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Studies in Research
and Higher Education

Scientific Expertise and the Public

Conference proceedings

Editor: Hans Skoie

INSTITUTE FOR STUDIES IN RESEARCH
AND HIGHER EDUCATION

The Norwegian Research Council
for Science and the Humanities

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P R E F A C E

"Scientific Expertise and the Public" was the topic of an international conference sponsored by the International Council for Science Policy Studies and our institute at Voksenåsen, Oslo, June 20-22 this year. The Conference was prepared by a Programme Committee including:

Yaron Ezrahi, Hebrew University of Jerusalem
Elisabeth Helander, Academy of Finland
Everett Mendelsohn, Harvard University
Dorothy Nelkin, Cornell University
Peter Weingart, Universität Bielefeld
Hans Skoie (Chairman), Institute for Studies in Research and Higher Education, Oslo

At the institute the following has taken part in the organization of the conference and the publishing of this report: Arild Steine, Nils Roll-Hansen, Robert M. Friedman, Sveinung Løkke, Mari Heiberg and Kristin Rosenberg.

The conference dealt with important questions related to the use of scientific expertise in modern society, and we are happy to make the conference papers available to a wider audience.

Oslo, September 1979

Sigmund Vangsnes

PREFACE BY THE CHAIRMAN OF THE PROGRAMME COMMITTEE

The relationship between scientific expertse and the public is growing increasingly complex. Today it is hard to avoid seeing the immense bearing of science and technology upon the work of government agencies and, indeed, science and technology as crucial factors in everyday life. Energy and medicine are obvious examples. The question of how to make better and more proper use of scientific expertise has come to the forefront, and the impact of science and technology has led to critical questioning of the very direction of science and the way in which scientific knowledge is acquired.

The aim of this conference has been to examine through some case studies questions arising out of the use of scientific expertise in some areas. In the cases examined we have tried to bring out what kind of expertise was involved, the ways in which this involvement took place, and direct and indirect consequences of the use of expertise. We encouraged a historical and comparative approach in the presentation of the studies. For the same reason a few papers of a more general and theoretical nature were included.

We were in the fortunate position of being able to draw upon the experience of the conference convened in May 1978 by the International Council for Science Policy Studies and the Science Studies Unit (Forschungsschwerpunkt Wissenschaftsforschung) of the University of Bielefeld, with the closely related theme of "The Social Assessment of Science". (The proceedings are published as Report No 13 of the Science Studies Unit.)

We are grateful to the speakers, commentators, and participants from various parts of the world who contributed towards a better understanding of these complex matters. Limited as our approach may be compared to the vast topic of "Scientific Expertise and the Public", we still feel that these papers deserve a larger audience than the one present in Oslo. We publish the proceedings in the hope that they may inspire more studies and analyses, and make further international comparison possible. We are grateful to the speakers for pleasant cooperation and contribution towards a rapid publishing of all papers.

Lastly, we thank the Norwegian Research Council for Science and the Humanities (the Programme in History and Philosophy of Science) for financial support to the conference.

Hans Skoie

C O N T E N T S

WELCOME ADDRESSES

Anders Omholt, Director General of the Norwegian Research Council for Science and the Humanities

Ladies and Gentlemen, it's a great pleasure for me on behalf of the Norwegian Research Council for Science and the Humanities to wish you heartily welcome to this conference on Scientific Expertise and the Public.

Mankind has passed through the energy revolution, now using amounts of energy which are orders of magnitude greater than man's own muscles can provide. The data and information revolution has just started, probably leading to even greater changes in the life of man than has the energy revolution caused.

We have an efficient but complicated industrial structure, based on science and advanced technology. Defense systems, the infrastructure of our society and our daily life is likewise heavily influenced by science. This leads to a society which is increasingly dependent on science and scientists.

Opposition against our scientific and technological society has arisen. People feel increasingly dependent on forces which they cannot control, but which they think should be controlled by the public, and not by the scientists and the technological elite. In ancient times, when nature provided the dominant forces in man's daily life, such control was usually impossible. This is no longer so. And the public now wants to exercise control over the present day's dominant forces, either itself, or through the political system.

Unjust distribution of wealth among the world's nations is another challenge to scientists and to politicians. Serious concerns have arisen about man's environment. Conservation of nature and more difficult access to resources is yet another challenge. And if man's curiosity does not stop, and fortunately it does not, intense efforts will be put into research on an ever increasing number of fundamental problems in all branches of science. No wonder that the relation between the scientific expertise and the public is a serious and challenging problem.

Our research council has as its main responsibility to try to make Norwegian research flourish. But we also feel as a great responsibility to contribute to the public's understanding of science, and science policy, as well as to the scientist's understanding of the society in which he lives. Science policy is, in our opinion, an increasingly important element in our total national policy. These are some of the important reasons why we created and maintain the Institute for Studies in Research and Higher Education. We think that this is a worthwhile effort, and will continue to support the institute.

In this field, as in most others, international contact and collaboration is a necessity. This conference fosters such contact and collaboration, and it will, as I tried to demonstrate, illuminate very important problems. I am sure that you who have come to Norway to participate in this conference - and also the Norwegian participants - share this view. I expect and trust that we can look forward to interesting and valuable sessions. I wish you and ourselves all success with this conference.

Jean-Jacques Salomon, President, International Council for Science Policy Studies

Mr President, Ladies and Gentlemen, it is an honour and a privilege for me to open this conference in the name of our International Council for Science Policy Studies. I would like to use this opportunity to thank the Institute for Studies in Research and Higher Education as well as the Norwegian Research Council for Science and the Humanities, which have accepted to co-sponsor this conference.

The theme of our meeting this year, "Scientific Expertise and the Public", is a follow-up of that of our last meeting in Bielefeld in 1978, on "The Social Assessment of Science". The notion of a social assessment of science is not exactly obvious. When, ten years ago, we were preparing at the OECD what came to be known as the Brooks Report, we felt free to discuss any problem raised by technological development. To speak of a social assessment of science was,however, at that time as unorthodox as possible, and it was assumed that the scientific establishment would be totally challenged by exposure to critics outside the scientific community. To many of us it seems as if a striking change in attitude has taken place during this decade. When we now convene here in Oslo to discuss the relationship between scientific expertise and the public, there is hardly anyone feeling that by bringing in critics who are themselves not scientists, we mount a total challenge to the scientific establishment.

Increasing scientific and technological sophistication has brought real problems and dangers in its wake. Science now has to live with being questioned by people outside the scientific community, by the public at large. This is certainly a relatively new challenge. John Dos Passos once wrote of Edison, "the Wizard", that "he never worried about mathematics, nor about social systems, nor about generalized philosophical concepts". Dos Passos was certainly wrong with regard to mathematics and science; Edison did his work being formally informed of what were the real trends not only in physics, but also in chemistry. But certainly it is true with regard to his lack of concern towards social systems and philosophical ideas. In the nineteenth century of industrial expansion neither science nor technology had to take up challenges from people outside the scientific community. This era of innocence for science and scientists is now over.

Because of technological proliferation, because of the many threats resulting from scientific development, there are today many problems not only in the relationship between science and the public at large, but also between science and those who are directly involved in the practice of scientific research. Certainly we need to know, to learn, how to make better use of scientific development and, all the more, of technological applications. This is, quite simply, why I think this conference is relevant to the concerns of the public today. In the name of our Council, I welcome the initiative to organize this meeting, taken jointly by our Council and the Institute for Studies in Research and Higher Education.

This is the last time I have the privilege of addressing you in the capacity of President of this Council. I would like to take this opportunity to thank all my colleagues for the support they gave me during the many years I assumed this responsibility, and for the friendly cooperation all our members have shown. And it is a pleasure to express our gratitude to Hans Skoie and his Norwegian colleagues who have taken the burden of organizing this meeting so well. I think we are starting under the best auspices, and I hope we shall continue successfully these two days.

CONFLICTS BETWEEN POLICY RESEARCH AND DECISION MAKING

James S. Coleman, Department of Sociology, University of Chicago

I want to discuss a set of questions concerning the relation between social research and social policy. The general import of my remarks will be that matters are not at all as the conventional wisdom would have it.

But first, as a counterpoint to the relation as it actually occurs, it is helpful to indicate some common conceptions about the way research and policy are related. It is often believed that research is part of an orderly and systematic policy-making process. As this conventional wisdom would have it, research constitutes the information base upon which policy decisions are made. Seen in this way, policy decisions require information if they are to be made objectively, and research is the handmaiden to policy, providing that information base. Or, more nearly as the researchers would see it, research results tell the policy-makers just what policies they should carry out.

Reality, of course, is quite different. Research and policy are often uncomfortable as bedfellows. Research does not fit well either in its execution or in its results, with the organizational structure and the time requirements for decision-making in an administrative system. Perhaps the greatest source of incompatibility is in timing. Policy decisions have a time schedule of their own, and research has its time schedule as well. These schedules are often in serious conflict. For example, there is now in progress in the United States an ambitious health insurance experiment, to determine how the use of medical facilities by persons is affected by the size of the deductible and the coinsurance. The experiment is designed to provide an information base for national health insurance legislation which is imminent. However, the legislation will hardly wait for the research results. Legislation is even now being prepared, and there are various political careers, including those of Edward Kennedy and Jimmy Carter, which will be affected by the timing of that legislation. Thus it is unlikely that this elaborate research will provide results in time to be of use for informing policy.

When the details of policy making are seen at close range, it becomes quickly apparent that policy has a gestation and birth process all its own, to which research could accomodate itself only with great difficulty. For example, late in 1969, President Nixon and his advisors in the Department of Health, Education and Welfare conceived of legislation to appropriate federal funds to aid school districts undergoing extensive school desegregation for the first time in the Fall of 1970. This was the first time any President had proposed what was certainly a most sensible policy: to provide financial aid for the extraordinary reorganization of which school desegregation consisted in the South. Presidents had sent in troops, and had cut off funds; but none had provided funds to aid the process. One might have thought it to have wide political support. But it did not. Many liberal Congressmen opposed it because it lacked the punitive quality toward school districts that had been a hallmark of desegregation policy, and also because it benefitted the South. Northern Congressmen opposed it for the latter reason alone; it would give money to Southern school districts, but none to their own. Many conservative Congressmen didn't like it because it encouraged school integration, which they were fighting against. Within the Nixon administration itself, there was a strong division of opinion about the bill.

The upshot of all this was that it was not possible, in the Fall of 1969 or Winter of 1970 to foresee that a bill could be passed in the Spring or Summer, and thus to plan a short-term intensive research activity which would inform the bill, by indicating ways in which a school district should spend the money to make integration successful. Instead, all efforts were devoted to gaining support for the legislation, or to infighting within the administration (primarily over use of the funds for bussing.) Sporadically, efforts were made to draft legislation, but these were on-again off-again attempts, periods of frenetic activity followed by complete inactivity, as the political prospects of the legislation waxed and waned, as the Administration grew hot or cold on it, or Congressional prospects looked bright or dim. There was one such attempt: a policy research organization run by a political scientist, Anthony Downs, was asked to survey districts which had desegregated, to learn what policies helped and what ones did not. And because of the political difficulties, passage of the bill was delayed; thus those results could have been used. But the unpredictability of the legislation destroyed all incentive for coordinating the content of the legislation with the research results as they became available. Instead, there was hurry up and wait: a frenzy of activity, using whatever wisdom was easily available, when it appeared that some political consensus had been reached; and then total inactivity when it appeared that the bill was dead. In the meantime, the researchers had developed their own schedule, their own activities, and were carrying these on, in order to provide information when the research was complete (not when the information was needed), oblivious to the peripatetic gestation of the legislation. Thus even when research was explicitly commissioned to inform policy, the two were imcompatible, each marching to its own drummer. (1)

Admittedly this is an extreme case. Yet even if the example has qualities of a caricature, one can see traces of the same incompatibility in a variety of cases where research was intended to inform policy.

I have dwelt at some length on the timing incompatibilities between research and policy, incompatibilities which reduce the likelihood that research will inform policy. There are other incompatibilities as well. These incompatibilities, however, are not wholly overriding. Research does sometimes affect policy, although not always in the neat and orderly way envisioned in the example I have just presented. It is useful to attempt to see just how this has happened in some cases, to get some suggestions of what strategies might be used to make research more generally useful in policy.

I will begin with the example of the report Equality of Educational Opportunity of 1966, both because I know it well, and because it was very widely used in policy, in unexpected ways. (2) This research was completed in 1966, as a report of the U.S. Office of Education to the President and Congress. However, the U.S. Office simply did not know what to make of it. The research results didn't fit with existing policies. (For example, just the year before, extensive new legislation to provide financial aid for school buildings had been designed by the Office of Education and passed by Congress; yet the report said that physical facilities of a school were unrelated to the learning that occurred within it.) The research made no recommendations for new policies, but merely threw doubts on the effectiveness of existing ones.

The reactions of the Commissioner of Education and the Secretary of Health, Education and Welfare, both very enlightened and research-oriented men (Harold Howe and John Gardner), and of their respective organizations, had two components: a wariness about how the research might be used by political opponents in ways that could hurt them; and a scepticism about how they could use it in any positive way. Until the report's results were brought to light in a Senate hearing by Abraham Ribicoff, the report had been an embarassment not to be displayed in public. I suspect this reaction is a more general one at the interface between research and executive agencies. It suggests a kind of ill-fittingness of research to policy that is different from the timing incompatibilities I have described before. I believe this is because research results can legitimate a challenge to policy, and thus to the authority system that makes such policy. Administrative authority obtains its legitimacy from the political mandate which has endorsed (explicitly or implicitly) its policies. Research obtains its legitimacy from a different source: from its claim to express the objective facts. Thus if its "objective facts" fail to support the administrative policy, it undercuts the legitimacy of that policy.

If all this is so, then how is it that the 1966 research I referred to was widely used in policy? The answer is clear: it was not used as an input to policy-making in an authority system; it was used by protagonists in a conflict. It was never used, and so far as I can see, would never have been used, by the U.S. Office of Education. There was, in fact, very little they could use it for, with one exception, which I will note shortly. But what it could be used for, and what it was widely used for, was to challenge the existing policies of local school systems of assigning students to schools. It was used in this fashion by civil rights groups in school board deliberations, and by plaintiffs in court cases.

How could the U.S. Office of Education have used these results? If the authority structure in education were a strict authority system, and they could authoritatively dictate a new policy to the local level, they would have needed no research. Since the Office of Education did not have that authority, the research would have been useful to them in exactly the way it was useful to those who did use it: as a way of legitimating a policy different from that used at the local level - in this case, as a legitimation for a policy of affirmative school integration of blacks and whites. That they failed to use it in this way indicates merely that an administrative agency of this sort is not accustomed to requiring such "objective fact" justification, since it ordinarily operates simply through authoritative command.

A second and somewhat extended example, involving the same agency, the Department of Health, Education and Welfare, illustrates perhaps even more strikingly the conflict between administrative authority and research results. The example is the income maintenance experiments carried out in the United States, initially in New Jersey and shortly later in Seattle, Washington and Denver, Colorado. These experiments were an ambitious test of a proposed policy to replace existing welfare programs with a guaranteed annual income, a payment that would become progressively less as a person's earnings increased from zero to some point at which the payment would vanish altogether. The initial proposal for such a policy was made by Daniel P. Moynihan, while he was a Cabinet member in the Nixon administration. The policy had not been initiated, but the experiments had been. They were designed by economists, with the principal question being the potential effect of such a policy upon labor supply, and thus indirectly upon the welfare (or income maintenance) cost burden. In this case, it turns out that the experiment was initiated in time to be of some use in policy formulation, for an income-maintenance proposal has only recently been raised again, this time a proposal of the Carter administration, and formulated in H.E.W. And by now, the research results are in.

But now the plot thickens. What are the research results? Have they been used by the Administration in formulating the policy? Have they been used

by anyone else? First, the research results on labor supply show that the effect of a maintenance program at the level proposed would be to reduce the labor supply among those eligible for the program by about 6%. Whether one thinks of this as large or small depends on the point of view. But the cost of the program, above current welfare costs, can be fairly well estimated, and it comes to an amount in the billions of dollars. Has this result been used in formulation of the policy by those in the Administration? There is no evidence that it has. But it has been used in a different way: the Administration has brought researchers to testify in Congress about the research results, primarily - it appears - for one purpose: to allay fears among Congressmen that a guaranteed annual income would have a sharp and severe effect on labor supply, with all those eligible for it immediately stopping work in order to receive the "free money". Thus the results were used, not as an instrument to affect the policy, but as an instrument to sell the policy, to convince those necessary to its enactment of its harmless and altogether beneficial effects.

This is an interesting point, but not so interesting as the next: the Administration never issued a report that had been prepared by the analysts of the Seattle and Denver experiments, and never called the investigators who prepared it to testify before Congress. Yet the report had been submitted to H.E.W. in 1974, over four years before the hearings began, and the investigators who prepared it, three sociologists, were among those who had made the most careful estimates of the effect on labor supply, subsequently confirmed in New Jersey as well by economists. (3)

What was the report? It was a report on the effect of income maintenance experiments on divorce and decreasing remarriage. The analysis carried out by these investigators showed that there was a strong effect of the income maintenance program in increasing divorce. It showed that where the proportion of persons ever divorced among those at the low income levels that would make them eligible for income supplements is now about 33%, the rate would become about 50% if an income maintenance program were in effect. Apparently what happens is this: the existence of a guaranteed annual income makes some women who would not have left their husbands now willing and able to do so.

Is this a good or a bad thing? Again it depends on one's point of view. But the point of interest here is that the Administration authorities who authorized the research and paid for it did not use these results, and even suppressed them. The apparent reason was the potential danger that this result would have had to the policy. For in fact President Carter had announced the plan for such a policy by stating it would "strengthen the American family".

The research came into the open only because of intensive Congressional questioning, initiated by some Congressmen who had heard of the result. (It is an interesting footnote to history that the Senator who was most

instrumental in uncovering these results in Senate hearings was Daniel P. Moynihan, who had been the original architect of the policy in an earlier administration. And this was the same Daniel P. Moynihan who in 1966 had informed Ribicoff of the results of the Equality of Educational Opportunity study, thus stimulating Ribicoff to question John Gardner and Harold Howe about that report, and bringing it into full view for the first time. Moynihan,in discovering these results concerning divorce, withdrew support from the policy and opposed it.)

The point of the story is as before - that an administrative authority interested in enacting certain policies seldom has use for research results, except as an instrument to aid in overcoming opposition to the policy. There is, of course, another point here, for this one involved actual suppression of research results by the Administration. The point is the necessity for strict requirements for open publication of research results, and open availability of research data for reanalysis, if research results are not merely to serve the interests of those in power.

I should mention in addition that it is cases such as this one that are of aid in bringing requirements of this sort into existence. The Government Accounting Office, Congress' watchdog on the Administration, is now investigating this case, and the suppression involved in it.

In general, it appears that the use of social research to legitimate a policy is less frequent and less valuable to an administrative body with authority than it is to external groups, without authority, who hope to challenge the existing policy.

If this is correct, then it means that the conventional wisdom with which I began this presentation is completely wrong: that social policy research is less useful to the authorities than it is to their opponents. It may be, to put it another way, that research provides a window into social policy for those who are otherwise shut out from knowledge which would allow criticism of that policy. Two principles can be drawn (tentatively, of course) from the discussion thus far:

1. Social policy research will be more widely used and have more impact when there is a conflict between those in authority and those without authority than when no conflict exists, and

2. Research results will be more valuable to,and more widely used by, those without decision-making authority than by those with such authority. The very conduct of research, then, will, on the whole, favor outsiders rather than insiders.

A broad class of cases which fits this generalization is again provided by research on education. For most aspects of education, the Federal Government does not have direct authority; that authority is held at

the local level. Then we might ask what weapons the Federal Government has, and what weapons it has used, to counter the authority of the local school districts. One answer is very simple: it has used the threat of withholding Federal Aid funds, particularly in desegregation policy. Although these funds constitute only about 9% of total educational expenditures, the weapon has been an effective one. But a second strategy of the Federal Government, in the absence of authority, is to commission research, the results of which can be used to challenge the local authority. This motive on the part of some Federal officials who control research funds within the National Institute of Education is very clear: it is their one potential source of power.

However, this use of research by Federal "authorities without authority" suggests a broader use of social research in education on the part of the authorities themselves. It is seldom the case that any person in any position has full authority to determine educational policy. There is often conflict within an authority system (such as the Federal, State and local educational authorities). When such conflict exists, we would expect the same principles to apply within the authority structure that applied between that structure and the outsiders. That is, the more conflict, the more use of research. And the research will be, on the whole, more useful to those with less power (whatever their administrative level) than to those with more power. A principal conflict in education is, as I've stated earlier, between Federal, State, and local levels; and research is used by each of those levels to strengthen its hand against the others. Another conflict in various kinds of social policy is between the President and the Congress. This is strikingly exemplified by the income maintenance experiment and described earlier. If we view the Federal Government as an authority structure, but members of Congress as having less power than the Administration, then these research results clearly gave power to those in Congress who did not favor Administration policy.

It is important for me to be clear: I do not mean that research can be "bought", to provide the kinds of results one wants; indeed, to a person in authority, the very uncertainty of the results is one of the most disconcerting things about research. What I mean is that those without power to bring about change, but with sufficient power to sponsor research, can do the latter with the hope that the results will provide legitimation for their position in the power struggle within the authority structure. And they have some justification for such a hope, because research results are, as I've suggested earlier, less likely to be useful to the status quo than to those favoring change.

The question arises then if the administrative authority, whether it is the U.S. Office of Education, or the local school administration, or H.E.W. or the Department of Labor, will be so selfless as to sponsor research which could cause it trouble by strengthening the hand of the

outside group. The answer is that in a particular case it might do so, but it can hardly be counted upon to do so. For example, the U.S. National Institute of Education sponsored research on school desegregation; but the official in control of that research did not support research that examined questions that could undermine the particular desegregation policy currently favored by the Federal Government. As a consequence, no one was informed that these plans were inducing enormous losses of white children from central city schools, and thus defeating their very purpose by creating segregation between central cities and suburbs. The result is irreparable damage to the possibility of racial integration in some of our largest cities, such as Detroit, Boston, Los Angeles, Denver, and New Orleans.

It is here that I believe the greatest value of the independent foundation as a sponsor of research lies. It is difficult enough to design research to examine issue from the perspective of all interested parties when the research is sponsored by an independent entity like a foundation; it is nearly impossible when it is sponsored by the administrative authority itself, which has a vested interest in a particular policy.

What I have tried to do in this presentation is to raise some pointed questions about the relation of research to policy. When one looks closely at these two beasts, one sees that they have very different forms and colors indeed, and that the articulation between them is not a simple one. I have only begun the task of examining this articulation; I have tried to do so provocatively, but in doing so my aim is that of making social research valuable both for those who make policies and for those who experience the consequences of those policies.

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THE PROFESSIONALIZATION AND DEPROFESSIONALIZATION OF SCIENCE IN DEMOCRACY

Yaron Ezrahi, The Hebrew University of Jerusalem

1. Introduction

In the most general terms, the thesis I am proposing in this paper, can be formulated as four interrelated assertions:

a) Despite their latent incompatibility, the movements to professionalize and deprofessionalize science have evolved since the early rise of modern science as complementary strategies reconciling the advancement and the legitimation of science in the context of democratic values.

b) Beginning already at the end of the 19th century but culminating towards the second half of the 20th century, these social strategies for the advancement and the legitimation of science have increasingly come into conflict.

c) This development can be traced more directly to particular changes in the fabric of democratic politics than to changes in the scientific profession.

d) Finally, these developments are having profound effects on the traditional relations between science as a profession and its public.

2. Professionalization and Democratization as Complementary Strategies in the Institutionalization of Science

Since the early rise of modern science the community of scholars confronted the problem of balancing the internal code of the scientific activity and the external socio-cultural requirements for the justification and legitimation of science as an organized social activity. Where the external socio-cultural ambience of science is suffused with democratic values, this task of reconciliation raises special difficulties. The professionalization of science as an activity based on specialized skills, training, orientations and organizations has not been fully harmonious with democratic principles. By emphasizing

the requirement that the claims of science be based on the mastery of certain skills and methods, the professionalization of science has been founded on a concept of authority which limits indiscriminate access and participation. Professionalization, by institutionalizing a principle of exclusion which divides the population between participants and observers, doctors and patients, teachers and students, experts and laymen, has in many respect been a counter-democratic process. Historically, however, the inevitable tensions between professionalization and democratic values were mitigated in two principal ways: a) by trying to ground the selective recruitment of professionals in universalistic standards and b) by a commitment to the universal diffusion of knowledge.

A. First the scholarly community has attempted to base and justify procedures of professional exclusion by rejecting particularistic criteria of membership, such as heredity, divine grace, wealth or personal charisma. The membership in the professional community came to depend, at least formally, more on merit, training and skills - virtues which are open to impersonal tests and are largely acquired. Already the apologists of the Royal Society were mindful of the dangers which may come from blurring the social lines dividing men of knowledge and laymen, and the lines dividing classes, communities of faith or ethnic and family groups. Thomas Sprat observed that in the Royal Society

> ... the soldier, the tradesman, the merchant, the scholar, the gentleman, the courtier, the Divine, the Presbyterian, the Papist, the Independent and those of orthodox judgement have laid aside their names of distinction and calmly conspired in a mutual agreement of labors and desires.

This, he observed, is a blessing

> which seems even to have exceeded the evangelical promise that the Lion and the Lamb shall lie down together.[1)]

In no historical context, of course, could all overlaps between social privilege and professional qualifications be avoided, nor the tensions between criteria of selective professional inclusion and the democratic ideal of unrestricted participation be entirely eradicated. Nevertheless the ideal of opening the gates of the academy before men of merit and commitment, regardless of their social or religious affiliations, could at least promise a way for reconciling professional and democratic political norms of authority.

B. The other way by which the tensions between professional exclusion and the democratic commitment to universal inclusion were mitigated was through the commitment and the policies aimed at the public diffusion of knowledge. The ideal of the diffusion of knowledge presupposed that, although those who actually participate in the advancement of knowledge may be limited in number, the capacity to share the light of truth,

arrive at an understanding and appreciate the uncovered riddles of the universe was - pending the right techniques of diffusion and instruction - universal. As the popular comparison between knowledge and light suggested, although the source of the light of scientific knowledge could be specific, the beneficiaries of the illumination were unlimited. The religious meaning ascribed to the diffusion of knowledge by early Protestant, and primarily Puritan, leaders of the scientific revolution suggests the links between universal light and universal redemption. In the mid-17th century, people who associated, for instance, with Samuel Hartlib, and who played a leading role in the movements which gave birth to the Royal Society, found a special reinforcement for their activities in the prophecy of Daniel (12:4):

> But thou, oh Daniel (saith the Angel to him) shut up the words and seal the book even to the time of the end; many shall run to and fro, and knowledge shall be increased.

The belief that the increase of learning will correspond with the end of man's fall and the restoration of his dominion over nature implied that the few men of knowledge are not constituted as, nor do they aspire to be a monopoly.[2] On the contrary, in the anti-monopolistic mood of the Puritan revolution, these people saw themselves as harbingers of a great renewal in which all would share the light of truth and even the poor and the destitute would be among the participants and the beneficiaries of the increase in knowledge.[3] This idea found one of its most powerful expressions in the works of the influential Bohemian priest, Amos Comenius, whose pansophic philosophy elevated the spread of knowledge as a remedy for the barbarism that threatened civilization.[4]

With the declining saliency of the religious justification of the diffusion of knowledge as a step toward a universal redemption, the ideal of universal diffusion of knowledge was largely sustained by the secular values of social, cultural and moral progress. To the critics who argued that democratization would give license to the tyranny of the mob and the instability that comes with disrespect for authority, the democratic intellectuals of the Enlightenment could counterpose the ideal of the diffusion of knowledge as a way of fusing freedom and order. Thus although the use of knowledge to enhance social discipline and conformity was loaded with conservative and often anti-democratic overtones, the idea of knowledge resolving conflicts and coordinating social behavior served also the liberal-democratic defence of freedom guided by reason as a freedom uncorruptible by the forces of anarchy. Condorcet held that

> experience...proves that in all countries where the physical sciences have been cultivated, barbarism in the moral science has been more or less dissipated and at least error and prejudice have disappeared...The more men are enlightened, the less those with authority can abuse it and the less

> necessary it will be to give [to men of authority] social powers, energy and extent. Thus truth is the enemy of power, as of those who exercise it. The more it spreads, the less they will be able to mislead men, the more force it acquires the less societies need to be governed.[5)]

The practical side of this position led Condorcet to link the institutions which serve the advancement of knowledge with the educational system through which the relatively few who profess science could pass the fruits of their labor to the many.[6)]

During the upheaval of 1793, and prior to his execution as a prominent member of the aristocratic class, even Lavoisier defended science against the "prejudice that has grown up against learned corporations" by insisting that knowledge should be accumulated and distributed as a communal property.[7)]

Following the closing of the French Academy as a relic of the ancien regime, the political pressures of the Revolution led to a growing stress on the role of the scientist as an educator and a diffuser of knowledge relative to the former emphasis on his role as researcher or adviser to government.[8)] Those years of turmoil in France demonstrated the tension between the institutionalization of exclusive professional forums and the pressures to democratize access to science and render knowledge an instrument for the evolution of enlightened public opinion. But during the 19th century a more balanced integration of the functions of advancing and diffusing knowledge was achieved in the evolution of institutions of higher learning in which teaching and high level research were combined.[9)] This institutional development helped mitigate the tensions between the professionalization of research and egalitarian values in the period after the French Revolution.

In the context of the spreading democratic values the scientists' commitment to apply universalistic standards of merit and skill in the recruitment and promotion of professionals could enable their professional authority to emerge as independent of discredited grounds of traditional authority such as kinship, class, religious identity and other particularistic social affinities. The commitment to the ideal of a universal diffusion of knowledge further facilitated the integration of the authority of the scientific profession into democratic values by furnishing a form of accountability and service of science to the larger society.

Although universalistic criteria of recruitment integrated professionalism into the novel conceptions of post-traditional authority they were not sufficient to mitigate professionalism as a form of elite exclusion. It is precisely in this connection that at the level of education and communication, the diffusion of knowledge could constitute a form of inclusion balancing off the requirements of exclusion in the context of

scientific inquiry. As selective recruitment was thus a step towards professionalization as a way of separating experts and laymen the policy of diffusion could function as a counter measure of partial de-professionalization narrowing the gap between scientists and the public. As complementary institutional strategies, professional recruitment could enhance the autonomy of the scientific profession vis-à-vis social structure and affiliations while the universal diffusion of knowledge could serve as a condition for the contribution of science to a transpolitical public opinion. On the other hand, both the universalization of standards of selective recruitment and the commitment to the diffusion of knowledge have opened the way for challenging and limiting professional authority. The insistence that faith, class, family and so on, are irrelevant to professional recruitment made it possible for the disaffected and the socially underprivileged to effectively attack the legitimacy of professional scientific authority by pointing out any discrepancy between the claims of universalism and the perceived imbalances in the representation of the various social groups in the social composition of the scientific community. As a means to effectively propagandize for wider and more equal participation in the scientific enterprise, the universalization of selective recruitment opens the way for conceding criteria of merit where their application results in glaring disproportionate representation of competing groups in the professional community. Especially where the equality of merit across all social groups is a sacred dogma, the commitment to universalistic standards could exert the kinds of pressures which would force the scientific profession to trade decreased institutionalization of functional technical standards for enhanced legitimation.

Similarly, the commitment to the universal diffusion of knowledge presupposed that laymen, if not as active participants at least as audience, could assess and hence share a degree of discretion in appreciating and also criticizing the works of science. Moreover, at any time critics could point out that the universal sharing of knowledge is not a reality and dramatize the unequal social distribution of knowledge.

Despite the actual and potential contributions of the universalization of selective recruitment and the diffusion of knowledge to deprofessionalization through democratization, these institutional strategies could remain compatible and even reinforce the professionalization of science in democracy. Precisely by integrating the authority of the scientific profession into the principles of a new political order which rejects exclusive privileges entrenched in sacred hierarchies or particularistic group affinities, both the universalization of selective recruitment and the universal diffusion of knowledge, if not as accurate descriptions of actual practice, at least as feasible and progressively realizable ideals, could serve the legitimation of the process of professionalization in democratic culture.

In the 20th century, the very conditions upholding this balance between the socio-institutional requirements for the advancement and the social legitimation of science in the democratic state have undergone a profound transformation.

3. The Evolving Conflict Between the Advancement and the Social Legitimation of Science

From a socio-historical point of view, the compatibility of universalistic criteria of selective recruitment and the commitment to universal diffusion of knowledge with the professionalization of science was based upon the premise that there are public values which transcend the political orientations and interests of sectarian groups. Universalization of selective recruitment and diffusion could be interpreted, therefore, as acts consistent with such values and as measures which link the scientific profession with the trans-political, non-partisan sphere of commonly shared values. Criteria of merit and competence were linked not only to the rejection of kinship, class or faith as irrelevant considerations in professional recruitment. The cultivation of competence was justified as serving the public interest and protecting the community from the threats of quackery and charlatanism. Similarly the diffusion of knowledge was not only a check on exclusive authority. It was justified also as a means to rationalize and technically develop the working of society and its material basis. Hence, although under certain conditions the commitment to recruitment procedures which prefer merit to status rendered the scientific profession vulnerable to criticisms of the legitimacy of its social composition; and although the commitment to universal enlightenment through the diffusion of knowledge acknowledged a role for laymen in assessing and evaluating science and judging the adequacy of its social organization, these developments could remain consistent with the premise separating the professional and political realms. Although in enhancing the role of non-professionals in judging and evaluating the scientific community and its work, these strategies have led at least to a partial weakening of professional controls, this deprofessionalization seemed to remain within the framework of the public values underlying the very mandate of professionalism. Especially the concept of "publicity" in the sense of making knowledge available to "all the public" presupposed the concept of a transpolitical universal public as the ultimate beneficiary, the uncontroversial client of the scientific profession.

I would like to suggest that it is precisely the erosion of this notion of the trans-political universal public and the growing elusiveness of nonpartisan public values which, in the second half of the 20th century, upset the balance between the institutional conditions congenial for the advancement and the social legitimation of science. With the erosion of the rationalistic and normative presuppositions which upheld the faith in the concept and the institution of the public as the ultimate authority extending beyond and above all the sectarian political associ-

ations, the public accountability of the scientific profession could no longer remain outside of politics. There is a basic difference between deprofessionalization as a strategy for legitimating the scientific profession before and after the deterioration of public cultural values. Whereas before reaching that stage, both the professionalization and the deprofessionalization of science were enclosed within the common matrix of public values, in the 20th century the strategy of deprofessionalization became identical with the process of politicization. Although in earlier times professionalization could be viewed as a process of entrusting a skillful group with the power and authority to serve obvious public objectives and de-professionalization could be viewed as increasing public authority and power in evaluating, judging and influencing the ways in which professionals operate, both professionals and the public appeared to be guided largely by shared values. But when "the public" and "public values" broke up into a multitude of parts which no longer formed a common whole this system was bound to degenerate. In the absence of universal, and therefore nonpartisan references, any enhancement of the role of nonprofessionals in the life of science has inevitably come to mean a progressive transformation of science into a subject of partisan political contention.

This difference is far-reaching. Insofar as the accountability of the scientific profession was supposed to be directed to a nonpartisan noncontroversial public, any attempt to radically enhance the influence of any particular group over science could be resisted as an illegitimate intrusion of partisan political interests into the domain of public values. But once the restraining power of such values in checking the claims and pressures of various social groups in the democratic state decreased so dramatically, the politicization of accountability became inevitable and political conflicts over directing or controlling the uses, claims and even the process of research became the only operationally available method for ensuring professional accountability. In an age where the reflection of current political alliances is the only method of legitimation, the practical mandate formerly invested in professionals, whereby they could take for granted - together with the rest of the community - the common values around which they can organize their work, could not be sustained.

In such a context the further integration of the political process in the continual assessment and delineation of the normative boundaries of professional discretion becomes a necessary basis for its legitimation. The present state of affairs suggests, therefore, an unprecedented conflict between professional autonomy as an institutional condition for the advancement of knowledge and the accountability of the scientific profession through the direct or indirect influence of political institutions.

4. The Erosion of Public Culture

The dramatic change in the relations between the scientific profession and its social ambience is fundamentally a result of a transformation in the political culture of the democratic state and not a change in the scientific profession. It is not difficult, of course, to recognize changes within the scientific profession which have contributed to this process. Its growing size, increasing dependency on public funding, the proliferation of specialized communities, and the discrediting of positivist presuppositions of the philosophy and ideology of the scientific profession are only a few of the developments which may relate to the change I have noted. I have intentionally refrained from discussing these contributing factors in order to emphasize the primacy of the decline of public cultural values in altering the social position of the scientific profession and in the politicization of the process of its social legitimation.

