Part IV: Presuppositions underlying science

Presuppositions such as those relating to the burden of proof may be called *surface* presuppositions. Typically they are made by individual scientists or groups of scientists about what they expect to find or how they will approach a problem. Surface presuppositions can vary from scientist to scientist or research group to research group. They also can be embedded in isolated scientific techniques or in particular methodological assumptions.

In contrast, what may be called *deep* presuppositions are assumptions built into the nature of science. They exist by virtue of the very doing of scientific research, are reflected in the nature of scientific knowledge, and are manifested by the organisation of the scientific community. If these assumptions were altered, science would be enormously changed.

In the following chapters, I attempt to describe some of the deep presuppositions underlying science. For illustrating my points, I turn wherever possible to SST-NO_x-ozone research and related fields. The way I use to get at deep presuppositions is to look at the answers to the following sort of questions:

Why is certain scientific research done, rather than other research or other activities? (Chapter 9)
Who can use most easily the products of science? (Chapter 10)
What practices and policies does science justify? (Chapter 11)
What is a scientific fact? (Chapter 12)
Who does scientific research? (Chapter 13)
How is scientific research used?
Who pays for scientific research to be done?
Who judges the adequacy and value of scientific research?
How apparent are the presuppositions of science?
What activities does science as presently practised replace or negate?

The answers to such questions are built into current science in a fundamental way. For example, the reasons for doing a particular research project are bound up in current research methods, in methods of communication between scientists, in the rationale for particular types of research organisations, in the division of labour in scientific research and so on. In order for the reasons for doing certain types of research to change fundamentally, many features of science would need to be different. What I call the presuppositions underlying science, then, are the answers to the above sort of questions which are inherent in all aspects of science.

In the following chapters (which can be read in any order) I use some of the above questions as points of initiation for analysing the constitutive features of science. The questions and their answers of course are interlinked, and are used mainly for convenience in splitting up the presentation. Also there are other important areas which I don't treat, such as the relation of sexism and science, and the way that existing scientific techniques — such as regression analysis or chromatography — are both the product and source of values in scientific investigation.

The other part of my argument is that science is organised at present — and it is this organisation which is reflected in the presuppositions underlying it — in a way that makes it selectively useful to certain* powerful groups in society. The particular presuppositions I have chosen to analyse, and my attention to science's selective usefulness to powerful groups, reflect my belief in the value of a different organisation of science and society. Such a different organisation is the subject of part V.

---

*By 'certain' I mean that some powerful groups are benefited by science and other powerful groups aren't. For example, if scientific organisations are set up and scientific paradigms generated partly because of their selective usefulness to the military, then this science will help serve the interests of those groups in society whose objectives overlap with those of the military, but may not serve other groups, such as some manufacturers, whose interests do not overlap with those of the military.

Chapter 9: Why is scientific research done?

What are the reasons behind the doing of any particular bit of scientific research? Why is certain research done instead of other research that might be done?

There are many answers to such questions, ranging from psychological motivations of individual scientists to the existence of particular scientific disciplines and the funding of particular scientific organisations. Here I am going to concentrate on one particular type of influence on the direction and content of scientific research: the interests of influential groups and institutions in society. This is not the only or even necessarily the dominant influence on scientific development. But it is an important influence because it conditions science so as to make it selectively useful in sustaining inequitable political and economic structures and in serving powerful groups in society. Conditioning of one sort or another is inevitable; however, I would prefer science to be conditioned instead to promote more equitable political and economic structures and to serve the needs of the populace as a whole.

General examples

Raymond Williams in *Television: technology and cultural form* describes how in the history of communications technology, there were a number of developments which

promised to be able to serve social needs. Some of these developments coincided with the priorities of real decision-making groups. As a consequence, these particular developments attracted both the necessary investment of resources and the official permission and encouragement which were required to promote rapid advances in certain directions. These advances and their selective technological development and application in turn created new possibilities and new needs.

This sounds very abstract, and even Raymond Williams doesn't give many specifics. A difficulty here is that television technology developed in one way historically (by definition), so that it is very hard to imagine how it *might* have developed in an entirely different social and political order. *If* one could witness history repeated several times with the creation of entirely different social and political orders, then one *might* get an idea of how technology could develop differently.

Let me speculate on some ways in which *telephone* technology *might* have developed. It seems to me that it would have been relatively easy to develop: a simple means for simultaneous connections among 3 or more phones; a device to signal that someone is dialing your number while you're using it; a convenient way of setting phones so they won't ring; simple instructions and easily available parts so that anyone could construct a telephone handset; similar instructions and materials for laying local telephone cables (perhaps along with removable pavement construction) and setting up local exchanges; a network containing places for plugging in phones. Some of these possibilities depend on the existence of different social values or political structures, such as people not feeling the urge to steal or lack of profit-making on all telephone connections. These requirements highlight the dependence of the development and implementation of technology on the structure of society.

The actual development of telephone technology seems to owe a lot to the interests of those who benefit from controlling the operation of a centralised, technocratically run enterprise. Multiperson connections are developed only in the form of highly sophisticated closed circuit TV systems suitable for business meetings; complex switching systems are designed to mesh with office hierarchies; compatibility with bulk data transmissions, as from computer systems, is ensured; and technologies for collection of centralised data on telephone usage and for surveillance are developed. In short, telephone technology reflects the social, economic and political structures in which it arises. So while telephone technology serves social needs (it is useful to many people), at the same time it reflects and reinforces the dominant institutional forces in society.

The influence of social, political and economic factors on scientific development is most obvious on the level of technology, as in the case of communications technology. This is because technological innovations usually have some fairly straightforward practical use. There are several ways that prevailing institutional arrangements may influence the development of technology. Most obviously, the search for new technology may be directly funded, as in the case of ever more destructive weapons or of more profitable or prestigious air transport. Also important is the power to promote some technological innovations and not others: this power effectively serves to define what is worth discovering at a given time. Independent inventors will tend to look for, or to be on the lookout for, mainly those innovations which can and will be successfully promoted.

The influence of society on scientific research is not so direct and obvious as the influence of society on technological research, but it can be just as pervasive. Boris Hessen has analysed the social and economic roots of the work of Isaac Newton. There were many unsolved technical problems in Newton's day which were of immediate and pressing interest. For example, there were problems in navigation, mining, armaments and commerce. The theories of Newton, though seemingly abstract, can be seen as being concerned with many of these problems whose solution was important to the bourgeois class of his era. Naturally Newton's work did not solve all problems whose solution was useful to the bourgeois class: his work necessarily built on previous scientific efforts. Neither was Newton's work available only to the bourgeois class: his work could be used for a number of purposes. But by being concerned with topics pertinent to military and commercial problems and being done within the social, political and economic context of the time, Newton's work in practice primarily served the purposes of the bourgeois class.

Today the most direct way in which economic and political forces influence science is through funding of scientific research by interested bodies. This directly contributes to particular problems being studied and particular types of conclusions being obtained. *A large majority of modern scientific research is funded either by the military, government, or business.* This fact is for many people sufficient to conclude, in the absence of convincing evidence to the contrary, that most modern science is selectively oriented towards the needs of high level military, government and business interests.

Besides direct funding, the long-term direction of scientific development is influenced in several ways by powerful groups in society, through their influence on institutions which provide the climate in which research decisions are made. First, problems may be chosen for study because of their direct or indirect relevance to general or specific problems in society. Second, certain subjects or topics may be emphasised because of their technological importance. Third, the theories adopted by groups of specialists — in particular, the features of the prevailing paradigm — may reflect an orientation that is selectively useful in solving certain problems and ignoring others.

These three sorts of influence can be seen clearly in the case of modern military influences on science. First, particular problems are chosen for abstract scientific study. For example, many abstract problems in control theory arose out of and reflect the practical military problem of avoiding or tracking a guided missile. Second, certain subjects come to have a higher prestige because of their relevance to military problems. An example is nuclear and particle physics. In fact, the status hierarchy of scientific disciplines — such as the prestige order of physics, chemistry, biology and the rest — in part reflects the selective usefulness of the various disciplines for military purposes. Third, the choice of theories developed, or of the paradigms adopted in research areas, may be influenced by their use to the military. The approach of operations research, which had its origins in the study of wartime problems, can be seen as reflecting a problem-solving orientation which is selectively useful to those in a position to exercise economic or military decision-making power.

Let me summarise what I've been saying so far.

In principle, all groups in society may benefit from the particular research that is done, since scientific knowledge is 'free' to the public. In practice, science often tends to develop in a way that is selectively useful to those groups which are powerful, prestigious and wealthy. The scientifically important problems in any era are usually defined as important by those groups in control of money for research. Even if a scientific discovery has a number of potential applications, only selected applications may attain the backing and support that is necessary for practical use. The selective application of scientific results then leads to a lopsided development in science itself. However, the question is not one of whether science serves *only* dominant groups. Certainly there are scientific discoveries, theories and applications which benefit all groups or even specifically the disadvantaged groups in society. Rather, the question is one of the main emphases in scientific research and of the dominant forces influencing and using science.

SST-NO_x-ozone and related examples

Johnston's work can be seen as being partly motivated and shaped by social, economic and political developments in

society. Directly influential was the imminent introduction of the U.S. SST. Suppose the SST had not been proposed at the time that it was. Then it is likely that Johnston's analysis would have been oriented differently: probably he would not have been so concerned about the effect of NO_x introduced into the stratosphere by humans. No doubt his conclusions would have been less spectacular, the work sent to a different journal* and read by a smaller and more specialist audience, and so forth. Finally, without the imminent introduction of SSTs it is unlikely that Johnston would have studied the effect of NO_x on ozone at all.

More generally, the orientation of scientific research in the study of the upper atmosphere has been strongly influenced by the particular pattern of economic and technological development. The research has also been influenced by the political systems in the U.S. and the U.K. which allowed opposition to SSTs on environmental grounds to play a part in decision-making.

This example depends on the assumption that the way technology is developed and introduced in society is not inevitable. I can imagine that society might just as well have developed in a way such that the question of the rapid introduction of the SST or of its environmental impact never became important. This assumption seems supported by analogous historical examples and by the apparent neglect of upper atmospheric effects in the U.S.S.R.'s introduction of its Tupolev-144 SST.

Incidentally, the work of Johnston and of Goldsmith et al. illustrates how economic and political influences on the development of scientific research cut across the conventional distinction between 'pure' and 'applied' science. The study of stratospheric chemistry is part of the study of the natural world and hence is 'pure' science, in the same way that astrophysics and molecular biology are part of 'pure' science. But the contributions of Johnston and of Goldsmith et al. towards knowledge of nature are intimately linked with social concerns over human impact on the environment and over technological development. The links between 'pure' science and political, social and economic factors are generally less direct and immediate than in the case of 'applied' science, but just the same they can be pervasive and important.

Somewhat broader than Johnston's and Goldsmith et al.'s work is the problem of explaining stratospheric ozone levels on the basis of photochemical reactions. The following are the earliest dates at which certain compounds and related reactions were seriously proposed as important in determining stratospheric ozone levels:

1930: oxygen compounds and reactions
1966: hydrogen-based compounds and reactions
1970: nitrogen-based compounds and reactions
1974: chlorine-based compounds and reactions.

Increasingly since the 1960's, concern about effects on the environment has been important in directing investigation into the effects of these compounds. Around 1971 the pace of investigation was dramatically accelerated. Major research programmes were initiated or augmented in a number of countries, especially the U.S. Thus the direction and intensity of scientific effort has been strongly affected by the economic and political pressures for and against the introduction of the SST.

It is true that in the case of each new set of compounds that was seriously studied, the concern over the environmental effects of the compounds occurred *after* the mechanisms for affecting ozone had been postulated (for hydrogen, about 1970; nitrogen, 1971; chlorine, 1974). But this does not mean that this research was not affected by environmental issues. Since the concern over pollution of the upper atmosphere has increased, there has been an increased sensitivity by scientists to the *possibility* that certain minor constituents in the stratosphere may play a major role in determining ozone levels. Certainly there is a good case for arguing that awareness of the potential importance of chlorine in upper atmospheric chemistry was stimulated by awareness of other upper atmospheric perturbations, as well as awareness of environmental issues in general. So although extra-scientific concerns have not determined scientific theories, they have certainly influenced the choice of theories to be given serious attention.

In selected areas the importance of societal influences is even more apparent. One such area concerns explaining why mean ozone levels over the globe increased (apparently) during the 1960's. Before the widespread concern over the environment, there were a number of proposed explanations for such increases, such as sunspot activity and changes in the temperature of the atmosphere. At least one later explanation is a fairly direct result of environmental concerns: that the increase in ozone is in part a recovery from a reduction in ozone caused by injections of NO_x from nuclear blasts.

The influence of social, political and economic structures on scientific development is also apparent in research fields broader than SST-NO_x-ozone.

In air pollution meteorology, the aim is to understand the physical processes which on a local scale (on a scale of kilometres — say in an urban area) determine the amount and extent of air pollution. For example, air movements and chemical reactions leading to photochemical smog in the Los Angeles area might be studied. On a surface level this work may seem worthwhile. But in the context of how it is done, who funds it and has access to research results and what it is useable and used for, most air pollution meteorology research implicitly supports the present structures of society. For example, the research is done by scientists, who are considered to be 'experts'. They report their results to commissions or other high level decision-making bodies. So the research helps reinforce hierarchy and dependence on expertise. The results of research in air pollution meteorology typically are used to decide where industries may be sited in order to minimise the environmental impact, or to decide what level of automobile exhausts may be tolerated, and so forth — all in the context of the existing social structure. This research does not encourage public discussions about science and about social and political issues involved in air pollution. It does not involve people in thinking and decision-making about the influence of technology on their lives, for example to decide whether they want or need a factory or an enlarged private transport system at all. And it does not encourage attempts to restructure society so that costs and benefits of technical change are equitably distributed, and so that the material wealth and life activities of society are redistributed. Generally, air pollution meteorology research is done under the assumption that society will remain the way it is: that powerful elites will make most of the decisions, that technical change will be to the benefit of the privileged, and specifically that roads and automobiles and polluting industries are unchangeable facts of life. By accepting this context, working within it and solving problems arising within it, air pollution research is implicitly biased.

Research that appears highly specialised and narrow almost inevitably is built upon and reflects the assumptions and biases of the wider context in which it is done.For example, one highly specialised research area related to meteorology involves determining on a computer how gases move and mix under certain conditions. Knowledge gained from such studies can be directly used in air pollution modelling studies, among other applications. (A closely related area, the study of neutron diffusion, is important for various problems in nuclear reactors.) The fact that the specialised research may *seem* to have no practical applications is no indication that it does not.

*An apt comparison is with Crutzen's 1970 paper in the *Quarterly journal of the Royal Meteorological Society*, also about the importance of NO_x in determining stratospheric ozone levels. Crutzen's work was done before it was realised *SST*-NO_x was important. The main sentence from the abstract to his paper suggests the difference in motivations for the work: "The probable importance of NO and NO_2 in controlling the ozone concentrations and production rates in the stratosphere is pointed out."

A second important problem area in meteorology is weather prediction and control. Indeed, air pollution meteorology can be considered to be one aspect of this general problem. Once again, this work may seem worthwhile on the surface. But in the context of how it is done, who funds it and has access to research results, and what it is useable and used for, research on weather prediction largely serves to support the existing power structure.

There is relatively little research on weather that is tailored to the needs of small-scale farmers, for example, who might find it useful to have local rules for estimating when freezing will occur or to conveniently determine distributions of sun and wind for use in constructing solar or wind energy collectors. Most research in weather prediction and control is motivated by possible military applications or is oriented towards large-scale studies and interventions that help groups such as airlines and mass production farming operations more than they help small farmers. Furthermore, the research is done by specialists and communicated hierarchically, and is not used to increase the understanding of the people who are affected by its results.

It is important to remain aware of the social and political context of research. The context of weather research is one in which a small percentage of farmers and marketers make a large share of the profits; in which surplus production coexists with malnourished and starving people, both in industrialised and non-industrialised regions of the world; in which the primary reason why centralised monocultures, pesticides and food additives are a normal part of the production of food is not consideration of the needs of the people, but the making of profits, the controlling of markets and the transfer of control over decisions from individuals to companies, bureaucracies and experts. Research on weather prediction and control works within this state of affairs and hence to some degree supports it.

Besides looking at what meteorological research is done and who benefits from it, there is another way of inferring the orientation of the research: by looking at who supports it financially. It is revealing that in the U.S., the Department of Defense is the largest employer of scientists who study the weather and how it may be modified. Of course, weather research can be used by or be useful to people other than the rich and powerful. But even if the research is somewhat useful to such people as small farmers, at the same time it can be much *more* useful to governments using it for military purposes.

Science out of control?

Many people have noted that the practice and use of science is contributing to what they consider highly undesirable developments, such as degradation of the environment, regimentation of the population and loss of meaningful human activities. One interpretation of these developments is that science and technology are in some sense out of control. For example, it is thought that decisions are made on the basis of technological feasibility rather than on the basis of human needs. In many ways I sympathise with this interpretation. But it seems inadequate to me. The idea of science out of control is based on the idea that science develops independently of the society in which it is done. It makes more sense to me to think of science as being under control — under the control of powerful groups in society. Of course, this is not a matter of conscious control on the part of scheming individuals, but only actions which are 'natural' in the world view of those who make them. These powerful groups promote, out of all possible scenarios for the development of science, only those which benefit them and maintain their position of privilege and power. Of course there is considerable opposition (conscious and unconscious) to these efforts by vested interests, as well as conflicts between different vested interests themselves. It perhaps is not surprising that the result is a world in which science seems out of control. For in selectively propping up illegitimate privilege, science seems to deny its promise as a liberating force. It is also not surprising that the idea that science is out of control is so prevalent, since it is precisely the sort of idea that hides the existence of the influence of vested interests on the direction of scientific development, the existence of an influence that serves the interests of those who wield it.

