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David Robbins; Ron Johnston

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The involvement of science in matters of public importance is not a new development. R.K. Merton's study of the Royal Society in 17th century England[1] depicts a group of scientists intimately concerned with the application of science to matters of public concern. Nevertheless, for a considerable period science remained a relatively isolated and uninfluential sub-culture of society. However, along with the growth of science witnessed in this century, both in terms of government support and numbers of scientists, there has been a widespread increase in the involvement of scientists in public affairs.[2] By the 1930s some scientists were actively seeking channels by which they could become involved, as scientists, in the political process. Werskey[3] has described the activities of 'reformist' and 'radical' scientists of this era in 'insider' and 'outsider' politics.[4] Also from this era came Bernal's[5] plea for a planned science responsive to social needs.

This concern with the public role of science, coupled with the dramatic rise in the status of the scientific profession that resulted from its war-time achievements, provided the basis for an ever growing post-war involvement of scientists in social and political affairs. In the realm of 'technical' policymaking, politicians and government bodies actively sought the advice of scientists. For their part, scientists showed an increasing willingness to become involved

Authors' addresses (respectively): Postgraduate School of Librarianship and Information Science, University of Sheffield, Western Bank, Sheffield, S10 2TN, UK; Department of Liberal Studies in Science, The University, Mancester, M13 9PL, UK.

in essentially political processes either by inside channels, as government advisers, or through 'outsider' organizations dedicated to issues such as social responsibility in science or environmental protection.

Generally it is for their specialist knowledge of certain areas of technical and esoteric subject matter that scientists' advice is sought.[6] As Barnes notes in another context, 'the scientist enters "outside" situations as a transmitter of expertise not as a participant in a debate or critical confrontation'.[7] The scientist provides the 'scientific facts' relevant to a particular political decision or public issue, the authority of these facts resting on the accepted power and objectivity of the scientific method. Furthermore this objectivity should ensure that the same 'facts' would be obtained from any scientist, provided he had the necessary competence. In short, the provision of scientific advice has been considered as quite straightforward, offering problems for neither the scientist nor the recipient of the information.

The first indication that there may be a flaw in these assumptions was provided by the bitter conflict between Lindemann and Tizard over advice to the British War Ministry.[8] Since that time, with the growth of the public role of the scientist, there has been a corresponding increase in the frequency of public dispute between scientist advisers. A number of attempts have been made to provide an explanation of the problem of conflicting expertise. Gilpin reached three general conclusions in his account of the disagreement amongst American physicists concerning the feasibility of monitoring an atomic weapons test-ban treaty:

> 1) that the intra-scientist dispute over nuclear weapons policy is due in large part to the scientists' sense of social responsibility;
>
> 2) that in matters of high public policy political and technical factors are often too intertwined to be isolated from one another in the advice of scientists;
>
> 3) that the non-technical assumptions of scientists legitimately and frequently become part and parcel of the scientist's advice. As a consequence of these facts the problem of conflicting expertise has arisen.[9]

Similarly Mazur has argued that the political context of public controversies crucially affects the advice that scientists give.[10] It is important to note that the argument is not that the scientific component of a scientist's advice is open to political influence but that this rational and objective information cannot be separated from non-scientific (i.e. 'political' – usually meaning 'irrational') elements.

The view that non-technical assumptions are part and parcel of scientific advice has been rejected by those who see conflicting expertise as a dangerous challenge to the status of science and the credibility of scientists. Provided advice is limited to questions 'within the competence of science', the claim is made that it should be of the same certainty as that which typifies propositions within science. This view is illustrated by the comments of Scorer, writing as pollution expert:

> An obstacle to good decision-making is disagreement among the experts on technical matters. This does not arise because of genuine disagreement . . . given the opportunity to discuss the question almost any group of scientists would come up with an agreed statement on the state of human knowledge on the subject and probably some recommendations on their degree of certainty. The disagreements arise because scientists are enrolled in opposing camps before they have had a chance to get together.[11]

A similar argument, albeit in a more extreme form, can be found in the controversial report prepared by the Operations Research Society of America on the debate over anti-ballistic missile defence which occurred in the United States in the late sixties.[12] It sets out a code of conduct which demands the stringent separation of the adviser's 'scientific and objective' activities as an analyst and his activities as an adversary.

> The pressures of moral and political preferences are great, but as long as the issues can be resolved by techniques which scientists agree are the right ones the dangers are not great.

Thus if adequate data are available there should be no possibility of disagreement because the use of 'operations research' methods will allow the advisers to reach an 'unquestionable consensus'.

These two camps are opposed concerning the feasibility of separating the technical and non-technical elements in scientific advice; however they are unified in sharing the assumption that conflict and dispute are to be explained in terms of the political views, the bias, or the 'irrationality' of one or more of the disputants. The possibility that there may be genuine scientific grounds for disagreement between different scientists is not admitted. Even Nelkin, in her otherwise admirable analysis of the role of technical advice in the public controversies over the siting of a nuclear power plant on Cayuga Lake and the proposed construction of a new airport runway in Boston,[13] implicitly rests the conflict between experts on their 'political views'.

