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SOVIET MARXISM AND BIOLOGY*

D. Joravsky

THOSE WHO WOULD WEIGH the influence of ideology in the last three or four decades of Soviet history are trapped in paradoxical difficulties. Ideology is all about them, but they cannot measure it. They can see only the public version of official perceptions and prescriptions; the private version is locked in impenetrable archives, along with independent evidence of the objective reality that must be the ultimate referent in any measurement of ideology. Political, economic, and diplomatic historians are reduced to the sloppy intuition and vague abstraction that are usually the besetting sins of intellectual history, while intellectual historians enjoy the possibility of rigorous and precise work — if they stick to tedious cataloguing of shifts and changes in official views. The historian becomes a nearly blind optometrist with prevaricating patients; he can hear their public readings of the eye chart but he cannot know their private readings, and he cannot measure the accuracy of their vision in any case, for he can hardly see what really is below the large letters that are nearly always read the same way.

These perplexities lend special value to the history of natural science in the Soviet Union. In this field too men labor to understand and master objective reality, but the difference between the public and private versions of their labor, of enormous importance in political and economic affairs, hardly matters. In an important sense the stream of publications is the development of natural science. The same can be said of artistic literature and the humanities, but an objective standard for measuring ideology in those fields is not readily available. In natural science it is. Inhuman nature is the ultimate referent, and the historian needs no inaccessible archives to get a glimpse of it. The published work of scientists contains the closest approximation he can find. Their international fellowship has come close to unanimity in its collective plodding toward the asymptote of objective truth. It is probably wrong to conclude, as many people do, that ideology has been eliminated from natural science; it seems closer to the truth to say that ideology has been reduced to the subconscious level and internationalized, operating now with considerable uniformity on natural scientists of all countries. Thus, when

* Many generalizations offered here require extended argument and documentation, which will be presented in a book now in preparation. The author wishes to thank the National Science Foundation, the American Council of Learned Societies, and Harvard's Russian Research Center, for grants that made his work possible.

Soviet science shows a marked divergence from the international norm, we can seek evidence of specifically Soviet influences.

One must stress the necessity of seeking, and the possibility of finding, solid evidence. Far too often a marked Soviet divergence from the international norm of natural science has prompted outside observers to leap from simplistic assumptions to speculative conclusions. Michurinism, as Lysenko calls his school, is so sharply different from standard international biology that communication between them is virtually impossible. Astounded by this sharp break in the international uniformity of natural science and by the intensity, not to say the ferocity, with which it was effected, many outside observers have leaped to the conclusion that Marxist philosophy has much more vital commitment in biology than in the physical sciences, and that there is a one-to-one correspondence between these commitments and the chief tenets of Lysenko's school. But the historical record does not support these conclusions. Soviet ideology served Lysenko by subverting traditional Marxist philosophy, by proclaiming the supremacy of "practice" over theory.

I

BEFORE THE Russian Revolution Marxists had looked at biology from the outside, as laymen, explicitly abjuring any effort to decide technical problems, interested only in the philosophical and sociological inferences that could be drawn from biology. Is reduction the chief method of natural science, and, if so, does it follow that biological questions are not finally answered until they are reduced to chemical and physical solutions? Are sociological questions similarly reducible, as the social Darwinists claimed, to biological, or even to chemical and physical solutions? Such epistemological issues had an important bearing on Marxist ontology and social theory. If thinking is to be reduced to the physiology of the nervous system and physiology to chemistry, are thought and life deprived of "reality"? If the functional adaptation, the purposefulness, of living things is reducible to the chance results of natural selection — and Marx and Engels hailed this reduction as "the mortal blow to teleology" — what would save sociology from becoming a fatalistic description of processes we have little power to change? This was the sort of problem that aroused the interest of Marx and Engels. Of course, when biologists showed an inclination to take up such questions and answer them in undesirable ways, Marx and Engels showed an inclination to cross the line between science and philosophy, to read the biologists lessons in their own subject. But these occasional adventures in biology were confined to their correspondence and notebooks. The layman's usual

diffidence, his essential lack of interest in the technical problems of biology, is quite evident in Engels' *Anti-Dühring*, the only extensive comment on natural science that Marx or Engels ever published.¹

As the founders passed from the scene toward the end of the nineteenth century, Karl Kautsky came to be regarded by orthodox Marxists, including the Russians, as "the most outstanding theorist of Marxism after Engels."² Perhaps because he started his intellectual development as a social Darwinist, Kautsky was dissatisfied with the usual Marxist attitude toward biology. Though he agreed with the masters that Darwinism leaves off where Marxism begins, the one explaining biological, the other social evolution, he insisted nevertheless that a wall should not be built between the two areas. The growth of human population and its changing balance with the growth of production were biological as well as sociological problems; Marxists must therefore concern themselves with birth control. Moral instincts were determined by biological as well as social evolution, and a comprehensive theory of ethics must take biological as well as social evolution into account. Nor could Marxists afford to ignore eugenics or the racial problem, Kautsky argued, for the massive improvement in education, health, and welfare that would come with the socialist revolution would heighten the significance of hereditary differences among men.³

Most other orthodox Marxists did not share Kautsky's special interest in biology. Plekhanov, to take "the father of Russian Marxism" as an example, defended Darwin's theory of natural selection against Chernyshevsky's attack without deviating from his initial declaration that biological questions need not be considered. He accomplished this feat by strict adherence to the rule that Marxism begins where Darwinism leaves off, that a Marxist need be concerned only with "the philosophy of biology."⁴ Lenin showed almost no concern even for that aspect of biology, though he did get involved in an acrimonious and extended argument over the philosophical interpretation of physics.

