

Nikolai Ivanovich Bukharin
A CENTENARY APPRAISAL

Edited by
Nicholas N. Kozlov
and
Eric D. Weitz

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the fairly well established argument by Barsov, Millar, Ellman, and others, which states that collectivization failed to transfer an increased surplus to industry, with the *non sequitur* that therefore collectivization was nonfunctional for rapid industrialization. Haynes, *Nikolai Bukharin*, pp. 84–85, 95. For a more extended treatment of this, see Mark Harrison, "Why Did NEP Fail?" *Economics of Planning* 16:2 (1980).

69. Carr and Davies, *Foundations* 1:1, pp. 44–48. The point made by Carr and Davies is not so much that agrarian policy in the late 1920s was good, but that the problem was exceedingly complex and intractable, and not amenable to solution by a "simple" price reform as Haynes implies.

70. Haynes, *Nikolai Bukharin*, p. 83.

71. Carr and Davies, *Foundations* 1:1, p. 259.

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The Bukharin Delegation on Science and Society: Action and Reaction in British Studies of Science

Val Dusek

ACTION

On 25 June 1931 a plane from Moscow landed at the airport in Croydon, England.¹ On board was a delegation of the Soviet Union's leading experts in the sciences and the history and philosophy of science, led by Nikolai Bukharin. At the time Bukharin was Director of Industrial Research and head of the history of science section of the Academy of Sciences, but no longer served as head of the Comintern and had been unseated from the Politburo. Although Bukharin's role as a world leader was over, his activity in 1931 as theorist of science planning and as organizer and leader of this Soviet delegation was to have great impact.

The delegation included the Soviet Union's leading physicist, A. F. Joffe; its most famous biologist, Nikolai I. Vavilov; Arnost Kolman (listed as E. Colman in the proceedings), mathematician, philosopher of science, and later influential policy figure; and a then little-known physicist and historian Boris Hessen (or Gessen), whose appearance at the conference would be crucial to later debates in the social history of science, but who was subsequently known in the West only for his paper at this conference.

Bukharin would in seven years be on the docket in the most famous of the purge trials and had already lost his major political power in the Soviet Union, although he was to remain influential in research and development planning for several years. Vavilov was to be the best-known victim of the agronomist and crank biologist Trofim D. Lysenko, in the latter's destruction of Mendelian genetics in the Soviet

Union.² Hessen also was to disappear in the purges. Kolman was to remain an important figure under Stalin, defending quantum mechanics and relativity against philosophical crises. He was responsible for reintroducing symbolic logic in the Soviet Union³ and defending the cold war hard line at the postwar international philosophical congress,⁴ and was involved after World War II in philosophy in Czechoslovakia.⁵ Ironically, some of the best and most creative work in science and its interpretation in the Soviet Union was presented in Britain just as the Stalinization of intellectual life in the 1930s was beginning.

The appearance of this stellar group of Soviet intellectuals at a Western conference was apparently instigated by Stalin as part of a policy "signal" that a recent campaign of discipline and repression of Soviet intellectuals was to be eased. Stalin apparently assigned Bukharin to head the delegation and choose the delegates. As late as June 22nd the delegates themselves were unaware that they would be going to London. On the next day Stalin brought the recent campaign concerning intellectuals to a close and launched a new propaganda effort to the West concerning Soviet technological progress. The participants (particularly Hessen) prepared their papers within a few days. An hour after the plane left Moscow it returned, as in the rush of packing Bukharin had forgotten his paper!⁶

The conference that Bukharin and his delegation were attending was the Second International Congress for the History of Science and Technology in London. Were it not for Bukharin's appearance and Hessen's paper, it would hardly have been noted by the general public or be remembered except in an occasional footnote in a history paper. However, the conference was the inspiration for a number of radical scientists in Britain who campaigned and wrote for the social planning of science. The papers of the Soviet delegation, particularly those of Bukharin and of Hessen, were the (often unmentioned) object of criticism for a number of mainstream historians and philosophers of science in the English-speaking world for the next several decades. They were the "other" who surreptitiously determined the arguments and claims in the field, as we shall see.

In 1931 the arrival of a group by airplane from Moscow was in itself a notable event. The organizers of the conference were somewhat taken aback by the sudden arrival of major Soviet figures, even more so when the delegation requested to deliver a dozen or so lengthy papers that had not been scheduled. The organizers of the conference were hardly willing to give up their planned weekend outing to listen to orations by a gang of communists, who were in turn "surprised to find that the arrangements included only... nine hours' actual discussion in five days."⁷ However, it was finally arranged that a special Saturday morn-

ing session would be added, and that the Soviets, limited to ten or fifteen minutes apiece, would have their papers published. The Soviet embassy was turned into a publishing house, and in five days the papers were translated and printed for conference members. Neal Wood writes:

The unprecedented attendance of the Soviet delegation, its large size and the eminence of its members, the leadership of Bukharin, for those times the spectacular mode of transportation, and the publication of the papers were all marks of a master touch, perhaps that of Bukharin himself.⁸

In another few days the papers were bound as a book and offered for sale.⁹ Within another two days a review appeared in *The Spectator* and a short piece by Bukharin appeared in the conservative *New Statesman*.¹⁰

One can imagine the reaction of the stodgy British historians of science to the Soviet participation in the conference. At one point, in response to a paper by two well-known British historians, daringly suggesting that political history be broadened to include intellectual history, five Soviet delegates rose to comment and suggest that this would only perpetuate "great man" history and needed further supplementation by social history.¹¹ The orthodox science historian Singer wished to cut off their lengthy comments with the aid of a large ship's bell. Even this did not silence those like Bukharin, who were used to speaking in far more tumultuous atmospheres than that of an academic conference.

