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A War on Two Fronts: J. B. S. Haldane and the Response to Lysenkoism in Britain

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The reverberations of the so-called Lysenko Affair in the Soviet Union were suprisingly far-flung. Not only did Lysenko's influence extend to Eastern Europe and to China, but also to England, France, India, Latin America, and doubtless to other places as well – the comparative history of Lysenkoism is yet to be written. Nevertheless, it can be asserted with confidence that the English situation was unique; its character is perhaps best exemplified in the person of J. B. S. Haldane, at once a founder of the modern synthesis of classical Darwinism and Mendelian genetics and a leading member of the British Communist Party who served at the height of the Lysenko controversy as chairman of the editorial board of its newspaper.¹

Although there were probably more actual Lysenkoists in India, France, and Brazil, only in England were large numbers of persons forced ultimately to choose between their political and their scientific loyalties. It could not have been otherwise in the country that had produced Darwin and many of the principal architects of Darwinism in its modern form and had also produced so many scientists (for some reason biologists in particular) who turned to socialism and to the Soviet Union as its practical embodiment in the 1920s and 1930s. The particular way in which the crisis over Lysenko worked itself out in Britain is directly related to the reasons why so many of its scientists came originally to identify the cause of science with that of Soviet-style

1. Haldane turned to communism in the late 1930s, although he did not formally join the Communist Party until 1942. Two years later (in 1944), he became a member of its executive committee. He resigned (probably by allowing his membership to lapse) in 1950. Haldane also served as chairman of the editorial board of the London *Daily Worker* from 1940 to about February 1950 and contributed a weekly column, usually on a science-related topic, from 1937 to August 1950. Haldane's role in the party and his contributions to the *Worker* are described in Ronald Clark, *J. B. S.: The Life and Work of J. B. S. Haldane* (London: Hodder and Stoughton, 1968), esp. pp. 132, 159, 166, and 185. See also Haldane's obituary in the *Times* (London) of December 2, 1964.

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socialism. We need to begin, therefore, in the pre-Lysenko period with a brief history of the "social relations of science" movement.²

SCIENCE, SOCIALISM, AND THE SOVIET UNION

Of the many British intellectuals attracted to Soviet Marxism in the twenties and thirties, a remarkable proportion were scientists. More than half the editorial board of the *Modern Quarterly*, perhaps the most distinguished journal of Marxist thought in Britain, were scientists, including such luminaries as Haldane, J. D. Bernal, P. M. S. Blackett, and Joseph Needham. Of course, these scientists were motivated by some of the same concerns that led so many nonscientific intellectuals to embrace Soviet socialism. Yet they also had interests and attitudes that set them apart from – and implicitly at least, in partial opposition to – those of other Marxist intellectuals.

The concerns that dominate the writings of other Marxists of the era appear only in passing in those of the scientists. Their interests were primarily in Marxism as a science, and as it applied to science. It was, they generally believed, the first genuinely scientific theory of history. J. G. Crowther expressed the feelings of many when he asserted, in his 1936 book, *Soviet Science*:

The social philosophy of Western Europe has roots deep in a pre-technological era. The social philosophy of Soviet Russia, dialectical materialism, is founded on modern physical and biological investigations. Natural science is an organic part of Marx's philosophy.

2. Gary Werskey recently published a sympathetic study of the social relations of science movement, *The Visible College: The Collective Biography of British Scientific Socialists of the 1930s* (New York: Holt, Rinehart and Winston, 1978). His research is summarized in Martin Green, "The Visible College in British Science," *Amer. Schol.* 4-7, (1977/78), 105-117. Also relevant, though its focus is largely restricted to the career of J. D. Bernal, is Hilary Rose and Steven Rose, "The Two Bernals: A Marxist Critique of J. D. Bernal and the Social Functions of Science Movement" *Fundamental Scientia*, 001.2 (1981) 267-286. C. P. Snow's "Rutherford and the Cavendish" in *The Baldwin Age* ed. John Raymond (London: Eyre and Spottiswode, 1960), pp. 235-248, remains the most important first-hand account of the movement, even though it is more narrowly focused than the work of Werskey and Green. Snow estimates that a poll of the two hundred brightest physicists under the age of forty in 1936 would have revealed that "about five would have been Communists, ten fellow-travellers, fifty somewhere near the Blackett position [noncommunist, but activist and fairly far left], a hundred passively sympathetic to the Left. The rest would have been politically null, with perhaps five (or possibly six) oddities on the Right" (p. 248).

Consequently, a social system established according to the principles of his philosophy must be founded on technology and science, and the scientific mode of thought must permeate the intellectual activity of its governors.³

Moreover, Marxism's scientificity derived from its dialectical "laws," whose ontological character and relevance to the practice of natural science these scientists took for granted. Some, therefore, tried consciously to guide their research by the precepts of dialectical materialism.

Their major concern, however, was not with Marxism as a guide to scientific practice or to the history of science [even though some of them had been first attracted to Marxism by the historical papers presented by the Russian delegation to the 1931 International Congress on the History of Science and Technology, particularly Boris Hessen's "Social and Economic Roots of Newton's *Principia*."⁴] their concern was with the social relations of science — with the relation of scientists to the mass public, the government, the schools and universities, the other professions, and the culture in general. They believed that culture to be largely ignorant of, when not actually hostile to, the natural scientific enterprise and they aimed to change things by reforming both science and society. In fact, they saw the reconstruction of science and the scientific reconstruction of society as interdependent tasks. Science they wished to rationalize and to redirect, away from war especially; and society they wished to reorganize in accord with scientific principles and in ways that would support further scientific progress. In general, they thought no one more qualified for the task of scientifically reorganizing society than themselves.

Perhaps the most striking example of the scientism that pervades their writings is the final chapter of J. D. Bernal's *The World, the Flesh and the Devil*, published in 1929, six years after he had joined the Communist Party. Bernal describes with equanimity a world divided into a small scientific elite, which does not just advise but which actually constitutes the state, and a mass of humanity unaware even that it is being manipulated. It is a world, in Bernal's words, "transformed into

3. J. G. Crowther, *Soviet Science* (London: Kegan Paul, 1936), p. 14.

4. These papers were published as *Science at the Cross Roads: Papers Presented to the International Congress of the History of Science and Technology held in London from June 20th to July 3rd by the Delegates of the U.S.S.R.* (London: Kniga, 1931).

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a human zoo, a zoo so intelligently managed that its inhabitants are not aware that they are there merely for the purposes of observation and experiment.”⁵

In important ways the attitudes of the scientific Marxists of the 1920s and 1930s more closely resemble those of the Fabians and their scientific predecessors than those of other Marxist intellectuals. They were certainly the heirs of a long tradition of scientific resentment against the preeminent position of men of letters in British culture. Where an earlier generation had looked to Germany for its model of state-science relations, this generation looked to the Soviet Union. As Martin Green (following Gray Werskey) wrote:

What united and energized this group of scientists was the example offered by Communist Russia, an example of monumental planning, of industrialization from scratch, of scientism in education, of nationalized efficiency, of technocracy. Shaw, Wells and the Webbs all made trips to Russia in the 1920s, and the Webbs said that the USSR was a new civilization because of its unparalleled devotion to scientific method and discovery. The Communist Party in Great Britain, as elsewhere, identified communism with the cause of science. John Strachey, in his Marxist phase, spoke of communism as “saving science.”⁶

Haldane, Bernal, Blackett, Needham, Lancelot Hogben, Julian Huxley, and N. W. Pirie (among the more distinguished leaders of the movement) shared with both the Fabians and earlier generations of scientists a belief that scientists deserved higher salaries, better facilities, freedom to pursue the research that *they* thought important, and an expanded role in developing social policy. These improvements, in turn, depended upon a scientifically literate public. Some of the scientists devoted considerable energy to writing books and articles for, and lecturing to, a popular audience. But they saw the heart of the problem as the existing system of education that inculcated its pupils with humanistic rather than scientific values and whose most respected

5. J. D. Bernal, *The World, The Flesh and the Devil: An Enquiry into the Future of the Three Enemies of the Rational Soul* (Bloomington: Indiana University Press, 1969; orig. ed., 1929), pp. 79-80. Bernal also writes: “In a Soviet State (not the state of the present, but one freed from the dangers of capitalist attack) the scientific institutions would in fact gradually become the government” (p. 78).

6. Green, “The Visible College,” p. 114.

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product was the man of letters. Twentieth-century scientists were no more hostile than those of the nineteenth to “the preposterous and disastrous tyranny of ‘classical education,’” and no less determined to replace it with a science-based curriculum.⁷ In 1916 H. G. Wells addressed a Committee on the Neglect of Science chaired by his close friend, the biologist E. Ray Lankester. Advocating a greatly expanded role for science in the schools, Wells said:

I think that we are all of us quite prepared to state what it is that we propose to crowd out because it means crowding out something in order to get that scientific work in. We want the elementary Greek that is done in schools, and which does not go on to a thorough knowledge of Greek at all, taken out of the conception of a general education. [Hear, hear] We also want to see time made for science by the removal of artificial elaborations of the teaching of Latin, such as verse making. A third thing that can very well go is the premature teaching of history to children whose political sense is entirely undeveloped [Hear, hear] That is what we want.⁸

To which Lankester added:

The science which we aim at getting introduced further at the schools, as has been explained by Mr. Wells and others, would be part of the general education in those schools. We do not mean it to be a mere fragment – an extra, as it were. We wish that natural science shall be the staple subject of school examination, and that classical languages shall be additional to natural science, not natural science additional to classical languages.⁹

The demands voiced by Haldane, Hogben, and Bernal are remarkably similar to those presented earlier by Lankester, T. H. Huxley, Faraday, Whewell, and Tyndall. The hope that science would replace the humanities as the core of British culture remained the same for at least three

7. E. Ray Lankester, in *Science and Education: A Collection of Lectures Delivered Before the Royal Institution in 1854* (London: William Heinemann, 1917; orig. ed., 1855), p. 6.

