

THE RADICALISATION OF SCIENCE

Hilary Rose and Steven Rose



THEMES

OVER the last five years there has been a clear shift in consciousness of many scientists—especially science students—of the role of science and technology in contemporary capitalism. This movement has been concentrated in the U.S.A. and Britain, the two most scientifically advanced Western countries, judged by such formal criteria as percentage of GNP spent on science, or numbers of papers published or Nobel prizes per head of the population.

Significant developments however have also occurred in Belgium, France, Italy and West Germany. The movement has embraced scientists and non-scientists alike in a variety of shifting and uneasy alliances of old and new left, liberal concern over actual or potential scientific abuses, explicitly Marxist attempts to analyse the contemporary functions of science and technology under capitalism, and libertarian, anti-scientific, or even frankly reactionary attacks on the rationality of scientific method and the anti-human quality of technology. The movement is still in rapid development and has few obvious historical parallels. This article attempts an analysis of its directions and future perspectives. As actors as well as observers in the movement, our account will focus on those aspects which we have most detailed knowledge of and which seem to us most relevant; hence this article mainly discusses Britain and to a lesser extent the U.S. and W. Europe. No reference to the interesting parallel developments in Japan has been made, an omission which reflects our own ignorance. Three major themes have, in varying degrees, been reflected in most of the groupings and debates that have occurred in the past few years.

1. The abuses of science

This is perhaps the broadest area, ranging from concern over environmental issues such as non-returnable bottles, DDT in mothers' milk and penguin fat and the 'population explosion'; through fears of potential scientific advances such as genetic engineering, psychosocial

control technologies and the computer invasion of privacy; to an explicit recognition of the harnessing of science to the purposes of imperialism, as in the application of new technologies to the war in Vietnam and the absorption of university science into secret research funded by defence departments. Although analyses of such abuses have often been politicized so as to demonstrate that they are an inevitable consequence of doing scientific work within an oppressive social order, many scientists have tended, nonetheless, to see them as indicative of external pressures upon a science regarded as a relatively autonomous (neutral) activity; the pressures generating, therefore, particular perversions or abuses of an otherwise desirable—even progressive—activity. The dominant ideology of science, particularly in the élite sectors of university and research institute, generates a mystifying conception of scientist's work, stressing his ownership of his paper, his membership of the community of science, so that 'uses' and 'abuses' are seen as independent and unrelated to his activity. It is the strength of this belief which leads to issues being posed—perhaps even necessarily in view of the very low level of consciousness—in terms of personal or even social responsibility, rather than in terms of alienation.¹

2. *Science as a non-neutral ideologically-laden activity*

Here the analysis goes deeper to consider the nature of the scientific process itself. Science done within a particular social order, it claims, reflects the norms and ideology of that social order. Science ceases to be seen as autonomous but instead as part of an interacting system in which internalised ideological assumptions help to determine the very experimental designs and theories (paradigms) of the scientist himself (herself).²

One consequence of this analysis has been that attacks on the role of science under capitalism have become expanded into attacks on science and scientific method, as themselves products of an oppressive capitalist order. As a consequence of a mystification which has elided the methods of science with its goals, a chorus of voices—ranging from the dropout to the religious high conservative—has ascribed contemporary social problems in large measure to the rationality and scientific method which lie at the core of technology-based industries. A retreat from industrialization towards a simpler life style, and a belief system based on anti-rational premises, typified by much of the environmentalist movement, are the proffered solutions.³

3. *The self-management of science*

This area of debate takes the argument within the laboratories themselves; how are research programmes and individual experiments planned and carried out? In whose names are they published?

Scientific tradition is élitist, dominated by individualistic expertise (and generally male expertise at that), despite the ostensible communality of the scientific method. Can laboratory work itself be collectivised and democratised, the barriers within the laboratory and between the laboratory and the external world be broken down?⁴

With these themes in mind we can now trace the roots and development of the present movement.

2

HISTORICAL ANTECEDENTS OF THE RADICALISATION OF SCIENTISTS

RECENT developments in the radicalisation of science and scientists must be seen as the culmination of a lengthy historical process during which the scale and nature of the scientific endeavour have been transformed and the work situation and consciousness of the scientist consequently profoundly modified. The late 1960s saw the end of a period of virtually continuous scientific expansion in Britain stretching back about as far as the seventeenth century. This expansion, in terms of both money spent and scientific knowledge gained (at least defined as papers published) had represented a regular doubling every 15 or 20 years.⁵ From the nineteenth century onwards the reciprocal links between science, technology and society became steadily firmer and more binding, resulting through that century and to the First World War in a steady incorporation and the institutionalization of the activity and consequences of science. At the same time this institutionalization permitted—indeed, even encouraged—the powerful mythology of scientific autonomy by which research—especially university research—was seen as pure and divorced from technology, industry and all their doings. This ideology of autonomy was seriously shaken by the 1914-18 war, when for the first time the technological weaknesses of the British Empire became so dramatically exposed that direct government intervention into the management of science, the establishment of the Department of Scientific and Industrial Research and the first joint State-private enterprise Co-operative Research Associations, became necessary.⁶

The inter-war years saw the slow response of scientists to the changed situation, beginning with the early attempts at unionization. The Association of Scientific Workers was established with an explicitly socialist platform in 1918*. At this point the problems of science were seen, even amongst socialist scientists, as predominantly related to economics—under the circumstances of British capitalism between the

* First as the National Union of Scientific Workers.

wars, scientific spending was pitifully small; an expansion in the scale of scientific spending, coupled with the adoption of centralist planning techniques, could, it was argued, release the transforming powers of science, help it liberate the world. Meanwhile abuses which arose from particular applications of science represented, it was assumed, the consequences of attempting to conduct research under capitalism. By the 1930s the model for the harnessing of science to the service of society was seen by most of this group of scientists as the Soviet Union.

It was indeed from the Soviet Union that the second of our major themes, that of the ideological determination of science, was injected into the British debate with the appearance of the Soviet delegation at the 1931 London conference on the history of science. Although the delegation was headed by Bukharin, its major contribution was provided by a paper from Hessen on *The Social and Economic roots of Newton's Principia*. This argued that the *Principia*, the high point of seventeenth century science, was not an isolated product of scientific genius generated by the internal logic of science alone, but rather had emerged as a consequence of specific technological requirements of the developing British bourgeoisie.