When the special session came Bukharin delivered his major contribution, "Theory and Practice from the Standpoint of Dialectical Materialism." Bukharin's paper bristles with quotations in the original languages from a variety of traditional philosophers such as Francis Bacon and George Berkeley as well as from the early logical positivists, the neo-Kantians, William James, Henri Bergson, Max Scheler, and others. This apparently did not impress the reporter from the right-wing *Daily Mail*, which had already headlined the arrival of the former chief of the Comintern in London with "Bukharin, Head of Hate Factory," "Simple Scientist Unmasked," and described the talks as "tub-thumping speeches about Russia," given in "broken English, sometimes in bad French, and again in execrable German."¹²

Bukharin, emphasizing the role of practice in knowledge, criticized the contemplative stance of most contemporary epistemology. He criticized positivism for basing science on sense data while neglecting the reworking of those sense data in thought and practice and condemned the emphasis on knowledge for knowledge's sake. The novelty of Bukharin's whole approach was compounded by a heavy dose of Marxist

theory. The talk was followed by silence punctuated only by nervous coughing and foot-shuffling.

Finally David Guest, a young radical, recently returned from study in Göttingen with David Hilbert, rose to comment that the contradictions in the foundations of mathematics were reflections of the social contradictions in twentieth century capitalism. This comment, understandably, more extreme even than Bukharin's or Hessen's, was greeted by further embarrassed silence.¹³ (Guest, along with other young Marxists such as John Cornford and the many-talented Christopher Caudwell, would be killed in the Spanish Civil War before the age of 30).¹⁴

Hessen's talk, "The Economic and Social Roots of Newton's *Principia*," was to have more influence than any talk at the conference. Hessen's paper was apparently written in immense haste for the conference, given the few days notice. In the paper he took a more extreme stance than he apparently did in his other, untranslated and relatively unknown Russian language papers in the history of science.¹⁵ Hessen took the most extreme position possible on the social conditioning of science. By choosing Newton's *Principia*, rather than some more obviously socially influenced work in economics, medicine, or biology, or some scientifically less solid and philosophically speculative work, Hessen was making the strongest possible assertion that the purely scientific core of a work, and not simply its metaphysical, religious, or practical periphery, was socially conditioned. Hessen portrayed Newton's position as a result of the political compromises of the period between Oliver Cromwell and the Glorious Revolution. This position was decades later developed in a much more qualified, nuanced, and subtle manner by Margaret Jacob.¹⁶ Hessen also argued at length that Newton's pure science was technologically inspired.

As historian Gary Werskey says, from a largely conservative audience to which it was delivered "we might have expected them to giggle or to get very angry."¹⁷ Indeed, those reactions are still common today. Stephen Toulmin once said at a Philosophy of Science Association meeting, to an appropriately giggling audience, that he thought that Hessen had written his paper "with tongue in cheek" and that it was apparently an elaborate joke. Jon Elster, the recently prolific and influential "analytical Marxist" who reconstructs Marx in terms of decision theory, refers to Hessen's work as one of the exhibits "in the chamber of horrors of science."¹⁸ Apparently the social determination of science is as threatening to Elster's positivist faith as are Feuerbach's and Marx's accounts of religion to that of fundamentalists. I remember the independent but identical reactions of two of my colleagues to the mentioning of the title of Gessen's work, despite one colleague's sympathy

for Marx and the other's Hegelianism—"A Marxist analysis of *Newton*?"

Loren Graham suggests that Hessen's paper was in fact part of Hessen's own strategy within the Soviet debates concerning the new physics. Hessen was suggesting to the Soviet Marxist opponents of relativity theory and quantum mechanics that just as Newton's work could be scientifically of the highest value, and at the same time socially conditioned, so could the work of Einstein and Heisenberg. The achievements of the science were not canceled by the idealist philosophies (Mach's sensualism, Poincaré's conventionalism) with which the invention and interpretation were associated.¹⁹ Hessen's paper was the forerunner of the social history of science, which has grown so significantly in the last two decades.

REACTION

The Bukharin delegation had an important effect on thought about science in the West, particularly in the English-speaking world. The theoretical issues can be roughly divided in two: (1) the validity of a social history of science, in particular one which claimed that economic factors and technology determined the history of science, and (2) the desirability and potential applicability of the social planning of science. The Soviet delegation's presentations, particularly those of Hessen and Bukharin, respectively, polarized positions on both of these issues.

An active and brilliant group of radical scientists who were either present at the talks or soon influenced by them were inspired to advocate the social planning of science and to advocate and practice the conservative (or nineteenth century liberal) figures concerned with the place of science in society, reacted against the proposals of Bukharin and those British scientists who launched their proposals and agitation from his work, and argued against social or political intervention of any sort in science. A number of English-speaking historians of science were also to construct their arguments and histories concerning the independence of the development of scientific ideas from social factors in reaction to the presentations of the Soviet delegation, particularly those of Hessen, often without mentioning those ideas.