8. *The Neglect of Science: Report of Proceedings at a Conference held in the Rooms of the Linnean Society, Burlington House, 3 May 1916* (London: Harrison, 1916), p. 24.

9. *Ibid.*, p. 33.

generations of scientists; what changed was the appearance of the Soviet Union as a model with which British society could be contrasted. When Bernal wrote in the first issue of the *Modern Quarterly* that "the present situation, where a highly developed science stands almost isolated from the traditional literary culture, is altogether anomalous and cannot last. No culture can stand indefinitely apart from the dominating practical ideas of the time, without degenerating into pedantic futility," he was only voicing, in relatively moderate terms, sentiments expressed by many of his scientific predecessors.¹⁰

Frustration had led some otherwise conservative scientists to support a vigorous state role in promoting scientific interests, reforming education, building scientific facilities, and funding research. The enthusiasm of so many scientists (not all of them Marxists) for the Soviet Union has to be seen in the context of a tradition in which scientists had long looked to the state as a potential ally in their struggle against the schools, universities, and other institutions they thought responsible for the neglect of science in Britain.

On the other hand, most scientists including those who were highly sympathetic to the Soviet experiment, worried about the possible limits on their freedom that might accompany a greatly expanded state role. The extent to which this was a concern is indicated by the number and fervor of Bernal's assurances (most notably in the influential *Social Function of Science*) that communists were as solicitous as anyone else of scientific freedom. "Any measures aimed at giving greater assistance and scope to research must be balanced against the possible risks of restricting its freedom or limiting its imaginative possibilities" is a typical passage from the Bernal of the 1930s.¹¹

Even if scientists remained concerned, the benefits that socialism would bring to science appeared immediate and real, the risks distant and theoretical. During the 1920s the Soviet government had substantially enlarged the resources available to science and to science education without major interference with those freedoms cherished by Western scientists. British scientists were frequent visitors to the Soviet Union and although few stayed for an appreciable length of time or spoke the language (in contrast to the American scientific Marxists of the same period), they returned with rapturous reports of the state

10. J. D. Bernal, "The Social Function of Science," *Mod. Quart.* 1 (1938), 15-22; quotation on p. 18.

11. J. D. Bernal, *The Social Function of Science* (London: Routledge, 1939), p. 261.

of science in the USSR.¹² With the advent of the Great Depression, the potential and highly theoretical costs of state action faded in comparison with its immediate and certain benefits.

It was not until the large scale break of 1929 to 1933, with its call for elimination of "bourgeois specialists," that the costs of the close links between science and the state first became apparent. About three years after this campaign, the Great Purges began. Ironically, two of their most prominent victims were Boris Hessen and Nikolai Bukharin, whose work had done much to attract British scientists to Marxism. Indeed, nearly all of the Soviet delegates to the Second International Congress of the History of Science and Technology lost their lives in the terror.

In biology, specifically, the situation began seriously to deteriorate about 1935. The events of this period, culminating in the official condemnation of genetics in 1948, have been chronicled in detail by David Joravsky and Zhores Medvedev; here it is necessary only to recount the most significant, and to indicate how these events were perceived (or failed to be perceived) by British scientific Marxists.¹³ The British, Marxists and non-Marxists alike, viewed these events not as they appeared to the participants, but through the filter of intermittent and often unreliable reports transmitted to an almost entirely non-Russian-speaking audience. And for the scientific Marxists, there was usually a further filtering as reports were selectively perceived and interpreted in the light of preconceptions about the nature of Soviet society, preconceptions with which these reports were at such variance as to present a stark choice between the conclusion that views deeply held and acted upon were fundamentally false or that the reports were in large part fabricated by enemies of the Soviet Union. Most chose the latter conclusion; and hence there was no real crisis among British

12. I know of nine American biologists who worked in the Soviet Union during the 1930s (H. J. Muller, Calvin Bridges, Percy Dawson, Daniel Raffel, Carlos Offerman, Sidney Halperin, Mark Graubard, Horseley Gannt, and Bronson Price); I believe all but Dawson were geneticists. Perhaps their having actually lived in the Soviet Union accounts for the considerably more critical attitude of American scientific socialists toward Stalinism in general, and toward Lysenkoism in particular.

13. David Joravsky, *The Lysenko Affair* (Cambridge, Mass.: Harvard University Press, 1970); Zhores A. Medvedev, *The Rise and Fall of T. D. Lysenko* (New York: Columbia University Press, 1969). For a shorter history see the chapter "Genetics" in Loren R. Graham, *Science and Philosophy in the Soviet Union* (New York: Alfred A. Knopf, 1972).

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scientific Marxists until 1948, when the Soviets themselves officially adopted as policy views hitherto characterized as slanders on Soviet society.

However, before considering the crisis generated in 1948, let us briefly consider the situation in biology as it actually developed in the Soviet Union and as it was perceived by the scientific socialists in Britain between about 1935 and the famous meeting of the Academy of Agricultural Sciences in the summer of 1948 at which genetics was formally condemned.

LYSENKOISM FROM 1935 TO 1948

By the mid-1930s Lysenko had won substantial support within the Soviet bureaucracy, his increasing influence signaled by A. I. Muralov's replacement of Nikolai Vavilov as president of the Academy of Agricultural Sciences. In 1938 Lysenko himself assumed the presidency. Two years later Vavilov was arrested and charged (in secret) with a variety of political crimes including sabotage and spying for England.¹⁴

The Seventh International Congress of Genetics had been scheduled for Moscow in 1937. Because of the unsettled situation in biology, the Soviets postponed the congress. Hoping to resolve the issues before a world meeting took place, the Soviets held a conference of their own in December 1936. By this time the issues had broadened beyond the purely scientific; in particular, Lysenko had begun to link his campaign for a change in agricultural practice to the charge that classical genetics was a "bourgeois science." Not surprisingly, the conference did not resolve any of the central issues in dispute. Hence, when the congress finally took place in Edinburgh two years later, Soviet biologists were barred by their government from attending (both Lysenkoists and orthodox geneticists had been invited by the organizers). The Soviets made a second attempt to achieve consensus in 1939; this conference adopted a final report tilted strongly toward the Lysenkoist positions. Thus the situation remained — with orthodox geneticists allowed to continue teaching and publishing (except in the area of human genetics), while Lysenkoists gained strength in the scientific academies and in secondary and agricultural schools — until the summer of 1948.

The year of the first Moscow conference also witnessed the beginning of the purges and, specifically, the arrests of three prominent biologists

14. My summary of events within the Soviet Union largely follows the account of Joravsky.

or theorists of biology: Israel Agol, Max Levin, and Solomon Levit. (Joravsky estimates that at least twenty-two geneticists or philosophical defenders of genetics were victims of the terror. If one adds to those groups non-Lysenkoist biologists and agricultural specialists, a total of seventy-seven – about 5 percent of the total – appear to have suffered repression during this period).¹⁵ Levit headed the Institute of Medical Genetics, the largest institute in the Soviet Union that was working on problems of human heredity. In 1936 both Levit and his institute were accused of fostering racism and fascism – a prelude to Levit's arrest and the disbanding of the institute. Although research in human genetics was not officially banned until 1948, the public association of all work on human heredity (including medical genetics) with racism and fascism had the same effect. With Levit's arrest research on problems of human heredity came to a virtual halt.

How much impact did the deteriorating situation in biology have on British scientific Marxists? The answer is, very little. The Communist Party, and communist writers in general, took virtually no account of the situation unfolding in the USSR. The events just described scarcely affected their "line" on science, which increasingly diverged from the direction in which the Soviets were heading. Until 1948, the Communist Party in Britain never wavered from its position that communism was not only compatible with scientific freedom (understood in the conventional sense), but the only system that could ensure it. Communist writers of the twenties and the thirties recognized that many scientists, including those on the left, had doubts about the compatibility of central planning and the freedom of scientific workers to pursue research of their own choosing. They replied that the freedom of scientific workers in Britain was more theoretical than real, given the paucity of opportunities to pursue research at all (in 1939 British expenditures on science were approximately 0.1 percent of the GNP) and the fact that the kinds of opportunities available were dictated in large part by the requirements of profit and production for war.¹⁶ But they almost never presented the choice as between a science controlled in the interests of monopoly capital and a science controlled in the interests of public good. Rather, they insisted that the progress of science depended upon genuine freedom of research. The ideal, Bernal wrote, was "to

15. Joravsky, *The Lysenko Affair*, p. 116.

16. Figures for science expenditure as a percentage of the GNP in the United States, Britain, and the Soviet Union may be found in Rose and Rose, "The Two Bernals."

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allow the same freedom to the scientist of staking his claim as still belongs to the prospector."¹⁷ And he maintained: "The primary condition is that any research for which there was a demand from inside or outside science should be not only permitted, but also aided. Though this might seem likely to lead to great wastage of time and money, that waste would be only apparent, for one or two really fruitful advances made in this way would more than compensate for the wasted work of a dozen others."¹⁸ The possibility of the state's direct intervention to settle a scientific controversy, a dispute over *theory*, was not even contemplated.