This focus may reflect the predominant orientation of studies of science by political scientists. In general they have adopted the positivist assumption that scientific knowledge is an unproblematic reflection of reality and concerned themselves almost exclusively with the administration of the scientific institution, with the simple implicit aim of maximizing gross knowledge production. Blume has criticized this approach and noted that 'attempts to assess the impact of political institutions and political ideology upon science have been limited largely to studies of the aberrations of Stalin's Russia.'.[14]

In the light of the post-Kuhnian developments in the sociology of scientific knowledge,[15] we claim there is a need that analyses of such issues as the basis of conflict between experts, and the relationship between political institutions and the scientific community and its sub-groups, be informed by a conception of scientific knowledge not as absolute or given, but as socially constructed. The consequences of such a perspective are considerable and will need detailed explication. More immediately, the possibility is admitted that science is differentiated, to the extent that scientists working within widely separated specialties can on strict scientific grounds reach genuinely opposed conclusions, and that the heat of disputes between experts may be an indication of attempts by competing groups of scientists to establish their professional authority over a particular issue or area.

At the same time as adopting this neo-Kuhnian perspective, it is important that sociological analysis of the production and use of scientific knowledge takes full account of the 'incorporation' and 'industrialization' of contemporary science[16] and avoids the implicit assumption of a closed social system. This paper represents an attempt to unite these perspectives in the analysis of particular instances of conflicting expertise.

THE SCIENTIST AS A PROFESSIONAL

Recognition of the socially constructed nature of scientific knowledge requires that an analysis be made of the factors which affect the particular knowledge the scientist-adviser chooses to draw upon in presenting his advice. This choice will be influenced by the values of the various social groups of which he is a member. Consequently, the analysis of conflicting expertise can be related to the more conventional sociological analysis of the scientist as a professional. The formation and expression of public attitudes by the scientist can be considered to be a manifestation of professional role-behaviour appropriate to dealings with 'outsiders'. 'Role' has been defined by Goffman as 'the activity that the incumbent would engage in, in terms of the normative demands upon someone in his position'.[17] The normative framework within which the scientist works contains components pertaining to cognitive and technical standards, to everyday practice, and to extra-scientific dealings. Thus the scientist will advise and inform outsiders in conformity with a professional ideology.[18]

The nature of the role-behaviour and ideology is, in turn, predicated by the professional identity and hence, professional situation of the scientist. However, not all scientists will have the same professional situation or professional identity. The multiplicity of professional identities available to scientists is a function of the hierarchical sub-division of the scientific community along dimensions of cognitive specialization and occupational variety.[19] The differentiated nature of the modern scientific community ensures that the particular professional sub-groups within it will each subscribe to somewhat different sets of norms considered appropriate to the extra-scientific dealings of its members. The analysis is further complicated by the fact that scientists usually belong to more than one such social grouping and consequently have a choice of role and ideology; for example, an academic scientist potentially may belong to one or more small specialist communities, a wider disciplinary community and the even wider community of academic scientists in general.

What sort of roles, therefore, does the scientist adopt in his relationships with outsiders? The first type we must consider is that related to the specialist community of which he is a member.[20] The specialist community holds a monopoly of information in a definable area of scientific expertise, but as Kuhn[21] has shown this information cannot be viewed simply as a conglomeration of raw facts and theories available for ready distribution to the outsider. Facts are ordered, sorted and interpreted within the theoretical framework currently shared within the specialty. Thus even specialist information on technical matters, widely regarded as the most neutral and 'objective' form of scientific advice, will be predominantly determined by a specialist ideology that is exempt from empirical test.

Secondly, we may consider the scientist as a member of the wider scientific community, which itself shares certain ethics, values and attitudes. In this case the scientist operates *qua* scientist rather than *qua* specialist, and the authority his advice commands will reflect the general status of scientists at the time. In Elliot's terms the role adopted is that of a 'status professional' rather than an 'occupational professional'.[22] In this case it is the norms of the scientific profession, rather than those of the specialist sub-group, which should influence most the counsel that scientists give. However, the tendency of scientists to combine 'specialist' and 'professional' roles without distinguishing between them is a frequent cause of confusion. This has been well illustrated by Reiser's analysis of the US Congressional hearings on smoking and lung cancer.[23]

Political scientists have in general perceived the function of the scientific adviser primarily in terms of these two roles. However, this simple duality is applicable only to academic scientists. In reality, of course, science is one of the most 'occupational' of professions. As Cotgrove and Box[24] and Ellis[25] have pointed out, many science students do not internalize 'the ethos of science' but employ their scientific qualifications in a purely instrumental way in order to fashion careers in industry or government service. If we include these scientists, as seems sensible, among the actors in the public and political discussions concerning science-based issues it becomes clear that we must also consider the effect of occupational differentiation within science on the provision of scientific advice.

Academic, government and industrial scientists operate in widely different institutional settings, with differing norms of behaviour, differing modes of advancement and different peers. It is, therefore, not unlikely that they will tend to adopt different perspectives concerning those issues of public concern on which they may advise and inform. This proliferation of occupational sub-groups within 'science' may well result in further sub-division of the broader specialist and professional roles.

