Part III: Pushing and presuppositions

Johnston and Goldsmith et al. push their scientific arguments in various ways. How can this pushing be explained?

One view is that pushing is a consequence of bias by the authors in favour of their conclusions. This makes sense, especially when one takes into account the strong psychological and career interests which scientists have in the validity and significance of their ideas. But this explanation does not go very deep: it doesn't say why particular scientists have particular biases, or where bias comes from. Furthermore, it suggests that pushing is a conscious activity, whereas scientists as a rule are totally sincere in their work.

Pushing can also be said to result from the expectations of the scientific audience, or from condensation of arguments or from editorial control. These and other such reasons each have their own place and usefulness. But they don't get at the question of why particular scientists have particular biases and where these biases might come from.

What I am looking for is a source of values which enter into scientific work. The perspective I will take in chapter 5 is that the pushing by Johnston and by Goldsmith et al. is a natural consequence of presuppositions made by them as to what they are trying to prove. These presuppositions can then be linked with the values associated with various social, political and economic assumptions.

If the perspective of the scientist having presuppositions is the one I have wanted to emphasise, then why have I devoted so much space in part II to an analysis of research papers from the point of view of pushing? There are several reasons for this. First, in the development of my ideas, the perspective of pushing came first. Second, not all pushing results from presuppositions. I wanted to emphasise the extent of the apparent bias in scientific arguments. Third, I present in part IV an analysis of basic presuppositions underlying science. I suggest an analogy: if the pushing of arguments in a scientific paper can be seen as resulting from certain presuppositions, then fundamental presuppositions underlying science can be seen as causing bias — the bias of science.

Given the ideas of pushing and presuppositions, there are a number of questions that may be asked about them. First, how does one determine the presuppositions underlying a scientific argument? One way is to infer them by inspecting various aspects of the scientific argument. This is done for SST-NO_x-ozone research by looking at the way evidence is used in a number of different articles and research papers (chapter 6). Second, are pushing and presuppositions a natural part of the way scientists work, and of the way scientific development occurs? There is considerable evidence that this is the case (chapter 7). Finally, how widespread are presuppositions? I argue in chapter 8 that presuppositions underlie all scientific work, but that most presuppositions are more deeply embedded in the context in which the scientific research is done than is the case with Johnston's and with Goldsmith et al.'s presuppositions.

A note on the structure of the chapters in this part is in order. Chapter 5 follows the same pattern as chapters 1 to 4: analysis of Johnston's paper, analysis of Goldsmith et al.'s paper, and then some discussion. Chapter 6 breaks with this pattern. It treats successively a number of pieces of evidence (such as the size of changes in ozone), and categorises a number of research papers according to how they treat these pieces of evidence. Chapter 7 is again different. In it I survey some ideas and evidence about the commitment of scientists and of the scientific community, and use examples from the SST-NO_x-ozone research area to illustrate these ideas. Finally, chapter 8 takes the form of an argument, namely that presuppositions are common in scientific research, but that their visibility depends on whether the research is presented in specialised or popular form. The argument is structured around some examples of specialised and popular writing in the SST-NO_x-ozone area.

Chapter 5: Presuppositions about what it is considered necessary to prove

I have tried to show that Johnston and Goldsmith et al. in their papers use a number of ways to push their arguments, thereby giving what might be reckoned unwarranted support for the conclusions they obtain. Here I suggest that Johnston and Goldsmith et al. make certain presuppositions or basic assumptions about the nature of their inquiry, and that their arguments are structured around these presuppositions. In the light of these presuppositions, much of the pushing done by them seems fairly reasonable and even inevitable.

What are the presuppositions underlying the work of Johnston and of Goldsmith et al.? Among others, I suggest the following. Concerning the possible dangers to stratospheric ozone due to SST exhaust, Johnston's work contains the built-in assumption that the burden of proof lies with those who claim that SSTs are safe: that all that he must demonstrate is that there is at least some small possibility of danger. Built into Goldsmith et al.'s work is the assumption that the burden of proof lies with those who claim that SSTs are dangerous to ozone: that all Goldsmith et al. must demonstrate is that the likelihood of significant danger is small.

These assumptions are mostly implicit: they are not normally the subject of argumentation as such. But what is more important is that scientific arguments are shaped in a natural manner by the assumptions. It is for this reason that I refer to the built-in assumptions about the burden of proof as *pre*suppositions about what it is necessary for their argument to demonstrate.

It is not necessary that the authors be aware of their own presuppositions or of the way their arguments are shaped by them. Indeed, to the extent that assumptions are conscious, there may be a conscious attempt to avoid at least the appearance of pushing. If in the following I ascribe motivations or knowledge of presuppositions to Johnston or to Goldsmith et al., it is mainly for stylistic convenience.

Let me now partially reanalyse the two papers from the new perspective of presuppositions about what it is considered necessary to prove.

Johnston

I adopt here this assumption: that Johnston presupposes that all he must demonstrate is that there is at least a small possibility of danger to stratospheric ozone from SSTs.

Previous to Johnston's study, a primary concern had been the possible effect of the water from the exhaust of SSTs in reducing stratospheric ozone. This problem had been studied by several authors and was generally considered to present only a very small possibility of danger. Johnston in his work presents the case that SST-NO_x presents a greater threat to ozone than SST-water. Therefore if the undesired effect of SST-NO_x is shown to be larger than one previously deemed small, it must present at least a *possibility* of danger to the environment. In other words, by showing that SST-NO_x is more dangerous to ozone than SST-water, Johnston lays the burden of proof on those who discount the danger of SST-NO_x.

Johnston's approach is suggested by his reference to the attitude of previous workers towards SST-NO_x. The previous attitude was that SST-NO_x could be neglected since its effects were less than SST-water, which had only a negligible effect. Johnston says, "The argument seems to be that H_2O is more of a threat than oxides of nitrogen, that the effect of H_2O is not very serious, and therefore that the SST poses no serious threat to stratospheric O_3. The original postulate that the oxides of nitrogen may be neglected is reexamined here." (page 517, column 1). Johnston's approach seems to be that the SST *might* pose a serious threat to ozone, since the oxides of nitrogen pose a more serious threat than any effect that had previously been discounted (namely, water).

Of course even if SST-NO_x is more important than SST-water, this does not in itself make it have a serious effect. Johnston's argument is designed to show that SST-NO_x *could* have a serious effect on ozone. Note the word could. Here is where the idea that Johnston's work is based on a presupposition is most useful. If Johnston only needs to show that there is a possibility (a small but nevertheless 'significant' possibility) of a serious effect due to SST-NO_x, then the pushing of his argument seems much more reasonable.

For example, Johnston gives only passing acknowledgment in a note to a study which gave results in disagreement to his own. Given his presupposition, Johnston does not need to analyze this alternative argument in detail. He only needs to show through his own calculations that a threat could exist. Another example: when Johnston emphasises his most significant result of a 50% reduction in ozone, he is only showing that there is a possibility of a significant change. He is saying in effect, there *might* be as much as a 50% reduction in ozone, not that there *will* be a 50% reduction. The burden of proof is laid by Johnston on others, for them to show that the chance of such a large reduction in ozone is insignificantly small.

As a last example, consider Johnston's treatment of uncertainties. With a full inclusion of uncertainties, Johnston would obtain a much wider range of results. But there would still be a possibility of a large reduction. By not taking all the uncertainties into account, Johnston is merely unencumbering his conclusion of a clutter of qualifications. These qualifications are not greatly important, given Johnston's presupposition. The essence of his conclusion, that there would be a significant possibility of a serious effect due to SST-NO_x, would be unchanged by the qualifications.

Considering all the ways described in part II by which Johnston pushes his argument, I find in most but not all cases that this pushing seems much more reasonable when it is presupposed that SST-NO_x is dangerous until conclusively proved safe.

Johnston states his presupposition quite explicitly at the conclusion of his 1972 article in the scientific journal *Search*: "the strength of the basic argument and the degree of damage that could be done to the world if stratospheric ozone were reduced by a large amount are such that the burden of proof should be on those who would add NO_x to the stratosphere, not on those who warn against such pollution."

Goldsmith et al.

I adopt here this assumption: that Goldsmith et al. presuppose that all they must demonstrate is that the danger to ozone from SSTs is very unlikely to be significant. This would mean that for their purposes it is sufficient to show that large injections of NO_x into the stratosphere are very unlikely to cause a significant reduction in ozone.

Previous to Goldsmith et al.'s study, the main research emphasis in the SST-NO_x-ozone field has been on the construction of theoretical models of ozone, and investigation of the effect on ozone of introducing SST-NO_x into the model. Many of such studies revealed a possibility of a significantly large reduction in ozone. But the effect of an actual rather than theoretical introduction of large amounts of NO_x into the stratosphere had not been studied.

Goldsmith et al. in their study present evidence that a large amount of NO_x had already been injected into the stratosphere by nuclear tests. and had no noticeable effect on ozone. Since the amount of NO_x expected to be injected into the stratosphere by SSTs is comparable to that injected by nuclear tests, Goldsmith et al. do not expect that the effect of SST-NO_x will be noticeable either. Showing that the effect of SST-NO_x is very unlikely to be large, Goldsmith et al. thereby satisfy their own criterion of what they have to demonstrate.

Consider Goldsmith et al.'s technical assumption about the equivalence of NO_x from nuclear blasts and from SSTs. Given their presupposition, Goldsmith et al. appear generous in comparing equal amounts of SST exhaust-NO_x and nuclear blast-NO_x. This is especially so considering the several reasons why SST-NO_x might be expected to be less effective in reducing ozone than nuclear test-NO_x.

Goldsmith et al.'s attitude to the work of other researchers reflects their presupposition. The work of Johnston and of Crutzen did not conclusively demonstrate a danger due to SSTs, because of numerous limitations to their work. As far as demonstrating the possibility of a large effect on ozone is concerned, and given Goldsmith et al.'s presupposition, this work is seen as "speculation". By comparison Goldsmith et al.'s work is a semi-empirical study of an actual large injection of NO_x into the stratosphere, and is directly concerned with the possibility that Goldsmith et al. attempt to discount.

Goldsmith et al.'s treatment of uncertainties also is logical given their presupposition. They are most concerned with the accuracy of their calculation of the amount of NO_x produced by nuclear explosions, since the result obtained is used to show that a massive injection of NO_x into the stratosphere occurred. In looking for the effect of actual injections upon stratospheric ozone, the uncertainties are not so important, since all Goldsmith et al. feel they must demonstrate is that the effect was not obvious and dramatic. For if a couple of massive injections of NO_x did not strikingly or even noticeably affect ozone, then further massive injections by SSTs cannot be expected to do this either.

Finally, their presupposition seems to lead them to their explicit conclusion. They note that the actual amounts of NO_x in the stratosphere have yet to be established. Given this uncertainty, and their own result that a massive injection of NO_x does not affect ozone significantly, they suggest that NO_x is not such an important variable, in terms of determining ozone levels, as had been assumed by other workers. In other words, they seem to assume that NO_x (as well as SST-NO_x) is not a vital factor in determining ozone levels until it has been proved to be one. This interpretation makes other statements appear more reasonable. For example, when Goldsmith et al. say "It is, however, incontrovertible that in laboratory conditions NO and NO_2 convert ozone and oxygen atoms to molecular oxygen." (page 545, column 1), the context includes their assumption that NO_x is not an important variable in determining stratospheric ozone levels, until proved to be. What has been conclusively shown is only that "in laboratory conditions NO and NO_2 convert ozone and oxygen atoms to molecular oxygen", not that this is important in determining *stratospheric* ozone levels. Thus instead of looking first at NO_x as an explanation of ozone levels, they first look at other factors: "It may be that other factors . . . may all play a part in bringing theory closer to measurement." (page 551, column 2).

The logical extension of Goldsmith et al.'s presupposition is the assumption that dangers from all SST exhausts (water, CO_2, NO_x, etc.) are to be dismissed until there is strong evidence to suggest that they are significant. This attitude can be inferred from the lack of any suggestion by Goldsmith et al. that "hitherto unconsidered chemical sinks" (page 551, column 2) might be of significance as potential environmental dangers, due perhaps to other SST-exhaust effects. These "hitherto unconsidered chemical sinks" are seen only as a possible explanation for why NO_x appears not to be a significant chemical sink for ozone.

In summary: when the presupposition is made that SST-NO_x is very likely to be safe until good evidence is presented to suggest otherwise, most of Goldsmith et al.'s pushing seems much more reasonable and acceptable.

Discussion

I have found it useful to approach the work of Johnston and of Goldsmith et al. in terms of hypothetical presuppositions on which their work is based. Namely, Johnston lays the burden of proof on those who say SST-NO_x is safe; Goldsmith et al. lay the burden of proof on those who say SST-NO_x is dangerous. In the context of these presuppositions, what would otherwise appear to be pushings of their arguments appear instead as reasonable and acceptable approaches to their chosen problems. Throughout, Johnston orients his arguments towards showing that there is at least a small possibility of a large reduction in ozone due to SST-NO_x; he feels that this possibility should not be neglected. On the other hand, Goldsmith et al. orient their arguments towards showing the high probability of the safety of SST-NO_x; they feel that the small possibility of danger has not been convincingly demonstrated. Thus it is possible that the same result for the probability of reduction in ozone would be interpreted by Johnston and by Goldsmith et al. in entirely different ways. For example, a small possibility of danger might be for Johnston *small but significant*, while it might be for Goldsmith et al. *small and insignificant*.

Is pushing on the basis of presuppositions justified? In terms of the presuppositions underlying the pushed argument, it seems reasonable to answer yes. In terms of some other presuppositions, it would probably seem reasonable to answer no. The basic question then is whether the presupposition itself is justified. This will usually involve questions of personal and social values, as well as purely 'scientific' considerations.

So as not to be misunderstood, I had better state my own feelings about pushing and presuppositions. I believe pushing and presuppositions are inevitable. I am not against presuppositions; I am against presuppositions in hiding. This implies that scientists should attempt to make their presuppositions explicit. I am not against pushing; I am against pushed arguments, the only type of arguments there are, masquerading as statements of incontrovertible and value-free fact. Of course, it is easier to ask scientists to make their presuppositions explicit than it is for them actually to do this — especially when scientists may not even recognise presuppositions for what they are. That is why it is important for people to be able to spot presuppositions for themselves, and to evaluate their impact on scientific arguments.

It may be that the presuppositions I have ascribed to Johnston and to Goldsmith et al. are somewhat simplistic. For example, it may be argued that Goldsmith et al. *do* accept the burden of proof, but differ from Johnston as to what level of confidence is required to say that SST exhausts are safe. I do not disagree overly much with this point of view, which seems to me to be a semantic variant of my own. As several people who read the first version of this chapter said, the whole thing depends on one's basis for what is very unlikely.