The Positive Left Response

The figures most immediately awakened and introduced to Marxist ideas or else encouraged or energized in already radical proclivities by the Bukharin's delegation's presentations were an extraordinary group

of British scientist-activists. These included J. D. Bernal, J. B. S. Haldane, Joseph Needham, Hyman Levy, and Lancelot Hogben, among others. Each of them was quite extraordinary in his own way and each deserves a book-length study. C. P. Snow portrays Bernal in his novel, *The Search*, in 1934. J. B. S. Haldane was fictionalized by Aldous Huxley in *Antic Hay* and in a less well-known novel.²⁰

Bernal, the brilliant crystallographer and polymathic scientist, was a lifelong communist activist. He already had strong Marxist sympathies before the conference, but apparently was further energized to travel to the Soviet Union a few weeks later. Bernal wrote to Beatrice Webb:

I can say that the inspiration for my own work and that of many others in science, notably Haldane, and Hogben, can be traced definitely to the visit of the Marxist scientists.... We did not understand all that they said, in fact I now suspect that they did not understand it entirely themselves, but we did recognize that here was something new and with immense possibilities.²¹

Bernal, more than anyone else, elaborated upon Bukharin's theses on science planning and the concept of an economic history of science that Hessen developed in numerous writings, most notably, *The Social Function of Science and Science in History*.²² The former presented the theses of science planning, and the latter the economic determinist history of science. The program for the social planning of science became so identified with Bernal that it was called "Bernalism" by its enemies.

The even more eminent biologist J. B. S. Haldane, who made major discoveries in biochemistry, population genetics, and other fields, wrote weekly science columns in the communist press.²³ The mathematical ecologist Richard Levins has suggested that Haldane was scientifically the equal of Einstein, but is less well known only because of the lesser prestige of biology relative to physics.²⁴

Joseph Needham was already an eminent biochemical embryologist and historian of embryology when he came to the conference. There he had his eyes opened to the Marxist approach to a social history of science. He also realized the similarity of Marxist dialectics of nature to his own approaches to biology in terms of integrative levels and process philosophy. Needham recalls that despite Hessen's mispronunciations and factual errors, he heard the "trumpet blast" of a new approach to the history of science.²⁵ During the subsequent decade he went on to learn Chinese in middle age, and, starting in the 1950s and continuing to the present, to write the magisterial multivolume *Science and Civilization in China*,²⁶ in which he almost singlehandedly discovered for

the West the riches of traditional Chinese science. Needham's work is one of the greatest projects of historical writing of all time.

Lancelot Hogben was a socialist who was more detached from the Marxist-Leninist regimes and parties than the others. Although hardly in the universal genius category of the other three, he was a highly influential popularizer of science as well as an able and well-regarded biologist.²⁷

Hyman Levy, already a communist before the congress, was a mathematician who wrote a series of highly original accounts of Marxism, which emphasized relations as more fundamental than things. His ideas on the interpretation of Marxism were revived by Bertell Ollman in his *Alienation*.²⁸

Despite the wide-ranging impact of the Bukharin delegation, Stephen F. Cohen, in his well-regarded biography of Bukharin, has little to say about Bukharin's views and activities in relation to the history, philosophy, and planning of science.²⁹ Cohen provides only the briefest mention of the London visit and of Bukharin's important speech on science planning, which he had presented in April 1931 to the First All-Union Conference on the Planning of Scientific Research Work.³⁰ Although Bukharin did not reiterate these ideas at the London conference, he probably communicated his views to Bernal, with whom Bukharin spoke at length.

In the speech to the All-Union Conference, Bukharin had laid out areas and, to a lesser extent, priorities for science planning. As Loren Graham remarks, the speech deals primarily with planning *for* science rather than planning *of* science. Following Graham's analyses, planning, according to Bukharin, should consist of:

1. Substantial labor and budget allocations: "the scientific research framework must grow even faster than the leading branches of socialist heavy industry."³¹
2. Strong support for scientific research institutes, with no false economies in research funding.
3. Geographical dispersion of research institutes. Centers should be started in Kazan, Siberia, and other areas lacking scientists. These centers should combine research institutions, educational facilities, and industrial production. (Novosibirsk in Siberia is a much later realization of this idea.)
4. Determination of the supply of cadres, including:
 - a. number of scientific workers,
 - b. distribution of researchers,
 - c. qualifications demanded,
 - d. correct use of workers with different skills and talents.³²
5. Determination of the subjects of research.

In this last area, one of the most contentious in the science planning debate, Bukharin is much more sketchy. He does say, though, that

scientific planners "should not fear the world 'utilitarian.'"³³ Bukharin proposes a hierarchy of priorities for research:

1. "First aid" projects, that is, emergencies arising from underdevelopment and wartime destruction
2. Urgent problems of development
3. Long-term plans

In evaluating the "urgency" of a project, Bukharin offers four criteria:

1. Impact on future industrialization
2. The degree of practical consequences
3. The extent to which capitalist countries are likely to solve the problem soon (presumably so that the idea or technique can then be used without developing it domestically)
4. The extent to which Soviet environment, resources, or traditions give special opportunities for Soviet advance in a field

The ideas proposed by Bukharin in his talks at the London conference stimulated radical scientists, as well as a few more moderate scientists awakened by the Depression to the need for science planning. These scientists soon launched a movement for the social planning of science in Britain.³⁴ Several aspects of planning science during World War II developed from their work and participation. The science planning movement, at first considered a radical and dangerous one, found moderate support from the advocates of planned capitalism during the Depression, and eventually was built into the science policy of Britain. *1935, The Next Five Years*, a report supporting planned capitalism, had the signatures of moderates such as Harold MacMillan, the philosopher Samuel Alexander, the biologist Julian Huxley, the psychologist Richard Gregory, the physicist Oliver Lodge, as well as H. G. Wells. By the 1950s major elements of "Bernalism" had become accepted by the majority of mainstream science policy figures in Britain. Harold Wilson's preface to the 1961 Labour Party report, *Science and the Future of Britain*, for example, embraces the science planning creed.³⁵

Meanwhile—and ironically—Bernal's *Science in History* was translated and published in Russian, thereby introducing Bukharin's ideas under respectable communist auspices, though long after Bukharin had become an unperson in the Soviet Union.