The scientist as mediator

The influence of society on science is not direct, but must be translated from one realm into another. The societal realm includes such things as the setting up of research institutes, the shaping of organisational relationships, exploiting particular scientific developments, and the use of particular ideas in explaining and justifying the social and political world. The scientific realm on the other hand includes such things as scientific concepts and methods and philosophies and the selection of research topics and goals. These two realms interact with each other through intermediaries such as education, media images of science and the personalities of individual scientists. Here I will focus mainly on the scientist as mediator of societal influences on science.

To efficiently translate societal influences into the scientific realm, it is entirely unnecessary for the individual scientist to be aware of any such influences. Indeed, even when actively trying to avoid reproducing the assumptions underlying the operation of society in the scientific realm, it is nearly impossible to avoid doing this. This is because existing scientific concepts, tools and techniques are products reflecting past influences, and because the assumptions underlying the operation of society are reflected in education, technology, communications and personal relationships, and so are hard to escape. So even if there were no particular manifest pressures on scientists to do science in a certain way, scientists would still tend to incorporate in their work the assumptions underlying current arrangements in society.

But of course there *are* many manifest pressures on scientists to do science in particular ways. Almost all of these pressures help to make it easier for scientists to faithfully translate the interests of dominant groups in society into the scientific realm. Here I will briefly survey some of these ways: the education of the scientist, organisational influences, the social system of science, professional self-interest and specialisation.

Education of the scientist. Scientific education helps in a number of ways — prolonged training, use of textbooks, and the structure of educational knowledge — to produce scientists who are strongly committed to the values which are built into the scientific enterprise.

To become a scientist, a person must undergo prolonged training. During this time prospective scientists tend to accept the values, standards and assumptions of science as presented by and, more importantly, as implicit in the actions and attitudes of their teachers. This internalisation of current attitudes is especially strong in the first stage of scientific training which usually continues through the later undergraduate years, when the emphasis is on conformity to externally imposed constraints (syllabuses, exams) and on acceptance of scientific authority. During this stage the student learns established 'facts' and skills by continued repetition in a rigidly defined and administered sequence of courses. Only the slightest encouragement is given for developing original ideas. By the time the second stage is reached when freedom is allowed and research begun, most nonconformers have dropped out or adjusted: research is undertaken within a narrow conception of what sort of science is possible. Originality now operates within strong internalised constraints as to what is possible and proper in scientific research and in the relationship of science to society.

Science students are helped to accept current ideas by their relative isolation from unconventional views about science. This isolation results partly from a heavy work-load, from peer pressures and from lack of public understanding of science.

Science students learn to a large extent from textbooks. Science textbooks present an unusually distorted picture of scientific practice, portraying scientific development as lacking strife, as being uniformly accumulative and as being independent of political and economic developments. Furthermore, in solving textbook-type problems there is little emphasis or awareness of the possibility of thinking creatively outside the context of the problems. The student is told what problems to solve and therefore implicitly told what problems are worth solving. The student is told what methods to use to solve the problems and therefore implicitly told what methods are acceptable. Ostensibly only a technique is to be learned; but inherent in the technique and the way in which it is taught are numerous assumptions, which are inevitably learned as well, usually in a much more thorough and lasting way than the specific techniques or problems. The same comments apply to laboratory exercises.

Basil Bernstein argues that the way educational knowledge is selected, classified, distributed and evaluated reflects the distribution of power in society and the principles of social control. The curriculum establishes what is valid knowledge, the method of teaching establishes what is valid transmission of knowledge and the examining of students establishes what is a valid understanding of knowledge. Thus through the medium of education, the structure of experience is shaped and regulated, generally in a way that supports the existing structure of society. (This is analogous to the way that the physical world and human perceptions of this world are shaped through the medium of science and technology, generally in a way that supports the existing structure of society.)

Bernstein discusses what he calls the *classification* and *framing* of educational knowledge. Classification refers to the strength of boundaries between subjects: for example between physics and chemistry. Generally these boundaries are strong. The typical scientist gains an educational identity through specialisation, becoming say a physicist or a chemist. The scientist's identity and status is based on identification with the subject, or perhaps with a more specialised research area. Changing subjects usually requires extensive re-socialisation. Framing refers to the strength of boundaries between what may and what may not be taught formally. From what is and what is not taught, the trainee scientist learns what questions may be asked at a certain time. For example, questioning of established scientific 'truths' is not normally permitted until socialisation is well advanced.

This perspective suggests that the way scientific knowledge is organised and presented to prospective scientists is a major factor in shaping the scientist's perception of the world and of the relation of scientific knowledge to the world. The implication is that if scientific knowledge were classified and framed differently, then the consciousness of scientists might be quite different. For example, the intense specialisation in science which makes science selectively useful to powerful social groups, or the apparent dichotomy between fact and value, might not survive different curricula and methods of teaching and evaluation of students.

In early research work, the scientist usually does things very similar to work done by others in the field. This is particularly so in the case of research for an advanced degree, which generally is chosen to be conservative and unexciting so as to give promise of safe, acceptable (marginally original) results within a reasonable length of time. By doing research only slightly different from past work that is acceptable and conventional, the scientist further absorbs the values that underlay the previous work.

What about those entering science through other than the normal channels? There are not many who do this. It is virtually impossible to become recognised as a scientist if one does not follow the standard paths. To get into a good university one must do well in high school. To get into a good graduate department one must do well in university science. To get a degree and a position afterwards one must satisfy one's supervisor and colleagues. Throughout this time one is subjected to peer (student) and teacher (supervisory) pressure to conform, as well as the more direct pressures of committing oneself to assimilating rigidly specified course material. Without this sort of background, one cannot get a position at a major university or research installation, which is the virtual prerequisite to having the time, money and facilities to do good (acceptable) research, and to have it accepted by the scientific community.

Organisational influences. Most scientists work in government or industrial scientific organisations, and even university scientists are strongly influenced in their work by the availability of funds or future jobs from government, industry or foundations. The organisational and funding context of scientific research acts as a pressure on scientists to do certain types of scientific research and to reach certain types of conclusions.

Of course, scientists are not normally *forced* to do certain research. Typically there may be a selection of topics from which they might choose, the range of the selection depending upon the particular organisation and scientist involved. Two things act as a pressure. First, the selection of topics for research available to the scientist is limited, and usually biased towards certain areas. Second, there are much greater financial, social and psychological rewards for pursuing certain paths.

The existence and effect of organisational pressures could be expanded upon at great length, but since it is a fairly straightforward phenomenon, here I only give an illustration. In table B the organisational and funding context of some SST-NO_x-ozone research is listed. (Since university scientists publish in the open literature much more than government or industrial scientists, this tabulation gives an exaggerated idea of the fraction of university scientists.)

Table B: Employing organisations for several scientists, and the source of funding for their research in the area of SST-NO_x-ozone.

Author and paper	Primary employing organisation as reported in paper	Support or funding of work as reported in paper (if distinguished from employing organisation)
Crutzen (1970)	Clarendon Laboratory, Oxford University (European Space Research Organisation post-doctoral fellow on leave from University of Stockholm)	
Johnston (1971)	Department of Chemistry, University of California, Berkeley	U.S. Atomic Energy Commission through the Inorganic Materials Research Division, Lawrence Livermore Laboratory, University of California, Berkeley
Foley and Ruderman (1973)	Department of Physics, Columbia University	Institute for Defense Analyses
Goldsmith et al. (1973)	Meteorological Office, Bracknell	
Hesstvedt (1974)	Institute of Geophysics, University of Oslo	U.S. National Aeronautics and Space Administration under contract NGR 52-155-002
McElroy et al. (1974)	Center for Earth and Planetary Physics, Harvard University	Atmospheric Sciences Division of the National Science Foundation under Grant GA33990X to Harvard University
Alyea et al. (1975)	Department of Meteorology, Massachusetts Institute of Technology	U.S. Department of Transportation through CIAP, under Contract AT 11-1-2249; computer time provided by Goddard Institute of Space Studies through Grant NGR 22-009-727 from the National Aeronautics and Space Administration

Table C: Papers emphasising the environmental safety of SST exhausts in the stratosphere. Employing organisations of the authors are listed.

Anderson (1973)	Lockheed Palo Alto Research Laboratory (U.S.A.)
Ashby[1], Shimazaki[2], and Weinman[3] (1972)	[1]Department of Meteorology, University of Wisconson (U.S.A.) [2]Captain, U.S. Air Force, Air Force Institute of Technology [3]Aeronomy Department, Environmental Research Laboratories, Boulder, Colorado (U.S.A.)
Foley and Ruderman (1973)	Department of Physics, Columbia University (U.S.A.) (research done at the Institute for Defense Analyses)
Goldsmith et al. (1973)	Meteorological Office, Bracknell (U.K.)
Jocelyn et al. (1973)	British Aircraft Corporation Limited (U.K.)
Machta (1971)	Air Resources Laboratories, National Oceanic and Atmospheric Administration (U.S.A.)
Swihart (1971)	The Boeing Company (U.S.A.)

Table D: Papers emphasising the environmental dangers of SST exhausts in the stratosphere. Employing organisations of the authors are listed.

Alyea et al. (1975)	Department of Meteorology, Massachusetts Institute of Technology (U.S.A.)
Crutzen (1972)	Department of Meteorology, University of Stockholm (Sweden)
Hesstvedt (1974)	University of Oslo (Norway)
Johnston (1971)	Department of Chemistry, University of California, Berkeley (U.S.A.)
McElroy et al. (1974)	Center for Earth and Planetary Physics, Harvard University (U.S.A.)

Tables C and D list papers emphasising respectively the safety and danger of SST exhausts in the stratosphere. It is not surprising that only university scientists emphasise the dangers. Naturally the sample here is biased; but there appear to be no studies by scientists working for aircraft companies which emphasise mainly the dangers of SSTs, and tables C and D merely serve to illustrate this point.

The social system of science. After a long period of training and socialisation, a scientist begins to practise science as a part of the scientific community. The scientist practises science in a social system — a set of relationships between people and an established set of practices and patterns of behaviour.

There are several features of the social system of science that serve to reinforce the normal operation of science. One is peer pressures. Scientists look to their immediate colleagues and to other researchers around the world in their speciality for acceptance or rejection of the research they have produced. Often the scientist's very assessment of self becomes identified with evaluation of the scientist's research by peers.

Evaluation by peers most simply takes the form of interest in work performed and a higher or lower status in the eyes of colleagues on the basis of research done. There are also several forms of institutionalised peer influence. The response of editors and the editor's referees to a scientist's manuscript is one of the basic institutionalised forms of peer influence. Scientists often measure their success in research according to research published. Access to jobs, to research facilities and to research grants is controlled by various members of the scientific community, usually the more senior and leading members. Scientists often measure their success in life according to their success in gaining a prestigious job, having influence or control over the work of other scientists or technicians, and according to the size and status of their research group.

Finally, scientists may be strongly influenced by the promise of certain honours. Typically such an honour is either the granting of membership in an elite professional group, such as the Royal Society, or the awarding of a special prize for excellence in scientific work, such as the Nobel Prize. Many scientists, especially leading scientists, measure their success in science and in life by their achievement of such honours.

Existing mechanisms for communication between scientists, for recognition of achievement and for access to facilities and to media of communication can all be seen as mechanisms that ensure the efficient production and distribution of scientific knowledge so that it is useful to powerful groups in society, and that ensure the continued operation and expansion of the scientific establishment in changing social and political circumstances. For example, the close interactions between scientists ensure that scientists derive their values from what research is being done and funded. The social system prevents the personal values of scientists being derived from direct involvement with members of the public.

Evaluation by peers serves to inhibit introduction of political consciousness into scientific work and in general to filter out nonconformist research and individuals. Professional bodies operate to sanction research work done within orthodox channels. The reward system of science encourages scientists to keep their entire attention directed towards matters of direct scientific interest.

In these and other ways, then, the social system of science encourages conformity to accepted practices in scientific research. Each of these ways is a form of social control. In each case it is not hard to show that this social control tends to reinforce the established features of science, to discourage and penalise nonconformity and to thwart challenges to the stability of the scientific community.

The professional self-interest of the scientific community. Scientists can be expected to look after their own interests. Similarly the institutional structure they have developed for their enterprise can be expected to reflect and promote this self-interest. For example, people with inconvenient scientific or political inclinations may be ignored or suppressed, albeit inconspicuously; faithful workers may be given jobs, awards or perks; organisations funding and promoting scientific research may be rewarded with scientific advice or scientific attention to their problems.

One of the ways in which the self-interest of the scientific community is expressed is through the ever-present suggestions for and promotion of further research. It can hardly be expected that in every case further scientific information is needed for the purpose at hand. The tendency to suggest the need for further research can be seen to be based on the scientific community's vested interest in expanding the domain of research. The importance of this point should not be underestimated.

For example, both the Australian Academy of Science report and the U.S. Department of Transportation's Climatic Impact Assessment Program (CIAP) report contained recommendations for further research. Of 8 specific recommendations listed in the Australian Academy of Science report, 6 have to do with the need for research. For example, "active encouragement should be given to the development in Australia of more expertise in atmospheric, including stratospheric, chemistry". In other words, the Australian scientific community, via some of its leading spokespersons, wanted more money from the government. Also noteworthy is the CIAP report's finding that the cost of unregulated flying of SSTs in the stratosphere was likely to be greater than the cost of regulation *and* associated research necessary (such as monitoring of ozone levels and further development of ozone modelling). It would have been interesting if the cost of research had been found to tip the decisional balance. Not surprisingly, this is seldom if ever found to occur. The primacy of the scientific organisation's need to survive, to receive continued support, is also suggested by the difficulty of thinking of cases in which scientists have counselled that further research and investigation would be wasteful and irrelevant.

A more dramatic example of the effect of institutional and professional vested interests is connected with the decision to drop atomic bombs on Japanese cities before the end of World War II. One contributing factor to this decision was awareness of the benefits which would accrue to scientific leaders and the scientific profession from a successful and decisive demon-

stration of the power and importance of the new weapon. Such a demonstration would justify the immense expense of the Manhatten project, vindicate the judgement of those who backed it, prevent congressional investigation into the project, lead to greater influence of scientists in government and fortify the newly cemented partnership among science, government, military and industry. This was not the only factor in the decision. Scientific leaders had as well some other, more admirable reasons for supporting the use of atomic weapons. What is important is the influence of the self-interest of scientists and other elites — military and governmental — on the direction and pace of scientific research (the hurried development of the atomic bomb) and also on wider questions affecting both science and society.

Specialisation. Scientific research is a very specialised activity. This has a number of consequences.

First, specialisation tends to reduce the individual scientist's contact with wider political issues. Scientists are encouraged to produce high quality results as quickly as possible. The assumption is that high quality results will automatically require decision-makers to make wise decisions. Because of strong research competition, just doing one's specialised job is often a full time job with little or no time for worrying about wider issues. Given the segmentation of research, the path of least resistance is to just do one's specialised job and ignore any indications of the context of the work.

Second, specialisation makes scientists and their work easier to manipulate by dominating groups. In advanced industrial society, the division of labour is one of the important means by which ruling groups maintain and reinforce the hierarchical structure of society. At the level of scientific research, the division of labour takes the form of specialisation. It is difficult for a worker of an assembly line to argue about the utilisation of labour and the type of product produced. Similarly, a specialised research worker is not in a strong position to argue about what sort of research is funded, nor about how research results are used.

Just as importantly, scientists and technologists are separated from working people through physical dispersal, task specialisation and separate administration. Therefore the organisation and co-ordination of scientific-technical elites and working people is inhibited. The structure of the work situation encourages each group to look after its own living conditions and other interests. And naturally this sort of concern lies within the context of society as it exists.

Finally, specialisation makes the results of science hard to understand by the non-expert, including scientists in other research specialities. Therefore: (1) inadequacies or errors in the work are harder to recognise; (2) it is harder for outsiders to step in and make a contribution to the research in the area; and (3) the need for the research is harder to dispute.

Beyond this, lack of understanding by the non-expert serves to make the values implicit in scientific research hard to identify by outsiders. Exposed to a barrage of specialised scientific knowledge, a person tends to accept the structure and world view inherent in the selection and organisation of the knowledge. This structure and world view is based on the assumptions involved in the selection of only certain problems, methods and solutions as being worthwhile. In order to grasp the significance of a mass of specialised scientific work, it is easiest to accept the underlying assumptions which led to this work being done.

Reference notes

Williams (1974), which I referred to in the text, is an excellent case study. It is especially rewarding since it is about a bit of technology familiar to everyone. Further information on telephone technology is found in Boyle and Harper (1976). Dubos (1969) gives examples from microbiology of how a direction is given by social forces (such as power politics and economic necessities) to the interests of scientists and therefore to their research activities.

A more comprehensive analysis of the effect of political and economic structures on technological development is Dickson (1974). Dickson argues that the nature of technology is strongly influenced by the distribution of power and the exercise of social control in the society in which the technology is developed and used. For example, technological innovation is partly determined by the requirements of maintaining authoritarian discipline, hierarchical regimentation, and in general the maintenance of the current economic structure and the survival of the dominant class. Furthermore, the myth of the neutrality of technology serves to justify this state of affairs and to hide the political dimension of technology. Dickson's book covers these and other fundamental issues concerning technology.

Braverman (1974) gives a large number of detailed examples of how technologies are developed to serve the interests of management, most particularly in the way in which the promotion of the manufacturing division of labour, which reduces workers' skills and allows for tighter control from the top, is a basic motivation for the design of machinery and of production systems. Braverman also presents an excellent analysis of 'scientific management' and its effects. The origins of the factory system during the industrial revolution is traced by Marglin (1974) to the capitalist's control of the work process, rather than the factory's technological superiority.

Another excellent treatment of the interaction of technology and society, and particularly of the view that technology serves as an instrument of social control, is given by Elliott and Elliott (1976). A further book presenting many of the different views of technology is Thrall and Starr (1972). See particularly the presentation of Melman (1972), who strongly argues that technology reflects the social structure: the preferences of the dominant people in society are embodied in the selection and design of technology.