One of my friends said, "In a blood transfusion there may be a 1 in 10 000 chance that I'll catch some disease. Does this mean I should wait for *conclusive* proof of safety? What is conclusive anyway? 1 in 10 000 000 000? 1 in 10^{50}?" Another friend said that "the crux of the matter is the assessment of significance, or to put it another way, the preparedness to take a calculated risk, which differs between groups." I agree. Ascribing different presuppositions to Johnston and to Goldsmith et al. about the onus of proof is for me a way of stating (and highlighting and perhaps overstating) their differences concerning the assessment of risks.

It is important to realise the extent to which presuppositions can *shape* a scientific argument. In this chapter I have tried to show that the content of the scientific argument — namely, the way it is pushed — is strongly influenced by the presuppositions. The presuppositions also can have an important influence on the scientist in determining what sorts of problems and effects are thought worthy of study. My feeling is that Johnston was on the lookout for possible dangers to the environment by SSTs which had been overlooked, whereas Goldsmith et al. were on the lookout for fairly broad tests that could show the low probability of danger to the environment by SSTs. Whatever the real reasons for Johnston's and Goldsmith et al.'s research, pushing and choosing of scientific arguments are good examples of ways in which scientific research can be shaped by presuppositions.

Differences in presuppositions are very likely to reflect differences in social, economic, political, and other values. Not knowing Johnston or Goldsmith et al. personally, I really shouldn't speak for their values. But I can give examples of differences in values that might plausibly be linked with differences in presuppositions. There is the value one attaches to technological innovation as opposed to the value one attaches to maintaining or improving environmental quality. There is the weighting one attaches to present benefits such as SST travel as opposed to future costs such as a possible long term reduction in ozone. There is the value one attaches to having an SST at all: those who look at the SST as one of the greatest recent technological boons to humanity are likely to place the burden of proof differently to those who look on the SST only as a way for flying a few capitalist exploiters around a little faster. There is the value one attaches to investments already made and to profits and jobs at stake. Here it is salutory to remember that the U.S. was about to shelve its SST project when the U.S. scientist Johnston did his research, whereas the U.K. had invested long years and lots of hard cash on the Concorde SST by the time the British team Goldsmith et al. did theirs. It is not hard to see that scientists have different values built into their perspective on the world and on science. What I have tried to begin to do here is show how such values may orient and shape a scientific argument.

Finally, one's own presuppositions strongly influence one's reaction to scientific work. If one disagrees with the assumptions built into a bit of scientific work, one is much more likely to consider its author to be biased. This is all the more likely if the presuppositions are rarely or never explicitly stated. But perhaps a greater danger arises when one *agrees* with the assumptions built into a piece of scientific work, for then the assumptions may never be recognised at all.

Other perspectives on pushing

An analysis in terms of presuppositions certainly cannot explain all the pushing in Johnston's, Goldsmith et al.'s, or other scientific papers. Here I describe some other reasons why the arguments in Johnston's and Goldsmith et al.'s papers may be pushed or appear to be pushed. (From some of the following perspectives, what I call pushing more properly would be called by another name. I retain the term 'pushing' for convenience.)

Communication in areas covering many disciplines. The subject matter of the papers by Johnston and by Goldsmith et al. is multidisciplinary. It covers areas such as meteorology, chemical kinetics, engine emissions, statistics, biological effects of ultraviolet radiation, and aviation economics. It is unlikely that any one person will be familiar with the detailed aspects of all these areas. Therefore in making a study the investigator must accept the statements of 'authorities' from other disciplines; alternatively, statements can be made on the basis of a limited understanding of particular areas. It may be argued that some of the pushing in the papers by Johnston and by Goldsmith et al., and in multidisciplinary topics in general, is due to lack of a unified expertise in all aspects of the problem being studied.

My perspective is somewhat different from this. To me, multidisciplinarity is not the cause of pushing, but rather provides a means for it to be expressed. If one decides to accept statements by 'authorities' outside one's area of expertise, there is opportunity to choose the appropriate authority or the appropriate statement: namely the one that supports one's argument in a crucial place. If one decides to base the argument on one's limited understanding of an area, there is the possibility that one's ideas about the area will reflect one's interest in a particular conclusion.

Certainly multidisciplinarity is important in pushing. I suggest that it provides a means of expression of pushing rather than actually causing it.

What about studies well entrenched within a single discipline? Does the relative lack of a means for expressing pushing mean that pushing does not exist? I would say not. First, there are other means for pushing, such as selection of evidence even when one knows about evidence supportive of several viewpoints. Second, disciplines may have presuppositions built into their definition and separation from other disciplines. For example, studies strictly within chemistry leave out a consideration of economics. Such studies implicitly assume or accept that the economic implications of chemical analyses or discoveries will be handled within the prevailing economic system. In this way presuppositions and hence what effectively is pushing are built into specialised areas of expertise.

Condensation of arguments. Scientific papers are never as long as they might be. There are a number of pressures within the scientific community for scientific papers to be as short as possible. Printing costs are an important influence. Also important are the desires of practising scientists for conciseness, so as to shorten their reading load. It is possible that in shortening an article for publication, that the argument is unavoidably pushed. For example, only a few graphs may be allowed, and the ones best illustrating the significance of the conclusion chosen. Or qualifying statements may be deleted.

The condensation problem is a real one. Johnston's *Science* paper is an abbreviated version of his 114-page UCRL report which he refers to many times in the *Science* paper (see Johnston's note 16). The UCRL report contains many calculations and comments not included in the *Science* paper. In my own experience with one scientific paper, a referee requested shortening the paper primarily by removing most of a section on limitations which was thought to be fairly obvious.

Nevertheless, my attitude is that the necessity to condense scientific material more often provides a means for pushing than actually causing it. Usually the scientist has a fair degree of control over evidence selected and comments made. There is the possibility of emphasising limitations and qualifications, or of emphasising supporting arguments and evidence. As in the case of Johnston's and of Goldsmith et al.'s summaries and abstracts, it is usually the latter that survives abbreviation.

Although Johnston's *Science* paper may be short compared to the UCRL report, there are still quite a number of words in it. There is ample space and opportunity for positive or negative arguments. With all due regard to Johnston's opinion, I believe that his argument is pushed in his UCRL report *and* that it is somewhat further pushed in the *Science* paper. This shortening may have been the immediate cause of some of the pushing in Johnston's argument, but I believe it is not the dominant reason for the pushing. Certainly Johnston's UCRL report is based on his presupposition that the onus of proof falls on those who argue that the environmental effects of SSTs are negligible.

Response to criticism. If one's argument is criticised or attacked, a natural reaction is to emphasise supporting arguments and evidence. If one is attacked for presenting unrealistic results, a natural response is to present reasons why the results *are* realistic. If one is criticised for making a certain assumption, a natural reaction is to offer justifications for that assumption. Much pushing can be seen as a reaction to criticisms and accusations.

In the case of Johnston's paper, there was intense opposition to its conclusions from some quarters after the first drafts of the paper became widely known. A similar reaction greeted Goldsmith et al.'s work. Attacks in some cases extended beyond the scientific arguments to the motivations and character of the scientists.* It would not be surprising if Johnston and Goldsmith et al. reacted to such attacks by emphasising reasons for the validity of their conclusions.

I am very sympathetic to this perspective. It gets close to the real operation of science, with intensely held opinions clashing in the intellectual and political and economic marketplace. Still, it is not likely to be the main cause of pushing, at least in a direct sense. Most scientific papers are not changed in any drastic fashion from the time of first response to the time of final publication. Some changes promoting pushing in Johnston's and Goldsmith et al.'s papers no doubt can be traced to an antagonistic response to earlier efforts. But the main features of the work remained constant. (See chapter 7 for support for these statements.)

Editorial control. It is possible that supervisors, editors and others who influence what scientific material is published could contribute to pushing. For example, an aircraft company might allow one of its scientists to publish a paper about SST-NO_x only if it put the SST in a suitably favourable light. However, although it is difficult to document, my experience is that most scientists and scientific institutions prefer to avoid controversial stands and to take a more or less middle-of-the-road approach on most scientific issues. In as much as this is the case, editorial comment and control more often than not has a restraining influence on the pushing in a scientific paper.

Author control. In most cases a scientific paper is not consciously biased towards a certain conclusion. The scientist typically writes down all the relevant arguments and evidence that come to mind at the time. Conscious control over this may be minimal. A particular choice of evidence which pushes the argument may be used essentially by chance. There may be other uses of evidence that actually hurt the argument, again chosen more or less randomly.

The argument may also be conditioned by various psychological preferences and motivations of the author. For example, the scientist may prefer certain pet ideas, whatever their relevance to the argument at hand. Or a scientist may dislike a colleague personally, and as a consequence unfairly criticise the colleague's work.

This lack of conscious bias may seem to contradict what I said earlier concerning condensation of arguments about scien-

*Most of the evidence I have to back up this statement is confidential.

tists having a fair degree of control over evidence selected and comments made. The difference between these points is that scientists have control in principle over what *could be* included, but at the same time considerably less control in practice over what it seems *natural* to include.

I entirely agree that most scientific arguments are not consciously biased. Most pushing develops in a natural manner from presuppositions as I have suggested it does. If a scientist is interested in obtaining a conclusion of a certain type, arguments will be perceived and selected according to their usefulness in promoting the conclusion. If the scientist then writes the manuscript on the basis of all personal relevant knowledge at the time, the argument will be pushed as a result of the normal process of doing research.

Certainly there will be some pushing that results from factors that are irrelevant to the conclusion: random choice of evidence or ideas, or personal motivations. Pushing of this sort normally will be balanced in a paper by a similar amount of pushing of the counterargument.

I have not tried to evaluate the extent of pushing of the counterargument in the papers by Johnston and by Goldsmith et al. Some of the cases of pushing which I diagnose may be due to random or personal factors. It is my opinion, though, that such factors are not a dominant reason for pushing. Otherwise pushing would not seem to be important for the argument as a whole.

I offered to Johnston and to Goldsmith et al. an opportunity to include here a written reply to the material in chapters 1 to 5 after I had finalised my text. Both of their replies follow.

Rebuttal by H. S. Johnston

In this book Brian Martin sees certain scientific activities as pushing ("biased, value laden, seamy side of the behaviour of scientists"). By setting up special definitions Martin transforms *pushing* into what I shall call *Pushing* ("presuppositions about what it is considered necessary to prove, typical of all scientific work, not consciously biased"). Over a long correspondence I had with Brian Martin, I explained my position and denied that I had engaged in either *pushing* or *Pushing*. With minor exception, he has never modified his interpretation of my work on the basis of this correspondence, but he did invite me to write a few pages stating my position.

Of course, there are human aspects and background factors in scientific research, especially if the topic impinges on matters of social or political concern. First, I shall review some of the background to my article in *Science*.

Over the period 1963-70, the U.S. Government was pushing (lower case) the development of a large commercial supersonic transport, and the Government supplied about 90 percent of the development costs of the Boeing SST. Some environmental groups actively campaigned against the SST, largely on grounds of the sonic boom and noise near the airports. During 1969-70, many members of the U.S. Congress opposed continuation of Government funding of the SST project, economics and priorities for funds being more important than environmental considerations. A hot and increasingly bitter political battle was being waged on these questions throughout 1970, culminating in a vote to terminate U.S. support of the SST project, March 18, 1971 in the House of Representatives and March 25, 1971 in the Senate. A surprise attempt was made to reinstate the SST program in May 1971, but this motion was strongly defeated in the U.S. Senate.

The primary environmental concerns were the sonic boom and airport noise. As early as 1964, J. Hampson had suggested that water vapor in the stratosphere would reduce ozone. Using rate constants for the water reactions *estimated* by Hunt in 1966 (first measured in 1973, accurate measurements in 1977), several atmospheric scientists calculated that water from 500 Boeing SSTs was expected to reduce ozone by a few percent (3.4 percent according to Harrison 1970, about one percent according to London 1970-71). The *climatic* impact of such ozone changes was regarded as small. Early in 1970 Paul Crutzen proposed that the oxides of nitrogen, NO_x, were probably important in the natural ozone balance, much more important than the water reactions. Early in 1970, Crutzen wrote a letter to the Boeing Company asking for information about NO_x emission from the SST and enclosing a reprint of his article; this letter was never acknowledged or answered.

Under the sponsorship of the Massachusetts Institute of Technology, an international body of scientists met in the summer of 1970 to study "Man's Impact on the Global Environment", and the SST was one of about a dozen topics considered. The report, published in the late spring of 1971, was subtitled "Study of Critical Environmental Problems," and it is abbreviated as the SCEP report or simply as SCEP. The SST problem was considered primarily by atmospheric scientists, and the climatic impact of SST exhaust gases was the main concern. This 1970 summer study gathered information about the American SST from the Boeing Company, the General Electric Company (engine manufacturer), the Federal Aviation Agency (FAA), and the U.S. Department of Transportation (DOT). Although this group considered Crutzen's 1970 paper about the effect of NO_x on ozone, no mention or reference was made to this paper in the SCEP report. Instead SCEP made the following unconditional judgment: "Both carbon monoxide and nitrogen in its various oxide forms can also play a role in stratospheric photochemistry, but despite greater uncertainties in the reaction rates of CO and NO_x than for water vapor, these contaminants would be much less significant than the added water vapor and may be neglected."

During 1970 two behind-the-scenes events occurred. In confidential testimony to the U.S. Department of Transportation in the fall of 1970, James McDonald stated that even a one percent reduction in ozone would lead to a large increase in skin cancers in the United States. This case was first publicly presented as testimony to a Congressional Committee in mid-March 1971, and the primary rebuttal was that McDonald was on record as having believed that "flying saucers" were real and piloted by intelligent extra-terrestrial beings. On condition that I not reveal his name, a governmental scientist told me this account in the summer of 1971. Soon after Crutzen's paper appeared in 1970, members of the U.S. military service carried out a feasibility study of reducing stratospheric ozone by NO_x bombs as a military weapon. They concluded that it seemed possible to effect a large reduction in stratospheric ozone; but this was not a good military weapon because ozone depletion could not be localized over an enemy and also the increased ultraviolet radiation would not have a significant short-term effect. According to 1970 knowledge, an NO_x ozone-reducing bomb seemed possible but ineffective.

About the first of March 1971 I received a telephone call from Professor Joe Hirschfelder. As was my custom, I took detailed notes during this telephone call. Hirschfelder explained that he was a member of a U.S. Department of Commerce Advisory Board for SST Environmental Effects, that the Board had assembled "state-of-art facts" on the question, and that it would present these facts before an invited audience of outside experts at a symposium in Boulder, Colorado, March 18-19, 1971. He invited me to attend the "presentation". I declined saying, "I am not interested in the SST.". Joe chided me for lack of interest in a social problem to which my field of research was relevant. Finally he persuaded me to come. During the next two weeks I went to the library and studied several books about the stratosphere and on the photochemistry of stratospheric ozone.

