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Innovation in Science—the Case of Cybernetics in the Soviet Union

DAVID HOLLOWAY
Department of Politics,
*University of Edinburgh**

Cybernetics is one of the notable cases in which the growth of scientific knowledge has appeared to come into conflict with political authority in the Soviet Union. Although it is now a prominent area of research and discussion in the Soviet Union, cybernetics was condemned in the Soviet press in the early 1950s as a reactionary pseudoscience. The public reception given to it was uniformly hostile. The chief attack, which appeared in 1953 under the title 'Whom Cybernetics Serves', declared that it served the interests of the reactionary *bourgeoisie* and reflected its desire to replace potentially revolutionary human beings with machines which would pliantly carry out the commands of the imperialists and militarists.[1] But these hopes, it was asserted, were futile, for cybernetics was a false science, destined to perish even before imperialism perished. Other wholesale refutations appeared in the early 1950s; but by 1958 cybernetics was acknowledged as a legitimate field of research, and in 1959 a Science Council for Cybernetics was established in the USSR Academy of Sciences. In 1961 the first of a series of collected papers was published under the Council's auspices. As if in reply to the earlier attack, it bore the title *Cybernetics—to the Service of Communism.*[2]

Since the late 1950s cybernetics has gained recognition in the Soviet Union as an over-arching or synthesizing theory in science and technology. It has been remarked by one writer that

> . . . in the United States, scientists and engineers working in the theory of self-regulation tend to avoid the term cybernetics which deals to a considerable degree with isomorphisms among various types of self-regulating systems. Since only a

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*31 Buccleuch Place, Edinburgh EH8 9JT, Scotland, UK.

[1] 'Materialist', 'Komu sluzhit kibernetika', *Voprosy filosofii* (1953, 5), 219.
[2] A. I. Berg (ed.), *Kibernetiku—na sluzhbu kommunizmu*, 1 (Moscow-Leningrad, 1961).

very limited range of systems and communications processes are presently amenable to mathematical formalization and manipulation, there has been a tendency to institutionalize fairly narrow disciplines concerned with limited formal or material applications of those concepts, such as computer engineering, bionics, and control systems engineering. In the Soviet Union, on the other hand, the term cybernetics is used quite broadly.[3]

There exists in the Soviet Union a school of cybernetics; and although one of its leaders pointed out in 1967 that many branches of cybernetics—the theory of information systems, pattern recognition and bionics for example—had become independent scientific disciplines, it is nevertheless true that in the Soviet Union these disciplines have been fostered by cybernetics and are still seen as part of the general field.[4] Thus Soviet cybernetics is remarkable both for the initial difficulties and for the ultimate success of its development. Consequently it raises interesting questions about the processes of innovation in science.

The reversal of fortunes prompts one to ask whether the conception of cybernetics which was accepted as useful and legitimate in the late 1950s was the same as that which had been rejected earlier in the decade. Or had it been divested of objectionable values and assumptions, and thus rendered acceptable to Marxist-Leninists? These questions can be answered only by comparing the initial objections with what was finally accepted. A more interesting problem arises when one asks by what processes cybernetics came to be recognized as a legitimate and fruitful field of research. In order to answer this question it is necessary to look, if only briefly, at some of the recent discussion which has taken place in the sociology of science about the processes of innovation. This will provide methodological guidelines for analyzing the case of Soviet cybernetics. At the same time, however, I shall argue that this case-study illustrates certain deficiencies in the current discussion of innovation and points, therefore, to the need for further work in this area.

Innovation in Science

It is on innovation that the growth of scientific knowledge depends;[5] and it is therefore not surprising that innovation occupies a central place in the

[3] Charles R. Dechert, 'The Development of Cybernetics', in Amitai Etzioni (ed.), *A Sociological Reader on Complex Organisation*, 2nd edition (New York: Holt, Rinehart and Winston, 1969), 107.

[4] See the survey of work in the various areas of cybernetics given in A. I. Berg (ed.), *Kibernetiku—na sluzhbu kommunizmu*, 5 (Moscow, 1967).

[5] For note 5, see next page.

philosophy and sociology of science. This place has been further highlighted in recent years by Kuhn's work on scientific revolutions, and by the critical revision—partly inspired by Kuhn's analysis—of Merton's interpretation of the normative controls in the scientific community.[6] Kuhn has emphasized the way in which paradigms provide the framework within which a community conducts its research. The activity of normal science continues until anomalies in that activity precipitate a crisis, which can be resolved only by the adoption or creation of a new paradigm. While Kuhn stresses that 'normal science' is the customary activity of scientists, he points at the same time to the discontinuous and revolutionary character of scientific progress. He argues that the 'issue of paradigm choice can never be unequivocally settled by logic and experiment alone' and declares that

> . . . as in political revolutions, so in paradigm choice—there is no standard higher than the assent of the relevant community. To discover how scientific revolutions are effected, we shall therefore have to examine not only the impact of nature and logic, but also the techniques of persuasive argumentation effective within the quite special groups that constitute the community of scientists.[7]

Kuhn's view that competing paradigms are incommensurable has led some of his critics to accuse him of attributing to 'irrationality', 'subjectivity', and 'mob psychology' the motive power of scientific progress.[8] But Kuhn has responded that the reasons he advances for theory choice are

> . . . reasons of exactly the kind standard in philosophy of science: accuracy, scope, simplicity, fruitfulness, and the like. It is vitally important that scientists be taught

[5] I here accept the current practice, among economists and others, of distinguishing between 'invention' and 'innovation'—the latter being an invention which is successful, and is hence adopted, widely diffused and established. But I am, of course, referring here to the introduction of new ideas and results into scientific knowledge, and not to technological innovation. By innovation I mean a new idea (paradigm, theory, model, hypothesis) which comes to be accepted by a specialist community as part of scientific knowledge; innovation may also refer to the process whereby this happens. The distinction should be clear from the text.

[6] Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: Chicago University Press, 1962 and 1970); R. K. Merton, 'Science and Democratic Social Structure', in *Social Theory and Social Structure* (Glencoe, Illinois: The Free Press, 1957 and 1967). Criticisms of the Mertonian norms may be found in Michael Mulkay, 'Cultural Growth in Science', in B. Barnes (ed.), *Sociology of Science* (Harmondsworth: Penguin Books, 1972), 126–42 and Leslie Sklair, *Organised Knowledge* (London: Paladin, 1973), chapter 4.

[7] *Op. cit.* note 6, 2nd edition, 94. It does not matter for the present argument whether or not cybernetics constitutes or embodies a Kuhnian paradigm, since the focus of my analysis is the community rather than the theory.

[8] Kuhn, 'Reflections on my Critics', in Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge* (London: Cambridge University Press, 1970), 259 ff.

to value these characteristics and that they be provided with examples that illustrate them in practice.[9]

And elsewhere he writes of

> . . . the manner in which a particular set of shared values interacts with the particular experiences shared by a community of specialists to ensure that most members of the group will ultimately find one set of arguments rather than another decisive.[10]

It appears from this that two sets of values must be taken into account in examining the processes of innovation in science: those values which bind a community together even while it is in a state of crisis (i.e. divided on the issue of paradigm or theory choice); and those norms which derive from the commitment to a particular paradigm. The former set of values provides the basis for that process of persuasion and partial communication by which, in Kuhn's analysis, the choice between incommensurable paradigms is made. The latter are the cognitive and technical norms which 'define which problems and techniques are legitimate and what kinds of solutions are acceptable', and are of no help in paradigm choice because they are paradigm-dependent.[11] The relationship between the two sets of values is clearly central to an understanding of scientific progress. As Kuhn remarks

> . . . whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains.[12]

Kuhn's emphasis on the role of dogma in science, and his attack on the image of the ideal scientist as objective and open-minded, has had an important influence on the sociology of science. It has helped to undermine Merton's analysis of the institutional imperatives of science (universalism, communism, disinterestedness, organized scepticism) by suggesting that science cannot be characterized as 'organized scepticism' and that, moreover, the growth of scientific knowledge is to be explained in terms not of a general norm of scepticism, but rather of the scientist's commitment to cognitive and technical

[9] *Op. cit.* note 8, 261.

[10] *Op. cit.* note 6, 2nd edition, 200.

[11] Kuhn has clarified his notion of paradigm in *op. cit.* note 6, 2nd edition, 175, where he writes that 'on the one hand it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples can replace explicit rules as a basis for the solution of the remaining puzzles of normal science'. See also Margaret Masterman, 'The Nature of a Paradigm', in Lakatos and Musgrave (eds.), *op. cit.* note 8. I have taken the description of cognitive and technical norms from Michael Mulkay, *The Social Processes of Innovation* (London: Macmillan, 1972), 15; cf. also 33 ff.

[12] *Op. cit.* note 8, 238.

norms. This is clear especially in Mulkay's work on the social processes of innovation, which attempts to extend Kuhn's analysis by rooting it in the social structure of science. Mulkay argues that researchers gain recognition from their scientific colleagues in return for results which are original yet do not depart from the generally accepted framework of cognitive and technical norms in the research community: research conducted within this framework is what Kuhn calls 'normal science'. Normal science comes to an end and innovation takes place, in Mulkay's view, because researchers compete with each other for the scarce resource of professional recognition, and this competition ensures that as significant problems are eliminated within a given area, the rewards for normal research in that area decline. Consequently the search for recognition leads either to rebellion against the current orthodoxy or to migration into new fields. Mulkay thus explains the social processes of innovation in terms of social exchange, or more precisely, of the information-recognition exchange model.[13]

Mulkay thus succeeds in extending Kuhn's work by explaining the processes of innovation in the light of the mechanisms of social control within the scientific community. He points to three such mechanisms, by which intellectual conformity is maintained in science: socialization, the exercise of authority, and social exchange; but he analyzes innovation in terms of this last mechanism alone, and does not examine that stratum of values which, in Kuhn's analysis, provides the basis on which persuasion and partial communication can be conducted in times of crisis. It is possible that this basis is provided by socialization and the exercise of authority. Yet if Kuhn's analysis of scientific progress is to be rooted more clearly in sociological explanation, it is vital that this stratum of values be examined, for it is on this level that Kuhn rejects his critics' accusations that he has brought in irrationality, subjectivity and mob psychology as a means of explaining the choice between incommensurable paradigms. Thus even if one agrees with Mulkay that 'scientific theory and methodological rules operate as the dominant source of controls in science',[14] the idea of more general normative controls cannot (at least if one follows Kuhn) be thrown out altogether. This of course does not imply that the norms need to be those specified by Merton.

This problem of the norms and values which bind the scientific communities together plays an important part in the Soviet discussion of cybernetics

[13] See Mulkay, *op. cit.* note 11; also W. O. Hagstrom, *The Scientific Community* (New York: Basic Books, 1965), for the full development of the information-recognition exchange model.
[14] *Op. cit.* note 6, 126.

in the 1950s: and two further points should be made here about the social processes of innovation. Firstly, Kuhn argues that

> . . . unlike most disciplines, the responsibility for applying shared scientific values must be left to the specialists' group. It may not even be extended to all scientists, much less to all educated laymen, much less to the mob. If the specialists' group behave as a mob, renouncing its normal values, then science is already past saving.[15]

The successful or normal functioning of the processes of innovation is thus seen to depend critically upon the locus of scientific authority: the specialist community must have the right to say what constitutes scientific truth. If the specialist community does not possess this right, it will not have control over the granting of professional recognition; for those who claim the right to define scientific truth claim also—by implication at least—the right to say what constitutes a contribution to scientific knowledge. The social process of innovation would be seriously affected if the locus of recognition were removed outside the specialist community, for social exchange—the mechanism of social control most relevant to innovation—would be extended to include non-specialists; scientific progress would thus be threatened.

