Soviet agriculture. For many years he and his collaborators made long journeys to many countries to collect seed and plants. In connection with this work Vavilov developed his famous theory about the geographical centers of the origin of cultivated plants.

As the main entrepreneur of Soviet agricultural science Vavilov was eagerly looking for young talents. With the rapid expansion, especially in the years 1929—34, this demand was not easy to satisfy. Already in 1927, when Lysenko first attracted public attention through a panegyric interview in Pravda, Vavilov tried to get him to work in the All-Union Institute for Plant Industry (VIR), the famous institute that Vavilov was then developing in Leningrad. Vavilov's attempt was stopped, however, by the institute's senior plant physiologist, N. A. Maksimov, who found Lysenko uneducated and unwilling to learn. (Reznik 1968: 263—66)

But Vavilov's interest in Lysenko's work continued, and his support reached a high point in the years 1932—34. At this stage the support was so strong and systematic that Lysenko can be characterized as the protégé of Vavilov, according to Mark Popovsky (1966: 18). This interpretation has been sharply criticized. Medvedev (1967) pointed to a number of inaccuracies in Popovsky's account and found it tendentious. Joravsky (1970: 379) complained that it is "spoilt by a picture of Vavilov as a dupe, who really believed in the scientific value of Lysenko's work". However, with a few corrections the main documentation of Popovsky stands uncontested. The disagreement lies in how it is to be interpreted. To what extent can Vavilov's public utterances be taken at face value? It should be noted that Medvedev (1969: 256—157), in contrast to Joravsky, does agree that Vavilov took a favourable view of Lysenko's theory of stages in the development of plants. What Medvedev denies is that this positive evaluation promoted Lysenko's career.

In the spring of 1932 Vavilov visited Lysenko's team in Odessa and wrote to his colleagues in Leningrad: "Lysenko's work is amazing and forces us to look at many things in a new way. It is necessary that the world collection is worked through with vernalization ..." (Popovskii, 1966: 16). In a lecture at the 1932 International Congress of Genetics in Ithaca, USA, Vavilov (1932: 340) praised Lysenko highly: "The remarkable discovery recently made by T.D.Lysenko of Odessa opens enormous new possibilities to plant breeders ... This discovery enables us to utilize, for breeding and genetic work in our climate, tropical and sub-tropical varieties, which practically amounts to moving the southern flora northward." Vavilov's support was also expressed in recommendations for scientific prizes and membership in learned academies. In 1932 Vavilov supported Lysenko's election to the Ukrainian Academy of Science, and in February 1934 he wrote to the Academy of Sciences in Moscow recommending a corresponding membership for Lysenko (Popovskii, 1966: 17).

Popovsky seeks a partial explanation of Vavilov's favourable attitude toward Lysenko in his frustation with politicized scientific recruits in Leningrad. As the son of a peasant Lysenko had the right class background. He was energetic and diligent and did not concern himself much with politics or philosophy. Like Vavilov himself he worked around the clock. Taking qualifications and working conditions into consideration Lysenko had produced some interesting results. If one was to make an effort to realize the aims of a new proletarian science, argues Popovsky (1966: 17), Lysenko appeared a good choice, compared to the available alternatives.

This evaluation is in good accord with the recollections of the British geneticist and plant breeder S.C.Harland who travelled in 1933 with Vavilov in the Soviet Union, visiting Lysenko's Odessa institute among other places. While Harland found Lysenko's ignorance appalling, Vavilov defended him. Lysenko studied important questions about the interaction of plants with their environment. Young men like Lysenko, who "walked by faith and not by sight" might discover important things, even how to grow bananas in Moscow. All progress in the world has been made by such angry men, so let him go on working. It did no harm and might do good, Vavilov had argued (Harland, 1948).