The Conservative Reaction

The conservative reaction to Bukharin's and the Soviet delegation's presentations took two forms: opposition to science planning in science

policy, and defense of the autonomy of the history of science from the history of economics and technology. In the first of these, the views expressed by the members of the Bukharin delegation were explicitly attacked, and became the focal point of a general and highly politicized indictment of socialism.

Michael Polanyi, the Hungarian physical chemist who later emigrated to Britain and shifted to economics and sociology, and now best remembered for his work in the philosophy of science and knowledge, states that his turn from chemistry to philosophy was triggered by the shock he experienced in hearing Bukharin's views. Indeed, Polanyi's whole philosophy of intuition and authority in science and his rejection of rationalism and critical philosophy was a reaction to his encounter with Bukharin. In Polanyi's case, this encounter occurred in the Soviet Union several years later than that of Bernal and others. Interestingly, Michael Polanyi, the arch-conservative, is the brother of Karl Polanyi, the social democratic author of some of the most creative Marxist social studies of the postwar era.³⁶ The politically conservative thrust of Michael Polanyi's doctrine is suggested by the fact that one of Michael Polanyi's leading economic disciples is "supply-side" economist Paul Craig Roberts.³⁷

Michael Polanyi writes:

I first met questions of philosophy when I came up against the Soviet ideology under Stalin which denied justification to the pursuit of science. I remember a conversation I had with Bukharin in Moscow in 1935. Though he was heading toward his fall and execution three years later, he was still the leading theoretician of the Communist Party. When I asked him about the pursuit of pure science in Soviet Russia, he said that pure science was a morbid symptom of class society: under socialism the conception of science pursued for its own sake would disappear, for the interests of scientists would spontaneously turn to problems of the current Five-Year plan. I was struck by the fact that this denial of the very existence of independent scientific thought came from a socialist theory which derived its tremendous persuasive power from its claim to scientific certainty. The scientific outlook appeared to have produced a mechanical conception of man and history in which there was no place for science itself.³⁸

Polanyi's activity took the social form of the Society for Freedom in Science. In response to Bernal's *Social Function of Science*, John R. Baker, a zoologist at Oxford, wrote "Counterblast to Bernalism," while Polanyi had independently written in *The Manchester School* a critical article on Bernal.³⁹ The two opponents of Bernal made contact and the next year founded the Society for Freedom in Science.

The society began with a letter from Baker, followed by the formulation of statements of aims. The group remained relatively small,

and had three projects: (1) the writing of a book defending the autonomy of science in which Arthur Koestler, author of the novel *Darkness at Noon*, which fictionalized the Bukharin trial, was asked to participate; (2) the production of a pamphlet presenting a positive view of the social organization of science, rather than simply decrying the views of Bernal and Bukharin; and (3) the formation of local branches. Although little of the society's work came to fruition, Baker and Polanyi wrote books and articles of their own.⁴⁰ During the war Polanyi was able to capitalize on exposing the evils of Lysenkoism for biology in the Soviet Union.

After the war the views of Polanyi and Baker received a more favorable hearing than they had earlier from the most influential science magazine, *Nature*, and from the British Association. This was partly because the aims of science planning in the Association of Scientific Workers had been largely satisfied. Science planning had been accepted in a limited sense, and the public prestige of science because of its use in war made the benefits of "pure" science more popularly evident. Baker and Polanyi won a wider hearing also because of the deracialized and subsequently cold war attitude after 1945, and because they toned down and qualified their demands for freedom of scientific research.⁴¹ The Society for Freedom in Science members such as Polanyi later joined the Congress for Cultural Freedom, which, together with *Encounter*—both of them CIA-funded—continued to espouse the autonomy of science.

However, Baker and Polanyi had to grant that total autonomy made no sense in engineering and industrial research geared toward particular goals. Years later the unrealistic nature of the demand for total scientific autonomy was evident in Jacob Bronowski's article on "The Disestablishment of Science." Bronowski was not motivated by the Manchester School ideology of Polanyi, but rather by horror at the use of science by governments. Unfortunately his proposal for a total separation of science and government, similar to the separation of church and state (which itself is not fully adhered to), was utopian in an age when huge government funds were necessary for equipment and institutions. Bronowski proposed that scientists refuse to accept government funding for specific projects and demand a general fund from government with no strings attached, which representatives of the scientific community would then allocate to specific projects.⁴² Bronowski, in later defending his proposal against objections to the feasibility of getting governments to allocate a general fund with no demands, emphasized the role of private foundations as a funding source for science. This is somewhat ironic, given that the Rockefeller Foundation in effect determined the direction of molecular biology in

its crucial early years, despite a rhetoric of "freedom in science," as will be discussed below.