Secondly, although I have focused in this paper on innovation in terms of the internal working of the scientific community, it is clear that external factors cannot be ignored. Even in an analysis based solely on the internal mechanisms of social control, the distinction between internal and external factors may break down. A group other than the specialist community may claim scientific authority, thereby affecting the processes of social exchange. Socialization and authority relations in science may be strongly influenced by the social and organizational context. The relationship between internal and external factors may differ from one social system to another. Moreover, the 'techniques of persuasive argumentation' used in the process of paradigm choice may be influenced by the social context, either because the scientists themselves share certain non-scientific or extra-scientific values, or because external authorities, whether cultural (for example, the Church) or economic (for example, fund-givers), have to be propitiated.[16]

Structure of Stalinist Science

Norbert Wiener's *Cybernetics* was published in 1948, at a time of difficult relations between science and politics in the Soviet Union. In 1948 Stalin

[15] *Op. cit.* note 8, 263.

[16] For a general discussion of the need to propitiate fund-givers see J. R. Ravetz, *Scientific Knowledge and its Social Problems* (London: Oxford University Press, 1971), chapter 10.

intervened personally in biology to support Lysenko in his battle against 'Mendelist–Morganist genetics'. The famous session of the Lenin Academy of Agricultural Sciences in July and August of that year was followed by a widespread purge of geneticists and plant physiologists from their teaching and research positions.[17] In 1950 a joint session of the Presidia of the USSR Academy of Sciences and of the Academy of Medical Sciences launched a purge of physiologists who had attempted to revise Pavlovian theories.[18] Other fields of science suffered too: quantum mechanics and relativity theory, and the theory of resonance in structural chemistry, came under attack for their supposed incompatibility with dialectical materialism.[19] In contrast to the situation in biology, however, these attacks seem to have had little, if any, effect on scientific research in these areas. The attacks on cybernetics as a reactionary pseudoscience were not, therefore, a unique case of the rejection of a scientific field on philosophical grounds; and in order to understand it one must look more closely at the broader pattern of which it forms a part.

This can best be done by examining, in a rather schematic way, the assumptions and principles which governed the relationship between science and politics under Stalin. The most important factor in this relationship was the determination to harness natural scientists and their work to practical—primarily economic and military—ends. This determination is not, of course, peculiar to the Stalinist leadership. But the Bolshevization of Soviet science between 1928 and 1932 introduced several crucial elements into the relationship. The first of these was the peculiar interpretation given to the principle that practice is the criterion of truth: this was taken to mean, in effect, that practical economic results were the basis on which the truth or falsity of natural scientific theories should be judged. It was this dialectic that made it possible for the crisis in agriculture to be transformed into a crisis in biology. Since it was the Stalinist leaders who were the arbiters of successful

[17] Most of the session is contained in *The Situation in Biological Science, Proceedings of the Lenin Academy of Agricultural Sciences of the USSR, Session July 31–August 7, 1948* (Moscow, 1949). For the background to the session, and an analysis of its significance, see David Joravsky, *The Lysenko Affair* (Cambridge, Mass.: Harvard University Press, 1970), 130–43; and Zhores Medvedev, *The Rise and Fall of T. D. Lysenko* (New York: Columbia University Press, 1969), 103–36.

[18] See 'Problemy fiziologicheskogo ucheniya I. P. Pavlova' (Nauchnaya Sessiya AN SSSR i Akademii meditsinskikh nauk SSSR), *Vestnik Akademii Nauk* (1950, 7), 68–118, and *Scientific Session on the Physiological Teaching of Academician I. P. Pavlov* (Moscow, 1951).

[19] For an extensive account of the 'crises' in quantum mechanics, relativity theory and structural chemistry see Loren R. Graham, *Science and Philosophy in the Soviet Union* (New York: Alfred A. Knopf, 1972), chapters 3, 4 and 8.

practice, they too could adjudicate the truth or falsity of a scientific theory.[20]

The criterion of practice was given its Stalinist interpretation within a particular framework of assumptions about the relationship between natural science, philosophy and politics. Natural science was seen as part of the social superstructure and consequently was thought to develop in response to the economic demands of society rather than in accordance with an internal logic; as part of the social superstructure, socialist science must therefore be different from *bourgeois* science.[21] At the same time it was claimed that dialectical materialism could lead the scientist to new discoveries, and even to the reconstruction of science: there were indeed sporadic attempts in the early 1930s to reconstruct various disciplines on the basis of dialectical materialism.[22] What made this relationship between science and philosophy so important was the fact that the debate which had flourished in the philosophy of science in the 1920s was brought to an abrupt end in 1931, and the final source of authority in philosophy was now clearly located

[20] On the Stalinist interpretation of the principle that practice is the criterion of truth see David Joravsky, *Soviet Marxism and Natural Science, 1917–32* (London: Routledge and Kegan Paul, 1961), chapter 17, which analyzes the immediate consequences of Stalin's lapidary statement in 1929 that 'it must be admitted that theoretical thought is not keeping pace with our practical progress, that there is a certain gap between our practical progress and the development of theoretical thought'. See also Joravsky, *op. cit.* note 17, 237–44; 249; 271–306. For an analysis of the 'Bolshevization' of the Academy of Sciences see Loren R. Graham, *The Soviet Academy of Sciences and the Communist Party, 1927–32* (Princeton, N.J.: Princeton University Press, 1967), and V. D. Yesakov, *Sovyetskaya nauka v gody pervoi pyatiletki* (Moscow, 1971).

[21] See Loren R. Graham, *op. cit.* note 20, 37–8. For a recent Soviet account of some of the efforts made to ensure that research in the Academy of Sciences did respond to economic demands see V. A. Ul'yanovskaya, *Formirovanie nauchnoi intelligentsii v SSSR 1917–37 gg.* (Moscow, 1966), 165–71. For a militant statement of the distinction between Soviet science and bourgeois science—including natural science—see the unsigned article 'Za patrioticheskuyu sovyetskuyu nauku', *Vestnik Akademii Nauk* (1949, 4), 3–14. Stalin's *Concerning Marxism in Linguistics* (1950) did renew the discussion about whether or not science belonged to the base or superstructure. In the same work Stalin attacked 'Arakcheev régimes' in science and declared that science could not develop without a conflict of views and freedom of criticism. On the importance of this see Joravsky, *op. cit.* note 17, 151–7. The recognition in science was not, however, relevant to the initial attacks on cybernetics, for, as was pointed out in *Vestnik Akademii Nauk* (1950, 8), 12, a distinction has to be drawn between 'criticism which is directed towards eliminating mistakes, inaccuracies, and assertions which have become moribund or obsolete or have been disproved by experiment, in the works of Soviet scholars' and 'criticism directed against the *bourgeois* world-view which is hostile to science, against the enemy's attempt to smuggle in elements of that world-view into Soviet science, against ideological contraband. Soviet scholars do not discuss, but unmask and smash to smithereens the antiscientific fabrications of the scholarly lackeys of the aggressive *bourgeoisie*'. Cybernetics was initially judged to fall into the latter category.

[22] See Joravsky, *op. cit.* note 20, chapters 16 to 19.

in the Central Committee and in Stalin personally.[23] And since it was asserted that philosophy could lead the scientists to new discoveries, scientific problems now became subject to adjudication by the Central Committee. Thus scientific authority—the right to say what constitutes scientific truth—was vested in the Central Committee.

The complex politics of Stalinist science cannot be explained without reference to the criterion of practice, and to the framework within which it was interpreted; but equally, these do not in themselves explain the individual crises. Besides, it cannot be inferred that the Stalinist leaders held a consistent attitude towards the scientific community. Anxiety and mistrust were most intense in the periods 1929 to 1932, 1936 to 1938, and from 1947 to 1952. In this latter period suspicion was increased by the tensions of the Cold War and attendant fears about the loyalty of scientists. These tensions were reflected in the ideological drive launched by the Party to combat alien influences in intellectual life.[24] Important though this drive was, however, it does not account for the political interventions in science in the late 1940s. Joravsky shows, for example, that it was the agricultural officials who presided over the triumph of Lysenkoism in 1948, while the ideological overlords were sceptical. By 1951 a curious reversal of roles had taken place: the agricultural authorities, who had supported Lysenkoism since the early 1930s, showed signs of withdrawing support, while the ideological establishment, which had temporized until 1948, now began to endorse Lysenko unequivocally.[25]

These complexities underline the fact that to point to the basic principles of the Stalinist relationship between science and politics is not to explain the individual crises. Nevertheless, given the Stalinist leaders' fear that political purposes were actually being thwarted by natural scientists—for example, marine biologists might purposely give wrong advice about fishing grounds, or geneticists wrong advice about wheat varieties—these underlying assumptions led naturally to a complex system of political and bureaucratic controls

[23] See the 1931 declaration by M. B. Mitin, leader of the philosophical 'Bolshevizers', that 'Comrade Stalin's instructions must be placed at the foundation of all future work in philosophy', quoted by Joravksy, *op. cit.* note 20, 262. Also Mitin's statement in 1933 that 'in all Comrade Stalin's practical achievements, and in all his writings, there is set forth the whole experience of the world-wide struggle of the proletariat, the whole rich store-house of Marxist-Leninist theory', quoted in G. Wetter, *Dialectical Materialism* (London: Routledge and Kegan Paul, 1958), 177.

[24] An analysis of the organization of the Academy of Sciences and of the ethos of Soviet science during the last years of Stalin's rule can be found in Alexander Vucinich, *The Soviet Academy of Sciences* (Stanford, California: Stanford University Press, 1956).

[25] For these complexities see Joravsky, *op. cit.* note 17, 153, 240 ff.

on the scientific community. Certain features of the behaviour of Soviet scientists can be explained in terms of this system of controls. Of particular interest, for present purposes, are those relating to the way in which scientific argument was conducted. Among the techniques used in Stalinist science, and subsequently condemned, were: quotation-mongering—appealing to the classics of Marxism–Leninism in support of one's arguments; and label-sticking—the attempt to defeat an opponent's argument by associating it with a political or philosophical deviation. These were symptomatic of the politicization of scientific authority and indicated an appeal to the political leadership, rather than to scientific colleagues, for recognition and approval. The use of 'administrative methods' to defeat one's opponents by removing them from their posts was symptomatic of the bureaucratization of science and the association of scientific authority with position in the administrative hierarchy.[26]

It can be seen that these 'techniques of persuasive argumentation' were symptomatic of a shift in the locus of recognition away from the community of specialists. In Stalinist science political recognition was sought along with the recognition of scientific colleagues; and the mechanisms of social control in the scientific community were supplemented by political controls which sought to ensure political and philosophical conformity. The Stalinist philosopher had an important role to play in this system of controls. During and after the Bolshevization of science, scientists were pressed into declaring their allegiance to Marxism–Leninism, and lest their professions of loyalty be mere hypocrisy, militant philosophers pursued them into their discipline to root out whatever philosophical or political deviations their work might betray.[27] In 1956 one of the leading Soviet philosophers of science outlined the consequences of what he called the dogmatic Stalinist conception of Marxism:

[26] See the resolution adopted at the meeting of the Moscow *aktiv* of the Academy of Sciences after the 20th Party Congress. This called for the 'overcoming of dogmatism and quotation-mongering and also the liquidation of the consequences of the personality cult which is alien to Marxism'. It also criticized 'unprincipled and unobjective attacks on the basis of hostile personal relationships' and 'the use of the authority of a great scholar to stifle other opinions and suppress criticism' (*Vestnik Akademii Nauk* [1956, 6], 49). In February, 1956, the President of the Academy of Sciences declared that biologists should be allowed to 'work and conduct scientific discussions and arguments without label-sticking or using in scientific discussion unscientific arguments' (*Vestnik Akademii* [1956, 3], 14). I am not suggesting that all Soviet scientists' behaviour during the Stalinist period can be attributed to this system of controls, nor am I claiming that it has been completely dismantled. But it seems impossible to explain the various crises in Stalinist science without reference to this system of controls.

[27] See Joravsky, *op. cit.* note 20, chapters 16 to 19.