In 1930 Vavilov stressed the close practical and political links of the newly-established Academy of Agricultural Science. It should "march in step with the agricultural revolution" that was taking place under the leadership of the peoples' commissariat of agriculture. It should even "walk ahead" of this revolution, Vavilov added enthusiastically. The fundamental principle was that research should be carried out "in the spirit of production" and that results should be "immediately transformed into production." The unity of theory and practice was an idea that appealed to Vavilov. He was clearly more reserved with respect to the *partiinost'* of science: "Scientific work is deeply international, the real scientist is an internationalist." (Vavilov, 1930).

In a planned economy one naturally wanted also to plan science. Vavilov participated in the first all-union conference for planning of science in April 1931, speaking on "agricultural science in a socialist economy". He started by describing the lack of science in earlier agriculture. Without explicitly mentioning collectivization, he maintained that in the new socialist system the possibilities of applying science to agricultural production were much better. The new economic order was to be matched by a better funded and better organized science. Vavilov was very optimistic with respect to the efficiency of this combination of a new economy and a new science: Tasks of improvement "that had earlier taken decades could now be solved in two or three years". (Vavilov, 1931a: 132)

The key-note speech of Bukharin at this planning conference sketched rather moderate principles for the steering of science. They needed not amount to more regimentation than is now commonplace in any Western country. The superior planning bodies should only give "general directions" for scientific research, and eminent researchers must be given ample opportunity to concentrate on what they found to be the most important scientific problems. (Graham, 1967: 34—45) Bukharin had at this point lost the power struggle with Stalin, but he was still an influential theoretician and party spokesman on ideological matters.

Though Bukharin's approach to problems of science planning at this conference appears to have been careful and moderate, he did not lack more radical visions. These he developed in his speech at the International Congress for the History of Science in London in the summer of 1931. His starting point was Marx's anti-idealism. The scientists' traditional question of what the world is like must be replaced by the question of how it can be changed. Bukharin played down the realism that Lenin had emphasized in his theory of knowledge as "reflection" of reality. Bukharin tried to erase the difference between realism and instrumentalism by identifying theories as reflections of the world with theories as instruments to manipulate it. The instrumental criterion of truth is not in contradiction with the realist criterion of correspondence "but coincides with it ..." (Bukharin, 1931: 18).

Bukharin's main example of real unity between theory and practice was Soviet agriculture and agricultural science: "One can feel with one's hands how the development of Socialist agriculture pushes forward the development of genetics, biology generally, and so on." He commented ironically on the worry that a too pragmatic view of science could have a degrading and narrowing effect. This could only happen in capitalist countries, but not in the Soviet Union, because "great practice requires great theory" (p. 31). In contrast with his speech at the planning conference in April Bukharin now described radical planning and steering of science according to economic and political aims as unproblematic. At the same time his activist social and non-realist conception of scientific knowledge made it difficult to differentiate between different kinds of "practice criterion": between the successful experimental test of a theoretical scientific idea, the technological usefulness of a method, and the political evaluation of the total social benefit of a theory and its associated technology. Bukharin wound up his speech by a declaration of general cultural revolution: "It is not only a new economic system which has been born. A new culture has been born. A new science has been born. A new style of life has been born" (p. 33).

In his speech at the London congress Vavilov exemplified Bukharin's claims. He explained how we can learn to control "the historical process" in one sector, namely the evolution of cultivated plants and domestic animals. In particular he discussed the usefulness of research into agricultural history (Vavilov, 1931b).

Big promises with respect to practical results were made by Vavilov when the Academy of Agricultural Science was established. He spoke enthusiastically about the new socialist science which united theory and practice in a way that was unknown in capitalist countries. But soon people started asking for the practical results of his research program, in particular the world collection of cultivated plants. Here Vavilov saw vernalization as a promising method. He emphasized vernalization as a method for analyzing the properties of the plants and as a technique to help breeding work by speeding up development. If one could cut the time it took to develop new varieties from ten to five years, for instance, this would indeed imply large economic gains for Soviet agriculture. Vavilov hardly thought of Lysenko's general theory about the stages in the development of plants as a really important contribution to theoretical biology, though he did occasionally say so in public. And it is not likely that he put much faith in the practical methods of production that Lysenko had so far developed, like the vernalization of spring wheat as a measure against drought. But he did praise Lysenko highly in general terms. And in the public debate these important distinctions were lost.