The attacks on genetics in the Soviet Union were generally ignored or characterized as a sign of healthy controversy. In the few instances where repression was admitted, its degree was said to be wildly exaggerated in the Western press, with the state of genetics in Britain asserted to be considerably more depressed. Haldane's statement is an example: "In view of the decreasing support given to this branch of biology in England, it is probable that, in spite of the dismissal of several Russian workers during the last year [1939], the prospects for genetical research are considerably better in the Soviet Union than in the British Empire."¹⁹ Joseph Needham made several similar statements during the late 1930s and early 1940s.²⁰

17. Bernal, *The Social Function of Science*, p. 277.

18. *Ibid.*, pp. 227-228. He also asserted that "the very essence of science is the spirit of free inquiry" (p. 470) and that "at all costs science must be prevented from becoming a hierarchic orthodoxy; it must be able and willing to defend its theses against all comers, and it should not exclude but encourage critics of all kinds however unbalanced or irrational they may appear to be" (p. 278). The 1947 statement of the Association of Scientific Workers, whose president was P. M. S. Blackett, drew heavily on *The Social Function of Science*: "It has never been part of the policy of the Association either to restrict the freedom of the individual scientist in any way or to suggest any limitation to the development of fundamental science either absolutely or relatively to that of applied science" (from the statement "Freedom and Organization in Science," 1947). Michael Polanyi perceptively noted at the time that Bernal was "trying to win the support also of non-socialists, mainly by emphasizing that no restriction of the freedom of science is intended." Michael Polanyi, "Rights and Duties of Science," *Manchester School of Economic and Social Studies* (October 1939), 175-193; quotation on p. 175.

19. J. B. S. Haldane, "A Note on Genetics in the U.S.S.R.," *Mod. Quart.*, 1 (1938), 393-394; quotation on p. 394.

20. For example, in "Genetics in the U.S.S.R.," *Mod. Quart.*, 1 (1938), 370-371 (signed "Helix" and "Helianthus"); "Biological Research in the Soviet Union," in *Science in Soviet Russia*, ed. Joseph Needham and Jane Sykes Davis

In general, the scientific socialists ignored most of the reports published in the Western press and related by visitors returned from the Soviet Union or, where such reports could not be entirely discounted, assumed that they grossly distorted the reality. This was not difficult, given their general ignorance of the Russian language and lack of prolonged contact with the country, along with the existence of genuine evidence for the continued teaching of, and research in, orthodox genetics during this period. In their failure to see clearly the situation in the Soviet Union, the scientific socialists were no different than many of their nonscientific colleagues or, more significantly, than those who minimized or disdained the significance of other equally well documented facts. Selective exposure and perception are distressingly common frailties. Many socialists managed not to see what, to others at the time and to everyone with hindsight, was obviously *there*; but they were hardly alone in this kind of self-deception. To cite only the most obvious example: there was also general reluctance to accept the numerous well-documented reports of the destruction of European Jewry during roughly this same decade.²¹ With this proviso in mind, let us turn to the specific factors that led so many scientific socialists of the 1930s and 1940s to ignore some events and to interpret those that could not be ignored in such a way as to make them consistent with an idealized conception of Soviet society.

The familiarity of the scientific socialists with the Russian language and with Russian society was remarkably slight. Unlike their American counterparts – some of whom went to help “build socialism” in the 1930s, encountered the terror of 1936-1939, and returned disillusioned (with the Soviet Union, not necessarily with Marxism) – the British had no real experience of life in the USSR. Like the Webbs and other Fabians, they made visits of at most a few days or weeks. Their Soviet Union was an imaginary country, one reflecting their own most ardent hopes but little of a reality with which they had virtually no contact.

Moreover, it was possible to point to evidence of genuine genetics teaching and research in the Soviet Union. Much is made in communist writings of this period of the fact that standard textbooks were used in

(London: Watts, 1942), pp. 24-28; and “Biological Science in the U.S.S.R.,” *Nature*, 148 (1941), 362-363. It should be noted, however, that Needham consistently expressed skepticism of Lysenko’s scientific claims.

21. See Walter Laqueur, *The Terrible Secret: An Investigation into the Suppression of Information about Hitler’s ‘Final Solution’* (London: Weidenfeld and Nicolson, 1980).

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university courses and that conventional articles continued to appear in Soviet journals. Indeed, many nonsympathizers were impressed by evidence for the continued vitality of genetics in the Soviet Union and characterized reports in the popular press as unduly alarmist.²² These judgments reflect a reasonable (if overoptimistic) assessment of the situation after 1936, in which Lysenkoists and geneticists struggled for control of the universities, scientific institutes, agricultural schools, and journals, with each group entrenched in certain areas. David Joravsky has summarized the situation as follows:

In short, there was a compromised deadlock, with science entrenched in academic institutions of higher learning and aggressive Lysenkoism trying to expand from its agricultural base. The long run seemed to favor the Lysenkoites, for biological education was severely mutilated. In secondary schools it became a mixture of natural history, old-fashioned Darwinism, and meaningless chatter about Michurinism. In higher education everything depended on the balance of forces at particular institutions, with Lysenkoites tending to win in the agricultural sector and scientists tending to hold their own in the academic.²³

If there was indeed evidence for the continued vitality of genetics in the Soviet Union, there was also evidence of serious problems – one of which was the suppression of human genetics after 1936. I know of not a single instance where this suppression was recognized. I am inclined to believe that the facts were not known, not through a willful refusal to know (since critics of Soviet society do not seem to have been aware of this situation either), but because British scientists did not – in fact, could not – follow events in a way that would have alerted them to an informal change in policy. Once again, their lack of Russian probably saved them from learning unpleasant facts. In particular, it is hard to believe that Haldane, who as we shall see never wavered in his commitment not only to human genetics but to eugenics, would have joined the Communist Party in 1942 had he believed that research on problems of human heredity had been halted.

Other events were official and public, the focus of much critical

22. For example, Eric Ashby, *Scientist in Russia* (London: Penguin, 1947) and P. S. Hudson and R. H. Richens, *The New Genetics in the Soviet Union* (Cambridge: Imperial Bureau of Plant Breeding and Genetics, 1946).

23. Joravsky, *The Lysenko Affair*, pp. 110-111.

attention, and hence necessarily known to the scientific socialists. The cancellation of the genetics congress scheduled for 1937, the refusal of the Soviets to allow their scientists to attend the 1939 congress (in spite of a plea directed to Maisky, the Soviet ambassador to Britain, signed by eight prominent geneticists including Haldane), the forcible repatriation of Peter Kapitsa, the unexplained disappearances of several prominent scientists including Vavilov, and the purge trials (especially that of Bukharin), were hardly events that scientists could ignore. And as far as I can determine, none of the prominent scientific socialists believed that Hessen or Bukharin was guilty of political crimes. They must, then, have had doubts about the truth of the charges in other cases as well.

They also realized that Vavilov, personally known to many of them, was at the very least in grave difficulties; after about 1945, it was hard to escape the conclusion that he was dead. It was perhaps reasonable to discount the reports of his death that began circulating shortly after he dropped from sight in 1940, since earlier such reports had brought indignant denials from Vavilov himself. But in response to rumors surrounding his disappearance, the Royal Society had elected Vavilov to foreign membership in 1942, hoping that the attendant publicity would save his life. The election was never acknowledged and the Royal Society made repeated efforts over a period of years, through both formal and informal channels, to ascertain his fate. No reply was ever made to any of the inquiries addressed to Soviet officials, who in 1945 eliminated Vavilov's name from the list of living and recently deceased members of the Academy of Sciences.

Hence by about 1945 the facts of Vavilov's arrest and subsequent death, though not the specific circumstances under which they occurred, were known to the British scientific community. Vavilov's disappearance was naturally a source of acute embarrassment, and apparently genuine concern, to the communists among them. Whatever they may have thought privately about the nature and circumstances of Vavilov's disappearance, the scientific Marxists did not acknowledge it publicly until 1948, when the endorsement of Lysenko's views by the Central Committee of the Communist Party focused tremendous publicity on the situation in Soviet genetics and thereby resurrected the issue of Vavilov's death. In November 1948 Haldane agreed to participate in a BBC "debate" (all four participants recorded their contributions separately) on the Lysenko controversy. He would certainly have expected Vavilov's disappearance to be made an issue (it was). At the end of his speech Haldane told his audience: "You may

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have been told that Vavilov, a famous Russian plant breeder, died in prison. His research station outside Leningrad became a battlefield in 1941, and according to a very anti-Lysenko article in the *Journal of Heredity* he appears to have died at Magadan in the Arctic in 1942 while breeding frost-resistant plants.”²⁴

Haldane must have assumed – apparently with reason, since no one at the time noticed his legerdemain – that only a few members of his radio audience read the *Journal of Heredity* (published in the United States) and that those who did would not identify the article cited. The only persons whose suspicions were likely to be aroused by Haldane’s remarks (scientists such as Theodosius Dobzhansky, author of the article, or H. J. Muller) were in America and did not hear the broadcast. It is worth quoting in full the passage in Dobzhansky’s article from which Haldane claimed to have obtained his information:

After the 1939 Genetics Conference, a shroud of silence envelops Vavilov. The closing chapter can be reconstructed only from unofficial, fragmentary, but apparently reliable information. Vavilov was arrested, probably in 1940. Part of the time during the winter 1941-1942, he was a prisoner in a concentration camp at Saratov (ironically, it was at the University of Saratov that he held his first post under the new revolutionary regime), and whence he was transported to Siberia. His destination was Magadan, on the Sea of Okhotsk, the capital of a rich gold-bearing region, but a place of sinister reputation, because of its deadly climate and even worse because it was built and operated by forced labor. According to some information, Vavilov was put to work on breeding varieties of vegetables capable of growing in Magadan’s climate, but this information is not certain. The release, through death, probably came in late 1942. No mention of N. I. Vavilov’s name can be found in the list of living and recently deceased members published by the Academy of Sciences of the USSR in connection with its 200-years jubilee celebrated in 1945.²⁵

It is obvious that Haldane knew, at the time he denied it, that Vavilov had been arrested and had died in custody. He would also have been

24. J. B. S. Haldane, contribution to “The Lysenko Controversy: four scientists give their points of view”, *Listener*, 30 (1948), 873-875; quotation on p. 875.