At the turn of the century professional biologists focused their attention on the mechanism of heredity. Experimental methods were discovered

1. See D. JORAVSKY, *SOVIET MARXISM AND NATURAL SCIENCE, 1917-1932*, ch. 1 (1961).

2. He was still described this way by the Bolshevik editor, Riazanov, in K. KAUTSKY, *12 SOCHINENIYA [COLLECTED WORKS] VI* (Moscow, 1923). This volume is a Russian translation of *VERMEHRUNG UND ENTWICKLUNG IN NATUR UND GESELLSCHAFT* (Stuttgart, 1910). The Russian edition contains a special preface in which Kautsky gives an illuminating account of the development of his ideas on these subjects.

3. *Idem.*

4. G. V. PLEKHANOV, *IZBRANNYE FILOSOFSKIE PROIZVEDENIYA [Selected Philosophical Writings] IV*, pp. 279 ff. (Moscow, 1958). Cf. *ibid.*, II (1956), pp. 677-678, for Plekhanov's reply to Masaryk's argument that Darwinism contradicts Marxism. Cf. also I, pp. 690-691, and V, pp. 288 ff.

for attacking this long-standing problem in a rigorous way, and modern genetics began the accelerating progress that has now brought it to the threshold of mastery over the reproductive function of living matter, with consequences for agriculture, medicine, and social policy that can be only dimly surmised at present. Fifty or sixty years ago geneticists could only dream of such consequences, especially since they began by denying the possibility of directed changes in heredity. Marxists showed only a slight interest in the new genetics, much less than the interest they showed in the new physics. Physics raised acute epistemological problems, as the new genetics did not. Kautsky, Plekhanov, and other orthodox Marxists occasionally cited De Vries' theory of mutations as confirmation of their conviction that nature does proceed by leaps, proving that revolutions are quite natural. But they showed no concern for the upheaval that De Vries and other pioneers of the new genetics were causing in the theory of natural selection, by their accumulating evidence that the hereditary mechanism of individual organisms cannot respond adaptively to environmental influences. (It is confusing to say that acquired characters cannot be inherited, and simply wrong to say that environment does not influence heredity.) Kautsky came to this problem only in the twenties, when he summed up his weltanschauung in a massive work that provoked scornful polemics from Soviet Marxists. For present purposes we can therefore ignore his contradictory efforts to teach biologists the necessity of some kind of Lamarckism, while still insisting that

historical materialism need not believe that its viewpoint obliges it, in the interpretation of nature, to decide for Lamarck rather than Darwin. This decision can be founded only on the researches and arguments of natural science.

Social laws are to be discovered only through the study of society, natural laws only through the study of nature.⁵

Kautsky's ambivalence stemmed from confusion. He simply did not understand the new genetics.

II

THE REVOLUTION compelled the Russian Marxists to shift their traditional interest in biology. For a time it seemed as if the traditional interest, the implications of biology for Marxism, might be extended and intensified, reaching even to biological determinants of social theory and policy. With Lenin's active support the Academy of Sciences established the Commission

5. K. KAUTSKY, 1 *MATERIALISTISCHE GESCHICHTSAUFFASSUNG* 199 (Berlin, 1927).

for the Study of Russia's Natural Resources, which included a Bureau of Eugenics.⁶ To be sure, this traditional Academy was quite far from Marxism until the end of the twenties, but the rival center of Marxist scholarship, the Communist Academy, also promoted an interest in eugenics during the twenties.⁷ Nevertheless, the implications of biology for Marxist social theory became increasingly dangerous ground. Stalin and his Central Committee were moving toward complete control of Marxist social theory; they wanted no instruction from biologists. The "great break" of 1929-31, by which Stalin established rigid control of intellectual life, put an end to meaningful discussion of the implications of biology for social science and social policy. The line that orthodox Marxists had traditionally drawn between biology and sociology was turned into a wall, with a little door for public health specialists to move through. The questions that Kautsky and a few other Marxists had tried to raise were simply tabooed. Biology had no relevance to social theory or policy — period. Only fascists and racists disagreed.⁸

This turn of events probably caused no great shock to Soviet Marxists, for the Soviet eugenicists of the twenties had been a tiny group bucking a strong tradition of Marxist coolness and hostility toward the drawing of social inferences from biology. But what of the inferences for epistemology and ontology? Strange to say, this traditional Marxist interest produced little more than crude antireligious pamphlets and popular articles even in the twenties, before the great break petrified Soviet philosophy. At the same time the twenties witnessed the birth of a lively new interest, an interest in the implications of Marxism for biology, for the professional issues of biology that Marxists had formerly ignored.