Lysenko's new school of agricultural biology

After a period of critical evaluation the Academy of Agricultural Science was reorganized in 1935. The presidency was taken over by a politician and administrator, the old Bolshevik A.I.Muralov, to secure better practical and political control of research. Vavilov was demoted to vice-president, and Lysenko became one of the academicians. In cooperation with the notorious popularizer and philosopher of science, I. I. Prezent, he now began to propagandize a general biological theory.

The strength of Lysenko's position was demonstrated at the congress for genetics and plant breeding that the Academy organized in December 1936. He emerged from this conference as the leader of an alternative school in opposition to classical genetics. In the eyes of the general public there now existed two schools in biology, both equally scientific.

During the one and a half years that passed between the reorganization of the Academy and this conference Muralov again and again emphasized the practice criterion as the way to judge between the various conflicting theories in the field. Lysenko called for a socialist competition between research institutions to decide who was right, and the call was supported by Muralov and other officials of the Academy. There was also a constant pressure to catch up with the new social and economic order. *"Ne otstavat' ot zhizni!"* ("Don't hang back from life!") was the standing slogan. The implication was that science was lagging behind because of its bourgeois heritage.

One of the first sharp confrontations between Lysenko and the established breeders occurred at an excursion session of the Academy at Lysenko's institute in Odessa in the summer of 1935. He then presented what he claimed to be a new variety of wheat that he had been able to produce in just two and a half years as opposed to the usual 10 to 12 years. This Lysenko (1935) took as a "practical" proof of the correctness of his ideas on breeding and heredity.

Lack of systematic scientific training made Lysenko an easy prey to the scientists' natural love of grand theoretical generalizations. He had a tendency to think that the truth of a consequence derived from a theory implies the truth of the theory. In general I believe that Lysenko's scientific "mistakes" should, at least in his earlier years, be attributed to a poor understanding of the scientific method rather than a conscious attempt to deceive. His "frauds" were simply too naive and obvious to be products of deception based on insight into a proper method and reasoning.

Despite the preoccupation with the practice criterion that characterized the preliminary discussions, the December 1936 conference itself focused on general theoretical, methodological and philosophical questions. (See *Spornye Voprosy* ..., 1937.) True enough, in the first part of the conference a lot of practical experiments and experiences in plant and animal breeding were presented. But the discussion did not succeed in linking this empirical material to the general questions in any precise and discriminating way.

Vavilov and classical genetics were criticised for not being sufficiently dialectical. The conception of heredity as based on material particles which had a high degree of stability and could be studied as independent of the environment, was thought to be idealistic or mechanical. Vavilov, as well as the two other geneticists giving the main lectures, the American H. J. Muller and the Russian A. S. Serebrovskii, were found guilty of this aberration from true dialectical materialism. Such philosophical objections came not only from Lysenko's camp, but also from breeders and geneticists who were no more inclined to believe in Lysenko's biological ideas than were Vavilov, Muller and Serebrovskii.

With respect to practical results Lysenko's camp boasted of achievements that were in reality nothing more than unrealized promises. Konstantinov repeated the criticism of the vernalization of spring wheat with ample documentation. The geneticists, on the other hand, critically examined the practical results of genetics and produced the impression that so far there was very little. In part, at least, this negative evalution was the consequence of a narrow view which did not regard the extensive selection work as being based on classical genetics. Historically the origins of plant selection were closely linked to the creation of basic concepts in genetics, though not to the chromosome theory which conceived the gene as a material particle (Roll-Hansen, 1986).