25. Theodosius Dobzhansky, “N. I. Vavilov, A Martyr of Genetics, 1887-1942,” *J. Hered.*, 38 (1947), 227-232; quotation on p. 232.

aware of the arrests, trials, and sometimes unexplained disappearances of other scientists and historians and philosophers of science. But if Haldane and the other scientific socialists were troubled by these occurrences, they also believed them to be exceptional. That there were labor camps they must have known; the character and extent of these installations they certainly did not. And what they did know they would not publicly admit, for fear of aiding the forces of reaction. The Soviet Union, for all its problematic aspects, remained for them the one hope for the world and, in particular, the one genuine counterweight to fascism. In the context of the world situation of the time, criticism of the Soviet Union appeared a luxury that could not be afforded.

H. J. Muller was probably the foreign scientist most aware of the true situation in the Soviet Union, having lived in the Soviet Union from 1933 to 1937 and having worked closely with a number of the geneticists who became targets of Lysenkoist attack. Disillusioned by his experiences, Muller managed to leave the Soviet Union by volunteering to serve with a medical unit in Spain. Ultimately, he was to become one of the most vigorous of Lysenko's critics in the West, but in the late 1930s even he did not want to criticize the Soviets publicly. This reluctance reflected in part his anxiety about possible consequences for his Soviet colleagues, most of whom (particularly Vavilov) were already in precarious positions. It reflected in part his desire not to be seen as an enemy of the Soviet Union, who would then lose all chance of influencing progressive scientists. But it also reflected his genuine desire not to *be* an enemy of the Soviet Union. In a number of letters of that period, especially to his closest confidant, Julian Huxley, he agonized over the correct course of action. In March 1937 he confided:

I have been asked to write private letters to my geneticist friends abroad, telling them that things are going well again for genetics in U.S.S.R. & asking them to use their influence with the internat'l committee, to have the congress held there . . . While I will not do that, neither will I do the opposite — tell the truth to the world about the situation there. It would be too damaging to the opinion of scientists about the U.S.S.R.

[I do not want to become an agent of anti-Soviet propaganda. While what I have told you are only facts, they cannot be appraised without taking them in connection with *favorable* facts concerning the U.S.S.R. and its system. I know you are familiar with these, & so I

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can tell you the above facts, but the mass of people can hardly see two facts at a time & so these facts might have a dangerous effect on them. When they are finally given out it must be in just the right setting].

At least, while I work for them, I am bound not to do that, for it would be regarded as traitorous. It could, moreover, be traced. So I must deliver all this to you . . . with the understanding that it is not to become in any sense public information. Haldane, especially, must *not* be informed – not now, anyway – for I judge from the tone & content of his letters to me that he is at present having his political opinions impressed upon him with a rubber stamp (greatly as I admire his intellect and person), and could be influenced in the reverse direction from that which I intended. He would think I had gone over to the conservative or Fascist camp, which is the very impression I am trying to disprove.²⁶

Muller never did try to influence Haldane's thinking; by the time Muller felt able to criticize Soviet policy to other than very close friends, Haldane had become deeply and publicly committed to the Soviet cause. Whether Muller's intervention in the late thirties would have made any difference is impossible to say. It does seem reasonable to assume that Haldane would not have joined the Communist Party in 1942 if he had had any real understanding of the situation in the Soviet Union in general and in genetics in particular. Indeed, had Haldane been aware of the precarious position of genetics, he would not have been taken by surprise – as his correspondence clearly indicates he was – by the events of August 1948.

It seems likely that even the party leadership was not fully aware of the situation in science. Otherwise, how can one explain their promotion of a line that had to be precipitously abandoned after the conference of August 1948? As we saw earlier, charges of unwarranted state intervention in Soviet science had consistently been met by the British Communist Party with countercharges about the situation in Britain, rather than with a defense citing a legitimate state role in the settling of scientific disputes. Until 1948 the party never wavered from its position that there existed a single world science, that the state

26. H. J. Muller to Julian Huxley, March 9, 1937 (Muller collection). Material in brackets is an addendum to the preceding paragraph and appears at the end of the letter.

ought not to meddle in it, that reports of such meddling by the Soviet state were fabricated — or at least greatly exaggerated — by elements hostile to it, and that scientific freedom was seriously threatened in Britain, as it would be in any society where science served the interests of monopoly capital. The situation in the Soviet Union might not be perfect, party spokesmen occasionally conceded, but the condition of science at home was far worse.

The party's consistent and frequent reiterations of this theme would return to haunt it in 1948, when genetics was formally condemned as a bourgeois science, the research institutes dissolved, their staffs dismissed, standard textbooks removed from the schools, and further publication in genetics banned. For while the party had always insisted that communism could only enhance scientific freedom, it now was obliged to maintain that scientific freedom was no more than a bourgeois ideal.

Such a dramatic reversal of long-standing policy was not accomplished easily. Let us turn our attention to the struggle that ensued, within the party and between the party and its external critics, when the situation in Soviet science could no longer be misinterpreted and the policy of the British Communist Party had to be brought into conformity with that of the Soviet Union.

THE PARTY AND THE PUBLIC: 1948-1950

The struggle between the party and its critics in Britain was very largely a struggle over which issues would be joined. In this contest Lysenko's critics focused on his nonscientific claims, particularly his denial of the universality of science, and the methods by which his views had triumphed, while his defenders emphasized the substance of his scientific claims. This may appear odd, given the bizarre character of some of Lysenko's scientific views; but it was possible to present them in such a way as to seem reasonable to many non-Marxists. And the focus on Lysenko's scientific methods served to draw attention from the other aspects of the issue which, in light of the party's frequently reiterated commitment to the ideals of world science and virtually unrestricted freedom for scientists, were acutely embarrassing.

Given its past history, which in turn reflected the party's understanding of what would appeal to scientists, the rational course after 1948 was evasion whenever possible. Its insistence that communism would enlarge, rather than restrict, scientific freedom was the *sine qua non* of its campaign to attract scientists to Soviet-style socialism.

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Although Bernal tried to put the best face on it, arguing that the policies adopted in August 1948 were required by a society that took its science seriously, few persons outside or within the party – and probably no scientists – were attracted to the position that the state should play a direct role in settling scientific controversies. In fact, the only person who could be found in full support of this view was the non-Marxist George Bernard Shaw. In a widely advertised and much-quoted article in the *Labour Monthly*, Shaw insisted that the “determinism” inherent in Darwinism “is a doctrine that no State can tolerate, least of all a Socialist State, in which every citizen shall aim at altering circumstances for the better purposely and conscientiously.”²⁷ Commenting on the resignation of Sir Henry Dale from the Soviet Academy of Sciences in protest of the decrees of August 1948, he remarked that the real issue

is between the claim of the scientific professions to be exempted from all legal restraint in the pursuit of knowledge, and the duty of the State to control it in the general interest as it controls all other pursuits. To my old question “May you boil your mother to ascertain at what temperature a mature woman will die?” the police have a decisive counter in the gallows. To Lysenko’s question “Can the State tolerate a doctrine that makes every citizen the irresponsible agent of inevitable Natural Selection?” the reply is a short No. The Yes implied by Sir Henry Dale’s resignation is a hangover from the faith of Adam Smith, who believed that God interferes continually in human affairs, overruling them to a divine purpose no matter how selfishly they are conducted by their human agents. Experience has not borne this faith out. *Laissez-faire* is dead. Sir Henry should think this out.²⁸

The glee with which so many of Lysenko’s critics read Shaw’s contribution to the debate is evidence for the rationality of the party’s approach of “the-less-said-the-better.” Its strategy of silence was repeatedly attacked by the party’s critics, who maintained that the issue was not the validity of Lysenko’s scientific claims, but the ways in which they were imposed and the “two-camps” philosophy that accompanied and justified them. Julian Huxley, in his influential book *Soviet Genetics and World Science*, asserted that “it is subsidiary

27. George Bernard Shaw, “The Lysenko Muddle,” *Labour Monthly*, 31 (1949), 18-20; quotation on p. 18.

28. *Ibid.*, p. 20.

whether or not Lysenko's claims to have made certain new discoveries are substantiated and whether his theories are partly or wholly sound."²⁹ John Langdon-Davies, in an oft-quoted book that was primarily a critique of Haldane's role, insisted: "What Lysenko says about the changing of heredity is not the essential thing. The essential thing is the relationship between the State and scientific research."³⁰ But these and many similar pleas, provocations, and taunts could not move the party or its apologists to speak to those issues. It stuck doggedly to the subject of heredity.

In addition to the desire to avoid nonscientific subjects that were unpopular and only served to call attention to the party's abrupt reversals, there was a positive element in its emphasis on Lysenko's scientific claims. The most obvious is that they were difficult for the public to evaluate. When Haldane described experiments ostensibly supportive of Lysenko's claims, who were the readers of the *Daily Worker*, or those who listened to the BBC's broadcast on Lysenko, to say otherwise? When he invoked the work of Gregory on vernalization, Gustaffson on mutations, or Daniel and L'Heritier on grafting as experimental confirmations of some of Lysenko's theories, he sounded convincing even though few of those he cited could have been pleased with the use he made of their work (Gustaffson was probably the most impassioned of Lysenko's critics in Sweden, and Gregory wrote to Haldane protesting that what his work on vernalization really showed was that Lysenko talked mostly nonsense).³¹

The evidence presented by Haldane and others, however, was supportive of a viewpoint with which large segments of the public were in sympathy. Whether or not there is a bias in theoretical Marxism toward a "plastic" genetics, rank-and-file Marxists certainly preferred the view that heredity can be directly manipulated to the view that the genome is relatively stable, relatively impervious to the environment. Haldane himself (along with Muller in the United States and Filipchenko in the Soviet Union) was hostile to "Lamarckism," arguing that if heredity were directly alterable by the environment, those groups, races, and classes that lived in deficient environments would be genetically lamed.