The theories presented by the Soviet contributors introduced at least one aspect of Marxist analysis of the internal content of science to an audience, partly of highly empiricist British historians of science, and more importantly, to a group of young British Marxist-inclined scientists. The Soviet contributions were derived from debates on what a socialist science would be, which had begun to develop during the late 1920s and the early 1930s in the U.S.S.R. These debates had two strands; the first was drawn from the discussion in Engels' *Dialectics of Nature* and Lenin's *Materialism and Empirio-Criticism*, which had explored the relationships of dialectical materialism to scientific knowledge. In these strands of the debate the objectivity of the natural sciences was assumed, and represented the model towards which Marxism, as a scientific socialism, should approach. As objective science proceeded, Engels assumed, dialectical laws would be found to operate in nature; Lenin's contribution was the argument that materialism demanded a copy-theory by which perception was related to reality. (This is perceptively discussed by Coulter in his article in the 1971 issue of *Socialist Register*.)⁸

It was this first strand in the Soviet debate which was to become of major significance internally in the late 1930s and again in the period 1948 to 1953. The second strand raised the question of whether a socialist society would generate a specifically *socialist* science; was there a unique socialist biology, by contrast with bourgeois biology, for instance? In so far as Newtonian mechanics were seen by Hessen as the product of a particular historical period in bourgeois society, the

answer to that must have been seen in the affirmative; what Hessen's contribution in 1931 (and indeed subsequent Soviet discussions in this area) have not adequately analysed, however, is the question of whether there is indeed a bourgeois, by contrast to a socialist, science? If it is accepted that there is, the implication is that there is an epistemological problem at the core of natural scientific knowledge itself. Under these circumstances, science ceases to be the objective accretion of truth about materialist reality, but instead is a reflection of that reality shaped by the perceptions—themselves partially ideologically determined—of the scientist himself.

It is easy to see why this extension of the Hessen analysis* has not found ready acceptance within the Soviet Union, for its consequences, by implying a non-objectivity to science, smack of the Machian Marxist heresy which, however attractive it had been to many revolutionary scientists of the early twentieth century, including Einstein,⁹ Lenin himself emphatically rejected. Rejection of this form of scientific objectivity explicitly denies the copy-theory of perception which Lenin embraced, and would therefore be seen by him and subsequent Leninists as idealist.¹⁰

Yet there does not seem to be any convincing obligation on the part of dialectical materialists to accept that the necessary alternative to the copy-theory is idealism; materialists must accept the objective nature of external reality, but our knowledge of that external reality is by definition mediated by our perceptions. It is not unmateriast—far from it—to claim that human perception is itself socially modified, that the way an individual perceives, and interprets what he perceives, is not absolute and independent of context. Indeed rather than search for a dialectic in nature, following Engels, it would make more sense to examine the dialectic between nature and human perception of nature. If perception is socially modified—if, therefore, there are ideological elements within it—it follows that science, which is a product of this dialectic between the external world and man's perception of that world, has itself an ideological component. The science that is done, the experiments that are made, and the interpretations that are offered, are none of them independent of human society. Insofar as they are not independent, it becomes simultaneously fruitless to search for dialectics in nature, and to argue for Marxism itself an objective status, external to man, merely because it is scientific. On the other hand, it becomes extremely fruitful to ask questions concerning the nature of a socialist as opposed to a bourgeois biology, and to anticipate that their interpretation of nature will be different.

* Hessen's work does not reappear in later (post-1936) Soviet discussions in this area.

This is of course not to say that there is *no* relationship between scientific knowledge and objective reality. Neither under socialism nor capitalism should the individual 'facts' accumulated by science¹¹ be different. Using the same instruments and doing the same experiment, socialist and bourgeois scientists should obtain the same results; it is in this sense that science is objective. Where the difference (that is, the non-neutrality) emerges lies precisely in the *selection* of facts to accumulate, experiments to perform, and theoretical frameworks within which to set these facts. Even here there is not total ideological freedom for the scientists to design his theories; they are limited both by the internal logic of his science^{2,6} and the necessity that there is no mismatch between predictions derived from the theory and further facts collected in order to test it. It may be the case that the dialectical mediations between objective reality and the scientific interpretations of this reality are not uniform in all sciences, but there is a gradation, the mediations being strongest in the social and biological sciences, weakest in physics and mathematics. But such analyses are presently only tentative. The point which concerns us here is that Lenin's dichotomisation, which labelled such relativism as idealism and from which the present day Althusserian concept of science is in linear descent, is inadequate, as Korsch and Lukacs were to point out in the context of philosophy¹², and it has confused analysis subsequently. Much of the Soviet debate, for instance, was to proceed as if there was a socialist as opposed to bourgeois science, but it could never bring itself to state clearly the premises upon which such a proposition was based, for fear it would be labelled idealist.

But the unravelling of this argument, though implicit in Hessen, was not perceived by the British Marxist scientists in the 1930s. Rather, like Haldane, they were to spend their theoretical strength over the next few years in a relatively fruitless endeavour to demonstrate the negation of the negation, the interpenetration of opposites, and the transformation of quantity into quality in a variety of scientific developments.¹³ Only when, much later, Needham turned his attention to the history of Chinese science and technology^{*14} and Bernal attempted first the seminal *Social Function of Science* (1939), and later the rather more synoptic and less satisfactory *Science in History*, was the Hessen experience to bear fruit.¹⁵

Meanwhile another strand of relevance to the later debate emerged with the foundation in 1932 of the Cambridge Scientists' Anti-War Movement, a loose grouping which was to provide the scientific and

* Needham himself, in his foreword to the new edition of *Science at the Cross Roads*, has stated that the Hessen paper was not perceived as very relevant even by the Marxist scientists when it was first given.

political cradle of many of those who were to become the grand old men of the 1960s movement. The group concentrated its attack on the abuse of science by the military, and many of its members were to become associated with attempts to rescue Jewish scientists from Hitler Germany. Later they were to be involved in the big ARP campaign to improve civilian precautions against air attack.

But the apotheosis of the 1930s movement was certainly Bernal's *Social Function of Science*, which effectively summarized the critique of science under capitalism which the group had developed. For our purposes there is one aspect of the book which is strikingly in contrast with the present situation. Bernal's science is optimistic, of its nature progressive and hence the natural ally of socialism. Socialism above all means planning, the rational and logical exploitation of science for the benefit of the people. Only plan, increase the scale of scientific spending some ten-fold (to the level of that current in the USSR for example) and defeat capitalism and all would be well. The ideology of what was to become techno-economism began to take shape. The élitist assumptions of this model of a socialist science, which was certainly reflected in the work relations of the Marxist scientists themselves, were to represent a source of trouble that was only to begin to be analysed more satisfactorily in the late 1960s*. In a sense the Wilson election manifesto of 1964, concerning the forging of socialism in the white heat of the technological revolution, was a debased after-echo of the Bernalian analysis of 25 years previously.

1939-45 was to complete the process which 1914-18 had commenced, the rationalization and institutionalization of the organization of science itself, with the energetic support of even the most laissez-faire members of the scientific community. A register of scientific manpower was completed, and scientists permeated most branches of government. The anti-war group of a few years previously now advised on the most effective types of bombing patterns, radar, chemical and biological weapons, or defence against them, and of course, the Bomb. The Bomb above all meant the mobilization of scientists; individual brilliance became secondary to team work in the biggest co-ordinated research and development effort ever. For many scientists the brief years of the New Atlantis at Los Alamos, before Hiroshima and Nagasaki, were to remain thereafter a creative and emotional high point in their lives. Yet the mushroom clouds of the two explosions symbolized, as all involved clearly recognized, the end of an era.