In addition to this methodological and philosophical bias which blunted the bite of scientific biological argument, Muller played into the hands of the Lysenko camp by including a defense of a socialist eugenics in his talk (Adams, 1989). Eugenics had been officially stamped as a reactionary and fascist ideology and instructions had been given from the top political level that human genetics and eugenics should not be discussed. But Muller broke the taboo and even suggested that the real fascist was Lysenko, since his Lamarckian theory of inheritance would imply that the working class which had for generations lived under poor conditions was really inferior. By this faux pas Muller mobilized progressive socialist fervour on the opposing side. He made it easy for the Lysenko camp to discredit Serebrovskii by drawing attention to his past as an enthusiastic eugenist.

The conference had been organized to "clear up a number of controversial questions, to give the necessary and correct direction to further work on selection and genetics," as president Muralov (1937) said in his introduction. His words indicate a certain weariness with the endless theoretical discussions of the scientists as well as an overestimation of the ability of science to impose instant rationality by marshalling all available evidence. From Vavilov's Institute of Plant Industry, for instance, he expected "exhaustive answers with respect to the accusations that had been raised against their theoretical and practical work". And to give weight to his summons Muralov quoted Stalin's version of the practice criterion of scientific truth:

> "Science is called science because it accepts no fetishes, does not fear to lift the hand against the obsolete and closely listens to the voice of experiments, practice."

This kind of rhetoric was not conducive to a differentiation between the scientific, the technological and the social criteria of practical success.

The official summing up of the conference was carried out by vice president of the Academy G. K. Meister (1937), an older plant breeder from Saratov and party member. His survey emphasized the relevance of the methodological, philosophical and ideological criticism directed at Vavilov, Serebrovsky and Muller. Vavilov had made a rather weak impression, defending his Institute of Plant Industry rather than addressing the problems under discussion, said Meister. And the storm of criticism that the lectures of Serebrovsky and Muller had raised was deserved. By concentrating on the stable gene, neglecting Darwinism, and dragging in eugenics they had done a bad job of explaining and defending genetics. Meister praised Lysenko for the contribution that his theory of stages and techniques of vernalization had made to efficiency in plant breeding. And his work to bring science to the peasants was characterized as a "high example of linking science and practice." Lysenko's experimental work on directed change of heredity through environmental influence Meister treated with kind but firm criticism. He was also sceptical of Lysenko's method for vernalizing seed grain, praising the criticism formulated by Konstantinov.

Not surprisingly, such a sophisticated evaluation that differentiated between the levels of scientific experiment, technological application and ideology, was lost on the general public as well as on the political leadership. Their evaluation took into consideration those arguments that they understood and felt they could master, namely those of a philosophical and ideological nature. The rhetoric linked to the principles of unity of theory and practice, *partiinost'*, and practice criterion of truth gave them confidence that such arguments were highly relevant to the scientific dispute.

The reactions of the minister of agriculture Iakov Iakovlev can be taken as representative of the strengths and weaknesses of the politically committed person with little biological knowledge. He addressed the controversial questions of genetics and selection in a speech to authors and employees of the publishing house for agricultural literature. The theory about stable or unchanging genes is in fundamental contradiction with Darwinism, declared Iakovlev. The distinction between genotype and phenotype, with the genotype being propagated unchanged to new generations, is analogous to the distinction between mind and body. The theory that characteristics that are acquired by the individual during its life cannot be inherited is a symptom of idealism (Iakovlev, 1937: 22—23). The neo-Mendelian theory thus denied the changeability that was essential to Darwinism, and Iakovlev saw few scientific reasons that could explain the success of neo-Mendelism. This success must therefore be political rather than scientific in origin.

Iakovlev pointed to the political use that the fascists made of neo-Mendelian theory, and to the eugenic interest of leading people in Russian genetics, the very people that were so hard set against neo-Lamarckian ideas in general and Lysenko's genetics in particular (pp. 24—25). Iakovlev made no reference to weaknesses in Lysenko's experiments or to the doubtful value of his technological proposals.