29. Julian Huxley, *Soviet Genetics and World Science: Lysenko and the Meaning of Heredity* (London: Chatto and Windus, 1949), p. 35.

30. John Langdon-Davies, *Russia Puts the Clock Back: A Study of Soviet Science and Some British Scientists* (London: Victor Gollancz, 1949), p. 119.

31. Ake Gustaffson to H. J. Muller, January 26, 1949 (Muller collection); F. G. Gregory to J. B. S. Haldane, August 21, 1947. (Haldane Papers).

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As he wrote in response to one right-wing Lamarckian and eugenicist:

Reactionary biologists, such as Professor MacBride, who thinks that the unemployed should be sterilized, naturally use the theory of the transmission of acquired habits for political ends. It is silly, they say, to expect the children of manual workers to take up book-learning, or those of long-oppressed races to govern themselves. Laboratory experiments agree with social experience in proving that this theory is false.³²

(However, Haldane himself denied that the rejection of so-called Lamarckism implied a relatively stable, nonmanipulable heredity, insisting not only on the importance of mutation – “if they [genes] were unchangeable, I, as a Marxist could not believe in them” – and our ability to increase its rate, but also, more significantly, its direction).³³

In any case, one would have to be very careful in generalizing from the views of professional geneticists such as Haldane, Muller, and Filipchenko who, by virtue of their work, had to face facts (or at least what were accepted as facts by their peers) to the views of rank-and-file Marxists. It would be surprising if Marxist *geneticists* did not reject

32. J. B. S. Haldane, “Heredity: Some Fallacies,” in *Science and Everyday Life* (London: Lawrence and Wishart, 1939). See also his articles “The Inheritance of Acquired Characters”, *Nature*, 130 (1932), 20 and “Domestic Animals and Evolution,” *Daily Worker* (London), October 6, 1948, p. 4. Muller’s views were succinctly stated in his resignation from the Soviet Academy of Sciences: “Faith in the inheritance of acquired characters must lead inevitably . . . to the same dangerous Fascistic conclusion as that of the Nazis: that the economically less advanced peoples and classes of the world have become actually inferior in their heredity” (reprinted in *Science*, 108 [1948], 436). Filipchenko’s views are discussed in A. E. Gaissinovitch, “The Origins of Soviet Genetics and the Struggle with Lamarckism, 1922-1929,” *J. Hist. Biol.*, 13 (1980), 1-51, esp. p. 21.

The earliest use of this argument of which I am aware occurs in Arthur M. Lewis, *Evolution Social and Organic* (Chicago: C. H. Kerr, 1908). Lewis writes: “If it were true that the terrible results of the degrading conditions forced upon the dwellers in the slums were transmitted to their children by heredity, until in a few generations they became fixed characters, the hope of Socialists for a regenerated society would be much more difficult to realize. In that case those unfortunate creatures would continue to act in the same discouraging way for several generations, no matter how their environment had been transformed by the corporate action of society. This much at any rate Weismann has done for us, he has scientifically destroyed that lie” (pp. 78-79).

33. See his “In Defense of Genetics”, *Mod. Quart.*, 4 (1949), 194-202; quotation on p. 200.

Lamarckism and hence emphasize the doctrine's potentially reactionary consequences. But there is reason to doubt that ordinary Marxists (in fact, ordinary people – nonprofessionals – in general) found this line of argument appealing. If one looks at the records of the debates within the party resulting from the edicts of August 1948, the letters received by Haldane during the same period, as well as correspondence in the party press and in orthodox Marxist journals, it seems clear that for most people the Lamarckian view appeared the more optimistic. If certain groups, races, or classes were indeed genetically disadvantaged, this situation was remediable.

Prior to 1948, when the experts had been in apparent agreement on the scientific issue, the party rank and file reconciled itself to the unfortunate but seemingly undeniable genetic facts of life. The reaction that occurred when the first cracks appeared in what up until then had been a united scientific front indicates that their acceptance of orthodox genetics had been a reluctant one. Naomi Mitchison, Haldane's sister and a biologist herself, spoke for many others when she wrote to her brother expressing concern that the Lysenkoists were using the same methods rightly condemned in the Nazis, but noting that "on the scientific side [she] would be delighted, personally, to find that acquired characteristics were inherited, if only to some extent, because I was always sneakily on that side."³⁴ It was to tap the residue of Lamarckist sympathy that her comment reflected, as well as to avoid the embarrassing issues of Lysenko's two-camps philosophy and his methods of dealing with his scientific critics, that the Party focused its defense on the scientific content of Lysenko's views.

THE DEBATE WITHIN THE PARTY: 1948-1950

The strategy that the party adopted vis-à-vis its own internal critics, almost all of whom were scientists, was the reverse of that adopted with respect to the public at large: it did not talk science to its own scientists. The proceedings of the Engels Society (the natural science group of the British Communist Party) indicate that the party hierarchy and most of its scientists talked past one another in the same way as did the party and its external critics, only in this case it was the scientists and not the party who emphasized Lysenko's scientific claims. What they emphasized was their inadequacy – primarily the scarcity and unreliability of his data, the absurdity (and, equally objectionable,

34. Naomi Haldane Mitchison to J. B. S. Haldane, December 1, 1948 (Haldane Papers).

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the idealism) of his rejection of a material basis for the gene, his claim that plants and animals never compete with their own kind, and his distortions of the work of geneticists in the West. If one theme in particular resounds through their protests, it is that geneticists simply did not hold the views attributed to them by Lysenko – that he appeared entirely (and willfully) ignorant of all work since the time of his *bête noire*, August Weismann, in the late nineteenth century.

Such arguments were unavailing, since the validity of Lysenko's scientific views was not exactly of prime interest to the party hierarchy. J. D. Bernal made this clear when, at the major conference called to discuss the issues raised by Lysenko's biology, he asserted: "The first and most important thing for us to remember is that this controversy is already part of politics and is used today in this country as an auxiliary in the attack on the U.S.S.R. and in the drive to war."³⁵ Even more explicit were the remarks of the conference's chair, Emile Burns, who ended the meeting with the following statement:

We meet here as communists. Hence we shall never tolerate any expression hostile to the USSR. For we regard the attitude to the USSR as fundamental to loyalty to the party and to the working class. We need political firmness, not to be stampeded by the bourgeois criticisms of Soviet science.

Our approach to questions raised by the Soviet Union is always to seek to understand what is the basis of the Soviet standpoint – not to take up an attitude that "we know best," but to take up an attitude of studying the conclusions of the leading communist authority in order to understand them.³⁶

Hence virtually all of the party's biologists, including its eminent geneticist Haldane, were forced to fight a war on two fronts: within the party they denounced, and to the outside world they defended, Lysenko's theories.³⁷

35. J. D. Bernal, remarks quoted in "The Situation in the Science of Biology; Report of a Conference Called to Discuss the Issues Raised by T. D. Lysenko's Address on Soviet Biology," *Trans. Engels Soc.* (April 1949), 11.

36. Emile Burns, remarks quoted in *Transactions*, pp. 11-12.

37. Angus Bateman was an active anti-Lysenkoist within the party (who also wrote in defense of Lysenko for non-party audiences). He considered that none of the dozen or so party geneticists whom he knew at the time were genuine Lysenkoists; even J. L. Fyfe, author of the party pamphlet "Lysenko was Right"

In Haldane's case, however, both the defense and the critique were somewhat limited. Haldane genuinely respected some of Lysenko's work; this is clear from his arguments within the party where he went, in his own words, "further in support of Lysenko than some of the other comrades;"³⁸ from letters to friends and acquaintances, in which he praises some aspects of Lysenko's work; and from his "self-obituary" (recorded for the BBC after his operation for cancer), in which he affirmed: "In my opinion, Lysenko is a very fine biologist and some of his ideas are right."³⁹ In his nonscientific articles of late 1948 to 1950 (and particularly in his *Daily Worker* columns) Haldane went considerably further in defense of Lysenko than he did in private. There is no doubt that, for the public, he greatly exaggerated what merits he found in Lysenko's work. Even so, his defense was always limited to a few issues, and even these were hedged with qualifications and reservations. Ultimately it became impossible to maintain this restricted defense of Lysenkoism, given the party's increasingly rigid line. The distance between Haldane and party officials grew to the point that the *Daily Worker*, on whose editorial board he was serving as chair, published an uncompromisingly Lysenkoist "Educational Commentary" without his knowledge. Shortly thereafter he resigned from the party.

Having summarized Haldane's career as the party's leading spokesman on science, an involvement that came quietly to an end in 1950, we turn now to the actual content of his defense of Lysenko.⁴⁰

and whose defenses of Lysenko were particularly uncompromising, was in practice an orthodox Mendelist-Morganist plant breeder (letter of Angus Bateman to the author, November 17, 1980). Following the decision to ban "formal" genetics in the Soviet Union, Fyfe wrote that "we are forced, if we are still capable of facing facts, to conclude that this was an outstanding example of democracy in science." J. L. Fyfe, "The Situation in Biological Science I," *Mod. Quart.*, 4 (1949), 291-295; quotation on p. 294.

38. J. B. S. Haldane, remarks quoted in *Transactions*, p. 9.

39. "J. B. S. Haldane's self-obituary," recorded at University College, London, on February 20 and televised on BBC-2 after his death on December 1, 1964. Published in *Listener*, December 10, 1964, pp. 934-935; quotation on p. 935. Also important is an eight-page letter to M. Teich (undated, but replying to Teich's letter of October 3, 1948). It is difficult to know, however, whether this very detailed letter, which expresses considerable sympathy for some of Lysenko's scientific ideas (although characterizing them as greatly exaggerated), is an indication of Haldane's genuine beliefs or the strength of his loyalty to the party.