A set of quotations from the work of J. V. Stalin instead of a profound investigation of the problem, the didacticism of philosophers in their treatment of scientists, the philosophical—if one can call it that—summons, and even sometimes the philosophical cudgel (*filosofskaya dubinka*), which under the name of the criticism of idealism were directed against scientists who dared to say anything disagreeable to the philosopher-mentor—all this and much else was the result of the cult of the personality.[28]

In so far as philosophers were responsible for ensuring adherence to the cognitive norms of dialectical materialism in natural science, they could act as 'gatekeepers', preventing, or at least delaying, the publication of scientific theories which seemed to require new philosophical thinking. In other words, their role is important not merely by virtue of their philosophical responsibilities, but also because of their organizational powers. In this way they could impede the dissemination of new scientific ideas and thus lessen the opportunities for gaining professional recognition for innovative research. Moreover, the professional recognition to be gained might be counterbalanced by the political costs of being suspected of adhering to a philosophical—and hence political—deviation. These features of Stalinist science complicated the social process of innovation. They played an important role in the debate about cybernetics in the mid-1950s: indeed, the fate of cybernetics became bound up in discussion about the locus of scientific authority and the 'techniques of persuasive argumentation' appropriate to scientific debate.

The reasons why cybernetics was rejected, and the issues that arose in its subsequent legitimation, cannot properly be understood without reference to this background. Some Soviet scientists have attributed the initial public hostility to cybernetics solely to the baneful influence of the philosophers. In 1962 Academician P. L. Kapitsa remarked caustically that

> . . . had our scientists back in the year 1954 paid attention to the philosophers, had they accepted that definition [of cybernetics as a reactionary pseudoscience] as a guide to further development of this particular science, we may safely say that our conquest of space, of which we are so proud and for which the whole world respects us, could never have been a reality, since it is wholly impossible to steer space vehicles without recourse to cybernetics.[29]

Another scientist explained the rejection of cybernetics as the result of attempts to achieve a kind of 'total criticism' in which the simplifying

[28] M. Ye. Omyel'yanovskii, 'Zadachi razrabotki problemy "dialekticheskii materializm i sovremennoye yestyestvoznanie"', *Vestnik Akademii Nauk* (1956, 10), 4.

[29] Quoted in Lee Kershner, 'Cybernetics: key to the future', *Problems of Communism* (November–December 1965), 63–4.

assumption was often made that a scientist who does not hold Marxist views is not in a position to give birth to a useful technical conception or scientific theory.[30] And Academician A. I. Berg, who became the first chairman of the Science Council for Cybernetics, spoke of 'some of our philosophers who . . . held up the development of computer technology' on the grounds that mathematical machines had been credited with the ability to think.[31] Before judging the truth of these claims it is necessary to look more closely at the rejection of cybernetics.

The Rejection of Cybernetics

The number of attacks on cybernetics published in the Soviet Union in the early 1950s was small.[32] It is unnecessary to examine each of them individually, for certain features were common to all. First, a distinction was drawn between computer technology and the theories of cybernetics. The former was regarded as an important technological advance, while the latter were seen as a malignant ideological growth on the real science of automatic control. Second, the central focus of cybernetics was seen to be the analogy drawn between the brain and the computer; and particular exception was taken to the view ascribed to cyberneticians that the only feature distinguishing brain from computer is the former's size and capacity. Cybernetics was condemned for attempting to transfer the laws of motion peculiar to some forms of matter to qualitatively different forms where other, higher, laws operate. It was mechanistic in its disregard for such differences; but in so far as it ignored, dismissed, or failed to solve the problem of human consciousness, it was held to leave the door open to idealism and clericalism.

Cybernetics was seen as an excrescence on the decaying body of capitalism, reflecting its inhumanity, its aggression, and its fear of the proletariat. The fascination of the 'thinking machine' for the *bourgeoisie* lay, it was said, in the hope of substituting automatic machines for recalcitrant workers, or for pilots who might refuse to bomb peasant women working in the rice fields. Finally cybernetics was said to embody the vain hope that 'the contemporary technocrats—the cyberneticians' would be able, with the help of computers,

[30] A. V. Khramoi, 'O nekotorykh nedochotakh v planirovanii nauki', *Vestnik Akademii Nauk* (1956, 1), 40.

[31] A. I. Berg, *Izbranniye Trudy*, **2** (Moscow–Leningrad, 1964), 37. Berg made this comment during his report on cybernetics to the Presidium of the Academy of Sciences on 10 April, 1959.

[32] See Mikhail Yaroshevskii, 'Kibernetika—nauka mrakobesov', *Literaturnaya gazeta* (5 April 1952), 4; 'Materialist', *op. cit.* note 1; the entry on cybernetics in *Kratkii filosofskii slovar'* (Moscow, 1954), 236–7; and T. K. Gladkov, 'Kibernetika—psevdonauka o mashinakh zhivotnykh, cheloveke i obshchestve', *Vestnik moskovskogo universiteta* (1955, 1), 57–67.

to effect substantial changes in the social system.[33] But these ambitions were doomed to failure, for the fundamental problems of capitalist society were not amenable to technological solutions. It was the character of the economic system that determined the course of technological development, not technology that determined social development.

This amalgamation of the main points of the Soviet criticism of cybernetics obscures some of the differences between the writers but does not misrepresent the burden of their objections. Two aspects of the arguments against cybernetics do, however, merit further discussion. One of the Soviet writers repeated the charge of a French Marxist that cybernetics was 'an instrument of the Cold War against Pavlov'.[34] Soviet critics complained that the concept of feedback was much cruder than the Pavlovian concept of reflex. Moreover, cybernetics left open the question of the nature and origins of consciousness, which Pavlov was said to have explained by reference to speech, the 'second signalling system' which was peculiar to man alone. This had developed as a result of man's involvement in labour and social interaction, with the consequent need for extensive communication between man. Further, in neglecting the content of speech, cybernetics denied an active role to man's mental activity.

One of the Soviet critics went on to comment on cybernetics as a social theory.[35] He argued that cybernetics, by claiming that man is not, in essence, different from a machine, played down the crucial fact that man lives in society. Hence it made no distinction between different socio-economic formations, and conceived of society merely as a complex mechanism, consisting of a certain number of elements, and subject to mechanistic laws such as that of feedback. By focusing on the structure of communications it ignored the laws of social development; by ignoring the content of social information it made it impossible to grasp 'the essence of the phenomena of social life'. As a social theory cybernetics rationalized capitalist society by explaining social change in terms of improvement in 'group information', without reference to the mode of production. The crisis of capitalist production could be explained away as the self-regulating mechanism of the market. Because of the need for centralized control the cyberneticians argued that world civilization should be centralized—with its headquarters in Washington.[36]

[33] 'Materialist', *op. cit.* note 1, 214.

[34] 'Materialist', *ibid.*, 217. The French Marxist was Andre Lentin, in 'La Cybernetique: Problèmes Réels et Mystification', *La Pensée*, **47** (March–April 1953), 47–61.

[35] Gladkov, *op. cit.* note 31.

[36] For note 36, see next page.

I shall not comment here on the validity of these arguments. But it is important to ask what the effects of this criticism were on Soviet science. In 1960 Academician Berg wrote that 'it took such a long time to form a sensible attitude to cybernetics that undoubted harm was done to our science and technology'.[37] It is difficult to assess precisely what this harm was. Berg had referred to the way in which the fears of philosophers had held up the development of computer technology; but, as has been mentioned, computer technology was exempted from the initial attacks on cybernetics. In 1949 the first department of Computer Mathematics in the Soviet Union had been set up at Moscow University, and in the following year the Academy of Sciences established an Institute of Precision Mechanics and Computer Engineering. Work on digital computers had begun in the late 1940s, and by 1953 several different computers had been completed.[38]

The frontiers between physiology and engineering are those where cybernetics has had most effect on the conduct of research, and here the situation was more complex. Cybernetics was condemned as incompatible with Pavlov's theories; consequently the reaffirmation of Pavlovian teaching in 1950, and the subsequent purge of those who had attempted to revise his work, provided a powerful obstacle to cybernetics. One of those purged in 1950 exemplifies this clearly. In the 1930s P. K. Anokhin, a former pupil of Pavlov's, had carried out research into functional systems and had introduced into the physiology of the nervous system the idea of the 'return afferentation' of the results of an action to the actor—almost identical with the concept of feedback. This work, however, was condemned for conflicting with the

[36] Compare that with: 'Unless we Americans as a people, and we in the Air Force in particular, understand those momentous trends [developments in Soviet cybernetics], we may not have much choice. The system could be imposed upon us from an authoritarian, centralized, cybernated, world-powerful command and control center in Moscow'; Col. Raymond S. Sleeper, 'Cybernetics in the service of communism', *Air University Review* (March–April 1967), 13.

[37] A. I. Berg, 'O nekotorykh problemakh kibernetiki', *Voprosy filosofii* (1960, 5), 54.

[38] The development of Soviet digital computers began in 1948 in the Institute of Electronics of the Ukrainian Academy of Sciences under the direction of Academician S. A. Lebedev; subsequently this work was conducted jointly with the Institute of Precision Mechanics and Computer Engineering of the USSR Academy of Sciences. The MESM (an acronym of words meaning 'small computer') was tested in 1950 and modified in 1951; the BESM ('large computer'), which was developed in the Institute of Precision Mechanics and Computer Engineering, came into use in the autumn of 1952. (D. M. Berkovich *et al.* [eds.], *Ocherki razvitiya tekhniki v SSSR. Mashinostroenie, avtomaticheskoe upravlenie mashinami i sistemami mashin, radiotekhnika, elektronika i elektrosvyaz* [Moscow, 1970], 402). The Academy of Sciences' Institute of Electronic Control Systems, under I. S. Bruk, developed the M-1 digital computer in 1951 and the M-2 in 1953. (George Rudins, 'Soviet Computers: A Historical Survey', *RAND Soviet Cybernetics Review* [January 1970], 7).

Pavlovian theory of the reflex arc. Anokhin had attempted to rehabilitate his own work in the light of cybernetics:

> When cybernetics appeared on the scene and when I began to talk of our Soviet priority in the theoretical treatment of physiology, friends told me: 'Give up talking about that!' It's alright to outstrip a scientific discovery by eleven years, but we don't advise you to outstrip bourgeois obscurantism by eleven years.[39]

In as much as research in physiology was held up it was by the stress on Pavlovian orthodoxy, and only at second remove by the attacks on cybernetics.

It may be argued that a small number of articles rejecting cybernetics as a reactionary pseudoscience could have had little effect on Soviet science. The first attack on cybernetics was published in *Literaturnaya Gazeta* in April, 1952, and the two most authoritative condemnations appeared in *Voprosy Filosofii* in September, 1953, and in the 1954 edition of the *Short Philosophical Dictionary*. Yet by 1954 lectures were being delivered in defence of cybernetics, and in the following year articles were published explaining what cybernetics was and stressing its compatibility with dialectical materialism.[40] Indeed Berg's biographer concedes that 'it would seem that a few ignorant articles do not make the weather'; but she goes on to declare that 'the misfortune was that they created an unfavourable background for the new science. Taking on trust what was said in the articles mentioned, some scientists did not make the effort to form their own opinion, while others took someone else's opinion as their own'.[41] The articles published in *Voprosy Filosofii* and the *Short Philosophical Dictionary* must have had the approval of the Central Committee's ideological establishment: thus the condemnation of cybernetics was authoritative in the sense that it was denounced as inimical

[39] Anokhin, in a talk entitled 'For the creative collaboration of philosophers and physiologists', given at a conference on the contemporary problems of the materialist dialectic in April, 1965. (*Leninskaya teoriya otrazheniya i sovremennaya nauka* [Moscow: Nauka, 1966], 287). At an expanded meeting of the Presidium of the Academy of Medical Sciences in 1950 Anokhin was condemned for straying from Pavlovian teaching and for being attracted by 'the fashionable reactionary theories' of Coghill, Weiss, and others. His lapses were attacked as a form of 'kow-towing to foreign science and of cosmopolitanism' (*Vestnik Akademii Nauk* [1950, 7], 75).