This differentiation and stratification of the modern scientific community along cognitive and occupational lines provides a basis for an explanation of the high incidence of conflict between scientists over issues of public concern. In order to justify and develop this argument we will examine in detail the case of conflicting expertise over the safety of environmental lead concentration; the controversies over environmental irradiation standards and the United States ABM system will be drawn on for illustrative support.

THE PROBLEM OF ENVIROMENTAL LEAD LEVELS

Before the last decade little interest was shown by toxicologists in the possible dangers of lead levels in the circumambient environment. Classical work on lead poisoning centred around two areas:

1) The mechanism of and treatment for acute poisoning resulting from the accidental ingestion of massive amounts of lead.

2) The problems resulting from the exposure of workers in the lead manufacturing industries, particularly lead alkyl production, to environments high in lead.

The first notable departure from these approaches was contained in work carried out in 1949 by Monier-Williams concerning the role and dangers of trace elements in food.[26] Williams warned:

> It cannot be emphasised too strongly that in discussing the amount of lead which may be considered as negligible in food, consideration of toxic limits, as far as these are defined by the appearance of poisoning, is beside the point, and tends to obscure the real question. What we want to know is not so much the toxic limit, as the safe limit, if indeed any limit, however small, for a cumulative poison can be considered safe.

These conclusions elicited little response at the time and doubts did not begin to reappear until the late 1950s when an American committee jointly representing the petroleum industry and the US Department of Public Health considered raising the maximum amount of lead alkyls allowed in petrol.[27] While the committee felt that an increase would not constitute a public health hazard, they noted that 'a conclusive answer is impossible at the present time because of lack of medical data'. As a result the permissible levels of lead in petrol were raised. However, in the early 1960s the discovery by analysts of unexpectedly high levels of lead in roadside vegetation confirmed the possible existence of a problem.[28] Thus in 1965, the President's Science Advisory Committee reported,[29] somewhat uneasily, that, while present levels of lead were not apparently causing disability, 'the margin of safety between present levels and deleterious levels is not certain'. In the same year a scientific paper marked the opening of a full-scale scientific, and later public controversy. The paper[30] by Patterson, a geochemist, argued that citizens of America and other industrialized countries were being subjected to 'chronic insult' by environmental lead. Acrimonious debate as to the validity of such conclusions ensued, and has continued since. Although initially confined to the scientific press it spread rapidly to popular scientific journals and the national press. In addition it has inspired books, television programmes and government reports.

At least partly as a result of the escalation of the controversy into the public and political arenas, advisory and legislative measures to control environmental lead concentrations have emerged.[31] These include new health guidelines for permissible levels of lead in the air, reduction of permissible levels of lead in petrol in many countries and, recently, revised World Health Organization levels of the maximum permissible levels of lead in water.

THE ISSUES INVOLVED IN THE CONTROVERSY

Before examining the bases of conflicting expertise concerning enviromental lead contamination it is necessary to outline the substantive content of the controversy. Four 'key questions' have been identified as being at the heart of the disagreement.[32]

(a) Has the rate of environmental lead contamination increased in recent years?

(b) Does the rise in environmental lead contamination necessarily result in an increase in the average human body burden?

(c) Has the body burden of lead increased in recent years?

(d) Does long-term, low-level exposure to lead damage health?

It is these questions that have so sharply divided scientists during the lead debate. As a US Public Health Service official remarked:

> Some maintain that a large segment of the population is already perilously close to the threshold of lead toxicity as a result of environmental exposure; others take an almost diametrically opposed view.[33]

At one end of the spectrum Patterson and his closest supporters have argued that the answers to each of these questions is 'yes'. This school of thought suggests that modern environmental lead levels are, largely as a result of atmospheric pollution, many times that of natural levels and increasing rapidly. In addition, they claim that, as a result of this increase, average lead concentration in the bodies of modern industrial man is many times the natural lead concentration in human tissues. Finally, they warn of the likelihood of chronic, low-level damage to health as environmental contamination and body burdens increase.

At the other end of the spectrum, a substantial group of scientists, largely occupational hygenicists and other medical scientists, have insisted that the answer to each of these questions (with the possible exception of [a]) is 'not'. This school of thought is exemplified by T.J. Haley's paper,[34] published as a rebuttal to Patterson's work. The paper concludes:

> It has been shown that the U.S. population is in lead balance and that the old causes of acute and chronic lead intoxication have been reduced to very low levels by changes in technology. Lead body burdens have been maintained at the same level for the past thirty years regardless of the fact that there has been a 2.5 fold increase in the use of leaded gasoline. The bulk of lead has been obtained from the food chain, not by inhalation from the atmosphere. Although lead is present in the inhaled air . . . only an extremely small amount contributes to the body lead burden. Body defence mechanisms make it highly unlikely that environmental lead could induce lead insult in the population. The supposed chronic lead intoxication from environmental contamination is myth not fact.

These two viewpoints – the one emphasizing 'sub-clinical hazards' and the other emphasizing 'clinical safety' – represent the extremities of the spectrum of opinion.The controversy attracted the attention of a wide range of scientists, including chemical analysts, ecologists, botanists, agricultural scientists and medical scientists, who took up stances of varying opinion and intensity. As the issues raised health questions, many physical scientists were only marginally involved, whereas medical scientists, particularly those whose work was clinically oriented, were highly committed.

