The Determinants of Scientific Behaviour

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Observations of the response of scientists to unorthodoxies indicate that conventional explanations of their behaviour are not fully adequate. Two further factors, links to outside interests and vested interests in professional expertise, demand greater attention by sociologists of science.

A school student sends a letter to a well-known scientist, head of a major school of physics. The letter is passed down to one of the professors in the school. Then the letter is passed down from the professor to a senior postgraduate student. A note of explanation is added: "If you feel kind, answer; otherwise hurl it away." Finally, a beginning postgraduate student in the same office takes on the job of answering the letter. Its subject: an idea that time can be measured in terms of heat. (This case is based on the present author's personal experiences, as are the other undocumented examples in this paper.)

A small group of researchers obtain some startling experimental results, results that cannot be explained using current scientific theories. Most other scientists ignore the findings and their implications: the findings are assumed to be incorrect because they cannot be explained using current scientific theories. A few scientists take the attack: the results are claimed to be insufficient to establish that some previously unexplained factor is operative; the methods of the researchers are discredited; and when other criticisms are inadequate, the possibility of fraud is postulated. Although certain sections of the public and of the intellectual community are excited by the results, most of the scientific critics do not try to understand the phenomena or reproduce the results, even if only to close the issue conclusively. Instead, they denounce the work and the researchers, and attempt to limit the research by withholding financial support and professional legitimisation. (This description applies to parapsychology, especially in the days of J. B. Rhine, and perhaps also to the study of UFOs.)

Why do scientists behave the way they do? Why do they react favourably towards some ideas and evidence and methods of analysis, and unfavourably towards others? What are the determinants of the response of the scientific community to changes in the content of scientific knowledge and to changes in scientific practice?

The introductory accounts in the first two paragraphs are examples of the ways in which scientists react to what they call "cranks". The study of such examples affords a ready opportunity to investigate and test explanations for the behaviour of scientists and of the scientific community. The study of the response of scientists to "cranks" is especially valuable because their actual behaviour in this area conflicts with what might be expected using accepted methods of explanation.

The conventional explanations of the behaviour of scientists and of the scientific community stress either adherence to social norms within the scientific community, such as disinterestedness or universality, or adherence to scientific paradigms, such as the theories and methods of approach used in quantum physics or behaviourist psychology. The main point of this paper is to argue that there are at least two other important determinants of individual and communal scientific behaviour which have been downplayed or ignored by conventional sociology of science. One is the interlinking of scientific goals with the goals of outside interests, such as industries or government bodies; the other is the direct vested interest of the scientific profession in its own prestige and privileges.

These topics are treated here in the following order. First, the way scientists respond to "cranks" is discussed in some detail. Some of the ways scientists might be expected to act, as well as the way they do act, are covered. Second, the way that links with outside interests may affect scientific behaviour is discussed using a number of examples. Third, the influence of the professional status of scientists on scientific behaviour is treated in a similar manner. Finally, some implications of the analysis for the sociology of science are presented.

The Response of Scientists to "Cranks"

Who are "cranks" ("crackpots", "faddists", "quacks", etc.)? Care must be taken in answering, for even to define "cranks" is to impose a theoretical explanation for the way they are treated.

Perhaps the easiest definition of a "crank" is someone who many or most scientists would call a crank. Naturally this varies considerably from scientist to scientist. Typical "cranks" are non-scientists who claim serious consideration for ideas that are considered unsupportable or outrageous according to the currently accepted views of the scientific community. Examples are people who believe the earth is flat, or a non-scientist who argues in favour of a personal view that neutrons and protons are hollow. Scientists are also considered to be "cranks" when they support an idea or believe evidence that is considered unsupportable or outrageous according to the current views of the scientific community. Examples are belief in extra-terrestrial intelligence interfering in affairs on earth, or (bordering on acceptability?) opposing fluoridation for health reasons. Indeed anyone, scientist or non-scientist, who argues long and strenuously for a view that does not have much support among orthodox and dominant scientific opinion, is liable to be labelled a "crank". The stories of some well-known "cranks" are described (unfavourably) by Gardner (1957) and (more favourably) by Gordon (1966), among many other accounts. It is salutary to remember that many now famous scientists, such as Galileo and Freud, were at one time generally considered to be "cranks". The local responses of scientists to small-time "cranks" — as in the case described in the opening paragraph above — are by contrast not well documented.