During 1937 the Stalinist terror took its toll among Soviet leaders. Party members with independent intellectual stands were among the most exposed groups. When Muralov was arrested Meister took over the presidency of the Academy of Agricultural Science. After Meister had been arrested, the presidency went to Lysenko in February 1938. Taking Iakovlev to be typical of the thinking among the political leadership, it is not surprising that the scientific criticism of Lysenko's work was disregarded. The political leadership was trapped in an ideology that made this criticism appear to be of little significance. One expression of this ideology was an undifferentiated practice criterion that lumped together "practical" success on the scientific, technological and social levels.

Loren Graham in his classical study of *Science and Philosophy in the Soviet Union* tried to disconnect Lysenkoism and Soviet philosophy[4]. How ironical it is, wrote Graham, that the word which is usually associated with "Marxist ideology and science" is "Lysenko", and yet this affair "had less to do with dialectical materialism as Marx, Engels, Plekhanov, and Lenin knew it than any of the other controversies considered in this study. The interpretation advanced by Lysenko arose neither among Marxist biologists nor established Marxist philosophers" (Graham, 1972: 195). It may be correct that Lysenko did not originally derive his ideas from Marxism. But there were many biologists, philosophers and others who did believe that Lamarckism was more compatible with dialectical materialism than was Mendelism. The acceptance and spread of Lysenko's genetic ideas no doubt benefited from such philosophical support.

More important was the role that philosophy played in the failure to understand and take into proper account the criticism that scientists levelled at Lysenko's scientific experiments and practical technological proposals. By neglecting these levels of the discussion, or more precisely, by taking it for granted that Lysenko's basis in these respects was no poorer than that of the opposing "school", the philosophers helped to legitimate Lysenko as a scientist and to concentrate the discussion on the philosophical and ideological level.

The attitude of the philosophers came out very clearly at a new conference on "controversial questions of genetics and selection" organized in October 1939 by the leading Marxist theoretical journal of the country, *Under the Banner of Marxism.* Instead of helping to separate the various levels of the practice criterion the philosophers let them coalesce. It was a sad example of how wrong philosophers can go in their analysis of science when they do not understand and take seriously the subject matter.

The dean of Soviet philosophers, M. B. Mitin (1939), summed up the conference under the heading: "For a progressive Soviet genetic science." Here the philosophical and ideological criticism of classical genetics was wholeheartedly sanctioned, and Lysenko's great practical success taken for granted, while the scientific criticism of Lysenko's work was given little weight. Some of Lysenko's followers were warned against anti-intellectualism. But in general the philosophers' conclusion was clear: Lysenko was basically right, though there were things to be learned from classical genetics. Even mistaken theories can yield useful results, reminded Mitin.

Conclusion: autonomy of science

In a study of the Kol'tsov Institute Mark Adams (1980) has distinguished between science, ideology and social structure. He has demonstrated how important the continuity of social structure is for the continuity of scientific work. As long as certain research groups continued to exist, retaining their personnel, or at least their intellectual traditions, the scientific work was not much affected by ideological pressure. I believe this is an important point when one tries to understand the development of Soviet science. It does not imply, however, that ideology is unimportant for the overall development. For example, changes in social structure, in the personell structure and aims of scientific institutions, will often be motivated by ideology. In the present paper I have tried to show how ideology formed a conceptual framework that helped Lysenko and his school to change the social structure of Soviet biology and agricultural science.The ideology gave leverage to their research policy.

From 1936 onward the scientific community struggled systematically to regain its autonomy and to break down the superficial and misguided understanding of science and its social role that was expressed in the undifferentiated Stalinist criterion of practice. The importance of the analytical distinctions between evaluating scientific experiment, technological application and general social effects made its impact, and gradually science regained much of its autonomy relative to the political institutions. But the struggle was long and hard. Only around 1950 did the tide turn. And only with Kruschchev's fall in 1964 did Lysenko lose his influence. (Cf. Vucinich, 1984.)