An examination of Patterson's paper,[35] reveals that the author attempted to introduce new cognitive and technical elements[36] into the study of problems related to the health aspects of environmental contamination. Techniques derived from geochemistry, Patterson's own particular specialty, were employed, and these in turn generated new kinds of data. The main target of Patterson's attack was the medically important concept of a 'threshold' below which lead levels in human beings were considered innocuous due to the maintenance of a 'lead balance' by the excretive mechanisms of the body.[37]

In this view, lead poisoning occurs only when the body's defence mechanisms are overwhelmed by massive amounts of lead. This concept had been successful in reducing poisoning amongst workers in high lead environments and had been elevated to paradigmatic status by the research of R.A. Kehoe[38] and his associates from the 1930s onward, providing the basis for most of the work in the field of lead toxicology, particularly within occupational toxicology. This cognitive structure was derived from the methods and expertise particular to medical scientists, depending as it does on the identification of clinical symptoms of lead poisoning and correlation with lead concentrations in blood and urine, the latter data obtained by fairly elementary methods of analysis. It followed also from this paradigm that lead levels in the tissues of the general population, who were not exposed to abnormal amounts of lead, were to be considered as 'natural' – being safely maintained by the body's defence processes.

In contrast, Patterson used the methods of geochronology to calculate biospheric lead levels in the pre-industrial era, from which he concluded that 'natural' lead levels, as found in pre-industrial man, were approximately one hundredth of those found today. In this way he attempted to redefine the concept of 'natural levels', emphasizing that the term as used by the threshold paradigm meant only 'currently typical' levels. In addition, Patterson supported his conclusions by gathering together various pieces of medical research, much of it by Russian authors, which did not conform to the standards of the threshold paradigm and which had been discounted, or at least ignored, by the scientific community.[39] He concluded with a direct attack on the threshold concept:

> The acceptance of typical lead levels in humans in the United States today as normal and therefore as safe or natural is founded on nothing more than an assumption that these terms are equivalent. On the contrary . . . the 0.25 ppm level of lead in blood, which has been and still is regarded with ill-founded complacency actually seems to lie between the average natural concentration of 0.002 ppm and an acute toxic threshold of 0.5 to 0.8 ppm. This suggests clearly that the average resident of the U.S. is being subjected to chronic lead insult. The threshold for damage concept, as applied to lead, is an ill-defined opinion unsupported by any evidence.[40]

THE CONDUCT OF THE CONTROVERSY

The controversy bears all the marks of a conflict between self-contained systems of belief; lacking concepts and terminologies in common, the protagonists tended to 'talk through' each other.[41] The conventional response of occupational toxicologists was the wholesale dismissal of Patterson's approach coupled with a restatement of the threshold paradigm. Thus Machle, an associate of Kehoe, wrote:

> Many clinical and physiological investigations reported in the last thirty years

> have failed to reveal one iota of evidence that existing levels of lead exposure pose any immediate threat to urban populations . . . there is scarcely an element common in man that does not exhibit a critical threshold for damage. Fortunately, many of these levels, including lead, have been established and accepted.[42]

As one commentator perceptively observed:

> The crux of the problem is that the classical concept of acute lead toxicity lacks terms to answer Patterson's arguments. There is no recognition of low-level damage, lead poisoning is defined by clinical signs and symptoms . . .[43]

Only on very few occasions did occupational toxicologists attempt to argue on the basis of particular empirical issues arising from the 'environmental' paradigm. When they did venture empirical evidence, they found themselves on unfamiliar ground and were consequently vulnerable. Thus Haley[44] produced a selection of published data which he interpreted as indicating that environmental and body lead levels had remained constant since about the 1930s. In contrast the scientists who inclined towards Patterson's views, many of whom were physical scientists with expertise in analytical methods, interpreted the results as indicating a well-established exaggeration of lead values by earlier analytical techniques.[45] These different interpretations of lead concentration data clearly have their origin in the different cognitive and technical standards of the specialties of the disputants; in particular, the analysis of the medical scientists reflects their traditional utilization of analytical techniques in a purely 'tool-using' capacity.[46]

This type of clash on substantive issues was not, however, typical of the controversy. In general, occupational toxicologists responded to Patterson's attack in two related ways, both designed to stigmatize the environmental paradigm as inapplicable to medical matters. Firstly, they mobilized the full weight of their professional authority behind toxicological orthodoxy. This type of response is illustrated most convincingly by the statement which appeared in the *British Medical Journal* in 1968,[47] when the environmental arguments were gaining ground. The statement, which was signed by an international élite of toxicologists, flatly declares that adult blood lead levels of up to 0.8 ppm are 'acceptable'. Two sorts of function may be ascribed to action of this form. Its external function is to persuade and reassure public and clients in order to maintain the professional authority of the group with regard to that aspect of the public good for which it is implicitly held responsible. Its second and internal function is to underline cognitive orthodoxy and so to discourage deviance amongst toxicologists.