The decline of public cultural values to which I am alluding has, of course, a wide range of manifestations. On the intellectual plane, one of the early expressions of the change was the attempt to fuse psychology and political science in accounting for the structure and dynamics of political behavior after World War I. "The dominant movements in psychology since the turn of the century, rejected the idea of rational man, one of the central postulates of democracy."[10] Harold Lasswell, for one, integrated psychiatry in the analysis of political behavior.[11] The trend towards perceiving politics from the perspective of individual psychological worlds was associated with the kind of orientations which led eventually to the replacement of the idea of public opinion constituted from sharing commensurable minds with the idea of a multiplicity of heterogeneous individual universes of perception. The growing emphasis on the irrational in human behavior was connected with the declining optimism regarding the very possibility of rational public discourse. In 1925, Walter Lippmann in The Phantom Public, voiced this pessimism in questioning the feasibility and effectiveness of the public to which liberal democratic political theory had entrusted such a central role. In the period before and even immediately after World War II, there were, of course, powerful protagonists of the opposing view who, like John Dewey, defended the concept of "collective intelligence" and the promise of realizing public values with the powerful contribution of science.[12] But from the perspective of the last decades of the 20th century, the progressive erosion of faith in trans-personal or transsectarian public cultural values seems to be the dominant trend and the optimism and reconstructionism of the believers in the redemption of public culture appears as but the episodic upsurges of a losing creed. The disenchantment with the contribution of the social sciences to the rationalization of public affairs,[13] the influence of new theoretical insights into the irreconcilable conflicts between equally rational individual and collective choices,[14] the growing preoccupation with self-fulfillment, the

proliferation of psychiatric and psychological vocabularies in the reconstruction of experience, and, finally, the eclipse of the political culture of public enterprise by the personalist "culture of narcissism"[15] are only some of the factors associated with the decay of public values in our society.

In a society in which personal variance in fundamental values has replaced universal values as referent for professional service, the accountability of the professions to the community disintegrates into a multitude of discrete and unique pacts between individual professionals and their particular clients. The pressures which led to the requirement of informed consent in medical practice is perhaps one of the clearest manifestations of this trend.[16] Technology assessment, as the attempt to tap the value choices of diverse groups in guiding the application of science to social problems, is yet another such indication at the wider level of the general society. Whether at the level of the individual or at the level of the particular groups - where conflicts are settled by compromise - the replacement of universalistic and therefore supposedly given norms leads to the celebration of legitimation as the exercise of the rights to choose and participate at the expense of the quality, the effectiveness and functional adequacy of the judgements.

In as much as the developments I have been discussing can be traced to a fundamental change in the fabric of democratic politics, they must be recognized not only in the relations between the scientific profession and its social context, but also with respect to the social position of other professions. I do not intend to dwell on this point beyond admitting that this is a consequence which is indeed implicit in the above analysis. The nature of the argument, nevertheless, warrants special consideration for the effect of the decline in public cultural values on the status of the scientific profession and more specifically on the status of basic research. The reason is obvious. Unlike the other traditional professions, like medicine and law, or the more modern social service professions, basic research has not evolved as an activity aiming at serving a particular identifiable clientele distinguishable from the wider community. Although in some respects all the professions are committed to public values, the historical association with specific clientele groups have already forced these professions to adjust and balance partisan-sectarian and public ends. By contradistincion, the profession of research guided by the objectives of advancing and diffusing knowledge appears the most publicly oriented of the professions. As such it has been more vulnerable than the other professions to the politicizing effects which have come with the decline of public cultural values. The crisis in the social position of basic research as a professional activity in our culture is therefore a more direct manifestation of the shift in the normative structure and the concomitant practices of politics in the modern democratic state.

5. The Consequences for the Relations Between the Expert and His Public

One of the most significant consequences of these developments in the socio-political ecology of science is the shift in the basis for the social assessment of the acceptability and the authority of science in the context of public affairs. This shift can be described as a change between stressing the supposed objectivity of the scientists and stressing the supposed fairness of the political implications of the knowledge they advance and deploy. It is manifest also in a change in the indicators used by laymen in evaluating and judging the motives and conduct of scientists. If formerly laymen would question or accept the objectivity of scientists with reference to their manifest social, economic or political affiliations, there is a growing tendency to link the imputed motives and neutrality of scientists to references to the perceived or anticipated socio-political implications and effects of their recommendations. The relevant questions about an Arthur Jensen, a William Stockley, a George Wald, or a Barry Commoner have not been their class, economic, or even political affiliations.

Their political identity, their acceptability as scientific influencials in public affairs, is not determined so much with reference to personal or social traits - although such factors are not entirely suppressed - but with reference to perceived consequences of their recommendations for the political contestants struggling over public policies.

This shift to imputing motivations and values to scientists on the basis of results rather than traits is directly related to a declining faith in the possibility of universally benefitial or even neutral results. Given the perceived heterogeneity both within and between individual and group interests and preferences, results are usually judged as asymmetric distributions of political assets and liabilities. Hence results are used as indicators of political motivations and preferences. This development has had a profound impact on the relations between the scientific profession and its political environment. Given the decline of public cultural values which are shared by the entire community, any application of the authority or technical resources of science in public affairs is likely to have differential anticipated results for the contending political actors.[17] Any such deployment of science is bound to benefit or hurt some groups or interests more than others. Hence there is almost no conceivable state of affairs where the application of science in such a social context can be regarded as apolitical or where the professionals involved can safely assume the protection of manifest neutrality. This, of course, applies also to basic research inasmuch as it is an activity which competes with other social activities for a share in public finance. But beyond the competition for funds different areas and directions of basic research represent different and largely politically competing interests and preferences.[18] The social authority of science has come to depend more on

its compatibility with the everchanging compromises which determined the politically authoritative distribution of material and cultural assets than with the requirement of depoliticization.*)

6. Conclusion: The Strains in the Historic Alliance Between Reason and Freedom

If in the early years of the scientific revolution wide circles could link the advancement and the diffusion of learning to the religious vision of renewal and the restoration of man's dominion over nature, in the secularized visions of the Enlightenment, God was removed and man's conquest of nature became identified with the humanist vision of historical progress. But in both these visions it was a universalistic concept of mankind, of entire communities, which underlay the role of science in enhancing human understanding and control. Since the mid-19th century and influenced by vulgarized Darwinism and Marxism, images of social conflict and struggle have undermined the faith in the unity of mankind vis-à-vis nature. In a cultural climate in which theories of conflict have come to represent immanent rather than passing states, and in which the image of politics is that of endless struggles that can never be fully resolved but at best settled by compromises and the balance of opposing powers, science could not sustain its earlier posture. The former association of science with the values of human and social progress has been gradually replaced by the association of science with the advancement of partial, and therefore, controversial values. Hence science could no longer appear to be an appeal to reason and knowledge which transcend current socio-political conflicts. Instead, it has emerged more clearly as just one more weapon in never-ending domestic and international political conflicts. These developments constitute a crisis in the traditional historical alliance between scientific reason and democratic political ideas of freedom. Far from substituting impersonal-neutral knowledge for personal-arbitrary authority as the parameters of public policies and actions; far from replacing the certainty of truth for the uncertainty of ignorance in the guiding of human action; and, far from securing public rather than partisan bases for decisions and actions, science has come to be regarded as an inherently destabilizing factor which is integrated into rather than acting as a constraint upon the influence of political interests. The social contract between the scientific profession and the larger society has come to an end not to be substituted - at least as yet - by another such social contract with revised terms. It has been substituted instead by a continual and often open bargaining and negotiations - through which multiple transactions are being made each time reflecting the current equilibrium of values and interests.

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*) Depoliticization has, of course, always depended on the presence of certain political conditions which permit that the treatment of certain subjects not be mediated by the political process.

For some, the politicization of science may be a sad demise to the cherished vision of redemption and depoliticization through universal enlightenment. To others, however, the progressive increase in the ethical and political controls over science may express the elevation of the value of free individual or group choice even above the values of scientific truth, technical advance, or material welfare. What to some may indicate a decline in the cherished influence of reason in our culture may be celebrated by others as just another stage in the triumph of freedom.

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THE DE-INSTITUTIONALISATION OF TECHNICAL EXPERTISE

K. Guild Nichols, Science Policy Division, Directorate for Science, Technology and Industry, OECD

Government has never before come to depend as much as it does today upon technical expertise; expertise to facilitate decision-making and expert opinion to justify, defend and legitimise the merits of specific decisions. This dependency is, of course,linked to government reliance on science and technology itself. For, the application of scientific and technical knowledge to support government policies raises technical issues which, in turn, require specialised advice. This is evident in most areas of governmental activity: from weapons development and strategic planning to the design and enforcement of regulations and the granting of licenses. Moreover, as the roles and responsibilities of government have increased in recent decades, so too has demand for expertise which is often only available from outside government.

Recourse to "outside" technical expertise has been necessitated in large part because of government's inability to recruit sufficient numbers of skilled scientific and technical personnel; most scientists generally prefer doing research, not just evaluating the research of others. Government has therefore been obliged to adopt approaches to ensure the timely availability of technical information to meet its decision-making needs. This has led to the development of highly institutionalised mechanisms and processes by which "outside" technical expertise is obtained, evaluated and applied by government.

This paper focuses upon one facet of government dependency on expertise: that is, with technical expertise and political controversy. For it is this issue - how government has sought to cope with controversy by taking recourse to technical expertise - that has done much to damage government credibility and engender public distrust of technical experts.

Much has been written about how disputes between experts in areas of public controversy have often served to increase conflict and public uncertainty. Mazur points out, for example, how such conflicts are a major source of public confusion and how disputes between experts reduce

their political impact (1). While Nelkin asserts that conflict between scientists also highlights "their fallibility, demystifies their special expertise and calls attention to non-technical and political assumptions that influence technical advice" (2). This paper tries to show how attempts by government administrators to institutionalise processes for the provision of technical expertise have been a central factor in the growth of public concern and hostility toward technical experts. It is argued that increased public recourse to independent, citizen-controlled sources of expertise does not contradict observed phenomena of public antipathy and mistrust of experts, but is, rather, a reaffirmation of public concern about the uses to which expertise is put. The implications of increased citizen scientific and technical expertise and understanding are discussed in terms of their impact on processes of de-institutionalisation of expertise and, thus, of the destabilisation of traditional government decision-making structures and processes.

I

There is a common and wide spread assumption that government decision-making is usually based solely upon "the facts of the case", or as someone else has put it somewhat more rhetorically, upon the "instrumentality of rationality" (3). This, of course, is rarely the case. Decisions are seldom based on facts alone, since subjective values and bias enter into even the most "factually-based" decisions.

Government administrative authorities, in taking recourse to "outside" technical expertise, have nevertheless gone along with popular folk wisdom which says that "the facts speak for themselves". With this idea in mind, they have sought to employ expertise to de-politicise political controversy. Moreover, by controlling the way expertise has been institutionalised, they have been able to control the content, use and power of technical expertise.

What is here meant by "institutionalisation" is something both general and specific. As a general sociological concept it refers to processes by which organised relationships and patterns of behaviour are established, which are themselves generally accepted as serving an essential social function. More specifically, to "institutionalise" means to facilitate relations and render them stable.

Up through much of the 19th century, the institutionalisation of science and of government proceeded independently of one another. Only in this century does one witness the large-scale development of institutionalised government-science relationships which, while allowing for considerable self-autonomy, are nevertheless based on recognition of the fact of increased interdependency. Government's stake in science has come to be perceived as no less crucial than science's stake in government policy. It is within this context of mutual interdependency that the institutionalisation of technical expertise has developed.

Government relationships with scientific and technical experts have traditionally been predicated, at least in part, upon the following four assumptions:

first, that a distinction can and should be maintained between scientific and technical "facts" and social and political "values";

second, that government retains exclusive responsibility for defining the issues for debate and that the role of experts is to determine the facts of those issues;

third, that technical experts should conform to the same general standards of behaviour expected of government functionaries; and

fourth, that access to technical forums of debate should be restricted to those persons bearing the "requisite" professional credentials.

Two ancillary, but by no means unimportant assumptions - shared by government administrators and their expert advisors alike - concern the definition of what constitutes "expertise" and what is considered appropriate analytical methodology. Expertise has generally been construed as meaning special skill or knowledge in a particular field or specialised discipline. Whereas, appropriate methodology is that which is generally associated with traditional scientific methods for obtaining, assessing and evaluating technical evidence.

All of these assumptions taken together have served as the traditional basis for the institutionalisation by government of technical expertise; that is, for the establishment of the client/expert relationships and the behaviour patterns governing these relations.

The mechanisms devised for the provision of technical expertise to government have proliferated over the last several decades. In the United States, for example, nearly 1,300 Federal agency advisory boards currently exist, the majority of which are involved in providing technical advice to government. While in Germany, the Federal Ministry for Research and Technology (BMFT) presently employs over 900 individual technical advisors; more than any other German ministry. The pattern of growth has been similar in most countries as government authorities have taken increased recourse to high-level panels of experts, standing and ad hoc commissions of inquiry, science academies, councils and professional societies, inter-ministerial coordinating bodies and special task forces in order to secure needed advice and expert opinion on matters of technical complexity and public controversy.

That the assumptions on which such technical support and advisory bodies have traditionally functioned should appear biased in favour of the governmental "client" is of course not surprising. Traditions of government

secrecy, of ministerial responsibility and civil service neutrality have influenced heavily government approaches and attitudes toward the generation and use of technical expertise. By controlling certain key parameters - such as access to forums of technical debate, the behaviour of experts, information flow and problem definition - government authorities have sought to institutionalise technical expertise in an efficient, effective and stable manner.

To a large extent, this has been achieved, at least in the past. Institutionalised mechanisms and processes for the provision of expertise have served as an efficient and economical means for securing timely "outside" advice. They have been effective in helping to clarify uncertainty and evaluate technical alternatives. They have often had a stabilising influence on debates over competing technical options.

But, they have also come to be the focus of growing public criticism. By seeking to maintain a strict separation between facts and values, especially on issues of great public controversy, government has facilitated the task of technical experts, but often at the expense of its own credibility. This is because, rightly or wrongly, the public is usually less interested in the facts of a given controversy - especially when the facts conflict - than it is with the choice between different political and social values. Government recourse to technical facts as the principal arbiter of political disputes has, therefore, been criticised as being politically expedient.

Moreover, by exercising exclusive authority to define the issues for debate, government authorities have also been criticised for having defined them in narrow technical terms and having obscured the political nature of the decisions to be made. And, by limiting access to technical forums of debate to those with the requisite professional credentials, the distance between the experts and the general public has been widened, not bridged; thus encouraging public fears as to the uses to which expertise is put. In sum, the central thrust of these criticisms has been that government-institutionalised processes for the provision of technical expertise has allowed policy-makers to seek refuge behind technical experts when confronted with conflictual issues demanding difficult political choices.

II

There are a number of factors behind growing public criticism of government use and alleged mis-use of technical expertise for coping with political controversy. First, there is the change in public perceptions as to the implications of scientific and technological issues themselves. Issues associated with technical uncertainty and controversy are increasingly being perceived as involving choices between not just technical alternatives, but between competing sets of values. This can be seen in

many environmental controversies which involve fundamental disputes over social and political values, over contradictory attitudes on the part of the public toward growth and conservation, and over different levels of social acceptability to risks. Government authorities often find themselves today entrenched in defence of technological means with a public more concerned about contesting policy goals.

Government authorities have also found it increasingly difficult to enforce conformity to institutional standards of behaviour and, thus, to contain disputes between experts to internal governmental forums of debate. Technical experts are more inclined to "go public" with their facts when they feel that their evidence has received unfair treatment from scientific peers or when they disagree with governmental assumptions about the definition of the problem on which they have been asked to provide advice. The net result has nevertheless been the same: disputes between experts have served to increase public uncertainty and, in many cases,have resulted in criticism of government for relying on the advice of technical experts and not on the wishes of the electorate.

A final factor relates of course to the emergence of a more educated, articulate and fragmented public; a public comprised of many competing and politically organised special interests. This has meant that the "general political public", once assumed to be the basis of democratic institutions in the past, is today perhaps more than ever before difficult to ascertain. Who defines the "public interest" and how that interest is defined,who is to share the costs and risks of government decisions and how are the benefits to be distributed - these are questions of growing local and national concern; questions which, because of their inherent political nature, are increasingly viewed as too important to be left solely to government administrators and their appointed expert advisors.

Demand for more direct public participation in government decision-making is therefore closely associated with public apprehensions about government reliance upon technical expertise. It is important that we examine briefly, then, the nature of this demand and governmental response in order to understand more clearly the impetus behind the emerging processes of de-institutionalisation of technical expertise.

The emergence of new participatory demands for involvement in the exercise of decision-making power is directly linked to the general decline of public confidence in government itself. In almost all highly-industrialised countries, this decline in public faith in the competence of government officials has deteriorated steadily over the last two decades.(4) This crisis of credibility - or the loss of legitimacy of traditional political institutions - has worsened at the very time when the role and responsibilities of government have greatly expanded and the problems of governability have become increasingly complex.

Many people today perceive traditional channels of democratic representation as having become oligarchical. There is an apparent widespread feeling that political parties no longer adequately reflect contemporary socio-economic conditions and values. People are concerned about trends toward increased concentration of economic and political power; power which is perceived by some as resting too much in the hands of too few. They are therefore seeking new ways of expressing their discontent, of exercising their collective influence and of imposing their views.

At the same time, science and technology, like government itself, have come to penetrate more and more into everyday aspects of private life. Some of their impacts are unavoidable and irreversible while the dimensions of others are unknown. Moreover, because some scientific and technological issues affect all people, whether they like it or not, the ethical and value dilemmas they raise are often perceived to be of transcendent social importance and, hence, of growing controversy. Thus, one encounters increasing levels of concern about the need for more thorough and careful processes for public assessment and control over certain scientific and technological developments. (5)

The general thrust, then, of new participatory demand has been for a greater degree of public accountability, freer public access to technical information, more timely consultation on policy options, and more comprehensive and thorough approaches to the assessment of the socio-economic and environmental impacts of government decisions.

Government has sought to respond to these concerns and to demand for increased public participatory involvement in a number of ways. (6) Laws for providing for enforceable public right of access to information in the hands of government have been enacted in the Scandinavian countries and in the United States. (7) In Australia, Canada and the Netherlands, similar proposals for freedom of information legislation have been under consideration for nearly half a decade. In the United Kingdom, France and Germany information has become more freely available to the public through enactment of specific administrative and legislative measures governing, for example, the implementation of physical planning procedures and the conduct of local planning inquiries.

The liberalisation of regulations for the disclosure of government information has generally meant greater citizen access to the conduct and substance of government business and, most importantly, to technical information upon which government decisions are allegedly based. Increasing acceptance of the public's "right" to know what is going on in government administration has also led, in a growing number of countries, to more open and explicit attempts to inform citizens on decision-making procedures and participatory opportunities.

Governments have sought by means of a variety of ad hoc mechanisms to better inform the public on such complex and controversial matters as national energy policy and the development of civilian nuclear energy. In Austria, Denmark, Germany and Sweden, public information programmes have been devised for these purposes with often mixed results. While in the Netherlands and the United States a number of broader initiatives have been taken to try to encourage increased public understanding on scientific and technological issues. (8)

New consultative measures have also been adopted and public hearing and inquiry processes expanded. Three recent national experiences with the conduct of public inquiries - the Windscale Inquiry in the United Kingdom, the Canadian Mackenzie Valley Pipeline Inquiry, and the Ranger Uranium Inquiry in Australia - suggest several of the limits and potentials of the inquiry mechanism as a tool for public consultation and involvement in government decision-making.

Government has tried to overcome mounting public criticism as to the representation bias inherent in many of its technical advisory bodies. (10) It has sought to achieve better representation of the views of the electorate and of certain organised interests by including members of the general public as individuals and as interest group representatives on governmental advisory boards, task forces and special governmental commissions.

Moreover, there has also been a general trend in many countries toward requiring all government licensing and regulatory hearings to be held in public. In Canada and the United States, for example, restrictive rules of "standing" - the right to participate as "full parties" in regulatory and administrative appeal proceedings - have been relaxed somewhat with the granting of greater discretionary powers to regulatory and appeal boards. Even in those countries, such as Denmark and Germany, where more restrictive rules of "standing" still exist, citizen groups have often been able to circumvent these procedures by aligning themselves with and providing assistance to persons having been granted rights to intervene.

Finally, the establishment of procedures for the assessment and review of environmental impacts has also been one of the more important developments in relation to public participation. In almost all industrialised countries today the preparation of various forms of environmental impact statements with respect to proposed government or government-funded projects is required by law or administrative decree. (11) These procedures have served two important purposes; that of informing policy-makers of the possible effects of proposed actions and of providing the public with opportunities to express their views concerning those actions.

All these different government initiatives, when added together, seem impressive. And indeed they are, especially in comparison to traditional

government practices of less than two decades ago. But a little deeper analysis of these initiatives reveals, in fact, just how limited and cautious they really are.

Why they are limited has much to do with government ambivalence as to the degree to which participation should be encouraged. Concern is often expressed by policy-makers about the consequences of expanding public participation and its impact on decision-making efficiency and on existing administrative and political structures and processes. Moreover, because some "public interest" and environment-related organisations which demand participatory involvement do not fit the landscape of traditional political pressure groups and lobbies, they cause consternation. Because they do not share common sets of ideologies or always abide by traditional political tactics, they are sometimes worrisome. And, because many attract support from increasingly large numbers of citizens, they are often seen as threatening to the "established" bureaucracy.

How these initiatives are limited has everything to do with how public policy issues are themselves defined and who defines them. For, it is most often not the public, but a government department that has the final say. This is, perhaps, as it should be. But if so, it should also be clear what all the implications are. Because, how issues are defined determines what kinds of information are judged relevant and made publicly available. Even in those countries with so-called "freedom of information" legislation, certain exemptions are still made for government-owned information, the disclosure of which would prejudice national security, defence, international relations or national economic interests.(12) Thus, to define broad political issues in, for example, narrow national defence or economic terms can serve to justify the withholding of information which is required for effective participation. In the same way, government often retains considerable discretionary power over both the granting of "standing" and over the determination of what issues may and may not be open to debate.

Now, all of this may seem far afield from the issue of institutionalisation of technical expertise. But it is not. Because by its response to participatory demand, government has set the stage for its de-institutionalisation, and for the development of processes of political de-stabilisation.

III

One of the hallmarks of many contemporary technological controversies is what has often been referred to as the "politicisation of expertise". By this is meant the process by which technical expertise serves or is employed for strategic political purposes, not just for the elucidation of facts or clarification of technical uncertainty. Citizens' groups today are increasingly inclined toward developing their own technical expertise

and using it as a political weapon: to undermine the credibility of government-appointed experts and project proponents and neutralise the impact of their evidence. Citizen technical expertise has also served a potentially more important, long-term function - as a tool for promoting broader public understanding and appreciation of the political and value choices inherent in many technological decisions.

Government authorities - first, by encouraging more direct involvement in decision-making and, second, by granting greater public access to information previously held to be confidential or of proprietary interest - have both stimulated and facilitated the development and growth of citizen technical expertise.

For example, many "debutante" intervenors, involved for the first time in government advisory and decision-making processes, have quickly come to realise the indispensability of technical expertise. It is seen as essential not just for arguing and defending one's particular case, but for establishing one's own credibility. This has been especially true with respect to government advisory, regulatory and licensing proceedings where environmental, consumer and other citizen group representatives have traditionally been treated as outsiders - not so much because of political or ideological differences - but because they are not considered members of the "community of professionals". Citizen group recourse to technical expertise has served therefore as a means for establishing their legitimacy and, thus, for enhancing their professional standing and credibility. (13)

Professional credibility, in turn, has served as an instrument for gaining further access to government technical information. This is because much government-held information, while publicly accessible in theory and by law, is not in practice. Thus, developing credibility often increases chances of gaining further access to information for the development of still greater technical expertise, and enhanced credibility.

But, of course, access to information is a necessary but insufficient condition for the development of citizen expertise and for effective public participation. Considerable scientific, technical and financial resources are also required. To an increasing degree, these traditional barriers are being overcome, although the financial one remains of continuing concern to many citizen groups. In a few countries, government funding measures have been adopted to facilitate the developments of citizen expertise and reimburse non-governmental experts. (14) In most, however, citizen groups are largely dependent upon grants and proceeds from the sale of publications, as well as on voluntary contributions of time and talent.

Nevertheless, despite some of these difficulties, there has been a considerable expansion in recent years in the number and diversity of sour-

ces of citizen technical expertise being brought to bear on specific issues of public policy concern. And, it is this growth, expansion and diversification of technical expertise which gives specific shape to the emerging processes of de-institutionalisation. These processes have a number of characteristics which contrast sharply with government assumptions underlying traditional approaches to the institutionalisation of expertise.

First, and most obvious, is the fact that by definition processes of de-institutionalisation of technical expertise lack a common, coherent pattern. Assumptions as to what constitutes "expertise", how it should be generated, and the purposes it should serve vary widely between and sometimes even among different citizen groups.

The defining of issues and development of expertise are considered, not as separate and consecutive steps, but as inter-related phases of the same iterative process. The assumption being that a certain level of technical understanding of an issue is required before being able to identify how one's views, interests and values are affected. This means that there is often considerable interplay between how issues are defined and re-defined and how different kinds of knowledge and expertise are brought to bear. Processes of de-institutionalisation seek to avoid the more rigid imposition of "prior problem constraints" characteristic of more traditional institutionalised processes of assessment and evaluation of technical facts.

In this fashion, they also seek to establish more explicit linkages between technical issues and value concerns. However, to say that processes of de-institutionalisation of expertise seek to integrate, not separate, "facts" from "values" is not to suggest that "scientific" methods for the evaluation of technical evidence are subjugated only to emotive or intuitive "analysis". Rather, the attempt is to demonstrate how factual disputes are often founded upon the application of different conceptual models, and therefore, the importance of trying to clarify and make more explicit the premises and assumptions upon which technical evidence is based.

A further characteristic of the de-institutionalisation of technical expertise is its degree of openness to different kinds of knowledge, not just that of scientific and technical specialists. Citizen knowledge and experience relating, for example, to living and working conditions and environments are often used to complement the sometimes more esoteric evidence of technical experts. This can also be interpreted, in part, as an attempt to de-professionalise processes for the generation of expertise.

What this implies of course is a broader definition as to what constitutes "expertise". Whether such a broader definition - one which also

embraces layman knowledge and experience - is acceptable to government authorities, whose task it is to evaluate often conflicting technical arguments and evidence, is in one important respect quite beside the point. For the point is that citizens' group recourse to their own sources of expertise is not predicated solely upon establishing their credibility or of convincing authorities as to the merits of their case. Citizens' groups seek to influence public opinion.

It is not an incidental fact that many on-going environmental, consumer and other such citizens' organisations and action groups maintain as a central component of their activities information dissemination programmes. One pragmatic and near-term objective of these activities is to broaden group membership and thus promote financial solvency. A more central long-term aim, however, is to encourage broader public understanding and awareness of the technical and political choices inherent in government decision-making.

IV

The de-institutionalisation of technical expertise can be seen, then, as a process of change in which traditional assumptions and attitudes about what constitutes "expertise" and who is an "expert" are being challenged. It is one manifestation of growing public concern and mistrust about heavy and sometimes exclusive government reliance on technical experts as a means to de-politicise public controversy.

Processes of de-institutionalisation represent attempts at ensuring: a broader public dissemination of scientific and technical information; more effective approaches to citizen involvement in the assessment of technical facts and uncertainty; more explicit linkages between technical issues and value concerns; and, thus,a more equitable re-distribution of power over how public policy issues are themselves defined.

Citizens' group recourse to their own sources of technical expertise has served a number of near-term strategic political objectives: to achieve professional standing and credibility; to secure access to certain kinds of information and entrance to decision-making forums; and to lend legitimacy to the facts of their own particular case.

Citizen group technical expertise has also been employed as a weapon in pursuit of a kind of techno-bureaucratic guerilla warfare; for neutralising the impact of opponents' evidence and counter-evidence, for discrediting their expertise, and for challenging the credibility of government motives for changes in policy rationale. Such skirmishes have become all too common as citizens' groups have, in many instances, been able to delay and block the implementation of many government decisions. By appealing beyond government to public concerns and anxieties, some citizens' groups and the technical expertise at their command have come to assume considerable deterrent power.

Now it can of course be argued that this is all to the good; that it helps to ensure that government authorities and their technical experts are held more accountable to the general public. But the fact of the matter is that many technical disputes remain unresolved and decisions still have to be made.

It is, therefore, perhaps naive to assume that greater public political discourse, alone,will serve to resolve disputes over technical uncertainty and risks. Nor are "the answers" to be found solely in the development of more pluralistic sources of technical expertise. What is required is a clearer understanding and articulation of the appropriate role and limits of technical expertise in processes of democratic decision-making.

The issue of "limits to expertise" raises an essential question: is scientific and technical expertise the only effective approach to "knowing"? One value of scientific and technical knowledge resides in its predictive capacity. And yet, because "the experts" have been proven so w ıg so often, their predictions carry today a peculiar weight for a pubıic conditioned to believe in the sanctity of Murphy's Law ("If anything can go wrong, it will").

If certain specific issues entail uncertainties and risks which are beyond the present predictive capacities of science, then complementary strategies for the assessment of risks are required. This is not to suggest that permanent chairs be provided for astrologers, mystics and magi on technical advisory boards and expert panels. Rather, more effective institutional forms need to be devised by which citizens and experts together can seek to ascertain the limits of available technical and non-technical knowledge and the appropriate conditions for decision-making on issues involving high risks and uncertainty.

De-institutionalised processes for the generation and use of technical expertise can be seen then as a positive, and perhaps even inevitable, development. It is still too early to measure their full impact on more traditional institutionalised processes for the provision of government expertise. However the challenging question that is posed is a clear one: who should exercise what power to define and decide the issues upon which public choices are to be made?

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4. Daniel Yankelovich points out, for example, that in 1964 seven out of ten Americans believed in the competence of government officials. By 1976, the number of Americans continuing to have this confidence had shrunk to 44 percent. By 1978, it had further declined to 30 percent. See D. Yankelovich, "Farewell to 'President Knows Best'," Foreign Affairs, 57, 3, (Spring 1979) pp. 670-93.

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7. Freedom of Press Act (Sweden), The Law on the Public Character of Official Documents (Finland), Law on Publicity in Administration (Denmark and Norway) and Freedom of Information Act (USA).

8. Nichols, op.cit., pp. 26-53.

9. Ibid., 61-77. See also Guild Nichols,"Public Participation in Canadian Energy Policy," (1979 forthcoming).

10. For example, in the two national examples cited above, nearly 50 percent of the total membership on US Federal agency advisory boards is comprised of industrial representatives with only 7 percent coming from consumer and environmental groups, while at the German BMFT there is a representational bias in favour of researchers (55 percent) and industrialists (25 percent).

11. See OECD, Environmental Impact Assessment, (Paris, OECD, 1979)

12. For example, under the US Act, exemptions are made for documents the disclosure of which would prejudice "national security, defense and

international affairs," (Para. 522(b)1), while in Norway, these exemptions are extended to "national economic interests" and commercial and financial proprietary information(Law on Publicity in Administration, Section (6) (1)).

13. Robert Friedman, in an analysis of advisory committees at the US Food and Drug Administration (FDA),concludes that "the consumer movement needs to employ a strategy designed to gain legitimacy for its representatives on advisory committees... When consumer representatives in the health field attain this legitimacy, their impact on the system will increase immeasurably." See R.Friedman, "Representation in Regulatory Decision Making: Scientific, Industrial and Consumer Inputs to the FDA," Public Administration Review, (May/June 1978) pp. 205-14.

14. A number of US regulatory agencies have devised compensatory programmes of financial assistance to citizen groups. These include the FDA, Civil Aeronautics Board, Federal Energy Administration and Environmental Protection Agency. Under the US National Science Foundation's "Science for Citizens" programme, stipends are awarded to scientists and engineers engaged in providing assistance and technical expertise to citizen organisations. In Canada, government financial assistance to citizen groups to help them develop their own expertise was initiated with the Mackenzie Valley Pipeline Inquiry and has been continued for other national and provincial inquiries and as a part of the Federal environmental assessment and review process.

THE CASE FOR NUCLEAR POWER EXAMINED

Robert Olby, Department of Philosophy, University of Leeds

The germ around which the World Power Conference was conceived was very simple. A vision of the nations of the world after the great war revealed the need for a conference of practical men, scientists, engineers, manufacturers, financiers, and politicians, to consider the utilization of the forces of nature, in the light of a new internationalism, and to attempt to discover a means by which the nations of the world might be preserved from the constant action and reactions of past history, and might all advance together... above all it seemed desirable that the engineers of the world should take a counsel together... The more highly developed a community or nation, the greater its knowledge and use of the resources of nature. This knowledge had resulted in the creation of a machine driven by destructive energies. One good custom - the harnessing of the powers of nature to human needs - appeared to have corrupted the world. Even the winds or the seas were used for purposes of destruction.

...His (man's) clenched fists must again become open palms to cover the continents. His latest achievements in science and engineering must proffer a torch to light up the hitherto obscure chambers of his ascending pathway through nature...

What programme could be outlined for the realisation of such an ideal! An international morality seemed necessary. In this spirit the World Power Conference has been called into being. (1)

It is not enough just to take this weapon out of the hands of the soldiers. It must be put into the hands of those who know how to strip its military casing and adapt it to the arts of peace. The USA knows that if the fearful trend of atomic military build-up can be reversed, this greatest of destructive forces can be developed into a great boon for the benefits of all mankind... (2)

These two quotations, though separated by some three decades, bear marked similarities: - swords into ploughshares, international co-operation, the sharing of technical and scientific information. Both the establishment of the World Power Conferences and the Atoms for Peace Conferences took place within a decade of world wars in which science had been a crucial factor, and was directly associated with human suffering and death - the effects of poisonous gases in the First World War and of atomic bombs in the Second. Both were conferences of experts from government, industry and universities. The occasions offered a platform for public acknowledgement of the important role of these experts and for the consolidation of national and international support for their work.

Here the parallels for the first World Power and Atoms for Peace Conferences end. The former was dominated by experts concerned with fossil fuels and with alternative energy sources other than nuclear power. Unlike these conferences,the Atoms for Peace conference was born in the shadow of the nuclear arms race, and concerned weapons both quantitatively and qualitatively different from any of their antecedents. It has been claimed that the carnage in Hiroshima and Nagasaki, was no different in scale from that of the fire-bombing of Hamburg, Dresden and Tokyo. (3) This may be so, but by 1953, when Eisenhower conceived the atoms for peace programme, no barrage of fire bombs could equal the effects of the further developed nuclear weapons by then available (the USSR exploded its first H-bomb in 1953 and the USA exploded a thermonuclear device in 1952 followed by its first H-bomb two years later.) Moreover, the survivors of Tokyo, Hamburg and Dresden did not live in fear of developing some form of cancer or leukaemia,of becoming sterile, of developing cataract and of having their lives shortened. Survivors at Hiroshima and Nagasaki, initially reassured, subsequently had good grounds for concern. Year after year they were "monitored" by the experts of the Atomic Bomb Casualty Commission. By 1951 the Commission's reports revealed an unprecedented frequency of leukaemias in the exposed populations. Not until 1957 was it clear that the years 1951-53 marked a peak of incidence. (4) Hence the urge to turn swords into ploughshares was more intense in 1945 than at any time after the First World War. No matter what historians may say, scientists like Oppenheimer, Szilard, Bohr, and Rabinowitz felt in their bones and expressed in word and on paper the quantitative difference between nuclear and conventional weapons. (5) It did not require Hiroshima and Nagasaki to do this - Alamogordo was enough, (6) although the Japanese bombing did, of course, bring home this stark reality, and compound and intensify the sense of guilt.

1. The Historical Context of Nuclear Power.

As early as 1939 uranium fission was seen as a potential source of electricity as well as an explosive weapon. (7) The group under Joliot in Paris were exploring the controlled release of nuclear energy. At the Nuclear Laboratory at Ivry they built a pile. They had a contract

with Union Minière to attempt a "fullscale trial". (8) According to one of the group, Halben, they " were absolutely bent on creating a nuclear chain reaction which could be used for industrial power." (9) "To be the first to achieve the (controlled) chain reaction was like achieving the philosopher's stone," declared Kowarski. (10)After Samuel Goudsmit had investigated the German atomic programme in 1945 it became clear that they, too, had only been working on the controlled release of energy from uranium fission. (11) In the United States in 1939 the sole interest in the application of nuclear fission was for submarine propulsion. (12)

It was not, therefore, inevitable that nuclear power should have been associated with the bomb. This association, which I see as historically contingent, was due not only to the fears of certain émigré physicists in the USA concerning German aspirations, but to the British effort, together with the Halben-Kowarski work in Cambridge, summarized in the MAUD Report of 1941. This top secret document gave a surprisingly accurate estimate of the quantity of 235U required for a nuclear explosion (critical mass), it gave information on 235U enrichment by gaseous diffusion and an assessment of the time needed to produce a uranium bomb. (13) The report also included a section on power production from a "uranium boiler", which it considered would prove cheaper than coal or oil. (14) In Russia, too, both the weapons and power producing aspects of uranium were explored in a report which I.V. Kurchatov presented to the Praesidium of the USSR Academy of Sciences in 1940. (15) The MAUD and Kurchatov reports led, however, to the bomb first and power reactors later.

Although after the War Canada, Norway, and subsequently many other countries developed and industrialized nuclear technology without producing nuclear weapons, the nations which sponsored the most active development of nuclear power - USA, USSR, UK, and France, (15a) all produced "the bomb" first. As a result, the promotion of nuclear power has been strongly dependent upon a previous commitment to produce nuclear weapons, and its governmental support initially depended upon the extent to which research and development of nuclear power could aid the weapons programme. For those scientists who went into nuclear power just after the war there was no question that they were contributing first and foremost to military requirements, and they accepted, albeit grudgingly, the secrecy involved.