At the theoretical level, Polanyi later presented an account of scientific knowledge and of the community structure of science which was, according to him, as evidenced by the autobiographical passage above, meant to counteract the acceptance of Bukharin's science planning. In several works, the most important of which is *Personal Knowledge*,⁴³ Polanyi presents a theory of the role of authority in the scientific community. According to Polanyi, science is the very opposite of the open, democratic, critical community that Popper, for instance, envisages, let alone the "communitist" enlightenment community that Robert K. Merton portrays in his famous characterization of the ideals of science.⁴⁴ According to Polanyi, the authority of the intuitive judgments of prestigious scientists rules the scientific process. There are no useful explicit guidelines or "scientific methods." For Polanyi this authoritarianism of the scientific community has positive value because it allows for the discovery of scientific truth. Polanyi's portrayal of science is like the portrayal of the larger society by traditional conservative thinkers. There exists a natural elite (of scientific geniuses) who govern the customs and culture of the community through their intuitive good taste. This view was taken up to a certain extent by Thomas Kuhn, especially in his article "The Function of Dogma in Science," but without the old-European conservative aura.⁴⁵

There are certain paradoxes in this view, which Polanyi embraces as, in a sense, paradoxes of faith. The scientific sage can on occasion be wrong. However, this error is only corrected later by further dispensations of scientific sages. Polanyi accepted that his own early work in physical chemistry, which later turned out to be correct but which was rejected at the time, was correctly so suppressed.

Polanyi's general epistemology, which introduces the notion of "tacit knowledge," the aura or context of nonexplicit awareness that makes possible explicit perception, judgments, and actions, is beyond the scope of this chapter. Polanyi saw it as a counter to all forms of positivism, enlightenment rationalism, and critical philosophy. In certain respects his theory has affinities with the notion of context in Dewey's pragmatism and with prereflective awareness in existential phenomenology. The emphasis on skills and know-how counters the passive observer standpoint to an extent, and has some affinities with twentieth century "Western" Marxism, as in the work of Merleau-Ponty. Polanyi, however, identified Marxism with mechanistic thinking (as we saw in the quote above) and with enlightenment positivism. To the extent that Bukharin has been associated with the "mechanist" school of Marxism, he was an appropriate foil for Po-

lanyi. However, Bukharin's London lecture, much more than his earlier writings, emphasizes a praxis-oriented conception of the character of humans. Indeed, much of Bukharin's work in the exposition of Marxism in the 1930s puts more weight on the active, constructive aspect of Marxism than did his theoretical work in the early 1920s.

Interestingly, Bukharin, in his science planning speech, does briefly deal with the claims of defenders of the tacit and the unconscious. Incidentally, this was three decades before Polanyi's writings on this issue, and was in relation, presumably, to currents of German philosophy of the early twentieth century, and most directly to Freudian psychology of the unconscious, which is more mechanistic in its own way. Bukharin's brief remarks note that it is possible to have an irrationally proposed solution to a rationally posed problem, and that there is a dialectical interaction between rational knowledge and intuitive guesses. "A concrete guess is an unconscious continuation of a conscious process."⁴⁶

Experiments and computations hone the intuition, and science planning should take into account the role of intuition. Bukharin, however, believes that large laboratories that supply numerous experiments will foster this intuition. Bukharin's remarks are hardly adequate to deal with this large issue. His conception of the unconscious is rather like the rationalistic conception of, say, Helmholtz. His emphasis on the mutual interaction of experiment and calculation with intuition suggests Peirce, who of course strongly rejected infallible intuitions, and Popper, who allowed a large role for the "guess" but thinks it outside the realm of analysis. This is the famous distinction in the philosophy of science, characterized by Hans Reichenbach, between the context of discovery and the context of justification. But this passage does at least show that Bukharin attempts to take the issue into account.

Although the contemporary followers of Polanyi's philosophy have had little concern with Bukharin since the science planning debates of the 1940s and 1950s, the rehabilitation of Bukharin in the Soviet Union was the subject of a note from the general coordinator of the Polanyi Society.

The acute insight of Polanyi's thought turns up again as we witness the revival of the importance of Nikolai I. Bukharin in Soviet history. Bukharin is seen as the basis of *perestroika* [sic]. . . . Contrary to this optimism Polanyi reminds us that it was Bukharin that [sic] in 1935 described pure science as the morbid symptom of class society. . . . Despite Gorbachev's favor and the hope for reform, Polanyi claims that Bukharin represents the kind of objectivist Marxism that produces moral inversion.⁴⁷

Presumably Polanyi's criticism of Bukharin serves as a warning not to be overenthusiastic about Soviet reforms, or to succumb to "Gorbys fever."

Friedrich Hayek and Karl Popper both have criticized science planning (without explicit reference to Bukharin) by means of arguments concerning the prediction of knowledge that one does not yet possess and reflexive predictions, that is, predictions concerning one's own behavior.⁴⁸ Interestingly, it is the prediction of new knowledge, closely tied up with the notion of planning the content of science, which is crucial for Popper and very important for Hayek in their criticisms of planning in general. Hayek, like Polanyi, has of course argued against the possibility in principle of total planning. The arguments are directed at the straw man of the ideal of total planning of every detail and aspect of the economy.

Bukharin's own writing on economic planning emphasized the planning of the production of only certain major products, allowing flexibility concerning other elements of the economy. Similarly, in the planning of science Bukharin did not propose to plan future discoveries of knowledge.

As Graham notes, most of what Bukharin had to say dealt with planning *for* rather than planning *of* science. Bukharin was primarily concerned with the provision of supplies, facilities, training, and salary for scientists. It was this planning for science which eventually came to be generally accepted and implemented after World War II in all nations. The Soviet Union in the mid-1920s had led the rest of the world by several decades in making systematic assessments of scientific resources, and the United States, in 1938, evidently became second.