The second aspect of medical response, which was designed to achieve identical ends, consisted of a denial that environmentally trained scientists had any competence to discuss matters that were essentially medical. A typical comment was that made by a toxicologist in the process of criticizing the editors of *Archives of Environmental Health* for allowing publication of Patterson's original paper: '. . . The kindest thing you could have done for this young man was to have advised him to stick to his own area of competence'.[48] Another remarked that Patterson 'had confused competence in geology with competence in biology and has used geochemical data to draw curious biological inferences'.[49]

This denial by occupational toxicologists of the 'environmentalists'' cognitive competence is clearly bound up with a reassertion of their own professional authority as arbiters of such matters.[50] Thus, in a reply to an article by the chemist Bryce-Smith, the toxicologist Williams wrote:

> Before being considered expert in a discipline it is usual to undergo training and to undertake original work; it is also usual to produce evidence before publishing hypotheses. Bryce-Smith says he is like a commentator at a football match – a game he has never played, as an organic chemist with no training in toxicology, epidemiology or statistics . . .[51]

Bryce-Smith's reply was equally acrimonious:

> . . . Williams attempts to present an industrial view on what is a problem primarily outside industrial medicine. The preposterous nature of some of his assertions and the general non-constructive tone of his article do not encourage one to treat this polemic as a serious contribution to the debate.[52]

It will be apparent from the above discussion that, in the course of the controversy, disagreement over the validity of rival sets of cognitive and technical norms is intertwined with conflict over professional norms. Scientific research in occupational toxicology is bound up with the practice of industrial medicine. The two share the same subject matter, the same need to cooperate with employers and, frequently, the same personnel. Academic occupational hygenicists generally operate as consultants to industry and maintain collaboration with their industrially practising colleagues, with a resulting lack of separation of peer groups.[53]

In a real sense, therefore, industry and the public health service (which rely on industrial medicine in the formulation of regulations dealing with working conditions) are 'clients' of occupational toxicology. As a result, professional behaviour is regulated by a set of norms which have emerged in order to stabilize and safeguard the professional-client relationship.[54] It was these norms that came under attack from other scientists who considered, in effect, that they were inimical to a rational resolution of the problem, and the interest of society as a whole.[55] Thus Patterson wrote in response to his critics:

> It is becoming increasingly difficult for industrial toxicologists and occupational health physicians to act as chief judges in these matters. If they wish to minister to the health of millions exposed to pollution they face the hard eyes of men who understand the differences between a scientific activity and its utilization for a purpose, of men who cannot be deceived into believing that the purposes of industry and public health are parallel rather than opposed, and of men in basic research disciplines in the universities who will compete determinedly with them for the right to guide the views of the Public Health Service. It behoves industrial toxicologists and physicians in occcupational health not to respond to this challenge as if their heritage were being stolen, but to reflect whether they have renounced it . . .[56]

These exchanges typify much of the controversy, in which attacks on opponents' competence, accusations of data manipulation and, lately in the US, court actions for defamation have all occurred. There is precious little evidence of the disinterestedness and universalism held by Merton to be the ethos of science.[57] Rather, the violence of the controversy suggests conflict between two groups dedicated to distinct integrated sets of professional and cognitive norms and determined to gain or maintain respectively their professional authority over an issue. As Elliot has observed:

> The patterns of thought and activity which develop in a profession are supported internally and externally by its own structure and the relationship it has established with other organizations and associations. Not only careers and economic interests are at stake, but also established patterns of thought and ways of approaching the world.[58]

It thus appears that the difference in technical, cognitive and professional standards, and the way they are mixed between the rival sets of experts provides an explanation for the conduct of the controversy and for their inability, at least in the short term,[59] to reach any form of consensus.

CONCLUSION

We have been able to show that the controversies associated with the environmental lead levels, low-level radiation and the ABM system have a number of common underlying causes. In particular, the conflict between various groups of scientist experts has been related to cognitive and occupational differentiation within the scientific community — that is, to differences in technical and cognitive standards and/or professional norms between different sub-groups of scientists.

This focusses interest on the formation and maintenance of what might be called the ideology[60] of particular groupings of scientists. There has been relatively little empirical investigation of this phenomenon, though studies of American psychiatrists[61] have identified three conflicting ideologies within psychiatry, based on whether the problems of the mentally ill are seen as being of physical, psychological or social origin. The first two viewpoints are firmly established and are supported by an educational and organizational infrastructure, but the socio-therapeutic ideology appears to be much less well developed. Strauss found that differences in ideological position could be related to differences in training, in links with professional organizations, and in occupational situation in different institutions. For example, the constraints of large-scale mental hospitals (large case-load) served to emphasize an ideology which stresses the physical basis of mental illness and the use of drug therapy as a form of treatment. In contrast, psychoanalysts in private practice are oriented towards the needs of single fee-paying patients, and as a result stress the psycho-social basis of illness and therapies based on intensive, long-term personal analysis. Strauss also points out ways in which differences in ideology extend into wider points of view. Different psychiatric ideologies involve not only differing cognitive content (that is, differing 'scientific' facts and theories) and therapeutic practice but also 'different views on the morality of different types of treatment and on the general role and responsibility of the psychologist'. In effect, ideology forms an integrated normative system governing not only practice but also scientific and professional belief. The analogy between the conclusions of these studies and the analysis of conflicting ideologies in the lead and radiation controversies needs little elaboration. In a similar vein, a study of a cancer research institute[62] revealed marked differences of ideology between workers oriented to medical practice and those oriented towards pure science. The former adopted a 'therapy' ideology stressing empirically developed therapy techniques, the latter adopted a 'basic science' ideology stressing fundamental scientific problems. The differences were correlated to differences in training, specialization and occupational role.