The most influential early work on the social and economic determinants of science is contained in Bukharin et al. (1931), in particular the article by Hessen (1931). These studies are from an avowedly Marxist point of view, in which the development of science is seen as almost exclusively a function of the economic organisation of society. The result is that the authors often go too far in ascribing scientific development to social influences. But for someone otherwise exposed only to conventional histories of science, this approach is a refreshing antidote. For this reason alone these studies are worth reading. Furthermore, they offer a useful basis for more refined studies. A more restrained, but similar sort of analysis to that of Hessen's is given by Merton (1938, chapters 6-10).

In the sociology of knowledge, it is taken as a matter of course that the form and content of knowledge is affected by the social structure. In two thought-provoking studies, Barnes (1974, 1977) applies the sociology of knowledge to science. He notes, for example, that even if the choice between theories is rational, the theories that are considered may be partly determined by social factors. In a detailed analysis of 'internal' and 'external' factors in the history of science, Barnes suggests that it is useful to regard ideas as tools which social groups may use for their own purposes; the social structure may influence ideas and science, but not in any direct and explicit way.

A number of studies show how ideas, images, or moods — the intellectual milieu — can influence scientific development. Forman (1971) gives a detailed account of how the anti-rational mood of Germany after World War I helped the development and ready acceptance of a quantum theory interpreted as indeterministic. A most significant part of this account is the description of how so many leading scientists found it easy to renounce the ideas underlying their enterprise, when these ideas became unfashionable in the surrounding culture. In another significant study of the same era, Tobey (1971) describes how Robert Millikan and other leading scientists in the U.S. adopted an ideology of progress based on science. This ideology was

closely tied up with an attempt to sell and justify science to the public; it was also of overwhelming importance in providing working hypotheses, in suggesting lines of research, and in designating what kinds of deductions should be drawn from limited data. Miller (1972) describes how the ideas for certain theories in physiology were taken from an evolutionary model of mob society. To complete the dialectic, these theories were later used to justify the organisation of society from which they were originally abstracted. At the psychological level, Cohen (1972) describes how Francis Galton's most important discoveries in statistics were motivated by his political beliefs, in particular those related to eugenics. In a similar vein, MacKenzie and Barnes (1975) find that positions taken by protagonists in the scientific debate between biometricians and Mendelians reflected their social origins and political beliefs.

The study of the interplay among societal structures, prevalent ideas and scientific theories has been carried out with greatest sophistication in the area of evolutionary biology. Young (1969, 1970, 1971, 1971a, 1973, 1973a) analyses the interactions between the various philosophical, theological, political and scientific ideas of the 1800's, such as the influence of the ideas of Malthus on Darwin and Wallace. Young argues that these interactions are seldom simple and straightforward and emphasises the importance of intervening factors. He hopes to show that there is no cutoff between ideas in science and in the wider society, and thus to transcend the 'internal'-'external' dichotomy in the study of the history of science. I strongly recommend Young's studies (especially 1971, 1973, 1973a).

Farrington (1939), in a study of the reasons for the failure of ancient science, finds that the internal explanations are inadequate. He presents two reasons for the decline of Ionian science. First, the religion of the state was transformed by the ruling class into an instrument of mental oppression, used to justify extremes of inequality and injustice; this promotion of ignorance in the support of oligarchy was incompatible with free inquiry. Second, the foundation of ancient society on slavery institutionalised a divorce between theory and practice, and led to the development only of speculation and abstract science and a failure of concrete applications which would form and direct theory. Farrington comments that the problem for modern oligarchies is to combine political ignorance with technical efficiency — a comment made before the recent 'solution' of specialised, professionalised, and bureaucratised science.

It is generally accepted that until the mid-1800's, science usually had a negligible influence on technology, while technology often had a strong influence on science. (For a short readable account, see Ziman (1976).) For example, Zilsel (1941) finds that Gilbert's experimental method was heavily indebted to the work of miners, foundry-workers, navigators, and instrumentmakers. Modern science and technology are closely interwoven, and distinguishing between them is difficult. So, science as well as technology is strongly conditioned by economic forces. The relationship between science, technology, invention and economics is analysed by Schmookler (1966), Mansfield (1968), Jewkes et al. (1969) and Langrish et al. (1972). Mason (1956) in a history of science emphasises more than most authors technological and other societal influences on scientific development.

The links between the structure of society and the development of science are not direct. The way some of these connections operate is presented, with detailed illustrations from the area of nutrition and food technology, by Hall (1974). First, scientists working for companies may be insensitive to the direction of research — a direction taken under the imperatives of profit and efficiency — because they are immersed in their work. Second, academics may conservatively adhere to the prevailing paradigm, a paradigm that arose and persisted because of its usefulness to industry. And third, scientists tend to develop theories that will gain public approval.

The dates I have given concerning explanations for stratospheric ozone levels are associated with the following papers:

base for compounds and reactions considered	(thought to be) important in determining ozone levels	(thought to be) of environmental importance
oxygen	Chapman (1930)	
hydrogen	Hunt (1966)	Harrison (1970)
nitrogen	Crutzen (1970)	Crutzen (1971), Johnston (1971)
chlorine	Stolarski and Cicerone (1974) (and others)	Molina and Rowland (1974) (and others)

This chronology of the explanation of stratospheric ozone levels on the basis of photochemical reactions is based mainly on my reading of scientific papers in the area. Most papers begin with a brief review of previous work in the area. (The most comprehensive review I have come across is Nicolet, 1975.) One warning: at the level of scientists reviewing previous work, there is a tendency to interpret past scientific work in terms of present ideas. Scientific history also depends on who is interpreting it. For example, it may be argued that serious consideration of the importance of hydrogen compounds and reactions for stratospheric ozone began with Bates and Nicolet (1950) rather than Hunt (1966). Different perspectives on this point can be obtained by reading Nicolet (1975) and Hunt (1969).

The explanation of ozone increases during the 1960's as a result of recovery from nuclear blasts is presented by Johnston et al. (1973).

The impact of military, government and industrial funding of scientific research on the direction of scientific advance is analysed by many authors, such as Rose and Rose (1969), Melman (1970), Clarke (1971), Hightower (1973), Salomon (1973), Blume (1974), Ziman (1976) and Noble (1977).

Salomon (pp. 58-59) comments on the way that problems which depend the most on science and technology and which have military aspects — such as meteorology and the mastery of climate — are allotted the greatest resources. The importance of military influence in meteorological research, and the use of weather modification as a weapon, is discussed in some detail by Looney (1975). Rose (1972) describes how scientific and technological innovations in chemical and biological warfare are not a result of 'pure' science, but are intimately linked with the tactical and strategic needs of the military-industrial complex.

Slack (1972) outlines how the importance of different theoretical approaches in organic chemistry is strongly influenced by the values of the chemical industry. For example, he points out that the strong emphasis on the study of substances produced by fast, high-yield reactions arises because such substances are useful (i.e., profitable) according to social and political (rather than 'purely scientific') criteria. Rothman (1969) documents a similar sort of influence in the field of economic entomology.

Rahman (1972, p. 17) comments on how scientific research in India — in the areas of natural resources, agriculture and tropical diseases — was directed "to suit the economic and political requirements of the occupying power".

Clarke (1971), in a survey of the frighteningly extensive and pervasive activities of the military, gives many examples of how projects which are apparently 'pure science' are actually motivated by military problems. For example, it is easier to obtain research funds to study the noises that fish make than to learn how to catch them. This is because the motivation is detecting enemy submarines, not feeding people. Clarke also comments on how the influence of the military in research leads to a lop-sided development of scientific knowledge.

Bernal (1939, chapter VII; 1969, volume 3, 10-10) describes in detail the impact of war on science; his historical analysis shows that the military influence on the direction of scientific research is nothing new. See also Ziman (1976). The most dramatic statement of the impact of war on science ("War is

the principal motivational force for the development of science at every level, from the abstractly conceptual to the narrowly technological.") is given in Lewin (1967, p. 85).

The idea that science is out of control is dramatically and convincingly portrayed by Ellul (1965) in an acute account of the actual consequences of the increasing use of efficient techniques as the sole criterion for social organisation.

The inevitable assimilation of societal assumptions by scientists, and their expression in scientific work, is lucidly stated by Caudwell (1939, pp. 24-25). The lack of any need for consciousness on the part of scientists in doing this is spelled out by Bukharin (1931, p. 20).

For an excellent but abstract description of socialisation, see Berger and Luckmann (1966). They argue that reality is a social construct, hence socialisation is the internalisation of reality. Therefore scientific education is one means of *producing* reality.

For a description of the education of scientists, see Hagstrom (1965). The distortions of science textbooks and of science teaching are discussed by Kuhn (1963, 1970), Feyerabend (1975), Haberer (1969), Lévy-Leblond (1976) and Gorz (1976). Bernstein's (1971) analysis of educational knowledge is not about scientific knowledge in particular, but his insights are easily applied to this area. Schefflen and Schefflen (1972) give a down-to-earth account of the way behaviour can be shaped and controlled through the structure of communication systems. Good critiques of schooling are Illich (1971), Spring (1972), Reimer (1973), Holt (1973) and Bowles and Gintis (1976).

The priority of career demands in the life of scientists is pointed out by Horrobin (1969, p. 9) and Salomon (1973, p. 206). Krohn (1971), to explain how the opinions of working scientists come to reflect the immediate situation of their research, argues that scientists are drawn to their positions according to the conditions of research and according to their own career strategies. Sklair (1973), to explain how science works, argues that people do what they do because (1) they see no viable alternatives, and/or (2) they are literally forced, and/or (3) they are mistaken as to the true nature of the relationship between their interest and their action.

Blume (1974) emphasises and elaborates on the political nature of the social system of science. There are few works which do this; this lack reflects the orientation of orthodox sociology of science, whose political uses are noted by Sklair (1973).

Boffey (1975, especially pp. 83-87) documents how the U.S. National Academy of Sciences, when advising the government on matters affecting the scientific community, acts more like a self-serving advocate than an impartial judge. The unscrupulous struggle for resources by the scientific community is best documented by Greenberg (1967). The case of the decision to drop the atomic bomb is analysed by Haberer (1969). Haberer (1972) argues that leaders and members of the scientific community are more interested in the survival and growth of the institutional framework than anything else.

Mills (1963, pp. 61-62) and Elliott and Eliott (1976, p. 65) note that precisely because of their specialisation, scientists are among the most subservient and easily manipulated groups in modern society. The obliging acquiescence of German scientists to the Nazi regime gives support for this argument. (For an excellent account of thc capitulation of the German scientific establishment; see Haberer (1969).) The importance of specialisation in making possible the 'industrialisation of scientific production' in recent decades, and thus its subordination to the interests of those who fund research, is spelt out by Gorz (1976) and Lévy-Leblond (1976).

Cirino (1971, p. 135) describes how, in the environment of a deluge of isolated news fragments, bias serves as an underlying unifying factor to give meaning to the news. He gives the analogy of learning a first language, through which process one accepts the world view inherent in the structure of the language. Similarly, the non-expert exposed to a barrage of specialised scientific work tends to accept the assumptions inherent in the selection of problems and choice of methods and types of solutions sought.

Chapter 10: Who can use scientific research?

In practice the results of scientific investigations are not equally useable by all people.

I discuss here the useability of scientific research in terms of accessibility, understandability and exploitability, first treating these factors in the abstract and then giving examples from the SST-NO_x-ozone area.

Useability of scientific research, in the abstract

First consider physical accessibility. For the purposes of considering accessibility, scientific research can be classified into three categories: material not available at all, material very difficult to obtain and material in scientific journals.

The first category is material that is not available at all. Classified research done for the military and proprietary studies done for companies constitute most of the work that is completely inaccessible to outsiders. As well, there may be various reports or studies done in government departments or universities which are considered confidential. A large fraction of scientific studies are of this type: available neither to the general public nor to other scientists. Not only is such research never made publicly available, but usually the knowledge of its very existence is also restricted.

The second category is material that is very difficult to obtain. Usually material of this type is easily accessible only to scientists working in a particular research area. Examples of such material are advance reports of research in progress, papers presented at conferences, reports for internal circulation in organisations and submissions to government bodies. It is true that much of such material would be made available to any interested person who asked for it. But knowledge about the significance or even the existence of such material is difficult to obtain. In practice most scientific work not published in journals or books is effectively inaccessible to all but specialists.

The third category is scientific research that is published in

journals and books. Such material is officially public. Even so, much of it is not readily available to the majority of people who do not live conveniently near an appropriate library.

Closely related to purely physical accessibility are other factors. One of these is time. Much material communicated in internal reports or conference proceedings eventually appears in scientific journals. But often only specialists have access to the material at significant early stages of the work. For example, decisions may be made on the basis of testimony by committees of scientists. Outside groups may only learn about the material later from news reports or from scientific journals. As a result their influence in the decision-making process is greatly reduced.

Another factor is money. Organisations which employ full-time scientists usually will pay for any reasonable amount of material needed for the work of their employees. Expenses will include the photocopying of papers out of journals, the buying of unpublished documents from central information agencies, and the buying of conference proceedings, official reports and books. A private individual or group may find that keeping up with material in a given area is an expensive venture.

In summary, reports of scientific research are not equally accessible to all people. Much research is easily available only to specialists and various insiders. Even publicly published material may be inconvenient to obtain, out of date, and costly to the outsider. Finally, the very knowledge of what material is available is often difficult or impossible to obtain.

Second, consider understandability. Most scientific research is not easily understood by the non-specialist. Indeed, much research would hardly be less understandable if it were written in hieroglyphics. As a result, the question of accessibility becomes irrelevant in many cases. The practical use of scientific results is restricted to those people who can gain an effective understanding of the material without an undue investment of time and effort.

Large and powerful organisations of various types are able to hire people to search out, filter, understand and explain scientific results that will be useful to the organisation. For example, often companies hire top ranking scientists to do research in a particular field, which is related more or less closely to company interests. Basically the company is paying for two things: first, an increased probability that useful breakthroughs will be made by the company's own scientists; and second, knowledge about what is going on in the research field around the world. A large part of the value to a company of employing scientists lies in the ability of these people to understand and interpret scientific research for the purposes of the company.

On the other hand, less favoured social groups, without as much money or power, cannot afford to maintain experts to understand and interpret scientific results. As a result, the difficulty of understanding most scientific research puts such groups in a relatively disadvantageous position.

Once again the questions of time and money are important. Gaining a practical understanding of much scientific research would be feasible *if* one had the time and money to devote to the task. If one could take off a year to study a topic, a useful understanding of it probably could be gained. But most people do not have a spare year nor the money to support themselves during the time off from work. So in practice the research is only understood by those who have been given outside support — university training and a job — for the express purpose of understanding the research.

Third, consider exploitability. Most scientific research can be exploited only by persons or groups possessing a certain minimum amount of money, or experience or power. The research on most topics I can think of — such as rocketry, lasers, polymers or hormones — cannot be exploited directly by individuals or non-specialist groups. Instead, the exploitation of the research usually occurs through the medium of a large organisation. So even if people had access to and could understand scientific results concerning such topics, in most cases they would not be able to exploit the results effectively due to lack of resources.

The selective exploitability of scientific research has wide ramifications. Because most individuals or groups of people cannot exploit scientific research for their own purposes, they are not really interested in gaining access to it or in understanding it. In spite of scientific research being selectively accessible and understandable to powerful groups in society, it is still possible for reasonably persistent outsiders to overcome many of the difficulties. But there is not much point if the research cannot be exploited. Because of this, people do not object too much to selective accessibility and understandability, which come to seem somewhat irrelevant as long as exploitability is equally selective.

Some examples

Consider research on mathematical optimisation. Mathematical optimisation or optimal control theory is a branch of mathematics concerned with determining conditions for the control of physical equipment in an optimal manner. Typically this control involves feedback, as in the case of a thermostat. The theory may be applied to such areas as industrial processes, resource allocation or modelling of the economy. Research in this field is not readily available to the outsider, is not readily understood by the non-expert and is not easily exploited without heavy financial backing. On the other hand, large organisations such as corporations or the military have ready access to research on mathematical optimisation. They employ scientists who do research in this area, who keep up with other research that is done in the area and who interpret it for others. Finally, large organisations have the funds and the authorisation to exploit the research for their own purposes.

As I see it, these same features of useability apply to almost all modern scientific research. It is primarily powerful organisations in society, and the groups they serve, that are able to use the results of conventional scientific research for their own purposes. From a general perspective, this is not surprising. First, much research on most important scientific problems is funded directly by the organisations interested in particular problems. Second, even research not funded by these organisations is selectively refined, developed and applied by these organisations. This selective attention provides an incentive for work in the particular fields and in the particular manner which is especially useful to the large organisations. So even in so far as the results of science have a range of potential uses, particular results are singled out for development, application, exploitation and further attention. Other scientific possibilities may be ignored or even repressed.

Consider a further example, research in theoretical nuclear physics. Much of the work done in this area is not top secret material. But from the point of view of the public it would hardly matter if it were, for it is extremely difficult to understand except for specialists, and extremely difficult to exploit except by massive organisations. Even if an individual could understand the research, practical use of it would be impossible. In contrast, huge military and civilian organisations have the resources to exploit any development in the field that is useful to them, whether in relation to military hardware or to nuclear power production.

The bulk of theoretical nuclear physics research is not directly useful to anyone, even to massive organisations. What purposes does this 'useless' research serve? Scientists would claim that it serves to attain scientific knowledge. Even if this claim were accepted, apparently 'pure' research such as in theoretical nuclear physics also serves several other purposes. The research can be seen as an investment useful to powerful organisations, paid for primarily by the general public.

First, even though most theoretical nuclear physics research is valueless for any human purpose, there is always the possibility of a discovery useful to those groups which can

recognise and apply it. From the point of view of the large organisation, a diversity of 'pure' research can be seen as an investment: some developments are likely to be useful to the organisation, and the bulk which are irrelevant are unlikely to serve groups with alternative goals. Second, the mass of research in theoretical nuclear physics serves to maintain a trained core of scientists and technical workers. These workers are indirectly made faithful to organisations promoting military use of nuclear weapons or civilian use of nuclear energy, through the dependence of their livelihood on the continued funding of nuclear physics. The skills of these specialists are not easily transferable to projects serving other ends and benefiting other groups. Finally, the 'investment' in theoretical nuclear physics research — in the training of the researchers in schools and universities, and often in slaries and equipment as well — is paid for primarily by the general public. Because the burden of funding of theoretical nuclear physics research falls on the taxpayer, it is not surprising that funding organisations often do not mind if much of the research turns out to be irrelevant to their immediate needs. Furthermore, this irrelevance makes it easier to justify the research in totality as valuable for its own sake.