By the end of the war, the autonomy of science had become a myth

*In its early days the A.Sc.W. had two grades of membership, one for scientists, the other for technicians, and there had been serious debates as to whether technicians should be admitted at **all**.

helping to ensure that those students with first class degrees remained in the universities and those with second classes went into industry. The utility of science to capitalism and imperialism had been demonstrated with a classical simplicity which was to result in the exponential growth of research and development budgets with scarcely a question from government or people for the following twenty years. In addition, however, the public image of science began to change from the ambivalent to the downright malevolent. Perhaps most important, the scientists' consciousness had been modified because they felt themselves not merely responsible for the production of the Bomb but its very conception, by proposing its manufacture to governments both in Britain and the U.S.A., whilst at the same time they had failed to prevent its use against Japan.

This conjuncture of attitudes amongst scientists was to characterise their political style in subsequent years. Partly because the Bomb had been so devastating, the scientists, as an *élite*, seemed to assume that they carried particular political responsibilities in that they believed that the research they did, unlike that of the historian or artist, reacted very directly upon society. At the same time, there was a danger that the Bomb would be seen as an *inevitable* result of physics, so that anyone with qualms concerning its use would have to stop doing physics; the responsibility would have been too much (indeed many did go into biology for these reasons). The defence against this criticism was to claim the *neutrality* of science, as a force either for good or evil depending upon the whims of society. This convenient conjuncture enabled many scientists in the two decades that followed, to continue simultaneously to do high science—even accepting research grants from the military to do it—while at the same time professing radical political attitudes or arguing against particular developments in the arms race; thus the discord between the objective reality of the uses of science and the myths of its protagonists became almost complete.¹⁶

This move towards the attempted divorce of science from politics, leaving only the *uses* of science in dispute, was hastened by events in the Soviet Union. The debates over the distinctions to be drawn between socialist and bourgeois science which had begun in the late twenties had long since been confounded with the vices of Stalinism; the equation of Russian patriotism with socialism, so that even bourgeois Russian scientists of the nineteenth and early twentieth centuries were to be lauded to the skies (Lobachevsky, Butlerov, Popov, Pavlov, Michurin); the ossification of bureaucratic solutions to intellectual problems; all combined with a desire to adopt scientific strategies which would most rapidly yield technological and industrial payoff.

In almost every branch of fundamental science serious questioning of

theoretical formulations became hopelessly and tragically confounded with a variety of these other factors. Some of these debates began in the 1930s; others were to appear only after 1945; all were to reach their maximum intensity over the period 1948-53, where they are often (wrongly it would seem) associated with the high peak of Zhdanovism in cultural control.¹⁷ Complementarity, quantum theory and relativity in physics were attacked as undialectical and unmaterialist (at one point Einstein was under attack both in Nazi Germany and in the Soviet Union). The idealism and mysticism of Jeans and Eddington in astronomy in the 1930's and the later 'big bang' theories of the origins of the Universe were challenged. In the late 1940's Linus Pauling's resonance theory in chemistry which claimed that for some molecules there exists no single unique structure, came under heavy assault as unmaterialist.

But the weightiest challenge to western scientific orthodoxy came in biology—the genetics dispute associated with Lysenko.¹⁸ In this debate more than any other, scientific and philosophical issues were obscured by extraneous noise. In principle, as Haldane and the American socialist geneticist Miiller who worked in the Soviet Union in the mid 1930s (and indeed many others) recognised, the concept of the gene is thoroughly materialist. It can be isolated, it has a defined chemical composition, and indeed it can be manipulated. Miiller, for example, put forward a programme of socialist eugenics, including the preservation of Lenin's sperm for subsequent implantation, which would surely have appealed to Stalin in his most grandiose moments¹⁹ Soviet plant genetics, particularly the research of Vavilov, became world famous. The attack by the agronomist Lysenko on 'Mendel-Morgan-Weissman' genetics as idealist and reactionary, anti-darwinist and anti-social was not only based on inadequate scientific data, it was refuted by Marxist biologists both in the U.S.S.R. and the West. The success of Lysenko's attack, from the late 1930s onwards, when Vavilov was arrested and subsequently died in captivity, depended partly on Lysenko's association of the orthodox geneticists with the racist views of the Nazis. Even more important was the appeal of Lysenko's claim that a socialist biology, based on the inheritance of acquired characteristics and later of the denial of competition between plants of the same species (for instance his insistence that trees of the same species co-operated with one another for mutual survival à la Kropotkin, and should therefore be planted in close-packed clusters, where in reality they generally failed to survive) would transform the backward nature of Soviet agriculture. Lysenko claimed that his biology derived from the experience of the people—he was himself of peasant origin—and that it contrasted favourably with Vavilov's bourgeois approach which could not

promise instant results.* The consequences for Soviet agriculture were disastrous. It was precisely the genetic approach of the breeding of hybrid wheat and exploitation of the genetic techniques of polyploidy which were to improve crop yield so dramatically in N. America. For many years, as the Minister for Higher Education in Vietnam put it somewhat ruefully in 1971, 'It was difficult to develop biology when polyploidy** was not a socialist concept'.²⁰

So far as the debate in the Soviet Union was concerned, and despite the arrest of Vavilov at the end of the 1930s, the arguments were not really over until 1948, when following a full-scale conference of geneticists, agronomists and philosophers of science—in which the geneticists had much the better of the argument—Lysenko was able to reveal that his line had the support of Stalin himself. Virtually all his opponents were then forced to recant. The bureaucratic pressure towards narrowly conceived ideological purity in science did not slacken in the Soviet Union until 1953.

The Lysenko debates were published in full in Britain, and considerable argument took place amongst the Marxist scientists, reflected through several issues of the proceedings of the Engels Society.²¹ Several of the participants, certainly to their later embarrassment, swallowed scientific caution and accepted Lysenko whole despite their difficulties in coping with the manifest inadequacy at best and fraudulence at worst of Lysenko's experimental data. Few, other than Haldane—who categorically denied Lysenko's claims and later quietly resigned from the Communist Party after many years, during which he had contributed a regular science column to the *Daily Worker*—seemed to have had any firm grasp of both the socialist and the scientific issues. Bernal, in contrast to Haldane, saw the debate in primarily power political terms, arguing that to victimise the Soviet Union at the height of the cold war was to help the capitalist system and that the Soviet Union was doing socialist science irrespective of the validity of Lysenko himself.

But the repercussions of the Lysenko affair could not be damped down. One consequence of the embarrassment they caused was to speed the move towards the concept of 'neutrality' as a refuge for left scientists in Britain. If 'socialist biology' led into such open conflict with both intellectual integrity and scientific orthodoxy then it was far better to abandon that particular way forward. Meanwhile the geneticists either left the Party or relabelled themselves as radiation

* Maoist students in Britain were to revive the cult of Lysenko as a "Peoples' Scientist" in the late 1960s.

**That is, the presence in a single cell of double the complement of chromosomes—a situation that occurs particularly in some plants, and represents a favourable springboard for the development of new varieties.

biologists. Bureaucratic distortions of dialectical materialism in science in the Soviet Union and the industrialisation of science as an essential support of post-war capitalism in Britain and the U.S.A. were together enough to inhibit further development along these lines for more than two decades.