My analysis has focused on a set of conceptions linked to the practice criterion of truth of scientific theories. I have tried to show how an illusory belief in the practical usefulness of vernalization came to play a crucial role in Soviet discussion, and how the origin and spreading of this illusion was made easier by the lack of distinctions concerning science and its social roles, for instance, the distinctions between science, technology and their social effects. Neglect of such distinctions is not, however, something which disappeared with Stalinism.

I have mentioned that Lecourt in his analysis of Lysenkoism took the experimental success of vernalization as evidence for its economic success. The presently quite widespread view that there is no essential distinction between basic and applied science or between science and technology is conducive to similar mistakes.

Those schools of modern science studies that conceive the formation of scientific beliefs as "social construction" rather than as discovery of an independently existing reality should look upon the Lysenko affair as a challenge. A main problem of these schools is to explain how "nature herself" enters into the process of scientific research and sifts those theories that are more adequate to the real state of the world from those that are less so. This has been pointed out for instance by Donald Campbell (1986: 115 ff.).

Proponents of strongly relativist views like Harry Collins (1985) or Barry Barnes and David Bloor (1982) reject even the distinction between true and false claims or rational and irrational judgments as a basis for the historical explanation of science. A radical pragmatic idea of "social contruction" is found in some contemporary microsociological studies of science. One example is *Laboratory Life* (1979), B. Latour and S. Woolgars study of a biochemical discovery at the Salk Institute. K. Knorr-Cetina in *The Manufacture of Knowledge* (1981) presents a similar behavioral approach towards science in a more generalized theoretical fashion. In her view not only natural science but also its object, nature, is to be considered as a social construction — at least to the extent that we know it. The "known world is a cultural object, a world identified and embodied in our language and our practices". (Knorr-Cetina, 1983: 136)

But also more moderate social constructivists who reject relativism tend to overlook or play down the importance of distinctions between scientific and general social practice. They are so intent on describing the dependence of science on broader social context, the penetration of science by external social values, that the difference between science and other activities becomes unclear. A central claim in this paper is that unclarity about the difference between scientific and other activities was a major factor in facilitating the rise of Lysenko. There was an inability to differentiate the scientific from other kinds of (social) practice, and therefore the "practise criterion" of truth tended to become broad and blunt.

The contextualist trend in recent history of science stresses that science can only be properly understood when it is considered in a broad [illegible]cal, economic and cultural context. Norton Wise and Crosbie Smith, for instance, want to demonstrate "how major socio-economic transformation can penetrate to the core of a scientific viewpoint" (Wise and Smith, 1986: 147). This contextualism tends toward a pragmatist interpretation of theoretical entities in terms of their technological and economic effects. But it champions a more realist view of science than the strong programme of Barnes and Bloor or the laboratory studies of Latour and Knorr-Cetina. It is also critical of relativism. Tim Lenoir, for instance, emphasizes the role of nature in the formation of scientific beliefs. To Latour and other adherents of the "strong programme of social contructivism" Lenoir puts the rethorical question, "if nature is not the guide and arbiter of historical process, what is?" (Lenoir, 1988: 13)

My study of Lysenkoism differs from this contextualist approach in emphasizing the importance of distinguishing basic science from applied science (technology) and setting them both apart from the rest of society as relatively autonomous entities. Contextualism tends toward a holistic view which makes difficult the attribution of different roles and effects to the different parts of science. While the epistemological holism of people like Kuhn, Hanson, Quine and Feyerabend is incompatible with my analytical approach, it is apparently accepted, for instance, by Lenoir. He seems to have no strong objections to the claim that "theories, their associated observation language, and the entire technical culture they support must be accepted or rejected as wholes". (Lenoir, 1988: 3) In my analysis it is essential that the truth of scientific theories can be judged independently of the technical culture they give rise to.