[40] In November, 1954, Ernst Kol'man gave a lecture on cybernetics at the Academy of Social Sciences attached to the Central Committee. This was published in a revised form, in *Voprosy filosofii* (1955, 4), under the title 'Chto takoe kibernetika?' ('What is Cybernetics?'). The same issue of *Voprosy filosofii* carried a paper by three mathematicians, S. L. Sobolev, A. A. Lyapunov and A. I. Kitov, entitled 'Osnovnye cherty kibernetiki' ('The Fundamentals of Cybernetics').

[41] I. Radunskaya, *Aksel'Berg. Chelovek xx veka* (Moscow, 1971), 251. This is 'improving literature' for the young, rather than a scholarly biography. It does, however, draw on Berg's diary and reminiscences, and appears to provide a reliable, if often vague and incomplete, source of information on Soviet cybernetics and Berg's active and interesting life.

to the communist world-view by those who were officially responsible for saying what that world-view was. The first published defence was made in 1955, and it was not until 1958 that books and papers on cybernetics appeared in any number. It seems clear that the advocates had to exert considerable pressure to gain the right to defend cybernetics in the press;[42] it seems likely (though it is not in fact known) that the philosophical objections influenced editorial boards to hold back the publication of papers defending cybernetics. It cannot, however, be concluded from this that the attacks on cybernetics delayed the recognition of those problems and approaches which were later found to be interesting and fruitful, since the recognition and elucidation of those problems was intimately bound up with the need to refute the philosophical objections. Without the need for such rebuttal the problems might have been defined differently and given a different importance.

It is important to note, however, that it was not the philosophers alone who rejected cybernetics:

> In the arguments which were carried on about cybernetics some engineers, technologists and mathematicians, who were themselves doing both practical and theoretical work in the field of automatic systems, came forward as its opponents. They asserted that cybernetics had no right to existence as an independent science, that theories of automata were sufficient. Neither did narrow specialists in such fields as biology, physiology, psychology and linguistics like what they considered the misalliance with such sciences as mathematics and the various technical disciplines. In addition there was, it seems, an unconscious feeling of resentment: how could a machine, an assemblage of dead matter, suddenly accomplish thought processes peculiar to man alone! The attitude of these comrades can be explained by a certain narrow-mindedness, from which the most prominent specialists in one field or another sometimes suffer. A not inconsiderable part was played by the fact that at one time they were 'intimidated' by some of our philosophers who had proclaimed cybernetics reactionary.[43]

The delay in accepting cybernetics was the result of conservatism on the part of scientists, as well as vigilance by the philosophers. And the main accusation that can be made against the philosophers is that, in the event, their rejection of cybernetics as a reactionary pseudoscience merely reinforced the scientists' conservatism. Moreover—and this is a vital point—most of the 'philosophers' who rejected cybernetics were not professional philosophers, but seem to have earned that title only because they used arguments drawn from dialectical materialism in their evaluation of cybernetics. I shall return to this point later.

[42] See *ibid.*, 251–3. See also note 54 below.

[43] Ernst Kol'man, 'O filosofskikh i sotsial'nykh problemakh kibernetiki', in V. A. Il'i: *et al.* (eds.), *Filosofskie problemy kibernetiki* (Moscow, 1961), 70.

The Legitimation of Cybernetics

The advocates of cybernetics had to take account of both the philosophical objections and the scientific conservatism in their attempt to gain recognition for the new science. Part of their task consisted in extracting the scientific content of cybernetics from the undesirable philosophical and sociological embellishments with which its Soviet critics had associated it; but they had, besides, to establish just what that scientific content was.

The first public attempt to distinguish between the scientific basis of cybernetics and *bourgeois* distortions of its significance was made by Ernst Kol'man, a mathematician and philosopher, in a lecture given at the Academy of Social Sciences attached to the Central Committee in November, 1954.[44] He argued that the analogy between computer and nervous system was useful in the study of physiology. The analogy was restricted, however, to the quantitative aspects of electronic and physiological processes, for cybernetics was a scientific theory which studied processes which were completely different in their nature, but alike in their quantitative form. Yet the significance of cybernetics was not to be underestimated. Hitherto technology had provided an extension to our hands: now it would extend our brains. The most notable feature of Kol'man's paper, however, was his attempt to demonstrate the legitimacy of cybernetics by apposite quotation and historical precedent: he stressed the contribution of both pre-revolutionary Russian and post-revolutionary Soviet mathematicians to the development of cybernetics; he pointed to the *bourgeois* attempts to distort its scientific content and put cybernetics at the service of idealism and metaphysics; he claimed that Wiener's *The Human Use of Human Beings* had been removed from some American libraries because of its criticism of the capitalist system. Kol'man argued that the positive side of cybernetics deserved the constant attention not only of engineers and mathematicians but of philosophers, too.

[44] See *Voprosy filosofii* (1955, 4), 148–58. Kol'man has had a long and curious career as a philosopher of science. He contributed two papers to the Second International Congress of the History of Science and Technology in 1931, on 'The Present Crisis in the Mathematical Sciences and General Outline for their Reconstruction', and 'Short Communication on the Unpublished Writings of Karl Marx dealing with Mathematics, the Natural Sciences, Technology, and the History of these Subjects'. Cf. N. I. Bukharin *et al.*, *Science at the Crossroads* (London: Frank Cass, 1971). For comments on Kol'man cf. P. G. Werskey's introduction to this book, p. xvi. Kol'man took the Lysenko side in the genetics affair. Cf. Joravsky *op. cit.* note 17, *passim*. Joravsky writes that in the 1930s and 1940s Kol'man 'was one of the most savage Stalinists on the front of science and philosophy' (p. 361). Kol'man was at the time chief of the science department of the Moscow Party Organization (p. 385). In 1940 and 1941 he and Lysenko became engaged in a controversy with Kolmogorov over the validity of using statistical analysis in genetics. Cf. P. S. Hudson and R. H. Richens, *The New Genetics in the Soviet Union* (Cambridge: Imperial Bureau of Plant Breeding and Genetics, 1946), 28.

In 1955 *Voprosy Filosofii* published an article by three mathematicians, Academician S. L. Sobolev, Professor A. A. Lyapunov and A. I. Kitov, whose aim was to give an outline of the 'theories, hypotheses and points of view' relating to the questions of control and communication in automatic machines and living organisms.[45] They distinguished three main branches of cybernetics: the theory of information; the theory of electronic computers as self-organizing logical processes similar to the processes of human thought: and the theory of automatic control systems, including the study of the nervous system from the functional point of view. The unifying concept in these three fields was information:

> Just as the introduction of the concept of energy allowed one to consider all natural phenomena from a single point of view and overthrow a whole series of false theories (the theory of phlogiston, of perpetual motion machines, etc.) so the introduction of the concept of information, of the single measure of the quantity of information, permits one to approach from a single general point of view the study of the most varied processes of the interaction of bodies in nature.[46]

In spite of the qualitative difference which existed between brain and computer, it would be possible not only to model, but also to construct machines to perform, certain human mental functions.

The three mathematicians had a less polemical aim than Kol'man, but they did not wholly eschew the techniques of advocacy. The initial critics had suggested that cybernetics had been concocted in order to give idealist philosophy a scientific appearance.[47] Kol'man retorted that the capitalist powers had more effective ways of waging the Cold War than by creating special institutes, arranging national and international conferences, and publishing specialist journals, merely in order to discredit Pavlovian theories and drag idealism and metaphysics into psychology and sociology.[48] Sobolev and his colleagues wrote that it was possible that the 'reactionary, idealist interpretation of cybernetics in the popular reactionary literature' had been especially organized, as a kind of camouflage, in order to prevent Soviet scientists from seeing the value of cybernetics.[49] Thus the hostile image of capitalist society which had played an important part in the early attacks on cybernetics, was now turned to its defence. The main burden of the mathematicians' paper was to argue that it was time to stop speaking of cybernetics

[45] *Voprosy filosofii* (1955, 4), 136–47.
[46] *Ibid.*, 136–7.
[47] See, for instance, 'Materialist', *op. cit.* note 1, 218.
[48] *Op. cit.* note 43, 148.
[49] *Op. cit.* note 45, 147.

as a pseudoscience, and to investigate the limits of applicability of electronic models. It was clear, they said, that some philosophers had mistaken the sensationalist fringe for the real thing.

By the end of 1955 serious discussion of the scientific uses of cybernetics was getting under way. The article by Sobolev, Lyapunov and Kitov was based on seminar papers which had been discussed in various mathematical and scientific institutes at the Academy of Sciences and Moscow University.[50] During 1955–6 a seminar on cybernetics and physiology was held in the Department of Computer Mathematics headed by Academician Sobolev at Moscow University. The purpose of the seminar, which was organized by Professor Lyapunov, was to establish general points of view and to pose questions of common concern to the specialists of different fields who took part.[51] In the following year a seminar on cybernetics was formed which held twenty-six meetings and was attended by mathematicians, physicists, physiologists, psychologists, engineers, geneticists, philologists and economists.[52] The change of title was significant, and the range of specialists taking part in the seminar indicated the wide interest now being shown in cybernetics. In July, 1956, a small delegation of Soviet engineers attended the first International Congress on Cybernetics in Namur, and returned to report that computing, logic and control machines were being widely used abroad in the most diverse fields, and that analogies between machines and organisms were being developed successfully. Moreover, they noted that there was a great variety of views on cybernetics, its limitations and prospects of development.[53] Early in 1957 Lyapunov reviewed the situation thus:

It must be said that the publication of literature on cybernetics in Russia has unfortunately been greatly held up. Even worse, the development of work in this field has been delayed. However, interest in cybernetics has recently grown very much among us.[54]

[50] The papers had been read at the Academy of Sciences' Institute of Energetics, the Academy's Institute of Precision Mathematics and Computer Engineering, the Steklov Mathematical Institute, at the seminar on machine mathematics in Moscow University's Faculty of Mechanics and Mathematics, and in the University's Faculty of Biology.

[51] *Problemy kibernetiki*, I (1958), 265–6.

[52] *Ibid.* The papers read reflected the wide range of disciplines represented at the seminar, for example: Yu. T. Medvedev, 'The presentation of events in automata'; N. Ye Kobrinskii, 'Logical networks'; V. S. Gurfinkel, 'Control units in the nervous system'; A. A. Lyapunov, 'An information-theoretical approach to the fundamentals of genetics'; S. V. Yablonskii, 'The algebra of logic and its applications'; N. V. Timofeev-Resovskii, 'The factors of evolution'; L. V. Kantorovich, 'Mathematical methods in economic planning'.

[53] V. A. Il'in, 'Na kongresse po kibernetike', *Vestnik Akademii Nauk* (1956, 11), 75.

[54] *Problemy kibernetiki*, I (1958), 6.