Instead, activity was diverted into other areas more directly related to the immediate political crisis; the shadow of the Bomb and the threats of the cold war. Characteristically different developments occurred in the U.S.A. and Britain. In America those most immediately affected by the Los Alamos/Manhattan Project experiences, the high physicists, launched a dissenting journal of studies in disarmament and the social problems of scientists, the *Bulletin of the Atomic Scientists*, which was, because it concentrated on the nuts and bolts of disarmament rather than the political problems, to prove readily capable of co-option. The U.S. scientists had made their protest before the Bomb was dropped, and they failed. But their technological and scientific confidence was high. Strongly in the rationalist tradition of science, they seemed to believe that if only a technological fix could be found, disarmament could be achieved despite the underlying political and ideological issues which divide the U.S.A. and U.S.S.R. It is not surprising that the consequences of this approach were on the one hand the co-opted Think Tanks and games researchers, the Kennedy Liberals and even, eventually, the Department of Defense contracts, and on the other the élitist attempts to promote convergence with the equivalent Soviet élite by way of institutions such as the Pugwash conferences. Meanwhile unionisation and political activity amongst rank and file U.S. scientists slumped.

In Britain some of the left scientists co-opted during the war, like Blackett, remained close to government thereafter." Those who remained outside government campaigned actively on the Bomb issue. The physicist E.H.S. Burhop for instance claims to have spoken at 150 meetings on atomic weapons within two years. Science was seen as a neutral bridge across the cold war gulf.

A Science for Peace group (along with peace groups for other cultural groups) was set up in Britain in 1950, but it had an active period of no more than a few years. At the same time, union work attracted a good deal of attention; the A.Sc.W. passed through one of its more flourishing periods before becoming an almost exclusively technician's trade union at the end of the 1950s. It was scientists such as Bernal and Wooster who saw in the A.Sc.W. the possibility

* It was hard to credit that Blackett, who ended in the 1964 Labour Government's Ministry of Technology and as President of the Royal Society, had ever had his passport blocked to prevent him visiting the USSR, for fear he would be taking with him vital atomic secrets.

of establishing an international scientific union. The World Federation of Scientific Workers was inaugurated in 1948 with a statement about the social role of science and a charter setting out the rights of scientific workers, seen as including some control of the utilisation and consequences of their science. The Communist physicist Joliot-Curie in Paris was the first president of W.F.S.W., and its leadership has remained with the Anglo-French physicists ever since. Its major source of finance, because affiliation is on a per capita basis and the Soviet Union has the largest relevant union, has been Soviet Russia.

The flurry of activity in the 'late 1940s soon died down however. Science for Peace gently submerged and the early burst of activity over the nuclear weapons issue gave way to the quiescent despair from which C.N.D. and the New Left were to emerge so dramatically from 1956 onwards. Scientists were prominent in the Campaign—not merely the older ones but a new generation as well—but because of the essentially moral-pacifist, apolitical nature of C.N.D., explaining the facts of fallout did not demand a Socialist commitment from the scientists. Their role as representatives of a particular Bite (along with clergymen, actors or M.P.s) and their activity in pamphleteering or addressing meetings, did not necessarily either politicise or radicalise them. When they took on the government experts on radiation levels and hazards and won handsomely, they found themselves in their turn co-opted as experts on seismic detection of underground explosions, the monitoring of fallout, radiation hazards and so forth, travelling the same path as the physicists of the *Bulletin* had taken earlier in the States. The rapid incorporation of Pugwash, following its inception in 1957, into an organisation fostering semi-official communication between the U.S. and U.S.S.R., demonstrated this process.

Disarmament was now a matter for experts, conservative or liberal-dissenting; the S.A.L.T. talks would follow the Partial Test Ban Treaty separated only by learned articles in the specialist journals and almost unaffected by such irritating tremors on the geopolitical scene as Vietnam or Cuba. Co-option of the scientific élite in a part-time capacity was matched by the diversion of many rank and file peaceniks into research institutes ostensibly to work on issues of 'peace', 'conflict', etc. often sponsored by the State, and leading to such subsequent ironies as conflict research on Northern Ireland in 1969 being sponsored by the Home Office. Thus, despite their goodwill and integrity, the growing self-awareness amongst scientists was safely contained.

C.N.D. therefore, did not result in the politicisation of scientists as scientists; Bomb-makers could be anathematised, for the *basic* scientific research on which the Bomb depended was largely complete; the escalation of *technology* did not find European university scientists—

or many industrial scientists for that matter—actively involved in research which directly led to further Bomb production; the high technology and specialised scientific base of the Bomb ensured that the challenge that it presented to scientists was almost as satisfactorily moral as that it presented to the clergy.

It is perhaps more surprising that the emergence of the New Left at about the same time as C.N.D. was not to catalyse a much deeper analysis of the contemporary role of science, as it was to assist in the transformation of many other areas of cultural and political analysis in Britain. The *Universities and Left Review* leadership was uneasily conscious of the importance of science and the desirability of mobilizing scientists; there were attempts to set up a New Left scientists group, but the response was slight and the enterprise short-lived. Even on New Left platforms the scientists who spoke remained firmly in the social democratic mould; any discussion which did occur concentrated almost exclusively on the use/abuse issue.

The ideological concerns which formed so important a theme even a decade previously remained buried; the question of *élitism* had not been raised; rather, even on the left, the mystification induced by Snow's high liberal concept of the Two Cultures was to confuse debate for several years to come, for no socialist analysis of the cultural role of science was attempted by the New Left theorists. Science finds no discussion in *The Long Revolution*¹² for example, and Anderson's otherwise brilliant essay on *The Components of the National Culture*¹³ excludes science from his analysis. The 1930s Marxist scientists are dismissed with a passing reference to 'the fantasies of Bernal' as leftist intellectuals who 'would be blown away by the first gust of the international gale'. This is precisely what did *not* in fact happen to the 1930s scientists, whatever was the case for the poets. Anderson's list of the components of British culture includes sociology, philosophy, political theory, history, economics, psychology, aesthetics, psycho-analysis, anthropology and literary criticism. His table of eminent *émigrés* who have contributed to British culture makes no reference to perhaps the most conspicuous group of all—the scientists such as Born, Chain, Frisch, Gabor, Krebs, Peierls or Rotblat. Ironically, the political position of many of these figures would have sustained rather than refuted Anderson's analysis concerning the reactionary nature of those *émigrés* who came to Britain rather than the U.S. The point is that, a passive victim of the Snow thesis, he cannot see them as 'cultural' figures any more than he can accept the serious contribution of the British Marxist scientists such as Haldane and Needham, who fail even to find a mention in his analysis. It is hard to escape the conclusion that there was a ducking of the question of science by the *New Left Review* which has in more recent years

reflected itself in an enthusiasm for the pretentious scientism of Althusser, an arid reversion to the most mechanist interpretation of the relationships between reality and scientific epistemology²⁴.

In their concern to rediscover the young Marx, to pursue the themes of alienation and explore the French structuralist tradition, the New Left thus ignored the continuing role of science and technology in transforming, as well as being transformed by, the economic base." They showed, and still show, no recognition of the need felt and clearly expressed, by Marx, Engels and Lenin, to understand the *content* of science and to assimilate this content to their social analysis. It is not merely that the question of the relationship, for example, of D.N.A. and the genetic code to Marxism was ignored by the New Left in a way which would have been inconceivable to Marx, but that very little recognition of the fact that the consequences of the application of science can now readily destroy the world appears to their writings.