The integration of science into its wider social context is also strongly emphasized in the recent book by S. Schaffer and S. Shapin on Hobbes and Boyle and the experiments with the air pump. Discussing the dependence of science on the political order they declare that: "Solutions to the problem of knowledge are solutions to the problem of the social order". (Shapin and Schaffer, 1985: 332). They do not discuss the extent to which knowledge solutions are independent and neutral with respect to the political order. For instance, a certain scientific result can be used by different groups for quite contrary political purposes, and the same scientific result can be reached under quite different political regimes. Problematic is also the suggestion that such scientific conflicts are decided by political power: "He who has the most, and the most powerful, allies wins" (p. 342). In the case of Lysenko there seems at least to be a distinction needed. He was superior in political power but ultimately lost the genetics debate.

Shapin and Schaffer hold that the "thruth", "adequacy" or "objectivity" of the claims of the actors are historical products and topics for inquiry but not categories that can be used by the historian in his inquiry and his explanation of the events, and the category of "misunderstanding" should be dispensed with (Shapin and Schaffer, 1985: 12—14). In the Lysenko case it is hardly possible to give an adequate account without taking into consideration his misunderstanding of the scientific method and his insistence on the truth of claims about nature that by normal scientific standards were false. The view of the Soviet scientists that opposed Lysenko was that his program should be resisted and rejected because it was mistaken.

Contrary to relativist and contextualist views that are popular in present studies of science I argue that scientific decisions often have a high degree of independence from politics. Scientific controversies are not generally decided in favor of the side with the most political power. Truth and rationality can be suppressed, but falsehood is not thereby turned into truth. There are genuine scientific ways of deciding when a controversy has been scientifically solved, when it has been abandoned without solution — as often happens, and when it has been solved by illegitimate use of social power. The Soviet solution to the genetics controversy from the 1930's into the 60's was suppression of scientific rationality and truth. This was how the scientific opponents of Lysenko understood their situation. It was also the view of an overwhelming majority in the international scientific community. And after 1964 it became the officially accepted view in the Soviet Union. Can social construction or contextualism provide a different and better explanation?

NOTES

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1 Lysenko's claims in genetics and plant breeding were pseudoscientific in the sense that they were stated as obvious truths without adequate empirical evidence and in contradiction to thoroughly tested biological theories. It is, above all, Lysenko's poor method that makes his claims scientifically unacceptable. Lysenko's early work in plant physiology was not pseudoscientific in this sense. It respected established theory, was more modest in its generalizations and also more conscientions about presenting the empirical evidence for its conclusions. But those methodological weaknesses which later made his gentics pseudoscientific are present in lesser degrees. (Roll-Hansen, 1985)

2 The British plant physiologist Eric Ashby was in Moscow as a diplomat in 1944—45 and tried to obtain information about the status of vernalization as an agricultural method. Direct questions to Soviet scientific colleagues gave only evasive answers. But his definite impression was that the so-called vernalization was now nothing more than a germination test, and that the great campaign for treatment of seed grain had been a big failure. Among the people that Ashby talked to were N. V. Tsitsyn, an earlier collaborator of Lysenko. (Interview with Ashby, 20 May 1983 in Cambridge, England. See also Ashby (1947), e.g., p. 115.) The Soviet plant physiologist Adolf T. Makronosov, corresponding member of the Academy of Science, told me (Moscow, 5 December 1986) that during the years 1936—39 there was a big struggle in agricultural circles about vernalization, with the result that it was completely abandoned. Makronosov experienced this conflict as a boy on a Siberian collective farm where his father was an agronomist.

3 For more biographical information on Vavilov see Joravsky (1965), Adams (1978), Popovsky (1984) and Reznik (1968).

4 A new edition, Science, Philosophy and Human Behaviour in the Soviet Union (New York: Columbia University Press, 1987), contains no important changes in the interpretation and explanation of Lysenkoism.

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