How do scientists respond to "cranks"? Before they can respond, the unacceptable idea naturally must be communicated, either to scientists or to members of the general public. The variety of responses is so great that describing them requires either excessive length or unfair generalisation. The short description here tends towards the latter failing. A general characterisation of the typical response of scientists to "cranks" is "unfavourable". A few examples may add flavour. A letter from an "amateur" to a scientific establishment, detailing personally thought out explanations or questions, is as likely as not to be thrown away. If it is considered outrageous enough, it may be posted on a bulletin board to be laughed at. This then is one attitude: unthinking superiority, often contemptuous. Second, the "amateur" who persists in attempting to communicate his or her ideas to scientists may be fobbed off, or referred down the hierarchy of scientists, perhaps to get a reluctant hearing at some stage. This is another attitude: annoyance. Third, almost by definition "cranks" cannot get their ideas published in the scientific literature. So when the more ambitious of them do publish their ideas, it is for the general public. A typical response of sci-

entists is angry and bitter attacks on the writer and the ideas. This is another attitude: outrage. Finally, perhaps the most common response is that of not caring — or not knowing — at all. This is the response, for example, to established "cranks" such as astrologers.

With the strong qualification that responses vary tremendously, these then are some of the common responses: contemptuous superiority, annoyance, and outrage. But how might one *expect* scientists to react?

Here is one line of argument. Scientists today are worried about the status of science in society. The lack of public understanding of science is often bemoaned. Furthermore, science students seem increasingly dissatisfied, and leading scientists are worried about the "swing away from science". Along comes a person who on his or her own has thought out and developed some ideas or questions about topics for which there are more or less accepted scientific explanations. Admittedly, the ideas may be unacceptable. But the person is interested, after all, and furthermore is personally motivated to think and work on topics of scientific interest. Such a person might seem ideal for cultivation as a potential scientific worker. A bit of sympathetic encouragement (and benevolent redirection of effort) might be all that is required.

Of course this is demonstrably what does *not* happen. People with unformed or unorthodox ideas about science are typically ignored or repulsed as painlessly as possible, rather than being challenged and cultivated. There must be some other factor or factors that are more important to the scientist and which dictate this behaviour. One value of looking at how scientists respond to "cranks" is that scientists do not respond the way one might expect that they would.

This is the time to counter one of the common reasons offered for why scientists are annoyed by "cranks": that dealing with all of them would take up too much time. It is perhaps necessary to mention that no one would expect that all ideas offered by non-scientists (or scientists) should be given long and detailed attention. By force of circumstances, judgement always must be used. The man who claims to be able to see little white bugs that are invisible to everyone else, and asks a famous biologist to investigate before the bugs take over, presumably is not in need of long and detailed taxonomic advice. (In our society, other sorts of advice might be thought more in order.) The question remains: is saving time really an excuse for off-hand, cavalier treatment of virtually all ideas which are unorthodox or which are proposed by amateurs? Scientists already spend an immense amount of time trying to interest people in science: writing books, planning syllabi, and talking to students. Many "cranks" are already interested in science, and only a relatively small amount of time might be required to entice them into more serious study. A more likely explanation of scientists' behaviour is that they have decided they don't want people (self-motivated or not) entering science except through the official channels, and use "lack of time" to justify their actions.

There is another argument for expecting scientists to react relatively sympathetically to "cranks". This is that a seemingly wild idea by an outsider might turn out to be an inspiration for a new scientific theory or approach. Historically, it is well known that observations by the common people and phenomena promoted by "charlatans" were often valuable clues for revising erroneous scientific beliefs. Examples are the reports of meteors in the early 1800's when official scientific opinion said it was impossible for objects to fall from the sky; and the often demonstrated but scoffed at phenomenon of hypnotism. At the other end of the spectrum, modern theories in the physics of elementary particles are so abstract and weird that even the most crazy-sounding idea — at least if proposed by established workers in the field — may be given at least temporary credence (see Koestler, 1972, or *Physical Review*!). It is quite possible that the fresh insight of an unconditioned outsider — an amateur — may be what is needed for a major breakthrough in this field, as well as others. Watson (1938), commenting that "the kinship of the scientific crank with the scientific creator is more than a superficial one" (p. 41), argues for the importance of cranks in this respect. Yet, once again, this possibility does not seem to hold much weight with scientists in terms of their actual behaviour.