At the Advisory Board's presentation, we were first issued Xerox copies of page proofs (pp. 15-18, 67-74) from the yet unpublished SCEP report, which were cited as the primary data we should consider. These data included: "Statistics of Emissions from one GE-4 Engine, Cruise Mode" for CO_2, H_2O, CO, NO, SO_2, and soot; the cruise height of the Boeing

SST; the expected future number of SST; the hours per year at cruise height; the factor of 10 local maximum over "significant volumes of the stratosphere"; the two-year stratospheric residence time, etc. The Board spokesman read out the key quantities, including emissions. The first morning of the meeting was a "tutorial" concerning the stratosphere by staff members of the National Center for Atmospheric Research (NCAR), presided by W. W. Kellogg, Associate Director of NCAR. The tutorial emphasized stratospheric ozone and the effect of water on ozone.

Kellogg described an upcoming paper by J. Park and J. London as giving "a careful calculation for the first time." Professor Julius London presented what he described as "preliminary calculations" on the equilibrium effect of water vapor and the oxides of nitrogen on stratospheric ozone. London presented his estimated vertical distributions of water vapor and NO_x both for the natural atmosphere and for the atmosphere with the "maximum credible" increase in water and NO_x following full scale operation of the American SST. He calculated water from the SST to reduce ozone by one percent and NO_x from the SST to reduce ozone by two percent. During his talk I had written down his numbers concerning NO_x, and I made some simple comparisons of natural ozone formation rates and the destruction rates by NO_x. When he concluded his talk, I asked: "Professor London, have you double checked your calculations for NO_x? I find a much, much bigger effect on ozone than you do."

During the next talk, someone tapped me on the shoulder and whispered that the SST would emit 10 times less NO_x than the Advisory Board had stated, and one need not worry about NO_x. A bit later someone else similarly told me that the SST would emit fivefold less than the value given out that morning. During the intermission, someone told me the SST would emit 50 times less NO_x than the SCEP value. At that, I said: "Congratulations! That is the lowest bid I have had yet. But tell me, if the NO_x emission rate given out this morning is fifty times too high, why didn't you say so during the discussion period." At the time his reply seemed non sequitur: "Do *you* believe in flying saucers?"

During the night of March 18, I wrote out my analysis of why I thought NO_x could not be "neglected" in the context of the SST, and I presented a short paper and talk to the group on the morning of March 19. After my talk, A. A. Westenberg presented the results of calculations he made before coming to the meeting. With the same computational procedure as that used by Julius London, Westenberg (see footnote 30 in my *Science* paper) showed that NO_x at 20km would strongly reduce ozone there. In the light of these two arguments, I asked the group what was the basis of the SCEP statement that NO_x "may be neglected." London, who had attended the SCEP conference, said that the amount of NO_x from the SST was so much less than the amount of natural ozone that they felt it could have little effect; also the measured amount of NO in the mesosphere (about 70 km) was a much higher mixing ratio than that expected from the SST and the natural NO_x in the stratosphere was probably correspondingly high. I again pointed out the *catalytic* nature of NO_x with respect to ozone; and as to the second point, if NO_x in the stratosphere were as high as that in the mesosphere, the rising moon would always appear blood red and the astronomers would have detected atmospheric NO_2 decades before.

During the afternoon of March 19, I reviewed my calculations and those of Westenberg, and I proposed a formal motion to the group:

> "We recommend that the oxides of nitrogen be recognized as potentially an important variable in problems concerning stratospheric photochemistry."

Members of the group, one after another, proceeded to heap on me the burden of proof concerning everything in the stratosphere: what about its motions, what about the absorption and emission of all forms of radiation, what about all aspects of its photochemistry. Then it was publicly stated for the first time that NO_x emissions from the SST were less than the values given out the day before. Finally someone said: "I don't think we are in any position to make firm conclusions at this time. I move the motion be tabled." The motion was tabled by an overwhelming vote.

Let me also note that Paul Crutzen's work was not described by anyone at the meeting. I never heard of his work on NO_x until May 1971.

When I got back to Berkeley and repeated London's calculations, I discovered that he had mistakenly used a rate constant 10,000 times too large for the classic, well-known reaction

$$2\,NO + O_2 \rightarrow 2\,NO_2$$

I wrote a letter to London about this and in his letter to me, London acknowledged that he had made this error; but to my knowledge he never publicly acknowledged this error (his uncorrected, erroneous, unpublished manuscript handed out at Boulder was still being quoted by the Australian Academy of Science a full year later). When I repeated London's calculation with the correct value for this rate constant, there was a 40 percent ozone reduction between his case for normal NO_x background and his case for SST increased NO_x. The details of this calculation I finally published in "Catalytic Reduction of Stratospheric Ozone by Nitrogen Oxides," *Advances in Environmental Science and Technology,* Vol. 4, John Wiley & Sons, Inc., 1974, pp. 328-332.

I would have expected the group at Boulder to respond to me and to Westenberg somewhat as follows: "Could it be that we have underestimated the importance of NO_x on stratospheric ozone? We think not, but let all of us with expertise in this field study this question, correspond with each other, and report our findings to the Advisory Board, who invited us here primarily to see if they had overlooked anything important."

Instead, all kinds of rigid lines were drawn and accusations were made. It took me years to understand what some of these rigid positions were. It appears that in some quarters there had long been a feud between chemists and atmospheric scientists. At Boulder, I was a chemist telling some atmospheric scientists that they had made a mistake, and then many atmospheric scientists closed ranks and from then on refused to accept anything I said or wrote in terms of the English-language meaning of the words used. Over and over again, they misquoted my work and then attacked it on the basis of their misquotations. As an example, let me quote P. Goldsmith, "Pollution of the Stratosphere and the SST," British Meteorological Office, September 1971:

> "Currently much publicity is being given to a paper by Prof. H. Johnston of the University of California in which as a result of a model of photochemistry of the stratosphere, he concluded that full scale operation of 500 SSTs, with the majority operating at altitudes of 70000 to 80000 ft., would result in a decrease in the mean worldwide ozone concentration of about 50%."

The reader of Brian Martin's book has a copy of my article and can readily see that this is a gross misquotation. For worldwide average I illustrated situations that gave calculated ozone reductions between 3 and 23%. It was only for the localized tenfold larger vertical column that I illustrated possible ozone reductions of 3 to 50 percent. I can cite numerous similar cases of misquotation over the period 1971-77.

In my correspondence with Martin, I complained about people who misread or misquote my articles. Since then he has added a section in Part I that describes "what scientists generally mean by 'reading' a scientific paper." His apology for careless reading of scientific papers seems totally unacceptable to me. If anyone is going to attack or criticize a scientific paper, then I think the person is duty bound to read all of it carefully.

Brian Martin's printing in full the two articles that he criticizes is certainly an act of openness and completeness. His invitation for this rebuttal is an act to promote fairness. Even so, I regard Martin's analysis of my 1971 *Science* article as being very much in the tradition of atmospheric scientists who refuse to accept my written words for their actual literal meaning.

Martin "assumes" that my presupposition is that all I "must demonstrate is that there is at least some small possibility of danger." I think I stated my presupposition quite clearly in the 1971 LBL report: "The point of this report is not to assert that SST flights will reduce the ozone shield by some precise factor; rather the point is that NO_x is a highly important variable in this problem and it must be given realistic consideration." This idea is expressed not quite so succinctly in the last paragraph of the *Science* article. I presumed that what I was trying to demonstrate was that SCEP was wrong in saying that NO_x may be neglected in stratospheric photochemistry. Any examples that showed NO_x to be an important variable within the framework of 1971 knowledge were appropriate arguments against SCEP's unconditional assertion. I tried, especially in the LBL report, to give several different examples specifically involving a wide range of mathematical sophistication. The most powerful and the most appropriate argument to use against SCEP's assertion "NO_x may be neglected" is to identify the largest effect of NO_x on ozone within the context of 1971 knowledge. The possible factor of two ozone reduction featured in the abstract is the appropriate example to support the contention that NO_x may not be neglected. Recall that London's highly praised model actually showed a 40 percent ozone reduction by NO_x from SSTs, if correctly applied.

During the development of the *Science* article, my computer programmer suggested that I replace the uniform shells of SST exhaust by a more realistic gaussian plume. I turned down this suggestion saying: "No, I want the square corners to show. If I rounded them out someone might think that I am pretending to carry out model calculations. I am trying to identify important variables, not solve real-world problems."

What was at stake in the spring of 1971 was whether we understood the stratosphere so thoroughly that we could confidently assert that there would be no significant environmental effects from a large fleet of SSTs or whether the SST developers (mostly the U.S. Government at that time) should launch an expensive program of stratospheric research. The SST planners believed that 68 parts per billion of NO_x over a substantial (10%) portion of the atmosphere could be neglected, and at Boulder they were prepared to close the book on this question. I was convinced that this was a potentially dangerous misconception. (Even in the light of 1978 chemistry, 68 parts per billion of added NO_x would reduce stratospheric ozone by a large amount, although small amounts of added NO_x in the lower stratosphere are now thought to increase the ozone vertical column.) I argued with, I think, impeccable science that the oxides of nitrogen were an important consideration in stratospheric photochemistry and must be given realistic consideration, that is, an expensive stratospheric research program should be inaugurated.

In discussing my paper, apparently the only thing Brian Martin sees in it is my "theoretical model." For point of reference he gives what he regards as the capabilities of a good 1971 "comprehensive model of stratospheric ozone." No model in 1971 came anywhere close to meeting Martin's specifications, and it was 1974 before any model appeared that approached these specifications. Martin's historical perspective is quite poor here, and some of what he calls my pushing is a matter of the state of ignorance in 1971.

By 1972 most atmospheric scientists accepted the proposition, rejected in 1970 and 1971, that NO_x is an important variable in stratospheric photochemistry. Apparently Brian Martin also accepts this, my primary, proposition. If this proposition is accepted as self-evident, then my *Science* article seems to be only a low grade exercise in stratospheric modelling. Most of Martin's analysis of my paper is from this point of view. He never accepts my words at face value that I sought to identify important variables and correct SCEP's mistakes. All of my examples indicated that NO_x was an important consideration; the calculation involving shells of various thickness showed the distribution of NO_x in the stratosphere (a matter of atmospheric motions) was equally as important as the amount.

In his section on "Method of referring to alternative arguments," Martin says that I am pushing my argument because I relegated to footnote 5 my mention of an article (that of Park and London) that disagreed with my position. However, Martin fails to mention that I relegated to footnote 30 my mention of Westenberg's calculations that agreed with my position. It seems to me that these two footnotes balance so far as "pushing" is concerned. In this and in all other cases I do not regard my article to be "pushing."

Since Brian Martin so beautifully transforms *pushing* into *Pushing* by special definitions, one would think that I would have the grace to accept his charge of *Pushing*. However, I also reject this somewhat more benign accusation. The reason for my doing so is given by a light-hearted anecdote, with which I conclude this somewhat heavy rebuttal.

The Fallacy of Malovism

Hungry for breakfast, a customer sat down at a restaurant table and began to study the menu. The waiter asked: "Instead of ordering from the menu, would you like the chef's specialty for this morning?"

"What is it?"

"Ah," sighed the waiter in delight at the thought. "Raw Rotten Eggs."

"Rotten eggs!" exclaimed the customer. "Certainly not."

"You don't understand," said the waiter. "In this restaurant we have *special definitions*. When I say 'Raw Rotten Eggs,' I really mean the freshest of eggs, laid no sooner than yesterday, boiled for four minutes, and served on a silver platter with homemade rolls, cheese, and jam."

There was only a skeptical silence.

"We get our eggs from a nearby farm. The hens run free in a green field. The yolks of the eggs are large, red, and delicious from the clover. Every item on the tray receives the chef's special attention. You really should try our breakfast specialty."

Finally the customer let himself be persuaded.

The waiter went to the kitchen window and called out: "Special breakfast with two Raw Rotten Eggs."

But in the kitchen they had no special definitions. The cook grumbled to his helper: "Raw rotten eggs! What sort of creature wants that for breakfast? Besides, I don't have any rotten eggs in my kitchen."

"Wait," said the helper. "Remember last month when you found a stale egg in one box, and you said not to use any of them. That box was left in the back room; I saw it there yesterday."

"Why didn't you throw it out?" asked the cook.

"I was going to do it today. Anyhow, if that guy wants rotten eggs for breakfast, these ought to be in good shape — been there about four weeks."

The cook arranged pieces of toast and rolls around the outer edge of a silver serving tray, added butter, two kinds of cheese, and three kinds of jam. In the centre he placed two porcelain egg cups each with a raw rotten egg.

The waiter carried the breakfast to the hungry customer and spread it out ceremoniously.

The customer left the restaurant in disgust after cracking open the foul smelling eggs.

Statement by P. Goldsmith

"I regard your implications about our motivations, and the way in which our studies were made and presented, as unfounded and to some extent derogatory. As reputable and responsible scientists the whole object was to improve our understanding of the science involved, thus clarifying the basic problem and nothing else.

In the event, all the conclusions of the paper have been substantially confirmed by further work in our own organisation (the UK Meteorological Office) and other institutes particularly in the USA. This includes the amount of NO production by nuclear tests in the stratosphere, more sophisticated analyses of the ozone records and the comparison of our findings with those of numerical models.

The main consensus in recent reports based on modelling results is that flights in the lower stratosphere by aircraft actually increase ozone, those at Concorde levels produce very small effects and any higher levels would produce ozone decreases much smaller than estimated originally. These revisions have come about mainly through recent advances in the chemistry such as inclusion of the methane and chlorine chemistry and changes in rate reactions involving HO_x.

In retrospect, it now appears that the inferences we made from the evidence provided by past nuclear testing (partly observational) about the possible degree of ozone depletion by high flying aircraft has been largely confirmed by the later theoretical modelling results."

Reference notes

Presuppositions about the burden of proof can be found in many controversies in many fields.

One area in which conflicts over the burden of proof are quite dramatic is the area of food additives, where in the U.S. it has been explicit in the law that additives must be proved to be safe. This does not stop companies from making the opposite assumption in their arguments. For many examples see Turner (1970, especially chapter 5).

An excellent analysis of assumptions about the onus of proof in the field of SST-NO_x-ozone is given by Pittock (1972), in his comments on the report of the Australian Academy of Science on the atmospheric effects of supersonic transport. Pittock describes the assumptions very usefully in terms of the two types of error generally used in statistics: accepting a false hypothesis as true (the error considered more serious), and rejecting a true hypothesis as false (the error considered less serious). In the SST-NO_x-ozone problem, a value judgement is involved in choosing what hypothesis is 'given':

> A committee of distinguished scientists may thus understandably produce recommendations which in effect are based on achieving a very low risk of wrongly asserting that SSTs will cause significant atmospheric effects. It should be equally understandable, however, that the public interest might be better served by recommendations based more on seeking to reduce the risk of wrongly asserting that SSTs will *not* cause significant atmospheric effects. (Pittock, 1972, p. 287.)