The "Freedom in Science" advocates themselves had proposed government economic support for specific scientists, based on the prestige of the scientist within the scientific community. As their position shifted in reaction to the growth of postwar research and development, Polanyi advocated support to specific research projects in fields related to technology. It was only Bronowski's later article that suggested eliminating any government support of specific scientists or projects to ensure scientific purity. The proponents of freedom of science also supported the planning of facilities and educational resources. Thus their defense of the autonomy of science was not absolute. Meanwhile the Association for Scientific Workers, which had advocated the planning of science, had become more cautious about the planning of the content of science. This was partly because of the horrible examples of state intervention in science supplied by Nazi race science and Soviet state support of Lysenkoist biology. It was also probably partly due to the fact that the non-Marxist members of the Association for Scientific

Workers and moderate advocates of planning such as those associated with the journal *Nature* became more conservative with the escape from the pressing economic problems of the Depression and the beginning of the Cold War. This led to a certain convergence between former Bernalists and the Polanyiites.

CONCLUSION

The issue of the possibility of planning the direction of scientific research is still disputed and unresolved. Sketchy as they were, Bukharin's proposals were the end, not the beginning, of specific, theoretically explicit proposals on these matters in the Soviet Union. The First All-Union Congress on science planning was never followed by a second. As Stalinization developed during the 1930s, explicit theoretical discussion in economics and theory of science was muted and suppressed. Science planning often became the cover for ideological intervention at the behest of power-political struggles and the expulsion of scientists for their political views. Lysenkoism was only the most widespread and intellectually most degraded example of this. Lysenkoism itself was a kind of magical, positive-thinking response to the agricultural crisis.⁴⁹

Discussion of science planning of a genuine sort (not simply the purging of scientists for power-political reasons) was for the most part limited to relatively inane discussions of note-keeping and information retrieval techniques. These were an outcome of earlier attempts to apply Taylorism to science. While these techniques were not entirely without value, they did not deal with the general problems of science planning. Bukharin's 1931 ideas were borrowed but not credited to Bukharin, especially after his trial and execution.

In the West, lip service was of course paid to the freedom and autonomy of the scientific workers, who were allegedly free to follow their curiosity in any direction. But even Polanyi and his allies had to grant that industrial and much engineering research was directed to specific goals. Even if specific discoveries could not be legislated, it is obvious that funding of topics of investigation and of certain methods and research programs, in preference to others, channels scientists in certain directions.

The fact that half the world's physicists and almost three-quarters of U.S. research and development funds are serving military-related research, channels workers in certain directions and depletes resources from certain topics. Seymour Melman has extensively documented this effect on engineering resources.⁵⁰ The leading economist and dean of the Massachusetts Institute of Technology School of Management Lester Thurow has made the flippanant remark that U.S. scientists prefer

to work on missiles rather than on toasters because the former are "sexier." This not only shows his preference for Freudian symbols but also his willful *naïveté* in neglecting the role of the social background of such judgments.⁵¹

Jacob Bronowski's hopes for the "disestablishment of science" seemed to presuppose that once direct government control of research funding was removed, research would be truly free of influence of social powers. But a good example of the "planning" of the nature of research by private charitable support is the support of "molecular biology" in its most reductionistic forms by the Rockefeller Foundation.⁵² In this case the content of the research supported certain views of the world (atomism, master molecule models, "master" genes versus "housekeeping" genes, protein production as assembly line, DNA as the administrative director who gets the credit for the results of the protein workers). Furthermore, among the scientists with a less reductionistic (but materialistic and nonvitalistic) alternative were precisely Bernal, Needham, and Needham's associate Waddington, who were ideologically suspect as well as proponents of alternative approaches.⁵³ It is particularly ironic that Warren Weaver, the Rockefeller Foundation manager of science largely responsible for directing the content of this major area of research, wrote a letter to the *New York Times* defending "freedom in science," which the Society for Freedom in Science reprinted as an occasional pamphlet. The introduction to the pamphlet states that "when Dr. Weaver wrote the letter he did not know of the existence of our movement. It is most interesting to notice how he has quite independently arrived at many of our chief ideas."⁵⁴

These examples only transpose into the context of science familiar Marxist arguments about replacing a "freedom" that is in fact planning for the interests of ruling classes with planning for the benefit of other groups.

The issues raised by Bukharin, the members of his delegation to Britain, and the followers and opponents of his doctrines on the social role of science and the desirability of the planning of science have been found to be more complex than they were thought to be by either side in the debate during the 1930s. Both sides qualified their positions as the debate went on. Practical, often makeshift compromises were found that embodied neither the full "freedom in science" advocated by Polanyi and Bronowski, nor the comprehensive rational planning of science advocated by Bukharin and Bernal. But these makeshift policy compromises in the Western capitalist countries and in the Soviet Union have hardly resolved the issues concerning the desirability of various kinds of scientific independence and social influences on science in contemporary society.