This ideological differentiation provides the basis for conflict. But with the fragmented structure of modern science, scientists in different specialties are relatively isolated. The overlap of cognitive authority on particular problems may lead to collaboration between the specialists involved and the emergence of a new cognitive consensus with its consequent social institutionalization. On the other hand, if the specialties possess strongly established professional and cognitive structures their interaction may take the form of a bitter conflict exemplified by the cases discussed in this paper. This form of conflict may occur in the case of 'purely' scientific matters or it may involve those science-based questions of public concern discussed above.[63] The fact that such issues are broad in scope, containing a multiplicity of variables, tends to ensure that they cannot be confined within the cognitive province of any one group; and, in addition, the prevalence of considerable empirical uncertainty allows the conflicting cognitive structures to exist in a state of stalemate. The violence of the dispute over professional hegemony is only increased by being carried out in the public arena where the status of the professional groups involved can be seen to be under challenge.

Thus, whether a scientist, as a specialist, offers his accredited opinion via 'insider' or 'outsider' channels, his perception of the problem and the knowledge he can offer to resolve it will be structured by a cognitive and professional framework. Even in the role of 'status professional' his views will not be free from sub-group ideologies. Hence analysis of scientific 'advice' will need to take full cognizance of the differentiated nature of the scientific community and the consequences of this differentiation for the knowledge that is produced. Indeed, this claim can be extended in general to 'political' analysis of the scientific community and its operations.

NOTES

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1. R.K. Merton, *Science, Technology and Society in 17th Century England* (New York: Harper and Row, 1970).
2. The involvement of US scientists in public affairs has been charted by R. Gilpin and C. Wright (eds), *Scientists and National Policy-Making* (New York: Columbia University Press, 1964); the best, though inadequate, study of UK developments is H. & S. Rose, *Science and Society* (London: Penguin, 1969).
3. P.G. Werskey, 'British Scientists and Outsider Politics, 1931-1945', *Science Studies,* Vol. 1 (1971), 67-83, and *Scientists for Socialism* (London: Allen Lane, forthcoming).
4. A scientist may act politically by advising officials in the executive branch of government, in which case he is using insider channels. On the other hand he may operate as an outsider, offering information and opinion direct to the legislature or to the public; studies of the effectiveness of the two roles and the consequences for the scientist of adopting them include Gilpin and Wright (eds), op. cit. note 2, and R. Williams, 'The Political Context of Scientific Advice', IPSA IXth World Congress, Montreal (1973). Throughout this paper 'advice' includes information tendering through either insider or outsider channels.
5. J.D.Bernal, *The Social Function of Science* (London: Routledge and Kegan Paul, 1939).
6. The scientist's knowledge is, of course, not sought merely to inform; the political influence of science through the power of its rationality 'to authorize and certify facts and pictures of reality' has been noted by Y. Ezrahi, 'The Political Resources of American Science', *Science Studies,* Vol. 1 (1971), 121; the use of science to depoliticize public issues, such that they become 'technical' (to be decided by experts) rather than political (where the public may have a say) has been emphasized by many critics of technology: e.g. J.D. Douglas (ed.), *Freedom and Tyranny* (New York: Knopf, 1970), and D. Nelkin, 'The Political Impact of Technical Expertise', *Social Studies of Science,* Vol. 5 (1975), 35-54.
7. B. Barnes, *Scientific Knowledge and Sociological Theory* (London: Routledge and Kegan Paul, 1974).
8. C.P. Snow, *Science and Government* (London: New English Library, 1963).
9. R. Gilpin, *American Scientists and Nuclear Weapons Policy* (Princeton: Princeton University Press, 1962).
10. A. Mazur, 'Disputes between Experts', *Minerva,* Vol. 11 (1973), 243-62.
11. R.S. Scorer, *Air Pollution* (London: Pergamon, 1968).
12. The ORSA report, 'Guidelines for the Practice of Operations Research' was produced in response to the conflicting analyses presented to the US Senate Armed Services Committee and later the public, during the 1969 debate on

antiballistic missile defence. On the basis of the code of conduct which it sets out, those scientists who testified against the ABM system were censured and those who advised for it were praised. Much of the subsequent debate on the role of scientists as advisers was conducted in the pages of *Minerva,* Vol. 10 (1972).

13. Nelkin, op. cit. note 6; *Nuclear Power and its Critics* (Ithaca, N.Y.: Cornell University Press, 1971); and *Jetport: the Boston Airport Controversy* (New Brunswick, N.J.: Transaction Books, 1974).