Pittock's view is that differing value judgements lie at the root of many seemingly technical disputes. This view is also supported by Schneider (1976, pp. 188-192), who feels that interpretation of evidence in the SST-NO_x-ozone debate was partly based on scientists' personal philosophies.

A detailed discussion of the issue of the burden of proof in the debate over the impact of fluorocarbons on stratospheric ozone, a problem closely related to the issue of SST-NO_x, is given by Dotto and Schiff (1978).

Another good example of the importance of the location of the burden of proof in environmental studies concerns the question of the influence or non-influence of cloud seeding in the causation of a flood at Rapid City, South Dakota, costing many lives. See the letter of Reed (1973) and the following comments on the letter. It is notable that there seems to be little effective communication or even understanding between the proponents of each perspective in this case. (Barrie Pittock kindly referred me to this example.)

An excellent description and analysis of the effects of the assumed onus of proof in medicine and psychiatry is given in Scheff (1966). The effect of existing law on the attainment of optimal solutions (Mishan, 1969) provides another example of the social impact of an implicit onus of proof. M. Roberts (1976) describes how, in social sciences, values enter into the choosing of null hypotheses and in formulating a problem statistically.

The explicit statement of Johnston's presupposition is quoted from Johnston (1972, p. 282).

Much of my analysis of other perspectives on pushing is based on and in response to valuable comments by Harold Johnston on a preliminary version of the early chapters in this book. The importance of psychological motivations of scientists was brought home to me by comments by Jean Autrey. Some of the biases that result from publication policies of journals are treated by Mahoney (1976, pp. 95-103).

Chapter 6: Detecting presuppositions from the way evidence is used

Given a piece of scientific work, how can one find out what the presuppositions underlying it are? In the previous chapter, I argued that pushing often is a manifestation of presuppositions. If this is so, then by looking at the particular ways a scientific argument is pushed, one may be able to select out what presuppositions underlie it. This assumes that one knows what presuppositions one is looking for. How to determine what sort of presuppositions one should look for in the first place is a more basic problem.

One way to infer the presuppositions underlying a piece of scientific research is to inspect the way particular pieces of evidence are used. Here I will treat these classes of evidence relating to the SST-NO_x-ozone problem: the size of changes in ozone; SST versus Concorde; limitations of photochemical modelling; stratospheric flights and observed increase in ozone; the size of the SST fleet; and the reversibility of changes in ozone. These classes of evidence are not perfectly well-defined, nor wholely independent of each other. Also, there are other important types of evidence that might be used for inferring the existence of presuppositions. The categories used here are for illustrating the close link between presuppositions and use of evidence.

The size of changes in ozone

Many workers have calculated the likely change in the mean ozone column due to SST-NO_x. Suppose that a reduction of about 5% is obtained, and that a worker wishes to note that a change such as this is a 'small' one. The question is, how big can a 'small' change be? There are many criteria that could be used. A favorite one seems to be the size of naturally occurring changes in ozone levels. These changes can be quite large, especially if day to day fluctuations, place to place variation, or seasonal oscillations are chosen as criteria. Those who wish to note that the SST-exhaust induced change in ozone would be 'small' note that it is much smaller than the natural variation in ozone levels. *Then* they often conclude that this change will not be significant. That is, it will not be harmful to the environment or humans, because natural changes are not harmful.

On the other hand, those who wish to emphasise the importance of a change in ozone induced by SST exhaust note that most living organisms affected by ozone changes (that is, by changes in the amount of ultraviolet light not absorbed by ozone) respond to the *average* amount of ultraviolet. If the amount of ozone were lower by a certain average fraction, then as a result the amount of ultraviolet would be larger by a certain average fraction. Organisms would be affected by the change in the average, even though the day to day or year to year changes in ultraviolet were larger than the average change. Thus the skin on the back of a person's neck would be expected to respond to the average amount of ultraviolet over a certain fairly lengthy period, with the size of the fluctuations in ultraviolet absorbed presumably not being too important.

An analogy can be drawn between ultraviolet and rainfall. If the average amount of rainfall in an area is reduced by 10%, this would not affect significantly the required capacity of a city's stormwater drainage system, as long as there remained some periods of extremely heavy rainfall. However, the change would be important to farmers, whose crops might respond within certain limits to the average amount of rainfall over the growing season.

Thus, whether the change in ozone is significant depends on one's purpose. It is noteworthy that those who otherwise tend to dismiss the importance of stratospheric pollution by SST exhausts tend to use the argument suggesting smallness, while those who otherwise tend to emphasise the importance of this possibility use the argument of taking the average.

Table A lists various papers according to their stance on this and the following issues. As in any classificatory scheme, the papers listed do not always fall neatly into one category or another. In some cases I have entered papers which have an implicit emphasis on one view or another; in other cases where an implicit attitude may exist I have omitted classifying a paper at all.

SST versus Concorde

A Concorde aircraft emits about one third as much NO_x as a U.S. SST would be expected to, the precise fraction depending on the estimates used. Those who wished to emphasise the inapplicability to Concorde of results such as Johnston's referring to the U.S. SST exhausts emphasise this difference.

There is also the additional consideration that per passenger, or per passenger-distance, the Concorde emits about as much NO_x as larger SSTs would be expected to. The Concorde emits less NO_x, but seats only about 120 passengers compared to the U.S. SST with its planned 300 passenger capacity. Those who wish to emphasise the applicability of SST results to Concorde emphasise the per passenger or per passenger-distance criterion.

(This is not to deny real and important differences between the SST and Concorde as concerns their potential effect on ozone. In particular, since the U.S. SST was to fly at roughly 20 km and the Concorde at roughly 17 km, the average time spent in the stratosphere by NO_x from a U.S. SST would be longer

Table A: Categorisation of selected papers according to presuppositions inferred from use of various types of evidence.

	Emphasising safety of SST exhausts	**Emphasising danger of SST exhausts**
(1) Size of changes in ozone due to SST-NO_x (or SST-H_2O)	(small compared to natural changes)	(significance lies in average change)
	Daniels (1970) Lloyd (1972) Machta (1971) Swihart (1971) Goldsmith (1971) Scorer (1972)	Crutzen (1972) Johnston (1972) McElroy et al. (1974)
(2) SST versus Concorde	(Concorde emissions smaller; reference only to Concordes, or to limited geographical region)	(Concorde emissions per passenger similar to other SSTs; reference to all SSTs in all areas)
	Lloyd (1972) Goldsmith (1971) Australian Academy of Science (1972) Johnston (1972)	Crutzen (1972) Johnston et al. (1973) McElroy et al. (1974)
(3) Limitations of photochemical modelling, and other uncertainties	(Predicted SST-NO_x effect will be smaller when dynamics better modelled, or other areas of uncertainty clarified)	(Predicted SST-NO_x effect will not necessarily be smaller when dynamics better modelled, or other areas of uncertainty clarified)
	Lloyd (1972) Goldsmith (1971) Goldburg (1972) Australian Academy of Science (1972) Scorer (1972)	Crutzen (1972) Machta (1971) McElroy et al. (1974) Johnston et al. (1973) Johnston (1972)
(4) Military, subsonic flights in stratosphere, and ozone increases	(Correlation noted)	(Don't know why ozone increases)
	Lloyd (1972) Goldsmith (1971) Swihart (1971) Australian Academy of Science (1972) Scorer (1972)	Machta (1971) Johnston et al. (1973) Johnston (1972)
(5) Reversibility of changes in ozone	(Monitoring can be used to determine if and when SST operations should be curtailed)	(Effects of SST-NO_x may be difficult to detect, and may persist for long periods)
	Australian Academy of Science (1972)	McElroy et al. (1974)
Explicit statement of presupposition about what it is necessary to prove		
	Goldsmith (1971) Swihart (1971) Scorer (1972)	McElroy et al. (1974) Johnston (1972)

than for NO_x from Concorde. This difference is certainly an important one.)

Proponents of SSTs tend to refer to SST fleet sizes in special contexts. In particular, they often refer only to a fleet of *Concordes,* and do not aggregate SST numbers to include other existing or potential SSTs such as the Soviet or U.S. SSTs. Also, proponents may refer only to the SSTs which fly in a certain area. For example, The Australian Academy of Science devoted a section of its 1972 report to determining the amount of NO_x produced by SSTs which would be flying in the region of Australia. On the other hand, opponents of SSTs tend to refer to SST fleet size as a whole, and emphasise calculations based on a world fleet of SSTs.

Limitations of photochemical modelling

Although Johnston includes the effect of wind and turbulence in an indirect way in this work, he does not directly model these dynamic effects. This omission affects his results in two major ways. First, at low stratospheric altitudes the ozone concentrations he obtains are different from those that would be obtained if winds and turbulent diffusion effects were included. Second, winds and turbulent diffusion are vitally important in determining where SST-NO_x will spend its time in the stratosphere. (Remember that Johnston solved the problem of where SST-NO_x would go in the stratosphere by assuming several arbitrary distributions of SST-NO_x and obtaining expected ozone reductions for each.)

These limitations to Johnston's model can be interpreted in several ways. One argument or statement of faith has been that a more detailed consideration of winds and turbulence will lead to results showing a smaller effect of SSTs on ozone. An alternative argument is that there is no particular reason to expect comprehensive calculations to show the effect to be either larger or smaller. Those generally backing the SST tend to use the first argument, and those opposing it the second. (There are other, in my opinion generally inconclusive, technical arguments on which a stance on this issue can be based.)

The use of the interpretation of the limitations of photochemical modelling applies to other limitations as well. In general, those emphasising the environmental safety of SSTs interpret every limitation of the work of Johnston, Crutzen, and others of a similar nature, as showing that the conclusions are exaggerated and probably do not apply. Those emphasising the environmental danger of SSTs interpret limitations in SST-NO_x-ozone studies as showing that not enough is yet known to absolve the SST of danger.

Subsonic and military flights in the stratosphere, and ozone increases

Item: There has been and continues to be some military supersonic flying in the stratosphere, and an increasing amount of commercial subsonic flying in the stratosphere. Item: During the decade 1960-1970 there apparently was an increase in average ozone levels on a global scale.

Those people emphasising the environmental safety of SSTs tend to mention these two items together, suggesting implicitly or explicitly that if NO_x injected into the stratosphere by aircraft actually affected ozone levels significantly, then some effect would have been noted already.

Those people emphasising the environmental danger of SSTs often argue that proposed SST fleets would emit many times as much NO_x into the stratosphere as has military supersonic and commercial flying in the past. Therefore, they say, the 1960's ozone increase is not indicative of the potential effect of a major SST fleet.

The military supersonic flight issue seems ideally designed for discerning presuppositions. This is because data on the extent of such flights is extremely scarce due to military security. Therefore the advocates of the environmental safety or danger of SSTs can freely interpret the evidence according to their presuppositions. There are plausible arguments on either side, but little that can be firmly tested. For example, opponents of SSTs can note that military supersonic aircraft are much smaller and have shorter flights than SSTs. Proponents can refer to the enormous sizes of military fleets, and the large number of flights which are part of military preparedness for war. My impression is that people first develop notions about the safety or danger of SSTs, for whatever reason (because they are pro- or anti-technology, or live in the U.K., or dislike sonic boom). *Then* they assume an extent and an interpretation of the effect of military flying in agreement with these notions.

There is also a difference between the groups about the significance of the 1960's ozone increase. Those emphasising the environmental safety of SSTs usually assume that the ozone increases during the 1960's show that SST-NO_x or even NO_x in general cannot be all that vital in determining ozone levels. In contrast, those emphasising the environmental danger of SSTs note that no one really knows why mean global ozone levels had been increasing. Some even dispute that there had been a significant increase at all. They use this lack of scientific understanding to argue that SSTs cannot be shown to be environmentally safe if actually occurring ozone changes, apparently due to natural causes, cannot even be explained.

Reversibility of changes in ozone

Proponents of SSTs often suggest that if ozone levels are carefully monitored, and if a reduction in ozone levels correlated with SST flight in the stratosphere were observed, the amount of stratospheric SST flight could be reduced in time to avoid any dangerous consequences. On the other hand, opponents of SSTs tend to note uncertainties in ozone levels and the determinants of these levels, to note the long time needed to verify any effect due to SST-NO_x, and to note the possibility of irreversible or long-term effects due to NO_x deposited in the stratosphere. These are all reasons to doubt that SSTs would be or could be withdrawn in time to avoid any danger to ozone that developed from an SST fleet.

Discussion

For each of the above classes of evidence, the evidence may be interpreted to suggest either the safety or the danger of SST exhausts interacting with the upper atmosphere. Almost every one of the papers classified in Table A uses evidence from several categories to consistently suggest either safety or danger, but not both. This consistent use of evidence strongly suggests that presuppositions about what it is considered necessary to prove about the safety or the danger of SST exhausts underlie the use of evidence in each paper.

Some papers give a reasonably explicit statement of their presupposition about what it is necessary to prove, and these are also listed in Table A. For example, Swihart says, "A study . . . reveals no basis for these claims [that environmental effects of SSTs could be serious]. However, in some cases more data is required to show that there is no effect." This may be contrasted with McElroy et al.'s statement that "Stratospheric aircraft, injecting major quantities of NO into the atmosphere, represent a potentially important environmental hazard. Their development and operation should continue to be viewed with caution."

Even in cases where my categories do not demonstrate this clearly, my impression is that most of the papers cited in Table A use evidence mainly in such a way as to promote a certain viewpoint. For example, some authors accord more importance to some categories of evidence than others, especially by referring to some evidence and not mentioning other, usually conflicting, evidence. Even when evidence on both sides of an issue is presented, one side may be favoured by being given a more extended discussion or being referred to with a favourable tone. For example, the article by Goldburg considers at great length reasons why Johnston's model of ozone is inadequate and hence overestimates the effect of SST-NO_x, but does not at all mention counterarguments or strengths of Johnston's argument. It seems reasonable to infer that Goldburg, like most of the other scientists in Table A, has used evidence in a way that reflects his underlying presupposition about what it is considered necessary to prove.

Summary

Presuppositions underlying a scientific argument may be inferred by looking at how certain ambiguous pieces of evidence are used. In the field of SST-NO_x-ozone there are many such areas, and I have discussed several of them. In a similar way presuppositions might be inferred by looking at technical assumptions, selective considerations of uncertainties or other apparent pushings of the argument.

Reference notes

In referring to natural variations in ozone levels, different workers appear to refer to different types of variations. The ozone level at a given place may vary from day to day or even hour to hour by from a few percent up to 50% (see Dütsch, 1969, 1974); the monthly means vary from season to season by say 25% (see Craig, 1965); the yearly means vary randomly from year to year and also show long term trends (see Goldsmith et al., 1973; Johnston et al., 1973). At a given time, ozone levels at different stations typically differ by as much as day to day fluctuations at a single station.