Besides the activity in Moscow, work was going on in the provinces, in Kiev in particular.[55]

In the course of these discussions the term *cybernetics* began to acquire a currency quite different from its earlier notoriety. One Soviet engineer has remarked that when she went to work in a radioelectronics institute in 1953 cybernetics was a word 'we used to frighten children with'.[56] In 1956 a bibliography on cybernetics and its applications appeared in which twenty-three popular articles and pamphlets were listed, most of them dealing with computer technology and automatic control systems.[57] Articles on computers as an extension of the brain were published in popular science journals.[58] Kol'man gave a public lecture in Moscow, in 1956, in which he was much less concerned than he had been before to demonstrate the legitimacy of the new scientific infant. He concentrated rather on the need for, and the benefits of, technological progress: 'cybernetic technology', he declared, 'is the technology of the society which is building communism'.[59]

What is most interesting about the use of the term *cybernetics* is the way in which it now came to embrace computers and automatic control systems, which had been excluded from the attacks on cybernetics. This usage undoubtedly created some difficulties for the advocates of cybernetics by drawing attention away from the general theory of control processes and focusing it on computers. But it was also of the utmost importance in helping to legitimate cybernetics. For the practical usefulness of computers was being more clearly realized in the Soviet Union, and military and space successes were claimed by the advocates of cybernetics as evidence of the practical value of their science. Though understandable, this was rather curious, for—as most cyberneticians are at pains to point out—computer engineering and cybernetics, though intimately related, are not the same. Indeed, unless its principles and laws can be applied to biological systems, cybernetics has no right to exist as a general science of control and communication. It was, in fact, the analogy between machine and organism that provided the chief point of controversy in the mid-1950s.[60]

[55] See *op. cit.* note 2, 11; in 1957 a computer centre was created in the Ukrainian Academy of Sciences.

[56] Quoted in Albert Parry, *The New Class Divided* (New York: Macmillan, 1966), 63.

[57] A. N. Buchenkov, *Kibernetika i ee primeneniya: rekomendatel'nyi obzor literatury* (Moscow, 1956).

[58] Articles appeared in the popular science journals *Tekhnika molodyozhi, Priroda* and *Nauki i zhizn'. Znanie-sila* devoted one issue (1956, 7) to cybernetics.

[59] E. Kol'man, *Kibernetika* (Moscow, 1956), 39.

[60] For note 60, see next page.

In the mid-1950s the discussion of cybernetics became both more extensive and more intensive. If the Central Committee had given its support to the condemnation of cybernetics as a *bourgeois* pseudoscience, there is no evidence of a sudden or specific decision by it to endorse cybernetics as a legitimate field of scientific research. The recognition of cybernetics as a politically acceptable and scientifically useful field of enquiry was a gradual process. The absence of an entry on cybernetics in the 1955 edition of the *Short Philosophical Dictionary* suggests a decision to withhold judgment rather than to grant explicit approval. This decision reflected the intellectual climate of the mid-1950s which saw a renewal of debate in many areas of Soviet life.[61]

Science and Authority in the mid-1950s

This period saw also an important change in the relationship between science and philosophy. The relaxation of intellectual control and ideological vigilance after Stalin's death, and particularly after Krushchev's secret speech at the Twentieth Party Congress in February, 1956, helped to undermine the position of philosophers in general. The renewal of discussion in areas of natural science hitherto considered closed made it clear that the 'gate-keeper' or 'watchdog' role of the philosopher would have to be re-examined: should he intervene in natural scientific debate to pronounce on the philosophical acceptability of competing theories, or should he confine himself to the philosophical interpretation of what was accepted as true by the specialist communities ? The point at issue was expressed with admirable succinctness by G. V. Platonov, one of the more militant philosophers, when he complained that

> . . . if a direct call to isolate philosophy from science, and the attempt to build a 'theoretical' platform under it, are characteristic of only an insignificant part of our philosophers and scientists, then in fact, the tendency not to interfere in questions of science is fairly wide-spread amongst us.[62]

By 1956 it was generally acknowledged that there had been 'a weakening of

[60] Yu. P. Frolov, 'Sovremnaya kibernetika i mozg cheloveka', *Voprosy filosofii* (1956, 3); I. I. Gal'perin, 'O reflektornoi prirode upravlyayushchikh mashin', *Voprosy filosofii* (1957, 4); Ion N. Belenescu, 'Kibernetika i nekotoriye voprosy fiziologii i psikhologii', *Voprosy filosofii* (1957, 3); P. K. Anokhin, 'Fiziologiya i kibernetika', *Voprosy filosofii* (1957, 4); V. I. Kremyanskii, 'Nekotorie osobennosti organizmov kak "sistem" s tochki zreniya fiziki, kibernetiki i biologii', *Voprosy filosofii* (1958, 8).

[61] See my 'Scientific Truth and Political Authority in the Soviet Union', *Government and Opposition*, **5** (1970, 3), esp. 351–5.

[62] G. V. Platonov, 'Znachenie dialekticheskogo materializma dlya bor'by s idealizmom i metafizikoi v yestyestvoznanii', in *Dialekticheskii materializm i sovremennoye yestyestvoznanie* (Moscow, 1957), 4.

the alliance between philosophers and scientists'.[63] The situation was judged so serious that in June of that year a committee was appointed by the Presidium of the Academy of Sciences and the Ministry of Higher Education to organize the first All-Union Conference on the Philosophical Problems of Contemporary Natural Science.[64] The conference did not take place, however, until October, 1958: the preparations ran into some difficulties, perhaps associated with Lysenko's changing fortunes in 1956 and 1957.[65] When it did meet no paper was read on genetics and the conference resolution explicitly, if halfheartedly, endorsed Michurinism and Pavlovian teaching on the higher nervous system.[66]

In the closing speech at the conference, Academician P. N. Fedoseev, director of the Academy of Sciences' Institute of Philosophy, emphasized that

> . . . the papers which were sent out by the organizing committee and which were heard here are not materials for guidance, or reports giving direction. They are the results of the researches of scholars, or scientific institutes, which were reported as a matter for creative discussion for the free exchange of opinions. Of course comradely advice was expressed by members of the committee, and separate comments were made on the reports. These comments were likewise not binding on the speakers and, I repeat, the reports published must be viewed as material for a creative exchange of opinions, and not as directives.[67]

The beneficial effects of the organizing committee's tolerant approach were evident in the discussions about physics and cybernetics, but the omission of genetics from the conference programme showed that free discussion was to be confined within certain limits. This created an anomalous situation for the advocates of cybernetics, who found that their claims for cybernetics as a general science of control and communication involved them in the discussion of biological questions over which a politically supported orthodoxy still reigned.

This anomaly became clear in the paper given by Academician Sobolev and Professor Lyapunov on 'Cybernetics and Natural Science', and in the subsequent discussion.[68] The declared aim of the paper was to dispel the misconceptions which underlay the criticisms of cybernetics by defining its subject-matter and its relationship to other scientific disciplines. The authors

[63] M. Ye Omyel'yanovskii, *op. cit.* note 28, 3.
[64] *Filosofskie problemy sovremennogo yestyestvoznaniya* (Moscow, 1959), 650.
[65] See Joravsky, *op. cit.* note 17, 157–62.
[66] See the Conference resolution in *op. cit.* note 64, 602–5; also Joravsky's comments in *op. cit.* note 17, 246–8.
[67] *Op. cit.* note 64, 589–90.
[68] *Ibid.*, 237–60; Sobolev's remarks when introducing the paper are on pp. 260–7; and his closing comments on pp. 572–3.

argued that an examination of how computers worked showed that there was much in common between them and the higher nervous system in so far as both possessed mechanisms for performing specific actions, mechanisms for controlling those actions, and communication between the two. Cybernetics, they said, studied the information flows in control systems, the algorithms by which the information was processed, and those features of the structure of control systems which were relevant to the transmission and processing of information. They described some of the basic principles of information theory, the theory of algorithms and programming and discussed how these could be used in the study of physiology and genetics. They argued that the successful practical application of cybernetic ideas in industry and defence showed that cybernetics was not based on false philosophical assumptions.

Sobolev and Lyapunov were criticized for doing no more than expounding some of the basic concepts and theories of cybernetics. They earned a rebuke from Professor Anokhin for not discussing cybernetics' claim to be able to discover general laws for such qualitatively different phenomena as machine, brain and society: it was precisely this claim, he declared, that should be examined at a conference on philosophy.[69] G. V. Platonov attacked the authors for underestimating the importance of the struggle against idealism, and for seeking to extend the principle of peaceful coexistence into the realm of ideology.[70] Sobolev and Lyapunov did indeed avoid general philosophical problems in their paper. For example they noted the 'somewhat morbid interest' excited by the analogy drawn between brain and computer, and declared that this question related 'not so much to philosophy as to cybernetics as such and ought to be solved by the accumulation of further experience in this science'.[71] Sobolev stated that they could not really understand the anxieties of the philosophers, nor did they know how one could 'explain in a materialistic way the philosophical problems of electronic computers', as the philosophers asked them to do.[72] Sobolev insisted on a subordinate role for philosophers in science, and went so far as to demand that the conference resolution stipulate that only those who had contributed something to natural science could discuss its philosophical problems. But it was the claims of cybernetics in genetics and physiology that aroused most criticism at the conference. Two speakers expressed serious doubts about the analogy drawn between brain and computer.[73] Others took exception to the

[69] *Ibid.*, 405.
[70] *Ibid.*, 474–8.
[71] *Ibid.*, 250.
[72] *Ibid.*, 260.
[73] Professor V. N. Kolbanovskii, *ibid.*, 547; and Professor Yu. P. Frolov, *ibid.*, 486.

statement that 'the data of classical genetics correspond fully with the ideas put forward by cybernetics'.[74] This view contradicted the Lysenkoist position endorsed by the conference, and evoked predictable, if surprisingly mild, protests.

Replying to the discussion Sobolev remarked that

> . . . unfortunately, from the questions put it is evident that the philosophers and ourselves do not always understand each other very well.[75]

Yet of those who raised objections to cybernetics only two—Platonov and M. N. Rutkevich—were professional philosophers. The others—G. V. Nikol'skii, K. Yu. Kostryukova, V. N. Kolbanovskii and Yu. P. Frolov—were biologists or psychologists of one kind or another.[76] And the same was true of the 'philosophers' who had originally rejected cybernetics: the article in the *Short Philosophical Dictionary* may presumably be attributed to philosophers, but it is said that 'Materialist' was a pseudonym for V. N. Kolbanovskii, and M. Yaroshevskii, author of the article in *Literaturnaya Gazeta*, was a psychologist.[77] Thus it emerges that the 'philosophers' who were accused of having retarded the development of Soviet cybernetics were not all professional philosophers. What they did have in common was their

[74] M. N. Rutkevich, *ibid.*, 431; K. Yu Kostryukova, *ibid.*, 481; G. V. Nikol'skii, *ibid.*, 504–5.

[75] *Ibid.*, 573. Academician V. A. Ambartsumyan agreed with Sobolev's proposal that only competent scientists should engage in the philosophy of science (p. 576).

[76] G. V. Platonov was one of the chief Lysenkoite philosophers, who, even after Lysenko's fall in 1965, made an attempt to defend Lysenkoism as a coherent doctrine. M. N. Rutkevich, one of the best-known philosophers of practice, had also supported Lysenkoism; since the mid-1950s he has been one of the pioneers in the revival of Soviet sociology. G. V. Nikol'skii, a corresponding member of the Academy of Sciences, was an ichthyologist. Professor Kostryukova was a Lysenkoite. Yu. P. Frolov was a physiologist, who had worked in Pavlov's laboratories in the 1920s and 1930s; he had also been chief of the Central Psychophysiological Laboratory of the Workers' and Peasants' Red Army. It is more difficult to assign V. N. Kolbanovskii to a specific category. In 1931 he became director of the State Institute of Psychology, Pedology and Psychotechnics, and in the 1930s he is said to have worked on the problems of the development of the individual and the collective (A. V. Petrovskii, *Istoriya sovyetskoi psikhologii* [Moscow, 1967], 244). But he seems to have acted also as a guardian of the ideological purity of Soviet psychology. See Petrovskii, *op. cit.*, 79, and also Kolbanovskii's attack on S. L. Rubinstein's textbook *The Fundamentals of General Psychology*, 'Za marksistskoe osveshchenie voprosov psikhologii', *Bol'shevik* (1947, 17, September), 50–6.