In society as it is presently structured, it seems inevitable that theoretical nuclear physics research will be available, understandable and exploitable primarily by large organisations.

As well as research in mathematical optimisation and in theoretical nuclear physics, there are many other, more clear-cut examples. Here are a few research areas and some prime beneficiaries: missile guidance systems (the military); tranquilising drugs (drug companies); mob psychology (police); and keeping the orange pulp from going to the bottom of orange squash (manufacturers of orange squash). Once again, I feel I must emphasise that I am talking about *selective* useability. The proper comparison is not between the 'bad' and the 'good' uses of nuclear physics research, but between nuclear physics research which by its nature and the nature of human society *lends* itself to 'bad' uses compared to research into organic gardening that *lends* itself to 'good' uses.

Science can be used for many different purposes, but it is *easier* to use for some purposes than others. One can use a machete to butter bread, but it is more natural to use it for more energetic or grievous purposes. Just as the selective useability of the machete is inherent in its size, shape and cutting ability, so the selective useability of scientific research is inherent in its content and structure.

Useability of SST-NO_x-ozone research

Who can most easily use the results of SST-NO_x-ozone research? It seems that large organisations do not have such an advantage as they do in some other fields. Research results have been used to argue both for and against the development of SSTs. In particular, environmental groups were able to find evidence to use against the development of SSTs. It might be that this situation depended vitally upon certain scientists, such as Johnston and Crutzen, having the same presuppositions about the burden of proof as did the environmental groups. In any case, here I will try to illustrate the features of accessibility, understandability and exploitability with examples from the SST-NO_x-ozone research area.

As far as completely inaccessible work is concerned, it would seem unlikely that there was a significant amount of classified military research involved in this area. But military secrecy still can affect the research done in the area. For example, Goldsmith et al.'s calculation of the amount of NO_x produced in a nuclear blast strongly depends on available scientific knowledge on nuclear explosions, such as how much and how rapidly energy is produced and dispersed in a nuclear explosion. Goldsmith et al. based their calculation on the model of Taylor, which was published in 1950. Undoubtedly more complete calculations now exist, but the most comprehensive certainly are classified. Even Taylor's paper was only declassified long after having been written in 1941.

On the industrial side of SST-NO_x-ozone research, there almost certainly have been numerous studies made by aircraft company scientists, which have never been published. Of course it is difficult to prove this claim, since there is no way of knowing about things whose existence has not been revealed.

On the level of research that is very difficult to obtain, there has been in the SST-NO_x-ozone area the typical plethora of advance drafts of papers, internal reports, conferences and private communications. Here again the factors of time and money are important. For example, Johnston's work, in various draft versions prior to its final published form in *Science*, was circulated to various scientists, environmentalists and politicians. This was part of an attempt to influence the decision of the U.S. Congress about funding of development of SST prototypes. (Of course there were many other people and groups attempting to influence the decision.) In order to have had knowledge of and access to Johnston's work at this stage, and thereby to have had even the opportunity to influence decisions through this work, one would need to have been an insider of some sort.

Consider an environmental group opposed to SSTs, which wished to study the scientific literature pertaining to the SST-NO_x-ozone problem. Some members of the group — perhaps specialists from a different discipline — may have been familiar with methods of communication in science and with the basics of the research area. But even these members would have found it a laborious and time-consuming, if not unsuperable task to unearth all the pertinent materials: unpublished articles, conference proceedings, internal reports, so-called private communications and articles in obscure journals — not to mention vital documents whose existence is known only to insiders. No one is actively stopping the inquiry of interested people. But the structure of the scientific communication system makes it difficult for even a fairly knowledgeable person to gain access to relevant material.

This is not to mention the many very real cases where scientific material is not made available to interested parties. Often there is no legitimate channel through which important material may be obtained. For example, material presented by scientists at closed governmental hearings might be obtained through the scientists involved, but in general it would be unavailable to the outsider. The same applies to private correspondence. Often copies of revealing letters are distributed widely, but again they are not available to the outsider.

The expense of SST-NO_x-ozone material is perhaps not a vital issue. Probably there are few people who know about and want the material and who cannot charge it to the organisation for which they work. But for those few, the expense might have a deterring effect. For example, the proceedings of three U.S. conferences cost me U.S.$42. This is not even to consider the expense that would be involved in attending conferences or visiting scientists — often nearly indispensable ways of obtaining important current information.

If a person familiar with the rules of the scientific communication system and experienced in searching out information for a case has trouble getting hold of material, what of the typical concerned citizen? It would seem a mild comment to say that scientific research relevant to the development of SSTs was not available to the public in such a way as to permit widespread and effective involvement in the issue.

Next there is the understandability of SST-NO_x-ozone research. It is an understatement to say that much of this research is not easy to understand. Furthermore, as research in the field developed, ozone models tended to become more complex and comprehensive and so less understandable by non-specialists.

The amount of material involved is relevant here. The major investigation into the SST-NO -ozone problem, the Climatic Impact Assessment Program based in the U.S., involved hundreds of researchers working over a period of years. The

report of findings alone covered six volumes; there are also hundreds of published and unpublished studies relevant to the topic. The very quantity of material is intimidating. There are few groups that have the necessary human and monetary resources to challenge a view supposedly based on this mass of allegedly relevant knowledge. At the very least, some familiarity with the material would be required, just to challenge its pertinence to the issues at stake.

I would also contend that many papers in the field could have been written in a manner that was considerably more understandable both to the specialist and to the non-specialist. There are various explanations for poorly written and hard-to-understand papers, such as time pressures. One contributing factor is the unconscious desire of many researchers to impress colleagues and appear inaccessible to the non-expert.

To blame only individuals for this is a little unfair. What happens is that the standards of communication — what jargon and writing style is acceptable, and what level of audience is written for — come to reflect the interests of the research community, and especially the leaders of the community, in their professional status. Within the speciality, communication based on difficult and obscure concepts and styles is justified in terms of efficiency. The implicit assumption is that communicating to a wider audience at a research level of understanding is of low priority or actually undesirable. In this way institutionalised interests in maintaining professional status may contribute to much research being hard to understand.

Finally, there is exploitability. It is my belief that most SST-NO_x-ozone research can be used in the long run more easily by groups supporting SSTs than by their opponents. (This claim depends somewhat on the location of the burden of proof: I am assuming some stance intermediate between the attitudes of Johnston and of Goldsmith et al.) This is because the results can always be used only to set limits on SST operations: on the number of aircraft in the fleet, on the permissible routes or on the amount of NO_x in the exhaust of SSTs.* For example, if the amount of NO_x in SST exhaust could be reduced by a factor of 60, then with no increase in the particular environmental dangers due to SST-NO_x, the fleet of SSTs could be roughly 60 times as great. Such a technological solution is available to the aircraft industry. No such options are available to opponents of SSTs, because they do not have the social and financial power to implement them. Alternative options such as the development of transport systems based on minimal use of energy or on equality of access are primarily political and social solutions, not technological solutions.

Let me rephrase what I am saying. In the short term, some SST-NO_x-ozone research *may* be disadvantageous to aircraft companies, if it sets limits to their operations. Likewise, it may be advantageous if it shows that these limits are not serious — in this case the research justifies their operations. In the long term the research is almost entirely useful to the aircraft industry, since it provides knowledge within which a technological solution to the environmental problems due to SST-NO_x — reducing NO_x emissions — becomes feasible.

That SST-NO_x-ozone research is selectively useful to the aircraft industry in the long term is only clear if one is conscious of the *context* of scientific research and other activities which might have been done instead of SST-NO_x-ozone research. Millions of dollars and many years of intensive effort by many people were put into SST-NO_x-ozone research. What else might have been done instead? There are many possibilities, from studies of low energy transport systems, to studies of leisure, to the promotion of community involvement in decision-making. Compared to many of such possibilities, SST-NO_x-ozone research is selectively useful to the aircraft industry.

This is not to say that the aircraft industry has been pleased with this research. They have pushed every scientific fact and argument suggesting the innocuousness of SST-NO_x, attempted to discredit advocates of the danger of SST-NO_x, and in other ways resisted admitting the significance of this work. The existence of the research is certainly to their disadvantage. But given that it exists, extensive and detailed study in the area serves the interests of the aircraft industry to a much greater extent than would many of the possible alternatives to this research.

It is only fair to say that SST-NO_x-ozone research probably has been more widely accessible, more understandable and more easily exploited by groups with divergent values than is the case with most scientific research. At the same time, the work has not been equally useful to all groups: large organisations promoting SSTs still have had the advantage in terms of accessibility, understandability and exploitability. However, this advantage has not been as great as it is in other areas of the SST debate. This is because many political and economic issues are automatically decided in favour of large organisations. For this reason environmental issues have been very important in the SST debate. In this context, research on environmental issues provides a basis for justifying decisions made primarily for political and economic reasons. This is the subject of the next chapter.

The inherent nature of selective useability

The actual type of science that is done, the audience it caters for and its exploitability by different interests, represent only one possibility out of a range of alternatives. But this choice out of a range of alternatives does not *seem* to be a choice at all, because of the constraints placed on conventional scientific activity: specified criteria for adequacy and value, selective funding, specialisation and institutionalisation. In effect, then, these constraints make selective useability a built-in, structural feature of the conventional practice and products of science — a presupposition.

The selective useability of scientific research as seen by the outsider directly relates to the structure of the scientific establishment. Problems of accessibility reflect the organisation of the scientific communication system, which in turn is suited to the needs of scientists and their sponsors. Problems of understandability result in large part from the extensive specialisation in science, which protects professional scientists from the scrutiny of outsiders as well as making their work easier to manipulate by dominating groups. Problems of exploitability reflect the funding of most scientific research by large organisations.

The selective useability of scientific research also is built into the structure of society. The hierarchical structure of society with an extensive division of labour and the control of real choices by political, managerial, technological and other elites, makes the achievement of a science useable by less privileged groups extremely difficult without major changes in society. But there is no conscious plan to ensure this state of affairs. Rather the selective useability of scientific research is a natural part of a science and a society geared towards maintaining their essential structures.

Reference notes

There is quite a lot of material written about the accessibility of scientific research. Most of this is in the form of sociological investigations into the communication system of science: the means by which scientific information is communicated, the

*The Climatic Impact Assessment Program (CIAP) as a whole indeed seems to have been based on the premise that, at most, limits would be set on SST operations. The stated CIAP 'objective' is: "In order to determine regulatory constraints on flight in the stratosphere such that no adverse environmental effects result, DOT [the U.S. Department of Transportation]/CIAP will assess, by 1974 report, the impact of climatic changes resulting from propulsion effluents of vehicles in the stratosphere, as projected to 1990".

scientific audience of a scientist's work, the social structural reasons for the development of certain communication techniques, methods of handling the information explosion, 'invisible colleges' of closed interacting research groups, and so forth. Much of this material is interesting and I would not disagree with most of what is said in it. But this material is about the way science *is*: it is concerned with explaining, and therefore often implicitly justifying, current practices. My perspective is different. I look at features of the scientific communication in terms of how it affects the useability of scientific research. Because the standard literature on the subject is from a different perspective, I have drawn mainly on my personal experiences in describing the accessibility, understandability and exploitability of scientific research.

Gellhorn (1950) gives a thorough discussion of secrecy in scientific research done for the military. Some indications of the effect of secrecy in science are given in Committee on Science in Promotion of Human Welfare (1969), in an otherwise conventional article. Taylor (1950) at the beginning of his paper, tells how he had written it in 1941, and how it had been superseded by more complete calculations (presumably classified at the time). My knowledge about early versions of Johnston's *Science* paper was gained from comments by scientists in a draft of "Summary of discussion meeting of NAS ad hoc panel on the (NO_x)-ozone problem. Washington, D.C., 29 July 1971". This document is also labelled "Privileged. For panel use only." Other examples of documents very difficult to obtain are Goldsmith (1971) and Crutzen (1971), which were referred to in the Australian Academy of Science report (1972), but which were not available to the public. No doubt there are many other examples, but not being a real insider in this field, I only know of a limited though revealing number of examples.

To see how SST-NO_x-ozone models increased in complexity as the CIAP programme progressed, compare for example Johnston (1971) with McElroy et al. (1974). The latter study still involves a fairly simple model by CIAP standards.

I have mainly looked at accessibility from the point of view of the outsider. From within the scientific community, the emphasis is on the effective functioning of the scientific communication system for the purposes of scientists and those who exploit the products of scientific research. This latter perspective is adopted in most accounts of the scientific communication system; accessibility by the public is just not of concern. But because of the increasingly private nature of important scientific communication, there are increasing problems even for the scientist outside the in-group; in this context there are a number of sociological studies that comment on how scientific communication constitutes a closed system in the main (Price and Beaver, 1966; Garvey and Griffith, 1967).

There is a fair bit written on the understandability of scientific research. Most of it is concerned with such things as improving communication between scientists and popularising science. (Whatever the intent of popularisers, popularisation often serves mainly to ensure public support for expenditures on science.)

Most of the work on such topics assumes that most science necessarily will be hard to understand by non-experts. That is, it assumes that science is (and should be or must be) based on specialised expertise that is not readily communicable. Keep this in mind when you read about communication in science or the communication of science to the public. Galbraith (1971, chapter 2) makes some insightful observations about the understandability of economics. Whitley (1977) describes how theoretical scientists promoted the prestige of mathematical formalisms as a scientific ideal, a process which serves to establish a cognitive distance from the lay public and as well ensure that control over resources used in scientific production remains in the hands of those at the top of the professional hierarchy, especially the theoreticians.

An extreme example of the idea that technological solutions can be developed to serve vested interests is given in a short article in the U.N. journal *Important for the future* (1976). It suggests that a study should be made into the possibility of replenishing the ozone layer — depleted due to human activities such as SSTs and fluorocarbon aerosols — by ozone manufactured by human technology.

Easlea (1973) notes that conceptual revolutions in modern physics are not resisted by ruling political and economic elites and their apologists, whereas in fields with direct social implications, such as economics or history, conceptual revolutions often are resisted. To me this does not indicate that modern physics is ideologically neutral. I interpret it as resulting from the strong control over the direction of modern physics research exercised by the military and by large governmental and corporate organisations, and from the fact that almost all the research is only accessible, understandable and exploitable by these same groups.

The statement of the CIAP objective is found in many CIAP publications. I have taken it from the U.S. Department of Transportation CIAP Newsletter of September 30, 1973.

Chapter 11: What is science used to justify?

Much scientific knowledge and practice does not serve any special interests in a material fashion. That is, it is not useful in the development of practical techniques in areas such as manufacture, management or social control. Yet this 'pure' science may serve to reinforce inequitable political and economic institutions by justifying certain practices or policies.

Let me begin with a simple example. Consider a decision to be made about where a public swimming pool is to be located: in a rich area or a poor area. One way to make this decision would be on the basis of existing power structures — say through a planning board. Since the richer sections of the community normally have more power and influence, on this basis the pool would be put in the rich area. However, the use of power alone to make this decision may have some disadvantages for its beneficiaries. It is a little too blatant: it might make the people in the poor area unduly aware of the bias against them in social decision-making and action, and in a small way jeopardise the organisation of society geared to reward the rich at the expense of the poor. It might be much 'better' to have the same decision made and validated on the basis of a 'scientific' evaluation of the situation. A typical technique that might be used in this case is cost-benefit analysis.

An appropriate analysis of the 'facts' would demonstrate that a pool in the rich area is a smaller burden on the taxpayers (that is, it would be more profitable) than one in the poor area. This might be simply because the rich would pay more to use the pool, although other reasons could be presented as well. Cost-benefit analysis could thus sanction or legitimise the decision.

It is not hard to see that this particular scientific sanction is not free of values. Cost-benefit analysis is normally based on many implicit assumptions. One of these is that the existing distribution of wealth is just. Another is that social costs and benefits can be reduced to a common measure, usually money. If a different set of values were used, a different decision might be made. For example, the benefit of the pool might be measured by the number of people who would use it, or whether the people who used it made decisions about it or ran it. Methods of 'scientific' evaluation normally incorporate the same values that are implicit in the current organisation of society. Therefore these methods tend to make wealth, power and prestige appear to be the outcome of procedures that are inevitable and just.

When science is used to justify certain practices, the issues are almost never so clear as in my illustration of the swimming pool. In this case itself, cost-benefit analysis most likely never would be brought up explicitly. Instead, use might be made of the particular value structure built into the accepted and publicised explanation of the economic organisation of the society — for example, the market system. This explanation serves as a justification for a particular allocation of resources. The presence of numerous private swimming pools is seen as the natural and just outcome of the proper functioning of the economic system. In this way economics can serve as a sanction or justification for inequality and for inefficient use of resources.

There are a great many excellent studies of the way science is used to sanction certain practices. Indeed, in many of the social sciences the entire body of orthodox knowledge may be seen as a rationale for the existing organisation of society. Furthermore, orthodox knowledge through its monopolisation of the terms of analysis may serve to limit or divert more critical analyses. Although the use of the physical sciences as legitimisations is not so extensive nor so obvious as it is in the social sciences, it is still an important aspect of these disciplines. But instead of trying to survey the work already done on this topic, here I will elaborate on my favorite example.

What practices or policies does SST-NO_x-ozone research serve to sanction? I suggest that the primary use of this research in this respect is to justify the decisions made concerning the development and introduction of the SST. This occurs primarily through the placing of the debate about SSTs on the level of environmental issues, thereby downgrading political and social issues. It also operates through reducing the divergence of opinions about the SST to the level of a scientific debate between experts. I will try to explain this perspective on SST-NO_x-ozone research in some detail.