For obvious reasons, there are few references to the amount of military supersonic flying in the stratosphere. One source I did come across suggests that the amount of NO_x injected by military flights in the stratosphere is probably less than that injected by 10 to 20 operational Concordes (British Aircraft Corporation/Aerospatiale, 1973). The amount of commercial subsonic flying in the stratosphere is analysed by Anderson (1973), Jocelyn et al. (1973) and English (1974).

There are various proposed explanations of the apparent increase in mean global ozone levels during the 1960s. Besides Johnston et al.'s suggestion that it may represent in part recovery from catalytic destruction of ozone by NO_x released by nuclear tests up to 1962, long term changes in ozone levels have been correlated with changes in solar radiation, atmospheric dynamics, the tropopause height, and the sunspot cycle, among others (see Komhyr et al., 1971; Angell and Korshover, 1973; Christie, 1973; Pittock, 1974; Johnston, 1974a).

The quotes of the explicit statements of presuppositions are found in Swihart (1971, p. 93) and McElroy et al.. (1974, p. 301).

Mazur (1973) describes some of the different ways of interpreting evidence and displaying data used by scientist advocates on either side of the fluoridation and low-level radiation controversies; Diesendorf (1975) describes a number of ways of presenting evidence used by scientists who are attempting to minimise the importance of the effects of ionising radiation. A study of these methods of pushing through use of evidence is useful in determining presuppositions in scientific articles.

Chapter 7: The psychological and sociological context of pushing

I have claimed that pushing of scientific arguments is inevitable. Yet, contained among the standard stereotypes about scientists and scientific development are images of the disinterested and dispassionate scientist, and of the steady accumulation of scientific knowledge by a scientific community dedicated only to achieving that knowledge most efficiently. In terms of such stereotypes, pushing of a scientific argument certainly would be unusual, if it were not immediately condemned as unscientific.

The point of this chapter is to survey evidence that suggests that pushing of scientific arguments is to be expected and accepted rather than unexpected and rejected. There is good evidence for the view that individual scientists are strongly committed to particular scientific theories, to certain orientations towards the world, and to special types of conclusions. On the level of the scientific community and scientific knowledge as a whole, there is good evidence for the view that there are no unambiguous and indisputable criteria for choosing between rival scientific theories; that the development of science is uneven, hesitant, and based to a certain extent on acts of collective faith or commitment; and even that there are no hard and fast rules for distinguishing between scientific beliefs (scientific knowledge) and other belief systems.

Commitments of scientists

Most scientists strongly back their own ideas, results, and theories. The most direct and dramatic analysis of this facet of scientific work is Ian Mitroff's *The subjective side of science.* Mitroff studied about forty scientists who were involved in studying rocks from the moon. He found that the scientists tended to be strongly committed to, or biased in favour of, certain theories and certain interpretations of experimental evidence. The scientists did not keep an open mind to all possibilities, but rather looked at data selectively, interpreting it and using it to bolster their own viewpoint whenever possible. In agreement with what one would expect on the basis of this commitment, Mitroff found only a relatively small change in the opinions of scientists about the validity of various theories after new evidence, based on the study of rocks from the moon, became available. Mitroff also found that out of his select group of first-rate scientists, those considered by their peers to be the most outstanding scientists also tended to be considered to be the most deeply committed or biased of the group. Finally, not only were the scientists quite aware of the commitments of their colleagues, but they believed that this sort of commitment was necessary for the successful practice of science.

People tend to selectively observe and interpret information in a way that supports their preconceived ideas. Because of this, the personal commitments of individual scientists can help to explain the link between the scientists' presuppositions and their pushings of the argument.

In a scientist, this process might operate as follows. The scientist starts with an original idea or hypothesis, perhaps arrived at as a creative solution to a certain problem.* In testing or validating the idea, the scientist will tend to notice and use supporting evidence and arguments. Data that seems mainly supportive will be studied, analysed and applied so that every possible advantage can be drawn from it. Seemingly irrelevant or inconclusive items will be filtered from advantageous components, or interpreted in a way which promotes the argument. Evidence that seems mainly to contradict or challenge the argument at hand may be ignored completely or explained away or reinterpreted and twisted into support for the argument.

Some of the ways in which a person may deal with a challenging item of information are: (1) flat denial of the item; (2) scepticism about the source of the item; (3) ascription of a motive to the source of the item; (4) isolation of the item from the context of one's attitude; (5) minimisation of the importance of the item; (6) interpretation of the item to suit one's purpose; (7) misunderstanding of the item; and (8) thinking away or just forgetting the item. Several of these processes seem to be at work in Johnston's interpretation of the ten times factor for high density air traffic, Goldsmith et al.'s interpretation of fluctuations in ozone, Johnston's denial of the validity of contradicting results in his note 5, and Goldsmith et al.'s examination of ozone records.

All of the 8 ways of dealing with a challenging item of information occur frequently in science. Here I'll give only a couple of examples. Concerning (3), in science it is considered improper to ascribe motives, so it is difficult to find examples except in informal conversations with scientists and in letters and editorials. The best example I have found in a scientific publication is this excerpt from an article by Scorer: "The environmental questions raised by the possible operation of supersonic transports are very largely misunderstood"; "The misunderstandings result to some extent from the deliberate efforts of some scientists, who, for non-environmental reasons . . . bend their science on behalf of their cause"; "They receive much encouragement to do this from people who imagine that their moral motives are of some superior merit". In a cutting rejoinder, Adams concludes with these same quotes, and adds, "on these points we are in complete agreement."

Concerning (6), interpretation of the item to suit one's purposes, McElroy et al. in a study of the effect of SST-NO_x on ozone comment that the results and conclusions of Crutzen and Johnston "were clearly imprecise and subject to interpretation at the discretion of the reader. In view of the political volatility of the issue, it is perhaps not surprising that this discretion was exercised widely and diversely by different readers!"

Selective observation and interpretation of information also can come about through presupposing what is required as a result of the study and thereby excluding much conflicting evidence, and through making assumptions about the appropriate means for studying a problem and thereby avoiding the necessity for a detailed consideration of contradicting evidence. For example, Goldsmith et al. assume that ozone models such as Johnston's had not yet given useful (that is, conclusive or accurate) results concerning the effect of SST-NO_x on stratospheric ozone. (Goldsmith et al.: "It is fair to say that none of these papers [by Johnston and by Crutzen] contains the necessary full, quantitative consideration of the interaction of radiation, photochemistry, and the atmospheric circulation" (Page 545, column 1). This implies that any 'simple' theoretical ozone model, containing less than a full, quantitative consideration of the problem, cannot give useful results for their purposes. Thereby,Goldsmith et al. excuse themselves from a further consideration of the results of the theoretical ozone models of Johnston and Crutzen.

This is not to say that scientists only notice supporting information, or that they misinterpret all evidence that conflicts with their preconceived ideas. This is only what *may* happen in the less well-defined aspects of the problem and the less clear-cut areas of the analysis. Nor is selective observation and interpretation of information something that is obviously incorrect according to some supposedly neutral observer. All observation is selective, and evidence must always be interpreted according to some basic assumptions. There is no position, itself free of presuppositions, from which all pushing may be evaluated.

One result of the commitment of scientists to their ideas, and of their selective observation and interpretation of information to support their ideas, is that often their opinions remain relatively unchanged in the face of opposing arguments and alternative interpretations of data.

Johnston is one of the most prolific workers in the area of SST-NO_x-ozone. Since his original 1971 papers in the field, he has written again and again on the subject. In each of these papers and articles he has held firm to his original views.

This is not to say that Johnston has not modified his views at all, or not taken cognisance of new evidence. Rather it seems to me he has consistently looked at new evidence from a certain point of view, a point of view which incorporates the presupposition about the burden of proof. As a result, he has consistently arrived at conclusions that differ in a particular direction and emphasis from the average or typical scientific opinion in the field, namely towards demonstrating and emphasising the possibility of a large reduction in ozone.

For example, Johnston states, "In 1974, as in 1971, it appears that 500 Boeing-type SSTs would cause a major reduction in stratospheric ozone." This may be compared with the report of findings of the major U.S. study programme into the effects of SST exhausts on the upper atmosphere. This report I call a consensus presentation since it involved many scientists and reviewed the work of many scientists. It gives figures for estimated percent ozone reductions per 100 aircraft, with or without emission index controls, and for subsonics, Concorde, and advanced SSTs. The values range from 0.000070% reduction for 100 subsonics of the 707/DC-8 type with 1/60 of present NO_x emissions, to 1.74% reduction for 100 advanced SSTs without emission controls. For the same level of NO_x emissions into the stratosphere, Johnston obtains a somewhat larger reduction factor than the consensus presentation, mainly for various technical reasons. More significant is the consensus presentation's use of figures for 100 aircraft rather than a full fleet, their emphasis on the possibility of emission controls, and their reference to different types of aircraft (subsonic and supersonic, Concorde and advanced SSTs) individually rather than collectively. Johnston concentrates on the possibility of danger, while the consensus findings are oriented towards the most likely probabilities. Although the comparison can hardly prove my point about the continuity of Johnston's orientation, it is certainly compatible with it. My impression is that this comparison is typical, and that there is a strong continuity of attitude in Johnston's work. This continuity can be interpreted

*Scientific creativity is not a logical or planned process, but rather an intuitive and spontaneous affair. Therefore scientific creativity seldom is based on a reflective consideration of the presuppositions inherent in the problem at hand. Instead, it reaches a solution in the *context* of underlying assumptions. On the basis of an inspiration or insight, a scientist may become convinced of a certain conclusion, only afterwards work out arguments or make investigations in the hope of justifying it, and finally as an afterthought consider limitations of the study.

This sequence may apply to Johnston, who after noting his involvement with chemical reactions involving O_3 and NO_x since the late 1940's, says, "When in March 1971 I was presented . . . with the SCEP estimate of 7 to 70 ppb NO_x in the stratosphere and the statement that it 'may be neglected', I instantly knew that there was a contradiction in these two statements."

as being evidence for a strong commitment or bias in favour of a certain way of viewing the problem.

The commitment of Goldsmith et al. to their point of view may be inferred from their response to criticism. Their work was first presented at a conference in April 1973. Johnston attended the conference and soon after Goldsmith et al.'s presentation offered a number of criticisms. Some of these criticisms concerned one aspect of Goldsmith et al.'s calculation of the amount of NO_x produced in a nuclear blast.* In spite of these criticisms, the version of Goldsmith et al.'s paper in *Nature* is virtually unchanged from the version submitted to the conference. The only change relating to Johnston's criticism is the addition of one long sentence to the *Nature* version (the third sentence after the list of equations, page 546). The authors make no attempt to meet Johnston's arguments with quantitative calculations, or to estimate the error introduced into the calculation by the assumptions which Johnston attacked. This may be interpreted as being evidence that Goldsmith et al. were so committed to their own ideas that they were unwilling or unable to give serious attention to alternative evidence or viewpoints, or to let it be known that the objection might be important.

Thus, by looking at a series of papers by a given author, or by looking at different versions of the same paper by a given author, it is noticeable in many cases that there is little change in attitude even in the presence of strong conflicting and challenging evidence. This constancy of attitude may be interpreted as being due to the presence of personal commitment or bias. This commitment or bias in turn may result at least partly from unconscious adherence to certain fundamental assumptions and orientations, from a certain way of viewing the world.

The idea that scientists are often strongly committed or biased is quite compatible with the fact that scientists are human beings. And it is important to remember that scientists *are* only human. That is, they are subject to motivations and failings similar to those of other people. They may strive for money, power and prestige; they may work for the satisfaction of a job well done or for revenge or to relieve boredom; they may make terrible blunders as well as have brilliant insights.

It is sometimes said or suggested that scientists, at least when it comes to their work, live on a higher moral plane than other mortals. Don't believe it! Those scientists who get involved in scientific controversies, or who work on research where important discoveries and fame are a possibility or who serve on committees where decisions are made, will be aware that in doing their work scientists are no better morally than other people doing their own jobs. Many scientists attempt to claim undue credit for scientific discoveries, take credit for other people's work, use personal influence to grab funds for personal research and deny them to competitors, use the physical and intellectual labour of colleagues and students to further their own career, slander colleagues and other disciplines, block appointments, etc. These sorts of activities are not common knowledge, as the scientific community looks after its image. In the same way that other groups with vested interests (such as corporations, doctors or ministers) carefully filter what information is leaked to the general public, so scientists promote the image of the honest, generous, retiring, public-spirited scientist.

Commitment of the community of science

Just as individual scientists are strongly committed to particular theories and orientations, so the community of scientists in a research speciality or discipline as a rule is strongly committed collectively to a certain orientation or paradigm. Transitions from one paradigm to another often are made on the basis of changing commitments, rather than upon any hypothetical absolute criteria for judging between scientific theories. These collective commitments are likely to reflect values built into current political, economic, and social arrangements in society, as well as factors such as intellectual fashion and the self-interest of the scientific community.

The now classic work in this area is Thomas Kuhn's *The structure of scientific revolutions*. Kuhn argues that within a given research speciality or discipline, work is governed by a paradigm: a set of prescriptive guidelines (often implicit) about the way the world is, what questions are worth asking, what concepts are useful, what techniques of investigation and analysis are valid, etc. Most scientific research does not attempt to challenge the basic assumptions underlying the speciality or discipline, but works within the assumptions, elaborating the paradigm to cover more detailed and more diversified evidence. With any paradigm there will be a certain amount of evidence apparently incompatible with, or unexplained by, the paradigm. These anomalies do not cause the paradigm to be rejected immediately. Sometimes they stimulate more detailed research; at other times they are assumed to be incorrect or irrelevant and therefore ignored.

A given paradigm is passed on from one generation of scientists to another through textbooks and personal instruction through apprenticeship and by interaction among working scientists. Learning how to do scientific research, which means such things as learning techniques, how to formulate questions, criteria for evaluating evidence and how to develop plausible hypotheses, is by its nature a craft activity. Much of what is involved in doing science cannot be stated explicitly; it is tacit knowledge, learned by watching other scientists and by doing science oneself. Part of the tacit knowledge learned by the scientist during the long period of training and apprenticeship will be gained through unconscious assimilation of the basic ideas of the prevailing paradigm.

Occasionally an established paradigm is challenged. A new viewpoint is offered, with a new way of looking at the world and categorising its important features, new techniques, new criteria for evaluating evidence, etc. It is *not* the case that the new paradigm explains everything in the old paradigm, plus a bit more. Instead, the new paradigm reclassifies what is important and what is not, and changes the very meanings attached to various aspects of the world. What were anomalies in the old paradigm may be simply explained in the new one, *and* vice versa.