There was considerable public euphoria for the immanent benefits of nuclear power to citizens of the USA in 1945-46, but the vast facilities of the Manhatten Project (or Manhatten Engineer District to give it its correct wartime label) were ill suited to a concerted attack on power production from controlled fission. It had been a deliberate wartime policy to scatter the facilities of the project with the results that no central laboratory existed. In the period August 1945 to December 1946 General Groves had carried on a holding brief, awaiting the decisions of Congress to hand over the project to the civilian Atomic Energy Com-

ission and then to organize the transfer itself. In the meantime, on the suggestion of his Advisory Committee on Research and Development, Groves had agreed to establish two new laboratories to undertake unclassified fundamental research requiring equipment too expensive for universities and private research institutes. These were the Argonne National Laboratory near Chicago and Brookhaven National Laboratory on Long Island, projects which necessarily further stretched the funding of nuclear research and exacerbated the problem of decentralized laboratory location.

When the Atomic Energy Commission took over in December 1946 they were horrified by what they found. Only the immense success of the diffusion plant for 235U enrichment gave them grounds for hope. At remote Hanford in Washington State the two piles for plutonium and polonium production were in a sorry state,many of their fuel element cans had buckled, corrosion was causing water to escape from the cooling pipes into the graphite moderator, and radiation distortion of the graphite (Wigner effect) threatened a major accident - like the one which occurred at Windscale in 1957. Recovery of plutonium from spent fuel at Hanford was slow; the technique then in use left the unused uranium to be poured away. Complete rebuilding and a new technique to recover plutonium and uranium seemed the only answer. Certainly no scaling up in temperature output of these Hanford reactors for electricity generation looked worth the effort.

At Los Alamos many of the staff had left and morale was low. Most of the laboratory's energies had gone into the first post-war bomb tests- "Operation Crossroads" on Bikini Atoll in 1946, followed in 1948 by "operation Sandstone" on Eniwetok Atoll. Although from 1946 onwards ordnance and weapons production was progressively transferred to the Sandia Base near Albequerque, leaving Los Alamos to concentrate upon weapon development and testing, this laboratory offered virtually no scope for peaceful applications. The facilities at Oak Ridge, by contrast, offered a very promising opportunity for research into the biological effects of radiation, which Alexander Hollaender seized in 1946. Thus Oak Ridge became the site of the Russells' famous "megamouse" experiment on the genetic effect of radiation in a mammal. Oak Ridge also offered a commendable programme of research and isotope production from its experimental reactor which could be continued. At this time the only established peaceful application for nuclear energy was isotopes for biology, medicine and agriculture. (16) Oak Ridge played the key role in expanding the supply of such isotopes. It was also the laboratory where the power-producing reactor proposed by Farrington Daniels in 1944 was being actively studied with a view to construction. This helium-cooled reactor was based on the "Mae West" design considered but rejected by Fermi at Chicago in 1942. (17) The Commissioners, however, were torn between wanting to support the reactor development group in Oak Ridge and their desire to centralize reactor development at Argonne near Chicago. This aim was never achieved. Instead reactor development was apportioned as follows: - Argonne National Laboratory - liquid metal

fast reactor and later with Westinghouse the submarine reactor. Oak Ridge - under threat of little reactor development work when the Daniels reactor was cancelled in 1947. Some work continued on the Materials Testing reactor, and later on the homogenous reactor. Knolles Laboratory, Schenectady, near West Milton in N.Y.S. - intermediate breeder reactor. (This laboratory was set up by General Electric when they took over management of the Hanford site in 1947).

These plans were altered by several events. First Alvin Weinberg in 1946 became keen on the possibility of using ordinary water ("light" water in contrast to "heavy" water, i.e., deuterium) as coolant and moderator. The trick was to use water at high pressure and enrich the uranium fuel. Second, Hyman Rickover, who was impressed with Weinberg's suggestion, had been prodding the Navy and the A.E.C. to undertake the building of a reactor for submarine propulsion. Finally in the autumn of 1948 his efforts bore fruit. That October Westinghouse set up a separate atomic power division, and in December collaboration with the Argonne National Laboratory was agreed. As we all know, this led to the successful operation of a high pressure light water reactor, America's first nuclear submarine, and America's first commercial nuclear power station run by the Duquesne Light Company at Shippingport near Pittsburgh. Rickover played a central role in all these achievements; in the case of Shippingport he had designed a reactor for an aircraft carrier, but when the Eisenhower administration axed this in 1953, Rickover modified it to suit land-based power generation. Hence it came about that the first nuclear power station in the USA was of the pressurized light water type. These successes rescued the A.E.C. from its history of floundering in a mire of indecision caused by devotion to a multi-pronged attack on so many fronts in the design of different reactor systems.

Although the Americans continued their wartime style of contracting out work to industry and involving public companies in the operation of some of the A.E.C.'s laboratories, a very tight policy was maintained over classified information and over all processes relating to fissile material. The McMahon Act prohibited private ownership of any fissile materials, the production of which became the exclusive responsibility of the Commission. All patents relating to the production, refining or other processing of fissile materials were to become the property of the Commission. No licenses for the use of atomic energy devices were to be issued by the Commission until it had provided Congress with a report on such practical applications and had prepared additional legislation. It was not until 1951 that the Commission allowed four industrial groups to see classified information essential to any evaluation of the economics of nuclear power. This special treatment of nuclear power in a country dedicated to the free market economy stemmed not only from the policy of government ownership of the technology pending international agreement on the control of nuclear weapons, but also from the fear that the impact of nuclear power might be so great that no one company should corner the

market and hold the vital patents. Eight years passed before significant amendments to the 1946 act were made with the aim of attracting industry into nuclear power production. In the event, those companies which had already acted as contractors for the A.E.C., played the major role in exploiting nuclear power . Westinghouse which had built the propulsion plant for Nautilus and the land-based prototype, and General Electric which had built the sodium-cooled propulsion unit for the submarine prototype and subsequently the prototype boiling water reactor, and research into a nuclear powered aircraft.

In addition to the trauma of post-war transfer to civilian control and the administrative indecision of 1947, the U.S.A.E.C. had been forced to devote its major effort to weapons production and testing. Only by such a policy could it hope to amend the truly shocking state of affairs which Lilienthal revealed to President Truman in April 1947, when not a single assembled weapon was available. (19) The AEC's policy on power reactors began to crystallize with the formation of its Division of Reactor Development in February 1949, and recognition of the feasibility of nuclear power began to grow in 1952 when Truman presided over the keel-laying ceremony of the Nautilus. In his speech the President recalled that in 1945 all men asked themselves: "What is this awful force (of the atom)? - Can it be used only to destroy men, or can it be harnessed to help them?" He continued:

> For seven years we have been working to find the answer. And now we have found it. This vessel is the forerunner of atomic-powered merchant ships and airplanes, of atomic power plants producing electricity for factories and homes, and farms. The day that the propellors of this submarine first bite into the water and drive her forward will be the most momentous day in the field of atomic science since that first flash of light down in the desert (of Alamogordo) seven years ago. Then we knew we had a bomb. Now we will have a working power plant for peace. (20)

Truman said these words nearly a year before even the prototype submarine reactor went critical at the AEC's desert testing site, and two-and-a-half-years before the Nautilus underwent sea trials! Five more years passed before the Shippingport reactors supplied electricity to the grid. When they did Rickover put the cost at more than ten times that of conventional fossil-fueled plant. (21) Subsequently the project to develop a nuclear powered aircraft was abandoned after spending $1000,000,000 (22) and nuclear powered (surface) ships proved too expensive save for military purposes. (23) Nor were these the only hopes for nuclear power dashed by careful research. Project Plowshare which aimed to use nuclear explosions for engineering work - canal building, natural gas liberation - came to an end in 1970 by which time $ 138,000,000 had been spent on this project. (24)

In the U.K. the priority in the nuclear programme until 1956 was for weapons. For this purpose the following installations were built:

Fundamental research	at Harwell AERE
Uranium enrichment	at Capenhurst
Fuel Fabrication	at Springfield
Reprocessing	at Windscale
Reactor design	at Risley
Weapons Development	at Fort Halstead & Woolwich, from 1950 at Aldermaston

The gaseous diffusion plant at Capenhurst was not expected to yield enriched uranium until 1956. In the meantime strenuous efforts were devoted to producing sufficient plutonium for the Chiefs of Staffs' bomb requirements. The military demand for plutonium not only gave justification for the construction of the U.K.'s first "civil" nuclear power station "Calder Hall", but influenced the U.K.'s first civil power programme of Magnox stations. The design of all these stations was influenced by the need to extract ^{239}Pu with the minimum of the unpredictable ^{240}Pu so that the product would be suitable for Nagasaki-type bombs.

Despite the initial military emphasis, enthusiasm within the laboratories to develop nuclear power was expressed by the holding of Power Conferences at Harwell, beginning in 1948. That year saw the start of work by Parolle, a subsidiary of Reyrolle Parsons, on the generation of electricity from nuclear power. This was followed by specific proposals for a nuclear power programme which reached the government in 1950.

In view of the then allegedly slender world deposits of reasonable grade uranium the U.K. had, like the USA, devoted its energies initially to the Fast Reactor. For the demonstration of the feasibility of nuclear power in the near future, however, opinion moved in favour of thermal reactors fueled with natural uranium. Enriched uranium was excluded because the Americans were forbidden to make it available to us. This had the effect of ruling out light water reactors. Water cooling was considered too dangerous in heavily populated Britain, so gas cooling was chosen instead, and since the Americans were also unable to supply us with helium the most suitable coolant appeared to be carbon dioxide. Thus arose the gas-cooled designs of Calder Hall, followed by the Magnox stations and the much maligned Advanced gas-cooled reactors (A.G.R.). The submarine route to enriched light water moderated reactors, which might have been followed in the U.K., was terminated for lack of enriched uranium and with the knowledge of the more advanced state of US and USSR submarine reactor technology. Before the accident to a light-water reactor at Harrisburg many commentators have criticized the UK policy of keeping to gas-cooling. (25) Now those responsible for nuclear power in the UK are happy to dissociate their reactors from light-water technology. (26)

Before the success of the Shippingport reactors in 1957 each country involved in nuclear power pursued its own path dependent upon the raw materials at its disposal. Thus France, like the U.K., developed gas-cooled natural uranium reactors, Norway with its ready supply of heavy-water from Norsk Hydro, developed heavy-water moderated reactors. The USSR was able, like the USA, to develop light water reactors because of its supply of enriched uranium and its experience in developing nuclear icebreakers and submarines. Their first power reactor at Obninsk (1954) was a pressurized light water design, though including graphite as well as water for moderation.

2. The United Nations Atoms for Peace Conference.

This Conference, held at Geneva in August 1955, served its prime political function of bringing together scientists and technologists of East and West in an atmosphere of co-operation for peaceful ends. It also served as a platform for the promotion of the case for nuclear power to an extent not achieved at the fourth World Power Conference of 1951. There, Ward Davidson of Consolidated Edison, New York, remarked: "Three years ago it seemed reasonable to discuss definitive estimates of costs of nuclear power plants... Today it seems better to avoid any numerical estimates and to discuss only some factors which influence costs in important ways." (27) Other speakers feared that nuclear power was commandeering research funds and scarce trained staff to the detriment of other power technologies. Thus Sir Harold Hartley warned: "It would be a great tragedy for the world if enthusiasm and optimism for nuclear energy were to delay in any way the immediate development of other more conventional and better understood sources of the energy of which the World is in such need today." (28) Another speaker from the U.K. urged a balanced view of the potential of nuclear power. He went on:

> If one listens to some of the prophets of the atomic age one could get the impression that without atomic energy the world would soon be doomed to extinction because of the lack of power. That is, of course, completely wrong; not only are our conventional sources of power going to last for a very considerable time, but in addition there are plenty of untapped power sources around us. Let us only mention solar energy; the equivalent of the total power which is consumed on the earth in a year is intercepted by the earth in six minutes. The real question is, which of the many sources of energy presented to us in profusion is it easiest for us to convert to the forms directly useful to us?
>
> Atomic energy has a number of most disagreeable features quite apart from the political implications of using it on a larger scale. Its great advantage is the negligible weight of the fuel and this will, with absolute certainty, lead to important specialized applications. Whether or not atomic energy will be able to provide a large part of the world's power sources will, as we have heard today, depend

> on lengthy and very difficult developments and the one thing which is certain is that this will not happen in the next few decades. Of course, there are many reasons why we must develop nuclear energy but one should not neglect the other sources, as has been already emphasized by Sir Harold Hartley today. For instance one should also devote comparable efforts to improving conditions of coal-mining, for instance, by replacing mining by underground gasification. A balanced picture of the whole situation must also include the question of waste of energy. We in this country only utilize properly about 4% of the coal which we are burning, which means less than $10X10^6$ tons, while about $190X10^6$ tons per year are burnt to waste.(29)

In a hard-hitting speech the Technical Director of Sweden's Asea Company claimed that nuclear power would not bring about an economic revolution if it were "freely available at practically no cost." In fact he gave reasons for expecting the price of highly purified uranium to be five to ten times the pre-war price of ordinary commercial uranium (an accurate prediction). "It is important", he declared, "not to exaggerate the significance of atomic energy for the production of power..." (30)

The tone of this discussion held in 1951 is in marked contrast with that of the U.N. Conference on the peaceful uses of atomic energy. By that time some of nuclear power's credibility had been restored, and a very favourable political climate had been created for the young commercial industry which was taking shape in Europe and in North America. Although the Conference was intended to be scientific it was attended by an exhibition, which included both commercial and national sections. The USA national exhibit displayed models of pressurized and boiling water reactors (PWR & BWR). The commercial exhibitors included Westinghouse, General Electric and the well-known turbine and boiler makers, Babcock and Wilcox, all of them involved in either the PWR or the BWR. In the late 1950's this American light water technology began the process of capturing world markets. (31) Most significant in this connexion was the visit of the so-called "Three Wise Men", Armand, Etzel, and Giordani, to the USA and UK in the course of preparing their report : "A Target for Euratom" for the recently-established EURATOM Treaty. In the USA they were received by President Eisenhower himself, they met Dulles and Strauss (President of the A.E.C.), the heads of the principal industrial firms in the nuclear industry entertained them at the Atomic Industrial Forum, and they were shown around the nearly completed Shippingport reactor. The outlines of a USA-Euratom partnership emerged: "The United States would make available the necessary fissile materials and the technical knowledge to set our industries going. Once Euratom is established, a task force of some of America's most able men would be at our disposal to continue studying with European experts the many technical problems posed by our programme." (32) Most significant of all was Eisenhower's announcement in February 1956 of 20,000 kg. of 235U for sale or lease outside the USA for peaceful purposes, the price of which was substantially reduced in 1957.

Although an important result of the U.N. Atoms for Peace Conference was to accelerate world interst in light water technology,the idea of the conference seems to have arisen directly out of concern over the accelerating arms race. In 1952 the U.S.A.E.C., having tested a thermonuclear device, was well able to appreciate the magnitude of future nuclear weapons. President Eisenhower was deeply concerned to find a way out of the impasse of the nuclear arms race. Columnists like Joseph and Stewart Alsop were calling for a frank statement to the American public about the effects of the new super-bombs. Adding pressure for action was Oppenheimer's advisory group report on the destructive effects of these weapons. Essential to averting a nuclear war, the report urged, was "wider public discussion based upon wider understanding of the meaning of a nuclear holocaust." (33) As a result of these pressures a committee known as Operation Candor was formed in April to draft a candid speech. The pessimism of these drafts troubled Eisenhower, and his concern grew when he received news of the Russian detonation of an H-bomb that August. Not until the following month did Eisenhower feel he had a worthwhile proposal to give an otherwise pessimistic speech a positive and optimistic slant. Would it not take some of the impetus out of the arms race, he ventured, if the USA and the USSR were "to turn over to the United Nations for peaceful uses X kilograms of fissionable material..." Three months later this plan had been worked out and was unfolded to the U.N. General Assembly on December 8. After mentioning joint contributions of natural uranium and fissionable materials to an International Atomic Energy Agency he went on to outline its duties:

> ... to devise methods whereby this fissionable material would be allocated to serve the peaceful pursuits of mankind. Experts would be mobilized to apply atomic energy to the needs of agriculture,medicine and other peaceful activities. A special purpose would be to provide abundant electrical energy in the power-starved areas of the world. Thus the contributing powers would be dedicating some of their strength to serve the needs rather than the fears of mankind. (35)

At the U.N. Conference, which took place twenty months after Eisenhower's speech, the Secretary General, Dag Hammarskjold, described the event as a "conference of master builders of nuclear science and nuclear engineering ... to discuss, exchange and share their knowledge with the aim of harnessing atomic energy to the purposes of peace and human welfare." Such an aim would show that in unlocking the atom we had done more than "unlock the most sinister Pandora's box in nature. This, in itself will have great psychological value and should free our best creative efforts. But,apart from that, I am sure this Conference will demonstrate the many practical uses to which these discoveries could be put for curing some of our worst physical, social and economic ills, for raising the standards of living, and for lifting mankind to a higher level of well-being". (36)

The major feature of the Conference programme which promoted the international establishment of nuclear power was the revelation of the details of reactor technology. Initially the Americans did not intend to include reactors on the programme. It was pressure from the Norwegians, supported by the British which changed the American's view. (37) What emerged was a clear competition between two developed technologies - enriched light water reactors and natural uranium gas-cooled reactors - the latter offering better supplies of plutonium for weapons, but higher capital construction costs.

On the political side the obvious impact of the scientists' East-West rapprochement stimulated other international organizations to support nuclear power such as the OEEC (which later became the OECD)and the World Bank. The latter organization devoted its 1956 annual meeting to atomic energy in economic development. In its pamphlet on atomic power of 1957 mention was made of the fact that over 36% of its funds were devoted to electric power projects,and its chairman, advised by a former member of the USJCAE, declared: "We must bend our minds and our energies to the task of bringing the bountiful blessings of atomic energy to mankind everywhere, for, by so doing, we shall not only effect material progress, but, more important, we shall engender a spirit of mutuality and of trust which will do much to prevent war and indeed, may abolish that term from the language of men." (38)

3. The Case for Nuclear Power.

The chief reason for the intense interest of scientists and technologists in nuclear power has undoubtedly been the challenge of the unknown which its control and application presented. Coupled with this we would put the scientific elegance of the principle of the Fast or "Breeder" reactor. Compared with this challenge, windmills, solar panels and tidal barrages seemed banal. Given, therefore, the degree of representation of scientists among the expert advisors of governments it was natural that research and development of nuclear power should have been promoted at the expense of all other alternative sources of energy which, before the advent of nuclear power, had been actively investigated.

To the scientist there seemed to be an overriding case for nuclear power which justified this choice. This was the intensity of the energy locked in fissile material . Thus the flow of heat from a fuel element in a modern fast reactor is about 20,000 times that from a water-filled radiator. And if we were "burning" pure ^{235}U in our reactors a one tonne load of this fissile fuel would be equivalent to three million tonnes of coal. This three-million-to-one ratio can be found in the claims made for nuclear power from 1939 to 1979. If it could be achieved it would mean vastly reduced costs for transport of fuel, the ability to stockpile fuel to safeguard against termination of supplies through strikes or international events, and reduced running costs because of the long residence time before

the fuel needs to be removed. Nuclear energy was thus seen as continuing the historical trend towards greater concentration of the energy source which had already been established in the transition from wood to coal and from coal to oil, a trend well suited to centralized production of power. To the scientist and engineer there was an inevitability about the continuation of this progressive trend.

The policy of scientists who have promoted nuclear energy has been to work towards the three-million-to-one ratio and hope that eventually they will get close to it. This was why the American and British scientists devoted considerable attention to fast reactors early in their programmes. Without such reactors the maximum utilization of the uranium appeared unlikely to exceed its content of 235U, i.e. 0.7%, thus reducing the famous ratio to around 20,000 to 1. In 1946 the only concrete data the American scientists had from the Manhattan project came from their Hanford reactors where the utilization was very low. By the time of the Atoms for Peace Conference of 1955 the situation was clearer. Supplies of traditional fossil fuels were predicted to diminish and Sir John Cockcroft could say:

> The papers presented to the Conference have shown that we must not expect the cost of nuclear power to be cheaper in the next decade than power from coal. The consensus of opinion is that capital costs will be appreciably higher - 50 to 100 percent higher - than the capital costs of coal stations, but that fuel costs will be less than half that for coal. So on balance there should be little difference in the cost of power, with nuclear power slightly more expensive than conventional power.
>
> But to many countries that is not the <u>important</u> point: The important point is to obtain an <u>additional</u> source of energy to our conventional energy resources where they are becoming overstrained.
>
> Nevertheless the whole history of engineering development shows how rapidly capital costs fall in the early stages of important new developments, and there is good reason to believe that in the second decade the cost of nuclear power will fall below that of power from coal and oil.
>
> Our second major objective is progressively to increase the amount of energy we can extract from each ton of uranium and thorium. We believe, although we have not yet proved the point, that in the early stations we can extract from one ton of uranium the heat equivalent of about 10,000 tons of coal in a single fuel cycle; but we have also heard of the promise of recycling the fuel in thermal reactors several times so that the energy extraction can be increased five- or ten-fold. We have also heard of the more ambitious <u>final</u> goal of achieving the nuclear physicist's dream of making use of the breed-

> ing principle, and so extracting a great part of the fission energy of the whole uranium. In this way we expect to make one ton of uranium do the work of at least a million tons of coal. The engineer has still to convert the physicist's dream into large-scale practical power stations but we have heard during the Conference of the building of large-scale experimental breeder reactors both of the homogenous and fast reactor types. Both have difficult engineering problems but sometime during the second decade breeding is likely to be an important characteristic of nuclear power stations. (39)

Despite the discovery of more deposits of uranium in the 1950's the future of a world programme for dependence upon nuclear power seemed just as limited as that of coal unless the utilization or "burn-up" could be greatly improved. In the fast reactor the non-fissile uranium (238U),which constitutes about 99% of the fuel, could be converted to plutonium and fissioned. Thus the fast reactor began and still remains the lynch pin to the special claim for a millenium of nuclear power from the fission of uranium. It is generally claimed that the fast reactor can increase the utilization of the fuel some sixty-fold, which Dr. Marsham, of the UK's Nuclear Power Company, remarked makes each tonne of uranium equivalent to two million tonnes of coal. He added - "The world's known uranium resources then become the largest and cheapest energy source available, capable of meeting any likely demand far into the future. I feel," he continued, "that the prospects for fast reactors really stir the imagination of anyone wishing to solve the long-term energy problem. It is a wonderful opportunity for which we should be thankful and we should concentrate on solving the problems rather than allowing them to overcome us. With fast reactors,uranium becomes the largest energy resource in the world". (40)

As reactor development has proceeded the case for nuclear power has altered its emphasis although not its central claim of intensity of energy yield through the Breeder or fast reactor. At first the world resources of uranium seemed so small as to demand the breeder. Subsequently, with the recognition of the difficulties of fast reactor technology, and with success in producing power from thermal reactors, a two stage development was envisaged starting with thermal reactors and reprocessing their spent fuel to win back plutonium for use in fast reactors. The first stage was strongly encouraged by interruptions to Middle East oil supplies, and in the UK poor performance from our coal industry. This security aspect to the case for nuclear power was put most strongly in the "three wise men's" report "A Target for Euratom". Here attention was drawn to Europe's increasing imports of fuel. "Europe's economic growth is in danger of being seriously hampered by the lack of energy to nourish it", the three men wrote. To rely increasingly on imports, particularly of Middle East oil would be "burdensome and hazardous. The advent of nuclear power now gives us a chance to stem their rising tide by building nuclear instead of conventional power stations using imported oil or coal". (41)

When the rate of increase in the demand for electricity began to fall after 1973 the emphasis of the case again changed. There would be an energy gap before the end of the century which only nuclear power could fill. In the 1970's the old alternative energy sources which had occupied a major place in the early World Power Conferences were now being reconsidered. Nuclear experts countered that such alternative technologies were not yet at a stage of development which would allow hope of a major contribution from them in time to fill the energy gap of the late 1980's and the 1990's. Their promotion of the fast reactor, however, ran into expert criticism - most noteworthy being that of a former A.E.A. expert, Leslie Grainger, who pointed out the very long time scale required before the breeder programme could make a significant impact upon our fuel demands. (42)

The case for nuclear power at its most aggressive has now come to rest on the old "Atoms for Peace" formula of energy for the developing countries to achieve a general level of consumption equal to that of the developed Western world and nuclear power for the developed countries to save oil and coal for the others. At its weakest continued support for nuclear power is claimed on the grounds of the need for an "insurance policy" - investing in all alternative sources including nuclear as we do not yet know how much energy we will need and how much the so-called "soft" energy technologies will yield. Although the technical case for nuclear power remains the same as it has always been, intensity of energy source - the three-million-to-one ratio, the political and social climate in which the promotion of nuclear power has had to operate has changed profoundly. Technical experts have only recently begun the process of coming to terms with this fact. (43)

4. Nuclear Scientists and the Public.

It is well known that after 1939 nuclear research was shrouded in secrecy. Those who worked in this field were bound by the Official Secrets Act. Construction activities were likewise kept as secret as possible. In the U.S.A. during the war and in the U.K. afterwards formal planning applications were not required for the many and large research and production facilities that were built. As shown in tables II and III the majority of the British sites were either former airfields, or ordnance factories, but in the States this was not the case. Thus over one thousand families were evacuated from the Oak Ridge site in Tennessee, and at the Hanford site half a million acres were compulsorily purchased from two thousand landowners who used this arid sage brush and cheat grass country for sheep grazing. At Los Alamos a private academy for boys - the ranch school - was obtained by compulsory purchase. Military needs ruled out the possibility of objection save over the scale of the compensation.

As a result of this secrecy there was no informed opinion about atomic energy at the end of the war either amongst the general public or in Congress. Even Truman had known nothing about the Manhatten project un-

til he became president. But as early as 1944 the atomic scientists began discussing the post-war development of nuclear power. They saw a role for themselves in creating awareness in government circles as to the profound impact which they believed atomic energy would have on society whether at war or at peace. Their concern was motivated both by the apparent lack of any plan for the post-war development of atomic energy and by concern for their own professional future. The younger scientists and engineers had invested several crucial years of their careers in this field and it was natural that they should want to build on the expertise they had developed. Those who had come from industry had an eye on its future involvement in the applications of nuclear energy - Zay Jeffries of General Electric for instance.

The result of these discussions was the accumulation of many memoranda and a number of reports - the Jeffreys, Tolman and Fermi-Szilard reports (44) - all of which were studied by the lawyers who drafted the May-Johnson Bill which, despite the scientists' views, gave continued control of atomic energy to the army thus involving continuation of secrecy. At this stage atomic scientists became vigorous in their efforts to influence Congress against the May-Johnson bill, and to inform and educate the public about their science. Their efforts led to the organization of the Federation of Atomic Scientists and the Federation of American Scientists, whose statements were given wide coverage by the press and television. As a result of the scientists' activities the May-Johnson Bill was replaced by the McMahon Bill which gave control of atomic energy to a civilian commission - the Atomic Energy Commission - whose activities were to be overseen by a Congressional Joint Committee on Atomic Energy, and which, under the Vandenberg Amendment, was to liaise with the military forces through a Military Liaison Committee.

The impact of the atomic scientists' pleadings for an end to secrecy was also seen in the publication, two days after the surrender of Japan, of the Smyth Report. This gave so much information about the Manhatten Project that the American public is said to have considered it a lapse of security, and the British government only conceded to its publication reluctantly. Although the Smyth report took some of the steam out of the scientists' demand for the maximum freedom of information, declassification did not extend to reactors, since they produced plutonium. Even after the Russian A-bomb explosion in 1949 information on power-plus-plutonium reactors was not declassified. This had to await the Atoms-for-Peace Conference of 1955.

Despite all that has been written about the American scientists' role as educators and as a pressure group for the involvement of the democratic process in decisions on atomic energy, (45) I consider their most important influence from 1944 onwards to lie elsewhere. Their success in throwing off secrecy was limited. The democratic machinery invoked by the Mc Mahon Act proved subject to abuse. Their most significant influence was

in urging a long term programme of government support for atomic energy and the consolidation of the partnership between this state-supported research and private industry instituted during the war. Admittedly only one of the five-member Commissioners had experience in the Manhatten project as a professional scientist - the physicist, Robert F.Bacher - but the A.E.C. in its early days relied very heavily upon the General Advisory Committee which, under the McMahon Act, it had to appoint. The nine members were: Oppenheimer, Conant, Fermi, Seaborg, Cyril Smith, Worthington, Dubridge, Rabi and Rowe. Generally their advice was followed; only when it came to Oppenheimer's suggestion, endorsed by this Committee, to publish a sober, realistic, assessment of the future of nuclear power which would counteract the public euphoria about the immanence of cheap nuclear electricity, did the Commissioners refuse, fearing that such realism might dispel Congressional support for their programme. (46) Here the responsible caution of the scientists, the majority of them not employed full-time by the A.E.C., conflicted with the institutional commitment of the commissioners. As far as concerns nuclear power this was a rare event.

The A.E.C.'s military commitments also influenced its response to expert advice. Thus the Scientific Panel to the Secretary of War's Interim Committee, which included Oppenheimer, Comption, Fermi, and Lawrence, presented a very circumspect view of the characteristic limitations of nuclear power. It was included in the Acheson-Lilienthal Report, but seems to have been ignored. In the light of subsequent wasted effort upon nuclear powered aircraft, and serious suggestions of small "packaged" reactors for remote sites this authoritative statement of 1946 has proved prophetic: -

> We have examined in some detail the technical problems of making available heat and power on the scale of present world consumption from controlled nuclear reactors. We see no significant limitations on this development, either in the availability or in the cost of the fundamental active materials. We see characteristic limitations and characteristic advantages in atomic power which make us regard it in great measure as a supplement to existing sources, and an incentive to new developments, rather than as a competitor, let us say, to coal or to petroleum products. We see no foundation in current science for the hope that atomic power can be effectively used for light, small portable units such as are required for aircraft and for automotive transportation; but we believe that the development of rather large power units for heat and conversion to electrical energy is a program for the near future; that operating units which will serve to demonstrate the usefulness and limitations of atomic power can be in existence within a few years, and that only the gradual incorporation and adaptation of such units to the specific demands of contemporary economy will involve a protracted development. (47)

There is a striking contrast between the above statement and some of the

ideas put forward in Congress,one of which was the infamous "pill-in-a-pail" suggestion. All you needed to heat your house for a year, according to this idea, was a small pill of uranium in a pail of water. Other calculations suggested that a piece of uranium the size of an egg would propel the Queen Mary from New York to Europe and back whilst other pundits claimed that one the size of a pea placed under the doorstep would suffice to heat a house for its entire lifetime.

> Such remarks amused one writer and led to the delightful piece of satire entitled: "The Cliché Expert Testifies on the Atom". Mr Arbuthnot, the cliché expert, advises his examiner from the witness stand that he had better learn to use the words "harness and unleash" if he wanted to talk about the atom. "They are two words frequently used. With pea,of course."
> Q. "Why pea?"
> A. "Oh, everything is in terms of the pea. You know how much 235U it would take to drive a car to the moon and back?"
> Q. "No, sir. How much?"
> A. "A lump the size of a pea".
> Q. "You wouldn't settle for a lump the size of a raisin or a bean?"
> A. "Sorry. The pea is the accepted vegetable in these explanations". (48)

Recently the same kind of illustrations have been used to explain how small are the quantities of solidified highly-active waste produced from nuclear generated electricity. A debate has taken place as to whether given all our power needs in the form of nuclear electricity it is a piece the size of one or more aspirin tablets per head per annum. Despite the caution of many experts in the early days they were unanimous in seeking special treatment for the funding and organization of research in atomic energy. In countries like Britain, France and Norway this was achieved through direct contact between a few highly-placed people. Thus Raoul Dautry, who as Minister of Supply before the war had supported the work of Joliot's team, impressed de Gaulle in 1945 with the importance of the early French work on uranium and the need to resume it. Shortly thereafter Pierre Auger returned from Canada's Chalk River project, with Joliot-Curie "convinced the President of the need to create an organization devoted to atomic energy in France". (49) Five months later the Commissariat for Atomic Energy (C.E.A.) was formed. In Norway a handful of scientists who had worked in Britain during the war, among them the physicist Gunnar Randers who had been working on radar, persuaded the Minister of Defense, Jens Christian Hauge, to set up a Norwegian Defense Research Establishment in which one of the projects was research into defence against radiological warfare. This involved reactor developments and led to the establishment of the Kjeller Institute.

In the U.K. in 1945 scientists had no need to lobby for support of atomic energy. It was axiomatic to the new Prime Minister, Clement Attlee, as it

had been to Churchill, that Britain as a major world power had to have the bomb, and if atomic energy yielded industrial applications, these too must be pursued. But for security reasons secrecy must be maintained. There was evident support in parliament for nuclear power as in the following exchange:

Sir Frank Sanderson: What steps have already been taken to speed up research in Britain into the industrial use of atomic energy; and in view of the widespread interest in this matter will the Prime Minister consider issuing an interim report on the Anderson Atomic Energy Committee?

Clement Attlee: The hon. Member can rest assured that all necessary steps are being taken to deal with this matter. It would not be in the public interest at present to give details, nor to publish the reports of the Anderson Committee.

Sir Frank Sanderson: Is it not a fact that work covering the whole field of slow controllable reactions is known to be going forward in the United States and Canada, and is it not essential that a programme of similar work should be developed in this country so that during the course of the next industrial revolution we shall not be left standing?

Clement Attlee: I can assure the hon. Memeber that we do not intend to be left behind in any revolution. (50)

When Attlee came to power in the summer of 1945 he appointed Sir John Anderson (later Viscount Waverley) Chairman of the Advisory Committee on Atomic Energy which Attlee set up. Anderson had charge of atomic energy during the war, first as Lord President of the Council, and then as Chancellor of the Exchequer. Until the demise of his committee in 1947 British Governments had expert advice from the time Anderson became Lord President in 1940. Anderson was followed by the nuclear physicist, Sir James Chadwick. Once the post-war project was under way, however, direction seems to have been in the hands of the Atomic Energy Council consisting of the five top men in the programme plus Ministry of Supply administrators. The only significant influence from outside the programme came from the industrial and university members of the Ministry of Supply's Technical Committee.

The activities of the project within the Ministry of Supply remained little known until reorganization in 1954 as the Atomic Energy Authority, an independent public corporation. Under the Act, the A.E.A. had to lay annual reports before Parliament, from which only matters touching on na-

tional security were to be withheld. Atomic energy still enjoyed a special relationship with the government in that its funds were fixed by the Lord President of the Council subject only to the consent of the Treasury. Parliament's consent was not required.

Apart from Sir Henry Tizard, Chairman of the Advisory Committee on Scientific Policy, it is very difficult to find for this period what later became known as "Counter Experts". The effectiveness of his influence was systematically undermined by both the Attlee and Churchill administrations. Nor did any effective opponents of nuclear power surface when the first planning applications for nuclear power stations took place. Britain's nuclear power programme was untouched by the fallout scare of the latter 1950's. Indeed scientists in the Campaign for Nuclear Disarmament argued strongly for nuclear power. What changed the scene was the disagreement within the nuclear and electricity industries as to the best reactor programme to pursue. The Government appointed Committees; a wide range of expert evidence was considered; white papers were produced, and parliamentary debates took place. Thus did the 1960's see the end of the closed policy decision process between committees of scientific experts, civil servants and the Cabinet.

The apparent open and independent character of the US Joint Committee on Atomic Energy (J.C.A.E.) Hearings has been recently questioned and has led to suspicion that this piece of Congressional machinery, in addition to keeping policy decisions in the field out of Congress, has also been largely a white-washing exercise perpetrated by those with vested interests. In the aftermath of Watergate nuclear energy became implicated. Thus Gulf Oil, which until 1973 was sole owner of the General Atomic Company, has disclosed illegal payments exceeding $ 10 million. Among the recipients were ten members of the J.C.A.E., including Craig Hosmer and Chet Holifield. (51) It was also noted in 1976 that twelve members of the J.C.A.E. during the 94th Congress "represented six states which received over 50% of all ERDA funding in the fiscal years 1976 and 1977 (nearly 5.5 billion)". (52) Nor has the Nuclear Regulatory Commission escaped suspicion of vested interests. Thus among its employees in 1976 were seventy former staff of Babcock and Wilcox, Combustion Engineering, General Atomic, General Electric, and Westinghouse. (53)

The assumption that the experts' assessments of the safety of nuclear power stations and the extent of the maximum credible accident were purely an on-going activity of the discipline of safety analysis has also been shaken. It now seems clear that both the Brookhaven and Rasmussen Reports of 1957 and 1974 respectively were carried out to smooth the way for legislation on state support for the insurance of reactors. The Brookhaven Report thus preceeded the Price-Anderson Act(54) which initially promised $ 500 million of federal government insurance, and the Rasmussen Report preceded the renewal of this Act in 1975. According to one critic "the AEC/NRC first briefed members of Congress on a draft of

the Reactor Safety Study without disclosing internal criticism by A.E.C. reviewers, rushed completion of the report to coincide with congressional schedules, and then presented the final report to the Congress without mentioning that interested scientists who had asked repeatedly to see the final document had not yet been provided with copies". (55)

Conclusion.