6. See Akademiia Nauk SSSR, Komissia po izucheniiu estestvennykh proizvoditel'nykh sil SSSR, Biuro po evgenike, *Izvestia*, 1922-24. From 1925 to 1929 it was called Biuro po genetike i evgenike. Subsequently eugenics was dropped from its title (and from its interests), and it was turned into the Institute of Genetics, which Lysenko gained control of in the late thirties, and which he controls to this day.

For another publication dealing with eugenics, see *RUSKII EVGENICHESKII ZHURNAL* [Russian Review of Eugenics], 1922-29.

7. See *VESTNIK KOMMUNISTICHESKOI AKADEMII* [Communist Academy Herald], kniga 20 (1927), for a transcript of an extended discussion of eugenics.

8. See, e.g., the works of M. F. Nesturkh, for the official view, following 1930, of the break between biological and cultural evolution. For the sharp attack on eugenics in 1930, see Joravsky, *op. cit.*, *supra* note 1, at 305-7. H. J. Muller was bold enough to defend eugenics in the following period. See, e.g., his *Eugenika na sluzhbe u natsional-sotsialistov* [Eugenics in the Service of the Nazis], *PRIRODA*, no. 1, pp. 100-106 (1934). But S. G. Levit, who was in charge of research on human genetics, stuck fairly close to technical problems. See, e.g., his "Meditsinskaia genetika," *Izvestia*, 1934, no. 163. When the Lysenkoites attacked him and other scientists interested in human genetics, they had to dredge up publications of the twenties and make perverse misreadings of bits and pieces of things published in the thirties.

Soviet medical genetics, psychology, anthropology, and human paleontology have all suffered from the dogmatic line drawn between biology and social science.

The anomaly is not hard to understand, if one bears in mind the situation created by the Russian Revolution. Marxism had become the established ideology of a great state with a large and growing system of higher education and research. The Marxist intellectual became a specialist, giving up the effort to be the universal thinker that Marx, Engels, Plekhanov, and Lenin had tried to be. (Stalin did not establish himself as their successor until the thirties.) Philosophers, carving out their area of special competence, tended to leave problems like the theory of natural selection to scientists. But virtually all of Russia's scientists were "bourgeois specialists." The philosophers, under the leadership of A. M. Deborin, started a campaign to transform them into "red specialists." All scientists were urged to recognize Marxism as a universal philosophy of science. The result was something new in the history of Marxism — groups of professional natural scientists examining their special fields in the light of Marxism. The novelty lay not only in their professional competence — before the Revolution hardly any natural scientists had become deeply involved in Marxism — but even more in the new direction that this professionalism gave the Marxist interest in natural science. Problems of epistemology and ontology tended to be glossed over. Serious attention was given to scientific problems that Engels or Plekhanov had tended to brush aside as merely technical.

The intellectual results were hardly earth-shaking, whether for Marxism or for biology. Between fifty and a hundred biologists were drawn into the discussion groups organized by the Communist Academy and the Timiriazev Institute for the Study and Propaganda of Natural Science from the Viewpoint of Dialectical Materialism (mercifully reduced to Timiriazev Institute or even Timirin in ordinary parlance). The biologists tended to equate Marxist philosophy with the simple mechanistic materialism that most natural scientists take for granted, and they spent most of their time discussing issues then agitating biologists qua biologists. At first most of them were inclined toward the Lamarckist rear-guard action against the new genetics, but by the end of the twenties most had shifted, as had the larger community of biologists both at home and abroad, to acceptance of the new outlook on heredity and evolution. (Soviet Marxists called it Morganism in honor of the American geneticist Thomas Hunt Morgan.) The official school of Marxist philosophy was quite willing to put its seal of approval on Morganism as the realization of Marxist philosophy in biology.

But Stalin's "great break" struck the philosophy of science in 1930, bringing a demand for the dialectical materialist reconstruction of all fields of natural science, for the creation of a natural science that would be sharply different from anything taught by such "bourgeois" professors as Morgan.

At the same time dialectical materialism was frozen in a set of unquestionable though highly ambiguous laws. The supreme law was the priority of "practice" over theory and the "party-character" (*partijnost*) of all knowledge. Since Stalin was the chief of "practice," he alone could attempt any creative development of dialectical materialism. At the same time Marxist biologists were supposed to use dialectical materialism for a radical reconstruction of their subject. In the process they were also required to produce results that would be of practical use to Soviet agriculture, then in the throes of collectivization. It was as if Bossuet, defining a heretic as one who has an opinion, had commanded the scientists in his flock to express opinions—and to produce fertilizer with them.⁹

The new situation was illuminated with distressing clarity at a meeting of the Timiriazev Institute in March, 1931. The institute was to be the center for the dialectical materialist reconstruction of biology, no longer for its "Study and Propaganda from the Viewpoint of Dialectical Materialism." "Without dialectical materialism," one speaker insisted,

it is impossible to reconstruct bourgeois science, and we need precisely the reconstruction of this bourgeois science. We can no longer tolerate the division of Marxists into schools according to their adherence to this or that little group of professors. We need to create a single Marxist-Leninist school in biology. This Marxist-Leninist school, mastering the method of dialectical materialism, must assume leadership of the mass of scientific workers, must really reconstruct the material that is at hand in accordance with our tasks, and must no longer tail along behind bourgeois science.¹⁰