Kuhn applied his idea of paradigms and paradigm change in a reinterpretation of the history of science, for example analysing the Copernican revolution. The question arises: are there any ultimate scientific criteria, such as explanatory power, beauty or simplicity, for judging between competing paradigms? I go along with the view that there aren't any. Paradigm change seems to require some shift in commitment by the community of scientists involved. This change in commitment cannot be justified in terms of scientific principles higher than and independent of the paradigms themselves. A consequence of this is that sometimes a new paradigm is adopted by the scientific community even though adherence to the previously established paradigm was 'rational' and 'justified' in terms of current scientific evidence. For example, in 1600 adherence to the idea of an earth-centred universe and to Aristotelian physics could be strongly justified in terms of the then prevailing knowledge and understanding of the universe. The success of the ideas of Copernicus and Galileo depended on a change in commitment by leading scientists and intellectuals

*Namely, Goldsmith et al. specify certain temperatures and pressures in the blast — which they take from another study — and calculate the amount of NO_x produced from reactions taking place within these assumed conditions. These reactions however would absorb energy from the blast and hence change the temperature and pressure. Johnston claimed that the amount of aborbed energy would so drastically change conditions that the amount of NO_x produced would be much reduced. Goldsmith in reply denied this.

which could not be justified solely in terms of evidence and rational argument.

If scientific theories cannot be justified by any absolute criteria outside the commitment of the scientific community, what is the basis for the scientific community's commitment? I would say that this commitment is strongly influenced by values — social, political, economic, and other values — that are built into scientific practice, into the organisation of the scientific community, and into scientific knowledge. In part IV, I describe some of the ways in which such values are built into science.

Kuhn's ideas have been argued, analysed, elaborated, qualified and challenged on many points. For example, critics have noted that there is certainly some communication between representatives of competing paradigms, and that new scientific theories may develop from application of ideas from other scientific disciplines without a scientific revolution. However, the usefulness of the idea of a paradigm and the commitment of the scientific community to it seems widely accepted. For my purposes of showing the link between presuppositions and pushing, it is only necessary to note a few further points.

First, the commitment of individual scientists seems to be the natural analogue of the commitment of groups of scientists. In the latter case the viewpoints adhered to will be more basic and longlasting. This suggests that it may be useful to look for presuppositions underlying science in the same way as looking for presuppositions underlying the work of Johnston and of Goldsmith et al. This is the subject of Part IV. Second, the presuppositions of individual scientists can be seen as growing out of or being based upon presuppositions inherent in a paradigm. This suggests that fundamental assumptions underlying scientific knowledge and scientific practice are worth studying, for they may not be necessary assumptions. Because these assumptions are not absolute, features of science may be strongly linked with features of the societies in which it developed and with the society with which it coexists and interacts.

Presuppositions and perception

Certain analyses of perception provide an excellent analogy to the idea that the work of individual scientists and of research specialities and disciplines is based on presuppositions. There is much evidence to suggest that reality cannot be perceived directly. There is no such thing as an ultimate sense datum. Physical perception of the outside world is contingent: it depends on experience and context. A person with one set of background experiences may 'see' and interpret the world entirely differently from another person with different background experiences.

The reason for this difference in perception primarily lies in a difference in the system of interpretation, or schema, which structures the perceptual field. The interpretative scheme filters, organises and classifies the field of experience. There are no individual, indivisible sense impressions; each bit of the field depends on the rest. Perception can be interpreted as being based on certain perceptual hypotheses about the way the world is. Given these hypotheses one may interpret and make sense out of the world. But with a different set of hypotheses different perceptions will result.

The crucial nature of experience is demonstrated by the experiences of congenitally blind people when they first gain their sight. These people can come to make visual sense out of the world only with great difficulty and after considerable time and practice and training. For example, at first they cannot distinguish anything recognisable from the visual field; they cannot tell a triangle from a square. Seeing in the normal sense is something that is learned.

Experience is doubly important because the nature of the perceptual hypotheses one can entertain depends on brain development. The wiring of the brain depends on one's experiences, and in turn restricts the future experiences of which one is capable. For example, cats which are denied the experience of self-controlled locomotion, but which are given a variety of visual environments, do not learn how to make sense of their visual environment — they remain effectively blind.

The crucial nature of context has been demonstrated by many experiments with illusions and distorted figures. For example, suppose you see a room which appears rectangular, and two people, each in a different far corner, one of whom appears twice the size of the other. Most people see this in terms of the hypothesis that the room *is* rectangular; the relative size of the two occupants is unexpected and inexplicable. Few people spontaneously entertain the alternative hypothesis that the room is distorted, but seen from a place that makes it appear rectangular, and that one person is farther away than the other. In short, there is an hypothesis involved in understanding the situation meaningfully.

The dependence of perception on experience and context applies by analogy most directly to questions of the stability of scientific facts, data or evidence. Scientific facts are not independent of the interpretative scheme by which they are apprehended. The status, implications and significance of a scientific fact may change drastically when it is interpreted from a different viewpoint. Therefore the existence of a scientific fact necessarily requires a theoretical orientation; the difference between fact and theory is one of degree rather than of kind. While facts are usually more stable and well-established than theories, if necessary any fact may be rejected or reinterpreted to preserve features of a total explanatory system.

Also, just as perception depends on experience and on context, so the development of scientific theories depends on paradigms. Before evidence can be evaluated, problems specified or experiments planned, a general interpretative context — a paradigm — must be specified. Current scientific research is based upon past work or experience in the field, similarly to the way that perception depends on previous experience in perceiving.

Actually, the relation of presuppositions to perception is more basic than one of analogy. Perception is at the root of the scientific enterprise, ranging from meter readings to the interpretation of mathematical equations. I have said that physical perception is socially dependent: it is dependent on one's upbringing, experiences and social role, as well as on the physical structures produced by society. Similarly, meter readings are socially dependent, in that they are based on humanly constructed apparatus built to interact with the world in particular ways. The interpretation of mathematical equations is socially dependent, in that it is influenced by human abilities for concept formation, which are moulded by methods of child rearing and by the physical and social organisation of society.

The essence of accepting an argument is seeing things from a certain viewpoint. Therefore, 'arguments' often consist of various statements, seemingly dogmatic, of the way things are as seen from the certain point of view being argued. Often the 'argument' consists mostly of repetitions of rather arbitrary statements (arbitrary from other points of view). Hopefully this gets the reader to accept the point of view implicit in the formulation of the statements, to reorganise the conceptual orientation towards the statements and to accept the point of view. Thus, by being based on a certain point of view, any argument is automatically pushed, in the sense that one is encouraged to think in terms of that point of view. This process is one means by which arguments of Johnston and of Goldsmith et al. encourage the reader to accept their respective presuppositions about what is considered necessary to prove. It is also apparent to me that in this book my 'argument' often consists of repeatedly stating things from my perspective.

Summary

Scientists usually are strongly committed to certain theories and ideas. The commitments and preconceptions of scientists strongly influence their selection and interpretation of evidence, and lead to a distinct continuity of viewpoint in the works of many scientists. The commitments of scientists are only one aspect of the fact that they are human and have typical human motivations and biases.

On a wider scale than the individual, research groups may be committed to certain techniques or ideas, and entire scientific disciplines may agree about fundamental aspects of their enterprise. There is not necessarily any method, free of fundamental assumptions itself, for deciding between the claims of research areas, the paradigms of larger research communities, or the fundamental attitudes which guide the quest for knowledge in different cultures or over different historical epochs.

Perceptions depend on experience and context. The contingency of perception serves as a good analogy to the limitations of both individual scientists and of scientific development as a whole.

Reference notes

Mitroff's (1974) book is for me the most direct, complete, and dramatic study of the extent of commitment in the day-to-day work of scientists. Also excellent are: Watson (1938), who delves into the importance of human abilities, drives, and experiences for scientific 'truth', as well as for scientific practice; Polanyi (1951, 1958, 1966), who argues that personal commitment and indeed dogma (see also Kuhn, 1963) is inevitable and also absolutely necessary for the effective operation of science; and Mahoney (1976). The best case study of dogmatic behavior by scientists is de Grazia (1966), who tells of the scientific community's unbelievably repressive treatment of Immanuel Velikovsky and his ideas (a comprehensive catastrophist interpretation of history, geology, astronomy, etc.).

In psychological studies of science, Roe (1952, 1961) has documented the extreme emotional involvement and intense devotion of top scientists to their work; it seems that such involvement and dedication is a prerequisite of major contributions. She also states (1963, p. 137) that, "It is inevitable that anyone who is very deeply involved personally in his work will be biased. It is impossible to avoid this . . ."

The importance of psychological attitudes in shaping desires to gain knowledge of only a particular kind is analysed generally by Maslow (1966), and more specifically by Kubie (1953-54) and Storr (1972).

For examples of the operation of scientific creativity, see Hadamard (1949) and Koestler (1964). The quote concerning Johnston's insight about SST-NO_x is from Johnston (1972, p. 276).

For general analyses of the ways that people selectively observe and interpret information in a way that supports their preconceived ideas, see Abelson et al. (1968), Barber (1973), Edelman (1971) (recommended), Hovland et al. (1953), Jervis (1968) and Smith et al. (1956) (which gives the list of eight ways in which a person may deal with a challenging item of information). In science, other factors besides selective observation and interpretation are undoubtedly important too. For example, scientists may do research in an environment which provides easier access to information supporting their own ideas.

The quotes from Scorer and Adams concerning ascription of motive are from Scorer (1972, p. 521) and Adams (1972). This encounter is continued in Adams (1972a), Scorer (1973a) and Adams (1973). The quote concerning interpretation of Crutzen's and Johnston's results is given in McElroy et al. (1974, p. 288).

The quotes from Johnston and the figures from the 'consensus presentation' are from Johnston (1974a, p. 171) and Grobecker et al. (1974, p.xvi). A study of the continuity of Johnston's orientation could be made on the basis of Johnston (1971, 1972, 1974, 1974a) and Johnston et al. (1973a, 1973).

Goldsmith et al.'s material was first presented at a conference in April 1973 (Goldsmith et al., 1973a). Johnston's criticism of their calculation is incorporated in the 'discussion' following the paper as reproduced in the proceedings of the conference (Goldsmith et al., 1973a). Also, there was a newspaper article about Goldsmith et al.'s conference presentation (Silcock, 1973); Johnston criticised Goldsmith et al.'s calculation in a following letter (Johnston, 1973). Speaking for his group, Goldsmith did reply to these criticisms in some detail in the printed discussion in the proceedings of the conference. However, only a tiny minority of those reading the *Nature* article would even be aware of the conference presentation, much less be able to read Goldsmith's reply to Johnston. Even if the validity of Goldsmith's reply were granted, its absence in the *Nature* article constitutes pushing of the argument through selective (non)reference to alternative arguments.

Another example of continuity in the orientation of scientific work in the face of criticisms is found in two versions of a paper by Foley and Ruderman (1972, 1973). The second version is significantly changed in many technical aspects, partly as a result of criticisms by Johnston et al. (1973a), but the continuity of the presupposition about the implications of the results is striking.

The fact that scientists are humans driven by prosaic human motivations and subject to conventional human failings is not an overly popular subject of study. Mitroff (1974) came across a number of interesting findings, for example the prevalence of the stealing of credit for ideas and results by scientists. Gaston (1971, 1973) gives many astounding examples of this sort of thing. St James-Roberts (1976) reports on the results of a questionnaire about the faking of research findings. There are also a number of documented individual cases of fraud (Wade, 1973; Kamin, 1974; Gullis, 1977; Smith, 1977), denial of proper credit for discoveries, and opposition to unwanted research work (Springell, 1976).

The few documented cases of stealing credit for ideas, blocking careers and the like are only the tip of the iceberg — most scientists hear of numerous cases through the local grapevine. One sceptical reader of a draft of this chapter demanded further substantiation of this claim. The best I can do is to present a selection of stories typical of those that I have heard. (1) A researcher submits a paper to a journal. It is held up and then rejected due to a referee who at the same time is publishing similar work for which total priority is claimed. (2) A Ph.D. applicant is required to virtually rewrite the Ph.D. thesis. The scientists who require this are nicely placed to follow up promising leads opened up by the thesis work. (3) A scientist talks to a colleague about personal research ideas, and only later finds that the colleague has published work based on the ideas, with no acknowledgement whatsoever. (4) A scientist reads a completed research paper, adds a couple of sentences, and decides to become a joint author to the paper; the necessity for getting recommendations makes it unwise for the original author to object. (In none of these cases are there any extenuating circumstances. For example, the Ph.D. applicant had published several papers in prestigious journals.) These sorts of stories are not restricted to minor aspects of research: they may involve work awarded with a Nobel Prize, or the destruction of a scientist's career, as well as partial credit for a minor research paper. But neither are scientists *consciously* as vicious as such activities may imply. Rationalisation and selective forgetfulness make it relatively easy for scientists to justify their activities to themselves.

The very human motivations and failings of scientists also are documented in the history of science, for example in the long history of fierce priority disputes (see Merton, 1973) and resistance to new ideas (Murray, 1925; Stern, 1941; de Grazia, 1966). Watson's (1968) popular account of the discovery of the structure of genetic material includes considerable reference to the importance of such factors as competition and the desire for prestige. (See Sayre (1975) for a complementary account of this episode.) Novels (Cooper, 1952) also testify that scientific practice is more than just the disinterested search for truth. Van den Burghe (1970) provides an amusing and biting satiric attack on the myths of the cloistered academic life of research and teaching, while Bensman (1967, pp. 125-159) gives a general account of the standard of ethics in academia.

Ridgeway (1968) gives examples of some of the grosser abuses of what is considered proper academic behaviour. A common failing is the compromising of academic competence by monetary considerations. For example, scientists may testify before government committees, overtly as disinterested experts, and not reveal that they are being paid by commercial or government interests. It is noteworthy that scientists with established reputations seem to behave this way at least as often as less prestigious scientists.

Kuhn's approach is given in his original book (1970). His basic orientation to the history and philosophy of science is shared by Norwood Russell Hanson, Stephen Toulmin, Michael Polanyi, and Paul K. Feyerabend. Some valuable critiques and extensions of Kuhn's ideas are Lakatos and Musgrave (1970), King (1971), Martins (1972), Tribe (1973), Young (1973), Bourdieu (1975), and Chalmers (1976). Kuhn's ideas are very popular in numerous fields outside of the natural sciences, which suggests that many people find that the idea of a paradigm usefully describes their own experiences. For example, Easlea (1973), in an excellent exposition of Kuhn's orientation, applies the ideas to economics.

Barnes (1972, 1973, 1974, 1977) takes Kuhn's ideas to their logical conclusion in claiming that there are no independent criteria for distinguishing scientific beliefs from other belief systems. Barnes presents strong arguments that the conventional criteria for choosing between theories (explanatory power, simplicity, etc.) are not independent of belief systems. Barnes' work leads one to a group of anthropological studies (see Barnes (1974) for references), which by analogy can also throw light on the scientific enterprise. (This anthropological analogy is probably just as useful in its own way as is the perceptual analogy I have presented.) Barnes points out the fallacy of rejecting relativism on the basis of circularity; his own work illustrates how a self-consistent and self-reflective relativistic perspective may be developed.