The long debates amongst Soviet philosophers as to whether the new science of cybernetics could be incorporated into the framework of dialectical materialism, or whether it required the addition of a fourth dialectical principle, which have occurred from the mid 1950s onward, find no echo amongst the New Left Marxists.

The consequences of this failure, together with the penchant of the NLR group in the late 1950s for collaboration with the left wing of the Fabians over a wide variety of practical issues, such as housing and poverty, thus left scientists without a sustaining alternative culture. Between this quiescence and the new phase of the radicalisation of science there was to be a discontinuity.

3

THE NEW SITUATION: INTERNATIONAL PERSPECTIVES

Vietnam was to be the symbol of this break. One of the earliest manifestations of the new wave was in 1966, when the Russell Tribunal sent scientists and doctors among its teams to collect evidence direct from Indo-China for its proceedings. Their evidence on the experimental nature of the war and its use of new technologies, together with the growing heap of news reports filed by war correspondents, compelled the scientists to speak out. Where in the past it has been the physicists, now it was the turn of the biologists, with the left and liberal scientists from a variety of disciplines coming to their support. For many of them the issues were moral rather than political; nonetheless some of the severest criticisms were made by scientists such as J. Mayer,

* Istvan Meszaros is an important exception to this.

Harvard Public Health expert, later to become science advisor to Nixon, who, in a carefully argued letter to *Science* was to describe the defoliation and crop denial campaign as *genocidic*.²³ But while a few of the older scientists like Mayer were co-opted, others, especially the younger ones, moved rapidly from a position of liberal concern to a recognition that the social order itself needed changing. It was not long before they discovered that what Eisenhower had called the industrial-military complex had become the industrial-military-scientific complex and that the universities were deeply involved. This was a unique feature of the American situation, and attempts to translate it into other societies were to come to grief. American universities, ever since the old land-grant colleges, had been heavily dependent financially on, and closely controlled by, big corporations. Following the 1939-45 war, a greater and greater proportion of spending on science and technology was carried by the Federal Government (reaching some \$17 billion by the end of the 1960s); however, rather than adopt the strategy of establishing Federal laboratories and a scientific civil service, as in Europe, in the U.S. the policy was towards contract research in the universities and industry. By the mid 1960s, by far the greatest proportion of university science was being done on federal contract, often for the Department of Defense, and the linkage was most powerful in the Clite, high science, ivy-league colleges. Napalm, for instance, was developed at Harvard and tested on its football pitches by the organic chemist Fieser; its development is described in his neutral-sounding book *The Scientific Method*.²⁴

From the mid-1960s, in campus after campus, students discovered a network of DoD research contracts and institutes; the Institute for Co-operative Research (biological warfare research) at Philadelphia in 1965, the Stanford Research Institute a year later, the Lawrence Radiation laboratory and the existence of a whole class of 'classified' Ph.D work at MIT, and so on. By 1968 *Viet Report* had published lists implicating practically every university in the country, and a variety of 'workshop' groups began devoting themselves practically full time to the collection of information about the involvement of universities and companies in war-related research. The work presented a natural rallying point for political struggle; only when, by 1970, most of the research had been driven off-campus did it become apparent to many that all that had happened was that the research was continuing under a different institutional hat; campus activism, it became apparent, could win quick small victories, but the overall effect was small.

Nevertheless, the campaign had the effect of radicalizing a proportion of science students and scientists; responses ranged from the liberal (the 'Union of Concerned Scientists') to the more radical

groups aligned with the left of SDS. MIT held a brief research strike on March 4th 1969, which soon spread to other universities;²⁷ the biochemist who had worked with the Russell Tribunal, J. B. Neilands, ran a lecture course at Berkeley on the Social Responsibility of Scientists,²⁸ some scientists worked within their profession, by attempting to make, for instance, the American Physical Society discuss the use of physics in the Vietnam war and to pass appropriate resolutions. Other professional groupings became locked into the ABM controversy, which, because of open confrontation over the interpretation of evidence on the effectiveness of the ABM, was by 1972 to have split the U.S. Operational Research Association over the very issue of neutrality and objectivity in science which was the attack point for many on the left. Student activity became more concerned with picketing scientific meetings, particularly the vast annual jamboree of the American Association for the Advancement of Science (AAAS), raising issues both of the uses of science and of the élitism of the platform scientific speakers. One of the most durable organisations to arise over this period has been SESPA (Scientists and Engineers for Political and Social Action) which, in a loosely federal structure, runs the gamut of the left political spectrum from concerned liberals to movement revolutionaries. Its primary concerns have become a self-activating analysis of the role of science under capitalism in the U.S. and a continued concern with elitism in science (its racism, maleness, etc.). For this group, the urgent necessity is to use their science 'for the people', in the service of the Black, Puerto Rican and other minorities, to turn the weapon of science against capitalism.²⁹

One development in the U.S. which has achieved considerable success in diverting the movement from its attempts to analyse the role of science has been the mushrooming growth of the pollution movement, suggested by many as having been fanned as a deliberately mystifying attempt to divert campus activists from the Vietnam war. With the fragmentation of SDS, it has indeed become the case that many who two years earlier would have been in the anti-war movement have turned their attention to the collecting of non-returnable bottles, campaigning for zero population growth or adopting the reactionary anti-rationalist stance which we identified in Section I as one response to the breaking of the myth of scientific neutrality. The politicisation of pollution (as an inevitable product of capitalism) has scarcely checked this tendency.

While the issues, both in the U.S. and Britain, were still being seen primarily in relatively simple use/abuse terms, May 1968 changed the political context of the analysis. Elsewhere in Europe, unlike Britain, the use/abuse themes had not, and were not, to achieve the same significance. Pollution issues for example, were firmly appropriated as

part of the working-class struggle at a recent symposium at the Gramsci Institute in Rome*. The reasons for this are probably to be found partly in the relatively backward state of science in France, Germany and Italy compared with Britain and the U.S.; only France was a nuclear power, and that only recently, and much less was being spent proportionally on defence research. Within the European cultural tradition 'natural science' is not separated off from the social sciences, or for that matter the rest of the intellectual superstructure**—all are part of *Wissenschaft*. Where the cultural web is relatively seamless, to isolate a component in order to examine its uses and abuses is a less convincing exercise; instead the critique can be more easily generalised to relate to the social and economic base. On the other hand the very different university structure of Europe, which, deriving from the Napoleonic reforms, places great authority and power in the hands of an undemocratic professoriat, reducing non-professorial researchers to a semi-technician status, has contributed to the development of militant teachers and scientists' trade unions, in contrast to the emasculated AUT and the weakness of the **ATTI**.

Thus the political activity of European scientists in the wake of the May events of 1968 concentrated on the nature of the work situation itself, with experiments in direct control of laboratories and the organisation of **research**.²¹ University and CNRS laboratories were occupied following May, but with the ebb of that high tide, the sharply posed questions rapidly blurred down to questions of pay, status and the overall reform of the French educational system, which have dominated the French University scene for almost the whole of the subsequent period.