Conventional Explanations of Scientific Behaviour: Norms and Paradigms

Consider very briefly some of the explanations of the behaviour of scientists by the people who study such behaviour, namely the sociologists of science. The most widely accepted and studied explanation until recently is one based on the idea that scientists behave according to a set of social norms. These include such things as disinterestedness, emotional neutrality, and organised scepticism (Merton, 1942; Barber, 1952; Storer, 1966). These norms, it is said, are the pre-condition for the efficient operation of science as we know it, and are also the result of the evolution of scientific practice in those societies in which this practice has best been able to flourish.

The explanation using norms has become more difficult to maintain in recent years, as studies have shown more and more convincingly that scientists just do not behave in practice anything like they are supposed to in theory. The challenge to the functionalist description of scientific behaviour comes from many angles. Krohn (1971) finds that there are at least three types of scientists, corresponding to three overlapping eras in scientific practice: scientists oriented by personal desires and interests; scientists oriented towards professional colleagues (the only group to which the postulated norms might apply); and scientists oriented towards their employing organisation. Sklair (1973) argues strongly that it is the work situation of scientists that determines their loyalties and activities. And Mitroff (1974) shows that the behaviour of leading scientists as often as not is better described by what he calls counter-norms — such as emotional commitment and organised dogmatism — than by the norms themselves. The obvious conclusion is that the approach using norms is not a particularly fruitful one. This is not surprising, since the idea of norms for explaining scientific behaviour was not originally based on observations of scientific behaviour at all, but upon a few sociologists' ideas about how scientists *should* behave — in order for their ideas to make sense!

Since the functionalist explanation using norms is linked with the traditonal Popperian falsificationist philosophy of science, it is not surprising that the advent of the more relativistic Kuhnian approach (Kuhn, 1970; Polanyi, 1958; Lakatos and Musgrave, 1970; Barnes, 1974) should lead to a different approach to the explanation of scientists' behaviour. The new approach is well expressed in the work of Mulkay (1969), who uses as a test case the example of the scientific community's response to Velikovsky, who for modern science is an eminent and almost archetypal "crank". The scientific community's response — involving refusals to test predictions, attempts to repress publication, and vicious personal attacks without offering an opportunity to reply (de Grazia, 1966; the journal *Pensée*) can only be ascribed to norms by stretching the imagination and the explanatory principles. But by utilising the idea of the intense commitment of the scientific community to paradigms — commonly agreed upon principles of explanation and practice — the outrage of scientists at Velikovsky makes much more sense.

The purpose here is not to deny that norms have some influence on scientists' behaviour, and still less to dispute that adherence to paradigms is very important. Rather, it is to emphasize the importance of two further factors: links with outside interests, and vested interests in professionalised scientific expertise. Rather than being minor disturbances to the

normal operation of science as interpreted by traditional sociologists of science, these latter two factors may be of central importance.

Links with Outside Interests

To many people it may seem obvious that the behaviour of scientists is strongly influenced by interests on whom their livelihood or prestige or self-image may depend. A scientist working for a bureau of mineral resources is likely to be antagonistic to those who want to stop growth in the extraction of mineral resources. Scientists working on guided missile control systems are likely to be sympathetic to game theorists who justify arms escalation as a peace-keeping mechanism. And scientists searching for the biochemical source of cancer are unlikely to drop their research to support those who say cancer prevention is better approached through improved nutrition and environmental conditions.

Of course the response of a scientist to a theory or approach may be dictated by a combination of norms, paradigms, and links to outside interests, and indeed the different factors may be conflated to some degree. But before going into this more deeply, some examples are in order to show the significance of links to outside interests.