NOTES

1. Colin Holmes, "Bukharin in England," *Soviet Studies* 24:1 (July 1972), p. 86.
2. David Joravsky, *The Lyсенko Affair* (New York: Columbia University Press, 1970), pp. 30–36, 89–90; Stephen J. Gould, "A Hearing for Vavilov," in *Hen's Teeth and Horse's Toes* (New York: W. W. Norton, 1984), pp. 134–44.
3. Helena Sheehan, *Marxism and the Philosophy of Natural Science* (New York: Humanities Press, 1985), p. 243.
4. Sidney Hook, *Out of Step* (New York: Carroll and Graf Publishers, 1988), pp. 408, 414–17.
5. Pavel Kovaly, *Rehumanization or Dehumanization?* (Boston, MA: Branden Press Publishers, 1974), pp. 35–64. Arnost Kolman, *Considerations on the Certainty of Knowledge*, AIMS Occasional Papers, no. 2 (New York: American Institute of Marxist Studies, 1965).
6. Gary Werskey, *The Visible College* (New York: Holt Rinehart and Winston, 1978), pp. 138–46; Neal Wood, *Communism and the British Intellectuals* (New York: Columbia University Press, 1959), pp. 123–25; Sheehan, *Marxism and the Philosophy of Natural Science*, pp. 305–8; Holmes, "Bukharin in England," pp. 86–89.
7. Holmes, "Bukharin in England," p. 90.
8. Wood, *Communism and the British Intellectuals*, p. 124. Actually, the printing was suggested by Lancelot Hogben: Werskey, *The Visible College*, p. 140.
9. Nikolai I. Bukharin, et al., *Science at the Crossroads* (London: Kniga, 1931); 2nd edn. (London: Cass Reprints, 1971) with prefaces by Gary Werskey and Joseph Needham. Hessen's contribution was republished as Boris Hessen, *The Economic and Social Roots of Newton's Principia*, introd. Robert S. Cohen (New York: Howard Fertig, 1971).
10. See Werskey, *The Visible College*, pp. 140–41.
11. George N. Clark, co-author of the paper, was later to reply to Hessen in G. N. Clark, *Science and Social Welfare in the Age of Newton* (Oxford: Clarendon Press of Oxford University Press, 1937). The other major rebuttal of Hessen is A. Rupert Hall, *Ballistics in the Seventeenth Century* (Cambridge: Cambridge University Press, 1952). See George Basalla, ed., *The Rise of Modern Science: External or Internal Factors?* (Lexington, MA: D. C. Heath and Company, 1968), pp. 106–7, and Michael Hunter, *Science and Society in Restoration England* (Cambridge: Cambridge University Press, 1981), p. 208. Hall never mentions Hessen or Bukharin by name.
12. Holmes, "Bukharin in England," pp. 87–88.
13. Sheehan, *Marxism and the Philosophy of Natural Science*, pp. 306–7; Werskey, *The Visible College*, pp. 144–45; Maurice Goldsmith, *Sage* (London: Hutchinson, 1980), pp. 59–60.
14. Sheehan, *Marxism and the Philosophy of Natural Science*, pp. 350–85.
15. Robert S. Cohen, "Hessen: Beyond Science at the Crossroads," paper delivered at the Boston Colloquium for the Philosophy of Science, Symposium on "Marx and Science," 12 February 1983.
16. Margaret Jacob, *The Newtonians and the English Revolution 1689–1720* (Ithaca, NY: Cornell University Press, 1976).
17. Werskey, *The Visible College*, p. 142.
18. Jon Elster, *Making Sense of Marx* (Cambridge: Cambridge University Press, 1985), p. 508; Jon Elster, *An Introduction to Karl Marx* (Cambridge: Cambridge University Press, 1986), p. 184.
19. Loren Graham, "The Social and Economic Roots of Boris Hessen: Soviet Marxism and the History of Science," *Social Studies of Science* 15:4 (November 1985), pp. 705–22.
20. Werskey, *Visible College*, is a collective biography of these five figures. On Haldane is Ronald Clark, *J. B. S.* (Oxford: Oxford University Press, 1968). On Bernal is Goldsmith, *Sage*. Neither work is up to the level of the science and politics of its subject. Haldane appears as Shearwater in Aldous Huxley, *Antic Hay* (London: Chatto and Windus, 1923). Haldane also appears as Mr. Codling in Ronald Fraser's *The Flying Drifter* (London: T. F. Unwin, 1924). See Clark, *J. B. S.*, pp. 57, 73. Bernal is portrayed as Constantine in [Charles] P. Snow, *The Search*, (New York: Charles Scribner's Sons, 1958). See Goldsmith, *Sage*, p. 165.
21. Bernal, quoted in William McGucken, *Scientists, Society, and State* (Columbus, OH: Ohio State University Press, 1984), p. 73.
22. [John] D. Bernal, *The Social Function of Science* (Cambridge: MIT Press, 1967); *Science in History*, 4 vols. (Cambridge: MIT Press, 1971).
23. [John] B. S. Haldane, *Science and Everyday Life* (London: Macmillan, 1940).
24. Richard Levins, lecture given at the Council for Philosophical Studies Summer Institute on Human Nature, 1 July 1977.
25. Joseph Needham, "Forward," in *Science at the Crossroads*, 2nd edn., pp. viii–ix.
26. Joseph Needham, *Science and Civilization in China*, 7 vols. (Cambridge: Cambridge University Press, 1954–).
27. George P. Wells, "Lancelot Thomas Hogben," *Biographical Memoirs of the Royal Society* 24 (London: The Royal Society, 1978), pp. 183–221; Lancelot T. Hogben, *Mathematics for the Millions*, 4th edn. (London: George Allen and Unwin, 1967).
28. Hyman Levy, *A Philosophy for a Modern Man* (New York: Alfred A. Knopf, 1938); Bertell Ollman, *Alienation* (Cambridge: Cambridge University Press, 1971).
29. Stephen F. Cohen, *Bukharin and the Bolshevik Revolution: A Political Biography 1888–1938* (New York: Alfred A. Knopf, 1973), pp. 466–67. Cohen provides only three footnotes on Bukharin's work in science.
30. Loren Graham, *The Soviet Academy of Science and the Communist Party* (Princeton, NJ: Princeton University Press, 1967), pp. 57–60; Graham, "Bukharin and the Planning of Science," *The Russian Review* 23:2 (April 1964), pp. 135–48; Nikolai Bukharin, *The Soviets Plan Science*, trans. Andrew Rothstein (London: Williams and Norgate, 1932); Sidney Heitman, *Nikolai I. Bukharin: A Bibliography* (Stanford, CA: Hoover Institution, 1969), p. 147, ref. 898.
31. Graham, *Soviet Academy*, p. 57.