The consequence of the occupations in Italy were more radical and less easily incorporated. The most conspicuous was the occupation of the International Genetics Laboratory at Naples (1968). There the key questions were starkly raised: who controls the laboratory? who owns the research? can the élitist tradition of science—whereby the great men gain prestige as a result of the work of their research assistants, technicians and laboratory cleaners—be broken down, and if so, could science continue to be done? Work stopped in the laboratories whilst the roles of technicians and cleaners—and the local community—in planning experiments and deciding on research directions, the **relation-**

*The struggle against pollution issues, particularly mercury poisoning, in Japan has also been firmly linked to class struggle.

** It is of interest that a recent attack on this cultural tradition by the ex-Marxist biologist Monod had achieved wide popularity in France. Monod argues an essentially Anglo-Saxon position on science, separating it from other social manifestations and elevating its **objectivity** towards the status of a **new ethical framework**.²⁰

ship of experts to non-experts in society, was debated. Eventually the Director, Buzzati-Traverso, resigned (he went to work at UNESCO instead, where it would no doubt prove more tranquil) but the attempt to create socialism in one Institute gradually petered out.²² But the debates were to be repeated in many other places, though without much cross-fertilisation of ideas; each time the arguments had to be—perhaps needed to be—slowly reworked.



THE NEW MOVEMENT IN BRITAIN

In Britain, the first ripples of the new wave began in 1967, in response to the Vietnam war. At first, much as in the earlier style of CND, scientists lectured and wrote pamphlets and articles exposing the nature of the war, making only little attempt to work within and organise their own constituency. One early move to take this second step occurred when a small group of scientists working in conjunction with the Angry Arts week at the Roundhouse in the summer of 1967, sponsored a meeting called 'Vietnam : the Abuse of Science'. This was followed up by a continuing group who set up an international meeting on Chemical and Biological Warfare in February 1968. In general the style of the meeting was sedate and highly professional. However, some light was thrown on the nature of the 'neutrality' of much of contemporary professional science when a French doctor (M. F. Kahn) presented data from the NLF and from the DRV concerning the toxicity of CS. The point he was making was the illegality of the U.S. government's use of an internationally banned poisonous gas. Some of the American scientists present were quite incapable of accepting this data; Hanoi or Vietcong science, by definition, was propaganda. This exchange probably did more to politicise the meeting than any formal analysis of the nature of American imperialism. In 1968 the public breach of the myth of the neutrality of science was beginning to be a means of raising the consciousness of the profession.²³

On the last day of the meeting C. F. Powell, Nobel Laureate physicist, a communist of long standing and President of the World Federation of Scientific Workers, appealed for the formation of a grouping of scientists to grapple with these issues in Britain. Progress was slow, for whereas science and technology played a central role in the war, and the issue of defence research on campus played a part in radicalising students in the U.S., the obstinate fact remained that the social science students were radicalising much faster than their natural science counterparts, who remained relatively conservative. Knowing

the constituency they were seeking to mobilise, and themselves part of it, the group of scientists who had come together initially to campaign against chemical warfare in Vietnam, defined their primary objectives as self education for scientists concerning the control of science, and exposing the abuses of science. Anxious to preserve a mass base, the new group, the British Society for Social Responsibility in Science (BSSRS) thus had to pitch its sights low, yet leave a sufficient openness about its goals for this minimum programme not to impede development.

It was difficult to communicate with the science students, particularly the postgraduate students half way to achieving professional identity. Discussions were often depressing, even frightening. Because science was preoccupied with *things*, scientists, particularly the chemists and the physicists, often almost took a pride in their alienation. Students would explain themselves by reiterating, 'we are only small parts in a machine', 'we are only cogs', and each of these mechanical images entwined with hopelessness would receive nods of agreement from fellow students. It was a little better with biology students, partly because their methodological problems were nearer those of a social world. But it seemed that almost only the religiously committed were uncrushed, because they saw both the problems and solutions in purely moral terms, therefore demanding only a simple voluntaristic answer, a personal moral renunciation of doing particular sorts of science. With the industrialisation of science, the rhetoric of the 'scientific worker', which had been so energetically made by the 1930s and '40s Marxist scientists (themselves typically of Clite origin and with all the confidence of the scientific Clite) became a reality to the student of the 1960s and 1970s. The sheer expansion of science had thus resulted in a proletarianisation of the bulk of scientists' producing a deepening gulf between the ostensibly non-alienated 'responsibility' of its Clite and the alienated apathy of its masses. Because BSSRS was born from the academics rather than from the student's movement, from scientists who were culturally isolated from the New Left (though not necessarily isolated in practical activities), the society began with this fairly modest programme of consciousness raising. Yet while there was substantial discussion concerning the objectives of the society, little took place about its structure so that, for example, although it had acquired a centralist constitution, the National Committee soon found it necessary to read it as federalist in intent. To some extent, confusion of political styles, which was at once the strength and weakness of BSSRS, was to do with the nature of the issues to which a scientific group addresses itself. The expertise required to contribute to the CBW debate, to name the chief preoccupation of the Society in its early days, meant that scientists with a very wide range of political beliefs, were able to

work together in technical discussions. Thus whereas on most issues Trotskyites and Communist Party members had long ceased to be able to work together, on scientific issues it was possible.

Because of this need for expertise the Society began by accepting mentally much of the *élite* structure of science. It acquired a distinguished sponsorship of left and liberal scientists and held its inaugural meeting in the Royal Society.¹⁴ Yet the libertarian tendencies which were later to manifest themselves much more strongly, both within BSSRS and other parts of the scientists' movement, were given some expression by the society's willingness to encourage non-scientists to join. Outside the scientific community this modest step as an indicator of a libertarian tendency may seem absurd, but the rigidity and hierarchical nature of the laboratory is perhaps not fully realised beyond its confines. To cite an equally absurd but illuminating example, a profile published by a scientific weekly of a Marxist professor described his willingness to let his students call him by his *first* name as an index of his revolutionary stance. A combination of inability to cope with the problem of the *élite* nature of science, the *centralist* component in its constitution and the need felt to finance itself and *expand* by playing the media rather than building a movement from the *grass roots*, characterised one continuous aspect of the Society's history.

Thus, from the beginning, BSSRS had tried to relate its science to revolutionary movements for national liberation, and sent a team of natural and social scientists to Derry after the RUC attack on the Bogside in August 1969 to collect evidence on the effects of CS.¹⁵ But their preoccupation with getting the science right and hammering the government experts meant that to a large extent they became caught by the same trap of neo-professionalism which had caught the left physicists against the Bomb. On the issue of CS, BSSRS made the bad mistake of submitting its evidence to the government's Himsworth Committee and failing to write the necessary agitational and educational pamphlets.¹⁶ It was left to others working directly with the Irish movement to do this. Not surprisingly the Society's national committee, weakened by addressing the wrong audience, lost itself in the liberal 'gas or guns' debate, and was doomed to do little more than express moral concern about an unpleasant technology which the left could see clearly was an inherently imperialist weapon.