How is a decision made about whether to produce and fly SSTs? Usually there are several groups that must be taken into account in such a decision. First there are powerful groups with direct vested interests, such as particular companies and government departments. In opposition to these groups there may be other powerful groups who favour a different decision, for varying reasons. Possibilities are companies or government departments which prefer investment on alternative projects, who stand to lose from the development, or who believe that the project as a whole will be detrimental to their political and economic interests.

In the case of the SST, the main groups with vested interests in the project would seem to be aircraft companies, numerous subcontracting companies, the military (wishing to help its sub-firms prosper), local government bodies in areas where construction work would take place, government bureaucracies involved in allocating development funds and regulating flights, politicians and bureaucrats who have tied their career to promotion of the SST, and various elite executives, politicians and rich people who would benefit from faster travel and the greater prestige of SST travel. For example, political considerations involving loss of 'national face' on France's side, and maintaining good relations so as to be able to join the Common Market on Britain's side, over the years have been important in maintaining the Concorde project.

The main groups with power who opposed SSTs would seem to be airlines hoping for a delay before introduction of supersonic operations so as to get full use out of their expensive large subsonic jets, companies hoping for allocation of government funds to themselves rather than to aircraft companies, middle class organisations opposed to the sonic boom and other environmental assaults, and politicians opposed to the entry of the federal government into private industry (in the U.S.). For example, many real estate interests with land near airports strongly support measures which reduce airport noise and thus increase the value of their land. There are also powerful interests opposing particular types of SSTs. In particular, many groups in the U.S. have opposed Concorde because they hoped to benefit from U.S. domination of the SST market later on.

These first two types of groups are the ones primarily involved in decision-making. The decision is made mainly on the basis of power. The criteria used are in practice mainly political and economic: that the development mainly benefits already advantaged groups (which have power); that it leads to profits or payoffs to already wealthy interests (which have power); and that it strengthens the existing systems for distribution of power, prestige and wealth.

The powerful interest favouring SSTs support the SST on all these points: benefits of fast and prestigious travel accrue mainly to the rich and prestigious who can afford to ride in SSTs, such as businessmen and politicians; monetary rewards go mainly to already advantaged executives and officials, such as aircraft company executives and their faithful supporters in government; the national prestige of having an SST benefits mainly those who already bask in national glory, such as national leaders; and the development project cements the symbiotic relations between government, big business, and the technocracy.

Those powerful groups opposing the SST use similar criteria: many of the privileged may find their private retreats invaded by sonic boom; the expense of the SST may be excessive; national prestige may suffer if the aircraft is not successful; and people may be stirred up if too many uneconomic and blatantly elitist projects are pursued. For example, airlines may (perhaps covertly) oppose the SST if they do not expect it to be sufficiently profitable.

My interpretation then is that the powerful decision-making groups agree about their basic aims — the promotion of their own power, prestige and wealth, and the stability of the society that ensures them these things — but disagree about the detailed methods to be pursued.

Where do the people as a whole come into this picture? The people as a whole have little say in the decision as to whether to develop and produce SSTs. It is true that citizen groups can lobby elected officials or other bureaucrats; that the private citizen can write letters of protest; or that individuals can withdraw financial support by not buying stock in the companies involved. But even to list these possible influences is to illustrate the lack of real power of individuals in such a decision. Even so, the extent of this powerlessness is more apparent in a catalogue of the activities in which the citizen is never involved.

First, the citizen has no say in planning: in deciding where public and private moneys are to be spent, in deciding what technologies are to be promoted, or in deciding what sort of society is to be fostered by the organisation of transport, housing, manufacture and conditions of work. Citizens had no say in the planning stages when various countries decided to

consider seriously the development of SSTs, rather than alternatives such as development of mass public transport or transition to a low energy economy.

Second, the citizen has no say in what choices are presented for consideration. Even if the question of SST development is presented for open debate, the question is presented as SST versus no SST. It is not presented as SST versus mass public transport versus low energy economy. Furthermore, the citizen has no say in the resources allocated to proponents of each viewpoint. Proponents of SSTs have extensive funds, mainly from aircraft companies and governments, to support their point of view. Opponents of SSTs are not so well funded. Proponents of alternatives to transport based on large amounts of energy and expertise have virtually no financial backing at all.

Third, the citizen has little or no say in what criteria are used in making the choices that are presented. SSTs may be supported or challenged on various grounds, from economics to equity to environmental impact. But only some of the arguments actually carry much weight, such as those concerning national security, economics and in recent years serious danger to the environment. If decisions are made mainly on economic criteria, it is difficult to have much effect in supporting or opposing SSTs on grounds of equity.

It may be argued that the citizen is free to get involved in these issues if desired. One could, for example, become a town planner, a government bureaucrat or an aircraft company executive. The basic presupposition underlying such arguments is that society is inevitably organised the way it is at present: hierarchically, with specialists who know what is best for other people, and with the possession of privilege being the automatic and just outcome of natural processes. Given a different presupposition, though, the current lack of participation may be seen both as unnecessary and as a cause and as a result of injustice and inequality.

Consider the limited power of the public in the question of the introduction of the SST. One of the only channels available for influencing decisions is through pressure groups, which may for example lobby legislators. To be successful, these pressure groups need arguments that are accepted as being both valid and important. Therefore, it becomes important to ask, what arguments can be generally accepted as valid and important in a debate about the introduction of SSTs?

There are several areas which are important: politics (national prestige, political vested interests), economics (profits, economic advantages of high-speed travel), social impact (inequity, employment) and environmental impact. It would be possible to discuss each of these areas and others at great length. Without arguing the points, my opinion is that political, economic and social arguments are not easily used by opponents of SSTs. There are two main reasons for this. First, evaluations of political, economic and social effects are generally made in terms of establishment criteria. For example, national prestige is generally determined by national leaders and economic viability is measured in terms of company profits and losses. Second, arguments based on political, economic and social considerations are obviously based on value judgements. Therefore any such argument can be countered by other arguments that are based on alternative values. For example, what are seen as great advantages of SST development from one perspective — faster travel, provision of jobs, fostering of technology — may be seen as of negligible benefit or as disadvantages from another perspective.

Given these conditions, an ostensibly value-free argument becomes useful to opponents of SSTs. And here is where science comes in. In recent years arguments based on the possibility of disturbance of the environment have been used to a great extent precisely because they apparently are strictly 'scientific' arguments — and because they have not already been co-opted by big business and government. Because science carries the air of neutral authority, environmental arguments can be and are used against development projects such as the SST.

The situation then is this. The citizen is excluded from planning and decision-making in a basically political and economic context. The only readily available channel left for opposition to vested interests is based on environmental considerations.

It is true that publicly expressed concern about the environment can have a great impact on decision-making in certain cases. This is especially notable in the case of the SST in which citizen organisations have mobilised a strong opposition to SSTs. On the other hand, it may be argued that the very fact of a great public debate signals a lack of any real power in the hands of the people. Most of the planning and decision-making takes place in the inner corridors of industry and government, with little public participation. Public protest against a project — the visible result of much planning and decision-making — may be a symptom of the lack of public involvement in the origin and development of the project.

There is nothing wrong with bringing up environmental issues in arguing for a certain action or decision. But the emphasis on such issues may not lead to a greater inclusion of political, economic and social issues in the publicly revealed realm of decision-making. In this situation, the dissatisfied citizen continues to accept implicitly the constraints under which effective public involvement can take place. The basic economic/political/social conflict becomes permanently displaced into the realm of scientific debate.

In these circumstances, scientific results can become a major constraint on a political decision, but they also can serve as a major justification for a political decision. On the one hand, the possibility of a large environmental impact can be reason to halt a development project. On the other hand, if the predicted environmental impact is sufficiently low, then further opposition to the development is seen as unjustified. In this way the study of environmental impacts can become the basis for a justification of decisions basically made on other grounds.

A critical test of the idea that environmental issues and studies are used to justify actions and decisions is the prevalence of this idea: that a sufficiently insignificant environmental impact means that the planned action can go ahead. In other words, the political, economic and social context of the decision is assumed to be the status quo, and the only remaining question is the environmental impact. I believe that this is indeed the case for the SST. I have read quite a number of newspaper articles and comments in scientific magazines — not to mention aircraft company statements — that assume that the only valid public opposition to the SST is opposition on environmental grounds.* Of course, the SST

*"The Minister for Transport Mr Nixon, said yesterday that environmental submissions which he had seen so far did not appear to contain any new factor which would stop services in Australia by the Concorde aircraft" — " 'Nothing new' on Concorde", *Canberra Times*, 15 April 1976, p. 1.

"Concorde also has to show that the concerns of environmentalists are either unfounded or overstated. That much has to be done before it will be allowed to fly into Australia on a regular basis. If it demonstrates that its manners have improved — that it is not as noisy and dirty as its sister prototype which flew here in 1972 — it still faces commercial obstacles." — "A fair go for the Concorde" (editorial), *The Australian*, 5 August 1975, p. 8.

"So conservationists are flying in the face of wide approval in trying to keep the jet out, and attempts to convince people that the Government paid only lip service to environmental considerations when it deliberated over Concorde's Australian future are likely to fail." — "Concorde wins approval from public", *The Times*, 28 July 1976, p. iv.

"It seemed to me that as nothing in environmental terms stood in the way it was proper that the British be advised as soon as possible of our decision to allow Concorde to come to Australia." — Mr Newman, *Hansard* (Representatives), Environment: Concorde aircraft, 2 June 1976, p. 2807.

See also the excerpt from the Project to Stop the Concorde leaflet reprinted at the end of chapter 8.

may be unsuccessful on other grounds. It must be politically acceptable — governments must continue to support it — and it must be economically viable — passengers must be found to ride in it. These grounds are not ones in which the majority of people have any say.

By posing environmental questions on the level of a debate between experts, most people cannot have much of a say even in the environmental debate. Pitching a debate on environmental terms of a technical kind means that scientific arguments and experts are needed for decision-making. It is not surprising that scientists — who naturally will not be averse to the creation of a demand for their services and of a base for their prestige — do not object to lack of attention to the social and political dimensions of the environmental problems they study.

There are several consequences of the displacement of political and economic issues into the environmental arena. One is the notable divergence of implicit values in scientific work relating to the environment: for example the different presuppositions underlying the work of Johnston and of Goldsmith et al. This divergence of presuppositions is not found so dramatically in most scientific fields, in which implicit values are very similar and hence less noticeable.

Another symptom is the use of 'scare tactics' — emphasis on environmental dangers — by many citizen groups. Valid opposition to a project on political grounds may not have a corresponding validity in the environmental sphere. When the argument is forced into unsuitable channels, the pushing of the argument often becomes most obvious.

What about scientific work on environmental topics, then? Much of it may be valuable. But in the context in which it is done, it serves to restrict the argument to the environmental level, and to obscure basic issues.

Let me provide an example of the way emphasis on environmental issues can obscure political and economic issues. One argument in favour of the U.S. SST was that initial development would be restricted to prototypes. That is, only one or two or several SSTs would be built initially. These prototypes could be used to learn about engineering problems, environmental effects and so forth. Opposition to prototype development on environmental grounds seems quixotic. For example, the effects on the stratosphere of two SSTs would be minimal. Furthermore, the prototypes could be used to determine more about the possible effects on the stratosphere, surely an admirable aim.

The significant feature of both the argument in favour of prototypes and the opposition to them on environmental grounds, is that they are based on hidden political and economic assumptions. In particular, proponents of prototypes often assume that operations could be curtailed if any dangerous effects were discovered. Opponents assume that once the development got started, it would be difficult to stop due to vested political and economic interests. (Actually, proponents sometimes make this latter assumption, but then assume that on further investigation the environmental impact will be found to be minimal.) As long as environmental effects are considered alone in isolation from political and economic assumptions, the environmental 'facts' can be used to justify the decision favoured.

How can SST-NO_x-ozone research be used to justify a decision? Consider for example the report of the U.S. Climatic Impact Assessment Program (CIAP). This might be used to justify a future SST by specifying the NO_x emission levels that would be 'acceptable' in terms of affecting ozone. The decision might actually be made on political and economic grounds. On the other hand, the CIAP report might be used to help justify denial of entry of foreign SSTs (the Concorde) into U.S. airports. Such a decision might actually be made on political and economic grounds, say to protect the U.S. aircraft industry. My point is that justification of certain decisions or actions may be a motivation for the doing of certain scientific research, for the way in which the research is done and for the form and content of the scientific results. And once produced, scientific results may be used to justify decisions and actions made on an entirely different basis than the scientific results. SST-NO_x-ozone research on a mass scale as in the CIAP project first serves to define the realm of discourse about SSTs as largely environmental, and then serves as a justification for whatever decision is actually made.

It is my feeling that in the end the emphasis on environmental issues will mainly serve to bolster existing political and economic arrangements. It is true that in certain cases environmental arguments can be used successfully against vested interests. The U.S. Congressional vote against further federal funding of the SST in 1971 has been claimed as a noteworthy example. But arguing primarily on only environmental grounds helps lay the basis for a new and more powerful sanction by science. Powerful interests have the ability to hire scientists, to prepare environmental impact statements and to make the necessary token or substantial environmental concessions to achieve their ends. Even cleaning up the environment quite a bit is no fundamental challenge to privately controlled industry in the long term. And there is no guarantee that a clean environment will lead to a more just and equal society.

This is not to say that present activities done in systems of capitalism and bureaucratic socialism do not have a tremendous adverse effect on the environment. There are many horror stories to show that they do. But these do not prove that capitalism cannot adjust so as to operate in some publicly acceptable harmony with the environment. After all, many problems due to technology can be alleviated or removed if enough time, effort and money is devoted to them. But big business and government will not start doing this until it is to their benefit to do so, or in other words until they have to. The powers in control of the design and operation of centralised technology have the money and expertise to solve the problems they want to solve when they want to solve them and in the way they want to solve them. If the structure of society remains relatively unchanged, cleaning up the environment will occur when and how it is most advantageous to vested interests. To argue on environmental grounds is not to change the locus of power in society, unless environmental demands are closely linked with social, political and economic changes in society.

It is inevitable that industry and government will use environmental issues to their own advantage. The selective useability of scientific knowledge is a big advantage to them in any debate on this level. Another advantage to them in using environmental arguments is that the values on which their actions are based are made less obvious. In arguing on economic and political grounds the clash of values is relatively overt. When the arguments are channelled into narrow and specialised scientific controversies, values are less obvious. And when the planning, the presentation of choices and the criteria for making a decision are made by vested interests, this implicitness of values will weigh heavily against those who oppose the vested interests.

The idea that scientific opinions and reports are used to justify a decision made on other grounds depends somewhat on the assumption that the scientific opinions and reports do not actually carry much weight in the decision. My impression is that this is very often the case. For example, in a study of the decision-making process in the issue of whether to install an anti-ballistic missile system in the U.S., a majority of scientists surveyed thought that scientists had some influence in the issue. However, none of 70 knowledgeable non-scientists surveyed attributed *any* influence to scientists or scientific panels in the basic decision. Scientific opinion probably has less influence on policy decisions than most scientists would like to admit. Partly this is because scientific reports and opinions are often couched in a tentative, non-commital manner. Strong, unequivocal actions are seldom advocated, especially when groups of scientists get together to draft a report. The result is that the reports and opinions of scientists are easily used to support differing policies and actions. And the most powerful groups in

society are the ones who can most easily use this situation for their own ends.

Comment

I am more ambivalent about the uses of environmental arguments than the foregoing spiel would suggest. It may be worth spelling out my opinion.

I do think it is legitimate to use environmental arguments to oppose development projects. What should not be legitimate is the automatic bias in favour of vested interests that pervades decision-making at the political and economic level. When developers decry 'uninformed' public opposition and ask for an argument on 'rational' grounds, they are really asking that the debate be conducted in terms that are strongly weighted in their favour. To decry exaggeration in environmental arguments is to divert attention away from the prevention of real debate on political and economic issues. I have no objection to debates involving environmental issues. But I feel it should be remembered that the ultimate debate should be as well on social, political and economic issues.

Reference notes

For excellent critiques of cost-benefit analysis, see Adams (1971), Self (1975) and Lovins (1977). The example of the swimming pool I have taken from Neutze (1972).

Blackburn (1972), Pateman (1972) and Roszak (1967) are excellent introductions to the way social sciences are used to legitimise the status quo. In most disciplines this legitimation no longer comes from crude rationalisations for existing power structures but instead results from sophisticated theories that arrest critical social theory at an elementary level. Also important is the promotion of the myth of value-free inquiry as a cover for accepting existing values inherent in the social structure.

For accounts of the political machinations involved in the planning and development of the SST, see Costello and Hughes (1971) for the pro-SST side and Wilson (1973) for the anti-SST side. Other accounts which tell more about the scientific conflicts in the SST debate are found in Primack and von Hippel (1974), Boffey (1975) and Schneider (1976). The answer to the question of what groups actually promoted the SST for their own purposes depends on one's background and perspective. For example, in a book demonstrating the empire-building tendencies of the U.S. Department of Defense, Melman (1970, p. 82) emphasises the importance of the DOD in sponsoring the SST; the environmentalist Commoner (1971) emphasises environmental opposition; and in a similar manner other accounts reflect the background of the authors.

Arguments in favour of citizen participation in the urban planning process are usefully discussed by Loveday (1972). The actual practice of planning, and the absence of real participation in spite of fine words, are discussed by many authors. Particularly good is Goodman (1971).

Arguments in favour of the funding of the U.S. SST prototypes are presented by Seitz (1972), many of them in a quite explicit form. The idea that unless stopped early, political and economic forces behind the development of SSTs could not be restrained is presented by Maddox (1972), Costello and Hughes (1971), as well as many anti-SST authors.

Rapoport (1974) claims that in the fluoridation controversy, the real issue was the autonomy of local people to make decisions about their own lives. But the actual level on which the controversy was carried out was scientific, presumably because this was the only available way for people to express their needs for autonomy. Barzun (1964) also points out the essential moral and political issues involved in fluoridation.