The technical expert has sometimes urged the politician to promote nuclear power, at other times the politicians have urged the expert to expand his programme. This happened twice over the U.K.'s first civil power programme. (56) At the same time the expert has been caught up in an institutional development of mammoth proportions in which regulation, promotion, and capitalist exploitation have been from the beginning interwoven. The military and secret character of the early phase of nuclear power development served to insulate the expert from public examination and accountability. As the extent of public involvement in energy policy has grown so the case for nuclear power has altered its emphasis. Meanwhile the basic technical claim of the three-million-to-one ratio , so often cited even today in the promotion of nuclear power, is well known to be an ideal situation that no advanced nuclear technology can ever reach.

TABLE 1

Event	Date
Smyth Report	August 1945
Chalk River Research Reactor goes critical	Sept. 1945
McMahon Bill introduced into Congress	Dec. 1945
United Nations Atomic Energy Commision established	Jan. 1946
Acheson-Lilienthal Report published	April 1946
Baruch Plan published	June 1946
McMahon Act becomes law	August 1946
UK Atomic Energy Act passed transfer of atomic energy from Dept. Scientific & Industrial Research to Min. of Supply	Nov. 1946
US Atomic Energy Commission established	Nov. 1946
Moscow Physical Laboratory No.2 Research Reactor goes critical	Dec. 1946
Canadian Heavy water reactor goes critical	July 1947
US Research Reactor goes critical (graphite-low-energy-experimental pile - GLEEP)	August 1947
Rickover's June 1947 memorandum on research for experimental submarine nculear power plant sent to Atomic Energy Commission	Jan. 1948
Norway's Institute for Atomic Energy founded	Jan. 1948
First Harwell Power Conference	Dec. 1948
First French research reactor goes critical	Dec. 1948
UK Natural Uranium Reactor Group formed	Jan. 1951
The Norwegian-Netherlands research reactor goes critical	July 1951

Event	Date
US experimental Breeder Reactor at Idaho generates electricity	April 1952
UK Monte Bello A-bomb test	Oct. 1952
US Thermonuclear Device exploded on Eniwetok	Oct. 1952
UK Government announces Nuclear power programme	Jan, 1953
US nuclear submarine, Nautilus, launched	Feb 1953
Elimination of US Nuclear Carrier project	March 1953
US land-based prototype reactor for Nautilus goes critical at Arco	May 1953
Full power operation of US submarine reactor prototype	July 1953
USSR - H-bomb explosion	August 1953
UK Waverley Committee on reorganization of UK atomic energy programme	Nov. 1953
Norway arranges first international Conference on nuclear reactors	Dec. 1953
Eisenhower UN General Asembly Speech on Atoms for Peace	Dec. 1953
US A.E.C. receives 9 proposals for nuclear power stations (including Shippingport)	Feb. 1954
UK Dounreay site announced for Fast Reactor	March 1954
US H-bomb test	March 1954
USSR reactor at Obninsk generates 5 MW electricity for town of Mosehnergo	June 1954
US Congress amends McMahon Act	August 1954
Eisenhower attends ground-breaking ceremony at Shippingport	Sept. 1954

Event	Date
UK government White Paper announces first civilian power programme	Feb. 1955
US General Electric experimental breeder supplies electricity temporarily to grid	July 1955
First UN Conference on Peaceful uses of Atomic Energy	August 1955
Eisenhower announces 20,000 kg of 235U for sale or lease	Feb. 1956
Norwegian parliament approves Halden heavy boiling water reactor	March 1956
Queen opens the UK's Calder Hall power station	Oct. 1956
Brookhaven Report: 'Theoretical Possibilities & Consequences of Major Accidents'	March 1957
Mary Kathleen miners' strike to demand services of permanent doctor	Feb. 1957
EEC members sign Euratom Treaty	March 1957
European Report - "Target for Euratom" published	May 1957
UK Chemical Workers Union discussion of safety of general population from nuclear wapons and nuclear power	June 1957
US Congress passes Price-Anderson Amendment to 1954 Atomic Energy Act (10-yr statute, renewed 1967 and 1977)	
US Government cuts price of enriched uranium by 34%	Oct. 1957
US Windscale accident	Oct. 1957
Shippingport nuclear power station reaches full power	Dec. 1957
King Olav opens Halden reactor	Oct. 1959

TABLE II

Atomic Energy Factories	Former use of site	Permissions	Date of Takeover
Oak Ridge - Clinton Laboratory and Uranium Enrichment facilities Tennessee	Agricultural	Compulsory Purchase	1942
Los Alamos Weapons Laboratory, California	Ranch School	Compulsory Purchase	1942
Hanford Reactor & Plutonium Separation Plants, Washington State	Agricultural	Compulsory Purchase	1943
Argonne National Laboratory	Argonne Forrest Reserve	Lease from Cook County Commissioners	1946
Brookhaven National Laboratory, Long Island	Agricultural	Compulsory Purchase	1946
Sandia Laboratory	Old Albequerque Airport	Compulsory Purchase	1944
Remote Testing Station, Idaho Falls	Part navy owned	Navy transfer & some compulsory purchase	1948

TABLE III

	Former use	Procured by	Established	Function
Harwell, near Didcot and Oxford	R.A.F. Airport	Transfer	1945	Research
Risley, near Warrington			1946	Design Centre and Headquarters of Industrial Group
Springfield, near Preston	Disused Poison Gas Factory	Transfer	1946	Manufacture of Uranium metal
Windscale, near Whitehaven	Munition Factory at Sellafield	Transfer	1947	Plutonium Production (includes reprocessing)
Capenhurst, near Chester	Royal Ordnance Factory	Transfer	1949	Gaseous Diffusion plant for Uranium enrichment
Drigg, near Windscale	Ordnance factory	Transfer	1947	Some waste disposal
Aldermaston	R.A.F. Airport	Transfer	1950	Weapons production

FOOTNOTES

1. D.N. Dunlop, Foreword to the First World Power Conference, London, 1924, vol. i, pp. vii-viii.
2. Dwight D. Eisenhower, Address to the United Nations General Assembly, 8th session, 8th December, 1953.
3. M.M. Gowing, Britain and Atomic Energy, 1939-1945, London, 1965.
4. Medical Research Council, Second Report, The Hazards to Man of Nuclear and Allied Radiations, Cmnd. 1225, London, 1960, p. 11.
5. The atomic scientists' perception of the quantitative difference between atomic and conventional bombs before Hiroshima is described in Alice K. Smith, A Peril & a Hope, The Scientists' Movement in America 1945-47, Chicago, 1965, chap. 1.
   J. Hirschfelder has supplied the author with a fuller account, including his perception of the qualitative differences - in particular the possible genetic effects from radioactive fallout. Other scientists did not share his concern on this point.
   See also the Franck Report in E. Rabinowitch (Ed.), The Dawn of a New Age. Reflections on Science & Human Affairs, Chicago, 1963, pp. 99-109. There is no mention in this report of the possible harm from radiation.
6. The first atomic bomb (Hiroshima type) was exploded at Alamogordo in the deserts of New Mexico on 16th July, 1945.
7. S. Flügge, "Kann der Energieinhalt der Atomkerne technisch nutzbar gemacht werden?", Naturwissenschaften, 27 (1939), 408. He reckoned that the energy output from the complete fission of a cubic metre of uranium oxide would be equivalent to the total output from the German power stations run on brown coals for eleven years!
8. S.P. Weart, Scientists in Power, 1979, p. 102.
9. Ronald W. Clark, The Birth of the Bomb, New York, 1961, p. 21 quoted in Weart, op.cit., p. 86.
10. Interview by Charles Weiner, October 1969, Cited in Weart, op.cit., p. 86.
11. See David Irving, The Virus House: Germany's Atomic Research & Allied Counter-Measures, London, 1967.
12. See R.G. Hewlett & F. Duncan, Nuclear Navy, 1946-1962, Chicago, 1974, p. 19ff., where it is pointed out that Ross Gunn's isotope separation studies at the Naval Research Laboratory did prove helpful in the Manhattan project development of thermal diffusion plant. Referred to in future as Nuclear Navy.

13. R.G. Hewlett & O.E. Anderson Jr., wrote: - "This report gave Bush and Conant what they had been looking for; a promise that there was a reasonable chance for something militarily useful during the war in progress. The British did more than promise; they outlined a concrete program." In: A History of the United States Atomic Energy Commission, vol. i, "The New World", Pennsylvania, 1962, p. 43. Referred to in future as The New World.

14. MAUD Committee, 1941, in M. Gowing, Britain and Atomic Energy, London, 1965, p. 427 ff.

15. See A.M. Petrosyants, From Scientific Search to Atomic Industry, 1972, English trans., Danville, Illinois, 1975, p. 11.

15a. The French case is complex. The first reactor at Marcoule produced electricity two years before the first French atomic bomb was exploded. The thinking behind the building of the Marcoule reactors, however, was to produce plutonium also, with an eye to the possibility of using it in weapons, should the decision be taken to make them. Thus Scheinman wrote: "In contrast to the Marcoule reactors, the later reactors of the Electricité de France built at Chinon were explicitely for commercial power production. Thus Scheinman wrote: "The atomic bomb project was not given the 'green light' until several years after work on electricity production had begun, but the bomb was completed two years before the first of the Chinon plants went into operation." L. Scheinman, Atomic Energy Policy in France under the Fourth Republic, Princeton, 1965, p. 94.

16. The only opponent among the Commissioners to the foreign distribution of isotopes was Lewis Strauss. Hewlett & Duncan explained Strauss' view as follows: "By distributing isotopes in large quantities abroad, the Commission would be committing a breach of security comparable to that of publishing the Smyth report." R.G. Hewlett & F. Duncan, A History of the United States Atomic Energy Commission. vol. ii. Atomic Shield, 1947-1952. Pennsylvania, 1969, p. 109. Referred to in future as Atomic Shield.

17. This rejection was strongly influenced by fears that ordering compressors for the helium would seriously delay the project. See The New World, p. 179.

18. The McMahon Bill (Statute 1717, Dec. 20, 1946) is reprinted in The New World, pp. 714-722. When passed by Congress it became the Atomic Energy Act.

19. Atomic Shield, p. 47. The authors do not reveal the actual number of unassembled bombs, but evidently it was small. See also Sheldon Novick, The Electric War; The Fight over Nuclear Power, San Francisco, 1976, p. 1.

20. Quoted in Clay Blair, The Atomic Submarine & Admiral Rickover, New York, 1954, p. 141.

21. Nuclear Navy, p. 255. Rickover's figures for nuclear (Shippingport) & conventional fossil-fuel stations were 64 mills per kW, and 6 mills per kW respectively.

22. Atomic Shield, p. 516. By 1952 the A.E.C. had spent about $100,000,000 on this project.

23. A factor in the Norwegian development of reactors was the hope to play a part in marine reactor development. See: O. Dahl, "Preliminary Study of an Experimental Pressurized Heavy Water Reactor", Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, New York, 1955, vol. 3, p. 243. This work is referred to in future as Peaceful Uses. The Norwegian hope of nuclear powered ship propulsion was given up in 1960, when it became clear that the cost was high and the U.S.A.E.C. refused to participate in the Norwegian programme. See: - Institutt for Atomenergi, Oversikt over virksomheten 1948-1960, Kjeller, 1960, p. 35.

24. B. Commoner, The Closing Circle, Confronting the Environmental Crisis, London, 1972, p. 60.

25. See Duncan Burn, Nuclear Power & The Energy Crisis: Politics and the Atomic Industry, London, 1978.

26. "Disaster unlikely in Britain", Atom News, No. 205, April 1979, p. 1.

27. Ward F. Davidson, "Nuclear Energy for Power Production - II", Proceedings of the Fourth World Power Conference, vol. ii, p. 2461.

28. Sir H. Hartley, "Discussion", in Section J, Fourth World Power Conference, vol. ii, p. 2474.

29. F. Simon, "Discussion in Section J", Fourth World Power Conference, vol. ii, p. 2484.

30. Ragnar Liljeblad, "Some Economic & Technical Aspects of the Use of Nuclear Fuel for Power Production", Fourth World Power Conference, vol. ii, p. 2471.

31. I. Bupp & C. Derian, Light Water: How the Nuclear Dream Dissolved, New York, 1978.

32. Louis Armand, Franz Etzel & Francesco Giordani, A Target for Euratom, May 1957, p. 26.

33. R.J. Donovan, Eisenhower. The Inside Story, New York, 1956, p. 184.

34. Donovan, op.cit., p. 186.

35. Cited from Donovan, op.cit., p. 191.

36. Dag Hammarskjold, "Address to the Conference", Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, New York, 1956, vol. xvi, p. 28.

37. Author's interview with Dr. Randers, February 1978. A search through the Eisenhower Presidential Library and the United Nations archive has not yet substantiated this claim. (It relates to Oslo meeting of 1953.)

38. Antonio C. Flores, Chairman of the Board in: Atomic Power. An Appraisal including Atomic Energy in Economic Development, World Bank, Ed. Corbin Allardice, Oxford, 1957, p. 85.

39. Sir J. Cockroft, "The Future of Atomic Energy", Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, New York, 1956, vol. xvi, pp. 121-122.

40. T.N. Marsham, "The Fast Reactor and the Plutonium Fuel Cycle", in G. Foley & A. von Buren (eds.), Nuclear or Not?, Heinemann, 1978, p. 152.

41. Target for Euratom, p. 39.

42. L. Grainger, "The Nuclear Issue as seen by a Competitor", Energy Policy, 4 (1976), 322-329; also "The Urgency of the fast breeder - to delay or not?", Energy Policy, 5 (1977), 245-250.

43. For example, Sir J. Hill, "The principle problems of nuclear power are now not engineering or technology but problems of political will and public acceptability. In the United States you have had for many years a wide public debate on nuclear power. There has been little debate in the U.K. until perhaps twelve months ago when it broke with all its fury and has been raging ever since." From: "The Politics of Nuclear Power", Atom, No. 243 (1977), p. 1.

44. The greater part of the Jeffreys Report has been published in A.K. Smith, The Peril & the Hope, 1965. The others to my knowledge still unpublished.

45. See A.K. Smith, op.cit., R.G. Gilpin, American Scientists & Nuclear Weapons Policy, 1962, and articles in The Bulletin of the Atomic Scientists.

46. Atomic Shield, p. 115 ff.

47. U.S.A., Department of State, Committee on Atomic Energy (Chairman D.E. Lilienthal), A Report on the International Control of Atomic Energy, Washington, 1946.

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49. Bertrand Goldschmidt, The Atomic Adventure, its Political and Technical Aspects, trans. Peter Beer, Oxford etc., 1964, p. 60.

50. Hansard, 5th series 1414 (1945-1946), col. 25-26.

51. This is according to the account by Peter Faulkner, "The Atomic Industrial Complex", in: The Silent Bomb. A Guide to the Nuclear Energy Controversy, ed. Peter Faulkner, Vintage Books & Friends of the Earth, San Fransisco, 1977, p. 203. Also see: Common Cause, Stacking the Deck: A Case Study of Procedural Abuses by the Joint Committee on Atomic Energy, Washington D.C., 1976.

52. See Stacking the Deck, op.cit., ref. 51.

53. P. Faulkner (ed.), The Silent Bomb, p. 207.

54. U.S.A.E.C., Theoretical Possibilities & Consequences of Major Accidents in Large Nuclear Power Plants, WASH-740, Washington, D.C., 1957, & U.S. Nuclear Regulatory Commission, Reactor Safety Study: An Assessment of Accident Risks in U.S. Commercial Nuclear Power Plants, WASH-1400, Washington, D.C., 1975.

55. Richard Selove, Review of Risk Assessment Review Group Report to the U.S.N.R.C., Bulletin of the Atomic Scientists, 35 (1979) No. 2, 47-48.

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EXPERTS IN A PARTICIPATORY EXPERIMENT: THE AUSTRIAN DEBATE ON NUCLEAR ENERGY

Helga Nowotny, European Center for Social Welfare Training and Research

1. Experts and the public: conflict by proxy?

In today's scientized world it would be hard to imagine to get along without experts. The widespread belief in scientific rationality has invaded all features of modern life: problems are primarily defined in ways that suggest scientific and technological solutions and experts are looked upon as the problem-solvers. They are specialists in their own, increasingly specialized fields, who are asked - usually on behalf of a sponsor - to study certain problems, to assess them and to derive conclusions from their judgement. The services experts render become more public in the sense that the problem-class has grown in which expert assessment and recommendations are in demand as well as the uses to which expertise is put. The problems range from hypothetical assessments of the various consequences of preferred policies to the construction of alternatives and to assess them typically involves a diffuse mixture of science-derived methodologies with in-built normative assumptions. Experts are used to legitimize political decisions already taken or planned and, more recently, to constitute a new political resource in the form of counter-expertise for those groups who feel that their concerns are not heeded otherwise. While the now familiar sight of experts contradicting each other in public is historically not entirely new (1), expertise as advocacy for policy alternatives has extended into areas previously held to be exempt and has led to some unusual coalitions between small groups of scientist-activists and segments of the public. These developments mirror the greater concentration of technological risks, embedded in highly interdependent and organizationally complex social structures which have led to a heightend awareness and concerns about them and the ongoing de-mystification of science as an institution by revealing its links to economic interests and political forces. These changes also led to a search for new responses to a problem-class which previously lay at the margin or outside scientific concerns properly speaking. As the Austrian case study will show, experts engaged on opposite sides in the debate on nuclear energy are not only at odds

in their assessment about the potential hazards of this energy form, but are engaged in a conflict whose wider significance touches upon the role of experts and how to cope with demands in a situation which poses a series of dilemmas for them.

The term "public" has become a catch-all phrase, usually referring to various pressure groups and citizens' initiatives which have come to a precarious existence outside the established political parties. The conceptual diffuseness of the term is in itself indicative for the lack of any institutionalized form in which participation in the decision-making process relating to technological developments could be launched or which would permit assessment in other ways than vociferous protest. Retrospectively it is clear that nuclear interests have enjoyed a virtual monopoly in the domain of energy policy in practically all countries in which confrontation with parts of the public would later arise. Pollak and Nelkin have summarized the situation by pointing to the exclusive dominance of scientific, industrial and governmental interests in the absence of other social groups (2):

> The organization of this nuclear establishment has been labelled a multi-monopoly, controlled by a small number of industrial and administrative groups; for there is virtually no competition in any area of activity involved in the production of nuclear power. Rather, the organization is based on a strong solidarity of interests among 'nucleocrats' devoted to rapidly expanding the development of nuclear power and to maintaining control over nuclear policy. Parliament and traditional political organizations have played no significant role in controlling this policy area.
>
> The anti-nuclear movement put into question the consensus prevailing in the establishment. Practically unchallenged for decades, the nucleocrats were to a large extent not prepared to face an open democratic debate.

While control over technology and related policies remains the dominant issue in the political arena, the conflict between different groups of experts and between them and the public can also be seen in the light of previous expropriation of a set of problems and subsequent attempts to re-appropriate them as falling into a legitimate domain for public discussion. To the extent that problem-expropriation and attempts at re-appropriation take place in the context of political institutions which so far have displayed little flexibility in becoming structurally more responsive (3), the conflict between different groups of experts and between them and the public can be seen as a conflict by proxy. As the debate progresses, it becomes clear that solutions cannot be found within the realm of expert discourse alone. The expert-public

discussions allow the political decision-making bodies a welcome breathing space and sufficient leeway to return towards the utilization of political mechanism for solving the conflict. This is also what happened in the Austrian case. We shall later explore the more general consequences of such developments.

2. Enlightenment from above and protest from below: some lessons to be learnt

Any national context is likely to leave a strong mark on the relation between experts and the public. The salient features relevant to the organization of the Austrian 'Information Campaign on Nuclear Energy' (the German word 'Aufklärung' carries an unintended double meaning of information and enlightenment) were essentially two: a strong tradition of 'reform from above' and an equally strong centralized bureaucracy. The idea was initiated by the chancellor and a small group of his advisors at a time when public concerns about nuclear power, at least in Austria, were still at a low ebb. It was organized by a ministerial bureaucracy, competent at setting up and administering the experts' discussions, but without previous experience in participatory experiments involving the public in any other than a purely administrative function. Its most original feature was the idea that pro and contra experts should be granted equal opportunity to present their views. The hope was openly expressed that this procedure should enable experts to reach consensus on a number of issues and to put into sharp focus which areas of disagreement persisted. Although it turned out that parity of experts was not to be achieved in practice, the in-built components calling for co-operation between experts, but also allowing for conflict (preferably channelled), set the stage for an interesting quasi-experiment. The experts, divided into ten working groups, were to answer a catalogue of questions being put before them, formulated after suggestions made also by critical experts. Later on they were expected to discuss the results of their deliberations with the public in the course of meetings organized in different parts of Austria.

In the following section I will summarize the main discrepancies which arose between the organizer's expectations and the actual course of events (4). As it turned out, the official administrative expectations made from above, as to how experts would relate to the public, differed considerably from the public's reaction to the campaign. There are several lessons to be learnt from these discrepancies.

Expectations of Organizers	Actual Developments
Stated Objectives:	
'objective information' of the public through experts	The predominantely critical public rejected 'objective information' as either not possible or as disguised partiality
General distrust of the public against the organizers and experts prevailed; suspicion that the pro-contra arrangements were only a clever tactical move to outwit the public.	
Target group:	
As yet uninformed, but neutral citizens; interested but no opinion on nuclear energy as yet.	The discussion evenings were frequented almost exclusively by more or less organized groups of opponents; the 'neutral citizens' chose not to participate.
The public should 'first be informed, then discuss'	The public felt already sufficiently informed and wanted to discuss.
Not just a matter of false timing, but erroneous judgement regarding target groups.	
Political component of public discussions:	
Not to be 'misused' for political demonstrations, but to facilitate 'exchange of information' only.	The opponents regarded the public discussions as a unique opportunity to demonstrate their opposition. They regularly voted resolutions addressed to the government and tried to reach a larger audience through the mass media.
Open conflict; the organizers had to compromise; increasing police protection; the final public discussion had to be cancelled for fear that violence would errupt.	

Political absenteeism:

Since the organizers distinguished between an opinion formation phase and a political decision-making phase (parliamentary decision), politicians were largely absent in the public discussions.	The public criticized the absence of responsible political representatives; experts were no surrogate. Distrust that experts had a mere alibi function and should pacify the public, while political decisions had long been taken.

The organizers wished to assure maximum non-interference with the political decision-making process; experts were invited in purely advisory function; the public's participation was limited to (non-political) opinion formation, but refused to accept this distinction.

Organization of the campaign:

The time-schedule of public discussions followed the systematic organization of the report into 10 different 'main themes'.	Complete rejection of this arrangement, since the public wanted to discuss a wider range of issues and not one.
In order to achieve regional diffusion, each main theme was to be discussed in a different part of the country.	The public invited to the 'less interesting' themes inevitably felt left out.
Initially, only questions related to the evening's theme were to be allowed.	Was not to be enforced after opponents took over the experts' rostrum in the second discussion.

The planning of the campaign followed rigid principles of a logical-systematic nature which were completely inappropriate for public discussion. After some initial 'incidents', a learning process set in and the organizers compromised increasingly. (E.g. additional 'resource persons' were invited; practically all questions were permitted, etc.).

Expert parity:

This was the genuine wish of the organizers which could not be realized; one third of the experts were opponents.	The public remained suspicious; it claimed the critical experts as 'theirs'; demands were raised for additional critical experts in those groups which had none or only one.

While the organizers felt committed to the ideal that areas of scientific-technological agreement and disagreement could be distinguished as part of an overall problem-solving strategy, the public was primarily interested in the political value of 'counter-expertise'.

Participation:

The organizers had precise ideas about the extent of public participation - very limited - and set the rules of the game.

The public refused to follow these rules and tried to contravene them whenever possible (e.g. appointment of an opponent as new chairman in one group).

Following the administrative tradition of an "Obrigkeitsstaat", the public participation was severely limited. Only later on informal contacts were established with the various citizens' initiatives and local protest groups as part of an overall attempt to avoid further escalation of the conflict. The public wanted political participation - the possibility to exert influence on the ultimate decision about nuclear power and rejected the role of an administrative by-stander.

Role of mass media:

The mass media were to act as multipliers of the information campaign, but failed to do so, since they only concentrated on the more spectacular confrontation events.

The protest groups could not win the mass media for their cause, with one - regional - exception; the reports on them were rather negative.

It remains unknown to what extent and under what conditions the mass media could have been involved to a larger extent and in a less biased way.

External constraints and influences:

While organizing the information campaign, Austria's first nuclear power plant was nearing completion. Although the organizers emphasized that the decision for its operation would rest with parliament, nobody seriously believed at the time that the ultimate decision would be a NO to nuclear power.

For the public this remained the most important fact. Their protest was primarily directed against setting the plant into operation; the campaign was used as an instrument to voice concerns and to demand a shutdown.

Apart from the experts' assessment and largely independent of it, the political opinion formation process went into motion through the usual political channels.

The protest groups who came largely from outside the established political parties, had no real access to the political decision-making structures. The campaign became a major rallying platform and increased the organizational capacity of the protest groups.

> The organizers, embedded into the overall administrative-political context, had no control over the processes of political - as distinct from public - opinion formation, nor could they negate the fact that previous decisions had been taken without public involvement. This points to a serious limitation of all participatory efforts.
>
> Subsequent events took place in the political arena only. The decision to hold a referendum was motivated by concerns to keep the forthcoming election clear of the issue of nuclear power. The result of the referendum came as a big surprise: 50,47% voted with NO; 64.10% of the population participated in the referendum. As a result, parliament passed a law, forbidding Austria's only nuclear power station to go into operation.

The lessons point to the overall limitations of participatory experiments in the absence of genuine structural responsiveness of the political institutions. Although a number of organizational improvements readily come to mind, the overall frame of the campaign was outside the organizers' control.

The public's reaction to the experts was permeated by suspicion, distrust, and a general form of hostility from which only the critical experts were exempt. Although the main wrath of successive audiences was directed against 'them up there', a category which included the government, politicians, parliament, industry and bureaucracy, the experts were physically present and seen as 'their' representatives. Protests and the articulation of discontent can also be viewed as a form of assessment. On a more nuanced level, the public approached the experts with an attitude which I would like to call scientific populism. They wanted to know what the benefits of science and technology were 'for the people' and they demanded that the experts accounted to them. This could be done in one of two ways: a paternalistic form of scientific populism and a militant one. The experts were either urged to remain loyal to the lofty ideals of science and its promises, namely to bestow benefits on the world and to ward off potential harm. Experts were accused of violating these ideals and threatened with the withdrawal of public support, belief, and loyality, if present wrongs would not be remedied. The

almost religious fervour exhibited in this strand of populism and its adherence to the paternalistic authority of science, which could only be broken if this authority in turn violated the terms of the implied 'social contract', contrasted sharply with the militant form. Here, experts were denounced as mystifiers and accused of working solely in the service of vested interest. It was up to the people to decide which science they wanted and presumably the institution of science was sufficiently flexible to follow such mandates. - Although only a minority of spokesmen and women articulated these demands, they point to a neglected component in the general public attitude towards science and technology. Science as a cultural form of thought and as a belief system has permeated everyday life and as an institution, transformed societies. Nevertheless, it has never been fully integrated into popular culture. It remained apart, a dominant system of thought and practice, either to be admired as benefactor or to be fought when corrupted. By rejecting its claims to superiority in the name of a democratic fundamentalism or by demanding the restoration of benevolent paternalism, the present rule of science as a form of cultural domination was disputed. Protests, when launched sporadically and without wider support are easy to ignore or supress. However, the underlying tension is likely to persist.

3. Assessment by argument: technical assessment and its sociological interpretation

Although care was taken to invite pro and contra experts, the selection procedure sought to assemble experts whose 'independence' was apparent and credible, i.e. they should not have any direct ties to the nuclear industry, nor have previously been involved as consultants in the state regulatory proceedings. University-based experts therefore figured prominently, with a smaller group coming from non-university research institutes and the rest, mainly economists, being drawn from quasi-official institutes. While the moderators of each expert-group had been carefully chosen among leading Austrian experts, it was up to them to assemble their own group. First deliberations in each expert group took place in the calm atmosphere of collegial gatherings; experts had to provide informed answers to the question catalogue which consisted of topics ranging from technical, social and bio-medical aspects of the hypothetical and real dangers connected with nuclear power, to a comparison of economic costs and the special safety provision of the Austrian reactor (5). This involved lengthy discussions on the available data, a comparison of findings reported in the literature, a judgement of their validity and generalizability and finally the process of systematic sifting and compilation of what was known and acceptable in the light of the prognosed questions. Occasionally a new search was initiated when gaps in knowledge became apparent but on the whole the experts relied on secondary data. There were few discussions on matters of principle, although both proponents and opponents made their views explicit at an early stage of the internal deliberations. Later on,

in the heated atmosphere of the public discussions, experts on both sides would come out more forcefully with their views and emphasize disagreement rather than agreement.

For purposes of an analytic interpretation of the assessment process, I wish to distinguish between a technical assessment phase and an argumentative. In practice, both were interlinked and alternatively made explicit by the experts. It is a moot point to insist that a logical ordering would call for the technical phase to precede the argumentative, or in other words to assume that experts would approach their task with blank minds. This was obviously not the case, since they had been selected knowingly for their stand on nuclear power. As in the judicial process, to the initiate the crucial legal categorizations and choices among them are known at a very early stage. The following procedure merely consists in gathering and subsuming the appropriate evidence, in comparing alternatives in order to eliminate the weaker ones and to map an unruly reality with its many subtle distinctions and inherent ambiguities into a rigorously ordered, logically constructed and universal scheme which permits one to arrive at only a small number of possible outcomes. In a similar vein, the experts knew, although this is uncoded knowledge, that only a few alternative routes exist in the assessment path. In choosing these routes they have to argue, and mostly do so sincerely, that their conclusions are based on irrefutable facts, guided by the certainty of their expert knowledge. In going through the technical assessment phase, apart from the manifest activity of comparing what each expert knew and of putting the collective store of knowledge and expertise into systematic order, they sought to establish their own credibility as scientists or technicians (and a tiny minority as 'practitioners'), in order to build up the necessary 'scientific' weight for their arguments. It would therefore have been a wrong tactic to engage in any fundamental discussion pro and contra nuclear power without having first established one's credentials. The argumentative phase of the assessment process is therefore the essential one, but in practice it had to appear overshadowed by the technical.

Any assessment procedure is sensitive to the assumptions from which one starts. The technical part is largely an honest attempt to create a secure factual basis, as 'scientific' as possible, by assembling data and establishing facts beyond reproach. Whatever the methodological framework or the body of guiding knowledge or skills may be - and however weak the overall state of the art - the argumentative phase lying ahead compels the experts to act AS IF their basis for argumentation were irrefutable. The greater the discussion of details, the more irrelevant the outcome, but important nevertheless for scoring points for one's credentials. Thus, the experts were locked into a closed universe of technical debate, which had to remain futile, as long as its overall purpose was to legitimize the argumentative phase. No wonder that none

of the experts were convinced by arguments of their colleagues or the information received; and although an astounding degree of cooperation was achieved, the stalemate persisted in areas that mattered.

The argumentative assessment consisted of a series of statements which were arranged in a small number of logically ordered sequences leading to a verdict: pro or contra nuclear power. And although conditional statements occurred as did differentiations, sometimes of highly sophisticated nature, it was nevertheless surprising how stereotyped, predictable, and restricted the chains of arguments were. Verbal cues, the differentiated use of 'I', 'we', 'one' or 'they' served as indicators for the individual's stand, but the block elements in the process of the argumentative assessment were highly standardized. They consisted of arguments related to technical and social hazards of nuclear power, the economic necessity or lack thereof, a general pronouncement on risk in general and on the risks of nuclear power in particular, comparison with other risks, comparison of economic costs and alternatives, and a general judgement of future developments. Since space does not permit me to go into details, I will briefly summarize some results standing out from the argumentative assessment procedure.

- arguments related to the technical safety of nuclear power dominated by far; only one third of all arguments referred to economic aspects;

- proponents and opponents differed above all in their assessment of the seriousness of potential hazards, their calculability and predictability and the ability to control them;

- by using their arguments, proponents and opponents can be placed on a continuum. The extreme pro-group argued that nuclear power is first of all economically necessary, since no alternatives exist;it is at least as safe as other hazardous activities and alternative sources of energy; risks are inevitable in general; the risk associated with nuclear power can be mastered. The extreme contra-group, while rejecting all these (and other arguments) maintained that nuclear power is extremely dangerous, cannot be mastered since it contains too many unknowns. Furthermore, alternatives exist or should be developed rapidly.

  Between these extremes,another pro-group argued that nuclear power is safe and can be approached positively. Their counterpart, the sceptical opponents, argued that at least now too little was known about the extent of potential hazards and their dimension. Neither of these two groups was much disposed to discuss the economic side. In the

middle of this continuum the arguments lead towards adopting a cautious but in the end still positive stand towards nuclear power. Under the condition that no alternative energy sources can be mustered, nuclear power was viewed by them as a transitory energy supplier until fusion or solar energy production will succeed. Risks associated with nuclear power were readily admitted, but again, under many precautions, held to be containable.

- The argumentative chains of assessment do not differ in essence from arguments put forth by politicians, the mass media or by parts of the public. They were only presented in a more systematic and logic form.

- Counter-arguments of the other side, referring to risk or economic necessity were routinely considered and invariably dismissed as false. This made for a highly ritualized and sterile exchange of arguments.

- Persuasion of the other side was not to be expected in the argumentative phase, nor did it occur in the technical assessment phase. Experts admitted that they had received new information and had learned more about the reasons why the others assessed the situation differently. This new understanding varied, however, and was not always positive.

- The arguments which guided the experts in their overall assessment provided a grid, into which technical details, sifted and compared in the technical assessment phase, could be fitted.

On the level of sociological interpretation, the patterns of arguments pro and contra revealed affinities with the experts' biographies and previous work experiences. The mechanism of remaining a proponent, essentially sheltered by a stable career development and a consonant cognitive environment was investigated, as was the genesis of breaking away and becoming a dissenter. Without succumbing to a misplaced sociological determinism there seems to exist a greater readiness to espouse a world view modelled to include discontinuities among those who have experienced some discontinuities or other instabilities in their personal lives, while linear assumptions underlying a continuous world view are more consonant with those who have experienced smooth careers. In addition to career and work context, disciplines provide somewhat different filters. Also apparent is a process of intersection between developments on the macro-level and on the micro-level. On the former, a gradual awareness of the potential hazards of nuclear power took place, patterned by collective experience and exposure to it; on the latter individuals proved either sufficiently sheltered in their pro-

attitudes, or - in the absence of such filters - were jolted or moved gradually to adopt a counter-position. Assessment in the form of arguments as presented by the experts constitutes therefore only one part of an ongoing process of expert opinion formation which takes place simultaneously in expert communities and on the individual level. A long-term stabilization can be expected between a slow increase in dissenting experts and softening of the stand of the majority, without either necessarily having any serious impact on actual developments of nuclear policies.

4. Assessment through pro & contra: two dilemmas

The most original feature of the Austrian debate was undoubtedly its adoption of an openly contradictory assessment procedure. The organizational constraints were shaped to guarantee, nevertheless, a maximum of cooperation between experts in the internal discussion rounds, aided by face-to-face communication. A subtle, but powerful instrument in structuring the internal debates was the catalogue of questions and the firm, on the whole excellent leadership, displayed by the moderators. They naturally had a vested interest in guiding their group to produce a systematic and detailed report which would cover all questions. Apart from that they could be tolerant of dissenting opinions which were already covered by the official mandate. Two expert groups finished by producing two separate reports each and in others some experts insisted on recording their dissenting view. Although we cannot know what the outcome of a non-contradictory procedure might have been, I can again briefly summarize the observed results of what has happened:

- Most experts agreed that they were not primarily engaged in a scientific controversy. 'Real scientific' issues were thought to be either completely lacking, or playing a subordinate role. Rather, the controversy, in the opinion of the experts, included many 'political' and 'ideological' elements whose proper assessment and relation to scientific and technical knowledge was itself the subject of much of the debate.

- The experts differed considerably in their conceptions of science and what they held to be the proper boundaries between scientific activities and non-scientific ones. One group of experts adhered to a positivistic conception of science with a strict hierarchical definition of the different bodies of knowledge and expertise. Another group made allowance for negotiations of consensus, assuming that there is always room for different interpretation of findings and to some degree for goal-directed research. Yet another group advocated an extension of the traditional (and narrow) boundaries of science, in order

to include social and political concerns as legitimate conceptual and methodological objects. For them, the positivistic ideal seemed out-of-date, while they exhibited the greatest understanding for the social dimension of scientific activity.

- On an epistemological level, experts were divided between those who remained faithful to a linear conception underlying extrapolations, risk assessment models and the art of making predictions, thereby emphasizing essentially a model of continuity. This contrasts sharply with those who argued for the possibility of discontinuous processes, allowing therefore for sudden breaks in hitherto continuous developments. Such breaks were expected to be catastrophic in the sense of opening up qualitatively new dimensions of developments and their consequences.

- On the social level, the conflict was about the role of experts and WHO was to be considered an expert. The expressed disagreements were partly derived from the different conceptions of science and its boundaries, e.g. experts adhering to a hierarchical-positivistic concept of science would only regard as expert someone whose expertise fell into a narrowly defined area of knowledge. Although the conflict about the status of experts was minimized by the selection procedure, it persisted nevertheless to some extent.

On the whole, the contradictory procedure achieved one of its stated objectives, namely to narrow the areas of substantive and methodological disagreement. The remaining differences were neither negotiable, nor could they precisely be located, since 'genuine scientific disagreements' played no or only a subordinate part altogether. The experts argued AS IF the underlying issues that still divided them could be solved by rational debate or would eventually find a solution by means of expert assessment.