This tendency to play the Bite-media game has continued to represent one aspect of the Society's *schizoid* personality. Even though it eventually did declare against both CS and rubber bullets, it still, even in 1972, felt it must give a commentary on the Rothschild report, which contained proposals to confirm the industrialisation of science by further limiting the autonomy of the Research Councils,

building an openly declared 'customer-contactor' relationship into the funding of large areas of hitherto 'pure' science. The Society's comment betrayed the confusion still central to its structure; it merely opposed the report for wanting to spoil science by placing it, even more crudely, at the disposal of capitalism, rather than accepting head-on the ideological challenge of confronting capitalism with the demand that science should instead serve the interests of the working class.

The other aspect of the Society was represented by the activities of its more rank-and-file membership. After the inaugural meeting, in April 1969, groups and local societies began to emerge on many campuses and in a few towns and by the first A.G.M., in November, membership had grown to nearly 1,000. Predominantly the membership was academic. But the varying styles between the differing local organisations was immense. Some were early captured by a scientific establishment that wished to do little more than express liberal concern. The Edinburgh society, for example, was initially sponsored by left scientists, but was seen by the University partly as a welcome evidence of social concern and partly as a potentially radical rival to the existing Science Studies Unit; it was soon safely captured by the Unit. The alliance which presently easily develops between the liberal and the old left scientific establishment, emerged in sufficient strength to exclude the possible intervention of young *post-graduates*, and Edinburgh SSRS was soon to settle down as a pleasant lunch club activity, unlikely to raise any serious critical issues.¹⁷

The Cambridge group on the hand never really received other than very nominal support from its very substantial scientific *élite*, and so was free to develop as a much younger and highly libertarian grouping, working with schoolchildren, beginning on a book on Race and Intelligence, and vigorously defending its local autonomy from the National Committee based in London.¹⁸

Others such as the group at University College (London) sought to study sponsored research on the university campus, but the line which had been so fruitful in the States was much less so in Britain, where the very large amount of defence research that is done is primarily located in government research establishments rather than university laboratories. Indeed, even where the defence research did exist, as in **Porton** contracts held by Southampton University, the student movement was slow to understand their significance. Ignoring the contracts when their details were published by the Society and on the front page of the *Sunday Times*, they were only turned into a political issue when sociology students liberated them from an office during the occupation. Even then the absence of any political consciousness among **Southampton** science students meant that the contracts had to be brought to BSSRS both for interpretation and for help in writing a commentary.

Both the Southampton affair and the U.C. students' strategies raised problems for BSSRS; several of the left academics were aware that the contracts existed; indeed they had helped expose the few that there were. Yet the denial of the expert, an essential strand in the revolutionary students' movement, often made it difficult for the students to locate and utilise the relevant information. It was as if until they had learned it by discovery, actually available knowledge did not exist.

But more than this it pointed to the very limited number of science students who were directly involved in such groups as the Revolutionary Socialist Students Federation. By the winter of 1969 a sufficient group had emerged to convene two meetings of a Federation of Socialist Scientists at Manchester and then Warwick. But although the group managed to publish six issues of *Red Scientist*, a small, primarily theoretical journal, before its demise, and although individual members of FORSS worked within BSSRS, the organisation was left to too few people, primarily based in Manchester, and when the mathematicians L. Hodgkin and M. Thomas ceased to produce the journal it disappeared. The theoretical slant of the journal appeared to be Althusserian. Despite some abortive attempts Red Scientist groups were unable to establish themselves.

BSSRS's attempts to move from the use/abuse issue to the broader one of the neutrality and role of science under capitalism were characterised by two large meetings, one in Cambridge in summer 1970, the second, five months later, in London. The theme of the Cambridge meeting was Race, Class and Intelligence, the immediate purpose being to air and refute Jensen's claims concerning the genetic inferiority of the U.S. Blacks' IQ.¹⁹ The consequence was to develop the first extended attack on bourgeois ideology in science (the mass media widely reported this attack as an attempt to 'suppress the search for truth' in science).¹⁰ Even though the first attack was in social psychology, where ideological forms are more plainly apparent, the next was to tackle a slightly 'harder' science. This was in the winter of 1970, at the meeting on the Social Impact of Modern Biology, where, because of the presence of the Society's {President, the Nobel Laureate M.H.F. Wilkins, on the organising committee, it was possible to command the participation of most of the luminaries of molecular biology. Although almost certainly the huge audience of 800 came and stayed for the three day meeting because of the presence of these stars, the discussions increasingly criticised the Society for its élitism. The American molecular biologist Jon Beckwith's paper, echoing this feeling from the radical wind of SESPA, was received with tremendous enthusiasm by the students and the papers raising issues of ideology in biology reopened the discussion which had been closed since the

Lysenko affair.⁴¹ Within the Society relations between those who wanted to explore the possibility of a socialist science and those who preferred to concentrate on issues of use and abuse became strained. The alliance of left tendencies and liberals which the lack of political clarity in the scientists' movement had hitherto permitted began to break down. Was the Society a broad front, or should it become clearly committed politically?. The federal structure prevented the issue being totally disruptive; each local group could do its own thing, but on the National Committee, still lured by the image of media politics, the issue was unresolved.

The anti-elitist aspect of the Society's development emerged most sharply in the summer of 1970 when, modelled closely on the SESPA demonstration at the Christmas 1969 meeting of the American Association for the Advancement of Science, a group within BSSRS attended the meeting of the virtually moribund British Association. Street theatre and some interventions at lectures brought some relief from the boredom of the B.A. meeting which, while of little significance to scientists, continued to command an inordinate amount of space in newspaper coverage. The journalists, with an almost audible sigh of relief, joined the BSSRS criticisms to some of their own. Few scientists attend B.A. meetings except to give papers, so there was little recruitment of scientists to the Society, but the issue of the neutrality of science received its first public hearing at a six hour Teach-In. In some ways the impact of the meeting was blurred by the proposal from the solidarity group to institute a 'Hippocratic oath' for scientists.⁴² What might have been a good consciousness-raising move a few months earlier now seemed, to quote one physicist, so woolly that any defence scientist could sign it in good faith. Perhaps the irrelevance of the B.A. meeting to the working scientist was plain; for, unlike SESPA, BSSRS has not so far repeated the exercise.

By 1971 the most important issues were seen as those of ideology in science and self-management. A self-management conference was held in 1972,⁴³ but, while the French experience was described, the essential difference was that in France the debate had taken place in the context of the May events, with whole laboratories participating; in Britain they were isolated, typically very young research workers. There was little sign of the research collectives which are emerging in several laboratories in the United States, France and Belgium at the present time.