In explaining the observed levels of ozone in the stratosphere, a problem became apparent in the late 1960's. Existing theory, which utilised chemical and photochemical reactions involving oxygen compounds, predicted roughly twice as much ozone as actually observed. (Results of both theory and observation were very uncertain, so that it was disputable that there was actually a discrepancy that required changes to the theory.) An attempt, eventually unsuccessful, was made to fix up the theory, by postulating that certain reactions involving hydrogen compounds were sufficiently rapid to destroy catalytically the appropriate amount of ozone (Hunt, 1966). A few years later, Crutzen (1970) suggested that certain reactions involving oxides of nitrogen would be significant in reducing ozone levels to those observed. This work did not attract much attention. But when Johnston (1971) published a paper on the same topic the next year, it attracted a storm of controversy. (On the scientific side, see the Science Citation Index for the volume of response. A general account of the episode is given by Schneider, 1976.) The reason is obvious. Johnston, as well as arguing that oxides of nitrogen are important in reducing stratospheric ozone levels, also argued that oxides of nitrogen from the exhausts of supersonic transport aircraft (SSTs, such as Concorde) might reduce ozone levels.* This was a direct threat to the business and government interests promoting the SST. (At the time of Johnston's paper, a decision was about to be made in the U.S. about government support for the SST.)

It may be presumed that those stirred into action by Johnston's work were primarily opposed to his conclusion about the effect of SST exhausts on ozone. But in their rebuttal of this latter conclusion, many of them also attempted to discount the idea that oxides of nitrogen are important in determining ozone levels (e.g., Scorer, 1971a; Goldsmith et al., 1973). Since there had been no similar response to Crutzen's original proposal (as may be verified by looking through the timing and citations of scientific papers on the topic), the obvious explanation is that the idea that oxides of nitrogen are important for ozone was challenged because it was the basis for the argument that SST exhausts had potentially dangerous consequences.

The traditional explanations of scientific behaviour cannot easily account for the difference in response to Crutzen and Johnston. In Crutzen's case and in Johnston's case, the social norms of the scientific community were adhered to. Likewise, each of them adhered to the prevailing paradigms in explaining ozone levels, which involved using plausible chemical reactions and air movements in mathematical models of various sorts. The difference in the response to the papers is most easily explained by the fact that Johnston's work was much more of a threat to the interests of business and government backers of the SST.

This example concerning ozone and SSTs involves established scientists and orthodox research. The second example here concerns "cranks" in the field of nutrition, an area where there is considerable revealing material suggestive of the motives of scientists' behaviour.

In 1974 the journal *Nutrition Reviews* put out a special supplement, subtitled "Nutrition misinformation and food faddism". This supplement contains a range of articles by different authors and committees, and includes many attacking "cranks". For example, there is an analysis of inadequacies in the work of "food faddists" such as Adelle Davis, dismissals of the value of supplements of vitimins C and E, and a discussion of "programs to combat nutritional quackery". Now in all fairness, most readers of this supplement would find much of the material presented to be perfectly acceptable and useful. For example, there is a report of the limited amount of vitamin C in rose hips. What is revealing is not so much what is said, but what is not said. First, the scientists writing in the supplement ignore, and implicitly deny, the possibility that the "faddists" they are criticising could possibly have something worth saying. The critics are quick to turn the intense beam of their attention on every inadequacy of non-orthodox practice that can be readily exposed or disputed. But they never mention that currently accepted practices may be harmful too. For example, they find "deplorable" the "massive publicity" given to megavitamin therapy and orthomolecular treatment. But there is no mention of the massive publicity given to soft drinks and party snacks.

Second, any practice advocated by those characterised as "faddists" is assumed to give no benefit unless modern "scientific" evidence confirms it. For example, commenting on the use of large amounts of vitamin C for prevention and alleviation of the common cold, the American Academy of Pediatrics Committee on Drugs (1971) summarises its statement in this way: "There is not sufficient evidence that ascorbic acid in doses recommended by Dr Pauling is either safe or efficacious in the prevention or treatment of the common cold. Until such data are available, ascorbic acid should not be used for this purpose."

Why is there all this attention to the dangers and expense of "quackery", and so little attention to much more pressing problems such as obesity, diverticulosis from lack of dietary fibre, high blood pressure from excess dietary salt, tooth decay from highly sweetened food, and heart disease and strokes from eating too much fat? A strong hint comes from the fact that *Nutrition Reviews* is a publication of the Nutrition Foundation, which is "created and supported by leading companies in the food and allied industries". The material in the *Nutrition Reviews* supplement is not easily or fully explained using norms or paradigms. The orientation of the material is better explained as being an attack on nutritional ideas and practices which are not in the best interests of the "food and allied industries".