None drew the more obvious conclusion, namely that the conflict could not be solved by experts, since they were only engaged in the production of similar arguments as they abounded in the normative-political arena, suggesting that they better withdraw. This points to an underlying dilemma of the experts' involvement in such debates in general leading them to collectively overestimating their own contributions and their ability to solve problems. Their vested interests as experts and their a priori agreement to play the role allotted to them by the political actors who will decide in the end, leads them to meet the political demand for performing certain services as experts which they could not honestly meet when adhering to strict principles of scientific practice.

It is the public role of an expert, the expectations put into their services and contributions which induce one part of them to overstep the boundaries between 'scientific' opinion and normative judgement that they would otherwise abhor, and the other part to consciously engage in normative activities in the name of their convictions, and utilizing the scientific paraphernalia which are necessary insignia of experts in our society. What proponents, uttering highly emotional horror, reject as the opponents' open partiality and their apparent cynicism about the 'scientific' basis of their judgements, appears to the opponents as an act of mere honesty, which the proponents in their institutional blindness cannot profess. On the social level, therefore, the conflict between proponents and opponents centers on the different conceptions they hold about the social and scientific responsibility of experts, while both sides are caught in the experts' dilemma: to act AS IF they were doing science.

Given the present interest formation of an alliance between nuclear industry, governments, political parties and trade unions on one side and a loosely organized,heterogenous opposition movement without any real power basis on the other, the value of expertise is quickly transformed into the question of 'expertise - FOR WHOM?' While these experts who adhere to the positivistic ideal of strict political neutrality of the scientist may not realize the extent to which they too are engaged in a political process, the critical experts are usually well aware of their open advocacy role and their political involvement, understood in a non-conventional sense of party politics. Seen once more from a long-term and broader perspective, it appears that the typical 19th century controversy within science whose cognitive object was a scientific theory or a method, has given way to controversies between experts, focusing on WHOM they wish to advise and support with their expertise. Underlying this change is a broadening of the production of scientific knowledge and expertise: it no longer suffices to produce knowledge which is addressed to one's colleagues and to be accumulated collectively, but rather expertise is to be put at the disposal of various social and political groups which will and may use it for their own ends. This universalization of expertise implies that expertise for the governmental-industrial establishment will increasingly be countered by expertise at the disposal of non-established groups in opposition to dominant policies.

A final comment concerns another dilemma which is inherent in the relations of any minority with a majority. The actual handling of conflict within the expert groups and in front of hostile audiences achieved its best results in those cases where true parity of pro- and contra experts was achieved AND their conflicting views were handled both competently and openly. Since critical experts are in short supply, this optimal model of conflict handling is confronted with serious limitations when an attempt is made to implement it on a wider scale. The competence

demanded of critical experts is context-bound and defined in a narrow field of a disciplinary speciality. In the old-fashioned terms of European aristocracy which resorted to duels as a means of handling conflicts, the opponents had to be of equal status, i.e. they had to be 'qualified to give satisfaction'. Since the actual recruitment of critical experts who can meet this criterium in the different contexts where it is required cannot be achieved without changing the present minority-majority relations, the optimal model is not applicable on a larger scale.

5. Experts and the public: towards the institutionalization of dissent

There can be no doubt that present relations are primarily characterized by a loss of credibility. In my study, experts were deeply aware of this fact, but not all of them deplored it. The critical experts, but also others, agreed that the previous image of an infallible and highly idealized science in the public view was out of tune with reality and in need of correction. Furthermore, it relieved scientists of acting as though they could never commit any error. The hope was expressed that more modesty on the part of experts and also greater collective honesty about the extent to which things were either not known or assessments rested on shaky grounds,would help to remedy a situation full of strains. From the preceding section it should also be obvious however, that the structural dilemmas reach deeper than individual commitments can hope to remedy.

If we inquire on a more general level into the cause for the loss of credibility of scientific expertise and the services experts perform for enlightening public policies, the political dimension looms large. Nuclear power is but one, although highly charged example, for the increasingly undisguised political use of scientific expertise and the ongoing resulting politization of certain sectors of the institution of science. In the Austrian case, the nuclear power plant which was the object of the whole exercise was nearing completion, when experts and the public were supposedly to be informed and guided in making up their minds. While the post-hoc legitimation of political decisions with the help of experts is no longer an exception and can, to some extent, be countered by political counter-maneuvers, other developments are more serious.

It is an open secret that it becomes more and more difficult to find a sufficient number of experts who are competent in a given specialized area and who can truly claim to be 'independent'. As knowledge and craft skills are produced inside scientific-technical work-environments like a given branch of industry or inside specialized governmental agencies, the probabilities that outsiders can be found who possess the necessary competence and have access to the detailed information which alone will enable them to assess a situation correctly, approaches almost

zero. This leaves those experts in command who work under the constraints imposed upon them by their employer's overall goals and makes it both difficult and unlikely for them to evolve a more detached and critical stance. But the covert politicization of expertise extends not only to create a scarcity of competent and informed scientific-technical personnel outside industry and governmental agencies, but reaches into the conceptual and methodological armory with which any assessment procedure needs to be carried out. The example of risk assessment is a good case in point. Developed primarily by professionals working inside industry and governmental agencies, the assessment methods used to calculate possible reactor failures had to be based on hypothetical reliability models in the absence of real world data, and often were little else but the 'revealed preferences' of the analyst. In a recent paper A. Mazur arrives at the following conclusion with which I fully agree (6):

> "These assessments have been increasingly based on hypothetical models which are supposed to make up for the lack of real experience. The numbers produced from these models are sensitive to the assumptions made by the analyst, and the results invariably reflect his biases. Risk assessment no longer determines policy here. Instead, one's policy preference determines the outcome of the assessment."

One has only to realize the central part played by risk assessment as a method in the public controversy about nuclear power and the disputed results at odds with each other, to fully understand the extent of harm caused by an immature and conceptually vacuous methodology claiming to be a 'guiding instrument for public policy'. Backed by the proponents of nuclear power who were intent to sell the methods and its results as 'indisputable scientific' to a public that initially had little to counter with, the mechanisms for some sort of quality control or standards of scientific integrity were clearly absent. Yet, faced with a small army of busy professionals calculating ever new hypothetical models and in the absence of real world data, what could critical experts do but dispute methods and results as outsiders and what else could the public opposition be expected to do but adopt the same strategy, and change the results?

Most experts feel, however reluctantly, that they cannot abstain from participating in the debate. Whatever the relative failure of certain assessment methods, whatever the immaturity of a field or lack of data which would permit a return to old-fashioned experimental methods - in transscientific debates nothing better is in sight. And while experts in these debates are led 'to often claim to know more than science justifies, that his opponent knows less than science justifies' (7), there are also concerns that upon withdrawal of 'serious' experts, a sort of Gresham's law of expertise would manifest itself. They are

drawn into the public arena through the value attached to expertise as a political resource and by their own motivation to prevent worse experts to take their place. They are caught by their status and have to act AS IF they knew and AS IF the issues under discussion could be solved by means of scientific reasoning. Yet, scientific reasoning and the development of higher standards and some quality control applied to assessment methods is prevented partly by experts colluding in the game of scientific trappings, in which they are arguing on a priori normative grounds. The experts have reason to worry, but not so much about the decline of the public belief in science or the status and prestige accorded to its practitioners, but about the structural deficiencies, originating in the politicization of expertise, which prevents the development and maturation of the conceptual and methodological instruments which are necessary to address the crucial issues. Few experts realize that the intersection of scientific-technological developments with the political and economic processes which steer them are increasingly supplanting the 'real scientific issues' which they have learned to solve. With the exception of the critical experts, they are genuinely at a loss, as to how this newly arisen problem-class can and should be approached with its typical mixture of political and technical elements, scientifically derived but full of practical consequences, and defying any neat separation in the pursuit of an ideal of scientific purity (8). Thus, they fall back on claims which in the context of ordinary scientific probity they would hardly wish to maintain.

The critical experts have moved one step further. They are aware of their situation, which places them into a promising, but still precarious new function. While they wish to fulfill the role of the public's advocate, they are bound by their own status as experts and have to remain within the universe of discourse set by their opponents. They are sympathizers, but non-combatants of the public opposition movement. They seem to have little to lose and something to gain when arguing for an extended role of experts and an opening up of the boundaries of science as institution to social demands and some degree of public transparency and control. They are right insofar as they are the contenders challenging the establishment and as the institutionalization of dissent, not just for a single issue, but on a more permanent basis, seems on its way.

The public, in opposition to what it perceives as threats stemming from technological and scientific developments and demanding a greater share of control, has no choice but to adopt similar tactics as the proponents in governmental and industrial circles. In realizing the political value of scientific expertise it unwittingly reinforces the above trends and its own expert-dependence, while being able to choose their own experts now. The illusion of a uniquely benevolent and infallible science being shattered, the way seems more open to develop a new kind of relationship. However, the present stage of expert-public relations

can hardly be expected to change for the better by searching for new formulas which would work miracles better left to public relations firms. It is important to realize that the degree of public participation and the form it takes, is strongly determined by the overall political context and allowance made therein for democratic procedures which are not only formally democratic. The lesson offered by the nuclear power debate in this respect is simply that vast areas of modern life - technology development and the research process - have virtually been inaccessible and closed to public scrutiny and a modicum of control. Science as a form of cultural domination, with technology as its material incarnation and in alliance with the powers of economic and political domination, has yet to come to terms with these demands for democratization which sound utterly alien to any elite, including the scientific.

The institutionalization of dissent would represent one avenue for building a new kind of relationship between the public and its demands and the world of experts who in their majority are at a loss as to how to respond to them. Without overestimating the critical experts' role, seriously limited and caught in their own dilemma,it may be facilitated by the realization that the lofty ideal of science being able to provide the one and only correct answer to a given problem, was itself the product of a historically unique constellation of cognitive and social factors. In a plurality of social and political contexts several possible answers, and several alternative solutions to one problem, equally grounded in what we still will call scientific activity, have become a fact. While we may not like this pluralism, which threatens to undermine so many cherished beliefs about science, we may have to learn to live with it. As always, there are better and worse ways to accomplish this.

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2. Michael Pollak and Dorothy Nelkin, "The Nuclear Establishment and its Ideology", paper presented at a Round Table Conference on the Social Aspects of the Peaceful Use of Nuclear Energy at the European Coordination Centre for Research and Documentation in Social Sciences, Vienna, November 1978.

3. In a yet unpublished paper "The Impact of the Nuclear Controversy on Decision-Making Structures", several collaborators and I have attempted to see what impact, if any, the public controversy on nuclear power had on energy models, on research policy related to alternative energy forms and on the political system. While we found some adaption on a fairly superficial level, we had to conclude that nothing much has changed.

4. The Austrian debate on nuclear energy has been fully described and analysed in a forthcoming book, Helga Nowotny, "Kernenergie - Gefahr oder Notwendigkeit", Frankfurt: Suhrkamp 1979; for a short English description see Helmut Hirsch & Helga Nowotny, "Information and Opposition in Austrian Nuclear Energy Policy," Minerva, Vol. XV, no. 3-4, 1977,pp. 316-334.

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IMPACTS OF THE NUCLEAR DEBATE ON SAFETY EXPERTS AND SAFETY ENGINEERING

Per Ragnarson, Institute of Technology, University of Lund

In the present paper, survey interviews with 125 Swedish nuclear safety engineers are summarized and commented upon.

Most of these engineers have 10-20 years' experience in nuclear research and development.

The interviews are part of a study that is both retrospective and explorative. The aim is to find out to what extent a public debate can bring about changes in an industrially established technology by influencing the attitudes and technical judgements of the individuals and/or organizations involved.

The survey was carried out during the spring and autumn of 1978 in a situation when the otherwise very intense nuclear debate had cooled down slightly and temporarily - a fact that was considered as beneficial to the purposes of the study.

A year after the interviews started, a serious reactor breakdown occurred on March 20, 1979 near Harrisburg, Pennsylvania. The reactions to the news and to the fairly detailed information available to the experts were surveyed using the same group of engineers about a month after the accident.

1. Background

The nuclear debate originates from two debates which were linked together in the US some 10 years ago; a professional debate on major issues of safety and a public debate, focussing on accidents of radioactivity releases and releases from normal operation. The public debate has broadened in scope during the 1970's, as it spread over the industrialized western world, thus including non-technological aspects as well.

The development of nuclear power started in Sweden in the late 40's, and it ended up in a commercial programme which ran for nearly five years practically without being exposed to a public technology assessment or to any serious questioning.

Nuclear technology became commercial in the US in 1964, as far as reactor engineering is concerned. Starting with the Oyster Creek plant, the power industry was quick to order 26 nuclear power stations before the end of 1966. At that time, the Swedish Oskarshamn was ordered by private and municipal utilities, and the State Power Board announced a comprehensive nuclear power plant programme.

At least two reactors went into operation before the nuclear debate became public in Sweden. The debate started very much through action taken by the Nobel Prize Laureate in Physics, Professor Hannes Alfvén who became a focal point as a "visible scientist" for the mass media.

The engineers interviewed in this study are inclined to separate the Swedish nuclear debate from the American one, seeing the domestic one as less professional.

From 1971, however, the debate has been increasing in breadth and depth, centering mainly on the issues already debated in the US.

For the first time in Swedish history "a technological issue" appeared to be more than purely technological; thus creating what turned out to be one of the most important and intricate political problems of the 1970's.

Public opinion polls indicate an increasing level of resistance against nuclear power around 54%. At the time of the interviews, the polls spoke slightly in favour of nuclear power and the mass media quoted the Swedish Energy Commission stressing environmental problems related to oil and coal.

Thus, the safety engineers have experienced a development programme sponsored by the state conceived in political unity and with vast resources involved. They have witnessed the American nuclear controversy initiated in the late 1960's and have experienced a Swedish version strong enough to contribute to the fall of a 44 year-old period of Social-Democratic government.

In cases where technical changes are related to the nuclear debate, the engineers see them as originating in the US. This goes also for the development of norms, guide-lines and direct remedies to certain problems. Only few fundamentally important improvements in reactor design and operation have followed as a result of public debate, according to the engineers.

The nuclear debate has spotlighted a number of scientific and technical problems which are characteristic of nuclear energy and have, at least in principle, been well-known since the 1940's and 1950's. The main idea is to control large amounts of energy, necessarily involving some accidental risks. Radiation is present, radioactive waste is being produced, and - not least - nuclear weapons proliferation was the subject of a warning given to President Truman already before the Hiroshima/ Nagasaki bombs. Such problems have regularly been the topic of international and national conferences, often open to press and political observers, although these not always attended. Sabotage and terrorist activities in their most dramatic forms should be considered as new problems of the 1970's. Divergent opinions prevail about the possibilities for such activitists to cause dangerous damage. In this investigation special attention is paid to the following safety and environmental issues which dominated the international nuclear debate from the mid 60's:

1. Radiation risks at normal operation of nuclear power plants;
2. Accident risks in nuclear power plants with the focus on loss of coolant and a conceivable melt-down of the reactor core whereby radioactive gas could be released to the environment;
3. Increased danger of nuclear weapons proliferation, especially in connection with the use of plutonium as a reactor fuel, but also as a result of a general extension of know-how from various parts of nuclear technology;
4. Increased danger of terrorist attacks, sabotage and theft of fissile material;
5. Safe disposal of radioactive waste.

With the exception of point 4, there have been open international professional debates, partly in publications and partly at conferences, during the 50's and 60's. Normally (and naturally?) these debates have ended up in rather optimistic conclusions.

2. The experts

There is a certain consensus of opinion among nuclear safety experts concerning the work that has been done during the last 30 years. They are not likely to abandon it, although they have been subjected to criticism, attacks and, finally, a total calling into question not only of their technical merits but also of their moral and ethical qualities. In some cases they claim that they have suffered "political homelessness", at least on occasions when no political party seemed willing to accept their arguments. Their very existence has also been threatened. The debate has caused some "social effects" in their private lives.

Referring to "unsurpassed industrial safety", with no fatal accidents in commercial operation of nuclear power stations, these experts refuse to acknowledge the technological endeavours of decades as being a total failure. They also demand some kind of continued development and utilization of nuclear technology. Only labour market problems could force them to accept some kind of "mothproof conserved technology", i.e. reactors without operation licenses.

Career expectations loom large in the background. This is perhaps not unique, but their expectations are in some sense related to the fact that quality serves as a pre-requisite for safety. The psychological situation is not altogether sound after a few years of political obscurity and increasing anti-nuclear opinions.

The nuclear experts are part of a very large international structure, a "nuclear society" possessing a joint economic and political influence. It has hardly been a daily topic of conversation that such a structure could be seen as a democratic problem. The group of safety engineers in this study deny such an importance, referring to how colleagues, in trying to counterattack anti-nuclear arguments, often have been refused access to mass media.

It follows from what has been said that I did not expect the group to show a positive attitude towards the nuclear debate as such. Since such a group has not been the subject of a study before, I had to rely on my own experience from regular contacts with many of them since the early 1950's. Any deviation from the following hypothesis is noteworthy:

> The scientists and engineers recruited during the 1950's and 1960's to develop nuclear technology often came straight from departments of technology or universities. By training and/or experience they belonged to a technical corps d'élite, faced with public, or at least political expectations expressed in terms of fund appropriations so far only given to military projects.
>
> They were faced with grave technical problems, challenging their creativity. A supreme degree of competence was demanded in very narrow specialist fields. Many difficult problems were resolved during industrious years, and the commercialization of nuclear energy in the mid-1960's was interpreted as a confirmation of technical success internationally speaking. It had been possible to satisfy political expectations of an economic breakthrough which - at the time - was considered as a way to save natural resources, restrict the great dependence on oil and ensure energy supplies for the foreseeable future.

> Under such circumstances there is no obvious reason to expect a group of engineers to accept a total revaluation based upon criteria some of which they find neither scientific nor technical. On the contrary they can rather be accredited with loyalty to the mission as well as to the professional group at large. They will safeguard competence in a technology they consider themselves able to master.
>
> Added to this - such qualifications have been highly valued and rendered social status (at least until 1972); the advantages of an interesting job in an encouraged and stimulating surrounding should not be forgotten.
>
> The engineers are reluctant to acknowledge any other competence but the professional. Neither are they willing to appear externally in a manner that could cast doubt on "internal" loyalties - nor, of course, be prepared to give up the tenure of their employment.

Not unexpectedly, an obvious preference for a scientific/technical, specialized debate in well reputed forms is visualized. The lack of practice in being the subject of a public debate contributes to a readiness to criticize the framework of the debate. The level is considered low-brow; it is "not founded on facts" and arises from "insufficient, if any, analyses".

Thus technical judgements and decisions are not likely to be influenced by the public debate, unless criticism or suggestions are of some "expert origin" or, in terms of political pressure as a result of a forceful opinion.

Nuclear technology is not entirely a new notion. It is built up by know-how and tradition from several other technical fields. Engineers from different branches co-operate within different companies and organizations. Nuclear safety is applied internally and externally. The aim is to minimize risks during normal operation as well as consequences in the event of an accident. In the choice of the group, "nuclear safety engineering" has been defined in the following way: All efforts within research, technology and administration aimed at retaining - within all stages of the fuel cycle - radioactive substances and ionizing radiation inside therefore intended barriers and below the permissible levels with regard to the protection of workers and the environment. The group is selected from three power utilities and two companies dealing with research, development and supply of nuclear equipment, as well as deliveries of complete nuclear power stations. Within their companies these engineers are in positions from which they may influence general considerations or technical solutions, in many cases make the decisions themselves.

They work on three levels, i.e. as technical directors, chief engineers etc, previously often spokesmen for their organizations - heading departments with supervisory responsibilities and an increasing external commitment during later years - specialists on rather narrow problems with less experience of external relations.

The interviews - generally taking at least 1 1/2 hours - are as unique as every individual, since nuclear safety is not a simple notion. In order to realize at least some uniformity in the material a questionnaire containing 28 questions has been used. It is divided into 1) presentation of the person interviewed, 2) technical assessments, 3) comments on arguments in the debate and 4) instruments and sources of information frequently used by nuclear engineers. Finally they were given the opportunity to comment upon personal relations in their private lives.

3. Experts exposed to a public debate

The experts have been exposed to the public debate in several ways. Historically, they followed the professional debate through special publications and conference reports. This was also how they met the professional and semi-professional debates which started in the US in the late 60's involving the Union of Concerned Scientists and similar groups. So far there were opportunities for a professional participation in these debates. Later, however, when the debate was taken up by the public mass media, a majority of the experts have followed its development passively, and, with some delay and distortion, through their specialized media.

Before 1975, less than 10 out of 125 in the group had tried to participate in the public debate, in one way or another. Most of these are supervisors, supporting the policy makers. Less than five per cent of the group, i.e. five or six persons, have tried to follow up the debate in serious intra-professional media as for example "Science" and "Nature". The influence of social contacts became important during the later phase of the debate, perhaps indicating that it had really become public.

Many of the questions in the interviews give rise to spontaneous comments concerning mass media. Almost without exceptions, a lack of confidence is expressed. Mass media have "made their decision" on nuclear energy. Facts are often "inadequate", "deficient" or "deliberately misinterpreted". Official material in the shape of writings to authorities are taken up and presented in such a manner that a number of those interviewed have considered it necessary to frame their wording more cautiously:

> "The debate has influenced my technical decisions. The influence is negative inasmuch as it contributes to less plain language and also prevents "calling a spade, a spade".

> Within the safety committee we are very open towards each other, and this is noticeable when you read previous minutes, because then we never hesitated to note that there had been a leakage if something had gone awry, and how much. This we do not do any longer."

Comments like these are more or less evidently said with regret - one does not like to be "interpreted" in the papers by people who are not sufficiently competent to assess adequately the importance of the question involved from a safety point of view. Neither is it pleasant to have to express oneself more cautiously, even if the authorities are under no illusion what it is all about. The reference to mass media is often formulated on the following lines:

> "You become doubtful face to face with mass media, when you see how the information has turned out. What will happen in areas where you have no insight?"

Mass media reports on problems in nuclear installations are checked, and - if possible - "corrected" through international telex links, including some professional media.

It is not evident that the dominating professional media have given a fair picture of the criticism and the critics. This concerns an international, specialized press (American, British and West-German), shaper of "expert opinion" on a specialized level. It has not been investigated, but it is well worth a study (the author's opinion being that this press has been very reluctant to admit the strength of the criticism and the increasing anti-nuclear movement). A national, specialized press does not exist in Sweden (company papers excluded). The leading technological journal in Sweden apparently avoided to admit that a certain technology was seriously questioned. Periodicals like "Science" are not regularly analyzed - not even glanced through - in order to study their (at least less sensational) approaches to the nuclear problems.

An important source of professional information is the international (and national) conference. The group is united in the opinion that they have less time for attending conferences which survey the general status of the art. Historically seen, such conferences have been very important in national technical policymaking. The specialized engineers, however, prefer specialized conferences, symposia or working groups. This may sound paradoxical because at the same time there is mention of an increased professional breadth necessitated by the debate. Information from colleagues and the rapid information service from various companies here play an important role.

It should not be forgotten that nuclear safety experts are also private individuals - they have families (nuclear energy is entirely male-dominated), they have a social life, cultural interests, etc. Obviously their private sphere has not been particularly affected by the nuclear debate, although problems are mentioned in some cases. It is often mentioned, however, that issues brought about by the mass media are reflected in social life. This direct confrontation with laymen - including relatives and friends - seems to have forced specialized experts to broaden their competence, seeking answers to questions about the entire nuclear system.

4. Impacts of the public debate

It took some time before the experts found something new in the public debate. Thus, much of the direct reaction to the mass media was irritation and a defensive attitude: "We have already thought about that."

The engineers question whether a multiplicity of safety remedies and more sophisticated equipment really mean improved safety. They tend to claim that the safety authorities are too eager to listen to the anti-nuclear critics. They also find that the norms and guide-lines which were established after public hearings (US hearing on Emergency Core Cooling) are very difficult to change in the light of new experience.

The only criticism that they acknowledge as correct and timely is that by the American professor Dean Abrahamson and others on radioactivity releases during normal operations. This debate has caused power utilities to install equipment in accordance with the principle "better to forestall than to be forestalled".

A list of examples where the nuclear engineers admit or even claim influence from the public debate can be given. Above everything else, they feel that the nuclear industry has been given entirely new orders from society concerning the approaches to the waste problems. The principle of developing a commercial "waste service" later on, when commercially interesting volumes are available, has been rejected.

This principle has come in for bitter comments addressed to the management level: "Interest decreased with the distance from the reactor building". Reactor engineering had high priority, particularly in the US, with commercial nuclear power operation and export as the immediate goals. Even in this case, Sweden followed US policy, and the revaluation of the waste problem was perforce deferred until the arrival of a new government with a Prime Minister whose views were anti-nuclear. The immediate result was a new Bill for fuel cycle safety. On the international level, experts view the INFCE-study - International Fuel Cycle Evaluation - as a result of the nuclear debate and its impacts on

politics. This study reflects the concern about the "plutonium economy" and nuclear weapons expressed by many people, including President Carter. This, in fact, is the point where the engineers share the opinion that the debate has highlighted a real problem, although failing to acknowledge it as a technical one. There are some acid comments on the fact that new enrichment techniques have hardly been dealt with in the debate.

To sum up in brief, the group indicates that the public criticism and debate has had its most intriguing effect on nuclear power technology by creating a "climate" of alerted interest and improved knowledge on the part of the public. It has compelled voluntary initiatives to be taken, and entirely new priorities to be drawn up by the authorities and in political resolutions. Separately, two radiation protection engineers acknowledge this effect:

> "It is a good thing that public opinion has been used to reach the present level of safety".
>
> "Public opinion is often referred to".

On the individual level, the experts share one clear opinion: The debate has forced them to broaden their technical competence - they do not regret that they have had to devote much time to information and education in trying to cover the whole system, i.e. the nuclear fuel cycle both on the national and international levels. Perhaps this broad technical scope is the most thoroughly positive effect of the nuclear debate. This is the opinion held even by those otherwise negative to the debate.

Although there are different attitudes to the debate as such, and divergence is obvious regarding the origin of certain measures taken during recent years, there are some good examples of how the existence of a public debate enforces improvements. The early criticism in the US concerning releases of radioactivity during normal operation is, as mentioned before, accepted without reservation. Later it provided arguments for the installation of certain delay equipment in order to minimize the releases. It led to improvements in fuel element technology so as to keep radioactive products within the first barrier, i.e. the fuel canning. One of the safety engineers describes this as follows:

> "We are favoured today by the fact that so far no fuel has collapsed. They have been able to produce retainable fuel. For this they ought to Thank the Lord every day. The day it collapses, they will not manage 5 rem per annum (for individual workers). I am afraid there will be stricter claims on lower doses. I do not know what will happen; I cannot assume the plants will function without fuel canning damage."

The debate created demands and still stricter demands, and the engineers had to respond with higher quality in canning tightness, and so on. Some feel that the critical "climate" is changing into a pressure that could kill the technology under an excess of expensive quality and safety equipment.

A common opinion is that there is often an indirect impact which is rather difficult to evaluate. To this end, more detailed technical studies including documentation of decisions, minutes from safety committee meetings, reports to the authorities etc, are necessary. This has not immediately been within the scope of the study.

Naturally, some comparisons between performances of safety authorities in the US and in Sweden are being made. Some engineers, who consider themselves able to make comments on this, refer to the fact that Sweden can benefit from a "continuous dialogue and a collaboration relation" between authorities and the nuclear industry. In the US, on the other hand, they find a tendency to try to avoid contact with federal authorities. Sometimes this involves as grotesque tokens as to refrain from improvements, with the motivation that the licensing procedure is expensive and time-consuming. Supervision of compact formal standards brings about an administration, perceived as "red-tape" by those closely involved. A reactor physicist expresses himself thus:

> "The emergency core cooling evaluation has become so fantastically formalised that estimates no longer are technical calculations but rather interpretations of statute-laws - which are time-consuming. Those engineers working with it, they had been of much more use, had they busied themselves as engineers and not as interpreters of statutory law. I'm rather critical".

Criticism aimed at a "standard-formalism" is predominant among the personnel directly involved with safety work concerning the actual reactor core - yet there are also some positive assessments within the same group.

There is a strong reaction against being held responsible for possible consequential occurrences which conceivably border on improbability, or are considered quite impossible:

> "I do not think that anyone understands what it means to work with a probability of 1:10 000 000.
>
> - - -
>
> You must put it in relation to something - a plane crash, a meteor alighting in a big city, etc, because there is nobody in everyday life constantly afraid of being hit on the head by an aeroplane or a meteor; that a tanker will turn over in the neighbourhood, or that potassium cyanide is put in the drinking water".

Without paying attention to psychological effects or possibly different opinions among the public about accidents, some experts are predicting a reactor breakdown, but they all find it difficult to believe that it can become a catastrophe:

> "A reactor accident surely will come - but we are not going to have the first one!"

This kind of statements is heard from people with a supervisory position and with a considerable access to information.

There is no divergence about the American Rasmussen-report on nuclear safety. This fact confirms a previous observation that "positive" arguments are very easily accepted. Thus the entire group was inclined to view the Rasmussen-report as the final proof of nuclear safety.

The interviews were made before a study was published by the US Nuclear Regulatory Committee (NUREG/CR-0400) recommending a more cautious use of the Rasmussen methods in evaluating technical safety.

A reactor engineer describes his own feelings of working with improbabilities thus:

> "You are forced to investigate a course which is unbelievably improbable. You have no previous reference in your own experience or in the technology under issue. You loose your balance in life when you have to deal with inconceivably low probabilities. There is something abstract about the entire problem. It is not possible to differentiate between problems of first and second degree. Everything you question may happen. Therefore, you cannot assess if it is a probability, or which degree of probability it concerns ... You never see the result of your work, because you cannot prove it; you can never experimentally verify until an actual reactor accident has happened".

Some of the engineers not only foresaw an accident, they also thought that it must happen so that theory and realities could be compared. This also reflects the opinion that a catastrophe (in deaths, injured and land destroyed) is "nearly impossible".

It is noteworthy that experts, who themselves consider their own technology to be "abstract", at least in some sense, also criticize their own efforts in public information!

The comprehensive Rasmussen study in the US started in connection with the emergency core cooling debate. So far it is an example of results achieved when the debate has been inspired by scientifically and technically trained critics. This is an important conclusion from my study: competent critics must become involved in a public debate on science and technology in order to make it work as a "technology assessment" and to enforce improvements, or possibly the rejection of the entire technology. Nuclear experts accept "technology assessment" as such, but they hesitate when the possible ultimate consequence of rejection is mentioned. They also criticize norms and guide-lines coming out of a public assessment, like it happened after the emergency core cooling hearing in the US in the early 70's. A safety engineer with a very central position made this statement:

> "I believe you can say that the emergency core cooling debate is an example of how you get stuck on a point and probe enormously deeply in detail on a particular section which really is not all that difficult; yet you go on long after the debate has resulted in such a point. Of course, a great deal of what you did were things that required attention anyway. So it was a calculating programme we had access to, which was far better than the one we had developed ourselves almost about the same time, but independently of this".

This sceptical attitude is dominant. Coming from an engineer with long experience and an unusually broad perspective, this understanding of the origin of the nuclear debate is rather unique in the group:

> "Certainly we have been influenced by the reactor emergency core cooling debate and changed standards. Connections are obvious. The issue exploded in the US, which is quite understandable, through alert persons. They were startled, and took action when the power industry suddenly ordered heaps of reactors. It is odd that it is so the system has functioned; by not having a central authority to balance this. On the whole, I find it far-sighted by those who said at the time that it is perhaps possible that nuclear energy is beneficial, but you do not know to what extent. It was really neatly done."

Although this statement is at the positive extreme, there are many indications that the debate concerning accidents and long term radiation risks have compelled the engineers to a certain awareness regarding values in society, which cannot directly be expressed in figures or overcome by some kind of technical logic. On the other hand, it can be maintained that only a few of the 125 nuclear engineers are prepared to accept such criticism if it is not aimed at other occurrences in civil life as well. In this argument, they feel, there is little or no support.

As mentioned earlier, the exchange of viewpoints with people outside the professional sphere has contributed to the social and psychological impacts, particularly in a situation when the attitude to mass media is totally negative.

One problem in private life is related to the "insecurity in employment". Another is the fact that the engineers come home from work with problems of a pronounced psychological nature. A general manager indicates that he has to devote more and more time to some of the employees in need of moral support to be able to cope with the job. Some express themselves in a manner perhaps more significant as regards the most pronounced dissatisfaction with the situation:

"It is so damned annoying not to be taken seriously!"

There is a small group which tries not to reveal that they are nuclear energy engineers. Most of them are moderately amused when they have to answer questions in their social life, like medical men, but being more in a tight corner.

It seems fair, however, to conclude that a majority of the engineers do not entirely dislike being in the spotlight of interest, provided nuclear energy is not totally questioned:

"It is good for you to live under some pressure".

A small group even enjoys discussions and they keep trying to convince people - man to man - that nuclear energy problems are not so great that they cannot be solved and above all "this new source of energy is needed".

After 1975 there is a remarkable change in Sweden: as a result of an initiative among the personnel of ASEA-ATOM "action-groups" have been formed to participate in the public debate, defending nuclear technology. This is a result of a criticism against the managements of nuclear power companies (some of which are public and even state owned enterprises) for having been "too careful" and often avoided counter-attacks upon anti-nuclear arguments. Those "action-groups" started their work with an educational programme for themselves in politics and mass media policies. Several other sides of society, which they felt that they missed during their technical education, were included. These groups are busy writing letters to editors, giving lectures in schools, attending public meetings etc. Some in the group have had, and still have personal problems of a psychological kind. They experience the situation as depressing, their working duties insecure and less meaningful. The transformation during the recent years from a genuine confidence to a crisis in confidence has caused bitterness in comments:

> "There is now a distrust which obviously has deprived us pleasure in our work. When the community turns its back on you, then you really do sense it. Your bad mood follows you home."

A sceptic who faced the consequences and left the nuclear energy industry quite recently, comments as follows:

> "Sometimes I am ashamed for having been engaged in a technology, so weakly standing on its own feet. Technical/economic failures are not good for your self-esteem."

This person was able to face consequences. He still gives a sound impression. But within the group, and probably also outside its circles, there are signs of internal criticism against superiors and disappointment because the debate has revealed that the nuclear technology is not uniformly strong. As specialized engineers they did not have a chance to assess the entire system, and the debate has exposed deficiencies.

> "My wife and children would prefer that I changed job."

Only a few, about 10, show a consistent "unaffected" attitude:

> "It has not affected me - it is like pouring water on a duck's back."

However, in these days after Harrisburg, I expect several important safety engineers to tire of waiting for new startups. Thus they might, by their own absence, create a new safety problem.

5. Conclusions

The impact of the public debate on nuclear safety engineering and on the engineers can be summarized in the following way.

Engineering

- new norms and guidelines
- additional equipment for monitoring
- improvements in building lay-outs in order to facilitate maintenance
- increased effort on quality assurance and quality control
- increased support to safety research and risk evaluations
- new priorities concerning radioactive waste disposal resulting in detailed studies and technical development
- less effort on alternative nuclear systems (for example breeders)
- concentration upon improvements and demonstration of safety within known areas of the light water reactor fuel cycle

Engineers

- intellectual impacts, particularly a changed problem awareness and increase in professional breadth
- social and psychological impacts: professional self-esteem, social esteem, economic security, work under public and political pressure

The group frequently refers to "indirect" influence of the public criticism. This would indicate that public awareness and criticism is good and necessary for technical development, compared to the relative isolation in which nuclear power was commercially introduced in our country.

Only two of the most frequent arguments against nuclear power are accepted as being entirely serious: nuclear weapons proliferation and some problems related to the ageing of nuclear power stations, i.e. more radioactive contamination demanding technical improvements (for example remote handling). Criticism is accepted concerning previous policies for waste handling and disposal, and the problems related to a "plutonium economy", particularly the status of the reprocessing technology. Here the international nuclear technology is blamed for having underestimated or misinterpreted the problems.

Care must be taken in drawing definite conclusions from a material of this kind. However, some observations can be summarized:

- the existence of a competent and thus in some sense powerful debate was denied as long as possible - some initial defensive reactions are still important;
- attention was drawn to lack of technical exactness in the criticism;
- general values among the lay public were not immediately understood and there is still a credibility gap between technologists and the public;
- in order to sustain a critical debate some professional knowledge must be involved, thus technical impacts from the debate are generally related to the American debate;
- most changes in nuclear safety techniques are seen as a result of indirect pressure in a climate of prevailing awareness;
- authorities are inclined to listen to public opinion - probably "too much" according to the group;
- mass media are strongly criticized, although an analysis is likely to show that only on occasions facts have been presented in a way that could be called a "campaign against nuclear power";

- the "internal" opinions among the engineers are to some extent drawn from specialized journals - only a few use to read general publications such as "Science" where the criticism has been balanced and not "built upon feelings";
- no single argument - probably with the exception of weapons proliferation - is accepted as powerful enough to stop nuclear power;
- about one fourth of the group say that they think that a public debate is good for any technology, but only a few of them are consistent, because they deny the effects of the debate, they dislike the mass media presentations in general, they criticize authorities for listening too much to the debate. Furthermore they say that they accept "technology assessment" but they do not accept the ultimate consequence that society could reject their own technology for reasons which are not purely technical;
- the group admits technical underestimations, particularly regarding waste, reprocessing and plutonium handling;
- there is internal criticism aimed at the management level, for the rapid, large scale introduction of nuclear technology, particularly since its quality has appeared to be uneven, leaving for example the waste problem behind.