However fraught with significance for the mores of academic life, these thunderous sentiments were, for the content of biology, nothing more than sentiments. Both Lamarckism and Morganism were condemned. Biology was to be reconstructed. But no speaker at this meeting could tell how. All agreed that Engels' fragmentary notes on biology contained the elements of a truly new theory of biology. Marxist biologists must develop those elements into a whole theory, even though all past efforts to interpret and expand on Engels in the light of new biological theories were being condemned as heresy, as "tailing along behind bourgeois science."

It was clear that *magister dixit* was henceforth to be one of the principal

9. See JORAVSKY, *op. cit. supra* note 1, ch. 16, 17, 19.

10. PROTIV MEKHANISTICHESKOGO MATERIALIZMA I MEN'SHEVISTNUIUSHCHEGO IDEALIZMA v BIOLOGII [Against Mechanistic Materialism and Menshevizing Idealism in Biology] 78 (Moscow, 1931).

methods of arguments, but it was also clear that the master had not said enough to settle biological arguments. Consider, for example, the problem of environmental influences on the individual organism's mechanism of heredity. In the thin anthology, *Marx, Engels, Lenin on Biology*, which the Timiriazev Institute published in 1933, only one fragment spoke to this point, Engels' unfinished essay, "The Role of Labor in the Transition from Ape to Man." Or rather, Marxist biologists of the twenties had assumed that it spoke to this point; they had all taken it for granted that Engels had endorsed the inheritance of acquired characters, for which he was applauded by the Lamarckists and gently criticized by the Morganists. Now both groups were harshly chided by Engels' new editors at the Timiriazev Institute:

... Engels, in the article "The Role of Labor in the Transition from Ape to Man," did not pose and did not solve the particular concrete problem of the inheritance of characters; he was not concerned with the problems of the biological "mechanism" of the inheritance of characters. But our revisionists contrived to cram Engels within the framework of the quarrels of two or three schools and theories of contemporary biology concerning the particular problem of heredity and variability.¹¹

The editors' explanation of what Engels *was* talking about — the continuity and discontinuity between biological and social evolution — was too vague to affect the work of biologists.

Thus, dogmatic insistence that the classical masters were the only proper source for biological theory was compatible with diverse lines of research and diverse theoretical conclusions, however deplorable such diversity appeared to ideological officials. To confirm this fact it is not necessary to examine the work of Soviet biological institutes as a whole, though such an examination would show that Soviet biology reached its peak of diverse, creative activity in the first half of the thirties. Within the Timiriazev Institute itself, alongside the inane editors of classical texts, an odd, diverse group of biologists developed their favorite biological theories in the name of Marxism.

Foremost among them were M. S. Navashin and N. P. Krenke. Navashin, a geneticist, tried to enlarge the plant breeder's methods of altering heredity, and at the same time to develop basic genetic theory, by studying the influence of the age of seeds on the rate of mutations. Neither immediate aid to plant breeders nor revolutionary alterations of genetic theory emerged from this work, but its declared aims of aiding agriculture and building

11. MARKS, ENGELS, LENIN O BIOLOGII 175 (Moscow, 1933).

Marxist biology won it the praise of such powerful figures as B. A. Keller.¹²

Keller also praised Krenke's work in plant physiology, which was newer and more interesting. Indeed, of all the work at the Timiriazev Institute it has proved to be the most significant and lasting. The most important of Krenke's many interests was his effort to understand aging and regeneration in plants. He devised statistical methods for correlating the appearance and the age of plants, and at the same time he thoughtfully re-examined such basic concepts as the stages and cycles in the life of plants. He had been doing this kind of quasi-philosophical thinking since the twenties, but in the thirties he began to express his thoughts, at times, in Marxist categories, such as the unity of opposites and development through contradiction. At the same time he tried to satisfy the demand for practicality, e.g., in a search for simple and effective methods by which plant breeders could predict earliness or lateness from a simple examination of the foliage of young plants. Winning the respect of biologists, whether Marxist or not, he was also acclaimed by Marxist-Leninist propagandists until Lysenko achieved hegemony over biology in 1936.¹³

Two other of the Timiriazev Institute's eight divisions merit special notice. The Division of General Biology, under the direction of the Hungarian émigré, E. S. Bauer, had the delicate, not to say dangerous, job of preparing encyclopedia articles and textbooks on general biology. That is, they formulated official views until 1936, when the Lysenkoites took over, simply shoving Bauer's works into oblivion under the label "obsolete."¹⁴ It is indeed a most striking indication of the complete transformation brought by Lysenko to Soviet Marxist efforts in biology that the Timiriazev Institute was dissolved in 1936, and nearly all its efforts to make a Marxist revolution in biology were shoved into oblivion.