Some of the advantages to scientists and administrators in attacking "cranks" are presented in a study (Turner, 1970) of the U.S. Federal Drug Administration. The FDA, nominally a regulatory body, has often actually promoted the interests of the industries it is supposed to regulate. Much of its regulatory effort has been spent chasing "quacks" (incidentally leading to excesses of law enforcement such as invasion of privacy), while little attention has been given to programmes that might help the consumer more, such as ensuring proper packaging and labelling or reducing food-borne disease. There seem to be

* It is only fair to note that Crutzen, working in the U.K., drew attention to the implications of oxides of nitrogen in SST exhaust independently and slightly prior to Johnston. However, for various reasons Crutzen's warning had little effect. Of course this is no reason to doubt that the sudden interest in the nitrogen oxide theory was directly due to its implications for SSTs.

several purposes served by this selective and excessive attention to the "quack problem". For one, it provides the basis for a claim that the agency has been active in enforcing the law. It also thwarts budding enterprises which might become threats to the pre-eminence of the established industrial giants of food manufacture, distribution, and sales. And perhaps most importantly, it is a way of diverting attention from what should be the real concerns of the agency (economic and health problems in traditional areas). (In passing, it should be mentioned that there is an extensive movement of staff — scientific and administrative — betweeen the FDA and the industries it allegedly regulates.)

How is it that scientists can come to defend scientific ideas on the basis of their usefulness to their employing organisation? Of primary importance is the influence of the day-to-day work situation: to get on in a job, the scientist tends to adopt and follow up ideas that are useful from a career point of view. Also, there may be considerable self-selection involved in the occupational location of the scientists (Krohn, 1971; Cotgrove and Box, 1970). Finally, some organisations may virtually force scientists to work in certain directions. FDA scientists tended to censor themselves: important scientific questions were not being asked because of fear that an unwanted answer might bring down the wrath of the administration on the researcher

At a much more fundamental level, business or government interests may influence the choice or maintenance of a paradigm by the scientific community. The community may adopt or adhere to a paradigm not only because of its usefulness in generating and resolving problems, but because the problems it proposes are not threatening to business or government interests. The process in the nutrition field is well described by Hall (1974). For example, it is not surprising that academic scientists working in insect control, who are often indebted to chemical companies through direct support or consultantships, should adhere to a belief that chemical control of insects is the only way. This entrenched belief then enters university curricula and research programmes, and is passed down to students and beginning researchers who may be unaware that the chemical industry has been instrumental in defining the paradigm.

A few examples of course cannot demonstrate the significance of links to outside interests one way or the other. But the ever-increasing number of studies demonstrating the pervasiveness of the impact of "non-scientific" interests on science suggests that the examples here are typical rather than atypical. Primack and von Hippel (1974) present an excellent set of case studies, covering different ways in which theories and arguments of "outsider" scientists and non-scientists are ignored, dismissed and attacked by scientists working for industrial or government research organisations, or consulting for these organisations. Naturally in each case the "outsider" ideas were threatening to the vested interest concerned. Boffey (1975) in a study of the U.S. National Academy of Sciences describes with many detailed examples how scientific committees are used (and even set up) to justify existing or planned policies and actions of government or industry. Boffey finds that the Academy "has often allowed itself to be used as a shield by those intent upon preserving business-as-usual" (p. 247). Similarly, the influence of outside interests on scientific paradigms is getting increasing attention. Clarke (1971) gives many eye-opening examples of the penetration of military motives into apparently "pure" science. Rose and Rose (1971) and Young (1971) argue that paradigms are never value-free or neutral, and provide illustrations from evolutionary biology. Finally, although the massiveness of the direct impact of government and industry on scientific research is fairly recent, the impact itself is a longstanding one, as a number of historical studies show (Merton, 1970; Bernal, 1969).

Professional Status

While links to outside interests are certainly widespread in modern science, more is required to explain all features of scientific behaviour. For example, the feeling of annoyance and hilarity at the follies of inconsequential "cranks" is not ascribable to such links. The further explanatory principle adopted here is the vested interest of scientists and the scientific community in their professional status and privileges.

It is well known that modern science is almost entirely professionalised and bureaucratised. The self-image of many scientists is likely to be tied up with their professional status, and with maintenance of the prestige of the scientific community as a whole. (This is especially likely to hold for those scientists who look to other scientists for confirmation of their scientific contributions — the professionalised group from Krohn's (1971) three categories of scientists.) If these considerations are significant, it is to be expected that scientists will tend to react antagonistically to people and to theories which represent a threat to their professional status.