Addendum: Harrisburg and the nuclear safety experts

A month after the Harrisburg accident the same group of engineers were used for a survey of reactions to the technical mishaps and the psychological and political impacts of a "near melt-down"-situation.

Although defensive reactions are obvious, more than a third of the group tends to judge the risk of serious accidents as being higher than a year before. At the same time, however, they mention the possibility that Harrisburg might improve safety, if properly handled.

As expected, they suggest that some particular reactors be checked but that no restrictions to reactor operation be forced upon other nuclear stations. Generally the group questions whether Harrisburg was an accident, since no people died or were injured. They prefer talking about a reactor breakdown or a "core damage", and they have some difficulties in accepting Harrisburg as a psychological and political catastrophe for the technology and nuclear industry as such. Again the mass media are blamed for a non-balanced presentation, particularly Swedish media. Although there are internal differences of opinion among engineers they are unanimous in considering nuclear energy necessary for the world's energy supply. They also believe in further improvements throughout the fuel cycle. They would prefer a more internal debate with critical

authorities, and they hold to attitudes implying that technical matters are not for the general public. Thus only a few accept the fact that Harrisburg finally made nuclear energy the subject matter of a public referendum in Sweden (to be held in March 1980).

General observations of expert loyalties indicate that similar problems are to be faced once a major new technology is questioned, as for example the recombinant DNA research or micro-computer technology. Lessons from the nuclear debate have to be learned through the training of new scientists and engineers. Harrisburg is not only a technical mishap - its impact, both political and technical, is likely to be stronger than the effect of ten years' debate.

THE RATIONALITY AND RITUAL OF NUCLEAR DECISION MAKING

Brian Wynne, School of Independent Studies, University of Lancaster

INTRODUCTION

The Windscale Public Inquiry began in June 1977 and ended exactly 100 days later. It was held to investigate the application by British Nuclear Fuels Ltd., (BNFL) to build a 1200 tonne per year thermal oxide reprocessing plant (THORP) for reprocessing spent nuclear fuels from Britain and overseas. Although ostensibly a factual investigation of a specific local plan, organised within the modest traditions of administration in practice the Inquiry was the key part of a major exercise in social conflict resolution. It was perhaps symbolic that it ended on the eve of bonfire night. A year earlier, when the frustrated public clamour to hold an inquiry was threatening to burst into flames, tens of thousands of demonstrators had stormed riot police at a nuclear site in Brokdorf, Germany. During the inquiry itself the following summer, similar confrontations at Creys-Malville in France, resulted in massive violence and the death of one demonstrator. In a few short years the nuclear issue had leapt beyond the capacity of traditional processes of decision making and investigation to contain it. It was invested by both sides with passions and symbolic associations rooted in the very basics of industrial society (though not in simple for and against terms). Thus the Inquiry was of more than a passing interest to the rest of the world, where the nuclear industry at large appeared to be squeezed in a tightening vice of conflict by the confrontation of mounting public protest with rapidly inflated estimations of its importance in the eyes of most political authorities, frightened as they were by the oil crisis of 1973, and the events in Iran of 1978.

The Windscale Inquiry was variously described as 'the Inquiry into the future of the world' (1); 'a total consequential impact analysis of an increasingly controversial and crucial part of the full nuclear fuel cycle' (2) and 'an international test case' (3). Senior personnel of the Windscale Inquiry privately issued self-congratulatory remarks contrasting the peaceful decorum of the Windscale war of words with the violent events in Germany and France.

Smelling the whiff of real battle not far away, the Guardian described the inquiry as "an epic and an example" (4) in decision making and conflict management to the rest of the world, and other governments notably Germany, looked to it as an example for their own decision making problems. The inquiry's importance was underlined by the appointment of no less than a High Court Judge - the ultimate symbol of impartiality - Mr. Justice Parker, as inspector. He described his inquiry as unique on two counts: firstly because of its unprecedented scope - 'the issues to be investigated may affect... those who live far away and who will not be born for many years ahead'; secondly because it was the first occasion in Parker's view (though here he betrayed his ignorance of e.g. American practices) that 'the arguments and evidence of both proponents and opponents on nuclear power issues will be tested by cross-examination in public' (5). He referred to the undisciplined rhetoric and incoherent public posturing which had hitherto passed for 'debate', and suggested that the rigours of formal public inquiry under judicial control offered a definitive alternative.

Perceiving something (though perhaps not enough) of the deeper political currents bearing down on his inquiry, Parker simply expanded the modest traditions of the local public inquiry in an attempt to meet and mitigate them. He listened, with more or less patience, to arguments ranging from pin pricks on Windscale workers' fingers, to the depradations and future of industrial society in general. Inevitably the inquiry was by no means the last word on the subject of nuclear energy, nor even on THORP in particular. Nevertheless in immediate effect it was successful. The nearest approach in Britain to the social eruptions in Europe was a decorous and relatively thinly attended march in London, after the Parker verdict had been published. It was not that the demonstration was totally without potential warrant, since there had been widespread condemnation of the inadequacies in Parker's analysis, indeed sweeping neglect of several of the broader issues which he had so patiently allowed to be put before him several months earlier. But the inquiry's simple public image of liberal vision and intense rigour was sufficient to cancel out this later more detailed and esoteric criticism of its narrow and inadequate analysis. This kind of contradiction inevitably raises the spectre of the inquiry as a mere ritual, allowing the force of opposition to spend itself in 100 days' fast and furious combat with the massed forces of the nuclear industry. To some extent this was true; on the other hand such public decisions must inevitably entail their ritual elements. Even their fact finding rationality can be regarded in an important sense as ritual which serves to channel and concentrate other less overt meanings invested in the overall process. The interactions and limits of such ritual are an important theme of this paper.

An issue related directly to the above is the extent to which decisions are decisions at all. Complex modern technology embodies an objectivity which often presents itself in the twin forms of inscrutable complexity and historical inevitability. To pin a decision down to spe-

cific acts, commitments, and casual chains is frequently far more frustrating and difficult than it seems at the outset. The political sphere is used to dealing with complex multidimensional interests and forces which make and develop issues (or suppress them). The key question often bears upon how to define, i.e. simplify to manageable dimensions,the issue in the first place. The usual treatment is to distil questions to their "essence", which means to simplify and in so doing, transform them. This often means delegating them to specialist institutions, with their own habitual rules and routines for fencing them off from the chaotic currents of politics at large, defining and deciding the case, then returning it to the political sphere in a form which points to its curtailment. The degree of insulation from the on-going political quagmire varies between different cases and different institutions. In the case of Windscale, the decision was delegated from the political sphere to a hybrid, judicial public inquiry arena.

The colossal political importance of the Windscale decision arose out of the accumulated paralysis of democratic decision making in this field over thirty years; the sense of anxiety within the UK nuclear industry that its very future was at stake with THORP; the profound problems surrounding energy supply and use in industrial society; and the background threats of social disruption if the decision were not handled carefully. There was thus tremendous political pressure to invest the decision process with elaborate forms which underlined the importance of the issue and the extremely serious approach being taken.

Yet ironically there was at the same time an impossible complexity, not only of technical facts and judgements of social and political futures beyond our understanding and control, but also of social and political values, choices and conflicts within our own sphere of 'control'. Thus in important ways the Windscale decision was strictly beyond our rational capability to deal with it, yet at the same time it was crucially important to society at large, to cultivate and establish the solid belief that we had indeed dealt with it comprehensively and objectively.

It is routinely accepted in social science, that such contradictions between experience and belief are bathed in rituals which serve to mitigate the potential social damage which would result from their public recognition. We are all familiar with the heavy ritual accompanying general elections, rituals which emphasise the important results attending our participation, and the supposedly important influence which we the electors can exercise; and which in so emphasising democratic power, background the embarassing realities of the elective autocracy which contradicts it. The ritual serves to plaster over a basic social anomaly, whose open recognition would threaten the social order.

The Nuer of Africa find twins a social anomaly because they would if acknowledged, wreck established patterns of social hierarchy. Rather than

allow their society to be undermined by quirks of nature, however, they lift the social anomaly out of the human realm into the sacred - twins are defined as special children of God. Paradoxically, in this cultural role they become the medium of many rituals which serve to underscore the very principles of social structure which they potentially threaten. In the jargon of anthropology,Turner has noted how:

> one often finds in human culture that structural contradictions, assymmetries,and anomalies are overlaid by layers of myth, ritual, and symbol, which stress the axiomatic value of key structural principles with regard to the very situations where these appear to be most inoperative. (6)

Perhaps we can draw upon these insights to suggest that the inadequacy of our decision making institutions, especially their emphasis upon the purely factual nature of the questions to be answered, in the face of a problem heavy with symbolic and social undercurrents, and with authentic complexity such as Windscale, often leads not to open recognition and articulation of those undercurrents, but instead to even more intense, ritualistic articulation of the purely objective, fact-finding nature of the issue in hand. The ritual elevates this to an inflated cultural esteem, in order to conceal its deeper incapacity to cope with such issues. 'Methinks he doth protest too much....'

In an attempt to pursue the theme of this conference, I shall in this paper outline some of the interesting problems in decision making thrown up by complex issues such as nuclear power, and examine in passing by how the interrelations between expertise and the public are affected by some characteristics of the technology itself, and by the shape of previous decision making structures. One might say that I shall focus upon the unanticipated consequences, not of technology, but of technological decision making processes.

The tacit dimensions of decision processes.

In my introduction, I have stressed that Windscale was a major conflict-resolution experiment. Yet in theory, the public inquiry is a highly confined, purely fact-finding exercise, on a local level. The more that the scientific aspect of such inquiries is stressed, the more their public image moves towards that of a consensual process, operating more by expert cooperation than by formalised strife. Indeed all Windscale's innovations from the usual local public inquiry - an actively investigative, High Court Judge, two eminent technical experts as assessors - lent it the style of a Commission of Investigation rather than an accusational legal setting. Yet these innovations should not be overstated - the dominant ethos of Windscale was adversary, and elaborately so. This is of

great significance because it was the first time that the long-standing tradition of closed consensus decision making in the UK nuclear field had been mitigated by any significant public scrutiny, couched in an explicitly adversarial form. It could perhaps be regarded as a belated recognition of the vastly greater legitimacy bestowed by the uncomprehending public upon a decison apparently reached by means of such robust open combat or public ordeal, as compared to one reached by obscure processes and people, behind closed doors. The Quality may be another matter, but the legitimation of a decision arguably requires an extravagant ritual combat of adversaries at some stage in its evolution. This is not the same as saying that the decision is made by such open adversary process.

On the other hand, one point I wish to stress is the importance of the hidden meanings buried in the decision process. There are at least two aspects to this:

Symbolic Role:

Firstly, there is the symbolism of the formal institution charged with the task of resolving the problem. If a decision is delegated to a scientific committee, that transmits the message to a broader public that the issue is only a scientific one. This is true even if, as they usually do, covert political factors intrude to "interpret" the scientific recommendations back into political decisions. Likewise a judicial inquiry, whatever its attempt at informality, transmits the tacit public message that the issue in hand is for fact finding experts to solve, as judges are taken to do. A combination - as at Windscale - of judge, expert scientific assessors, legal advocates and expert witnesses creates a powerful, dovetailing public message that only experts in 'objective discovery' are required to solve the problem, which is not one of social choice, but of objective discovery of a truth that supposedly exists somewhere, independent of 'mere' human choice, with YES or NO to THORP engraved upon it.

Ian Breach refers to a cartoon in the local Whitehaven News during the Inquiry, a cartoon which eloquently reflected a public sense of alienation from the proceedings:

> It depicted three pinstriped (suit) types standing outside the Civic Hall, scene of the hearings: their briefcases were labelled, respectively, Legal Expert, Environmental Expert, and Nuclear Expert. "Pity" one was saying to the others, "we'll miss Wimbledon..."(7)

Wimbledon with its self-indulgent exclusiveness, snobbery and strawberries with cream, was nevertheless probably nearer in the identification of ordinary people than was the Windscale inquiry. One can say that much of this alienation was encouraged not by underlying symbolisms, but by direct factors such as finance; the forbidding style of legal exchanges;

and the extreme sophistication and confidence or courage needed to intervene or even to spectate. But these elaborate, apparent trivia of legal style are the very ritual stuff which serve socially to distance the judge from the common folk, as part of the ritualised cultivation of his social authority as an expert fact-finder and decision-maker. They are part of the process which converts social issues into esoteric problems of "discovery", only penetrable by the elite inner few of initiates who have painstakingly learned how to unpick the lock concealing the supposed objective truths that those who actually make decisions like to suggest have dictated the decision for us all. As Drewry has put it, this type of use of the judiciary, the ultimate social symbol of impartial, fact-finding authority, to resolve controversial social issues, is "harnessing a myth" of the objectivity of judicial methods and thus of the issues involved. (8) This is the parallel of the political use of the myth of scientific objectivity to define and legitimate decisions justified by reference to the alleged inevitability revealed by 'objective' scientific knowledge.

The implication which I wish to expose for the moment, however, is that the alienation of the ordinary public is encouraged by this basic metaphor of a fact finding institution used to decide the issue. It symbolises no need for the ordinary persons to try to understand what is going on, nor to exercise social judgement. This passivation may or may not be functional in the short term, but over the longer term, it must lead to the relentless attenuation of any broad, mature democratic capability in these matters. It must of course be readily acknowledged that the Windscale inquiry was an improvement upon the previous traditions of private decision making. But it would be very misleading to conclude from this that there are not still some fundamental problems associated with what the new openness symbolises and encourages. In my own judgement it has symbolised an openness only to a somewhat wider constituency of "expertise", which, although it often does hold different value positions, finds it difficult to express these, and have them recognised as such in public. The danger is that we shall call this more sophisticated form of technicism, the ultimate in openness in decision making, and restrict our democratic ambitions to uncomprehending and apathetic spectator entertainment as elites from either side lock themselves in escalating, semi-private conflicts. Our ability to articulate values and choices in the light o reasonable technical understanding will not even be challenged to develop.

Ritual Functions:

A second set of tacit meanings embedded in the decision process relate to its ritual elements. Here I need to summarise what I mean by this, before going on to discuss it. I have proposed elsewhere that a useful way to interpret the Inquiry is as one major stage in a succession of escalating political rituals enacted ad hoc in order to contain political conflict which kept overtaking or threatening to engulf previous

more economical legitimation processes. (9) There was never any really serious consideration given to the possibility of abandoning oxide reprocessing and THORP - it was regarded largely as inevitable given the previous enormous, interlocking political and technical commitments made in the nuclear field.

Thus, for example, the Parliamentary Select Committee on Science and Technology took evidence in 1967 about the desirability of a separate nuclear fuel company from the Atomic Energy Authority, in oxide processing. Yet already by then, the AEA's fuel division (later British Nuclear Fuels Ltd., BNFL) had started a foreign business drive, and had signed contracts to reprocess foreign oxide fuel. It had even built the ill-fated oxide head-end plant in order to capture this export market.

By the time that the Government was forced in 1975 by media and public pressure to assert that it was reviewing the desirability of foreign oxide reprocessing, over 300 tonnes of foreign oxide spent fuel was lying in Windscale ponds awaiting reprocessing under previous contracts. Although the government reassured an anxious public that any foreign contracts signed would forbid the indefinite storage of the radioactive wastes in Britain, not only was this only an optimal clause in fact, but also, the neglect of such a 'return of wastes' clause in previous contracts was obscured. Mr. Benn, Minister for Energy, initiated "an extensive public debate", which consisted of two public meetings and a half-hour, parliamentary "debate", very late at night, with a very thin attendance. The culmination of this brief flirtation with public accountability was the parliamentary announcement on 12 March 1976, after three months of private review, the BNFL could proceed towards THORP, and seek contracts (e.g. with the Japanese, Germans and Swedes). No-one knew whether or not the government had received answers to some of the questions unanswered in the 'public debate'. But certainly those who had put them had not received answers. (10) There was no elaboration whatsoever upon the decisions, and in particular no details of what had been investigated in the review, who had been consulted, and with what results.

Mr. Benn clearly expected there to be no further delay, since in January he had remarked that a Select Committee investigation of THORP had been considered, but had been rejected because it might take too long, and time was pressing.

Nevertheless, this new, if modest, ritual of legitimation by the government was overwhelmed by the events of the next 9 months. Whilst pressure bore down from the industry's side upon the government to avoid a planning inquiry when the THORP plan went through the local authority planning system, a pressure which the government digested, in the end irresistible forces in favour of an inquiry pushed the government, clearly against its strong will, and very late in the day, to hold an inquiry. This was now an important extra layer of legitimacy, not previously regarded as neces-

sary. Even then, however, the government was keen to get it over with as fast as possible, and took it for granted that an ordinary local inquiry, with some ad hoc adaptations such as a high-status chairman, would be adequate.

The key aspect of this decision to hold a local inquiry was that under the rules, once the inquiry was over, there was no further chance for review, evidence or lobbying of any sort, and the confidential chairman's Report and Recommendation was transformed into a final, binding decision by the Minister, acting alone. In particular, Parliament would have no say, and the whole thing would be disposed of efficiently, after the tiresome necessity of the inquiry.

Yet again, however, new political pressure accumulated to undermine even this further legitimation ritual. Nearly half the members of Parliament demanded a debate and vote in the Commons, after publication of the Report and before a final Cabinet decision. Initially, just after the Inquiry had ended, Mr. Shore curtly dismissed the idea as unnecessary. By February, however, after he had seen the (confidential) Report, Shore enthused that a parliamentary debate "would be of great benefit to our democracy", and set about finding a way of allowing it that would not void the whole process. Thus again a reversal occurred in previous commitments as to what would be an adequately elaborate level of public 'involvement', and Parliament debated the Parker recommendations, first on March 22nd and again on May 15th, 1978. For the first debate, however, there was no time even for objectors groups to assemble criticisms of the Parker Report, let alone time for MP's to assimilate them for debate. For the second debate, Mr. Shore issued the government's hurried response to some of the conditional arrangements proposed by Parker so late that MP's complained that they had no time to digest them before having to make a final vote. Both debates were also very uneven and lacklustre in quality, which only reflected the majority ignorance about nuclear power and THORP. This inadequacy itself reflected the lack of involvement by Parliament in nuclear affairs, over the previous thirty years. Inevitably shepherded by Parker's unambiguous verdict, MP's voted in favour by 224 to 80. In the circumstances the only surprise was the significant minority who voted against. Thus the final act of ritual lay with Parliament, the last symbol of democratic legitimation - the last shot in the locker.

The ritual aspects of this evolving ad hoc series of events are underlined by the evident determination of the government to hold fast to a pro-THORP policy, despite wishing to appear completely open-minded by holding a very broad public inquiry. This schizoid stance is summed up in the contradiction between the Windscale Inquiry's apparent broadness, and the less widely recognised narrowness introduced by Parker's evaluation and Report of the Inquiry; and by the comment made to me in November 1977, by Dr. John Cunningham, MP for Whitehaven and then Parliamentary Under-Secretary for Energy, that despite all the elaborate fuss of the inquiry, the government considered that it had learnt nothing new from it.

The rituals of comprehensiveness

In this perspective therefore, the Parker Inquiry flattered to deceive. It purported painstakingly to analyse evidence and find facts, where in reality it issued principles and made social judgements which fenced off some arguments and augmented others, but which were not the exclusive province of a judge, nor of any other expert. Thus for example, Parker heard day upon day of evidence about alternative energy sources and futures, only later to dismiss it in his Report by asserting that we should keep all our energy options open - a presumption which directed all subsequent argument if favour of THORP. (13) On economics he heard much dubious factual evidence only to accept later that, at the end of the day the economics of THORP as against alternatives, were an open question. But this did not matter said Parker, because even if it is uneconomical, the political insurance offered by our own source of uranium and plutonium from reprocessing would be worth the cost - yet another social presumption which almost predetermined the final verdict. With respect to the question of whether one could safely store UK oxide fuel in water, as an alternative to reprocessing it, the issue of the integrity of the stainless steel fuel cans in water was crucial. However, it was soon discovered that no research had been performed on this matter, so Parker initiated some investigations by the AEA. The very hurried and unsatisfactory replies, very late in the inquiry, were not properly crossexamined due to lack of time for preparation, so that the doubts cast by the AEA were sufficient to allow Parker to assert that reprocessing was an insurance against likely corrosion in storage. Even Frederick Warner, Parker's engineering assessor, later admitted that this was a very unsatisfactory way of dealing with this central question. (14) On proliferation questions, Parker essentially argued that there were objective rules beyond the realm of social choice which obliged Britain to reprocess foreign oxide fuels and thus to build THORP. In the traditions of the judiciary he treated the ambivalent international compromise, the Non Proliferation Treaty, as if it were a strictly unambiguous statement of the laws of conduct in regard to nuclear materials and technology. He thus stressed those Articles asserting the obligation upon nuclear states to make all civil technology available to non-nuclear states, and derived from these the British "obligation" to reprocess for Japan. This ignored the endemic contradictions in the heart of NPT, as with most international treaties, that other articles in it state the obligation not to transfer anything which would contribute to a nuclear weapons capability. The THORP plan would involve the return to the customer of the plutonium extracted from their spent fuel. Thus did Parker, through his judicial worldview, attempt to convert the ambiguity and fluid political balance of choice in such issues, into mechanical and inevitable derivations from supposedly precise and objective rules.

On other issues where he could not find the means for such 'objective' conclusions, he also simply issued principles which effectively dismissed day upon day and ream upon ream of evidence. As with energy and

economics, so with radiobiology. Although he expressedly dismissed the objectors' evidence of Drs. Stewart and Radford, he then hedged his judgement by asserting that, in any case, the appropriate authorities (in this case the NRPB) would make decisions on radiation risks and standards, in their own way. Thus again, the scope of actual analysis was drastically slimmed, but not at the Inquiry itself, only afterwards, in the Report. Essentially the same post hoc rerouting of accident safety questions occurred, this time to the Nuclear Installations Inspectorate. To criticisms that these authorities were less than impartial, and far short of accountable, Parker responded that such criticisms were scurrilous accusations of the personal dishonesty of the scientists involved, thus exposing both his insensitivity to sociological structural influences upon scientific judgements and the misleading positivism of his overall approach.

On other issues, such as the viability of waste-disposal techniques, and the engineering validity of the plant designs, Parker simply accepted BNFL's credibility over that of their opponents. Especially in the latter case, there was too little to analyse, with at least £20m design development work still to be performed, and with the plan only at the 'conceptual flowsheet' stage. An alternative interpretation here is not that Parker endorsed BNFL's assertions that all was sound, but that he simply expressed himself agnostic, and left the verdict to the future. In either case, however, the impression of some definitive analysis and authoritative decision is quite false, as it is for the other aspects of the question outlined above.

That having been said, one must stress that the conclusion is not particularly that Parker failed to be as rigorous as he should have been, though that can certainly be said for some issues; nor even that Parker should have been more honest or careful to tell people at the time that he had decided that their evidence, e.g. on energy futures, was irrelevant. The probability is that he had not then so decided.

The important conclusion is that any kind of all-in, definitive decision was strictly impossible; some things were just beyond analysis; others were already determined by previous commitments or neglect; yet others were beyond the control even of national decision makers. In this sense

any 'decision' of this complexity is not a decision in the usual sense, taken by one and the same agent(s), even over several stages. Any 'decision' is inevitably flawed and incomplete. Yet, despite this, political pressure was so acute that it demanded such a comprehensive gutting and simultaneous synthesis of all the issues feeding into THORP. Thus an elaborate pretence of breadth, comprehensiveness and rigour was indulged in. This was not so much a carefully hatched conspiracy, as a collective self-delusion, with Mr. Justice Parker the unfortunate fulcrum between the unrealistic ideals vested in the inquiry, and harsh reality (as later expressed in his Report). It is significant that whereas most of the hardened professionals treated the Windscale inquiry as just another planning inquiry, subordinate to many surrounding and subsequent

political forces and decisions inaccessible to the inquiry itself, nearly every other participant regarded it as a last battle ground not just on nuclear power, but for some on industrial society in general. This sense was amplified by the knowledge that there would be no further chance to enlarge upon one's arguments once the inquiry was finished (this was only later superceded by the decision to allow a parliamentary debate), and by the general lack of trust vested in those institutions such as the NRPB; NII, MAFF, BNFL itself, and the Department of Energy, which exercised autonomous authority over many aspects of THORP, e.g. radiation standards, safety calculations, discharge authorisations, economic assessments and energy policy. That these institutions could not be completely invaded by democratic scrutiny is self-evident, yet the strong tendency of the inquiry as a unique, one off, "cup final" type event, was to condense all the pent up pressure for routine accountability in those institutions into an unrealistic desire for comprehensive once-and-for-all public evaluation of their operations and decisions. It was these combined forces which led to fundamental but unrecognised conflicts of attitude with respect to uncertainties. Whereas a major plank of several objectors was that the THORP plan could not be given approval with so many issues unresolved, BNFL, and Justice Parker argued that unless permission were given, there would be no chance to even explore them. This approach required faith in the surrounding authorities and their future decisions, and also a much reduced idea of the status of the inquiry, neither of which was held by objectors.

Political decisions - delegation, simplification and reintegration.

In the previous section I have discussed the inevitable contradiction between the pressure for a definitive, once-and-for-all decision, and the impossibility of such a tidy and final synthesis.

It is almost the defining feature of political decisions that they have several dimensions mixed up within them, with questions of social values at their root, even though often obscured. It is also usually the case that even the nature of the issue to be resolved is disputed openly. Both these were true of the Windscale THORP issue. The industry attempted strenuously (and in the end successfully) to fence off THORP from any question of further commitments in the nuclear field, which might weigh it down with further public concern. It could, they argued, be treated as a discrete issue, entirely separate from fast reactors, further thermal reactors, questions of civil liberties, energy alternatives, the fairness and competence of decision making authorities, and so on. Objectors on the other hand, tended to associate THORP with a catalogue of further commitments and wider questions, in their view inevitably demanded by a decision to have THORP. To them it was a trojan horse of hidden commitments. To some THORP even symbolised the extravagance and irresponsibility of industrial society at large.

With an issue permeated by such deep currents, the political sphere is forced artificially to simplify, to reduce it to politically manageable proportions. As Horowitz has noted:

> Each decision process leaves its distinctive mark on the issue it touches. Each of them snatches a few transactions from the flow of events, brings them to the foreground and blurs others into the background. Each applies its own mode of analysis to these magnified phenomena. Each has its own set of tools that it uses to devise solutions to problems that it has analysed. No one tool kit is exactly the same as any other. Equally important, each decision process decides some things and leaves other things undecided. There are significant patterns of non-decision as there are patterns of decision. (15)

Whatever institutions we employ to define and resolve issues, they mould the issue in characteristic ways. The 'reality' being analysed and decided is itself already socially constructed, according to social and cognitive traditions which are interesting and important.

One common institutional method which is employed to isolate issues and bring them within the reach of that institution's "tool kit" is to refine the issue down into discrete, detailed, factual questions, apparently unconnected with anything else. Science operates this reduction process, and the judiciary employs it with even more formality and rigour. Often they do so by increasing precision at the expense of becoming more divorced from the ordinary social world and its systems of discourse. The courts even have laws of contempt which serve to isolate an issue from the social world at large by strictly forbidding public comment on a case once it has gone sub-judice. Parliament effectively suffered this indignity before and during the Windscale Inquiry. As Horowitz has put it in connection with the judiciary:

> Piecemeal decisions also isolate artificially what in the real world is merged. It is a truism that everything is related to everything else, and of course the cliché proves too much, because no institution can or should attempt to deal with everything simultaneously. (16)

I have earlier emphasised the judicial need to construct decisions as if determined by objective rules, and to reduce issues to apparently black-white, discrete factual questions, amenable to unambiguous factual answers. I have also mentioned some damaging consequences of this. Various other characteristics of the judicial mould are significant for the way in which they reshape political issues.

Firstly they are unused to exploring alternative courses of action, yet this is usually a vital aspect of political issues. Certainly Justice Parker did not show up well in this respect. Nor did the adversary pro-

cess characteristic of the courts help to explore positions intermediate to those of categorical objection or unqualified advocacy of THORP. The judge's habitual experience of conducting (previously developed) court cases was also inadequate to recognise the extent to which the Inquiry, as a debate, developed the points of view of different parties during the proceedings themselves.

Secondly, they are unused to the political need to encourage the expression of differing intensities of social preference.

Thirdly, they are unused to recognising the value in political issues, of compromise and trade-off between different interests.

Fourthly, judicial thought is non-probabilistic, in that, even if a judicial decision is in reality based upon a fine balance of probabilities between conflicting pieces of uncertain evidence, the judgement is expressed (and acted upon) as if one hundred percent certain. Viewing the court's role sociologically, one can regard this as necessary to maintain the court's authority and to fulfill its daily functions. A certain decision is less likely to be reopened and returned endlessly to further court action, and more likely to secure broader acquiescence than one accepted openly as a balance of uncertainties. In political affairs however, the same tendency may or may not be functional. It may engender an image of (and thereby secure) broad authority,but in political issues there is more scope for retaliation and continuation of the issue by others who see through the spurious certainty thus lent to a decision. Nevertheless, political actors seem instinctively to look towards such a non-probabilistic, even dogmatic rhetoric of scientific objectivity to defend their judgements by concealment in a thicket of apparent objective 'facts', whether certified by science, the judiciary, or a hybrid of the two. Whether decisions on modern technology provide more pressure, and more scope, to use science as a rhetoric of justification is a germane question.

Although these features of judicial decision making are incompatible with the usual processes of politics, they are nevertheless arguably, precisely those characteristics which correspond to the 'needs' of big technology in political decision making. Thus several nuclear proponents have praised Parker's verdict for being so extremely crisp, and unambiguous. (17) It proposes an utterly unqualified commitment, not hedged with demoralising sideward glances and conditions. Furthermore it is arguable that the usual political traditions of recognising social preferences and balancing them in trade-offs and compromises, is moribund in the case of nuclear technology because it offers no compromise - it has nothing to trade in. It requires such an all-or-nothing commitment. Thus the judicial mould, for all its questionable features in regard to traditional politically defined issues, may actually be ideal

in the case of nuclear technology. In other words the absolutism of the judiciary, admirably summed up in the words of the judge, Lord Bessel, - "I may sometimes be wrong, but I am never uncertain" (18) - whilst it may be useful in its alloted institutional niche, may also resonate with the structure of political power and authority dictated by the unbroken commitment to big technologies like nuclear power. Whether this suggestion reveals the democratic shortcomings of nuclear technology, or the political superiority of judicial rationality, is a matter of argument.

Values, facts and the Nuclear Debate

Elsewhere, in a book I am writing, (19) I have referred to some cases in which the same events had been interpreted by the different sides of the nuclear conflict, in entirely incompatible ways. The facts were absorbed into fundamentally different frames of reference, or worldviews. The examples which I gave were the interpretation of the significance of accidents; the interpretation of contractual clauses obtaining prepayments from foreign reprocessing customers; and the interpretation of the lack of previous research on waste disposal techniques and sites. There was also the example of the fundamentally opposed interpretations of the determinist argument, where opponents took the proponents' view that we could not reverse previous commitments, to mean that with such irreversible inflexibility we had better not enter into any further open-ended commitments.

The most general such point of disjunction in these worldviews occurs in relation to the energy demand issue. The proponents of nuclear energy argue that without an increase in nuclear power there will be an inevitable energy shortfall in the next few decades, and that this will lead to social disorder and even the breakdown of industrial society. Alternative energy sources, they argue, would require unacceptable changes of life style, and restrictions of freedoms to which consumers are now accustomed (e.g. compulsory insulation of houses). The implication is that a nuclear future would not entail significant lifestyle changes.

On the other hand, opponents of nuclear power claim that it is nuclear power which encourages, even requires, the continuous expansion of consumption and energy production, without proper regard to its end-use. This in turn will exacerbate other resource-shortages and conflicts, and demand life-style changes of its own, which will result in the demise of industrial society.

Thus one faction assumes that present nuclear plans are vital to save industrial society from apocalypse. The other assumes that they will herald that apocalypse. It is emphatically not just a matter of straightforward defence or rejection of industrial society per se.

Every facet of factual conflict within the energy debate derives from conflicting frames of reference outlined above. 'The facts' as they are seen by either side, are rendered meaningful by these value positions. It is not that value positions and social policy views derive from a prior, value free scrutiny of 'the facts'. In debate, reference to 'the facts' is thus a rhetoric of moral persuasion, as anthropologists and sociologists of science have long recognised from other spheres. (20)

Facts and emotions - the false asymmetry

One very common feature of the nuclear debate so far has been the repeated assertion by nuclear proponents that the true nature of the debate is solely factual, and that opponents' views are riddled with emotional commitments. Sir John Hill has even likened the nuclear debate to that between flat and round earth antagonists in the Middle Ages. (21) Frequently the realisation that social values are at the root of much opposition, and frame its factual assessments, is treated by nuclear proponents as a triumphant exposé of a guilty secret furtively harboured by opponents. They are even harangued by Fred. Hoyle and even by some Trade Unionists for being in the league with the communist Russians, (22) whilst being suspected by Professor Fremlin of being in bed with the arch-capitalist oil companies. (23) Frequently, the criticism against nuclear opponents for basing their views on more than pure facts is nevertheless combined with assertions which are equally emotionally held, and which are beyond factual verification. Thus for example in a letter to The Times, a group of eminent pro-nuclear engineers decried the inability of their opponents to stick to facts, only in the same letter to assert that:

> if the world is not to face a substantial shortfall in living standards and a very real risk of civil upheaval - if not war - there is no alternative but to rely upon a major contribution of energy from nuclear sources. (24)

This may or may not turn out to be true. The point is that it is no more 'factual' than opponents' claims - labelled emotional and thus disreputable by nuclear proponents - that expanded nuclear activities will lead to, e.g. eroded civil liberties, nuclear war, or intolerable pollution.

There is a basic inconsistency in the asymmetry which recognises the emotional and symbolic underpinnings of one side of the argument, whilst ignoring the equally strong counterparts underpinning the other side.

Thus whilst the advocates of nuclear power articulate their values and commitments in the elaborate ritual language of 'objective facts', their language serves not as a medium of debate with their opponents but as a cause of further alienation between them. It is a mode of moral self-reinforcement. The dynamics of 'debate' are in fact polarising, not depolarising. The more that the pronuclear argument is phrased solely in

the language of technical facts, to the neglect of questions of social values, the more this exacerbates the opposition of those who seek authentic social interaction and meaning in public discourse. Stallen and Meertens have emphasised this basic gulf in what participants are ultimately seeking in the nuclear 'debate'. (25)

Viewed in this perspective, it is not surprising that the strongly fact-oriented image of the Windscale Inquiry was very comforting to the nuclear industry. Indeed it was an integral part of that cosmology, in that it was a major medium for the public elaboration of the overall positivist, fact-based political ritual of the pro-nuclear language. As Lukes has noted, however, such political rituals may be socially integrative, but they may also serve to integrate and reinforce some groups whilst in the very process serving to exclude, alienate and thus potentially enhance conflict with others. (26) Arguably, Windscale did this in a major one-off event, in the context of an ongoing less dramatic enactment of the same ritual 'debate' over a succession of lesser events and issues in previous (and following) years. In many ways the moral universe of the pronuclear school needed a big occasion to revive and reengrave it after the gradual erosion of morale and authority due to setbacks and criticisms over several years. (27)

The judicial method is to (a) find facts, then (b) evaluate these facts in the light of taken-for-granted objective and unambiguous (legal) rules. This structure is identical to the technocrats' approach to technology assessment decisions, wherein they (a) find facts, then (b) evaluate these into a decision, in the light of taken-for-granted, objective and unambiguous (social values) rules. It is not surprising therefore, that the judicial approach reflected in the Parker Report should be so welcome to the nuclear establishment and should so alienate those who see the very rules themselves as obscure.

A typical illustration of the relief which Parker's positivism and empiricism gave to nuclear proponents is the praise given by Francis Tombs, Chairman of the Electricity Council, in January, 1978:

> One problem in communication (in the nuclear debate) arises from the ability of objectors to make emotional and unquantified statements whereas the scientists' and engineers' replies are constrained by the need for accuracy... therefore objectors must be met face to face in rational and objective discussion...
>
> The Windscale Inquiry served...(this) purpose. The discipline of cross-examination and of the preparation of numerical arguments has done a great deal of good...I especially welcomed Mr. Justice Parker's emphasis on numerical and factual support for the arguments put forward. By such means emotional arguments can be reduced to an objective and intelligent level. (28)

There is of course much to agree with in this. The need for rational face to face discussion is self-evident. But the characteristic symbolism of 'objectors' on the one hand, but 'scientists' on the other, alerts one to the imbalance in the main point being made. Furthermore it is clear that Parker's insistence on a solely factual style corresponded completely with the industry's founding premises in approaching the debate, and acted as an elaborate reinforcement for them, of its moral authority and security.

The rationality of public attitudes

One common theme in the pro-nuclear argument is the irrationality or ignorance of objectors and the public at large.

I have suggested that the language of 'facts' as a sole framework for such decisions is a rhetoric which defends existing attitudes and structures of power from the threatening revelation that the social values supposedly legitimating those structures are obscure and in turmoil. It is part of a ritual of moral self-reinforcement of a particular sector of society. It is now necessary to clarify why it is quite rational for factual criteria often to be subordinated to other more complex criteria for judgement and commitment.