The only one of the Timiriazev laboratories to win the enthusiastic support of Lysenko's school was Olga Lepeshinskaia's. An elderly Bolshevik physician, she had been trying to revolutionize histology since the twenties. In primitive, confused pamphlets, published in 1926 and 1928, she had denounced leading histologists and cytologists as positivists, idealists, metaphysicians; she had called for a return — in the name of Marxism — to the tradi-

12. *Starenie zarodyssha kak prichina mutatsii* [The Aging of the Embryo as the Cause of Mutations], SOVETSKAIA BOTANIKA, no. 6, pp. 27-28 (1934). But cf. p. 42 for Keller's criticism of Navashin's theorizing.

13. For a neat summary of Krenke's life and work, see S. Iu. Lipshits, ed., *RUSSKIE BOTANIKI, IV* (Moscow, 1952), pp. 493-496. Cf. the respectful account of ERIC ASHBY, *SCIENTIST IN RUSSIA* 100-106 (London, 1947). Ashby is, however, unaware of the conflict between Krenke and the Lysenkoites. Taking advantage of his untimely death in 1939, they edited his works to bring them into line with Michurinism.

14. Cf. Bauer et al., "Zhizn'," 25 *BOL'SHAIA SOVETSKAIA ENTSIKLOPEDIJA* 404-25 (1st ed., 1932); and BAUER, *TEORETICHESKAIA BIOLOGIJA* (Moscow, 1935).

tion of her teacher, Lesgaft, a prerevolutionary physician who had been a crusader for physical education and higher education for women.¹⁵ At the time her outbursts had been totally ignored, not only by histologists but also by the community of Marxist biologists. In the mid-thirties the intellectual atmosphere became propitious; especially when the Lysenkoites endorsed her, Soviet biologists could no longer ignore her claims, though their foreign colleagues did. She claimed to have overturned Virchow's law, *omnis cellula ex cellula*, by experimental demonstrations that cells could be formed from noncellular material.¹⁶

Outside the Timiriazev Institute there were some other biologists whose claims to have revolutionized their specialties were also endorsed by the Lysenkoites. Of these, Vil'iams' reconstruction of soil science and crop rotation need not detain us, for it had relatively little bearing on the Marxist interpretation of theoretical biology. A. D. Speranskii, a student of Pavlov's, claimed a revolution in pathology by insisting that all diseases were due to disturbances in the "trophic function" of the nervous system. The result was a peculiar Soviet version of psychosomatic medicine, which has excited only derisive astonishment in the international community of pathologists, even though Soviet physicians were forbidden to laugh while Stalin lived.¹⁷

The general pattern is clear. For more than a decade, from the early twenties to the mid-thirties, a great deal of talk about the significance of Marxism in biology had produced little more than the pinning of Marxist labels on a haphazard array of preexistent theories, even after the Stalinist break in Marxism gave rise to loud demands for a revolution in biology. Now came a school of genuine revolutionaries, "scientists from the plow," throwing their support only to methods and theories that were scorned by standard biologists, casting aside with peasant laughter the "*logies*" and "*agogies*" of academic science.¹⁸ What part did Marxism play in this sudden overturn?

15. LEPESHINSKII, VOINSTVUIUSHCHII VITALIZM [Militant Vitalism] (Vologda, 1926) and ZACHEM NUZHNA DIALEKTIKA ESTESTVOISPYTATELIU [Why the Scientist Needs Dialectics] (Moscow, 1928).

16. See L. N. Zhinkin and V. P. Mikhailov, "Novaia kletochnaia teoriia" i ee fakticheskoie obosnovanie [A New Cellular Theory and its Substantiation], USPEKHI SOVREMENNOI BIOLOGII [Successes of Contemporary Biology], no. 2, pp. 228-244 (1955), for a restrained critique — which became possible after Stalin's death — and brief bibliography.

17. See A. D. SPERANSKY, A BASIS FOR THE THEORY OF MEDICINE (N. Y., 1944). The first edition of the Russian original appeared in 1935. For a discussion by Soviet biologists, see ARKHIV BIOLOGICHESKIKH NAUK [Archive of Biological Sciences], no. 2, pp. 116-58 (1937).

18. "*Logies*" and "*agogies*" are sarcastic references to the geneticists' scientific terms. See Lysenko and Prezent, "O 'logiiax', 'agogiiax', i deistvitel'noi nauke," *Pravda*, June 26, 1936.

III

THERE ARE many astonishing features of Lysenko's triumph. Most astonishing of all is the support he has enjoyed from the chiefs of Soviet agriculture for more than thirty years, in spite of the fact that not one of his agronomic proposals has had real merit. However harsh and exaggerated this judgment may seem, it becomes inescapable when one considers the striking inadequacy of his reasoning in support of his proposals, the refusal of agronomists outside the Soviet Union to adopt them, and the quiet abandonment of them by Soviet farmers. But full consideration of this anomaly would take us far from the present subject, into the tangled complexities of Soviet agriculture and politics. Suffice it to say that Lysenko's meretricious reputation as a genius of agronomy was the original and continuing source of the powerful support given him by Soviet leaders.¹⁹ In any event, the ideological element in Lysenko's career is only slightly less astonishing, and ultimately instructive, than the agricultural. Why should the king of Soviet agronomy have extended his realm to theoretical biology? Until the mid-thirties he showed no interest and took no part in the Marxist discussions of biology, yet he swept all before him the moment he entered.