To test this expectation, it is fruitful to analyse the type of response given to scientists and theories and evidence which might represent a threat to scientific professionalism. In particular, if protection of professional status is important, it is to be expected that negative evaluations of the professional standing of those proposing unorthodox or undesired ideas will be emphasised.

First consider the response of portions of the scientific community to Johnston's argument that oxides of nitrogen from SSTs might dangerously reduce levels of ozone in the stratosphere. One argument used was that Johnston was a chemist, and (therefore) his consideration of meteorological effects in his calculations was oversimple (therefore making his results overestimates of the effect of oxides of nitrogen from SSTs).* Note that the criticism of Johnston's professional competence in meteorology is distinct from the scientific evaluation of his calculations and their incorporation of meteorological effects. Thus while a large part of the antagonism to Johnston's work may be traced to the interests of the aircraft industry and government patrons of the SST, the importance of professional status to scientists is suggested by one of the ways in which Johnston's work was attacked (namely by denigrating his professional competence).

Once again, the nutrition field offers a number of clearcut examples. Turn again to the special supplement to *Nutrition Reviews*. From reading this it is clear that the contributing authors consider "faddism" a threat. The contributors say the threat is to the public; another interpretation, adopted here, is that the threat is to the scientists themselves. Confirming this idea is the way that the professional qualifications of the "faddists" are given a great deal of play. Here are a couple of sample quotes: "Jerome Rodale had no scientific training; his only formal background consisted of studying accounting at night school" (Rynearson, 1974); "The president of the National Academy of Sciences is anxious to obtain input from consumer-oriented groups, but does not want to add self-appointed protectors of the public to committees whose mission is to evaluate and interpret scientific matters for the public, especially when such persons are not qualified professionally" (Henderson, 1974).

Another symptom of scientists' concern for their professional status is the presentation of an analysis of the accuracy of Adelle Davis' references. Finding errors in her use of references is another way of demonstrating her lack of professionalism. However, conclusions drawn from such evidence of errors in references and attribution are spurious unless a

* It is not normally considered proper to air such attitudes in scientific papers. Criticism of Johnston's professional competence for the problem he tackled is implicit in the survey by the Australian Academy of Science (1972) and clearly stated in newspaper articles and trade journals (Scorer, 1971b; Silcock, 1973). Personal communications from Johnston to the author confirm the significance of this sort of attitude.

similar study were made of the accuracy of references in conventional scientific literature — where it would not be surprising if just as many errors were found (see Gouldner, 1973, p. 170).

Even in the *Nutrition Reviews* supplement the emphasis is on "scientific" analysis of the views criticised, so that fears of non-professionals are not spelled out all that often. Clearer statements can be found in less formal documents (not to mention private conversations). For example, in the general discussion at a nutrition conference (Marabou, 1975), one of the participants argued that a formally qualified person should refuse to appear on a radio programme or a debate with an unqualified person. This participant was delighted to have been able to say, on a TV show with Linus Pauling, "Professor Pauling is a great American and one of the few who has won two Nobel prizes, but I'm sorry to say he knows little about the subject of nutrition" (p. 60).

Also symptomatic of the vested interests of professionals in their status is their attitude towards self-help groups, such as Alcoholics Anonymous. The methods and results of such groups tend to be ignored by professionals, especially considering the degree to which these groups are more successful than anything the professionals can muster (Maxwell, 1962).

What is the basis of the professional's fear of non-professionals? Basically it is that if the non-professionals were to take over some or all of the functions of the professionals, then the status, privileges, even the jobs of the latter might be at risk. To maintain these things, professional scientists find it natural to monopolise facilities and opportunities for practising their trade, and create a mystique of their own indispensability. For the medical profession, self-ministration (or even treatment by semi-professionals) is dangerous. For the military, arming of the people is dangerous. Similarly, for scientists the involvement of the public in science policy, in formulating and debating theories, or in using and developing scientific techniques themselves, is dangerous.

Watson (1938, p. 202) suggests that the scientists' fear of public participation in science is similar to the Catholic Church's fear of Protestant reformers. The reformers demanded that there be no priestly machinery between the individual and God; this obviously was a threat to the domination by the Church. Watson comments that the danger that every man will think himself a Newton is no greater than the analogous exaggerated fear of the Church: "What will become of us when every man thinks himself a Mahomet?" (Bernard Shaw)

The best example of a society where at least an attempt has been made to deprofessionalise science is China since the Cultural Revolution. There, local commune research groups investigate problems of practical interest; researchers are expected to spend time doing practical tasks; and often projects declared impossible by the experts are carried out by group action (Science for the People, 1974).