Hirsch (1968) discusses how scientific advisors may be used by decision-makers to rationalise the decision-makers' perception and interpretation of events. The advisors may be chosen, whether consciously or unconsciously, because of their known views, and so that they can be used to give a stamp of approval on previously formulated decisions. Nieburg (1966, p. 125) also describes how this politicising of the expertise of the scientist takes place, while Perl (1971) gives a negative evaluation of the influence of the scientific advisory system on broad technical decisions. In many cases the opinions or reports of scientists or scientific advisory groups are used to justify a decision already made for other reasons (King and Melanson, 1972).

There are a number of case studies of this phenomenon. One of the best examples centres around the decision to drop the atomic bomb (Haberer, 1969). Another is the use of war theory to legitimise whatever crude military strategy has been decided upon (Horowitz, 1970). Turner (1970, chapter 9) finds that much laboratory research by the U.S. Federal Drug Administration serves primarily as window dressing for administrative decisions and positions previously arrived at. It is certainly possible to see the CIAP report (Grobecker et al., 1974) and other reports on the environmental impacts of SSTs in this light. Significantly, the British Aircraft Corporation (1972) has, unlike SST opponents, found it valuable to quote from the Australian Academy of Science report (1972).

Primack and von Hippel (1974) document a number of ways in which the U.S. scientific advisory system is used to serve the purposes of top U.S. political and economic interests. The administration may release primarily the information and analyses of scientific advisors that support the administration position (the SST); government officials may publicly misrepresent confidential advice (the ABM debate); scientific studies may be commissioned as an excuse for administrative inaction (on pesticides) (i.e., in the presence of disagreement between experts, the burden of proof lies on those who would challenge the existing policy); and committees of 'experts' may be drawn up to give a political opinion the appearance of technical legitimacy (cyclamates).

Boffey (1975) gives detailed examples of how scientific committees of the U.S. National Academy of Sciences are used or even set up to justify existing or planned policies or actions. In covering such cases as radioactive waste disposal, the SST, defoliation and pesticides, Boffey finds a general inertia on the part of the Academy which he principally ascribes to its "orientation towards serving rich and powerful institutions that do not generally encourage activism on behalf of reform".

Morgenthau (1972) states in several ways how science is used to justify the purposes of the powers that be. For example, he claims that while the scientific establishment continues to discover truths which are either useful or irrelevant but not threatening to the powers that be, it has lost the ability to distinguish what it is and is not important to know by any standard that is not acceptable to the powers that be.

My statement that cleaning up the environment is possible within present societal structures is highly debatable. I expand on some of the issues involved in Martin (1979).

The survey of attitudes and activities of U.S. scientists about the ABM issue is reported in Cahn (1974). She also notes that a scientist's perception of 'facts' about the ABM and about the reasons behind decisions made about it, depend on whether the scientist is for or against the ABM. Similarly, it is fairly obvious that the 'facts' about the environmental impact of the SST depend on the judgemental and perceptual spectacles through which one views the introduction of the SST.

Chapter 12: What is scientific knowledge?

Scientific knowledge is generated to serve certain purposes. I have been arguing that most scientific knowledge is selectively useful to the more powerful groups in society. This selective usefulness of scientific knowledge may be looked at in several ways: that the scientific research which is done is conditioned by economic and political considerations, for example through the allocation of government funds; that scientific knowledge is selectively useful to different groups depending on its accessibility, understandability, and exploitability; and that scientific knowledge may lend itself to being used to justify decisions made on other grounds.

It is easiest to understand the selective usefulness of scientific knowledge on the level of *what* scientific research is done. For example, it is easy to see that a research emphasis on nuclear physics is selectively useful to groups promoting nuclear power or promoting a state of permanent crisis through a policy of nuclear deterrence; in contrast, an emphasis on the ecology of low technology agriculture is selectively useful to groups favouring decentralisation and living in harmony with the environment. It is not so easy to see that scientific knowledge in itself — through its content, structure and organisation — may be selectively useful to certain groups. I contend that: (1) each bit of scientific knowledge is always inherently suited more for some purposes than for others, and (2) the purposes for which scientific knowledge is inherently suited are, more often than chance would indicate, the purposes of the groups in society which have the greatest power over the direction of scientific research and which have the greatest power to use the results of scientific research.

In arguing in favour of these ideas, I will present a number of perspectives, analogies, and illustrations. Naturally this is not an attempt to *prove* the validity of these ideas. Rather, I am just trying to express some of the ways in which I have found it useful to think about science.

The arbitrariness of scientific knowledge

Scientific knowledge is never unique: there is always a large number of ways of explaining any feature of reality. The actual way of explaining reality that is chosen or latched upon by a person or group will be suited more for some purposes and less for others. Because it is somewhat arbitrary, the actual content of scientific knowledge is in one sense socially defined. It is not the *only* way of viewing or understanding reality but *a* way that has been chosen or that has developed on the basis of human needs, desires and purposes.

Let me draw a simple analogy. There is an infinite number of ways of theoretically characterising a set of data points, ranging from straight lines and S-shaped curves to sophisticated models of various sorts. Some of these ways will seem to make more sense than others. They may be simpler or may better explain certain other data. It is likely though that even with a number of limiting criteria, the number of possible ways of characterising the data points will remain sizeable. And even then, the criteria used to select among explanations are themselves the product of some hypothesis or assumption. In other words, somehow one must decide which criteria for selecting explanations are to be used. Decisions about which explanations to use or which criteria to use will reflect the values of the people who make them, though not usually in a direct way.

Scientific knowledge of course is built up from more than just decisions about explaining data points. But in the same manner as values affect these decisions, values enter into the construction and validation of scientific knowledge. Because it is built on human choices, scientific knowledge will always incorporate values. At the same time, this scientific knowledge *could* be replaced or supplemented by different scientific knowledge, which incorporates different values.

An example of the arbitrariness of scientific knowledge lies in the formulation and interpretation of quantum theory. At least four levels in this arbitrariness can be specified: the selection of certain aspects of reality for attention and mathematical description; the designation of a specific mathematical model for the phenomena selected; the way in which the mathematical description is formulated; and the interpretation of the mathematics.

First, quantum theory is based on a selection of certain statistical regularities in atomic phenomena for attention. By this focusing of attention many aspects of reality are automatically excluded or transformed. For example, unique or rare events are ignored by design. Built into the mathematics of quantum theory, therefore, is a partial and biased view towards what nature is like.

Second, different mathematical models can be developed to describe and explain the quantum phenomena selected for attention. Most of these models will be found inadequate in the context of other current scientific knowledge, but there still may be several that could be used as a basis for further testing and elaboration. The choice of a particular model leads one to look for certain effects and ignore others.

Third, a given mathematical description of quantum phenomena may be expressed in a number of different formulations. Equations may be written in different forms and different quantities or parameters may be given prominence. This is similar to the different ways in which an idea may be expressed in words: the words and phrases used in any particular formulation will have different connotations. For any particular mathematical formulation, the association of certain ideas — such as probability, motion or cause — with certain mathematical expressions will be easy or difficult.

Finally, even for a single mathematical formulation, there are a number of different ways in which the mathematics may be characterised by ideas. One way in which quantum theory may be interpreted is based on the idea that the behaviour of individual particles is inherently indeterministic and that no further knowledge about particles can ever be obtained than provided by present quantum theory. An alternative interpretation is that quantum theory demonstrates only the behaviour of particles is indeterministic in a statistical sense, and that further knowledge may or may not be possible. (Actually, these four levels of arbitrariness are linked pretty strongly. For example, a deterministic interpretation typically will be linked with a mathematical formulation that highlights motions and particles rather than probability amplitudes.)

There is no question of scientific knowledge being wrong in the sense of having faulty mathematical derivations or incorrect logical deductions. Of course this may and does occur. What is significant is that there are a large number of phenomena available for explanation, a large number of possible theories for a given set of phenomena, a large number of possible faultless mathematical formulations of a given theory and a large number of interpretations of a given formulation. At each stage scientific knowledge will become biased in favour of certain uses.

I am saying that scientific knowledge *always* incorporates values, and hence *always* is selectively useful to certain groups. If this situation is inevitable there is no point in trying to change it. The attempt to develop value-free scientific knowledge only results in the obscuring or ignoring of the values implicit in the

knowledge. The object should be to develop and promote scientific knowledge that incorporates preferred values and which serves to promote desirable social and political structures.

I will try to illustrate the way scientific knowledge is selectively useful for certain purposes by considering: (1) the selection of features of the world to explain; (2) features of theories; and (3) the ease of practical application of scientific knowledge for different uses.

It is difficult in many cases to illustrate these features of scientific knowledge with illustrations from the SST-NO_x-ozone area or indeed from any part of physical science. The important biases of scientific knowledge enter in at a very fundamental level, often in terms of the criteria for what constitutes scientific knowledge at all. In the social sciences the biases of knowledge are more obvious to many. I will try to strike a balance between discussing abstract presuppositions underlying knowledge in the physical sciences and readily apparent presuppositions underlying knowledge in the social sciences.

Selection of features of the world to explain

Fundamental assumptions are involved in the choice of certain features of the world for study and explanation. These assumptions in many cases are strongly conditioned by the values of the investigator and of the society in which the knowledge is developed, disseminated and used.

First consider knowledge about stratospheric ozone.

One of its immediate uses is promotion of the careers of individual scientists or of funding of atmospheric research. Scientific knowledge useful for this purpose typically is that which leads to new questions which can readily be tackled with existing or straightforwardly achieved techniques so that impressive results can be demonstrated to the rest of the scientific community or to funding agencies. The features of ozone most likely to be selected and found useful for such purposes are features that are readily measured and which lend themselves to incorporation into existing theories of the atmosphere. Examples are the measurement of the ozone column or the measurement of the rates of chemical reactions thought to be dominant in determining ozone levels.

Another feature of ozone is its sensitivity or lack of sensitivity to artificial inputs of trace substances such as NO_x or chlorine. The recent selection of these features for study by Johnston, Goldsmith et al. and many others reflects the recent widespread interest in environmental issues. At the same time, knowledge of the effect on ozone of such artificial inputs has selective usefulness. For example, the existence of knowledge about the sensitivity of ozone to artificial inputs of trace substances can be used to argue against SSTs or aerosol sprays or to focus attention on environmental rather than political issues.

A further feature of ozone is its significance in absorbing ultraviolet light and thereby protecting life on earth. People interested in studying the effect of large changes in ultraviolet on evolution will focus on ways in which ozone may be drastically reduced by natural phenomena. Likewise, a study of these possible drastic influences on ozone levels lends itself to studies of evolution and biological catastrophe. In the wider perspective, these evolutionary studies may be based on certain assumptions about human nature or human adaptability and have wide ideological implications or uses.

These features of ozone — measurability and usefulness in theories, sensitivity to artificial inputs and susceptibility to massive alterations — are just a few of the features that may be selected out for study and which have special uses. Naturally any given feature of ozone may have more than one use. My point is that knowledge of any particular feature is not equally useful for any purpose. And in conjunction with this, the different features are not equally likely to be chosen for study.

At a more fundamental level, only those features of reality that satisfy specified criteria tend to be selected for scientific study. Examples of such criteria are measurability and reproducibility. I believe that such specified criteria only in part determine what types of facts are generated. It is likely that often the choice of significant facts is made first, and that the criteria for selecting significant facts are only developed afterwards, in order to explain and justify this choice.

It is easiest to illustrate the potential political significance of this interaction from a social science. In behaviourist psychology, attention is focussed on what are considered to be the unambiguous observable aspects of the behaviour of organisms. Whatever is the justification for such a stricture on the method of inquiry, it certainly serves to select out certain aspects of reality for attention. This selection makes the resulting knowledge selectively useful for certain purposes. What these purposes are is a matter of debate. For example, it may be argued that knowledge primarily of behaviour is most fruitful in developing social strategies for attaining and maintaining an ideal society. It also may be argued that knowledge based on study only of observable behaviour is likely to lend itself to therapies or social practices, such as aversion therapy, which are manipulative, unfeeling, and which incorporate the moral assumptions of the surrounding society at the expense of developing individual capacities.

It is often claimed that the restriction of attention to observables in behaviourism is a consequence of decisions about the most appropriate method to be used in studying behaviour. But it can just as well be true that the choice of what aspects of behaviour were important was made partly on the basis of what social practices were likely to be justified by the choice and that reasons for the choice were only introduced to justify it.

Let me state this idea more generally. Certain purposes will be served if the range of scientific attention is limited in a certain way. Due to the structure of society and the current aims of scientific practice, scientists may find it natural to limit their attention appropriately. Then after the choices are made, principles may be drawn up to explain and justify the choices.

Fundamental choices about what part of reality to study also underlie physical science. My presupposition is that scientific knowledge about the universe is probably strongly conditioned by these choices, so much so that it is difficult to understand the implications of these choices without conceiving whole bodies of alternative scientific knowledge.

Theories

The previous section has been somewhat artificial in that I have talked about selection of features of the universe for attention and study, and about facts, without bringing in theories. Underlying any selection of areas of attention, and underlying any facts, are theoretical assumptions. The theoretical assumptions tell one what to look for and what is irrelevant, tell one the significance of observations, and define the meaning of the investigative enterprise. The theoretical assumptions can make the resulting scientific knowledge selectively useful to certain viewpoints and purposes and hence selectively useful to different groups in society. But even beyond this, the ideas and constructs in scientific theories — assumptions about the way the world is — may more directly lend themselves to specific uses in society. It is my conviction that the guiding ideas in scientific theories often arise because they lend themselves to certain specific uses.

There are many examples of the way the ideas in scientific theories lend themselves to particular uses in society. Perhaps the most well known is the way Darwin's formulation of the theory of evolution lent itself to the promotion and justification of social policies based on competition and ruthless exploitation. In cases such as this, there was more than one possible explanation or emphasis. Even in Darwin's time it

would have been possible to emphasise co-operation rather than competition in explaining evolution. The specific ideas and orientations of theories can be explained in part as a result of various influences, ranging from prevalent philosophical ideas to the economic organisation of society. Scientific knowledge always arises in a context in which certain priorities and orientations are paramount. So it is not surprising that scientific knowledge often lends itself to particular uses.

Of course scientific knowledge can be used for many purposes and its ideas can be used in support of many policies. But any particular bit of scientific knowledge can still be more useful for some purposes than for others.

A useful analogy between language and science can be made. It is generally accepted that in a particular language it is easier to express some ideas than others. The particular types of ideas that are easiest to express may reflect and support the social structure in which the language is used. It has also been argued that languages actually channel perceptions and thoughts. If we spoke a different language, what we perceive and how we think might be very different. From this viewpoint, language becomes more than a tool for expression; in thinking and communicating, the assumptions of the language are always expressed too. Language is analogous to mathematics and science in that it is a humanly constructed tool for probing and understanding the universe. Languages may evolve in different cultures to reflect and reinforce basic features of those cultures which aid their functioning and survival. Similarly, the development of mathematics and science may reflect and reinforce basic features of the surrounding culture which aid its functioning and survival. A difference is that languages based on divergent attitudes to reality have developed over many centuries, and still exist. This probably depended on a degree of cultural isolation. On the other hand, the rapid rise of Western science and its evangelistic imposition on all societies has not permitted alternative sciences to develop to the same extent — at least not yet!

Now for the SST-NO_x-ozone research area: in what way do theories in this area lend themselves to certain uses? Most of the selective usefulness of theoretical concepts in this area is perhaps due to their orientation towards solving specific types of problems for certain groups. As described earlier, some of these groups are scientists interested in comprehensive theories; others are administrators, business people or environmentalists interested in human impacts on the upper atmosphere. Here I will briefly describe a couple of features of theories of the stratosphere which may be linked indirectly with socially significant ideas about the environment.

Until the 1940's, it was generally believed that the stratosphere and the upper atmosphere in general was stable and non-active. In the 1940's some scientists began to try out the idea that there was considerable motion of air in the stratosphere. In part this motion was introduced in order to explain the observed features of the ozone distribution, such as the high levels of ozone towards the poles, where very little was produced. At first the motion was thought to be mainly due to slow but steady winds. Later, from the early 1960's, the idea of turbulent mixing was introduced. It would be possible to link the belief in a stable stratosphere with the belief or desire for a stable society. Those favouring a stable society — generally meaning the status quo — would tend to entertain first the idea that nature is uniform and non-violent. Likewise, the assumption of a stable stratosphere would lend itself, in a very small way, to promotion of a belief in a stable society. It might be argued that stability is the simplest hypothesis. But simplest in what regard? Stability is certainly not the simplest hypothesis in terms of observed features of ozone: large fluctuations in ozone have been observed since the 1920's. The origin and uses of the idea of a stable stratosphere is only one tiny part of the general conflict between uniformitarianism and catastrophism.

In recent years with the increase of awareness about the environment, there are at least two fundamental and divergently oriented principles used in perceiving the stratosphere. One of these sees the stratosphere, like other parts of the world-wide ecosystem, as being very sensitive to human impacts. From this perspective even a seemingly small change to the system may lead to significant consequences from which the natural system may not easily recover. The direct implication is that human impacts are generally undesirable, and in as much as they are necessary should be studied carefully, taking into account the widest possible range of influences. This perspective is to a large extent that of the environmental movement. Many of the ideas and approaches and results of SST-NO_x-ozone research reflect this perspective and lend themselves to it.

A second perspective sees the stratosphere and most other natural ecosystems as being self-regulating and self-cleansing. From this perspective, human perturbations are not likely to have much effect since natural systems are organised to maintain their essential structure and function in the presence of many large natural perturbations. The direct implication is that there is not much to worry about if human impacts are not large compared with natural impacts. Only if human construction of the environment is dominant, as in cities, is danger likely to occur. Again, many of the ideas and approaches and results of SST-NO_x-ozone research reflect this perspective and lend themselves to it.

The presupposition about the burden of proof is one facet of SST-NO_x-ozone research which reflects a basic orientation to the environment.

Ease of practical use

Scientific knowledge is selectively useful for different purposes. There are several features of scientific knowledge that contribute to this selective useability. I consider in turn understandability, instrumentality, philosophical presuppositions and implicit moral imperatives.

Understandability. Scientific knowledge is organised so that it is primarily understandable by experts. When this is the case, the scientific knowledge is selectively useable by experts and the organisations which can afford the services of the experts.