Mitroff (1974) argues that scientific knowledge is attained through a sort of adversary proceeding, so that the commitments of individual scientists — which lead to full and exhaustive analyses of alternative possibilities — are *necessary* for the efficient attainment of scientific knowledge. But while commitment may be valuable for scientific practice, this is no reason to doubt the existence of collective commitments made on the basis of social or political values.

The craft nature of scientific research is excellently described by Ravetz (1971); the tacit nature of much knowledge of scientific practice is excellently described by Polanyi (1958, 1966).

For the dependence of perception on experience, see Gombrich's (1960) study of the history of art, and Gregory's (1966, 1970) studies of visual perception. Also excellent reading is Abercrombie (1960), who surveys evidence showing the contingency of perception and describes a discussion course based on studying perception and reasoning.

There are lots of intriguing examples of the importance of learned modes of experience on the way we believe the world is, the way we work in it, etc. Besides the blind people who can't 'see' objects when they first gain their sight (von Sedden, 1960) and Ames' distorted room experiment (see Ittelson, 1952, for details, or about any book on illusions and perception), I have come across: the Yaqui socerer's interpretation of experience (Castaneda, 1968, 1971, 1972, 1975); the illiterate eskimos who are unsurpassed in fixing machines (Illich, 1971); the Fuegians visited by Darwin who could not 'see' the ship at anchor in front of them (Polanyi, 1951, p. 19); and the biology students who cannot 'see' chromosomes through a microscope until told what to look for. See Held and Hein (1963) for the relation of locomotion and sight in cats. (Robyn McClelland provided valuable help in obtaining this and the reference on the blind people.)

Tajfel (1969) reviews research on the influence of social and cultural factors on perception. McLuhan (1964, p. 301) suggests that quantum mechanics was developed by physicists from central Europe because they were not encumbered by a visually oriented mode of thinking. Hanson (1958, 1969) closely links his criticism of the sense-data theory of perception with his criticism of the observation theory of knowledge. The analogy of perception to the development of scientific theory is made by Harris (1970), who also refers to a number of other pertinent works in the theory of perception.

The usefulness of the analogy between science and perception is highlighted in several of the articles in Gregory and Gombrich (1973). In particular, Gregory uses knowledge of types of error in science to give insights about the types of error in perception; Gombrich notes that the need of an organism to predict, and not just to respond to stimuli, demonstrates the need for perceptual hypotheses.

The relationships between hypotheses about the world in perception and in science are given a detailed treatment from the physical scientist's viewpoint by Bohm (1965).

The claim that there is no difference in kind between facts and theories has been argued by Feyerabend (1965, 1970, 1970a, 1975) and Hesse (1974), among others. Polanyi (1946, 1951) is very good on illustrating how 'facts' are not enough to dislodge theories. Hesse (1974) shows how a framework can be developed in which some observation statements are relatively entrenched or established, but any observation statement may be rejected to maintain the economy and coherence of the total system. In this framework, all observations are theory-laden, but not any theory can be imposed on the facts, nor is there any vicious circularity. Barnes and Law (1976) carefully argue that the meaning of any statement, scientific or otherwise, observational or theoretical, depends on its context, and that this meaning is revisable rather than fixed.

Chapter 8: How widespread are presuppositions?

In brief, my viewpoint is this: the presuppositions of Johnston and of Goldsmith et al. about the burden of proof are typical of a kind of presupposition commonly underlying scientific research; but they are atypical in being fairly easily recognisable. That is, other scientific research is likely to be based on similar sorts of presuppositions. But most such presuppositions are likely to be embedded in the context in which the research is done, and hence to be less recognisable than the burden of proof in SST-NO_x-ozone research.

Let me give a few examples of the kinds of presuppositions that may arise. First, assumptions about the burden of proof are common in many fields, such as the safety of radiation standards, nuclear reactors or food additives. Second, it may be assumed that a certain mathematical representation of nature and associated philosophical approach is superior to other methods of understanding; related to such an assumption, Isaac Newton apparently fudged many of his experimental and theoretical results to get good agreement. Third, in the area of I.Q. it may be assumed that intelligence is determined mainly by environment or mainly by heredity. Fourth, assumptions about consumer preferences may affect the results of technical calculations, as in the case of the analysis of nuclear power plant construction programmes. Fifth, in technological forecasting it may be assumed that the future is a simple extrapolation of the present, or alternatively any of a number of limiting constraints or policy options may be introduced. Sixth, in many areas involving public health assumptions may be made concerning the values of mass preventative treatment.

You can tell from this list that the presuppositions which I'm most concerned about are the ones which have direct social significance. Of course there are other presuppositions as well. Examples are the burden of proof concerning the validity of special relativity; an aesthetic preference for a particular potential field between nucleons; and a philosophical preference for a steady-state or a big bang universe cosmology. These sorts of presuppositions are found throughout science. My contention is that presuppositions of direct social significance are more common than they seem.

The person who does not believe that the sort of presuppositions in the SST-NO_x-ozone area are typical, might ask: do not the environmental and social implications of this research mean that values will be fairly directly involved, whereas in research not relevant to such social issues these presuppositions will not be important? This is indeed a possible viewpoint. I prefer another interpretation: that the presence of environmental issues in SST-NO_x-ozone research causes the value assumptions of the research to become more explicit; and that in research less directly relevant to social issues, value assumptions are more submerged, but are still present and vitally important. Under this interpretation, the SST-NO_x-ozone papers are useful as a tool for study precisely because some of the value presuppositions are forced to come out into the open: the social implications of certain conclusions, methods and assumptions are more obvious.

Embedding of presuppositions in a context

The main way in which presuppositions can be hidden from view is by being embedded in the context in which a piece of work is done. Basically this means that what is a fairly clear value assumption in one work becomes in another work a background or unstated assumption, or an unstated reason for doing the work. The more specialised a piece of work is, the more likely it is that presuppositions will be embedded in its context rather than being openly stated or implied. To illustrate these points, I turn to the SST-NO_x-ozone area. My assumption is that the embedding of presuppositions in contexts in this area is typical of embedding of presuppositions in contexts in other research areas.

Consider then the papers by Johnston and by Goldsmith et al. These papers are communications about the SST-NO_x-ozone problem at one level of discourse — fairly technical, but accessible to the scientifically minded reader. There has been much other material written on the SST-NO_x-ozone problem at different levels of discourse. Crudely, these levels of discourse may be divided into two categories: highly specialised studies, which appear in specialist scientific journals, and more general or popular articles, such as appear in editorials or news reports in scientific trade journals, or in the mass media.

My claim is that in the more specialised work presuppositions are still present, but that they are less apparent because they are subsumed in the context and even the very definition of the research area. Similarly, in more accessible discussions of the research area, presuppositions are likely to be more apparent.

One way to investigate this proposition is to look at a range of material, from the very specialised to the very non-specialised, all of which relates to the same topic. The object is to see if there are at the different levels of discourse presuppositions which are similar in content but which differ in their visibility and obviousness. Rather than present here a detailed classification of material on the SST-NO_x-ozone problem into different technical levels, and a content analysis of the material in the different categories, I will just discuss several selected articles.

In a specialised paper, Jocelyn, Leach, and Wardman (1973) are concerned with the extent of exhaust emissions of all aircraft flights in the stratosphere, both supersonic (SST) and subsonic flights. They find for example that by 1990 about 60% (with a large uncertainty) of fuel burned in the stratosphere will be by subsonics, and that about 50% of the NO_x emitted will be from subsonics.

On the surface, Jocelyn et al.'s study seems to be simply a technical study of the likely aircraft emissions in the stratosphere in the future. Actually, their work is strongly value-laden, because of the assumptions inherent in the context in which the study is based. These are some of their assumptions. (1) The total amount of air traffic will continue to grow at about the present rate. Jocelyn et al. do not explicitly state that this is a desirable state of affairs. But by working under this assumption — the assumption that other possibilities are not worth considering, not to mention promoting — they implicitly accept its desirability. (2) If SST exhausts in the stratosphere are not the dominant source of stratospheric pollution, then these exhausts should not be singled out for special concern or regulation. (Jocelyn et al.'s results might be taken to suggest that restrictions should be placed on subsonic as well as supersonic flights in the stratosphere. But it is evident from the tone of their presentation that they are concerned to justify SST flights rather than restrict subsonic flights.) (3) There will be technical solutions to problems resulting from technology. For example, Jocelyn et al. mention the possibility and likelihood that future aircraft will be designed so as to emit less exhaust that is detrimental to the stratospheric environment.

So while being a technical study, the work of Jocelyn et al. is based on various assumptions arising from the context in which their work is done and in which their results are assumed to apply.

(I cannot pass on without noting one way in which Jocelyn et al. push their argument. They classify aircraft emissions as tropospheric or stratospheric by noting whether the aircraft will be flying above or below the likely position of the tropopause. They do not emphasise, however, that the effect of emitted NO_x depends strongly on how long it spends in the stratosphere, and that therefore the higher the altitude of emission the greater the effect. They do not emphasise that SSTs fly higher than subsonics and that therefore the SST emissions on average will destroy more ozone.)

In another specialised study, Berman and Goldburg (1972) are concerned to estimate the peak concentration of SST exhaust in the stratosphere. The projected fleet of SSTs would fly more over some regions of the world than over other regions. For example, about half of all operations would be between the latitudes 39°N and 55°N. Berman and Goldberg use simple models of the emission and dispersion of this exhaust to estimate the ratio of the peak or maximum concentration of SST exhaust to the 'world average' concentration. (The 'world average' concentration is the concentration that would apply if the exhaust were evenly spread throughout the stratosphere.) For this ratio they obtain a value of 1.68, with an expected uncertainty that might let it be as high as 2.0.

The obvious but unstated implication is that the factor of 10 for this ratio, used by SCEP and by Johnston, is much too large. This would suggest that some of Johnston's results for the destruction of ozone, especially his value of a 50% reduction obtained using the factor of 10, are overestimates.

At a surface level, Berman and Goldburg's work is strictly technical. They do not refer to Johnston's work, and refer only to the SCEP report when referring to a definition of the 'world average'. The value assumptions of the paper are embedded in the context of the study. In particular, the paper seems to accept many of the assumptions of Johnston's work: that the problem of SST pollution is scientifically important, that ozone models are useful in studying it, and even that the question of how much SST exhaust spreads is significant. In addition, Berman and Goldburg make a vital additional assumption, namely that a limitation or correction to Johnston's work will seriously weaken the credibility of Johnston's conclusion. Thus, while Johnston's presupposition about what it is necessary to prove seems to be that the burden of proof lies on those who say that SSTs are safe, Berman and Goldburg's seems to be the converse. The corollary is that limitations to Johnston's work are worth studying and emphasising.

(I feel obliged to point out how Berman and Goldburg seem to push their argument. They consider only the variation in exhaust concentrations in latitude and *not* in altitude or longitude. From my reading of the SCEP report the factor of 10 seems to apply to the peak concentration at a point, and not averaged over all altitudes as by Berman and Goldburg. These authors thus seem to interpret the SCEP figures in a way which happens to suit their own purposes, a way which incidentally is quite different from Johnston's interpretation of the same figures.)

Thus, while Berman and Goldburg appear to differ with Johnston at one level — the burden of proof in SST pollution — at most other levels they appear to agree: on questions of who will do scientific research, what research will be done, etc. By attempting to substantiate a significant technical correction to Johnston's work, Berman and Goldburg necessarily accept many assumptions inherent in the context of that work. At the same time this context is not explicit and so the assumptions in their work appear less obvious than those in Johnston's.

To summarise: while assumptions may be present in specialised scientific research, they often are implicit in the context of the work. And for the very reason that these assumptions are taken for granted, they are often quite compelling. For even by the very fact of attempting to understand specialised research work, one may be forced to accept unconsciously many of the presuppositions on which that research is based: the validity of looking at the world in terms of certain concepts, the usefulness of working on a particular problem or the value of using certain methods of analysis.

In sharp contrast to specialised articles are reports of SST-NO_x-ozone work meant for a much less technically sophisticated audience — newspaper articles, and brochures put out by groups for or against the introduction of SSTs. My contention is that presuppositions in less specialised material are similar, but more apparent, than those in specialised work. Therefore I expect to find in these articles fairly obvious manifestations of pushing, and perhaps direct statements of some of Johnston's and of Goldsmith et al.'s presuppositions.

Two sample newspaper articles are reprinted at the end of this chapter. There are several points worth noting about articles such as these. First, arguments that are presented cautiously or in a roundabout manner in scientific papers often become quite clear in newspaper articles. For example, Beattie states most dramatically Johnston's mostly implicit argument that SSTs will cause a serious reduction in the ozone layer. Second, pushing of the argument occurs in the newspaper articles. Often this builds on the pushed argument in the scientific papers reported upon. For example, while Johnston only claims that projected SST nitrogen oxides *could* reduce the ozone layer by one half, Beattie states that this effect *will* occur. While Goldsmith et al. only claim that the calculated amount of nuclear test NO_x introduced into the stratosphere in 1962 was equivalent to 1047 Concordes flying 10 and ⅔ hours per day, in Silcock's article this becomes 2000 SSTs in continuous operation. Such pushing can also serve to set up an exaggerated claim which is more easily refuted. Note Silcock's comment about meteorologists who maintain that Johnston does not really understand the stratosphere. This can be considered an explicit version of Goldsmith et al.'s references to Johnston's work as "speculation". Indeed, most of the ways by which Johnston and Goldsmith et al. push their arguments can also be found in newspaper articles such as these. The main difference is that the pushing is much more obvious in the newspaper articles. Lastly, it is interesting that the presuppositions of what the scientists deem it necessary to prove — the assumptions of where the burden of proof lies — still seem to lie implicit in the newspaper articles.

Besides newspaper articles, accounts in books for non-specialist readers often demonstrate presuppositions clearly. A number of books contain accounts of the SST-NO_x-ozone problem. In order to infer the attitude of the author from the context of the account, it is sometimes sufficient merely to list the title of the book: for example, *The Concorde fiasco,* and *The disaster lobby: prophets of ecological doom and other absurdities.*

Now consider sections of two brochures put out by organisations explicitly opposed to or in favour of SSTs (see end of chapter).

The "Project to Stop the Concorde" is an Australian organisation whose aim is "to stop the introduction of any commercial Supersonic Transport (S.S.T.) until the aircraft's impact on the earth's resources and environment has been fully assessed, and proved to have no detrimental effects." The "Project" thus is an organisation explicitly opposed to SSTs. Indeed, the statement of its aim is an explicit version of Johnston's presupposition that the burden of proof should be placed on those who say SSTs are environmentally safe. This basic assumption can be seen throughout the Project's presentation of evidence concerning "ultraviolet radiation and changes in the stratosphere".

Pushing of the argument by several means may be noted in this presentation. What is important is that this pushing is fairly obviously based on the attitude that Concorde is dangerous until proved to be safe. For example, the juxtaposition of supersonic military aircraft flights and ozone increases is not taken to suggest that commercial supersonic flying will not decrease ozone, but instead that care should be taken until knowledge about possible effects of SSTs is more complete. The quote from Johnston's paper indicates that the Project realises that Johnston's assumptions are similar to its own on this point.