The theoretical advantages of deprofessionalisation in many fields have been adumbrated in recent years, especially by Illich (1971, 1973, 1975). Although Illich seems to think that the facts and arguments he presents are sufficient in themselves to make people see the advantages of deprofessionalisation, there has been no great surge towards this state of affairs. Least of all have the professionals themselves — who after all have a great deal of power over socialisation, recruitment, resources, and ideas — made any attempt to reconsider, much less to undermine their position. This may be traced to the vested interests of professional groups. Alternatively, professionals may be considered to work within a paradigm of society which includes the necessity for their own existence.

The Velikovsky Case Re-examined

In the light of the possible influence of scientists' vested interest in professionalisation, it is useful to re-evaluate the reasons for the scientific community's violent attack on Immanuel Velikovsky and his ideas.

It is difficult to see this attack as motivated by Velikovsky's transgression of the norms of science. For example, it may be said that he violated the norm of disinterestedness by publishing his work for profit before getting it refereed by scientists (Blissett, 1972). But Velikovsky's financial interest in his ideas would appear to be insignificant compared to the financial interests of professional scientists in their expertise or their ideas — ideas which have not been subject to attack.

Instead of using norms, the attack on Velikovsky may be ascribed to the rigid adherence of scientists to the accepted scientific beliefs in their individual specialist disciplines (Mulkay, 1969; Polanyi, 1967; Dolby, 1975). Velikovsky's work is widely at variance with the accepted ideas in a number of disciplines. In addition, it proposes to introduce a new method into science: the use of historical and mythic evidence as an aid and spur to the interpretation of the history of the universe and the mechanisms operating in it. Since it challenges both the currently accepted explanations in science and the current methods of study, it is not unexpected that scientists would feel severely threatened. The following quote from a scientist (Stevenson, 1976) expresses this form of opposition very concisely: "Velikovsky does not use the scientific method, and is not willing to take part in rational scientific discourse. Specifically, his ideas are not compatible with a number of the fundamentals of physics and chemistry".

Parenthetically, the scope and unorthodoxy of Velikovsky's work is so great that any attempt to introduce it through journals would have been doomed to failure (see Dolby, 1975). As a result, his work would seem much more suited to the old-fashioned method of publication in book form. In any case, Velikovsky's manuscript *was* refereed by several people (for details, see de Grazia, 1966).

However, the challenge of Velikovsky's work to current scientific paradigms seems inadequate to explain all aspects of the scientific response it induced. It should be remembered that there are many theories and much evidence proposed and published that radically conflict with current paradigms, and which never attract more than a passing glance by scientists. Furthermore, Velikovsky did not find much support within the scientific community, at least in terms of established scientists who were willing to follow up the ideas by tentatively adopting the alternative paradigm. So the scientific community was not responding to any significant threat to the prevailing ideas from within its own ranks.

On reading the accounts of the Velikovsky affair, it would seem that a fundamental reason for the extent of the antagonism of the scientific community to Velikovsky was the great popularity of his work with the general public. Blissett (1972) states, "the central source of concern seemed to be that Velikovsky's ideas were generating enthusiasm — without first having been qualified by reputable scientific opinion" (p. 69).

Now according to the sociologists of science, there is no norm which says that scientific ideas should not be published in book form, nor is there a norm which says that ideas should be attacked if they are popular with the public. Norms are simply inadequate to explain the response to Velikovsky. And according to Kuhn and his followers, scientific paradigms are concerned with the work of established scientists, and not with the beliefs of the public. So the idea of paradigm-boundedness, at least in the normal sense, is insufficient to explain the extent of the scientific community's sense of outrage at Velikovsky. The reason for the scientific community's response may not be that Velikovsky's ideas had not been validated by scientists, but that his work was encouraging people to think for themselves and not to defer automatically to the opinions of the "experts".

Of course it will surprise few people that scientists get

excited when someone undermines the public's belief in the authority of science and of scientists. Neither should it surprise anyone that norms and paradigms are inadequate to explain scientific behaviour.