14. S. Blume, *Toward a Political Sociology of Science* (London: Macmillan, 1974), 17. Similarly, 'science policy' in the sixties was concerned primarily with the management of science. Recent notable studies of political influence upon the development of science include R.M. Young, 'Darwin's Metaphor: Does Nature Select?', *The Monist,* Vol. 55 (1971), 442-503; P. Forman, 'Weimar Culture, Causality and Quantum Theory', *Historical Studies in the Physical Sciences,* Vol. 3 (1971), 1-115; and J. Harwood, 'The Race and Intelligence Controversy: A Sociological Approach', of which the first part may be found elsewhere in this issue. It should be noted that we are not seeking to deny that scientists can assume political roles or that their views can be affected by political convictions. However, we consider it might be useful as a first stage in the analysis of conflicting expertise to see what grounds exist for the conflict within the scientific enterprise itself.

15. Summaries can be found in R.D. Whitley (ed.), *Social Processes of Scientific Development* (London: Routledge and Kegan Paul, 1974), and B. Barnes, op. cit. note 7.

16. The incorporation of science as merely an element of the capitalist production system has been argued by H. Rose and S. Rose, 'The Incorporation of Science', delivered at the BSSRS Conference on 'Is a Socialist Science Possible?', London, 1974. The increasingly industrialized nature of contemporary science and the consequences for science as an institution has been emphasized by J.R. Ravetz, *Scientific Knowledge and its Social Problems* (London: Oxford University Press, 1971). Blume, op. cit. note 14, argues for a new sociology of science which takes full account of the links between the social structure of science and the political system.

17. E. Goffman, *Encounters* (London: Penguin, 1961).

18. The term 'ideology' is employed in this paper in a somewhat restricted sense. It refers solely to those systems of closely related beliefs, ideas and attitudes that exist within professional groupings of scientists by which such groups make sense of their work experiences and further their collective professional aims. Thus, the term is not used in its 'total' sense, as a '*Weltanschauung*', nor in the orthodox Marxian sense of 'false consciousness'.

19. A separate paper is being prepared which examines the forces behind the differentiation of the scientific community and which relates them particularly to a variety of social and political pressures.

20. The existence of distinguishable self-contained groups of scientists defined by specialty concerns has not been taken as self-evident: cf. J. Law and D. French, 'Normative and Interpretive Sociologies of Science', *Sociological Review,* Vol. 22 (1974), 581-95. The study has been based not on assumptions of conflict between specialties but on identifying the ideology revealed by particular vocal participants in the controversy – the fact that we may be dealing with an élite does not invalidate our findings. Moreover this study indicates the

methodological value for the sociology of scientific knowledge of examining controversies. The problem of identifying the boundaries between specialties is avoided and also, under situations of conflict, the cognitive standards and professional norms are more clearly revealed.

21. T.S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

22. P. Elliot, *The Sociology of the Professions* (London: Macmillan, 1972), 15-57.

23. S.J. Reiser, 'Smoking and Health: The Congress and Causality', in S.A. Lakoff (ed.), *Knowledge and Power* (New York: Free Press, 1966); see also S.B. Barnes, 'The Reception of Scientific Beliefs', in S.B. Barnes (ed.), *Sociology of Science* (London: Penguin, 1972), 269-91.

24. S. Cotgrove and S. Box, *Science, Industry and Society* (London: Allen and Unwin, 1970).

25. N.D. Ellis, 'The Occupation of Science', *Technology and Society*, Vol. 5 (1969), 33-41.

26. G.W. Monier-Williams, *Trace Elements in Food* (New York: John Wiley, 1949).

27. US Department of Health, Education and Welfare, *Public Health Aspects of Increasing Lead Tetraethyl in Motor Fuel*, Bulletin 712 (1953).

28. H.L. Cannon and J.M. Bowles, *Science*, Vol. 137 (18 August 1962), 765.

29. Report of President's Science Advisory Council, *The Quality of the Environment*, US Government Publication 1271 (Washington, D.C.: USGPO, 1965).

30. C.C. Patterson, 'Contaminated and Natural Lead Environments of Man', *Archives of Environmental Health*, Vol. 12 (1966), 781.

31. T.J. Chow, 'Our Daily Lead', *Chemistry in Britain*, Vol. 9 (1973), 258.

32. 'Introduction to a Controversy' (Editorial), *Scientist and Citizen* (April 1968), 50.

33. Ibid.

34. T.J. Haley, 'Lead Intoxication from Environmental Contamination – Myth or Fact?', *Archives of Environmental Health*, Vol. 12 (1966), 781.

35. Op. cit. note 30.

36. M.J. Mulkay, *The Social Process of Innovation* (London: Macmillan, 1972).

37. It is interesting that the same concept of 'threshold' was a critical focus of the debate in the controversy in the US over the biological hazards of low levels of ionizing radiation; see J.W. Gofman and A.R. Tamplin, *Poisoned Power* (London: Chatto and Windus, 1973) for a review of the controversy from the viewpoint of one set of contestants. The shape of the dose-response curve at lower levels is uncertain; Gofman and Tamplin argued that the relation is linear, whereas the Atomic Energy Commission and their supporters claimed that there is a threshold, with a cell regeneration effect at lower doses; see R.W. Holcomb, 'Radiation Risk: a Scientific Problem', *Science*, Vol. 167 (6 February 1970), 853.

38. R.A. Kehoe, 'The Metabolism of Lead in Man in Health and Disease: The Harben Lectures (1960)', *Journal of the Royal Institute of Public Health*, Vol. 24 (1961), 81-203.