32. Graham, "Bukharin and the Planning of Science," p. 143.
33. Graham, *Soviet Academy*, p. 59.
34. William McGucken, *Scientists, Society, and State*, Werskey, *The Visible College*, pp. 234-57.
35. Werskey, *The Visible College*, pp. 239, 320.
36. Eugene P. Wigner and R. A. Hodgkin, "Michael Polanyi," *Biographical Memoirs of Fellows of the Royal Society* 23 (London: The Royal Society, 1977), p. 413; David Kettler, "Culture and Revolution," *Telos* 10 (Winter 1971), p. 49; Karl Polanyi, *The Great Transformation* (Boston, MA: Beacon Press, 1957); Karl Polanyi, *Primitive, Archaic, and Modern Economies* (Garden City, NY: Doubleday & Company, 1968), pp. 101-2, 105-6.
37. Paul Craig Roberts, *Alienation and the Soviet Economy* (Albuquerque, NM: University of New Mexico Press, 1971) is based on Michael Polanyi's thesis concerning the nonexistence of economic planning.
38. Michael Polanyi, *The Tacit Dimension* (Garden City, NY: Doubleday, 1966), p. 3.
39. John R. Baker, "Counterblast to Bernalism," *New Statesman and Nation* 29 (July 1939), pp. 174-75; Michael Polanyi, "Rights and Duties of Science," *The Manchester School of Economic and Social Studies* 10 (1939), pp. 175-93.
40. Michael Polanyi, *The Planning of Science*, Society for Freedom in Science Occasional Pamphlet, 4 (Oxford: The Society, 1946); John R. Baker, *The Scientific Life* (New York: Macmillan, 1943); J. R. Baker, *Science and the Planned State* (New York: Macmillan, 1945).
41. McGucken, *Scientists, Society, and State*, pp. 265-305, 351-56.
42. Jacob Bronowski, "The Disestablishment of Science," *Encounter* 37:1 (July 1971), pp. 9-16, 96.
43. Michael Polanyi, *Personal Knowledge: Toward a Post-Critical Philosophy of Science* (Chicago, IL: University of Chicago Press, 1962), pp. 216-19; Polanyi, *The Tacit Dimension*, pp. 55-86.
44. Robert K. Merton, *The Sociology of Science* (Chicago, IL: University of Chicago Press, 1973), pp. 268-78.
45. Thomas Kuhn, "The Function of Dogma in Science," in *Scientific Change*, ed. Alastair Crombie (New York: Basic Books, 1963), pp. 347-69; M. Polanyi, "Commentary," in Crombie, *Scientific Change*, pp. 375-80; Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago, IL: University of Chicago Press, 1962), p. 44.
46. Graham, "Bukharin and the Planning of Science," pp. 137-38.
47. Richard Gelwick, "Preface: The Revival of Bukharin's Image," *Tradition and Discovery* 15:1 (Winter 1987-88), p. 2.
48. Karl Popper, *The Poverty of Historicism* (New York: Harper & Row, 1964), pp. 13-16; Friedrich A. Hayek, *The Counterrevolution in Science* (New York: The Free Press, 1955), pp. 87-91.
49. Lysenkoism, for all its destruction of the science and scientists of orthodox genetics in the Soviet Union, may not have hurt agricultural production. Soviet agriculture's problems had to do with collectivization and peasant unwillingness to work for the collectives. See Joravsky, *The Lysenko Affair*,

- pp. 78-86, and Richard Lewontin and Richard Levins, *The Dialectical Biologists* (Cambridge, MA: Harvard University Press, 1985), p. 163-96.
50. Seymour Melman, *The Permanent War Economy* (New York: Simon and Schuster, 1985), p. 23.
51. Lester Thurow, lecture given at the University of New Hampshire, 7 March 1981; Thurow, "How to Destroy the U.S. Economy," *New York Review of Books* 28:8 (14 May 1981), p. 6.
52. Prina Abir-Am, "The Discourse of Physical Power and Biological Knowledge in the 1930's: A Reappraisal of the Rockefeller Foundation's Policy in Molecular Biology," *Social Studies of Science* 12:3 (1983), pp. 341-82; Edward Yoxen, "Giving Life a New Meaning: The Rise of the Molecular Biology Establishment," in *Scientific Establishments and Hierarchies*, eds. Norbert Elias, Hermínio Martins, and Richard Whitley (Boston, MA: D. Reidel Publishing, 1982), pp. 123-43.
53. Prina Abir-Am, "The Assessment of Interdisciplinary Research in the 1930s: The Rockefeller Foundation and Physico-chemical Morphology," *Misneria* 26:2 (Summer 1988), pp. 153-76.
54. Warren Weaver, "Free Science," Society for Freedom in Science Occasional Pamphlet, no. 3 (Oxford: The Society, 1945).