Many have ascribed Lysenko's entry and quick victory to the influence of the philosopher, I. I. Prezent, and there is probably considerable truth in this widespread view. Prezent was the first member of Lysenko's entourage who had been involved in the Marxist discussions since the early twenties. But he was almost unique among the Marxists too: the central, determining fact of his intellectual biography had been — and has continued to be — his Party membership. Almost all the other major figures in the Marxist discussions of biology had some other profession — in medicine or biology — before they became deeply involved with the Party.²⁰ In tracing their intellectual development one can distinguish elements of Party and professional interest. Even in the case of I. I. Agol, who had been a propagandist or journalist before he became involved in the discussions of biology, one notes the rapid growth of a professional commitment: he took advanced training in genetics, did special research in the field, and is indeed remembered as a geneticist. In Prezent's intellectual career one finds nothing but dedicated service to the Party. He joined it at the age of nineteen, when he was beginning his studies at the University of Leningrad. At that time Marxist philosophy in Leningrad was

19. For a case study of this anomaly, see Joravsky, *The Lysenko Affair*, 207 *SCIENTIFIC AMERICAN* 41-49 (1962).

20. Exceptions to this rule are Lepeshinskaia, who became a physician about the same time she became a Bolshevik, and M. L. Levin, who seems to have been a philosopher by profession.

dominated by the Scholarly Society of Marxists, a group of old-fashioned intellectuals, abstract and speculative in their interests, fairly aloof from urgent Party problems, many of them heterodox in their ideological background. Prezent flourished in this as in all subsequent variants of the Soviet Marxist milieu.²¹ When the Deborinites started a crusade to bring Marxist philosophy to natural scientists, Prezent became a crusader, among the predominantly skeptical and indifferent biologists of Leningrad. By 1930 he was one of the leading Deborinites on the biological front, delivering a major speech at an all-Union Congress of Biologists on the harmony between Marxism and Morganism.²² Before the year was over this position was denounced along with the entire Deborinite version of Marxism, and Prezent joined in the vehement denunciations of the views he had just been advocating. Of course he also followed the new line by declaring his hostility to Lamarckism, his respect for standard genetics. But already in 1932, in Prezent's writings as in those of one or two other ideologists, there appeared indications of a basic dissatisfaction with standard genetics, not just with the ideological appraisal of it that he had been repeating after his Deborinite teachers. He had probably seen Lysenko's significance for genetics even before Lysenko did.²³

There is no point in dismissing Prezent as a mindless sloganeer or careerist. In seizing on Lysenko's work as the realization of the Party's new line in biology, he showed an unusual sensitivity to the mood of the Party chiefs. They were growing dissatisfied with the costly work of standard biologists, which was incapable of arresting the precipitous decline in yields that accompanied collectivization. (The Party chiefs had expected collectivization to boost yields.) In a momentous decree of August, 1931, the Party laid it down that plant breeders could no longer take ten to twelve years to produce each new variety; they must do so in three or four years.²⁴ There was no talk of Marxist biology in this decree. There was no talk of genetical theory, only of shock-brigade methods of work for geneticists. Certainly there was no talk of Lysenko's genetics, for he had not ventured into that

21. See I. I. PREZENT, *PROISKHOZHDENIE RECHI I MYSHLENIIA* [Origins of Speech and Thought] (Leningrad, 1928).

22. The speech was not published, perhaps because it was already heterodox by the time the *TRUDY* [Proceedings] of the Congress appeared. But see *PRIRODA*, no. 9, pp. 927-928 (1930), for a brief report of the speech. Th. Dobzhansky was kind enough to go through his correspondence with me. In it we found a letter from Shpet, May 14, 1930, also describing Prezent's speech as Morganist. In an interview, which Prezent was kind enough to grant me, he stated that the speech was published as a pamphlet in the Ukrainian language, but I was unable to find it in the major Soviet libraries.

23. See Prezent, "Predislovie redaktora" [Editor's Foreword] in Iu. A. FILIPCHENKO, *EKSPERIMENTAL'NAIA ZOOLOGIIA* [Experimental Zoology] (Moscow, 1932). See esp. pp. xx-xxi. Cf. also I. I. PREZENT, *TEORIIA DARVINA V SVETE DIALEKTICHESKOGO MATERIALIZMA* [Darwin's Theory in the Light of Dialectical Materialism] (Leningrad, 1932).