39. One British researcher commented that the Russian work was apparently carried out at such a level of precision that 'it is too good to be true, and anyway,

in the case of behavioural studies, it is impossible to attribute any reliability to the results unless you know the people doing the work and the animal handling techniques they use' (interview). A similar case of rejection of the research of others on tacit grounds has been noted by H.M. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology*, Vol. 9 (1975), 205-24. In a similar vein, the evidence produced by Gofman and Tamplin in the radiation controversy was discounted by the AEC on the basis of previous performance; in the words of the Director of the AEC Division of Biology and Medicine: 'This is the third time Gofman's taken off on a wild tangent'; J.R. Totter, in P.M. Boffey, 'Radioactive Pollution: Minnesota Finds AEC Standards too Lax', *Science*, Vol. 163 (7 March 1969), 1043.

40. See op. cit. note 30.

41. See op. cit. note 21.

42. W.A. Machle, 'Letters', *Archives of Environmental Health*, Vol. 12 (1969), 269.

43. Op. cit. note 32.

44. Op. cit. note 34.

45. For example, in the last four decades the accepted concentration of lead in sea water has dropped from 5 micrograms per litre to 1/1500 micrograms per litre as analytical techniques have been refined.

46. J.R. Ravetz, op. cit. note 16, 373.

47. 'Diagnosis of Inorganic Lead Poisoning: A Statement', *British Medical Journal*, Vol. 4 (1968), 501. Several of the scientists that signed the statement did so on the basis that it was a fair criterion of toxicity when applied to industrial workers exposed to lead. When it was published as a general criterion for both industrial and environmental toxicity these scientists subsequently withdrew their support.

48. K.R. Bencot, 'Letters', *Archives of Environmental Health*, Vol. 12 (1966), 138.

49. Ibid., 139.

50. Similarly, the ORSA report can be seen as an attempt by the growing profession of operations research to claim a monopoly of professional authority in the provision of scientific advice, at least where weapons systems are concerned.

51. M.K. Williams, 'Lead Pollution on Trial', *New Scientist* (9 September 1971), 578-80.

52. D. Bryce-Smith, 'Dr Williams' Lead Balloon', *New Scientist* (30 September 1971), 772.

53. The history of lead toxicology is an excellent illustration of this relationship. The manufacture of the toxic lead tetraethyl anti-knock only became feasible due to the efforts of R.A. Kehoe acting as a consultant to the Ethyl Corporation. In gratitude, the Corporation provided finance to support the founding of the Kettering Laboratories, associated with the nearby University of Cincinnatti and headed by Kehoe. The Kettering Laboratories became the centre of much important work on lead toxicology, in particular that concerning 'lead threshold' and 'lead balance'.

54. It is interesting to speculate as to the importance of this relationship in determining cognitive and technical norms. To what extent has the occupational role of industrial toxicology determined the type of knowledge

produced? Such cognitive norms as 'threshold' could have emerged in order to resolve the potentially conflicting demands implicit in the professional role of the occupational toxicologist as knowledge-producer and adjunct to the productive process; studies of this relationship are continuing.

55. Similarly, Gofman and Tamplin and their supporters (who were almost exclusively university scientists) were critical of the scientists of the AEC and allied government agencies because of their allegiance to an institutional ideology, based on promoting the safest radiation standards consonant with the relatively unimpeded development of atomic power.

56. C.C. Patterson, 'Letters', *Archives of Environmental Health*, Vol. 12 (1966), 13.

57. R.K. Merton *The Sociology of Science* (Chicago: University of Chicago Press, 1973), 267-78.

58. P. Elliot, op. cit. note 22, 150.

59. A follow-up study of occupational toxicologists revealed that many of the younger scientists had adjusted their cognitive system, at least partly in response to the lead controversy, to include a number of concepts and instrumental techniques from the field of chemistry.

60. See note 18.

61. A. Strauss et al., *Psychiatric Ideologies and Institutions* (New York: Grune and Stratton, 1961); see also D. J. Armor and G. L. Klerman, 'Psychiatric Treatment Orientations and Professional Ideology', *Journal of Health and Social Behaviour*, Vol. 9 (1968), 9-27, and M.R. Sharaf and D.J. Levinson, 'Patterns of Ideology and Professional Role Identification among Psychiatric Residents', in M. Greenblatt, D.J. Levinson and R.H. Williams (eds), *The Patient and the Mental Hospital* (Evanston, Ill.: Free Press of Glencoe, 1967).

62. P. Elliot, 'Men against Cancer and Men and Women against Cancer in a Research Institute and Cancer Hospital' (Leicester: Centre for Mass Communication Research, University of Leicester, 1971, mimeo).

63. This commonality provides the opening for the extension of concepts used in the analysis of conflict and its resolution within the scientific community on strictly scientific matters to controversies over quasi-scientific issues of public concern as described here; at the same time 'scientific' conflict may be illuminated by consideration of the cognitive standards of the disputants and their possible concern to maintain intellectual authority in a particular area; on this question see P. Bourdieu, 'The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason', *Social Science Information*, Vol. 14 (1975), 19-24.

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