Consider a given feature of reality. There will be different ways in which knowledge about this feature of reality can be selected and organised. Different concepts may be used, emphasising different perspectives; different numbers of concepts may be used; different terms may be used; and different relationships between concepts may be used. My opinion is that in many cases in which alternative formulations of scientific knowledge are possible, scientific knowledge is specificallly designed to be understandable only by experts. This does not mean only that jargon is used. More important is that sophisticated, specialised concepts and relationships are introduced. The experts often have a great time playing around with the fancy concepts, and find them necessary for their specialised purposes. That is precisely the point. The scientific knowledge is designed for use by experts and not for ease of understanding by non-professionals.

Instrumentality. Much of scientific knowledge is selected and structured so that it can be used, with a minimum of trouble and exposure of values, for any purpose desired by those who have the power to use it.

Imagine two different ways in which scientific knowledge might be oriented. One is that the widest implications for nature, for humans and for society of any action will be readily apparent from the concepts, emphases and structure of scientific knowledge. The other is that only the local, direct consequences of any action will be readily apparent from the scientific knowledge.

An orientation towards the wider implications of an action in effect builds values into scientific knowledge. An emphasis on implications for humans builds in values related to the significance of human well-being. The other orientation, limiting the range of understanding that is emphasised, is not

value-free either. Rather, such scientific knowledge tacitly lends itself to the value of the society or in the context in which it is used. A lack of explicit concern for consequences for humans downgrades the significance of human well-being and serves the immediate ends of the group using the scientific knowledge.

The best example of a scientific discipline which builds in an awareness of the wider implications of human action is ecology, in particular human ecology. The significance of all features of nature for all other features of nature, and for human and cultural functioning, are incorporated into ecological concepts and their relationships. An example is the idea that changes in an ecosystem may have ramifications far beyond their immediate impact.

Most scientific disciplines fall towards the end of the spectrum that restricts attention to immediate consequences. An example is chemical analysis of air quality. The air in an urban area may be analysed regularly. One use of this analysis might be to decide whether children should be cautioned to stay inside and avoid strenuous exercise. Such a use builds in a narrowness of vision: there is no attention drawn to the sources of the air pollution and strategies for combating it. Another use of chemical analyses of air quality may be to decide where to put a factory so that air pollutants are dispersed as rapidly as possible. Such a use again builds in a narrowness of vision: it is never asked whether the goods being produced are socially useful or necessary or whether a different organisation of production is possible.

Scientific knowledge with a built in narrowness of focus can be called instrumental. Such knowledge is useful for choosing between different strategies. What it does not do is draw attention to the assumptions on which all the explicit strategies are based. The constraints within which decisions are made are not pointed out or challenged.

Herbert Marcuse argues that modern scientific knowledge is inherently instrumental. Scientific knowledge is so structured that it is easy for ruling groups to use it, within the context of existing society, for their own ends. The extent and significance of the instrumentality of scientific knowledge is fundamental and deeply important.

Do fundamental biases of scientific knowledge such as instrumentality really matter? It is hard for me to get a balanced view on this because these biases fascinate me intellectually — as they fascinate the philosophers who study them deeply. It seems plausible to me that in a fundamentally different society such biases might change dramatically, though not quickly. However, scientific knowledge is not going to be changed by philosophers but by changes in political and economic institutions resulting in basic changes in scientific practice and in the social uses of science. Still, it is useful to be aware of fundamental biases in scientific knowledge if only to realise the mystifying nature of such concepts as the 'neutrality' of science.

Philosophical presuppositions. There are several features of scientific knowledge which reflect basic philosophical attitudes. Most scientific knowledge is built on such assumptions as determinism (in spite of quantum theory), separation of subject and object, reductionism and measurability. Scientific knowledge built on such assumptions or interpreted using such assumptions selectively lends itself to certain uses in society. For example, take the idea that the universe is something 'out there', subject to human control and manipulation. This orientation reflects and reinforces a society in which physical and social science are used to manipulate 'other' people.

Such relationships — between philosophical features of scientific knowledge and the institutional structures in which scientific knowledge arises and is used — are rarely clear or direct. But they do exist, and there is no reason to believe they are insignificant. Of course, any given scientific knowledge can be interpreted in a number of ways and any given philosophical assumptions can be reflected in many different types and forms of scientific knowledge. But the relationships between scientific knowledge and philosophical attitudes and particular formations of scientific knowledge are suited for each other. For example, one would expect that a belief in determinism would influence the generation, selection and use of scientific theories, such as in psychology, even if this belief did not directly specify the theory developed.

I am not going to try to demonstrate the details of connections between philosophical attitudes inherent in scientific knowledge and the structure of society. Instead, I am just going to illustrate some scientific categories and their philosophical associations and make some general (and perhaps unjustified) remarks.

I assume that there is no such thing as an ultimate reality which is independent of the human observer and interpreter. Therefore, any particular part of scientific knowledge will be based on one or a number of basic assumptions concerning how reality is to be interpreted.

One of the chemical reactions used in Goldsmith et al.'s work (p. 546) is

$$N_2 + M \rightarrow N + N + M.$$

This reaction takes place at a rate which depends on the concentrations of N_2 and M, on the temperature and on other parameters. There are a number of categories involved in the concept of this chemical reaction and its rate, such as the ideas of molecule and atom, of temperature and of chemical combination. A typical assumption involved at this level is that the categories used here are useful for certain purposes, namely for determining the rate at which a nitrogen molecule N_2 upon interacting with an arbitrary molecule or atom M will be dissociated into two nitrogen atoms N+N. The purposes for which the concept of a chemical reaction is useful — in the wider world of applications of chemical theory — will tend to involve the same sorts of categories and assumptions as the concept of a chemical reaction. In other words, research involving chemical reactions in nuclear blasts, in air pollution, in food technology or in human physiology will implicitly incorporate the attitudes towards nature inherent in the category of a chemical reaction.

The concepts involved in the N_2+M chemical reaction are both based on and lead to the acceptance of certain orientations towards reality. What these orientations are exactly is a matter of some debate, so all I will do here is suggest a few possibilities.

The concept of 'chemical reaction' and related concepts are closely associated with a deterministic view of the universe. The occurrence of a reaction is seen as a consequence of certain prior factors, say the nearness of two molecules having appropriate energy and momentum and so forth. Alternative explanations, such as that the N_2 molecule 'wanted' to split, are denied. More important is the argument that goes roughly along these lines: the behaviour of atoms is deterministic and the world is made up of atoms, therefore all features of reality are deterministic. This argument is used to deny the reality of 'free will'. In its ramifications through other fields, such as law and medicine, it can be used to justify various policies and actions. Of these, it is *easier* to use determinist-based arguments to attack the freedoms of people than to defend them. The above determinist argument is not a necessary consequence of the idea of a chemical reaction. But it may still be true that attitudes towards nature and humans associated with determinism are encouraged by the normal concept of chemical reaction — and vice versa.

Associated with the idea of a chemical reaction is the assumption that reality may be described usefully in terms of certain objects such as atoms whose behaviour may be observed experimentally and described mathematically. Often associated with this assumption is the feeling that an experimental/mathematical interpretation of reality is the 'best' or even the only valid view of reality. If it is granted that there are different levels at which reality may be described (for example: historical, sociological, spiritual, physiological, chemical, physical), then it is often assumed that the most fundamental view or the closest approximation to 'reality' is the view closest

to the physics end of the spectrum of levels. Indeed, scientists often feel that an experimental/mathematical description of reality is identical with reality itself. The corollary of the belief in the fundamental nature of an experimental/mathematical description of nature is the idea that anything not contained in such a description is spurious. Phenomena such as psi (ESP, precognition) are ignored partly because they cannot yet be explained with a mathematically-based theory. More important are the social consequences of ideas such as that humans are 'nothing but' the atoms that compose them. Reductionist beliefs thus may be both partly a cause and partly a consequence of the idea of a chemical reaction, though the link between the two is far from being clear and direct.

Also associated with the idea of a chemical reaction is the separation of the observer and the observed. The reaction is implicitly interpreted as occurring independently of the observer; by certain techniques or procedures a scientist may become aware that the reaction has occurred. The idea of the separation of the observer and the observed is closely linked with the idea of an external reality independent of the observer. Reality is not thought of as something that may differ from observer to observer and which may depend on the observer's experiences, desires and role in society. This attitude nicely ties in with a perception of existing society — economic structures, psychological attitudes, patterns of human interaction — as being independent of the vested interests of the people who have power in the society. If the virtues and defects of society are independent of who is observing them, it is easy to perceive one's own interests as being identical with the interests of all people.

The orientations to reality implicit in scientific concepts are not necessarily the ones I have suggested here, nor are they the same in all circumstances or constant in time. For example, the particular orientations to reality I have noted are typical of classical physics. The introduction of quantum theory leads to the universe being seen as fundamentally indeterministic, with a different type of mathematical description (but still mechanistic, according to David Bohm). But whether one thinks in terms of molecules in a deterministic universe or of wave functions in an intrinsically indeterministic universe, scientific concepts do reflect certain orientations towards reality. And these orientations towards reality are linked, in complex and indirect way which includes both cause and effect, with social and political values.

Not only are the concepts used in science based on certain interpretations of reality, but they tend to foster these same interpretations. Thus, in grasping the idea and use of the concept of a chemical reaction, it is easier to think within a framework of determinism and of reality as a mathematical description than within many alternative frameworks. Consequently, as science develops, the interpretations of reality underlying it tend to perpetuate themselves.

Is and ought. A fundamental feature of all scientific knowledge is an ostensible concern only for the way things *are* or could be, as opposed to the way they *ought to be*. Typical scientific statements are of the form, "A holds under specified conditions", or "If you do B, C will follow". Scientists do not often explicitly argue that A is desirable, or that C ought to be avoided.

Of course scientists look at the possibilities of a changed world. There is much attention given to discovering new elements, synthesising new chemical substances and manipulating genes. But scientific knowledge does not distinguish explicitly between what is possible and what ought to be done.

From my perspective, to say something *is* is to say it *ought to be*. In other words, every scientific statement is implicitly an imperative. This idea makes more sense if it is accepted that there are innumerable perspectives on the world and that what is thought to be significant depends on the perspective adopted. By investigating what is or could be *from a single perspective* is to make a value judgement: that one ought to look at these things from this perspective. In this way all scientific knowledge can be seen to consist of value-laden moral imperatives.

For example, a moral imperative is contained in the statement that a fleet of 500 SSTs will lead to a 20% reduction in stratospheric ozone. The moral imperative lies in the selection of a particular perspective. The perspective for this sample statement includes such features as an emphasis on a large fleet of SSTs, an emphasis on environmental consequences of SSTs and lack of emphasis on methods of decision-making about SSTs. There are many other statements that could be made, each with a different perspective and a different moral imperative.

In order to incorporate values, scientific knowledge does not have to be wrong, incomplete or biased in any obvious sense. All it has to do is promote a particular perspective on reality.

Scientific knowledge as buildings

Often it is difficult to think of scientific knowledge as being oriented in one direction out of a range of possibilities, and of being selectively useful to certain groups. The reason for the difficulty is the abstractness of scientific knowledge and of the methods of scientific practice. Therefore I often find it useful to think of scientific knowledge in terms of an anology to a more concrete system. So here I describe one possible such analogy, an anology between science and the construction of buildings.

In this analogy, scientific knowledge is represented by physically constructed buildings; scientists are represented by construction workers, the people who build the buildings; and scientific techniques or tools, such as mathematics, are represented by construction tools. (Because the analogy is between abstract and concrete, the building side of the analogy does not include such aspects as architects or general methods for constructing buildings. The philosopher of science is closer to a building inspector than to an architect.) Various aspects of the analogy can be elaborated. The system of exchange of scientific information becomes exchange of tools; the socialisation of a scientist becomes apprenticeship as a builder; great scientific achievements become original or monumental buildings, perhaps serving as models for future construction. While these elaborations are interesting, the analogy is most useful in showing how values can be built into scientific knowledge and in suggesting how scientific knowledge might be different.

The question, "who does scientific research?" becomes "who builds buildings?" It is by no means necessary that the trade be restricted to a few or that specialisation in work roles be extensive. Naturally not everyone can be a jack-of-all-trades or a champion brick-layer. But skills certainly could be greatly deprofessionalised, enabling each person to be involved in some non-trivial capacity. On the other hand, given a fairly tight control over selection of apprentices and their training, professionalisation and specialisation not only becomes difficult to challenge but also seemingly necessary and inevitable. The natural further development of professionalisation and specialisation is a change in the type of buildings constructed and the manner of their construction, so that only specialists are capable of building them. Instead of developing generalised tools and building materials, and encouraging the development of the ability of people to construct the buildings *they* want (singly or collectively), the use of tools and materials which can be handled only by specialists is fostered. For example, instead of buildings being constructed so that they can be easily changed by the user to suit the user, they are constructed to that major change requires massive expense, demolition experts and permission by numerous authorities. In this way the certified builders, and the people who benefit from exclusion of the public from construction, maintain a hold on what is built. This corresponds to the way that the

presuppositions underlying the answer to the question, "who does scientific research?" can directly affect the content and organisation of scientific knowledge.

Other presuppositions can be looked at similarly. The question, "why is scientific research done?" becomes "why is a particular type of building constructed?". Here it becomes necessary to look at who needs buildings, who pays for them and what the buildings are used for. The individual builder may only be interested in a solid construction, in being satisfied with doing a job well. But the innocuous motives of the builder are quite compatible with an influence by outside forces on decisions about what buildings are to be constructed. For example, glamorous buildings may be constructed to house the rich or to demonstrate the importance of the organisations that occupy them. In principle, anyone can live in a mansion. In practice, only those who are powerful or rich actually do live in mansions. The mansion lends itself to specific uses by specific types of people. This is similar to the presuppositions underlying the standard answer to the question, "who can use scientific research most easily?" Just as something besides a mansion could have been built, so scientific knowledge might be fundamentally different so as to lend itself to use for different purposes and by different people.

To reiterate: buildings are built in a given social context. Who can use them is not usually determined by the intentions of the builder but is determined by social processes which favour rich, powerful and prestigious groups. Finally, a building can be built in many ways, and indeed the whole development of the construction industry can follow different paths. Suburban housing favours a social structure based on private ownership and nuclear families, and vice versa: both the form of housing and the associated social structure might be very different.

The traditional perspectives on science can also be illustrated in the analogy. The claim that scientific knowledge is real and factual, unaffected by social factors, corresponds to the buildings being real and not just an illusion, existing just the same whoever paid for their construction. The social purposes by which deliberate falsification of data or cheating in science are minimised might correspond to similar processes acting against builders who purposely construct obviously defective buildings. In my opinion, these are not the real issues in science, or in building. The sort of question to be asked is, might not scientific knowledge or buildings be different from what they are? Might they be constructed to serve different social purposes? Whether or not scientific knowledge is 'right' — whether buildings are solidly constructed — is beside the point.

This analogy is meant only to help you and me to think about the way science is and the way it could be. I do not claim that there is a detailed correspondence between science and the construction industry. One possible defect of the analogy, for the purposes I suggest, is that you may not normally think of the construction industry as something that could be different, that benefits certain groups and so forth. This problem leads back to the general problem of imagining alternatives. You may find other analogies more useful, such as with transportation systems, education, sport or religion.

Objectivity

Scientific knowledge often is said to be objective. Furthermore, often it is said that scientific knowledge is the only type of knowledge that is objective. There are many different meanings of the idea of objectivity: it may mean that the investigator is emotionally uninvolved in the object being studied; it may refer to ideas about reality for which the explanatory pattern can be indefinitely filled in and extended; it may be considered to apply to scientific measurements that can be reproduced; it may apply to scientific statements that can be subjected to criticism; or it may refer to lack of a teleological principle in an explanation.

Actual scientific knowledge is objective in some of these senses and not in others. I do not want to argue on the level of whether scientific knowledge is or is not objective. Rather, I would like to question whether the idea of objectivity is an appropriate one at all. To me, one of the most important features of scientific knowledge is its built-in selective usefulness for certain purposes, in particular the purposes of certain powerful groups in society.

If I were to use a concept of objectivity — which I don't — I would use it to refer to actual knowledge being in no way a special sample out of all possible knowledges. In terms of the building analogy, objectivity would refer to actual buildings being in no way a special sample out of all possible sets of buildings. But this is impossible. Knowledge will always be oriented towards human needs and human purposes. According to this concept, scientific knowledge would always be non-objective.

I am not against the other meanings of objectivity. What concerns me is the selective usefulness of scientific knowledge: the values embedded in scientific knowledge. Emphasising concepts such as objectivity that do not focus on the selective usefulness of scientific knowledge serves to draw attention away from the political implications of scientific knowledge.

Summary

I have both assumed and argued that many features of scientific knowledge are contingent on human decisions and human society. The reality of the world does not lead inexorably to any uniquely specified scientific knowledge. The comprehension and explanation of reality must always be filtered through humans, through their senses and their methods of thinking and interacting with the world. And in this filtering process, the features of human society and human relationships affect the selection, organisation and use of scientific knowledge.

Reference notes

The idea that knowledge is conditioned by the society in which it is generated and used is a standard one in the sociology of knowledge: see Mannheim (1936), Schutz (1962), Berger and Luckmann (1966) and Gurvitch (1971). A less theoretical approach to the social construction of reality is found in Douglas (1973). Barnes (1974, 1977) argues that it is useful to regard ideas as tools which social groups may use for their own purposes. However, the fact that certain ideas are useful to certain groups is not usually due to a direct relationship between the idea and the group; typically there are intervening and connecting factors. In my statements that scientific knowledge could be different, I have found it useful to keep in mind Barnes' definition of scientific knowledge as beliefs accepted by the scientific community and to remember that accepted beliefs are not necessarily 'correct' beliefs. For my purposes, the idea of the usefulness of scientific knowledge is more useful than the idea of the truth of scientific knowledge.