24. *Pravda*, August 3, 1931.

field. But all the elements were at hand for the creation of a new school of genetics: the new line in the philosophy of science (the supremacy of "practice"), the crude subjective method that Lysenko had brought into agronomy and plant physiology, the Party's impatience with the expensive slow methods of standard plant breeders. Whether Present was the first to grasp the implicit harmony of these separate elements, we do not know for sure; it is a fact that the fusion occurred when he became Lysenko's collaborator in 1933 or 1934. A genuinely new, uniquely Soviet school of biology was born.²⁵

For present purposes, that is, for weighing the Marxist influence in Soviet biology, it is not necessary to examine the biological theories of the new school. They were clumsy articulations of folk biology, founded on the stubborn conviction that the man who raises crops knows what he is doing. Intrusions of folk biology were not new in the history of scientific biology, though it must be confessed that one must dig deep to find such blatant crudity and self-deception as in Lysenko's explanations of plant growth and reproduction. Nor was it new to argue that this or that biological theory was sanctioned by Marxist philosophy. And it was not new to hobble criticism in advance by calling attention to the approval of Party chiefs, who were the unanswerable judges of successful practice. What was new was the interlocking unity of these three elements, giving the new school a perfectly circular argument that was quite impervious to assault. No one could deny that theory and practice must be united in dialectical unity, with practice enjoying priority over theory. The practicality of Lysenko's agronomy was beyond dispute, for it had been endorsed by the highest Party leaders. It followed inexorably that his biological theories must also be correct. Anyone inclined to doubt that Lysenko and Present were correctly interpreting their Marxist quotations and practical success was referred back to the irrefragable principle that practice has priority in its dialectical unity with theory.²⁶

Marxism, in its new Stalinist version, did not contribute any biological theories to the new school. It contributed the principle that theory is subordinated to practice, and the habit of judging practice by the intensely subjective criteria of the Party chiefs. *Any* biological theory that Lysenko

25. See the first fruit of their collaboration, PRESENT & LYSENKO, *SELEKTSIYA I TEORIYA STADIINOGO RAZVITIYA RASTENII* [Selection and the Theory of Stage Development of Plants] (Moscow, 1935).

26. The controversy precipitated by the pamphlet cited in note 25 lasted only to the end of 1936, when a conference of biologists and agronomists gave the decision to Lysenko. For the transcript, see *SPORNYYE VOPROSY GENETIKI I SELEKTSII* [Controversial Questions of Genetics and Selection] (Moscow, 1937). The attentive reader will see that the opponents of Lysenkoism were already hobbled at this stage of the debate. From 1937 to 1948 they were able to conduct only a feeble rear-guard action against Lysenkoism. From 1948 to 1953 they could say nothing against it.

might put forward was proved right in advance by the badge of practical success that the Party chiefs had pinned on him.

One can argue that such a school, founded on such methods of argument, was fated to approve only crude, primitive biological theories, such as Lepeshinskaia's or Speransky's. Distinguished biologists who joined the new school, e.g., B. A. Keller, could do so only by spoiling their reputation for consistency or sincerity, by writing stuff that flatly contradicted their earlier publications.²⁷ But the fact remains that A. I. Oparin escaped the necessity of choice. He joined the Lysenkoite school without losing his international reputation as a pioneering theorist on the origin of life.²⁸ At first sight the anomaly is easily explained. Oparin was in a different field. He concerned himself with the evolution of chemical compounds up to the emergence of organisms capable of biological reproduction. Beyond that point he was perfectly willing to forsake biochemistry and concede the field to Lysenko's vitalistic genetics. But difficulties arise if one asks why there were no other alliances between the Lysenkoites and reputable scientists working in fields bordering on genetics, no other divisions of spheres of influence. The answer is to be sought both in the psychology of Soviet biologists and in the nature of contemporary biology.

Extremely limited in the possibility of confidential conversation with Soviet biologists, and lacking access to their private papers, one cannot gather direct evidence of their psychological reaction to Lysenko's campaign. But the indirect evidence is quite revealing. Men with Oparin's split personality, who could pursue standard science in their own fields while wholeheartedly applauding Lysenkoism in a neighboring field, have been very rare. To understand this fact it is not necessary to ascribe noble, self-sacrificing sentiments to Soviet biologists — though they have probably had some influence — or to doubt that scientists can divide their minds into unconnecting compartments. (See, for example, the ease with which many reconcile their self-respect as humanists or religious believers with their self-respect as nuclear or biological warriors.) After all, one of the most basic drives among scientists is the lust for fame in the international community of fellow specialists, and the Lysenkoites frustrated that drive by splitting the community. Beginning with the dramatic cancellation of the International Congress of Genetics, which was to have met in Moscow in 1937, Soviet Russian nationalism was a steady

27. Compare B. A. KELLER, *BOTANIKA S OSNOVAMI FIZIOLOGII* [Botany and Fundamental Physiology], 3 vols. (Moscow, 1932-33), esp. vol. III, *GENETIKA*, with his *Genetika i evoliutsiia* [Genetics and Evolution], *SOTSIALISTICHESKAIA REKONSTRUKTSIIA SEL'SKOGO KHOZIAISTVA*, no. 12 (1936) and his *PREOBRAZOVATELI PRIRODY RASTENII* [The Transformers of the Nature of Plants] (Moscow, 1944).