[77] My information about the identity of 'Materialist' came from a Soviet philosopher. It seems to be confirmed by Kolbanovskii's views at the October 1958 Conference and in V. A. Il'in *et al.* (eds.), *op. cit.* note 43, where he has some interesting things to say about applying cybernetic concepts in social analysis. M. Yaroshevskii is also a psychologist who has recently edited a book on scientific creativity. See M. G. Yaroshevskii (ed.), *Problema nauchnogo tvorchestva v sovremennoi psikhologii* (Moscow, 1967). I cannot discover anything about T. K. Gladkov. The entry on cybernetics in the 1954 edition of the *Short Philosophical Dictionary* is unsigned.

support for the politically sanctioned orthodoxies in biology and their appeals to the authority of those orthodoxies in their criticisms of cybernetics. Thus the term 'philosopher' was used to describe not only professional philosophers and officials in the ideological apparatus, but also those who employed the Stalinist 'techniques of persuasive argumentation' in natural scientific debate. Criticism of the philosophers was therefore a form of indirect attack on the politicization of scientific authority, and on the way in which political controls on the scientific community impinged on the processes of social control in science.

The very outspokenness of the criticism to which philosophers were now subjected was itself an indication of the way in which the Stalinist system of controls in natural science was being eroded. This was further illustrated by the mildness of the comments which Fedoseev, himself a former inspector of the Central Committee, made in his closing speech:

> S. L. Sobolev has rendered great services in the elaboration and propagation of cybernetics, and he was right in his polemics against the attempt of some poorly-qualified people to declare cybernetics a pseudoscience, an idealist science. The basic criticism of the paper expressed at the conference concerned chiefly the question of applying cybernetics in the field of biology. It appears that, generally speaking, it would be unreasonable to demand a 'prohibition' of the application of cybernetics to the field of biology. But biology must not be replaced by cybernetics.[78]

If there is any point at which the seal of political acceptability may be said to have been granted to cybernetics, this was it. Moreover, this was done at the very time when the position of those who had initially rejected cybernetics was being weakened. The conference resolution called for closer contact and better communication between scientist and philosopher: in the event, this meant that scientific authority was more clearly vested in the specialist communities, and that the 'techniques of persuasive argumentation' engendered by the Stalinist system of political controls would no longer have their former effectiveness.[79]

The Soviet School of Cybernetics

An important turning point in the development of Soviet cybernetics came in the years 1958 and 1959. With the initial critics now soundly defeated,

[78] *Op. cit.* note 64, 600. P. N. Fedoseev, born 1908, served in the Central Committee apparatus during the Great Patriotic War; from 1945 to 1949 was chief editor of *Bol'shevik*, the chief theoretical journal of the Central Committee; in 1949 became involved in a row with M. Suslov; from 1950 to 1955 worked as inspector in the Central Committee and chief editor of *Partiinaya zhizn'*; in 1955 was made director of the Academy of Sciences' Institute of Philosophy and editor of *Voprosy filosofii*.

[79] For note 79, see next page.

the advocates of cybernetics were faced with the task of establishing a Soviet school which would make it possible to pursue further the lines of research set out by Sobolev and Lyapunov. The discussions which had already taken place had apparently evoked amongst Soviet scientists a very widespread and intense demand for more information about cybernetics.[80] This demand began to be met by the flood of literature which started to appear in 1958: Wiener's *Cybernetics* and *The Human Use of Human Beings* came out in Russian, and a Soviet work on cybernetics, *Signal* by I. A. Polyetaev, was published; the first of a series of collected Soviet papers, *Problemy kibernetiki*, appeared under the editorship of Professor Lyapunov; and the supplementary volume of the *Large Soviet Encyclopedia* contained a lucid account of cybernetics by Academician A. N. Kolmogorov.[81] In the following year W. Ross Ashby's *Introduction to Cybernetics* appeared in Russian. Each of the translated books was provided with a Soviet introduction, pointing to the philosophical errors, but emphasizing its general validity. Thus the ideas and claims of cybernetics began to be widely disseminated in the form of both Soviet and Western works.

In January, 1959, three months after the philosophy conference, the Presidium of the Academy of Sciences appointed a commission of twenty members to draw up a long-term plan of research in cybernetics.[82] In spite of

[79] Philosophers were now expected to know something about natural science—in effect to accept the natural scientists' definition of scientific truth—before discussing its philosophical problems. At the second All-Union Conference on the Philosophical Problems of Natural Science in December 1970, Academician V. A. Ambartsumyan recalled that in October 1958, he had complained that the philosophers, in their zeal to prevent scientists from falling into idealism, were retarding rather than encouraging innovative thinking. He was happy not to have to repeat the complaint in 1970. See *Voprosy filosofii* (1971, 3), 24. From the mid-1950s the term 'philosopher' was one of abuse amongst natural scientists. See Fedoseev's plaintive cry in his closing speech at the All-Union Conference on the Philosophical Questions of the Physiology of Higher Nervous Activity and of Psychology in 1962; 'We have individual scientists who collect the mistakes of some philosophers, and for ten years or more we have been hearing and even reading in some articles that such-and-such a philosopher rejected quantum mechanics, and so-and-so rejected the theory of relativity, and that in the *Philosophical Dictionary* there were attacks on cybernetics. Of course, comrades, there were such facts, and we condemn them together with all scholars. But surely one cannot conclude from this, as some do, that philosophers retarded the development of science ? One cannot identify the development and significance of philosophy with the mistakes of some philosophers on individual questions. That is a basic condition, if we are speaking of business-like collaboration, of principled relations, of mutual help and mutual support.' *Vsesoyuznoe sovyeshchanie po filosofskim voprosam fiziologii vysshei nervnoi deyatel'nosti i psikhologii* (Moscow, 1962), 745.

[80] See Radunskaya, *op. cit.* note 41, 259.

[81] N. Viner, with an introduction by E. Kol'man, *Kibernetika i obshchestvo* (Moscow, 1958); I. A. Polyetaev, *Signal* (Moscow, 1958); A. N. Kolmogorov, 'Kibernetika', in *Bol'shaya Sovetskaya Entsiklopedia*, 51 (1958), 149–51.

[82] For note 82, see next page.

differences of opinion about the definition of the field, the commission did recommend the establishment of a Science Council for Cybernetics in the Academy of Sciences.[83] On 10 April the Presidium of the Academy created the Council, which comprised, at the outset, eight sections:[84] mathematics; cybernetics and living nature; engineering problems of cybernetics; mathematical machines; philosophical problems of cybernetics; economics; mathematical linguistics and machine translation; reliability. Engineers, biologists, linguists, mathematicians, economists and specialists from research institutes, design bureaux and industry were drawn into the Council's work on a voluntary basis.[85] From the very beginning the Council attempted to establish close contact with all the Academy's divisions, and helped to coordinate the activities of all those interested in cybernetics. Although it does not itself conduct research, the Council has played a crucial role in the development of Soviet cybernetics; it has drawn up lists of the most important research topics for inclusion in the Academy's coordinating plans, and has monitored the conduct of this research; it has organized conferences, symposia and seminars; and it has sponsored publications.[86]

The first chairman of the Council, and the dominant figure in the formation of the Soviet school of cybernetics, was Engineer Admiral Academician Aksel' Ivanovich Berg. A specialist in radioelectronics, Berg was elected Academician in 1946 and from 1953 to 1955 was the first director of the Academy of Sciences' Institute of Radioengineering and Electronics, which had been set up at his instigation. A career officer, Berg had served in both the Imperial and Red Navies. During the Great Patriotic War he played an important role in organizing and directing the development of Soviet radar: in July, 1943, he was appointed Deputy People's Commissar of the

[82] A. I. Berg, *Izbrannye trudy*, 2 (Moscow–Leningrad, 1964), 35. The commission consisted of eight members drawn from engineering, six from physics and mathematics, two from biology and medical science and two from economics. But some of the members of the commission took no part in its work; consequently other specialists were brought in to help. The commission was supposed to report within two weeks of being set up, but in fact took three months to complete its work. *Ibid.*, 35.

[83] *Ibid.*, 35. The commission did, however, settle on the view that the core of cybernetics was the general laws which lie at the basis of control and of control systems, and which are based on the collection, processing and transmission of information.

[84] Berg (ed.), *op. cit.* note 4, 9. By 1967 the Council had fifteen sections.

[85] V. A. Il'in *et al.* (eds.), *op. cit.* note 43, 157. Participation 'on a voluntary basis' (*na obshchestvennykh nachalakh*) brings neither payment nor formal powers. At the very beginning, it seems, the Council had no permanent staff; see Radunskaya, *op. cit.* note 41, 260.

[86] For an outline of the Council's activities see Berg (ed.), *op. cit.* note 4, 9. Science Councils are a traditional feature of the Soviet research and development system. Their main functions are to coordinate research, and to act as vehicles of government policy in research and development. They serve also to draw scientists into the higher circles of decision-making. See E. Zaleski *et al.*, *Science Policy in the USSR* (Paris: OECD, 1969), 227–31.

Electrical Industry with special responsibility for radar, a position which he held until October, 1944; he served also as Deputy Chairman of the State Defence Committee's Council on Radar from 1943 to 1947. In 1953 Berg was recalled from his academic work to become Deputy Minister of Defence in charge of radar and radioelectronics, but in 1957 ill health forced him to resign; from 1957 to 1960 he served as consultant to the Ministry of Defence. As Deputy Minister, Berg took an active interest in the military potential of cybernetics.[87]

After his appointment as chairman of the Science Council, Berg devoted great energy to propagating the ideas of cybernetics and pointing to their relevance in the most diverse fields of human activity, especially in economic planning and management. His experience as propagandist for radar during the war had equipped him well for his role, and the success of his work is illustrated by the following passage in the 1961 Party Programme:

> Cybernetics, electronic computer and control systems will be widely applied in production processes in industry, building and transport, in scientific research, planning, designing, accounting, statistics, and management.[88]

But Berg sought not only to gain political recognition for cybernetics and its practical uses, but also to ensure that cybernetics did in fact contribute to Soviet political purposes. He declared that it was

> . . . the task of Soviet scientists to create a Soviet school of cybernetics, based on dialectical materialism, in order to place its achievements and potentialities at the service of the Soviet people, who are building communism.[89]

This task was achieved in the years after 1958 when a Soviet school of cybernetics emerged, with its own organizational structure and lines of research, dedicated to Soviet social purposes and values. In order to understand more fully the process of innovation in Soviet science it must be asked what made the school 'Soviet'.

The first important feature of the Soviet school is the considerable recognition which cybernetics seems to have gained in the Soviet Union as an

[87] Radunskaya, *op. cit.* note 41, and I. V. Brenev, 'Akselyu Ivanovich Bergu—75 let', *Izvestiya vuzov SSSR—radioelektronika* (1968, 10), 1113–20. On Berg's early interest in cybernetics see Radunskaya, *op. cit.* note 41, 249–50. For further background information on the development of Russian radar, see J. Erickson, 'Radio-location and the Air Defence Problem: the Design and Development of Soviet Radar 1934–40', *Science Studies*, **2** (1972), 241–68.

[88] *Programme of the Communist Party of the Soviet Union* (Moscow, 1961), 66.

[89] V. A. Il'in *et al.* (eds.), *op. cit.* note 43, 159.

over-arching or synthesizing theory in natural science.[90] The first volume of *Problemy kibernetiki* contained papers on the general problems of cybernetics, programming, computers, control processes in living organisms, mathematical linguistics, the theory of information and coding and, from 1961, mathematical economics. Berg pointed out in 1967 that many branches of cybernetics—the theory of information systems, pattern recognition, bionics, for example—had become independent scientific disciplines. Nevertheless, in the Soviet Union these disciplines were fostered by cybernetics and are still seen as part of the general field. The same pattern is not found in other societies: consequently it is a peculiarity of Soviet cybernetics that there has been such a self-conscious school, and not merely a set of distinct, though related disciplines.