A detailed analysis of the way scientific knowledge is related to social uses and social structures is given by Young (1971, 1973). Earlier theorists such as Mannheim did not carry their analyses of knowledge into scientific knowledge; Young explicitly does this. Furthermore, he shows with concrete studies of evolutionary biology how such an analysis applies in practice. Young's studies are a gold mine for anyone interested in ideology and science, both in suggesting idea and methods for studying science and society, and in providing a wealth of references. In particular, he refers (1973) extensively to studies done from a Marxist perspective.

I have treated the relation between knowledge and society in the case of a particular mathematical theory in Martin (1978).

The inevitability of values in scientific knowledge can be approached in several ways: from the idea that facts are always theory-laden and theories always value-laden (Feyerabend,

1975; Young, 1971); from the necessity to make choices in the selection of problems, in the meaning of events and in the choice of scientific explanations (Kaplan, 1964); from the idea that the medium is the message (McLuhan, 1964; see especially p. 11) — in the case of scientific knowledge, the media of communication embody such features as selective understandability; from the selective funding of scientific research and the resulting development of certain types of paradigms (Rose and Rose, 1974; Rose and Rose, 1971; Marcuse, 1967); from the political and institutional context in which research is done and used (Salomon, 1973); and through examining the relationship of knowledge and action in actual societies (Habermas, 1974; Schroyer, 1973). To have personal meaning, these ideas need to be applied to one's own experiences in and with science.

Jung (1955, pp. 8, 49-50) points out how the experimental method in science automatically rules out consideration of rare or unique events, and that in general nature only answers the questions that are put to it. Some of the interpretations of quantum theory are: the orthodox or Copenhagen interpretation (Bohr, 1935, 1949); the statistical interpretation (Einstein, 1949; Ballentine, 1970); the hidden variable (determinist) interpretation (Bohm, 1952); the 'realist' interpretation (Landé, 1965, 1969); and the splitting universe interpretation (DeWitt, 1970). The mathematics associated with the different interpretations is usually formulated in different manners; see especially Bohm's work. In some cases the mathematical theories associated with the different interpretations can be different in their predictions as well as in their philosophical implications. (What I call interpretations might be called theories by some, or might not be distinguished by others.) The actual situation is not as clear as a list might suggest. The area of interpretations of quantum theory is so controversial that even the claim that there are alternative interpretations has been challenged. For an insight into the social factors influencing the initial choice of the orthodox interpretations, see Forman (1971). The mechanistic nature of the conventional interpretation of quantum theory is argued by Bohm (1957).

To speak only of *scientific* knowledge is, of course, narrow. Val Brown has kindly suggested the following references for some non-scientific ways of knowing: esthetic — Read (1966) and Langer (1953); spiritual — Otto (1952); interpersonal — Buber (1947, 1970). An exciting account of a direct encounter with an alternative way of knowing the world — interpreting it and interacting with it — is given in the books by Castaneda (1968, 1971, 1972, 1975) on his experiences with the Yaqui Indian sorcerer, don Juan.

Ratner and Walker (1972), Ruderman (1974) and Reid et al. (1976) are studies based on an interest in ozone levels, ultraviolet radiation and evolution.

For a statement on the behaviourist position, see the works of B. F. Skinner. Some alternative approaches are humanist psychology (Abraham Maslow, Carl Rogers) and critical theory (Herbert Marcuse, Russell Jacoby).

For an analysis of the social and political aspects of the origin and use of the Darwinian theory of evolution, see Young (1973) and the many references therein. Dubos (1968) notes that the early emphasis on competition in evolution and the attention given to co-operation later in the 20th century can best be explained by noting the prevailing social, economic and intellectual climate at each time.

For studying the selective expressibility of ideas in different langauges, and the assumptions built into languages, see Whorf (1956) and Hoijer (1954). Hanson (1958) and other philosophers of science emphasise that facts may be moulded by the logical forms of the fact-stating language.

Among the first studies to challenge the idea of a stable stratosphere were Wulf (1942) and Brewer (1949). The introduction of the idea of turbulent mixing began with Feely and Spar (1960). Evidence for sizeable and dramatic variations in ozone was available as early as 1927 (Dobson et al., 1927). A link between preferences for uniformity and stability in society and in describing the world may be found in areas besides explaining the stratosphere, such as the functionalist approach in sociology. The attraction of the idea of stability and equilibrium in economics is commented upon by Robinson (1964, pp. 77-78).

The attitude that the ecosystem is delicately balanced and sensitive to human perturbations is found throughout such works as Ehrlich and Ehrlich (1970), Commoner (1971) and Bryson (1974). The attitude that the stratosphere like other ecosystems is self-regulating and resistant to perturbations is expressed by Scorer (for example, 1973) and Beckerman (1974).

Douglas (1972) discusses in detail the way that particular ideas of nature and perceptions of the environment may sustain a particular social pattern and be sustained by it. This essay is an extremely important one for understanding the environmental debate and its links with scientific knowledge. My discussion of the different attitudes towards human impacts on the environment may be considered as an illustration of the more general perspective presented by Douglas on the environment. Indeed, her discussion is a model for analyses of the relation of social institutions to general orientations of the scientific enterprise. The ideological aspects of different approaches to the ecosystem are also treated by Biggins (1976).

Marcuse (1964, chapter 5; 1965) argues that modern scientific knowledge and the modern scientific approach to understanding nature are inherently instrumental, and that this instrumental science is based on a specific historically-based way of experiencing, interpreting, organising and changing the world. The instrumentalist rationality of science and technology is harnessed to the service of a society which simultaneously exploits the populace and shapes and satisfies its needs. What is needed is a new attitude towards nature. Rather than being treated aggressively for purposes of domination, nature could become a subject, a potential field for liberation. On this topic see also Marcuse's other writings (1956, 1969, 1972). Whether or not Marcuse's specific ideas about instrumentality and nature are useful, his general perspective — especially in raising an awareness of what could be, and how our thoughts and actions are infected by the society in which we live and which continually denies the validity of alternatives — has profound implications for a study of science.

Ellul (1965) gives a very perceptive account of the instrumentality of modern science and its implications in promoting the interests of the state over those of the individual. Ellul sees the development of scientific techniques as a relatively autonomous affair, whereas others such as Dickson relate this development to the control over decisions affecting technology by dominant social groups.

Habermas (1971, 1974) presents a deep philosophical critique of science and society. Science and technology are prime examples of systems of purposive-rational action; their rationality permits a choice between strategies while removing the social framework in which decisions are made from the scope of reflection. The form of scientific understanding originally reflected the repressive society which it served; now the tendency is for this form of rationality to engulf all institutions. What is needed is a form of understanding rooted in the knowledge and interaction of people freed of domination. Like Marcuse, Habermas' insights and ideas are invaluable for a deep study of scientific knowledge. A good critique of Habermas' ideas on science is found in Barnes (1977).

By reading Marcuse and Habermas, one can better realise the inadequacy of analysing science and society on the basis of the way things are and the way most people think they are. Considerable change in society will be required before we even have the opportunity to glimpse what is possible.

Schroyer (1971, 1973) and Salomon (1973) are other important treatments of the selective usefulness of scientific knowledge to powerful groups in society. Schroyer restates and

develops the ideas of Habermas and other critical philosophers. His work is especially useful in providing many references for a further study of the critical theory of society. Salomon approaches the instrumentality of science through a discussion of science policy, and as such is one of the few decent books treating science policy. The political nature of science arises from its social context; scientific knowledge cannot be dissociated from the function it fulfills or the power it wields as a political tool.

Burtt (1925; see also 1957, 1967) analyses historically the way the description of ultimate reality changed with the origins of modern science, particularly in the work of Newton. For example, the world as consisting of substances, possessing as many ultimate qualities as were experienced by humans, was transformed into a world of atoms whose characteristics are described mathematically. For Burtt, the importance of this transformation lies in the restriction on modern thought that follows from unconscious acceptance of the scientific characterisation of the world. He considers that the difficulty experienced by modern philosophers in reinstating humankind in the universe at least partly stems from use of scientific terms and concepts that contain within them an implicit placing of humans in the universe.

On the attitude towards nature implicit in science see also White (1968, chapters 5 and 6) who traces the ecological crisis to the exploitive attitude towards the environment inherited from Christianity, Skolimowski (1973) who argues that modern technology as an instrument designed to control nature is the product of a more recent metaphysics, and Leiss (1972) who traces the links between the idea of domination of nature by humans and the actual domination of humans by humans, as well as ideas used to justify this latter mastery. On these points see also Lewis (1943, chapter 3).

Another important characteristic of modern scientific thought is its emphasis on concepts which are discrete and nonoverlapping. Awareness of the difference between 'arithmomorphic' concepts and 'dialectical' concepts in science was first expressed by Georgescu-Roegen (1971). The importance of this difference for the social organisation of science is brought out by Whitley (1977).

Various other authors have studied fundamental shortcomings in the scientific approach and in scientific knowledge, and their implications for society. Whitehead (1926) discerns in science such features as anti-rationality and materialism; he argues for a more holistic approach to the study of nature. Whitehead repeatedly emphasises the need to study the assumptions underlying our thinking, especially those assumptions which are common to all variant systems in an epoch. Gellner (1973) presents four ways in which modern thought differs from the 'savage' mentality. The distinctions are thought-provoking, and help one to understand how world views radically different from the modern one are possible. Pirsig (1974) argues that the problems arising from technology are due to the traditional dualism of object and subject and furthermore that this dualism resulted from a removal of Quality (the original source of the dualism) from the world. The real excellence of Pirsig's work is in showing how the expression of philosophical insights can be woven into concrete human experiences. In this respect his book is an outstanding and brilliant exception to the general run of academic analyses. Mumford (1970) attacks the materialist world picture presented by science and gives many references to further studies on the subject. Mumford presents a diagnosis of human civilisation on the basis of an historical study of cultures and technology which includes numerous insights about science, its assumptions and its interactions with society.

Brown (1959) reinterprets Freud, and reinterprets history from a psychoanalytic point of view. In a study of rationality, money and the capitalist spirit, Brown also indicts scientific rationality. He believes that human life has been more and more organised around the incomplete and unconscious ideas governing the search for knowledge: namely the aim of possession or mastery over objects and the principle of economising in the means. Brown desires a new science based on an erotic relationship with nature.

Many conventional philosophical distinctions, such as between description and prescription, allow the ethical judgements inherent in scientific knowledge to be ignored or denied. Feyerabend (1965, 1970, 1970a, 1975) in attacking such distinctions draws attention to the values inherent in scientific knowledge and the scientific world view. Churchman (1971) argues that 'is' is a convenient expression for 'ought', blocking out uncertainty, and that the imperative 'is' incorporates an ethical judgement, so that accepting the imperative amounts to accepting a certain strategy or tactic.

The philosophy of science contains quite a few different definitions of objectivity. Philosophers are very concerned with this idea since if science can be shown to be objective in some sense then it is somehow justified. Just as good as any of the perspectives on objectivity is that of McLuhan and Nevitt (1972, p. 144); to them objectivity is an artifact of the visual mode of perception. An excellent exposure of the political implications of the usual concept of objectivity is given by Poole (1972). Poole finds that there are three major structures to objectivity (pp. 44ff): a firm attachment to knowing the way things are rather than the way they should be; a refusal by the leaders of the status quo to publicly justify their decisions and acts, so that the burden of proof lies with those who challenge the status quo; and a taking into account of quantifiable parts rather than the whole. These inbuilt features of objectivity discerned by Poole relate closely to my analysis of features of scientific knowledge. Elliott and Elliott (1976, pp. 92-95) also find that the idea of objectivity is part of an ideology which serves the interests of the status quo.

Chapter 13: Who does scientific research?

Who does scientific research? The answer, by definition, is scientists.

But beyond the definition, almost all modern-day scientists are professionals employed full-time by large organisations. The very concept of the scientist today reflects socialisation into a role, occupational professionalisation, a high degree of specialisation and a strong division of labour in society. Current science is structurally based on the assumption that this must be the case.

In theory, anyone is entitled to do scientific research. In

practice, only a few people are given the opportunity of doing science. These people must have undergone a lengthy and rigorous training. They must have been certified, for example by obtaining degrees or favourable recommendations from superiors. Even of those who have been certified, only a few are actively supported in the pursuit of science, namely by being given jobs, research grants, access to facilities and publication channels, interest and co-operation, and recognition. These few almost entirely consist of people who have conformed to the externally imposed requirements of their training, conformed to the expectations of superiors and colleagues in getting the opportunity to do research, and continue to conform to the currently accepted standards and requirements of the scientific establishment.

Look at the situation this way. A person may choose at an early stage in life to become a scientist. To get proper certification, the person must spend starting from the first science courses roughly ten years studying specified materials, in specified ways and in a specified sequence. The person must satisfy certain regular assessment procedures. On achieving sufficient certification, the person may begin to practice science, as a Ph.D. student or as a full or part-time researcher. To obtain and remain in a position where one is paid to do research, is given ample facilities and control over the work of others, it is usually necessary to do research on accepted topics according to accepted methods. To get into a powerful research position where one supervises the research of a group, or makes decisions about what research should be funded, or helps decide what research institutions should be founded, requires success at research according to the standard of the rest of the scientific community.

In contrast, the person who does not submit to years of externally directed discipline and who does not adapt to current research practices is very unlikely to be in a position to do much research. People entering research work from outside the normal channels are unlikely ever to be able to command the same research authority or the same amount of research funds as are the certified professional scientists. Also, researchers who by their choice of problems or type of solutions upset or antagonise their immediate superiors, the leaders of the scientific community or grant-giving bodies, are unlikely to continue to command an important authority in research. These last two statements are very mild. In actual fact, outsiders trying to enter scientific research have very little chance of success whatever their ability or the power of their ideas, while the penalty for promotion of unorthodox ideas is often a severe loss of influence. All the jobs, research funds, recognition and encouragement serve to channel selected people into positions where research and control of the research of others is a primary activity, and to make it very difficult for any other people to do much research at all. In short, social pressures and structures determine that doing scientific research requires years of specified preparation and a continuing major commitment of time, energy and belief.

It is not so much a question of there being particular researchers who are not supported because of lack of a proper background or nonconformity to the current norms of science. Certainly this does occur. But more important is the fact that there are not many people who did not pass through the proper institutional channels who even *try* to do science. Given the rigid social determination of who shall do science, it is part of a self-fulfilling prophecy to find that members of the general public are unable to do and undesirous of doing scientific research.

Let me illustrate this last perspective with an analogy with exercise. No one is actively stopping a person from getting an amount of exercise sufficient to improve health and induce a feeling of well-being. But society is so structured — with roads and automobiles, automated production of foods and goods, labour-saving devices and an ethos that looks on manual labour as degrading — as to make it very difficult and more importantly to make it unlikely that people would want to exercise. Similarly, no one is actively stopping a person from getting involved in scientific research. But the scientific community and the society are so structured — with lengthy training, specialisation, a sharp division of labour and a general feeling of disapproval or contempt for the nonprofessional or dabbler — as to make it difficult and more importantly to make it unlikely that people would want to study science or do scientific research.

What are the values contained in the conventional answer to the question of who will do scientific research? Some of them are: that the best interests of society are promoted by the existence of scientific 'experts'; that society should be organised hierarchically; and that people towards the top of the hierarchy are and should be in a position to decide what is best for other people. Stated in another way, these values deny the importance of the involvement of the populace as a whole in the practice of science, in deciding what research should be done and in controlling the applications of science. The values contained in the conventional answer to the question of who will do scientific research emphasise the importance of achieving technological ends in a short time, the criteria for such ends being decided upon by experts and powerful elites. Many of the values contained in the answer to the question of who will do scientific research are bound up with the values in the answer to the question of what will society be like.

Most scientists feel that science should be done only by professional, specialist experts. They recoil at the notion that the general public has any place in the practice of science — in participating in it or in deciding its direction in any detailed way. This value judgement is indirectly manifested in a number of ways: in the general denigration of the teaching role and elevation of the research role; in the distrust of generalisers and non-specialists, especially those bridging existing disciplinary boundaries; in the glorification of fundamental research, and condescension towards 'practical' problems; and in the scientist's contempt for amateurs, who are called 'cranks' and ignored, shunned or ridiculed.

The conventional, 'obvious' answer to the question of who will do science is by no means the only possible one. Given different value presuppositions a different answer is forthcoming. In a society in which many more people were involved in science, present-day specialised scientists would not be so lavishly supported, consulted and admired, if they existed at all. It is hard for a present-day scientist to imagine such an alternative society, precisely because scientists of the present-day type would not be in it. In this sense, the answer to the question "who will do scientific research?" is built into the very idea of present-day science.

Reference notes

The possibility that science could be deprofessionalised is treated in part V.

I have addressed the implications of the response of scientists to 'cranks' in Martin (1978a).

The professional nature of scientific research is fairly common knowledge. But the undemocratic nature of the scientific community is less well known to the public, though the extent of elitism and hierarchy in science is familiar to any scientist. Therefore it may be worth mentioning a few studies of this feature of science.

Blissett (1972) argues that a relatively small group of elite scientists — the scientific establishment — make the major decisions about what hypotheses are proposed and published, and what scientists are given awards, appointments and subsidies. Mulkay (1976) also documents the existence of a scientific elite. Ziman (1968) and Polanyi (1951) describe the elitist operation of science from the viewpoint of the elites; Gordon (1966) gives an outsider's viewpoint. Some of the sanctions applied to supporters of ideas unacceptable to the establishment are described by de Grazia (1966). Haberer

(1969) describes the general preference for closed decision-making in important scientific matters. Moyal (1975) presents a case study of the Australian Atomic Energy Commission, showing the operation and problems of closed policy-making.

Rahman (1972, chapter 6) gives a fairly detailed account of the effects of the existence of a decision-making elite in science, including the ignoring of conditions for working scientists, inhibition of freedom of expression and the linking of issues with personalities. Rahman also criticised the lack on the part of scientists of any effort to make science a part of culture. Actual attempts may dazzle the people and persuade them to accept, in the name of science and technology, the otherwise unacceptable. Participation of the people in decision-making about science is discouraged by the development of a mystique about science, a development promoted by scientists who would otherwise lose their own importance as decision-makers and evaluators. (See also Nieburg (1966, chapter 7), who attacks the myth that only scientists can make government science policy.)