28. For evidence of his international reputation, see his article in *ENCYCLOPEDIA OF THE BIOLOGICAL SCIENCES*, ed. Peter Gray (N.Y., 1961).

theme of the Lysenkoite campaign, offering Soviet scientists psychic compensation for the frustration of their international aspirations. Analogous trends in other fields of intellectual endeavor culminated, just after the second World War, in a burst of fantastic national self-congratulation, just at the time that Lysenko's total power was established in biology. It was a happy coincidence. The foreign scientists' scorn for Lysenkoism was hailed as additional proof of the doctrine's truth; the slightest sign of a Soviet scientist's concern for a foreign reputation was denounced as grovelling before bourgeois pseudoscience. After the death of Stalin in 1953, such extreme nationalism was officially disapproved, and standard biologists began to use Lysenko's separation from the international community of scientists as proof of his error. The Lysenkoites have responded by trying to prove that it is possible to reconcile standard international biology with their doctrine, but they are still far from recognizing the international uniformity of science, and therefore still far from satisfying one of the most basic instincts of scientists.²⁹

But it is not only the scientist's international psyche that resists Oparin's type of compartmentalization. Biology resists it. Even in the thirties it was becoming increasingly difficult for the biologist to do significant work in isolation from genetics. Within the most recent past the wall between biochemistry and genetics has been breached, and Oparin's luck is now turning against him. At a recent discussion in the Academy of Sciences he found himself in a small minority, almost alone with Present, who argued that "the essence of biological phenomena must be biological, there must be a biological method of studying them, and there is no need to address one's self to other sciences."³⁰ To be sure, Oparin did not go this far; he merely insisted that physical and chemical laws (*zakonomernosti*) cease to have "dominant significance" in living organisms.³¹ As the fusion of biochemistry and genetics continues, it will probably become more and more difficult for Oparin to maintain his good standing both as a biochemist and as a Lysenkoite. Bad luck for Oparin, but the exciting new developments in genetics, biochemistry, and information theory promise good luck for the Soviet Marxist philosophy of science. A number of Soviet scientists and philosophers show a readiness to go beyond the bare repetition of Engels' dicta concerning the ontological and epistemological significance of the reduction of life to the chemistry of proteins (or nucleic acids).³²

29. See, for example, N. I. NUZHDIK & T. D. LYSENKO, *ZA MATERIALIZM V BIOLOGII* [For Materialism in Biology] (Moscow, 1957). Compare the attitude of Nuzhdin in his "Gen," 10 *BOL'SHAIA SOVETSKAIA ENTSIKLOPEDIJA* 393-4 (2nd ed., 1952).

30. *VESTNIK AKADEMII NAUK* [Academy of Sciences Herald], no. 10, p. 107 (1962).

31. *Id.* at 105.

32. See, for example, the other speeches reported *loc. cit. supra* note 30.

IV

THE WHEEL SEEMS about to come full circle, back to the implications of biology for Marxism, but on a new level, as the Marxists would say. Marxists began with that interest, but were impelled by the Revolution to concentrate instead on the implications of Marxism for biology. A considerable variety of biological theories was put forward in the name of Marxism, though none were actually derived either from the explicit biological views of Marx and Engels or from the heuristic principles of their philosophy. The Stalinist version of Marxism, by making "practice" supreme over theory, made it possible for the Lysenkoites to cut off discussion. Soviet biologists were made the objects of a great experiment, which has tested the validity not only of Lysenkoite biology but also of the Stalinist formula of theory and practice. That formula has proved inadequate, not merely to outside critics, but to the most authoritative Soviet Marxists as well. On two recent occasions Khrushchev and the Central Committee have explicitly renounced the Party's competence to judge biological questions, tersely recalling the sad results of past efforts to do so.³³ But these disclaimers have not deterred them from repeatedly throwing their support to the Lysenkoites, using still the argument that Lysenko's practical success in agriculture justifies such intervention.³⁴

It would probably be foolish to expect a dramatic repudiation of the Stalinist formula concerning theory and practice. There is, after all, a portion of crude truth in it, along with a rationalization of the supreme power that the Party chiefs show no desire to relinquish. Biologists, agronomists, farmers, and politicians *are* all involved in dialectical unity, that is, in complex interdependence. As the Party chiefs begin to examine that interdependence realistically, asking themselves what measure of autonomy and power each group must have for the speediest improvement of agriculture, they are venturing into most delicate, even explosive issues. They have not permitted serious public discussion of these issues, but one wonders how long they can avoid it. Sound agriculture requires sound biology, and sound biology requires freedom from ideological restraint. Having confined biology within ideology (the supremacy of "practice"), the Party chiefs cannot set it free without confining ideology, or at least refining it. The influence of biology on Soviet Marxism may yet prove to be greater, more enduring, and more beneficial, than the influence of Soviet Marxism on biology.

33. See Khrushchev's speech in *Pravda*, December 25, 1961, and the Central Committee's refusal to intervene in a dispute about cancer, *Pravda*, August 1, 1962.

34. See, most notably, "O merakh po dal'neishemu razvitiu biologicheskoi nauki" [Measures for a Further Development of Biology], *Izvestia*, January 26, 1963.