40. Haldane had earlier threatened to resign from the Engels Society in protest to a statement drafted on behalf of the group by Alan Morton. (It was not

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HALDANE AND THE SCIENTIFIC DEFENSE OF LYSENKOISM

It appears that Haldane was taken largely unawares by the events of August 1948. He was therefore in no position to respond when, news of the academy's decisions having traveled to England, he was besieged with requests for public statements and for private advice. As the party's scientific authority – and a geneticist to boot – his opinion was solicited by scientific colleagues inside and outside the party, by newspaper reporters, by friends, and by troubled readers of his weekly *Daily Worker* columns and many popular books on science. They all asked essentially one question: how could he reconcile support of the academy's decrees of August 1948 with the views he had previously expressed and which informed his own work?

Haldane took a very long time answering, maintaining that until a translation of the full proceedings of the session of the Academy of Science that had officially adopted Lysenko's views became available, he would not – and others should not – make up their minds. This was clearly only a way to buy time; for Lysenko's full report, a summary of the debates, and all of the decrees were almost immediately available in English. When his answer did come, it was indirect, but clear enough from what he said and refused to say on Lysenko's behalf. Haldane could not reconcile Lysenko's methods of dealing with his critics or his two-camps philosophy with his own thinking, so he simply ignored them; he emphasized instead what he claimed were Lysenko's practical achievements on the collective farms, and what he alleged was the rational core at the heart of Lysenko's admittedly crude and exaggerated scientific theories.

He defended his claim that, from a practical standpoint, Lysenkoism was a great success by noting that any other conclusion was incompatible with 'the assumption that the Soviet regime was rational. He assumed, probably correctly, that communists and noncommunists alike would find it hard to believe that the Soviet regime would support Lysenko if his policies had been practical failures. As Haldane said in his BBC broadcast: "Lysenko says that these transformed wheats have proved useful in cold parts of Siberia. I find it very hard to believe that

adopted.) Haldane to Maurice Cornforth, November 20, 1948 (Haldane Papers); also Cornforth to Bernal, January 7, 1949, expressing relief that Haldane had after all paid his dues at the previous meeting (Bernal collection). Haldane had also apparently threatened at least once to resign from the party over a nonscientific issue. See Clark, *J.B.S.*, pp. 171-172.

the Soviet Government would back him were this false. We may not like this Government, but after their achievements in the war we really cannot say that they are idle theorists uninterested in practical results.”⁴¹ He was able to bolster this argument with quotes from a number of Lysenko’s scientific critics who conceded his practical efficacy. For in asserting that Lysenko had greatly boosted the productivity of Soviet agriculture, Haldane claimed no more than some prominent anti-Lysenkoists; only they believed that the successes were achieved in spite of, rather than as a result of, Lysenko’s theories. Eric Ashby, for example, a respected geneticist who had lived in the Soviet Union and visited Lysenko’s institute on several occasions, wrote that “his scientific theories may be rubbish, but his practical ideals do in fact work; it is what Lysenko does on the farm, not what he says in the Academy, which matters to the Soviet Government.”⁴² Haldane was fond of quoting the second half of Ashby’s remark, for although he knew that the practical success of a theory does not guarantee its truth, it certainly provides a better argument for it than failure.

It was not, however, an argument that Haldane himself believed. His comments during the Engels Society debates make that plain. He did not even go as far as Ashby and other critics; for while they tended to ascribe Lysenko’s apparent successes to the vernalization techniques he had popularized, and/or to his appeal to peasants otherwise inclined to sabotage Soviet policy, Haldane doubted that the progress of Soviet agriculture was importantly related to anything Lysenko had done. He assumed that the primary factor was the new system of collective farming.⁴³ Hence, when Haldane asserted (or more commonly, implied) that the productivity of Soviet agriculture validated some of Lysenko’s theoretical claims, he was simply using any stick with which to beat the dog.

This is also largely true of his defense of the concept of inheritance of acquired characteristics. Until late 1948 Haldane had been a consistent opponent of Lamarckian ideas, stressing particularly what he saw as their reactionary implications. From the early thirties to the 1948 congress, Haldane had been careful to distinguish Lysenko’s work on vernalization and the theories of heredity that Lysenko had derived from it. As late as 1947, in an article laudatory of Lysenko’s agricultural

41. Haldane, contribution to “The Lysenko Controversy,” p. 875.

42. Eric Ashby, “Science without Freedom?” *Listener*, November 4, 1948, p. 678. See also his *Scientist in Russia*.

43. Haldane, *Transactions*, pp. 8-9.

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techniques, Haldane remarked, "I don't agree with his views on heredity."⁴⁴ After the events of August 1948, however, Haldane began to argue that there might after all be something to Lamarckian inheritance, at least with regard to plants and animals. (He never wavered in his objections to the possibility of Lamarckian inheritance in man.) For example, in one of his *Daily Worker* articles Haldane reported on an experiment conducted by his colleague L. S. Penrose. Penrose had discovered that if mice from a healthy line were suckled by foster mothers from a line with a high incidence of breast cancer, they were likely to develop cancer themselves. Haldane remarked that these experiments reminded him of Michurin's claims "to have produced changes in plants by grafting which gave rise to similar changes in later generations."⁴⁵ Of course, he continued, it is much more difficult to graft animals than plants, but the Penrose experiments did suggest a way to apply the Michurinist methods of vegetative hybridization to animals. You may not be able to graft one bit of a cow onto another, but you can give calves from a low-milk-yield breed milk from a high-yield breed. He concluded:

It would at least be worth trying whether desirable characters in the mammary gland can be "inherited" in this way in cows, as bad ones certainly can in mice.

At any rate his work shows that we have got to take broader views on the questions of heredity.

This does not mean that we have to swallow the Michurin line whole.

It does mean that we have to start thinking along lines suggested by him and other workers in the Soviet Union.⁴⁶

There are at least two notable aspects of this argument. The first is the spurious analogy between the transmission of cancer through milk in mice and the transmission of high milk yield through milk in cows, given the radically different physiological processes in the two cases. Had Haldane chosen, for example, susceptibility to disease in cows as the focus of the experiment, his suggestion would have been somewhat more reasonable. It might also have been reasonable to suggest that the

44. J. B. S. Haldane, "How Heat Upset the Barley," *Daily Worker* (London), August 11, 1947, p. 4.

45. J. B. S. Haldane, "Can You Inherit Cancer?" *Daily Worker* (London), January 17, 1949, p. 2.

46. *Ibid.*

quality of mother's milk has some effect on the growth rate of the calf, but this would have provided no support for Michurinism. Haldane's argument involves the homeopathic fallacy; he is apparently assuming the reasonableness to his nonscientific audience of the notion that giving cows better milk will improve the organs involved in determining milk yield. Still more obvious is another stratagem. The implication of the article is that the Penrose experiments demonstrated a change in the heredity of mice. But even in Haldane's day, these and similar experiments were understood as demonstrating the presence of an oncogenic virus. The cancer in mice was not heritable, in the ordinary sense of the term (and certainly in the sense assumed by readers of the *Daily Worker*); it is not transmitted to further generations. That is presumably why Haldane placed the word "inherited" in quotation marks. In a case analogous to his use of Dobzhansky's memoir, Haldane did not technically lie to his audience, yet even the most sophisticated of them probably would not have realized how little support the Penrose experiments provided for the Michurin-Lysenko theory.

Haldane also expressed partial sympathy with Lysenko over the significance of intraspecific competition; here he was probably expressing his genuine beliefs.⁴⁷ The textbook formulation of Darwinism, in Haldane's day and in our own, goes like this:⁴⁸

Postulate	Deduction
Each population of plants or animals tends to grow geometrically — the more individuals that exist, the faster their number increases. But the space and food they have available to live on increases slowly or not at all.	There is a continuing struggle for existence among the members of the growing population.
Hereditary differences exist among members of the population that affect their ability to survive and to reproduce.	The result is a continuing process of the survival of the fittest (natural selection).
New hereditary variation continues to appear in the population independently of the selection process.	Organic evolution occurs.

47. See, for example, his article "Lysenko and Darwin," *Daily Worker* (London), November 1, 1948, p. 2.

48. This formulation is taken from Edward O. Wilson et al., *Life on Earth*, 2nd ed. (Sunderland, Mass.: Sinauer Associates, 1978), p. 636. Reproduced by permission of the publisher.

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Haldane rejected the Malthusian element in this Darwinian schema. That is, he rejected the notion that overpopulation, leading to conflict within species, was the rule in nature. He did not deny its existence (as did Lysenko), but thought its extent generally exaggerated. Like many population ecologists such as Andrewartha and Birch, he argued that if the popular formulation of Darwinism were correct (he did not address himself to the question if the popular formulation were also Darwin's), we should find many more trees shorn of their leaves; the world would not be green. Although there are many different insects on trees during the summer, we only rarely find trees that have been stripped bare and some of the insects left starving. Why? Because the numbers of insects have been reduced by parasites and predators long before they have gotten to the point of eating themselves out of house and home. Animal populations, Haldane insisted, are typically kept down, not by resources in short supply but by conflict with other species; direct struggle among members of the same species is rare. He proceeded to make a more general point, about which there is today no disagreement (however inconsistent it is with the standard textbook description of Darwinism, with its Malthusian postulate of an exponential growth in population resulting in a struggle among members of the same species for scarce resources): natural selection can occur in the absence of competition among members of the same species, in fact even when they are actively cooperating. Natural selection occurs whenever some genotypes leave survivors that contribute more to future gene pools. The greater fitness of some organisms can result from any of a vast number of factors, including better ability to withstand the rigors of extreme heat or cold or drought; superior resistance to toxic metals in the soil, parasites, predators, or disease; greater ability to invade new habitats; or simply the capacity to breed more rapidly.