All this is a sad commentary on the attitude of scientists towards the general public. One might think that a proper attitude would be to encourage people to think scientifically, to challenge themselves by evaluating debates of significance to society, and in general to learn from exposure to the current practice of science. If scientists cannot condescend to enter into open debates with the "cranks" they condemn because they are afraid the people will be unable to choose the "correct" opinion, then perhaps scientists should think again about what they are doing to let people know about how science operates. Less generously, perhaps it is time scientists stopped thinking that only scientists should think about scientific knowledge and its implications, and that communication of scientific knowledge is a one-way process from the scientist to the public.

Part of the threat of Velikovsky's ideas involves links between scientists' adherence to paradigms and their vested interest in specialist expertise. Most conventional paradigms are based on the ability of scientists to make valid contributions in a narrow area of research, without needing to be aware of wider aspects of their discipline, much less of radically different disciplines. Velikovsky's approach demands an integration of such fields as history, astronomy, and geology, and places a premium on the generalist. To the extent that the prestige and privileges of scientists are based on their adoption of research topics and ideas that are not easily communicable to scientists in other disciplines or to the general public, so would Velikovsky's multidisciplinary approach appear as a threat to this prestige and privilege.

In short, the antagonism to Velikovsky can be seen to stem from the wide popularity with the public of ideas that dramatically challenge current scientific paradigms. In their unorthodoxy, in their interdisciplinary implications, and in their popularity, Velikovsky's ideas posed a real threat to the professional security of the scientific community.

Conclusion: Comments on the Sociology of Science

Commitment to norms and paradigms, links to outside interests, and a vested interest in professional status: all these factors (and more no doubt) may influence the reactions of scientists and of the scientific community to theories and evidence, and guide individual and group research behaviour. Naturally there will be links between the different factors. For example, it might be that scientists working for companies or government agencies would use their professional status to justify support for theories or research proposals that selectively serve the interests of their employers. Still, it may be fruitful to try to determine what are the most fundamental determinants of scientific behaviour, at least in special cases. It seems likely that results of such an enquiry would be unappetising to traditional sociologists of science.

Sociologists of the functionalist tradition have treated violations of norms — for example by industrial scientists — as exceptions to their theory, which purportedly explains the "true" operation of science. Yet if the penetration of "non-scientific" motives into scientific research is as deep and extensive as suggested here, functionalist theory is treating only a few rare exceptions to the rule — hardly a recommendation for an all-encompassing explanatory system. Similarly, the explanation of scientific behaviour using adherence to paradigms is fine, except when it comes to explaining where the paradigms come from. A more fruitful approach to the study of scientific behaviour may be to look first at whose interests are being served by scientific research — the interests of outside groups, or of professional scientists themselves, as well as the pursuit of knowledge — and then see what sort of norms and paradigms one would expect to serve these interests.

It should not be surprising to any person experienced in the ways of the world that scientists in their work are influenced by links with outside interests and by their vested interest in their own professional position. What requires more explaining is how and why sociologists of science have avoided analysing these factors for so long. As to *how* these factors have been avoided, one major technique has been through assuming implicitly that scientific knowledge and the development of science is unproblematical. (Whitley, 1972, points out the importance that the choice of a philosophy of science, explicit or implicit, has upon the sociology of science that is developed.) The result is that sociologists look mainly at the social processes through which ideas are generated, communicated, and validated, but not at the relation of the content of the ideas to whom they serve in society and how.

The major reason *why* sociologists of science have avoided analysing links with outside interests and vested interests in professional expertise may be self-protection on the part of the sociologists. From the very beginning (e.g. Merton, 1942) the dominant brand of sociologists has explicitly or implicitly denied concern over the content of scientific knowledge: what the theories actually say. This makes it possible for them to avoid learning too much science, and also keeps the scientists out of sociology. It also means that the sociology of science so developed is quite unable to treat social and political influences on the creation of norms or paradigms. (Rose and Rose, 1974, comment upon how such sociology may mirror back the images of science which the scientists themselves preferred to promote.) And of course the idea that scientific behaviour may be constrained and oriented by interest in professional status is definitely dangerous. It might even apply to sociologists.

ACKNOWLEDGEMENTS

Valuable comments on this work have been received from David Bennett, Paul Crutzen, Geoff George, Bob Hewitt, Dougal Jeffries, Jarlath Ronayne, and John Skaller.

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