Meanwhile, as a practical response to the alienation of science from both the scientists and the masses, BSSRS began to discuss the concept of a 'community science'. Community science in this sense would involve the scientist, in his role as expert, working with local groups of activists on particular problems (are the levels of potential pollutants

in the area dangerous, for example?). One attack which could be levelled at this approach is that in such a role the scientist is not actually doing science as such—and often is merely educating himself in the techniques of social medicine. But there is a radical intent behind such proposals. Nonetheless, the very concept of 'community' as opposed to 'working class' science can translate the intent, from a transitional demand which heightens the consciousness both of the scientist and the group he works with, to one which carries all the undertones of political acceptability which have so confused BSSRS's endeavours. Community science may be as easily co-opted as community medicine or community care. The problem remains, for BSSRS, as for the socialist science movement in general, of distinguishing between voluntaristic, individualistic conceptions of the role of science and the scientist under capitalism, and those which recognize its structural situation. Only on the basis of this clear theoretical understanding can mystification be avoided and demands formulated which are more clearly transitional in nature.

5

PERSPECTIVES IN THE ORGANISING OF SCIENTISTS

THE problems in the way of the radicalisation of scientists remain formidable. The scientific training, ostensibly rational, value-free and based on rigorous analysis, remains a subtle mystification. It traps élite scientists into a belief in the democratic and open norms of their profession and at best a liberal concern for personal morality and responsibility (as Oppenheimer put it, the physicists who made the Bomb had 'known sin'). Simultaneously it degrades and dehumanises the rank-and-file scientific worker; he becomes an automaton doing his neutral research at the behest of any paymaster. The dichotomisation is insidious and well-nigh overwhelming, the product of a very deliberate educational programme which continuously confines the science student within narrow blinkers.

Three factors are currently militating against this pattern; there is the public revulsion from many aspects of science with its seeming technological imperative, typified by the environmentalist movement. A second is the recognition by more far sighted capitalists that they need generalists rather than specialists, resulting in the proliferation of—albeit still narrow—courses in 'engineering-with-economics', and so forth. The third is the very real contraction in the employment opportunities for some graduates. Perhaps 70-100,000 scientists and engineers are out of work in the U.S. (as the sign puts it on the road from one of the greatest aero-space cities, 'will the last person out

of Seattle turn out the lights'). The percentage is not so high in Britain, but both in industry and the university Ph.D. factories, the squeeze is on. Even a 1st class degree will not guarantee a place to do a Ph.D. any more. All three of these developments must contribute to the sense of uncertainty and lack of confidence amongst young scientists.

In this situation, the prospects for the development of consciousness amongst scientists are enhanced. At the same time, there is a danger of them simply falling into the personal responsibility trap or voting with their feet by merely opting out of science altogether. The tasks of a socialist scientists' organisation must thus include the provision of a viable alternative analysis of the role of science and a structure within which the scientists can learn and act. Faced with a broad mass to organise, the tendency either to become all things to all men or to crystallize into a purist, theoretically correct sects divorced from contact with the majority of one's ostensible constituency but preaching a revolutionary rhetoric, is almost irresistible; BSSRS has represented one extreme, *Red Scientist* and the Althusserians the other.

To avoid these failures, it is necessary to have an adequate analysis of the contemporary role and situation of science. Our contention is that, at least in Britain, Marxists have failed to develop this analysis; they have either ignored the issue, or attempted to impose a Marxist framework onto faulty models of the nature of science. The scientists have had to stumble, self-taught, through the use/abuse issues until they arrive at the core problems of ideology, alienation and expertise. Although in itself this self-activism can be seen as desirable, and it is surely significant that relatively isolated groups of left scientists, in Europe and the U.S.A., are arriving at a similar agenda and beginning to locate one another, they now need help if their theoretical insights are to be expanded.

Programmatically, the items which must now be on the agenda for action include:

- (1) The systematic exploration of the ideological components of capitalist science. As this field since the Lysenko period has been to all intents and purposes abandoned by orthodox Marxists, the task of recreating this understanding is immense. It is necessary to show both the forms of external constraint on science that capitalism imposes, directing research in the interests of profit or of imperialism, and the mechanism whereby these constraints are internalised by the researcher, determining his very paradigms, experiments and interpretations. And it remains necessary to grapple with at least trying to ask (for we cannot answer) the question: what would a socialist science be like?
- (2) The attempt, within the existing framework, to create a counter-science. The phrase 'science for the people' presently carries more emotional charge than substance. Among its implications are the

arming of the working class with knowledge, the scientific information with which to demystify and defeat the assumption and utilisation of expertise by the ruling class;* and the use by the scientist of his scientific skills directly in conjunction with existing working class, ethnic and minority groups in projects ranging from the development of insurgency technologies to the monitoring of levels of possible pollutants. But in exploring these possibilities we must always be aware that the science and technology which scientists are putting at the service of the people derive from—and are likely to remain—bourgeois culture. Bottomore, recognising this problem, extends Marx's famous aphorism: 'the hand mill gives you a society with the feudal law, the steam mill a society with the industrial capitalist'. He asks 'what kind of mill would give us a classless society?' and suggests that to resolve this it is necessary either to locate the technologies associated with the working class, or alternatively that the working class, unlike other emerging classes, is free to make its revolution without a specific technological force." In the struggle to make science for the people, we may thus expect the science itself to be transformed.

(3) This transformation carries with it the breaking down of the barrier between expert and non-expert; socialist forms of work within the laboratory, making a genuine community in place of the existing degraded myth, must be matched by an opening of the laboratories to the community. The Chinese attempts to obliterate the distinction of expertise, to make every man his own scientist, must remain the aim; laboratories which are integrated into their local communities, and within which the benches and equipment become open for all to develop their projects, would indeed transform the doing of science, help break its stubborn Clitism without diminishing its vital creativity."''

All these developments must be set into a political framework, which recognises both their long-term strategic significance as experiments in what socialist forms of science will be like, and their more immediate role in the raising of consciousness amongst scientists. To fail to appreciate this dual role would obviate their significance.

It is precisely because of the need for this political framework that the innovative developments of the last three years need consolidation within an organised Marxist group. The task can no longer be left to organisations of scientists, operating in a liberal-libertarian mould, or small theoretical groups. Scientists now need to bring their science

*Where are today's Haldanes' with their columns in *Socialist Worker* or *Red Mole*, or the educational agitational pamphlets to counter the bourgeois pollution myths?

**Experiments in both collectivised laboratories and 'open' community laboratories of this sort have been attempted, primarily in the States, but it is too soon yet to know how well they work.

into the area of activity of the Marxist groups, and the groups to accept their responsibilities for political work in science. This collective development is the way forward to a much wider and deeper radicalism amongst scientists than ever before.

NOTES

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4. e.g. *Labo Contestation*, Lyon, 1970.
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28. Brown, *op. cit.*
29. See the various SESPA and related publications, the magazine *Science for the People*, *Science for Vietnam Newsletter*, etc.
30. J. Monod. *Chance and Necessity*. Collins. London 1972.
31. *Labo Contestation*, *op. cit.*
32. Perche Si Occupa il Laboratorio Internazionale di Genetica e Biofisica. Collective of occupants. Naples, 11 May 1969.
33. *Chemical and Biological Warfare. op. cit.*
34. BSSRS Newsheet No. 1. April 1969.
35. H. Rose & R. Stetler. *New Society*. 25th September 1969.
36. S. Rose. *The Himsworth Memorandum*, BSSRS, 1970.
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40. H. J. Eysenck. *Race, Intelligence & Education*. Temple Smith, London 1971.
41. W. Fuller, *op. cit.*
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