There appear to be several reasons for this emphasis on cybernetics as a unifying approach in natural science. Some of these are to be found in the research traditions of Russian and Soviet science, but others have to do with the process of innovation itself.[91] The blanket condemnation of cybernetics in the early 1950s directed attention precisely to its claims of general application in the study of animal and machine. Moreover, the campaign on behalf of cybernetics became a *cause célèbre* in the scientists' fight for greater intellectual autonomy, and this, in turn, helped to keep attention focused on the most general aspects of cybernetics' claims. Another reason for the emphasis on cybernetics lies in the fact that certain kinds of biological research were less susceptible to political interference if carried out and published under the rubric of cybernetics. Even after December, 1958, when the Lysenkoist position was strengthened by the dismissal of the editorial board of *Botanicheskii zhurnal*, discussion of biological questions in cybernetics continued.[92] *Problemy kibernetiki*, for example, published papers on the application of cybernetics to genetics, thereby providing a haven for geneticists.

[90] For an outline of some of the Soviet work in cybernetics by a Western practitioner, see Michael A. Arbib, 'A Partial Survey of Cybernetics in Eastern Europe and the Soviet Union', *Behavioural Science*, **11** (May 1966), 193–216.

[91] On Russian and Soviet research traditions see A. V. Khramoi, 'Kistorii razvitiya kibernetiki', in V. A. Il'in *et al.* (eds.), *op. cit.* note 43, 180–212.

[92] See Joravsky, *op. cit.* note 17, 161 ff, for this episode and the subsequent situation in biology. Joravsky suggests that the dismissal of the editors, and Khrushchev's complaints about the opposition to Lysenko, may have been, in part at least, a response to the weak support Lysenkoism had received at the October Conference (p. 247). For Lysenko's complaints at the December Plenary Session of the Central Committee that many leading scientists—including the President of the Academy of Sciences, Academician A. N. Nesmeyanov—did not take his views seriously, see *Plenum TsK KPSS, 15–19 dek., 1958, Stenograficheskii otchot* (Moscow, 1959), 236.

This provoked the wrath of the Lysenkoists who tried to stifle such discussion. In January, 1962, the Ministry of Culture forwarded to the Science Council a fifteen-page review of the papers which had appeared in the 'control processes in living organisms' section of *Problemy kibernetiki*.[93] The reviewer attacked the paper as anti-Michurinist and proposed that all papers on biological questions be referred to the specialist biological journals so that, in the words of Berg's biographer, 'he and like-minded people could more easily take their revenge on the upstart cyberneticians';[94] the reviewer also proposed that Lyapunov be removed from the editorship of *Problemy kibernetiki*. The Science Council was asked to express its opinion of the review and to say whether it considered it permissible in principle, and useful in practice, to apply cybernetics to questions of biology. Berg's response was to make two hundred copies of the review which he then sent to well-known scientists for their comments. Having received the criticisms he anticipated, Berg consulted the Presidium of the Academy and the Central Committee of the Party, where he was advised to call a session of the Biological Division of the Academy of Sciences to discuss the biological aspects of cybernetics. This session was a complete victory for the cyberneticians, and it was recommended that publication in the field of biocybernetics be expanded, particularly in *Problemy kibernetiki*. A collection of papers entitled *The Biological Aspects of Cybernetics* subsequently appeared.[95]

The institutional arrangements of the Soviet school reflect the emphasis on cybernetics as a unifying theory in natural science. In 1960 an Institute of Cybernetics was formed in the Georgian Academy of Sciences in Tbilisi, and in 1962 the Ukrainian Academy of Sciences' Computer Centre was renamed the Institute of Cybernetics. Along with Moscow and Kiev, the new Siberian Division of the USSR Academy of Sciences, to which Sobolev and Lyapunov moved in 1958, was the third main centre of research in cybernetics.[96] It is, however, the Science Council that has played the dominant institutional

[93] The only account of this episode that I have seen is in Radunskaya, *op. cit.* note 41, 290–301. Among the papers to which exception was taken were A. A. Lyapunov, 'On some General Questions of Cybernetics', 1; I. I. Shmal'gauzen, 'The Bases of the Evolutionary Process in the Light of Cybernetics', 4; A. M. Malinovskii, 'The Types of Biological Control Systems and their Importance in Adaptation', 4.

[94] Radunskaya, *op. cit.* note 41, 299.

[95] *Biologicheskie aspekty kibernetiki* (Moscow, 1962).

[96] Sobolev became director of the Institute of Mathematics and the Computer Centre. The Computer Centre was to have four computers which would serve not only the new Academic Town, but also Novosibirsk and Siberia. It was also planned to have a design bureau which would work on the development of new computers. *Vestnik Akademii Nauk* (1957, 12), 9, 12. See also Arbib, *op. cit.* note 90, for comments on the main centres of research in Soviet cybernetics.

role in maintaining cybernetics as a general, overarching perspective in natural science. It is not only Berg's activities on behalf of cybernetics that account for this, but also the Science Council's role in providing a mechanism for cooperation and exchange across institutional boundaries, especially in Moscow, the main centre of Soviet science. The bureaucratic obstacles to scientific exchange in the Soviet Union have been remarked on frequently both there and abroad.[97] The Science Council has attempted to overcome these obstacles and to facilitate interdisciplinary and interdepartmental communication and cooperation.[98] If the interdisciplinary claims of cybernetics were an important reason for the creation of the Science Council, the Council, in turn, has helped to maintain the emphasis on cybernetics as a synthesizing and unifying perspective in natural science.

But it was neither the broad interpretation of cybernetics nor the institutional arrangements that constituted, in Soviet eyes, the peculiarly Soviet features of the school. Much more important was the school's commitment to Soviet values and purposes. In 1961, a Soviet philosopher concluded from a survey of the methodological problems of cybernetics that

> . . . cybernetics is connected with dialectical materialist philosophy as its natural and necessary world-view basis.[99]

Even in 1961, and certainly in the late 1950s, this was little more than a pious hope, and it was not until some years later that serious philosophical analysis of cybernetics was under way. Moreover, the initial arguments about cybernetics had shown great differences of view about its relationship to dialectical materialism. In 1958 and 1959 a series of discussions took place between engineers from the Academy of Sciences' Institute of Remote Control and Automation and philosophers from the Academy of Social Sciences, with the editors of *Voprosy Filosofii* also taking part. These discussions did much to create mutual understanding between scientists and philosophers, though it is clear from the papers subsequently published that not all disagreement was eliminated.[100]

[97] See, for example, V. V. Nalimov, 'Kolichestvennie metody issledovaniya protsessa razvitiya nauki', *Voprosy filosofii* (1966, 12), 43–5; Zhores Medvedev, *The Medvedev Papers* (London: Macmillan, 1971), 121 ff.

[98] 'One of the paramount tasks of the Science Council for Cybernetics is creating conditions for uniting scientists of different directions and ensuring their mutual understanding'. A. I. Berg in Berg (ed.), *op. cit.* note 4, 8.

[99] I. B. Novik in Berg (ed.), *op. cit.* note 2, 54.

[100] V. A. Il'in *et al.* (eds.), *op. cit.* note 43. Among the contributors were E. Kol'man, A. I. Berg, A. V. Khramoi, V. A. Il'in, V. N. Kolbanovskii and Yu. P. Frolov.

The advocates of cybernetics argued that cybernetics was a science particularly appropriate to a socialist society. They asserted, firstly, that whereas in capitalist society cybernetic technology (i.e. automation) would lead to unemployment, hardship and economic crises, in socialist society it would help to raise productivity and to put an end to the differences between mental and physical labour.[101] The fruits of scientific and technological progress, they argued, could be beneficial only in a socialist society. They went on, besides, to make an even more important claim, for they advanced the argument that socialist society provided an especially fruitful environment in which to apply cybernetics—the science of control—because it developed, not anarchically or spontaneously, but as a result of the conscious and purposeful activity of the masses, led by the Communist Party. The Party's leadership was based on an understanding of the laws of social development and was therefore organized, planned and scientific. Cybernetics would have a particularly important role in helping to manage the centralized planned economy.[102] This was undoubtedly a crucial argument, since it was advanced at a time when party and government leaders were becoming increasingly aware of the need for improved methods and structures in economic planning and administration. Cybernetics thus offered to meet a deeply felt political need.

Conclusion

We can now return to the questions raised at the beginning of the paper. The first of these was: how different was what came to be accepted from that which had been rejected ? It is clear that several important changes had taken place between 1955 and 1958 in the Soviet conception of cybernetics, and that most of the initial objections were thus invalidated or made to seem irrelevant. In the first place, the initial distinction between cybernetics and computer technology was abandoned. The engineering and theoretical aspects of cybernetics were now seen as intimately related, and the increasing importance of computers in defence, industry and science was claimed as practical proof of the theories of cybernetics. Secondly, the analogy between brain and computer, which had occupied a central place both in the rejection of cybernetics and in its subsequent legitimation, now commanded less

[101] See, for example, Novik in Berg (ed.), *op. cit.* note 2, 50–1; and B. S. Ukraintsev, 131–3, and V. A. Il'in, 225, in V. A. Il'in *et al.* (eds.), *op. cit.* note 43.

[102] This argument was pressed particularly by Berg. See Berg (ed.), *op. cit.* note 2, 1–2; in the same work see V. D. Belkin, 185, and A. I. Kitov, 212. See also S. M. Shalyutin, in V. A. Il'in *et al.* (eds.), *op. cit.* note 43, 55.

attention. The puzzles presented by cybernetics were now seen to be more diverse and complex than the question: can a machine think?

Cybernetics had been greeted in the Soviet Union as a bourgeois delusion, but this description was, of course, dropped as cybernetics came to be accepted as a legitimate area of scientific research. The hostile image of the West did remain, however, and much of the Western commentary on the social consequences of automation was seen to reflect the contradictions of capitalist society. The pessimism in this commentary was stressed along with the optimism: Soviet writers pointed to the *bourgeois* fear that technological process would sharpen the contradictions in capitalist society, and also to the technocratic hope that technology might make it possible to resolve those contradictions.[103] At the same time Soviet cyberneticians were beginning to show signs of espousing the technocratic ambitions which had been condemned in the first attacks on cybernetics—though they did of course declare that the practical uses of cybernetics in defence and the economy were to be put at the service of the Party.

Of the more specific objections raised to cybernetics, that based on Pavlovian theories about higher nervous activity no longer carried the same force, since the Pavlovian orthodoxy had been greatly weakened in the mid-1950s.[104] The claims of cybernetics in the social sciences, which had evoked such a strong rebuttal in the early 1950s, received little attention before 1958, apart from some discussion of operational research and data-processing in economic management.[105] It seems clear that the advocates of cybernetics wished to base their case primarily on natural scientific grounds, where they claimed authority, and on the utility of cybernetic technology to the state, where the benefits were obvious. They were wary of becoming involved in the politically more sensitive social sciences. In 1958, however, more extensive discussion of cybernetics and social science began,[106] and in his closing speech to the All-Union Conference on the Philosophical Problems of Contemporary Natural Science, Fedoseev declared:

[103] For example, E. A. Arab-ogly, 'Sotsiologiya i kibernetika', *Voprosy filosofii* (1958, 5), 139–44.

[104] This seems to have been the result not only of the influence of cybernetics, but also of the general increase in intellectual autonomy for natural scientists. The Council on the Problem of the Physiological Teaching of Academician I. P. Pavlov, which had been set up to ensure that the resolutions of the 1950 Conference were enforced, seems to have held its last meeting in 1953. See *Vestnik Akademii Nauk* (1953, 6), 61–2.

[105] A major turning-point came in November, 1959, when Berg, Kitov and Lyapunov discussed the possibilities of automating the management of the economy. See A. I. Berg, I. A. Kitov, A. A. Lyapunov, 'O vozmozhnostyakh avtomatizatsii upravleniya narodnym khozyaistvom', *Problemy kibernetiki*, **6** (1961).

[106] See, for example, E. A. Arab-ogly, *op. cit.* note 103, 138–51.