I have emphasised that the offical model of technology assessment treats it as a process of fact-finding. The problem is regarded as that of identifying factual consequences, and then synthesizing these into some cost-benefit framework, whether explicit or implicit. The question of how to evaluate the identified consequences into a decision, (i.e. the social values to apply) is taken officially as unproblematic, and if recognised at all, usually camouflaged in technical mystification.

With relatively small schemes, despite its widely recognised limitations this approach may be adequate, but with complex big technology developments problems arise in at least two dimensions. Firstly and most simply, the identification of consequences itself becomes more problematic, with extensive and interacting effects leaving more and more consequences uncertain and even unknowable. These may be the most important consequences.

Secondly, and less obviously, as the scale and interconnectedness of the developments in question increases, they become the medium of significant changes in social patterns and relationships as people accomodate to the new opportunities and restraints brought about by the development. These interact with similar changes from other innovations to produce entirely new and unforeseeable changes in social experiences and attitudes, and thus in social values.

This process means, however, that at least with respect to big technologies such as those of nuclear energy, genetic engineering, communications, etc., the decisions taken actually shape the framework of social values within which future decisions will be determined. This is what Tribe referred to as the recognition that decisions partly constitute ourselves and our values as social actors, rather than being merely derivative of autonomous social selves with clear and independent social values, as the official, fact finding approach implies. (29) It is not just a case of social values being in conflict, but of social values being unrecognised and to a significant degree created unpredictably as the technology develops.

These endemic uncertainties surrounding complex technological developments mean that to the extent that control resides anywhere, it does so not in the finding of supposed facts about a technology, but in the future power to control those unforeseeable but inevitable changes in social restraints and opportunities as well as in physical environmental factors. If one is attempting to assess a technology, perceiving the fact that most of its more distant but probably more important consequences are inscrutable, the best way to judge the technology is to judge how the institutions which control decisions about that technology and related fields, will react to those inevitable surprises over the horizon. These will include changes in social values themselves. Thus in my view three important points follow: I submit these in recognition of their present relative lack of empirical foundation, yet in the conviction that they are important enough at least to have them on the agenda for discussion.

(1) It becomes evident why it should be that social attitudes about technology appear to be concerned far more with questions of trustworthiness, accountability, responsiveness and intelligibility of the decision making institutions than with direct facts themselves, which are often in any case of chronically disputable relevance or validity. Questions of trust, credibility and openness of decision makers, and significantly, their past record in these respects, become of paramount importance in framing social attitudes, and rationally so. It is highly significant that Mr. Justice Parker treated these political matters as at best irrelevant in the Windscale case, thereby failing to recognise perhaps the main current of real and rational public concern on the THORP issue.

Put another way, the present point is that people perceive however dimly, that a 'factual' accounting process in technology assessment is not only useless but misleading, and that an intuitive judgement has to be made about whether the growing list of unresolved and increasingly pervasive uncertainties surrounding technological developments will be handled wisely and honestly by decision making elites, and according to what political criteria or social values?

Fact finding definitions and solutions (especially those many which understate uncertainty and conflict only to see the facade of certainty later demolished) are only likely to decrease the trust factor even further.

(2) Viewed in the light of the foregoing, the debate can be seen to express not a conflict between clearly held but opposing sets of values (e.g. for and against "industrial society") but a conflict over who shall exercise control over the articulation of values embodied in and influenced perhaps without enough reflection by the decision making process. Who shall influence even the horizons within which it will be possible to articulate social values in the future.

(3) A feature of the unforeseeable changes in social values set in motion by big technology decisions is that they are gradual, and all embracing. Each single development, e.g. in nuclear power, according to the account of public reason I have suggested, rationally invokes the legitimate fear of future consequences to which the single event may contribute, but only partly. The definition of the specific issue as separable or inseparable from such extensive future developments will move from the latter to the former in proportion to the lack of on-going control which people feel, over the relevant decision makers. The style, values and accountability of a whole historical mode of decision making becomes a more important focus of public evaluation the more open ended and pervasive is a field of technological innovation. This also becomes the more important for big technologies because in retroactively influencing the framework of social values and assumptions, they almost automatically accumulate self-justification at each single step of development. Thus the strongly evident condensation at Windscale, of fears extending over the full nuclear scenario onto one single development, THORP, can be clearly understood according to the insight that trust and credibility - therefore intuition - inevitably count for more than bare facts alone, and that this should be seen as rational and legitimate.

A further aspect of this last point concerns irreversibility as argument against nuclear technology.

In this perspective it becomes obvious that the irreversibility argument is more authentically one about the unwillingness to trust in the controlling elites' readiness to reverse their previous commitments if the road taken becomes unacceptable. Although in theory the commitments entailed in modern technology are reversible, the labyrinthine extent of social changes that result from them as outlined before, means that in practice this is not so. But the public feeling of and opposition to irreversibility will be amplified in proportion to the lack of control which they could have over the judgement of whether or not to reverse those social commitments if the consequences come to seem unacceptable. Thus even the irreversibility argument can also be attributed in large measure

(though not reduced altogether) to the sociological question of public accountability and trust in decision making processes and elites.

It seems that large sectors of the public respond to decisions and to risks in the entirely rational way of asking not, what are the benefits and costs entailed; but also, what elements of uncertainty are there in the facts being disseminated and the structure of decision making control involved? In this sense, uncertainty itself is a loss of control, a risk to be included in the judgement process. Furthermore, it is not only uncertainty of a kind which nature controls, but uncertainty controlled by other social actors, perhaps of alien social values and cultural style.

Anthropologists have demonstrated to legions of exasperated development experts bent on modernising peasant societies, the solid rationality of the peasants who refuse to adopt the alien and utterly uncertain futures and risks offered to them by the zealous agents of 'rationality' (30). The peasants are not pathological or naive devotees of an illusory freedom from any risk, but they have evolved reliable means of choosing them and insuring them which do not include risking comprehensive and entirely unpredictable social changes combined with the delivery of their fate into the hands of some person or group whose values and guiding interests they do not understand, let alone trust. Perhaps there is an enlightening analogy here with our own nuclear debate and the crippled, unrecognised dialogue of control and credibility going on between our elites and their public.

Summary and Conclusion

In conclusion therefore, I should like to briefly reiterate my main points:

1. It is important to understand the elements of social ritual involved in such big occasions of technology assessment like Windscale. There are several dimensions to this:

   (a) The elaborate pomp of this inquiry, and the superlatives by which it was described, ritually emphasised complete social control of the technology, thereby concealing the very large extent to which no such control is possible even for social elites.

   (b) The inquiry and surrounding procedures, notably the hurriedly organised Parliamentary debates, ritually emphasised democratic social control of the <u>decision</u>, as if it were open to such choice and influence, and thereby backgrounded the facts of strong and longstanding prior commitment to oxide reprocessing, and continuing government determination to have it.

   (c) The objective 'discovery' image of the inquiry ritually emphasised the factfinding dimensions of the issue in hand, thereby

concealing the aspects of social choice involved from a wider public. It acted as an important ritual occasion of moral self reinforcement for the nuclear establishment, whose morale had suffered erosion over the previous few years. This ritual defined the issues as those solely of fact partly so as to defend from wider debate the value and social premises inherent in its approach. Clearly in such a broadened debate its monopoly technical control would be less of a decisive resource. The judicial tradition of empiricism corresponds naturally to the technocratic worldview, and perhaps to the decision making needs of big technology promoters.

(d) In many senses no decision of the complexity and political importance of THORP can be definitive, because there are several relatively unrelated issues involved, all with their separate developmental dynamics and states of maturity. One issue may be too late for choice; another may be too early. The political pressure for a one-off, all-in, synthetic decision was colossal largely as a result of the history of closed policymaking in the nuclear field, and of the continuing lack of accountability of the institutions to which many important sub-issues would have to be left. Thus there were impossible public pressures for the most comprehensive definitive inquiry, yet inevitably narrowness to the eventual analysis in the report. Parker was at the crux of this conflict as reflected in the controversy surrounding his report. The ritual breadth of the inquiry was severely contradicted by the harsh reality of actual decison making, but the ritual was made all the more elaborate by the traditional closedness of the British decision making bodies in the nuclear field.

2. Although the Windscale Inquiry was a great advance upon previous traditions in British nuclear decision making, its dominant fact-finding approach, its intensity, and the forbidding demands of participation meant that although it allowed elite, specialist objectors to take part, the dynamics of sophistication of technical debate combined with the above factors to alienate the wider public. It served neither to involve them even indirectly, nor to educate them. Indeed it may have been a corollary of the public legitimation which the Parker Inquiry did secure, that the public's sense of interest in proceedings was reduced. Although much criticism was made of Parker's uncompromising, blunt rejection of all opposition arguments, and of the spurious objectivity of his justifying reasons, the objectivity ritual may have been a necessary condition of the (politically perhaps more important) legitimation secured, however passively, from the public at large. The alienation of the educated minority may have been worth paying in order to ensure the passive aquiescence of the vast majority. This may not have been secured by anything other than a judicial ritual.

3. In the longer term however, public attitudes can be treated I suggest, as rooted in the senses of intelligibility, responsiveness, trustworthiness and guiding values of decision making elites, not in the facts of technologies themselves. In other words, social-intuitional judgements, of decision making elites and procedures, become rationally the focus of interest, not just facts about consequences, especially for big technologies. Decision makers, still entrenched in a positivist cosmology do not understand this, and frequently appear to be sowing the seeds of the ungovernability of technology the more they elaborate the "facts only" worldview at the expense of an appreciation of the underlying social factors involved. In particular, passive acquiescence may be gained in the short term only to issue in inconsistent and fickle public attitudes in the longer term.

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THE ECONOMIST'S ROLE: AN INTERNATIONAL PERSPECTIVE

A.W. Coats, Department of Economics, Emory University

1. Introduction: Economics as a Profession and a Science

Since World War II there has been a remarkable growth in the numbers and influence of economists in business, government, and a wide variety of national and international public agencies. This expansion can be seen as a by-product of modernization, a complex process of economic, social and political change associated with industrialization, and what it is becoming increasingly fashionable to describe as post-industrial society.

It has been aptly remarked that "an industrializing society is a professionalizing society" (1), and economics is one among a number of modern professions. Unlike the traditional professions - such as law, medicine, and the church - their modern counterparts do not have strict controls on entry, formal codes of ethics, and effective procedures for disciplining and, in extreme cases, excommunicating their members. Nevertheless, they possess the essential internal and external requirements that their members are subjectively aware of themselves as professionals, and they are recognised as such by those who use their services, and by the public at large. As in other cases this recognition is based on the possession of degrees and other qualifications which are not readily accessible to laymen; and it takes the form of specialized appointments, high renumeration, delegation of responsibility or authority, and a measure of social esteem.

However, whether the economists are also scientists, as well as professionals is quite another matter. Nor is this merely a semantic issue. The organizers of this conference evidently have no doubts on this score, and by accepting their invitation I am presumably guilty of collusion. Yet it would be dishonest to conceal the misgivings which have so often been expressed on this matter, misgivings which have a direct bearing on our central theme: "Scientific Expertise and the Public".

It is unnecessary to bore you with yet another review of the interminable debate about the scientific status of economics, a topic which encountered diminishing returns long ago. There is, in fact, no general consensus about the discipline's precise nature, limitations, and boundaries. During the past decade or so the positivists' conception of economics as modeled on the natural sciences, especially physics, has been on the retreat. (2) Yet despite repeated attacks on economic theory throughout the past century or so, some of them both fundamental and effective, that central corpus of ideas has displayed a remarkable capacity for survival. Of course, cynics may argue that this is merely evidence of the economists' inertia, their obstinate refusal to abandon an entrenched intellectual tradition, and is not to be taken as proof of its scientific value. But whatever the explanation, it is noteworthy that even during and since the widespread so-called "crisis" in the discipline a few years ago, (3) there has been no perceptible slackening in the demand for economists. Whether this demand is attributable to the economists' scientific expertise, or some other causes, is a matter to which I shall return later.

2. Economists and the Public

Whatever the epistemological status of economics, the economists' public reputation is curiously ambivalent - and in this context the term 'public' can be taken to include not only the mythical average 'man in the street', but also politicians, civil servants, businessmen, and other professionals. These various 'publics' are certainly not prepared to view economists with the respect and sometimes even awe accorded to, say, nuclear physicists or space engineers. Too many laymen have strong preconceived ideas about economic affairs and unjustified confidence in their own ability to prescribe solutions to current economic problems. We are all familiar with the tedious old joke that if all the economists in the world were laid end to end they would not reach a conclusion; or that when four or five economists are gathered together there are usually at least six or seven different opinions, two of them Keynes's. These ideas are hardly compatible with unquestioning respect for the economists' scientific authority. (4) Yet even stranger is the common experience that many of those who are most outspokenly hostile or sceptical towards economists are strongly inclined to exaggerate their influence on public affairs.

In the early 19th century some of the literary romantics and social reformers claimed that the classical economists not only defended selfishness, but also exacerbated the evils of the industrial revolution by opposing essential legislative improvements. More recently, in response to the post-1945 boom, there have been complaints about the "plague of economists" (5), and it has even been suggested that there is an inverse correlation between the number of economists in government and the growth rate in any given country! Would that the relationship were so simple; the historian's task would thereby be greatly facilitated.

Oddly enough, the professional economists have entered enthusiastically into this game. As a conscientious student of these matters I offer the hypothesis that there are more self-deprecatory jokes about economists by economists, than comparable stories by members of other academic disciplines. Whether this means that the economists are more inventive or more self-consciously insecure than others is a matter for conjecture. They have certainly not been generally given to collective modesty. Perhaps the most frequently quoted passage from John Maynard Keynes, the most important economist of this century, is his emphatic assertion that politicians and policy-makers are usually "the slaves of some defunct economist". Less well known, however, and just as relevant to our subject is his cri de coeur of the early 1930's:

> If economists could manage to get themselves thought of as humble, competent people, on a level with dentists, that would be splendid. (6)

Needless to say, Keynes himself, though many things to many people, was seldom humble; nor has humility been a conspicuous trait among his professional successors.

The general public's attitude towards professional economists has not merely been ambivalent, it has also fluctuated markedly over time. In accordance with the tendency to overrate their influence they have been praised when economic affairs are proceeding smoothly and prosperously, and blamed when things go wrong. Thus the British classical economists and their popularisers received undeserved bouquets in the 1850's and 1860's, an era of economic expansion following the repeal of the Corn Laws and the adoption of free trade policies, and unmerited brickbats during the ensuing depression of the 1870's and 1880's.

The most revealing recent swings in the economists' reputation occurred during the 1960's, especially in the USA , with the so-called Kennedy tax cut, stage managed by the Council of Economic Advisers under the brilliant and persuasive leadership of Walter Heller. (7) Their manifest short-run success was greeted with widespread admiration, and it inspired a collective hubris among his professional colleagues, not only in America. However, by the late 1960's and early 1970's, when the economists' seemed unable to cope with the depressing combination often generally described as stagflation - rising prices, slower economic growth, and increasing unemployment - there was an exaggerated professional reaction amounting almost to an orgy of self-flagellation - to use Heller's term. (8) Needless to say, the economic repercussions of the energy crisis have not improved matters. No longer do we encounter, as in the 1960's, confident references to the technical problems of "fine tuning" - a vivid expression suggesting that the economic machine is under control and requires only minor adjustments to keep it on the required course.

It should be clear by now that the professional economists' relations with the general public are uneasy. Their reputation as scientific experts is uncertain and liable to sudden unforseeable changes, and their collective self-image has some of the same characteristics. Some of the reasons for this state of affairs are readily comprehensible but they are subject to a variety of methodological and ideological interpretations, and there are no easy remedies. The difficulties stem, among other things, from deficiencies in the economists' knowledge and from the range, variety, and rapidly changing character of their subject matter and the issues that concern them. The demand for their services is due to the persistence of such problems as inflation, unemployment, poverty, the optimum rate of economic growth and resource depletion, the distribution of income, the size of the public sector, international monetary and trading relationships, and the alarming gap between the income levels and growth rates of rich as against poor countries. In most cases the economic and non-economic dimensions are inseparable, and many of these issues are highly politicized. (9) It seems that whenever we make improvements in some directions we soon encounter setbacks in others; and the public is nowadays so sensitive to several of these issues that they are seldom absent from the headlines for long. In addition there is the peculiar difficulty, familiar to social scientists but unknown in the natural sciences, that the mere announcement of official intentions can affect the desired outcome. If the meteorologist predicts rain the weather is not thereby affected; but if, for example, the government predicts a gasoline shortage, as currently in the USA, it is almost certain to provoke public reactions which will tend to exacerbate the problem. On the other hand, if the public is deliberately kept in ignorance this can also be a source of instability.

The origins of the professional economists' present predicament can be traced back to the second World War and the postwar reconstruction or, more deeply, to the great depression of the early 1930's. At that time J.M. Keynes wrote that:

> For the next twenty-five years, in my belief, economists, at present the most incompetent, will nevertheless be the most important scientists in the world. And it is to be hoped - if they are successful - that after that they will never be important again. (10)

From today's perspective, more than twenty years after the end of the rainbow Keynes perceived, we are considerably less sanguine. Indeed, there are those who argue persuasively that matters are becoming more, not less difficult, for the following reasons: (11)

1. the policy-makers' explanations are more likely to be disappointed because the economic forecasters' exaggerated claims are fed into the policy decisions, which would otherwise be more cautious.

2. the increasing pace of technological change makes it less easy to predict the course of events.

3. with increasing affluence the public has more scope for 'discretionary' spending, saving, or withholding of labour. Waves of fashion are more likely to affect the pattern of consumers' demand.

4. interdependence is increasing, both within the field of market behavior, and because of the growing interconnectedness of economic and social factors. The growth of monopolistic and oligopolistic enterprises creates situations more complex, and accordingly more difficult to forecast, than the competitive markets presupposed in much economic analysis. The social effects of technological change upset the sociological parameters presumed to be fixed in much economic analysis.

5. in addition to the "endogenous" political factors increasingly influential in domestic economic affairs there is also the growth of international interdependence. External shocks are more likely to affect domestic economic management - the energy crisis being the most dramatic recent example. (Of course there were profound international repercussions of domestic economic fluctuations, tariffs, etc. in the pre-war world. But in the early post 1945 decades it appeared that this source of destabilization had been significantly countered by the growth of postwar international economic cooperation. Some observers doubt that such arrangements will continue to be as efficacious in the future as they have been hitherto.)

3. Economists in Government

Summarizing the argument so far, I have suggested that although the demand for economists has remained high, the precise nature and value of their scientific expertise has been severely questioned both from within their guild and by members of the various 'publics' with whom they deal Moreover their relationships with the public have not only been uneasy, but also unstable.

Undoubtedly the most important single factor affecting these relationships has of course, been their increasing involvement in economic policy issues - whether as ivory tower academics, journalistic or scholarly commentators on public affairs, civil servants, members of legislatures, cabinet ministers, or even heads of state. It is obviously impossible to cover all these aspects in the time available. Consequently I propose to illustrate the problems involved by citing some aspects of recent experience as revealed in a multi-country comparative study of the role of economists in government, since 1945. (12)

The intellectual background to the expansion of the role of economists in 20th century government is the breakdown of the 19th century belief in laissez-faire, a movement which antedates, but which was accelerated by, the severe depression of the 1930's. World War II, with the accompanying need for full mobilization of economic and military resources, provided greatly enhanced opportunities for economists to participate in policy-making and brought a widespread determination that the restoration of peace should not be followed by a return to prewar economic instability and periodic mass unemployment. Especially in countries devastated by the war or occupied by enemy forces the period of postwar reconstruction often involved a sharp break with pre-war attitudes and institutions, paving the way for the permanent adoption of socialist economic planning, or at least a substantial encroachment of central government upon the private sector in a variety of ways nowadays generally lumped together under the heading of "mixed capitalism". (13)

These general trends were both encouraged and facilitated by modern technology and the concomitant growth of large scale governmental and private organizations in both the national and international spheres, with a consequential expansion in the number of bureaucratic functionaries. The intellectual foundations of the enhanced demand for economic expertise were political and technical as well as economic - they included a varying combination of: socialist planning doctrines;national income analysis and econometric forecasting; and the application of macroeconomic theory following the so-called Keynesian revolution. The employment of economists did not in practice depend simply on the degree of attachment to conscious economic planning; it also depended on the available supply of trained personnel; bureaucratic conventions with respect to the role of specialists in the public service; and more general cultural attitudes towards economic rationality. Even in those countries where ideological opposition to economic interventionism was strong, and the scope for market-oriented enterprise remained substantial, the general character and objectives of economic and social policy did not differ radically from those accepted in more overtly planned economies. The timing and pace of change differed markedly from country to country, but there has been an almost universal tendency to employ economists in charting past economic movements, forecasting prospective trends, and making policy recommendations designed to adjust the future to bring it closer to the desire outcome. This is true in differing degrees in the communist countries of Central and Eastern Europe; explicitly socilist countries of Scandinavia, the Netherlands, and Israel, and more reluctant, half-hearted, or belated members of the planned economies club such as France, Belgium, the U.K., and the USA.

Against this background it is hardly surprising that the economics profession has become generally associated, albeit in varying degrees, with left-wing political movements - with Marxist, socialist, social-democratic, or labour, rather than conservative or U.S. Republican-type parties.

Admittedly there has recently been a resurgence of interest in neo-libertarian ideas and policies, especially the growth of anti-Keynesian monetarist doctrines advocated by Milton Friedman, and a revival of scholarly and ideological interest in Austrian economics of the kind associated with F.A. von Heyek and, at the limit, Ludwig von Mises. But as some of their disciples would agree, even the Conservative party in Britain and the Republicans in the USA, to cite only two examples, have moved substantially towards an acceptance of more interventionist ideas during the past two decades. And it was President Nixon, himself no conscious spokesman for socialism or communism, who once, no doubt inadvisably, is said to have declared: "I am now a Keynesian." (14)

This trend does not, in itself entail the explicit politicization of professional economics; but it does reflect an unavoidable danger where, as in Norway, members of the dominant school of economists have for a long period been working in close cooperation with a single ruling political party. (15) How far does a situation of this kind involve a conflict between advocacy and scientific objectivity; between professional standards and political commitments? To put the same question in more explicitly epistemological terms, how far does the economist's role in government, especially his active participation in policy-making processes, involve a conflict between facts and values - the problem of Wertfreiheit posed so long ago by Max Weber and his followers?

Of course, the suggestion that the economist's participation in policy-making involves value judgements is nothing new; it is simply that for reasons already indicated the issue is now posed on a larger scale and in a more significant form than ever before. In the past a favorite academic device has been to draw a sharp distinction between the economist's role as a technician prescribing means to achieve given non-economic ends, and his function as an adviser to politicians and policy-makers. It is in the latter capacity that value judgements figure most prominently - using the term value judgements to include political and ideological elements, and judgements of expediency based on immediate pressures, and considerations of administrative and political feasibility. (16) But one of the main conclusions of the international comparative study mentioned earlier (17) is that such activities on the firing line - in that no-man's land where politics and economics are inextricably mixed, constitute but a minor, though perhaps the most sensitive and visible, part of the professional economists' activities in government. And this helps to explain why the question whether economics is or is not a science, and if so precisely what kind of a science, is of less general practical importance than it has often appeared to be in the past.

There are two broad reasons for this conclusion - one directly arising from the nature of economic policy-making; the other arising from a review of the functions performed by professional economists in contemporary central government bureaucracies.

With respect to the former, economic policy-making has too often been conceived in unrealistic terms, as though the economic adviser were an Olympian figure offering words of wisdom to anyone willing to listen, but otherwise remaining aloof from the grubby world of day-to-day politics. Such figures may indeed exist; but the likelihood that they will be heard, let alone heeded, is remote. The process of policy-making in modern governments is often protracted, detailed, and highly complex. Policies may be initiated at any one of a variety of levels within the bureaucracy or from outside - e.g., in a Ministerial Cabinet, in Parliament, in the press, or even in the academic community. But most significant economic policy must necessarily be formulated over a lengthy period, during which time it will probably undergo a series of modifications and reformulations before it reaches the relevant decisions makers. If it is adopted, with or without further changes, it then has to be interpreted and implemented, often at a much lower level within the government machine. Economists may, and usually do, play a part at all these levels, and the interrelationship between economics, politics, and administration often becomes hopelessly blurred in the process. The virtual impossibility of isolating and evaluating the professional economist's contribution, qua economist, to the process, is suggested by the observations of Sir Alec Cairncross, an immensely experienced British government and academic economist, that any economist who believes that he and his professional peers can successfully take over the management of economic policy,

> has never been present at the kind of discussion between economists, administrators, and ministers, at which it is by no means uncommon for the economist to talk politics, the administrators to talk economics, and the ministers to discuss administrative problems. (18)

In some countries and at some times, economists and other professionals and specialist advisers are kept at arms length, or utilized only as providers of sophisticated rationalizations of policies already decided by politicians. But in the situations of the kind depicted by Cairncross, where they are active participants in the hurly-burly of decision-making under pressure, in conditions of ignorance and uncertainty, they cannot abdicate from their responsibility to contribute in order to preserve their scientific purity. Moreover, it is frequently impossible in retrospect to know exactly what occurred and why. To quote Cairncross again,

> ... it is often very hard even for those at close quarters with policy-making to know what does in the end shape the decisions that ministers take - or, still more, do not take... Where the issue is in dispute, who except the minister (or even including the minister) knows what clinched the matter? It is very rarely that one can say with confidence that the decision would have been different if X had not been there. The people who think they know and say so may, in fact, be ill-qualified to judge. (19)

This conclusion is reinforced by the knowledge that in some cases the ministers and their senior non-economist officials have also been trained in economics which is, in any case, not so esoteric a subject as to be totally inaccessible to the intelligent layman. This point is especially important when it is noted that at the point of decision, problems of economic policy rarely call for a detailed understanding of the latest developments on the frontiers of economic knowledge. (20)

However, while high level economic advisory work and participation in decision-making is the most visible and glamorous part of the professional economists' activities, it is but a small proportion of the total. Their main contribution, and the major part of the demand for their services, stems from two other distinct categories of activities - namely, narrowly technical functions, and general economic administration.

Of these, the technical functions are of less interest in the present context. They arise from the almost insatiable demands of modern government for ever increasing quantities of economic information which has to be compiled, processed and interpreted. Much of this information is in statistical form, and calls for the deployment of sophisticated techniques - such as economic model-building, econometric forecasting, cost-benefit analysis, planned-programme budgeting, investment appraisal, and so on. Whether the immense investment in sophisticated computerized econometric models is justified by the returns in terms of more accurate forecasting is a matter that fortunately need not concern us here. It is enough to note that during the past thirty years or so increasing numbers of economists have been engaged on work of this kind alongside other specialists, such as statisticians, mathematicians, operations researchers, computer specialists, etc. The division of labour between them has been growing increasingly complex in recent years.

In their capacity as economic technicians, professional economists tend to be congregated in specialist research or operational units or divisions somewhat removed from the mainstream of government administration. While they may enjoy a considerable measure of professional autonomy and self-control, offset in part by tensions or conflicts generated by their associations with other professionals, their opportunities for promotion within the main bureaucratic machine may be narrowly restricted. As specialists, they usually have fewer opportunities to demonstrate their capacity for general administrative coordination, personnel management, budgetary responsibilities, and other tasks of the kind undertaken in the upper levels of the organization. Hence, in the competition for the highest civil service posts their inexperience in these matters may prove a serious handicap; though it must be admitted that some specialists have no desire to abandon their professional activities and assume more general responsibilities for the sake of promotion.

Of more general interest, however, is the increasing recent tendency for professional economists to demonstrate their capacity for general administration, thereby substituting for the more traditional bureaucrats trained in the humanities or law.

Indeed, there is evidence in some countries that they are effectively undermining the established "juristenmonopol" of high bureaucratic offices, and earning the reputation of being the "new generalists". One reason for this state of affairs is the tendency for modern governments to move away from conventional rule-oriented bureaucratic procedures, for which a training in law is especially suitable, towards more discretionary activities, especially in economic and social affairs. (21) As governments have become more interventionist, the belief that economic affairs can be "managed" by a small central group of macro-economic experts has given way - partly owing to the manifest theoretical and practical inadequacy of "fine tuning" - to more detailed interference in an extraordinary variety of matters. The text-book distinction between macro- and micro- economic activities is not entirely satisfactory for our purpose; but it will suffice to explain why the economic implications of government policies have to be taken much more fully into account than was the case two or three decades ago.

In addition to changes in demand there have also been changes on the supply side resulting from developments within the discipline of economics. Alongside its advancing post-war technical sophistication there has been a parallel "spread" of theoretical interest beyond such traditional topics as industry, agriculture, trade, money and banking, communications, natural resources, labour etc. into new fields such as the economics of defense, education, health, social security, public goods, pollution, crime, discrimination, marriage etc. There seems to be no limit to the economist's interest in other subjects, and the rapidly growing literature on the economic analysis of law and property rights epitomises the tendency to encroach on matters usually regarded as the prerogative of the traditional law-oriented bureaucrat.

In other words, there has been both an extension of the role of the state into all the nooks and crannies of modern life, and a parallel movement of economists into an ever widening range of government departments and functions. Economics is a vocationally non-specific discipline. As mentioned earlier, its central corpus of analysis has demonstrated a remarkable capacity for survival; and in recent years it has also demonstrated remarkable adaptability.

4. Conclusion

However, in concluding, I must not leave the impression that the professional economists are in the process of taking over the modern world. Far from it. As indicated above, economics is not so obscure and difficult a subject as to be utterly beyond the grasp of the intelligent layman who

puts his mind to it for a moderate period. Its increasing utilization in modern government is not solely, nor even mainly, due to the indispensability of the economist's sophisticated models and techniques. It is due to the well-nigh universal broad applicability of his basic conceptual apparatus - the need to coordinate and administer scarce resources, to consider their alternative uses, to weigh costs and benefits, and always to bear in mind the interdependencies of a multiplicity of economic and social phenomena. These matters lie at the heart of public administration, and the well-trained economist can provide a systematic perspective on complex problems that arise at many different levels of modern bureaucratic policy making. Moreover, his combination of numeracy and verbal skills is also an invaluable asset.

The rapidly increasing demand for this kind of knowledge means that the supply of professionally trained economists has often been deficient. Consequently many governments are nowadays providing in-service training in economics for able young administrators who are likely to move up the official hierarchy. (22) Indeed, we find that in Japan - which is probably an extreme case - there are no identifiable professional economists in the central government. New recruits in the bureaucracy with degreees in economics are not deliberately placed in posts where their special knowledge is utilized. And although some civil servants are seconded to universities at home or abroad for specialist training in economics, they do not subsequently think of themselves as professional economists. Nor are they considered as such by other officials.

To the extent that this practice exists it implies that a specialist academic training in economics is no longer as indispensible in modern bureaucracy as it once seemed. And although some recent observers have deplored the rise of the "econocrats" (23), there may never in fact be an "economistenmonopol".

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3. For a review of the literature see A. W. Coats, "The Current 'Crisis' in Economics in Historical Perspective," Nebraska Journal of Economics and Business, Vol. 16 (Summer, 1977) pp. 3-16. Of course this

'crisis' is by no means confined to economics. For suggestive comments on the deeper underlying reasons see the fascinating paper in this volu by Yaron Ezrahi, "The Professionalization and Deprofessionalization of Science in Democracy,"especially pp. 29-30.

4. This, of course, means that to the economists and their public there is nothing novel in the open conflicts between the orthodox establishment and the so-called radical or "concerned" scientists which figured so prominently in other papers and discussions at the Oslo Conference. On the contrary, some economists have recently been complaining about the standardization of graduate training and the pressures to conformity of opinion among new recruits to the profession - a view which I find difficult to accept.

5. This is the title of M.M. Postan's article originally published in Encounter, 1968, and reprinted in his Fact and Relevance: Essays on Historical Method (Cambridge, 1971). An ammusing editorial in the Guardian (London, 25 July 1974) discussed the correlation between the number of economists in government and the growth of such undesirable phenomena as inflation, airline hijackings, and terrorist outrages, suggesting that "if it can be shown that the value of money varies inversely in proportion to the quantity of advice received, an important watershed in the field of economic theory will have been passed."

6. J.M. Keynes, Essays in Persuasion (London, 1931) p. 373. The oft-repeated remark quoted earlier appears in the concluding paragraph of Keynes' magnum opus, The General Theory of Employment, Interest and Money (London, 1936) p. 383.

7. Walter W. Heller, New Dimensions of Political Economy (Cambridge, Mass.,1966).

8. Walter W. Heller, "What's Right with Economics", American Economic Review, Vol. 65 (March, 1965) pp. 1-26. This was Heller's Presidential Address to the American Economic Association. In what he termed his "sampler of economic masochism" Heller concluded that economics more than any other social science "is affected by the common scold" (Ibid., p.3). As an example of what he had in mind it is appropriate to cite leading anti-interventionist economist, Milton Friedman: "I believe that we economists in recent years have done vast harm - to society at large and to our profession in particular - by claiming more than we can deliver. We have therefore encouraged politicians to make extravagant promises, inculcate unrealistic expectations to the public at large, and promote discontent with reasonably satisfactory results because they fall short of the economists' promised land." Quoted by Joan Robinson in "The Second Crisis of Economic Theory," American Economic Review, Vol 62 (May, 1972) p.11.

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9. For a stimulating analysis of the implications of this phenomenon see Assar Lindbeck, "Stabilization Policy in Open Economies with Endogenous Politicians," American Economic Review: Papers and Proceedings, Vol. 66 (May, 1976) pp. 1-19. His main message "... has probably been that as we have two interacting systems, the political and the economic, we cannot control one with the other, but we must try to redesign them both to improve the stability of each... The main problem is not that we cannot understand analytically what is happening, but rather that the institutional changes and the discretionary policies that are necessary for macroeconomic stability seem to be politically difficult to implement." (Ibid., pp. 17-18).

10. J.M. Keynes, "The Dilemma of Modern Socialism," Political Quarterly, Vol. 3 (April-June, 1932) pp. 155-61.

11. See, for example, T. W. Hutchison, Knowledge and Ignorance in Economics (Chicago, 1977) pp. 30-33, from his essay "On Prediction and Economic Knowledge." Hutchison is the most penetrating current commentator on the limitations of economic knowledge and the implications of this situation for the economists' role in policy-making. See also his Economists and Economic Policy, 1946-66 (London, 1968) and On Revolutions and Progress in Economic Knowledge (Cambridge, 1978).

12. This collaborative project, financed by the Ford Foundation, includes the following countries: Australia (R. Petridis); Brazil (R. Haddad); Hungary (E. Kemenes); India (S. Ambirajan); Israel (E. Kleiman); Italy (F. Ferraresi); Japan (R. Komiya); Norway (T. Bergh); United Kingdom (A.W.Coats); U.S.A. (W. J. Barber). The participants' names are provided in brakcets.

13. For a general survey of postwar developments in Europe and the USA see, for example, Andrew Shonfield, Modern Capitalism (London, 1965).

14. Quoted by Hugh S. Norton, The Employment Act and the Council of Economic Advisers, 1946-1976 (Columbia, S. Carolina, 1977) p. 225.

15. See Trond Bergh's excellent paper, "Ideal and Reality in Norwegian Macro-Economic Planning 1945-65," Scandinavian Journal of History, Vol. 3, 1978, pp. 75-101.

16. A.W. Coats, "Value Judgements in Economics," Yorkshire Bulletin of Economic and Social Research, (November,1964).

17. See reference no. 12.

18. Sir Alec Cairncross, Essays on Economic Management (London, 1971) p. 203.

19. Ibid., p. 210

20. Of the widely quoted remarks by Alain Enthoven: "the tools that we... use are the simplest, most fundamental concepts of economic theory, combined with the simplest quantitative methods... The economic theory we are using is the theory most of us learned as sophomores" - though he added that it often took two years of graduate work before sophomore economics was fully assimilated. Quoted by William R. Allen, "Economics, Economists, and Economic Policy: Modern American Experiences," History of Political Economy, Vol. 9 (1977) p. 73. There are, of course, exceptions to this general dictum, but Allen's article provides a highly stimulating compendium of views on this matter.

21. The implications of this development for bureaucratic practice are revealed in Robert. D. Putnam's analysis of "classical" as against "political" civil servants in Mattei Dogan, The Mandarins of Western Europe, The Political Role of Top Civil Servants (New York, 1975) pp. 87-126.

22. This might be viewed in Ezrahi's terms, as tangible evidence of the prospective "deprofessionalization" of economics in government. Op. cit., p. 22. See also the stimulating essay in this volume by K. Guild Nichols,"The De-institutionalisation of Technical Expertise", op.cit. p. 35. The application of Nichols' and Ezrahi's arguments to economics in the post-1945 period would be a revealing and rewarding exercise.

23. See, for example, the critique in Peter Self, The Econocrats and the Policy Process. The Politics and Philosophy of Cost-Benefit Analysis (London, 1975).

TRADE UNIONS AND TECHNICAL EXPERTISE: THE CONTROL OF ASBESTOS DUST IN BRITISH WORKPLACES*

Kenneth Green and Robert Hudspith, Department of Liberal Studies in Science, University of Manchester

Studies of the place of "scientific/technical expertise" in "public" controversies over social, economic and political and environmental problems caused by proposed technological and industrial developments have tended to exhibit a number of common features: the use of a somewhat undifferentiated concept of "the public" (usually "represented" by various ad hoc citizens or environmental action groups), a concentration on technological problems which are the result of the application of relatively new scientific developments (nuclear power, genetic engineering), a reliance on documentation presented in debates (public inquiries, legislature hearings) specially arranged to thrash out conflicting views with full media coverage.