The British Aircraft Company (BAC) was (and is) the company contracted to carry out the British side of producing and selling Concorde. Not surprisingly, the company has been explicitly in favour of the aircraft. In a brochure, "Concorde and the Environment" (1972), BAC attempts to counter arguments about the environmental dangers due to Concorde, and to allay fears. Again, pushing of the argument by several means may be noted in this brochure. For example, BAC refer to exaggerated claims of dangers which are easy to refute. Again, what is important is that this pushing is fairly obviously based on a presupposition that Concorde is safe until it is proved to be dangerous. For example, BAC refers to the most *likely* reduction of ozone due to Concorde, rather than the *possibility* of a large reduction. The quote from the Australian Academy of Science report suggests that this report was based on presuppositions similar to those of BAC.

Discussions of issues by explicitly biased groups such as the Project and BAC are very useful in helping one to study assumptions which may also underlie more technical work. The two brochures just discussed display their value assumptions on a range of issues besides the environmental impacts of SSTs. For example, consider the question of who benefits from SSTs. The Project decries the Concorde since it will benefit only an elite few ("passengers with money to burn, who need a superficial ego boost"), BAC lauds it because of these very benefits ("the long-distance air traveller will have a real choice").

Another interpretation

There is at least one other interpretation which may account for the presence of explicit value assumptions in less technical reports of scientific research. In this interpretation scientific knowledge is considered to originate in the specialised social and intellectual activities of scientists. This information, if significant, is eventually communicated from its highly technical and specialised research source to less specialised channels. It is claimed that in the progressive transfer and communication of this knowledge, there is a progressive degradation of the quality of the content of the knowledge. For example, facts are gotten wrong, misinterpreted, selectively used and spectacularised, and theories are simplified and confused.

Certainly there is some truth in this viewpoint. But such faults as biased use of evidence are hardly peculiar to the public domain, as in the case of newspapers. As I have tried to show, scientists engage in such practices to a considerable extent.

My complementary interpretation is that in less technical and specialised communication channels, presuppositions are more explicit. In highly specialised work these presuppositions may be built into the very practice of the scientific work: into the formulation of the problem, the question of the relevance of asking it and the interpretation of the results. Thus it is almost impossible to think about certain fields of scientific study without at the same time accepting the value assumptions which underlie it. It is perhaps this feature which makes so much of science *seem* value-free, and as a result allows the particular values built into science to so easily and unsuspectedly dominate our thinking.

Reference notes

In the project that led to this book, I originally had planned to analyse pushing in five or six papers: as well as Johnston (1971) and Goldsmith et al. (1973), there were the related papers by Crutzen (1972), Foley and Ruderman (1973), Johnston et al. (1973), and McElroy et al. (1974). I had to eliminate one after another of these because the analysis was getting too long. I mention this here to suggest that the pushing by Johnston and by Goldsmith et al. is typical rather than atypical of scientific work in the SST-NO_x-ozone field. It would be possible, I believe, to reveal extensive pushing in just about any paper in this area. That I ended up choosing the papers by Johnston and by Goldsmith et al. may indicate that it is easier to highlight the pushing and presuppositions in them. And in my opinion, the ease with which presuppositions in a scientific paper can be discerned is one of the most valuable factors about the paper, though my evaluation here is precisely opposite to the normal standard of a value-free appearance.

For a discussion of pushing and presuppositions in ozone theories, see Scorer (1976); in the work of Isaac Newton, see Westfall (1973); in I.Q. studies, see Kamin (1974); in nuclear power plant construction programmes, see Chapman (1975); in the limits to growth studies, see Cole et al. (1973); in the fluoridation and low-level radiation controversies, see Mazur (1973); and in numerous public interest controversies, see Primack and von Hippel (1974) and Boffey (1975).

A preliminary categorisation of material on the SST-NO_x-ozone problem into different levels of discourse might include: (1) research papers in specialist scientific journals, such as Crutzen (1970) in *Quarterly journal of the Royal Meteorological Society,* Johnston et al. (1973) in *Journal of geophysical research,* and McElroy et al. (1974) in *Journal of the atmospheric sciences*; (2) research papers in scientific journals with readerships from many different disciplines, such as Johnston (1971) in *Science* and Goldsmith et al. (1973) in *Nature*; (3) surveys or reports of research in journals aimed at a non-specialist scientific readership, such as Crutzen (1974) in *Ambio* and Stein (1972) in *Aviation week & space technology*; (4) reports of research in the mass media (see below for examples); and (5) summaries or reports of research by explicitly committed interests, as by companies building SSTs or by groups opposing them (see below for examples).

There are other studies similar to that of Jocelyn et al. (1973): Anderson (1973) and English (1974). Anderson states explicitly several of the assumptions that are implicit in the work of Jocelyn et al. English goes so far as to qualify his conclusions — for example, he notes that air traffic might not increase due to social reasons — but does not let this affect the thrust of this analysis, nor the values underlying it. It is relevant here that British Aircraft Corporation/Aerospatiale (1973) in a promotional booklet choose to use the subsonic/supersonic comparison studied by these authors to imply that if subsonics are allowed to fly in the stratosphere, then so should SSTs. Reinforcing my argument about the embedding of presuppositions in contexts in the work of Berman and Goldburg is the spelling out of the implications of this work for Johnston's conclusions by Goldburg (1972) in a later article in the trade journal *Astronautics & Aeronautics.*

Sullivan (1971) is an American newspaper article on Johnston's work that is similar to Beattie's article, though not quite so extravagant. Tucker (1971) is a similar British report. The case against Johnston is presented and pushed in newspaper articles by Fairhall (1971) (discussing work and claims by Houghton) and by Scorer (1972a).

The Concorde fiasco is by Wilson (1973) and *The disaster lobby* is by Grayson and Shepard (1973). Various glossy publicity booklets for Concorde, such as Hughes and Costello (1972) and Clark and Gibson (1975), also provide interesting reading on the SST-NO_x-ozone problem. The miscellaneous quotes in the text from the 'vested interest' sources are from Project to Stop the Concorde (1972, p. 8, p. 2), and British Aircraft Corporation (1972, p. 4).

The explanation of the presence of explicit value assumptions in mass media reports of scientific research as a degradation of scientific knowledge is presented in most detail by Ravetz (1971).

Watch on the skylight

PROFESSOR FEARS DAMAGE TO STRATOSPHERE BY SUPERSONIC JETS

From Richard Beattie, 'Herald' Correspondent in New York

THE FRIGHTENING argument goes like this:

1. If there are 500 supersonic aircraft flying by 1985 the concentration of ozone in the stratosphere will have been halved.

2. Once the protective shield of ozone has been so drastically depleted, there will be a vast increase in the amount of ultra-violet light reaching the earth.

3. The ultra-violet light will blind all men and animals in the world unless they remain under cover during the day or wear goggles.

It is the argument of Dr Harold Johnston, eminent scientist and Professor of Chemistry at the University of California at Berkeley.

Provoked a controversy

His conclusions were published this month in the technical magazine of the American Association for the Advancement of Science.

They have provoked a new controversy in the United States and in Britain, where the first six production Concorde aircraft have been started.

Dr Johnston used supersonic engine information supplied to the US Department of Transportation by General Electric engineers, who were to have built engines for the cancelled Boeing supersonic airliner, and flight statistics provided by the US Federal Aviation Administration.

He was told there would be 500 SST aircraft by 1985 (334 with four engines and 166 with two) and that each plane would cruise in the lower stratosphere for an average of seven hours a day.

It is in the lower stratosphere that most ultra-violet light from the sun is absorbed in a photochemical reaction involving ozone.

The stratosphere is largely isolated from the lower atmosphere, or troposphere, because of the presence of ozone. The reaction of ozone and ultra-violet light produces heat — making the stratosphere warmer than the layers of air beneath.

The colder air cannot rise through the warmer layer, which acts as a lid. Consequently the stratosphere is insulated from most turbulence lower down — making for smooth flying — but also, it is argued, making a trap in the ozone layer for the SST's exhaust gases.

Professor Johnston believes that the additional nitrous oxide and nitrogen dioxide gases emitted into the strato-sphere by the supersonic jets would react with ozone in such a way as to reduce stratospheric oxygen.

He says that if increased concentrations of oxides of nitrogen accepted as realistic by government agencies were, in fact, to be created in the ozone layer, then there would be a reduction in the ozone shield by about a half.

Gases leak into the stratosphere

Professor Johnston's argument has not been generally accepted by scientists concerned with the atmosphere.

There is evidence that some pollutant gases like oxides of nitrogen "leak" into the stratosphere from the troposphere, despite the "lid" effect of temperature inversion.

A large majority of the world's weather stations have reported that ozone in the stratosphere has increased in the past decade.

The British Government, which is investing the equivalent of $1,000 million in the Concorde, has dismissed Dr Johnston's research.

In the House of Commons the Minister of Aerospace, Mr F. Corfield, said: "Our studies indicate that the effect of operating even a large fleet of Concorde aircraft will be less than the range of normal changes from natural variations and will not lead to the harmful effects suggested by Dr Johnston."

POLLUTION

Concorde—OK for ozone

IF THE CONCENTRATION of ozone in the upper atmosphere fell by only five per cent the extra ultra-violet radiation from the sun reaching the earth's surface would cause 8,000 extra cases of skin cancer in the United States alone.

This is one of the reasons why supersonic transports like Concorde have aroused fears of environmental disaster, for according to some authorities SSTs could lead to a reduction in the stratospheric concentration of ozone far greater than five per cent.

Recently, however, the ozone scare received something as close to a coup de grace as it is possible to deliver in a field so full of uncertainties. The occasion was a NATO conference on Atmospheric Pollution by Aircraft Engines, held, somewhat inappropriately, at the London Zoo. Among the audience was Professor Harold S. Johnston of the University of California, who started the scare.

Briefly, Johnston's argument goes like this. Ozone, a form of oxygen, is formed in the upper atmosphere under the influence of radiation from the sun. Minute quantities of oxides of nitrogen are believed to have a key role in converting ozone back into ordinary oxygen, thus maintaining a balance. SST exhausts will inject substantial quantities of nitrogen oxides into the stratosphere in the region of the ozone layer, thus increasing the rate at which it is broken down and altering the balance in the layer.

Attempts at rebutting Johnston by the Concorde lobby and others have been mainly along the lines that natural processes inject great quantities of nitrogen oxides into the stratosphere anyway.

However, meteorologists, who have always maintained that Johnston (a chemist) does not really understand the atmosphere, have now produced a far more convincing argument against him. It was developed at the recent conference by Mr Philip Goldsmith and his colleagues from the Meteorological Office at Bracknell.

The argument is startlingly simple. Nuclear tests in the atmosphere also produce huge quantities of nitrogen oxides. The amounts can be estimated fairly accurately, and they turn out to be comparable to those that would be produced by large fleets of SSTs. In fact, during the peak testing year of 1962 the atmosphere probably received a dose of nitrogen oxides equivalent to that from 2,000 SSTs in continuous operation.

But when the rate of nuclear testing is compared with records of ozone concentration, which for some sites go back almost 50 years, there appears to be no connection whatsoever.

Concorde proving flights have provided an opportunity for making some of the first actual measurements of nitrogen oxide concentrations in the stratosphere. Starting tomorrow some flights towards the Arctic will be made specifically for this purpose. The results so far suggest a concentration of 22 parts per billion, for one of them. Unfortunately the maximum allowable according to current theories of the ozone layer is less than half this figure.

In fact the whole theory of the ozone layer may have to be revised for the third time in a decade. Nitrogen oxides may turn out not to be so critical after all.

Bryan Silcock

Excerpt from British Aircraft Corporation, "Concorde and the environment" (1972, pp. 2-3).

Claim: "Emissions from Concorde's engines will destroy the ozone layer, let through the ultraviolet rays and so give everyone skin cancer."

Answer: The main ozone producing region in the stratosphere is very much higher than the altitudes at which Concorde flies, which is under 59,000 ft. (18 km).

The first scare about the ozone layer was that water vapour emitted by supersonic transports would destroy the ozone through its chemical action. This speculation has now been discredited. In fact, adding water might if anything help prevent the nitrogen oxides present naturally from destroying ozone. A further suggestion that the nitrogen oxides added by Concorde to those already present would destroy ozone in the stratosphere, has been extensively researched and the amount so destroyed is likely to be insignificant (a fraction of one per cent).

The Australian Academy of Science reports: "Although there has already been a considerable amount of lower stratospheric flying over the last decade, the ozone concentrations have not decreased (in fact, there has been an increase, the cause of which is not known)."

About this and other theories, it concludes:

"We therefore believe that the effect of supersonic aircraft on the ozone layer is not likely to be serious, especially as there appears to be no possibility of a rapid change."

In the summary it states: "On the basis of data available to us we would not expect significant adverse climatic effects to derive from supersonic aircraft assumed by us to be flying in 1985."

Excerpt from Project to Stop the Concorde, "The Concorde crisis" (1972, p. 5).

4. Ultraviolet Radiation and Changes in the Stratosphere.

The research findings of Professor Harold Johnston (University of California, Berkeley) and other scientists have caused widespread concern.

Basically Professor Johnston indicates that under certain conditions the nitrogen oxides from the Concorde's exhaust could act as a catalyst and break down the Ozone layer of the Stratosphere.

At present the Ozone layer protects the earth from excess ultra-violet radiation. Without it animal and vegetable life could not exist in their present forms.

There is considerable controversy about the research of both Johnston and his critics, and of the assumptions they have made in reaching their conclusions. However, one thing is certain — not enough is known about the Stratosphere at present to risk its pollution or change by the introduction of tons of materials from S.S.T. exhausts.

Despite the flight of supersonic military aircraft for a number of years the Ozone content of the Stratosphere is actually increasing — the reason why is not known.

Man is playing with unknown factors which could be of critical importance to the survival of the Biosphere which supports life at present.

Professor Johnston said, "the stratosphere is vulnerable to added oxides of nitrogen and forethought should be given to this hazard before the Stratosphere is subjected to heavy use . . . A large reduction in Stratospheric Ozone would be expected to change the temperature, structure, and dynamics of the Stratosphere . . . However, the point of this report is not to assert that S.S.T. flights will reduce the Ozone shield by some precise factor; rather the point is that NO_x is a highly important variable in this problem and it must be given realistic consideration" (Johnston, H., "Catalytic Reduction of Stratosphere Ozone by Nitrogen Oxides" June 1971 pp. 105, 106 and 114)

It is the job of the Government, of Concorde's manufacturers, and of the Airlines that want to fly it, to prove conclusively in open inquiry that the Concorde's operation will produce no deterioration or harmful changes in the Stratosphere. If they cannot, then the Concorde must not fly.