It would be useful to investigate in what sense and to what ends the methods of cybernetics might be applied for economists, sociologists, statisticians, etc. This would probably help workers in the field of cybernetics to formulate more concretely what they are doing, what they can contribute, and what the prospects are for development in that field.[107]

Cybernetics thus became established as a legitimate field of enquiry in natural science before it came to exercise any substantial influence on social science.

The character of the Soviet School of cybernetics owed much to the process of innovation itself: the initial objections and the need to refute them ensured that cybernetics' claims of general relevance remained at the centre of attention. The blanket condemnation made it necessary for the advocates of cybernetics to establish the scientific content of cybernetics while cutting away the unacceptable ideological wrappings. In so far as they gained acceptance for cybernetics as a natural scientific discipline they were successful in this. [108] The claims of cybernetics in biology (which had provided the focus of the controversy) were accepted as legitimate subjects for research, while those in the social sciences were until 1958 at least, ignored. The social and political values with which cybernetics had been—sometimes inaccurately—associated in the inital attacks were discarded by the Soviet school, and new social and political purposes acquired. The process of innovation, though slow, was effective in making cybernetics 'Soviet'.

This brings us back to the second question posed at the opening of the paper: by what process did cybernetics come to be recognized as a legitimate and fruitful field of research? The initial rejection of cybernetics was subsequently blamed on the ignorance and lack of understanding of the philosophers. In the zoological metaphor of one Soviet writer

> . . . the dawn of the cybernetic era belongs to the middle of our century. As for the Owl of Minerva, at dawn she is often blind, for as is known, she likes to fly only at midnight. And therefore from her cloudy philosophical heights she did not immediately make out the 'ugly duckling' which had been born in the famous family of the sciences, and when she did notice, then she even tried to crumple it in her claws. But now all that is in the past, and the 'duckling' has long since grown up.[109]

Berg warned that from the fate of cybernetics

[107] *Op. cit.* note 64, 601.

[108] Cybernetics was generally acknowledged in the Soviet Union as a 'science', though not always as a 'scientific discipline'. Novik for one, however, made the latter claim, e.g. in Berg (ed.), *op. cit.* note 2, 34.

[109] Novik, in Berg (ed.), *op. cit.* note 2, 34.

. . . the appropriate lesson should be drawn, as it can be expected that many more useful ideas, worthy of attention, will penetrate to us in similar ideological decorations.[110]

These quotations are symptomatic of the commonest interpretation among Soviet scientists of the rejection of cybernetics and its legitimation. If the analysis presented above is correct, however, this view is less than complete. The initial rejection of cybernetics cannot be laid at the door of philosophy alone; nor can it be argued that the process of innovation involved only the extraction of the scientific kernel from its ideological shell. It is clear that opposition to cybernetics came not only from the 'philosophers', and that those so labelled were not all professional philosophers. Apart from the objections of philosophers, and the scepticism amongst scientists, opposition appears to have been most deeply rooted amongst those committed to the Lysenkoist and Pavlovian orthodoxies which had been established at the 1948 and 1950 conferences. Acceptance of the wider claims of cybernetics would have entailed the abandonment of theories and paradigms to which those communities were committed.

The label 'philosopher' is not, however, wholly arbitrary or without significance. It is to be understood in the context of the structures of Stalinist science, and of the effect which those structures had on the social control mechanisms in the scientific community. The name 'philosopher' was applied to those who availed themselves of the 'techniques or persuasive argumentation' characteristic of Stalinist science: the 'philosophical cudgel' was available not only to philosophers, but to natural scientists as well. This makes it difficult to distinguish scientists from philosophers, or indeed to say who is a 'scientist'. It is nevertheless significant that the advocates of cybernetics largely avoided philosophical discussion until their claims had been recognized as legitimate. They shied away from such discussion as long as its outcome might have affected the fate of cybernetics, for that would have been to recognize the claims of the 'philosophers' to authority in natural science. The central point at stake was the locus of scientific authority: who constitutes the relevant community for theory choice? The conflict thus went much deeper than disagreement about theories or paradigms; what was at issue was the consensus on values and cognitive norms which, in Kuhn's analysis, provide the sole basis on which paradigm choice can be made. The problem therefore is not philosophy or dialectical materialism as such, but rather their place in the structure of Stalinist science. It follows from this that to attribute the rejection of cybernetics to philosophy is incomplete and

[110] A. I. Berg, 'O nekotorykh problemakh kibernetiki', *Voprosy filosofii* (1960, 5), 54.

misleading, in as much as philosophical objections could have effect only within a particular set of structural relationships.[111]

The second part of the retrospective Soviet interpretation must also be challenged: the process of innovation involved much more than extracting the scientific kernel of cybernetics from its ideological shell. Certainly the advocates of cybernetics saw this as their task. But they concentrated, naturally enough, on discarding the hostile social and political values associated with cybernetics, while underlining those that were acceptable. They linked the theoretical claims of cybernetics and the practical uses of automation and computers in defence and the economy; they left intact the hostile image of capitalism while pointing to the benefits automation would bring in a socialist society; they stressed the importance of a science of control in a society which developed consciously and in a planned manner; they explained the intellectual traditions of which cybernetics formed a part—with appropriate emphasis on Russian and Soviet precedents. Thus both the advocates and the opponents of cybernetics were willing to appeal to factors which, by any account, must be seen as extrinsic to the scientific core of the subject.

The advocates of cybernetics used extrinsic arguments that supported their claim that they had the right to decide what constituted scientific truth. The opponents of cybernetics, however, made what could be interpreted as an implicit appeal to the authority of the Central Committee and the ideological establishment. The dispute can be seen, therefore, as an institutional conflict in which the relative authority of two communities, who did not share a common set of values and intellectual experiences, was at stake. The dividing line between the two communities was, however, very blurred, especially in the biological sciences: the use of the 'philosopher' illustrates this, since some of those so labelled considered themselves to be natural scientists, albeit of a new type. Thus the claim for autonomy provided a way of settling the bitter conflicts which had marked Soviet biology since the 1930s. I do not wish to imply that philosophers and scientists were divided because one community adhered to dialectical materialism while the other did not. I have not examined this argument here, but I think that it would be a quite misleading oversimplification. From the subsequent philosophical discussion of cybernetics it seems clear that the advocates of cybernetics were not opposed to dialectical materialism or philosophy as

[111] In saying this, I make no judgment as to whether the philosophical objections were valid, or Marxist, or whatever.

such, rather that they wished themselves to judge the relevance of philosophical arguments to their work. They resented the interference, or potential interference, of the Central Committee in what they felt was their own sphere of competence. Natural science, they argued, was to be the dynamic element in the relationship between science and philosophy. It should be clear that in sociological terms at least, the establishment of the Soviet school of cybernetics is not to be interpreted as the triumph of truth over falsehood, or virtue over vice, in as much as both groups used external arguments.

The external arguments used by the advocates of cybernetics were especially powerful in the mid-1950s because they coincided with a radical change in the social functions of science. There was a growing awareness in the Soviet Union in the mid-1950s that a revolution in science and technology had begun: science was in the process of becoming a direct productive force. Engels' comment that 'if society has a technical need, that helps science forward more than ten universities',[112] seemed to be outmoded since scientific progress was providing the basis for new technologies that were contributing to the political purposes of defence and economic growth. The association of cybernetics with computer technology and automation was thus a powerful factor in its favour. The political relevance of cybernetics was further enhanced by the need for improvement in economic planning and administration, for these seemed to offer great scope for a science of the general laws of control. In the 1960s, these social factors continued to provide the context in which Soviet cybernetics developed. They also made it easier for those who wished to move the locus of scientific authority back to the specialist communities—a process which seems finally to have been achieved in 1965 with the destruction of Lysenko's influence.

I have been concerned in this paper primarily with the study of a particular case, although I have tried to situate it in the context of recent discussion about the processes of innovation in science. In as much as some of the major issues in that discussion emerge as central problems in the Soviet arguments about cybernetics in the 1950s, the theoretical discussion serves very well to illuminate the specific case. At the same time, however, the particular case appears to raise an issue of wider relevance, for it suggests that the distinction between internal and external factors in the growth of scientific knowledge may be very unclear indeed. Kuhn speaks of 'the techniques of persuasive argumentation effective within the quite special groups that constitute the community of scientists'.[113] The arguments about cybernetics in the Soviet

[112] Marx and Engels, *Selected Correspondence* (Moscow, n.d.), 548.
[113] See note 7, above.

Union indicate that it may not always be clear who those quite special groups are; nor what techniques of persuasive argumentation are effective within them. If, in times of crisis, paradigm or theory choice is made on the basis of shared values and experiences within a specialist community, the choice can be influenced by social, economic, ideological or political factors in two ways. Firstly, scientists may appeal to these factors in the process of attempting to communicate with and persuade their colleagues. In this case the scientist appeals to non-scientific, or extra-scientific, values shared by the community. Secondly, the appeals to external factors may be addressed to other audiences: for example to cultural authorities, or to fund-givers. Such appeals imply at the very least that judgments made by those outside the community of scientists will affect the distribution of scarce resources—money, power and prestige—to that community. And, unless one makes quite special assumptions about scientists, the distribution of scarce resources within the community will also be affected, and the mechanisms of social control disturbed: why should one suppose, for example, that scientists will always value recognition from colleagues above recognition from outside groups—particularly when, as in a crisis, there is no agreement about what problems are important and what kinds of solution acceptable? And if the social control exercised by socialization, authority and social exchange is destroyed or disturbed from outside the community, paradigm choice cannot be explained in terms of the values and experiences of the community alone. In the light of these two possibilities the distinction between internal and external factors in the growth of scientific knowledge seems to break down, and the whole idea of an internal logic of development in science becomes problematic.

It may be objected that this is too far-reaching a suggestion—conclusion would be an immodest word—to make on the basis of a single case study; that the conditions of Soviet science were not normal; and that if any lesson is to be drawn it is that outside interference serves only to hold up scientific progress. Since as I have been concerned mainly to analyze a particular case, I do not wish to go into these questions here. But several points must be made. The case of Soviet cybernetics does suggest that external factors may reinforce divisions within the scientific community by affecting the mechanisms which normally ensure intellectual conformity. It does not, however, prove that paradigm or theory choice either is not, or ought not to be, influenced by external factors. Indeed it can be argued that the cases in which specialist communities resist external pressure should direct our attention to those cases in which external factors are influential and success-

fully appealed to, and not merely lead us to reject all external influences as harmful. Moreover, if one wishes to say that scientific progress was 'held up', one needs to have a criterion of scientific knowledge independent of the 'assent of the relevant community'. If one argues, as Kuhn seems to do, that such assent is the highest standard possible in the choice between incommensurable paradigms it becomes extraordinarily difficult to argue that scientific progress has been delayed by factors—whether internal or external—which affect the process of paradigm choice; for the understanding that emerges from the arguments may be different in important respects from what would have emerged otherwise.[114]

The case of Soviet cybernetics raises therefore an issue of general importance and relevance. If the values and norms which bind the scientific communities together in times of crisis provide the basis on which paradigm choice is made, it appears that the distinction between internal and external factors in the growth of scientific knowledge breaks down. If the idea of an internal logic of development in science is to be retained, some criterion of scientific knowledge other than the 'assent of the relevant community' is required. In saying this I am not arguing for a return to the Popperian concept of scientific progress; but I do wish to suggest that Kuhn's present position is unstable. For to point to the 'assent of the relevant community' as the highest criterion of scientific knowledge makes necessary a much wider sociological analysis—in order to examine what the 'relevant community' is, what position it occupies in the social system, and how and why it gives its assent—than Kuhn or the Kuhnians have yet undertaken.

[114] The case of Soviet cybernetics differs from the Lysenko affair in several respects, not least by virtue of the fact that the Lysenkoists attacked an accepted and well-entrenched paradigm, whereas the opponents of cybernetics were attacking an innovation.