It is notable that Haldane never tried to buttress his scientific argument for de-emphasizing intraspecific competition by linking it to a reactionary Malthusianism. Many Marxists, as far back as Marx himself and Engels, were troubled by the Darwinian emphasis on conflict among members of the same species, a notion that clearly derived from, and in turn was used to justify, the world-view of the English bourgeoisie. Many Marxists before Lysenko had denigrated the concept of intraspecific competition as a reading into nature of Hobbes's *bellum omnia contra omnes*; that is, as ideology.⁴⁹ Even Haldane's very un-

49. For examples and a fuller discussion of this topic, see my article "Marxism, Darwinism, and the Theory of Two Sciences," *Marxist Perspectives*, 2 (1979), 116-143.

Marxist colleague, R. A. Fisher, with whom he “debated” Lysenkoism on the BBC, asserted that the Malthusian emphasis in orthodox Darwinism was so much excess philosophical baggage.⁵⁰ Haldane himself never used other than strictly scientific arguments. Like most scientists, including Marxists, he believed in a unified world science. To sociologize science was, implicitly at least, to undermine that assumption. For if scientific theories reflect the world-views of the societies from which they have emerged, then it is reasonable to assume that the science of a socialist society would in some respects differ from the science of a capitalist society.

It is the rare scientist who is comfortable with that line of argument, for reasons perhaps best exemplified by Lysenkoism – however logical the argument in the abstract, its practical applications have been extremely problematic. No one was more aware than Haldane that Soviet philosophers had condemned virtually all Western science as “bourgeois.” However much, as socialists, British scientists might have taken pride in Soviet scientific achievements, they were also proud of their own. But if the science of a classless society were fundamentally different from, and superior to, that of all previous societies, their own contributions and scientific tradition were ipso facto devalued. Needless to say, such a conclusion did not appeal to most scientific Marxists.

Until 1948, therefore, this aspect of Soviet philosophy was entirely ignored; the unity of science was praised equally by Marxists and non-Marxists. Haldane continued to ignore it, even after the “theory of two sciences” was adopted as official Soviet policy. He would in any case have found it somewhat difficult to maintain that genetics serves bourgeois interests, given the frequency with which he asserted that it was starved for resources in capitalist countries. So he simply said nothing about it, as he said virtually nothing about the political means by which Lysenko had triumphed over his critics.

Haldane concentrated instead on Lysenko’s scientific claims, which he presented in the most reasonable light. The problem was that when his points were reasonable, they were not Lysenko’s. This became increasingly, and embarrassingly, clear over time, as Lysenko’s own writings became available in English translation and were widely publicized. Lysenko did not assert that in a few cases, probably restricted to lower organisms, acquired characters might be heritable. Nor did he say that we ought to pay more attention to extranuclear inheritance.

50. Ronald A. Fisher, *The Genetical Theory of Natural Selection* (New York: Dover, 1958; orig. ed., 1930), pp. 46-47.

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Lysenko denied the existence of genes — that is, of a material basis of heredity. (Ironically, the materialism of modern genetics was a particular source of pride to Marxist scientists. In his unpublished autobiography Haldane boasted: “I was the first person in Britain, perhaps in Europe, to support Morgan’s theory that the gene, the unit of heredity, is a definite thing at a particular place in a particular chromosome.”⁵¹ This is exactly what Lysenko denied.) Nor did he argue that selection can occur in the absence of competition; he argued that competition *never* occurs. Haldane consistently praised Lysenko for pointing out how “rarely” direct competition is found in nature. This may be an acceptable view, but Haldane’s critics, both within and outside the party, were quick to point out that its resemblance to Lysenko’s thinking was slight. Perhaps Haldane’s most embarrassing moment occurred when, in the course of his BBC defense of Lysenko, he illustrated our potential control over the mutation process by noting that we can double the chromosome number of plants by treating them with colchicine — unaware that Lysenko had condemned the use of colchicine as a “torture” and “mutilation” of plants.⁵²

Over time the extent of the gap between Haldane’s scientific views and Lysenko’s became increasingly clear to everyone — to Haldane himself, his colleagues, the lay public, and party officials. This was also true of his position on eugenics, which he stubbornly refused to alter in spite of its embarrassment to the party. In fact, his long-held eugenic views, which he declined to retract or even substantially modify in the course of the Lysenko controversy, probably caused the party greater distress than his refusal to unconditionally support Lysenko’s scientific claims.

HALDANE ON INEQUALITY AND EUGENICS

“The test of the devotion of the Union of Soviet Socialist Republics to science will, I think, come when the accumulation of the results of human genetics, demonstrating what I believe to be the fact of innate human inequality, becomes important.”⁵³ So wrote Haldane in 1932, about five years before his turn to communism and ten years before he

51. J. B. S. Haldane, “Why I Am a Cooperator,” manuscript, p. 17. (Haldane Papers.)

52. The incompatibility of Haldane’s and Lysenko’s remarks was noted by Eric Ashby in a letter to *The Listener* of November 25, 1948, quoted by Langdon Davis in *Russia Puts the Clock Back*, p. 93.

53. J. B. S. Haldane, *The Inequality of Man and Other Essays* (London: Chatto and Windus, 1932), p. 137.

formally joined the Communist Party. Haldane's new political commitment did not signal a change in his attitudes toward human inequality and eugenics, attitudes that remained remarkably stable throughout his adult life (although he moderated their expression slightly after the events of August 1948) and that he shared with a large number of Marxist scientists.

The history of eugenics has been written again and again as though it were simply the scientific expression of right-wing political and social views. It is easy to forget – indeed, it appears to have been nearly forgotten – that during the 1920s and 1930s there flourished in Germany, Britain, and the United States a movement popularly known in America as “Bolshevik eugenics.”⁵⁴ In Britain and the United States at least (I do not know about Germany), the enthusiasm that scientists felt for the Soviet Union was rooted in their conviction that it would spur scientific development and promote a scientific outlook. For biologists the test of a genuinely scientific outlook tended to be a society's attitude toward eugenics, or what was then often called “race betterment.” Marxist biologists believed that Western societies had failed in this regard; to the extent that eugenic sentiment had taken hold, it was used in a pseudoscientific way to buttress the conventional social order. Marxists therefore opposed both conventional eugenics – asserting that there could be no valid comparison of the intrinsic worth of different individuals in a class-stratified society – and those who insisted that biologists had nothing to contribute to the improvement of mankind's intelligence and character. (In the absence of revolution, some were willing to compromise by encouraging the obviously talented to reproduce or to participate in schemes for artificial insemination. Reminiscent of Robert Graham's recent scheme – originally inspired by the ideas of H. J. Muller, perhaps the most prominent of the Marxist eugenicists – was a sperm bank proposal of the mid-thirties to which Haldane offered donations of both his money and his gametes.)⁵⁵

54. Loren R. Graham has discussed eugenic sentiment in Weimar Germany and the Soviet Union in the 1920s in “Science and Values: The Eugenics Movement in Germany and Russia in the 1920s,” *Amer. Hist. Rev.*, 82 (1977), 1133-64. Other works challenging the conventional association of eugenics with the Right are: Linda Gordon, *Woman's Right: A Social History of Birth Control in America* (New York: Grossman, 1976), Donald MacKenzie, *Statistics in Britain* (Edinburgh: Edinburgh University Press, 1981), and Michael Freedman, “Eugenics and Progressive Thought: A Study in Ideological Affinity,” *The Historical Journal*, 22 (1979), 645-671.

55. Reported in a letter of Herbert Brewer, author of the proposal, to Joseph Needham, 1936 (Needham collection).

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The enthusiasts for a Bolshevik eugenics differed over what they thought a fair test would indicate about genetic differences among classes; the English tended to assume that that the upper classes contained a disproportionate number of the fit, whereas the Americans argued that if class-related differences existed, they would favor the masses. Both sides agreed that, at a minimum, *individuals* varied significantly in their genetic endowments, not just in trivial matters such as hair color or height, but in intelligence and traits of character; that the fitter should be encouraged and the less fit discouraged from reproducing; and that such a policy could be successfully pursued on a large scale only in a society that had provided approximately equal opportunities to all its members. The Soviet Union was perceived as such a society, and hence afforded the first opportunity for a genuine test of eugenic principles, a significant element in its appeal to scientists.

This is not the appropriate place to describe in detail the history of Marxism and eugenics. I have tried to provide a context for Haldane's comments — without which they might appear idiosyncratic. They were not. They represent views widely shared by scientists on the left, especially the Marxist left. They were not, however, shared by Lysenko, his supporters in Britain, or presumably by most of the public, including readers of the *Daily Worker*. Their egalitarian and antieugenic sentiment Haldane did little to accommodate. He did not write in the *Worker*, as he did elsewhere, that capitalism was dysgenic because it encouraged the rich to produce small families.⁵⁶ But even in the *Worker* (as late as November 1949) Haldane expressed his belief that races differ in their "proportions of highly-gifted people" and insisted that "the dogma of human equality is no part of Communism . . . the formula of Communism: 'From each according to his ability, to each according to his needs,' would be nonsense, if abilities were equal."⁵⁷ And in one of his last articles for the *Modern Quarterly* Haldane wrote:

56. He did write in a more intellectual Marxist journal, as late as autumn 1948, that "in many countries the poor breed much quicker than the rich, even when allowance is made for their higher death-rate. Thus the valuable genes making for ability, which bring economic success to their possessors, are getting rarer, and the average intelligence of the nation is declining." If true, asserted Haldane, the conclusion should be that wealth ought to be equalized. "Biology and Marxism," *Mod. Quart.*, 3 (1948), 2-11; quotation on p. 9.

57. J. B. S. Haldane, "Darwin and Slavery," *Daily Worker* (London), November 14, 1949, p. 2.

Haldane and Lysenkoism in Britain

Some Marxists have reacted too strongly against the application of biological notions to mankind, and assumed that all differences between human beings are due to differences of environment . . . But we know in practice, and should, I think, admit more fully in theory, that different people have very different abilities, that some are capable of making greater contributions to society than others, and that this would be true had they equal opportunities.⁵⁸

Following publication of Langdon-Davies' *Russia Puts the Clock Back*, Bernal prepared a review for the *Daily Worker*. Langdon-Davies' particular target was Haldane. He began his book with Haldane's assertion in *The Inequality of Man* that the Soviet Union's devotion to science would be put to the test when genetics demonstrated the fact of innate human inequality; this claim was repeated several times in the text. Bernal excoriated Langdon-Davis for dredging up Haldane's past statements which, he implied (without so much as alluding to their content), had long since been abandoned. Wrote Bernal: "Langdon-Davies makes a great play of quotations from Haldane's earlier works, showing him to have held views that would now embarrass him, but the greatness of a scientist is not shown by the rigidity of his views, quite the contrary."⁵⁹

The *Worker* did not print Bernal's review; it published Haldane's instead. Haldane's earlier statements on equality embarrassed the party, not him, and he proffered no apology. Neither did he defend his views, which would have involved him in an open disagreement with party policy. As on other issues where Haldane's own views seriously contradicted those of the party, he merely kept silent. The strength of his loyalty to the party prohibited a public dissent. Yet there were certain beliefs he could not bring himself to disavow: the universality of science, the right of scientists to decide scientific controversies in their own way, and the innate inequality of human beings.

In the same review Haldane did address the question, repeatedly asked by Langdon-Davies (and many others), of Haldane's likely fate as a geneticist in the Soviet Union. He responded that he thought he

58. Haldane, "Biology and Marxism," pp. 9-10.

59. Unpublished review submitted to the *Daily Worker*, with a copy sent to Haldane. Bernal also wrote that he had "talked with Lysenko and seen his results, and he has impressed me as a scientist more original than any I have met for years" and that "what Langdon-Davies and his scientific backers cannot see, because they do not want to see, is the intrinsic necessity of such steps in any state that takes science seriously" (mss. in the Bernal collection).

probably "should have lost my job and got another, as Dubinin did. Dubinin whom, in spite of Lysenko, I regard as a very fine biologist, is now a professor of ornithology."⁶⁰ This was an enormous concession, in spite of its appearance in an article generally defending Lysenko against Langdon-Davies' attack. Not surprisingly, it was Haldane's last published contribution to the Lysenko debate, although he continued to write for the *Worker* on other matters until August 1950.

The paper, to which he had contributed a weekly column for thirteen years as well as chairing its editorial board for ten, took no note of his leaving. Although he did not quit the *Worker* and the party until the summer of 1950, he had signaled his abandonment of the Lysenko issue a full year previously. In fact, his departure from the *Worker* was something of an anticlimax, for in the summer of 1949 he had published a widely quoted article, "In Defense of Genetics," in the *Modern Quarterly*; it ended with the following words: "I believe that wholly unjustifiable attacks have been made against my profession, and one of the most important lessons which I have learned as a Marxist is the duty of supporting my fellow workers."⁶¹ As far as his scientific colleagues were concerned, this article represented his break with Lysenko and the party. That he continued to write for the *Daily Worker* for another full year, defending Lysenko in his columns, indicates the strength of his party loyalty. He felt he must leave, but endeavored to do so in the way that would do the party least damage.

In the same issue of the *Modern Quarterly* appeared an article of Bernal's.⁶² With Haldane's replacement by Bernal as its most distinguished apologist for Lysenko, the character of the party's argument abruptly changed. Unlike Haldane, and in spite of his own past statements, Bernal was willing to provide not just an unconditional defense of Lysenko's theories, but also of the right of the state to adjudicate a scientific controversy. For the first time in the history of the controversy in Britain, Lysenko's views were said to exemplify the new "proletarian" science of the Soviet Union, destined to replace the decadent "bourgeois" science of the West.

60. J. B. S. Haldane, "Nonsense about Lysenko," *Daily Worker* (London), November 9, 1949, p. 2.

61. J. B. S. Haldane, "In Defense of Genetics," quotation on p. 202.

62. J. D. Bernal, "The Biological Controversy in the Soviet Union and Its Implications," *Mod. Quart.*, 4 (1949), 203-217.

CONCLUSION: FROM HALDANE TO BERNAL

Bernal's arguments of 1949-1950 make for depressing reading and it seems uncharitable to dwell on them. Suffice it to say that he gave unqualified support to Lysenko, as he did, publicly at least, to every aspect of Soviet policy – even to the point of sending a message to the Soviet Academy of Sciences expressing his full support of the August decrees.⁶³ The man who before 1948 and after Lysenko's fall consistently asserted the universality of science and stressed the socialist commitment to scientific freedom wrote that "the importance of this decisive step is that it marks for the first time the assertion of the independence of science in the Soviet Union from the previously universal community of world science; and also that for the first time the Central Committee of the Communist Party of the Soviet Union has declared itself on a scientific issue."⁶⁴ While Bernal's own laboratory was trying

63. Although Bernal publicly defended the Soviet invasion of Hungary, he was upset by it to the point of cooperating behind the scenes with the children of Michael Polanyi (his old nemesis) to aid Hungarian scientists. See his correspondence with George and Priscilla Polanyi, late spring and summer 1957 (Bernal collection). That he was even more disturbed by the invasion of Czechoslovakia is indicated by his unfinished and difficult-to-follow manuscript of September 1968, "The Doctrine of 'Peaceful Counter-Revolution' and Its Consequences" (Bernal collection). I am grateful to Dorothy Hodgkin for alerting me to the existence of this manuscript.

For information regarding Bernal's message to the Soviet Academy of Sciences see the article "Scientific Freedom" which appeared in the *Manchester Guardian* of January 29, 1949, p. 4, and Bernal's response, "Science and Freedom," of February 4, 1949, p. 4. Also relevant is a letter of Bernal to Julian Huxley, April 29, 1949 (Bernal collection).

64. Bernal, "The Biological Controversy in the Soviet Union," p. 204. The lengths to which Bernal was willing to go in his public defense of Soviet policy is indicated by the following passage from his essay "Stalin as Scientist," *Mod. Quart.*, 8 (1953), 133-142. "In thinking of Stalin as the greatest figure of contemporary history we should not overlook the fact that he was at the same time a great scientist, not only in his direct contribution to social science, but, even more, in the impetus and the opportunity he gave to every branch of science and technique in the creation of the new, expanding and popular science of the Soviet Union" (p. 133). Further, "Stalin's achievement is something greater than the building up and defending of the Soviet Union, greater even than the hope for peace and progress that he gave to the whole world. It is that his thought and his example is now embodied in the lives and thoughts of hundreds of millions of men, women and children: that it has become an indissoluble part of the great human tradition. However great the changes of the next few years, and there will be great changes which he worked for and would welcome, this remains" (p. 142).

(with techniques he had developed) to determine the structure of DNA — an event he later hailed, without a trace of irony, as “the greatest discovery in all modern science” — he was maintaining the “formal” genetics was only apparently materialistic.⁶⁵ “No one has actually isolated the genes,” he asserted. “Up to now they are factors, chosen simply to explain the phenomena in the simplest way, and are thus liable to the criticism that had been levelled against Machian idealism by Lenin.”⁶⁶ Echoing the views not just of Lysenko but of Shaw and of Shaw’s favorite, Samuel Butler, Bernal complained that according to the orthodox evolutionists, chance ruled the universe and that, like Weismann, the evolutionists insisted on the “isolation and autonomy of the gene.”⁶⁷ Both of these views had been characterized by Haldane, in earlier issues of the *Modern Quarterly*, as at best misunderstandings, and at worst willful distortions, of contemporary genetics.

Bernal remained loyal to the party until the end of his life. Most of the party’s scientific members ultimately left it but, surprisingly, not over the Lysenko issue. The scientists who left did so in general for the same reasons and in response to the same events as their nonscientific colleagues; the purges, the revelations of the twentieth party congress, the invasions of Hungary and of Czechoslovakia. As far as I have been able to determine, Haldane alone broke with the party over Lysenkoism, choosing his scientific over his political loyalties. His scientific colleagues at the time, and many persons since, have found this laudable. But both those who remained loyal to the party and those who left it over other issues said, in effect, that the cause of humanity cannot, after all, be wholly identified with that of science. Perhaps there is also something to be said for the values reflected in their judgment.

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65. Bernal, from the foreword to the second edition of *The World, the Flesh and the Devil*, p. vi.

66. Bernal, “The Biological Controversy in the Soviet Union,” p. 206.

67. Ibid.

questions regarding the debates over Lysenkoism within the British Communist Party; Richard Lewontin, for his considerable aid in evaluating Haldane's scientific arguments; and Naomi Mitchison and Loren Graham for their helpful comments on an earlier draft of this manuscript. I am also grateful to Mrs. Mitchison and to Thea Muller for permission to quote from unpublished material.

This article involved use of a number of collections of unpublished material, primarily letters and papers of J. B. S. Haldane, but also material from the collections of J. D. Bernal, H. J. Muller, and Joseph Needham. The following information may be of interest to others working in the same general field. The extensive Bernal papers are housed at the Cambridge University Library and an index to that material is now available. Joseph Needham's papers, to which Gary Werskey has compiled a very useful guide, are also deposited at that library. Letters and papers relating to Haldane's involvement with the Communist Party are deposited at the D. M. S. Watson Library of University College, London, designated the Haldane Papers. Muller's letters and papers are housed at the Lilly Library of Indiana University; as of late 1981, there was no index yet available to scholars. (The letters are currently arranged by year with some special name files for frequent correspondents).