This paper, however, is concerned with an area of technological regulation which, though continually presenting 'new' problems, has a considerable history - that of occupational health and safety. Because state activity over the regulation of the hazards encountered at work, in the modern sense, dates (in Britain) from the first half of the nineteenth century, it has a well-established institutional framework, with principle legal responsibility for regulation (in the sense of proposing, drafting, administering and enforcing the legislation and safety codes) residing in a state agency (called successively the Factory Department, Factory Inspectorate, Health and Safety Executive (HSE)). And, of course, though being subject to broader public pressures, the disputes and negotiations are, in practice, restricted to three main parties - the agency itself, the employers and the representatives of employees (the trades unions).

The focus of this paper is on one special occupational health problem - the control of asbestos dust since the late 1920s in Britain. It concentrates on the three periods in which the regulations governing the emission of asbestos into the workplace atmosphere were (or are) either being drawn up or revised - 1929-31, 1967-69, 1976-today. Although we discuss the activities and views of the state agency and of the employers

*Report of work in progress.

in the negotiations leading to the revisions, our principal concern is with the role played by the trades unions (usually through their federation, the Trades Union Congress or TUC). Our interest in their role and their use of technical expertise (whether medical, scientific or engineering) in arguing their case for a stricter control of asbestos dust, stems from two concerns. The first, a practical one, relates to problems of the provision of advice in our activities within the Work Hazards Group of the radical scientists' organisation British Society for Social Responsibility in Science, and within a local occupational health and safety information and advice service for trades unionists. The second, a more theoretical and policy oriented one, relates to that body of ideas (see Ref. 30) which has developed around the concept of 'acceptability of risk'. As we show in this paper, the weighing up of the 'costs' and 'benefits' of the strict control of asbestos dust was not posed as a matter to be negotiated as such in the regulation revision process until the 1970s; so, the undoubted right of those bearing the health risk to, at the very least, have a voice in the evaluations of risk being carried out by the asbestos industry's doctors and scientists has only recently begun to be exercised. Whether the form in which this voice began to be heard in the 1970s - through the Advisory Committee on Asbestos - is the most appropriate one for trades unions to operate in, or whether they should seek other means to gain official approval of their policies on the control of asbestos dust (and other hazardous materials), is one of the principal questions our work is hoping to answer. However, we do not offer anything like a full evaluation of the trades unions policies in the 1970s in this paper. This is because our work is still in progress; in particular we have not yet been able to study in sufficient detail the HSE and TUC files on the negotiations of the 1967-69 period (though these files have been made available to us).

It should be stressed that the conclusions and polemics presented in this paper, apart from being provisional, only apply to the situation in Britain; we have made no attempt to draw comparisons with trade union involvement in occupational hazard regulation in other countries, though we are aware of some considerable differences both in the degree of success they have had and in the policies that have been pursued.

I

The processing of raw asbestos began in the UK in 1879 but, though some cases of unusual lung disease in asbestos workers had in 1910 led the Factory Department (as the Factory Inspectorate was then known) to investigate the possible connection between asbestos and such disease, it was not until 1930 that the available evidence of the connection was considered conclusive enough to force an examination of regulations governing the asbestos industry. In that year, Merewether (Medical Inspector of Factories) and Price (of the Engineering Inspectorate), reported on the results of surveys on the incidence of lung disease in workers in asbestos factories revealing that inhalation of dust results in fibrosis of the lungs; that the period of development of the disease varies in direct proportion to the period of exposure and concen-

tration of the dust; that susceptibility of the disease is in no way dependent of the age or sex of the victim. (1) Merewether's and Price's Report made a number of proposals for the control of asbestos dust, emphasising the need for exhaust ventilation, thus controlling the dust as near as possible to its source of origin (as opposed to procedures of personal protection or, for that matter, reorganising the whole asbestos manufacturing process or banning the material altogether). These proposals were seen as considerably progressive for the time.

In subsequent discussions in 1930/31 between representatives of the Factory Department (including Merewether and Price) and of the Asbestos Manufacturers (the 7 main firms in the UK) a hygiene standard was agreed - the so-called "dust-datum". This was taken to be the level of dustiness associated with one of the least dusty asbestos manufacturing processes, namely, yarn flyer spinning, so that if "a particular process appears to give rise to dust in excess of that associated with such flyer spinning ... the need for preventative measures (is regarded) as established". (2) The Factory Department was not completely happy with such a standard as it assumed that there was a critical limit of dust concentration (which post-World War 2 toxicology would eventually call a "threshold") below which workers would not suffer injury, and the Factory Department's medical representatives had no reason to believe that such a 'safe' level existed.* Nevertheless, they decided to view the datum as a provisional working limit and, with the full agreement of the manufacturers, they drafted a code of regulations incorporating the dust-datum concept.

The Factory Department had made a specific decision not to involve the trades unions representing workers in the asbestos industry in the discussions and negotiations undertaken in 1930 and early 1931 between themselves and the employers, supposedly because no trade union "especially representative" (3) of the workers in the industry could be identified. (It is true that, in keeping with the British way, asbestos workers were not organised by one union alone; but since the majority of asbestos workers were organised by only three unions - Transport and General Workers' Union (TGWU), National Union of General and Municipal Workers (NUGMW), Amalgamated Weavers Association (AWA) - one can assume that the Factory Department's reticence regarding trade union involvement was due to reasons other than their concern for proper "representativeness".) Instead, they invited the Trades Union Congress, the umbrella organisation of British trades unions, to comment on the proposed regulations and, indeed,

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* The Merewether and Price report did in fact rate various processes according to their dustiness, indicating how each process compared with a 'base rate' - flyer spinning. However this was intended only as advice for exposition, not as an indication of any desired hygiene standard, though of course it quite easily leads to such.

to meet for discussions with the Factory Department before the draft of the regulations was formally issued.

The TUC's representatives at these discussions were able only to persuade the Factory Department to make minor amendments to the regulations. Two TUC proposals in particular were firmly rejected. Firstly, the TUC asked that if any relaxation of the regulations was to be granted to any factory (say, because though involved in asbestos-product manufacture, this was only in a minor way), the TUC should be informed of such requests. This was argued on the grounds that "it is a reasonable thing to consult the representatives of the workers who after all are the principal persons concerned before any safety regulations are relaxed". (4) The Factory Department was prepared to provide a list of factories who had been exempted after the exemption had been granted, but, presumably mindful of the likely opposition of the asbestos manufacturers and of the disputes over the requests for exemption which would undoubtedly occur, nothing else. Secondly, the TUC representative argued that the specific sections of the proposed regulations which required that exhaust ventilation should be applied only to those asbestos processes which produced dust above the 'datum' level (and therefore not to the yarn spinning process for example) should be extended to all asbestos-using processes, given that the empirical basis - Merewether and Price's recommendations had been based on only 51 measurements of various asbestos dust-producing processes - was not exactly a firm one. The presence of a number of different processes side by side in one factory made it difficult to establish that one particular process was more dusty than another and would lead to very subjective evaluations of whether that process was above or below some notional dust-datum, in the absence of routinely-applicable dust determination procedures. However, following their line that the dust-datum was a provisional standard and that it could be reviewed and refined and the regulations amended as needed, the Factory Department rejected the TUC's recommendations. Any way it was clear that the asbestos manufacturers would not agree to a further tightening of the regulations and their objections would delay the introduction of any controls over the use of asbestos. The Regulations came into force on March 1st, 1932.

From this extremely brief summary of the first intervention of trades unions in the making of regulations for the use of asbestos in factories three points can be made.

Firstly, it is obvious that, as far as the Factory Department was concerned, soliciting the opinions of the TUC was something of a formality. The political climate of the time (a Labour Government in office, the emergence of the policy of tripartitism, supported by big capital and the trade union leaderships, whereby capital, labour and state would seek some consensus on broad national issues, as opposed to the more conflictual relations prevalent in the early and mid 1920s) required some notice

to be taken of trade union views; but all the major matters - what factories and processes should be covered; what hygiene standards to apply for dust levels - had already been agreed on in the Factory Department/ Asbestos Manufacturers meetings.

Secondly, it is interesting to see what sources of technical expertise the TUC drew on in presenting their objections to the proposed Regulations. It seems that their main source was a retired ex-Senior Medical Adviser of Factories (in the Factory Department), Sir Thomas Legge, who had become Medical Adviser to the TUC in 1929. The TUC seems to have relied on him for an interpretation of most aspects of the various areas of knowledge involved in the control of the asbestos hazard - medical, epidemological, ventilation engineering. None of the individual trade unions representing workers in the asbestos industry (NUGMW, TGWU, AWA) seems to have drawn on any expert opinion, though no doubt they were able to draw upon the knowledge of their members regarding the actual position in individual asbestos factories. Since the main points of the proposed Regulations had already been established following the close collaboration between the Factory Department and the Asbestos Manufacturers (whose engineers had been particularly represented in the meetings concerned with establishing the dust-datum concept) any substantial alterations wanted by the TUC would have had to have been based on a detailed criticism of the empirical basis of the Regulations and/or the application of political pressures on the Factory Department through (Labour) Ministers or on the Asbestos Manufacturers directly. The TUC did lobby the Minister in overall charge of the Factory Department but to no avail and, as far as can be ascertained, made no approaches to the manufacturers. Legge presented no extensive critique of Merewether and Price's original analysis. In fact he thought it a very useful report, particularly for its ratings of various asbestos processes according to their dustiness. (5) His only substantial reservation was regarding the omission of any reference to "alternation of employment" (i.e. swapping jobs so as to spend only part of the working time in an area of particular hazard) as a means of reducing the time during which asbestos dust could be inhaled. This relatively cheap method of reducing the hazard for individual workers (assuming, that is, that asbestos-induced lung disease results from continuous exposure to asbestos dust as opposed to only one single exposure) was already required in Regulations governing the industrial use of lead and of carbon disulphide. Although Legge seems to have some reservations about the application of the dust-datum concept in practice in factories, he readily accepted the concept of the 'dust-datum' below which exposure to asbestos dust would cause no harm.

Thirdly, and a corollary of the first two points, the dominance of the asbestos manufacturers in the forming of the regulations is noteworthy. Although the medical facts relating asbestos to fibrosis of the lungs (asbestosis) were established by employees of the Factory Department the actual regulations (or at least the major points to be included in them)

were drawn up by a committee of three engineers in the employ of the three major British asbestos processing companies and two members of the Factory Department's engineering section. The 'dust-datum' concept was devised in this committee, clearly on the basis of straightforward economic criteria. The techniques for measuring dust levels were not very accurate so enforcement of the dust-datum standard would have to rely in practice on visual, subjective evaluations of dust levels. Such uncertainties (coupled with the reservations Merewether and Price had placed on their very limited dust-measurement and medical data) should, one could suggest, have led to a very tight control on all emissions of asbestos dust, or, at least, to regulations requiring exhaust ventilation on all asbestos processes (rather than just those dustier than the flyer spinning ones). It is of course not remarkable that when control measures are being proposed, representatives from the industry to be controlled should seek to minimise the cost of such measures to themselves and should, in areas of empirical uncertainty, err on the side of the least cost. It is perhaps also not remarkable that those charged with the drawing up and enforcement of regulations (in this case the Factory Department) might wish to speed up their negotiations with the industry to ensure that some regulations, however imperfect, can be framed, to be amended later as necessary. However, what is remarkable is that there should be no discussion of the former matter - the economics of asbestos control - with any parties outside of the Factory Department/Asbestos Manufacturers negotiations. The records of the interventions of the trades unions report no such discussions. And since no amendments whatsoever were made to the 1931 Asbestos Regulations till the 1960s then the speed with which the Factory Department accepted the position of the Asbestos Manufacturers cannot be explained merely by their desire rapidly to lay some basis for the better control of asbestos in the future.

II

By the 1960s it was clear that the 1931 Regulations were gravely inadequate. Developments of more accurate, more routine dust measurement techniques and increased medical monitoring of workers in industry - both those involved in asbestos processing and those involved in the use of asbestos products (particularly insulating products) - had revealed a considerable increase in lung disease clearly related to inhalation of asbestos dust. The number of cases of asbestosis (Merewether & Price's "lung fibrosis") was increasing in those parts of the industry covered by the 1931 Regulations and more rapidly in those parts not so covered. Further, a higher than expected rate of lung cancer in asbestos laggers who were also smokers was observed; also, a number of cases of cancers of the lung lining (mesiothelioma)in people exposed to low concentrations of asbestos dust for only short periods had come to light. (6)

The Factory Inspectorate (formerly Factory Department) decided, not surprisingly, that new Regulations needed to be drawn up. The series of

events and negotiations leading to the 1969 Asbestos Regulations* is very complex and is currently under investigation. However from our examination of publicly-available documents so far three points are of particular interest. Firstly, the degree of involvement of medical and engineering experts was much higher than had been the case in the negotiations leading to the 1931 Regulations. The Asbestos Industry side was particularly well-organised. In 1957 the three major UK asbestos companies had set up a cooperative research organisation - the Asbestosis Research Council (ARC)** - "to carry out a programme of research into the causation and prevention of asbestosis". (7) The ARC's research activities from 1957-71 involved sponsoring research projects in a few British University departments to examine the medical physico-chemical and immunological aspects of asbestos-induced diseases, and coordinate the member companies' efforts in dust measurement and control, with research policy being determined by medical officers and scientists from the asbestos companies.

The role of the ARC in the Factory Inspectorate negotiations to change the asbestos regulations is a fairly clear one. On their own admission, they were "fully aware of the content (of the proposed Regulations) long before publication in 1969", (8) and given that they were coordinating the research into the engineering side of dust control they were clearly able to keep the Factory Inspectorate informed of what the asbestos companies thought was technically and economically feasible. In particular it seems that they strongly influenced the Senior Medical Inspector's Panel (set up in 1965 to report to the Factory Inspectorate on the medical aspects of asbestos-induced disease) in their discussion on the appropriate techniques for measuring asbestos dust concentrations. The Panel concluded (in January 1968) that the standard then accepted by the American Conference of Governmental Industrial Hygienists (of 177 particles of asbestos dust per ml of air) was not satisfactory for application in the UK and that a standard based on fibre counts, techniques for the measurement of which had been developed by the British companies, was preferable.

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*The 1969 Regulations extended the FI's activities in asbestos dust control to any manufacturing workplace where asbestos dust was likely to endanger workers' health. In addition, a "Technical Date Note" recommending a specific level of exposure to asbestos dust which should not be exceeded (2 fibres of asbestos per cc of air) was issued. In 1970 a voluntary ban on the importation of raw blue asbestos (crocidolite), thought to be much more hazardous than white asbestos and, in particular, the specific cause of the observed mesotheliomas, was agreed.

** The choice of this name is quite interesting. The title "Research Council" in the UK, though not copyrighted, is usually given to those committees of eminent scientific, medical, social scientific and agricultural research workers who disburse state funds to University departments and special research units. The Asbestosis "Research Council" is funded and managed by Asbestos manufacturers.

Secondly, the bargaining strength of the trade unions in the negotiations should have been somewhat greater than in the 1931 negotiations. The startling inadequacy of the 1931 Regulations, as evidenced by the increases in cases of asbestosis and the fact that only 3 prosecutions of asbestos companies for infringements had been pursued since 1931, could have given them the stick with which to beat the Factory Inspectorate into devising much tighter regulations to control asbestos dust. Exactly how they used this 'strength' and their actual influence on the resultant negotiations is the subject of current work, but even now there are some indications that the trades unions and TUC were unwilling to challenge 'expert' medical opinion, despite the clear (in retrospect?) inadequacy of that opinion. The difficulty they would have faced in doing this is best revealed by a brief examination of the way in which the eventual standard of 2 fibres of asbestos/ml of air (see footnote on p. 159) was arrived at.

The standard was produced by a sub-committee of the committee on Hygiene Standards of the British Occupational Hygiene Society, an organisation of doctors (many of them works doctors) and others concerned with practical occupational medicine in British factories. Of the Sub-Committee's nine members four were from the ARC, one was from the Factory Inspectorate's Medical Section, and the remaining four were either medical researchers or industrial hygienists. At some of its meetings the sub-committee had invited six other people, two factory inspectors, three researchers from the Pneumoconiosis Research Unit (funded by the state-supported Medical Research Council) and a further member of the ARC staff. The ARC representatives made available the results of a study carried out by two asbestos industry doctors of 290 men who had worked at an Asbestos Textile factory for at least ten years (and some for over 30 years). Considerable criticisms have subsequently been made of their data, not least by the doctor who compiled them ("The information was, to say the least, scanty for the purpose, and some of us who were associated with it have become increasingly concerned with the authority with which it has become invested in the international field" (9)). However, the BOHS sub-committee at the time felt able to derive from the data a relationship between the level of exposure to asbestos dust and the observed incidence of asbestosis (though not of any cancers which they specifically excluded from their considerations on the basis of inadequate information). From the resultant "risk-exposure relationship", as they called it, it is possible in principle to estimate (though within what confidence limits it is difficult to know) what percentage of a given population of asbestos workers are likely to contract asbestosis at a given level of dust.

Having as Lowrance puts it,(10) 'measured the risk' they proceeded to 'judge the safety'. As they logically argued, "knowledge of the relationship between airborne dust exposure and the risk of asbestosis is not in itself sufficient to establish a hygiene standard. Another important problem and one which is very difficult to resolve, is that of balancing the risks to health against the consequences of demanding excessive dust re-

duction". However the BOHS sub-committee did resolve the problem, by concluding that "it is reasonable to reduce to 1 per cent the risk of getting asbestosis through having worked for a lifetime with asbestos". This leads to a hygiene standard of 100 fibre years/ml (equivalent to 2 fibres/ml inhaled over a 50 year working life). Why it is "reasonable" to reduce the risk to 1 per cent rather than, say, to 0.1%, given the absence of any recognisable threshold in the risk-exposure relationship is not stated. It is impossible to avoid the conclusion that economic arguments weighed heavily in the balance and that the sub-committee was eager that "excessive (i.e. expensive) dust reduction" should not have to follow from their medical evidence. The major asbestos companies, spurred on by the controversy over asbestosis since the mid-1950s had already managed to reduce dust levels in its best factories to about the 2 fibre/ml level.

The readiness with which the Factory Inspectorate was willing to accept the BOHS recommendation as its standard (the role of the FI representative in the BOHS's deliberations is not publicly documented) is a pointer to the continuation of the close working relationship of the FI and the asbestos industry established in the late 1920s. The trades unions were unable to interfere substantially with that relationship; even though the hygiene standard was clearly based on non-medical criteria, no debate seems to have taken place over the grounds for acceptability of the standard adopted.

III

The principal event in the regulation of asbestos in the UK in the 1970s has been the setting up of the Advisory Committee on Asbestos in 1976. Despite the introduction of the new regulations in 1969 bringing in a hygiene standard for white asbestos of 2 fibres/ml and imposing a gentleman's agreement ban on blue asbestos, the dispute over the hazards of asbestos continued. Numerous medical papers have been published over the last 9 years reinterpreting earlier data or reporting the results of new surveys on asbestos workers. As the effects of high exposures to asbestos dust in the 1940s and 1950s has begun to make itself felt the number of cases of asbestosis has continued to rise. More cases of asbestos-induced cancers have been diagnosed, and the period of exposure to asbestos dust likely to induce disease is now thought by some medical opinion to be extremely short. (12)

In the UK the publication in 1976 of the Ombudsman's report on the Acre Mill asbestos plant at Hebden Bridge, West Yorkshire, where 70 cases of asbestosis were reported up to 1974, brought full public attention on the hazards of asbestos. The report severely criticised the Factory Inspectorate ("probably the most critical official indictment ever of (its) conduct") (13) for failing to give the control of asbestos dust as practiced at the Acre Mill sufficient attention. Different sections of the FI were accused of giving different and inconsistent advice to the factory management. (14) Following the Ombudsman's report the press began

to show great interest in any stories about asbestos - with particular emphasis on the dangers to the general public of asbestos in insulating materials and on waste dumps. The Times group of newspapers published a total of 80 items about the asbestos industry and the hazards of asbestos in 1976 and 1977 compared with a total of 10 in the period 1948-64. There was also considerable activity amongst trade unionists in their workplaces, particularly in those occupations not adequately covered by the 1969 Regulations.*

The greater attention being focussed on asbestos coincided with a radical revision of the whole law governing occupational health and safety in Britain. All the existing agencies (namely, the Factory, Mines and Quarries, Alkali and Clean Air, Nuclear Installations and Explosives Inspectorates) were brought together (in 1975) within one organisation (the Health and Safety Executive) which was now charged with drawing up and enforcing regulations governing occupational hazards in all workplaces. The HSE is overseen by a Commission (HSC) made up of representatives of the TUC, the employers' organisation, and local authorities as well as various individuals knowledgeable about occupational health. One of the Commission's functions is to review the various regulations its Executive administers. The Asbestos regulations were clearly a prime candidate for such a review so in 1976 the HSC established the Advisory Committee on Asbestos to review the risks of exposure to asbestos to the health of people exposed to it as workers and as consumers, and to make any recommendations for further protection. So far (may 1979) the Advisory Committee has published two reports - one on the control of asbestos dust encountered in installing and removing insulating materials(15) and the other on the measurement of asbestos dust. (16) In keeping with the traditions of British public policy-making, the meetings of the Advisory Committee's working groups have been closed. However, in June 1977 the Committee held three days of public hearings. Thirty-four written submissions were submitted and representatives of twelve organisations were questioned by the Committee.** The rest of this section will discuss the submission of the TUC. (17)

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* For sympathetic accounts of some of the struggles conducted by workers over asbestos in their workplaces, see the tri-monthly publication of the Work Hazards Group of the British Society for Social Responsibility in Science, Hazards Bulletin (in particular Nos. 1, 5, 6, and 14).

** The 34 break down thus: 3 asbestos product manufacturing companies; 2 organisations representing asbestos company interests; 9 companies using asbestos products or organisations representative of them; 2 Local Authorities; TUC; one trade union; 3 professional bodies or institutes; 5 pressure groups urging stricter control of asbestos; 2 doctors; 5 individuals.

The principal plank in the TUC's submission was that the manufacture and use of asbestos - in all its forms, whatever their relative dangers - should be phased out; in effect that, eventually, asbestos should be banned. Whereas in the 1931 and 1967-69 negotiations the principal focus of attention by all parties was asbestos-induced lung fibrosis (asbestosis), in the 1970s disputes on asbestos's carcinogenic properties so dominates the medical discussion that the TUC submission only briefly mentions asbestosis. It argues that the 1969 Regulations were not designed to deal with lung and pleural cancers (and were not even effective at dealing with asbestosis) and that the 2 fibre/cc hygiene standard is far too high. In fact there can be no hygiene standard (or threshold limit value*) for carcinogens below which exposures would be safe, claims the submission, so a strategy for controlling asbestos dust which seeks to define such a standard will not lead to the elimination of asbestos-induced disease.

Following from this 'there-is-no-safe-level-for-asbestos' position the TUC submission puts forward a detailed plan to phase out the importation of raw asbestos, the processing of it and the use of asbestos in consumer products and the construction industry. In verbal evidence to the Advisory Committee a TUC spokesperson proposed a four stage phase-out, starting with asbestos-sprayed insulation products followed by asbestos cement and (some time afterwards) friction products and (a long time afterwards) very specialised products. In the short term the HSE would need to tighten up its regulations: to reduce the levels of asbestos dust in factories - the TUC suggests a tenfold reduction in the hygiene standard, to 0.2 fibres/cc, though such a standard should be seen, they argue, as an interim Maximum Allowable Concentration (see footnote p. 159); to ensure that more adequate Medical records of exposed workers are kept; to ban importation of blue asbestos products (as well as the raw fibre); to introduce licensing in the thermal insulation contracting industry, so as to eliminate gross violations of current regulations being perpetrated by small, non-unionised, 'hygienically-unconscious' firms (called, in Britain, "cowboys").

A preliminary examination of the TUC's submission and a comparison with its contributions to the negotiations of 1931 and 1968-69 reveals a substantial change in the relationship of TUC policy making (on occupational

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* The threshold limit value (TLV) of a substance is a measure of the concentration of that substance in the air below which it is believed that nearly all workers may be repeatedly exposed day after day without adverse effect. In the USA, TLVs are set by the American Conference of Government Industrial Hygienists (ACGIH). The UK's Factory Inspectorate uses the ACGIH's TLV limits with some alterations and additions. TLVs are average limits; concentrations above the TLV are permitted so long as they are compensated by balancing concentrations below the level, though there is an upper limit to these variations. In some countries, TLVs are not used; instead there are "Maximum Allowable Concentrations" (MACs) which are straightforward ceilings.

health problems of asbestos at least), to outside 'expertise'. In the previous two periods the TUC seems to have relied heavily on internal medical professionals (its Medical Advisers) for an assessment of the (relatively limited) available medical evidence. And indeed the 1970s Medical Advisers (Dr. R. Murray to 1976 and Dr. R. Owen from then) were closely involved in formulating and arguing the TUC's case on control of Asbestos (Dr. Owen is a member of the Medical Working Group of the Advisory Committee). However the TUC's Social Insurance and Industrial Welfare Department was also in receipt of advice from, and subject to pressure by, a broader constituency of opinion within its member trades unions - both national and local trade union officials and employees of the various trade union's research departments. This constituency has been larger in the 1970s for two reasons - firstly, the widespread use of asbestos products over the last thirty years has increased the number of workers directly exposed at work to asbestos dust, estimated by the TUC as one million in the UK. So, whereas in 1931 two general unions and one weavers' union were involved in the negotiations, by the 1970s the number of unions who have stated a direct interest in asbestos control had increased substantially - the list includes unions representing workers in metalworking, building and demolition, foundry, electricity generation, post office, school teaching, retail and distribution, fire fighting, and even tax-collecting occupations as well as those representing workers in the asbestos process and product manufacturing industries. Many of these unions are particularly concerned about the inclusion of asbestos products in the buildings, etc. in which or on which they work, a concern stimulated by medical evidence of asbestos's hazardous nature which had only partly emerged by the late 1960s and had been hardly hinted at in 1931. Secondly, the occupational health and safety legislation of the mid-1970s had led to a reasonably rapid expansion and reorganisation of some trades unions' research, legal and education departments, to provide information and training facilities for trade union members seeking to exercise their new rights. The people employed in these departments were usually graduates (though not medical doctors) and of a radical political persuasion (and thus, one might suggest, not predisposed to a hearty acceptance of the views of asbestos company and ARC doctors and scientists on the hazards of asbestos) and were in contact with the radical scientists' group, British Society for Social Responsibility in Science.(18)

However, this increase in the number of trade union officials etc. does not account for the particular position on asbestos dust control that the TUC has taken. The origin of that lies in the changed nature of medical and scientific opinion on asbestos-induced disease. Since 1955 when Doll first produced evidence of the relationship between asbestos and lung cancer, evidence indicting asbestos as a potent carcinogen has mounted but it is significant that the most influential research leading to this indictment has not been carried out by either the asbestos manufacturers or by the ARC. In particular two researchers have provided the evidence on which the TUC has based its case - Irving Selikoff of Mount Sinai Hos-

pital, New York and Julian Peto of the Department of Health and Social Security Cancer Epidemiology and Clinical Trials Unit, Oxford. Peto has made trenchant criticisms (19) of the asbestos industry's interpretations of the dose-response data relating to exposure to asbestos dust arguing that there have been serious underestimates of the risks of disease such that on the 'model' he uses, almost 10% (not 1%) of male asbestos workers are likely to die of asbestos-related disease after a 50 year exposure at a level of 2 fibre/cc. Further, he presents evidence to link white asbestos with pleural mesothelioma, whereas received asbestos industry opinion, except for one manufacturer (the one-time main importers of blue asbestos), accepts only that blue asbestos causes (pleural and peritoneal) mesotheliomas. Selikoff was one of the first to discover the high incidence of lung disease in American asbestos insulation workers and has revealed the higher than average rate of cancer (of lung, pleura, stomach, colon and rectum) in such workers. (20)

In basing its evidence to the Advisory Committee on such 'expert advice' it can be seen that the TUC's position on asbestos control illustrates well one of Nelkin's generalised propositions relating to controversies involving conflicting technical expertise - namely "the extent to which technical advice is accepted depends less on its validity and the competence of the expert, than on the extent to which it reinforces existing positions". (21)* Certainly, the rise in asbestos-related disease, the patent inability (or refusal) of the regulatory authorities to enforce the legislation controlling asbestos from the 30s to the 60s and the changed climate of opinion amongst workers regarding work hazards have certainly predisposed trades unions and the TUC towards doubting scientific opinion in the employ of or sponsored by the asbestos industry. However, at least in its evidence to the Advisory Committee, the TUC seems to have gone further than might be suggested by Nelkin's proposition (i.e. if the evidence to support our position hadn't existed, it might have been necessary to invent it), in rejecting as irrelevant much of the scientific dispute over the toxicological model that forms the basis of the hygiene standards applied by the Factory Inspectorate to British workplaces - the threshold model. This model presents the view that for almost any substance there is a certain concentration - the threshold - below which the natural detoxifying and immunological mechanisms of the human body can cope without any deleterious effect (short-term or long-term). Other models, pointing to the fact that over the last few years the thresholds

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* Unfortunately, some of her other propositions are rather difficult to test in this case, at the moment anyway, since much of the debate subsequent to the Advisory Committee hearings has taken place in private. Expert conflict then, as it takes place over the nitty-gritty of hazard control has not been sufficiently public to establish whether "conflict amongst experts reduces their political impact" or "those opposing a decision need not muster equal evidence".

of many substances (e.g. vinyl chloride, benzene) have been reduced as evidence has accumulated that low concentrations cause disease in the long term, have argued that, for many substances, there is probably no 'safe' level of exposure and that cancers in particular can be 'triggered' by only one molecule of some appropriate chemical. (22) The TUC refers to this dispute in stating "disagreement exists amongst various medical authorities as to whether a safe level of or exposure to carcinogens exists, and if it exists what the level for asbestos fibres should be". (23) But, it argues,, there are "more practical reasons for objecting to hygiene standards or TLVs for carcinogens", namely the sheer technical difficulty of measuring and eliminating low concentrations of asbestos dust. The only solution to these practical objections to reducing the TLV for asbestos is not, as the asbestos industry has argued, to keep the existing standard, but to proceed to stop the use of asbestos products.

Such a position extends the dispute into regions which the TUC, at least regarding asbestos, has not entered before. As we have pointed out in our discussion of the negotiations leading to the 1931 and 1969 Regulations the economics of asbestos control was never an explicit item on the agenda. In 1931 it was 'diffracted' through apparently complicated engineering problems in the committee which established the dust-datum concept. In 1969, the same thing happened, but in a much less disguised way in the BOHS sub-committee on hygiene standards. In the 1970s however, economic questions related to the use of asbestos have become much more important. Since the 1930s the range of products incorporating asbestos has increased considerably; in 1920 200,000 tons of asbestos was mined, in 1971 4½ m tons. (24) The British asbestos industry is now worth about £200 million per year and employs 20,000 people; another 100,000 people come into regular contact with asbestos. (25) These facts are constantly used by the asbestos industry to silence its critics - "It is doubtful if even an immediate and total ban on all asbestos products would save any lives. A ban would be certain not only to lead to extensive unemployment, disrupt the economy and cause further inflation but actively to cause a substantial number of deaths (due to road accidents, for example, brought about by the use of less efficient asbestos-free brake pads - KG/RH). Any balanced judgements on the advantages and risks entailed in the use of this unique natural resource must surely be that it is an essential ingredient of modern life and that our efforts should be directed not to banning it but to seeing it is always used safely." (26)

Such views have considerable support amongst those workers most likely to be affected by any rapid reduction in the consumption of asbestos - namely those employed in factories processing asbestos. As one of them put it at the Advisory Committee hearings, where he was a member of the Asbestos Cement Manufacturers Association delegation,

> ... I have worked in the (asbestos) cement industry for 32 years, so I am speaking from personal experience. I am concerned at the widespread loss of jobs which would result from lowering of the present (hygiene) standard in factories and on construction sites. This would be impossible to achieve and would lead to the closing of factories." (27)

In response to the specific question of the disease risks of asbestos, the asbestos industry agrees that in the past dust levels have been too high and have been the cause of undesirably high levels of asbestosis but that nowadays the risk is negligible. The TUC's submission, by presenting a programme for the substitution of asbestos products by safer materials, following from medical evidence which predicts significant morbidity and mortality for even low levels of asbestos exposure, clearly rejects this reasoning. The question remains, however, how is the dispute to be resolved? The asbestos industry argues that, in effect a 'cost-benefit' balance has been achieved in that the risk of death from asbestos-induced disease is less than the risk of death which an asbestos-free society would experience and that the costs of further control of asbestos emissions would result in economic disadvantages in terms of reduced employment and profits, though quantification of this complex balance, not surprisingly, has not been forthcoming. (28) The TUC obviously thinks that the balance has not yet been achieved, and is attempting to use the Advisory Committee on Asbestos (both in public and in its private meetings) to carry out what is an extremely complex social, economic and 'human life' calculation.

One of us has argued elsewhere (29) that while it is essential in the control of hazardous materials in the workplace for those experiencing those hazards to be included, as of right, in the negotiations between state agencies and employers over, say, hygiene standards, as is argued by those who consider that risks to health can only be considered "acceptable" if those who have to bear them so agree, (30) this is not sufficient. Groups of workers should be encouraged to include the control of hazardous materials in their normal bargaining procedures to seek better standards than the (guranteed) state minimum. Though this extension of the bargaining process has much to recommend it, it does mean that the better organised, better informed and more powerful workers would be able to improve their working conditions regardless of any broader social disbenefit. However the need for some institutional mechanism where such conflicts can be resolved does not mean that it will be easily forthcoming. And one of the principal obstacles to its arrival is the manner in which technical experts abrogate to themselves (at least up to now in occupational health and safety) the right to measure the risk and judge the safety.

The fact that the trade unions, through the TUC, and perhaps half-heartedly, are attempting (if the asbestos case is to be given typical status) to change the terms of their intervention in the negotiations over the regulation of hazardous materials, could indicate that that right is being challenged, not by loose coalitions of 'citizens' or by the disorganised 'public', but by organisations who by virtue of their power over productive activity, have, potentially, more considerable means of political persuasion. It is our opinion that whatever the results of that intervention in this case, the attempt should be approved of and assisted.

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SINCE SILENT SPRING: SCIENCE, TECHNOLOGY AND THE ENVIRONMENTAL MOVEMENT IN THE UNITED STATES*

Robert Cameron Mitchell, Resources for the Future, Washington D.C. and The Pennsylvania State University

Modern environmental problems share a set of common circumstances: the side effects of modern science and technology are one of their major causes; they are themselves highly technical in character; and their causes, solutions and potential effects are often the subject of controversy between scientists. Moreover, because of the need to mitigate environmental damage and to avoid potential disaster, public policy on environmental issues must be decided and implemented in the face of scientific uncertainties. Alvin Weinberg has called such issues "trans-scientific" - they are at once scientific yet incapable of being unambiguously resolved by timely scientific experimentation. He rightly argues that the debate over such issues cannot and should not be limited to scientists and experts (Weinberg, 1972:211).

The obstacles to meaningful public participation on issues of this kind are obviously formidable. First and by no means the least of the obstacles is that citizens need to become aware that a particular problem exists. Once aware of the problem they have to acquire sufficient knowledge of the points at issue to make a judgement about its severity, cause and solution. Then they need to be motivated to try to do something about the problem instead of thinking that their efforts can't make a difference. Finally, in order to influence policy they need to make their views known to the policy makers in a timely and effective manner.

In the United States, the major debates about environmental policy during this past decade ultimately have taken place at the national level either in Congress over legislation or, once legislation has been passed, in the federal regulatory agencies over rule-making and enforcement. Well

* This paper is a revised version of the one delivered at the conference. I have benefitted greatly from the detailed comments provided by Clifford S. Russel and Arie Rip.

organized economic interests typically have a high stake in the environmental status quo and the capability to bring considerable technical expertise to bear on their side of the issue. Moreover, as we will see in the case of the pesticide debate, some of the government regulatory agencies charged with responsibilities that affect the environment are predisposed towards the business point of view on these matters or are vulnerable to pressures from these interests.

Despite the obstacles, the environmental movement in the United States has enjoyed considerable success in influencing environmental policy over the past decade. There are a number of reasons for this: the rising tide of citizen activism in the 1960's, the high level of public concern about environmental questions throughout the decade (Mitchell, 1978a, 1978c), the development of citizen supported "public interest" lobbies (1) and law firms (2) as new institutional forms, and the environmental groups' ability to use scientific expertise in an effective manner.

In this paper I will analyze the last of these factors. After a description of the emergence of modern environmental issues and the contemporary environmental movement I will consider four interrelated questions: 1) How do environmentalists obtain access to scientific and technological information? 2) How do they use this information once they acquire it? 3) What does this reveal about their fundamental assumptions regarding science and technology, the American political system, and risk? 4) What judgement may be made about their contribution to the public's understanding of these scientific debates and their representation of the public's interests in these matters? The principal example I will draw upon is the debate over pesticides.

I

> The supreme conservation achievement of this century (was) the fashioning of an almost self-renewing source of energy by the atomic scientists...
>
> Stuart Udall, The Quiet Crisis (1963:174)

Citizen's groups in the United States have been concerned with environmental problems since the beginning of this century and before. The first generation of issues involved disputes over the management of natural resources such as forests and water, the preservation of especially important natural areas in parks and refuges, and the effect of gross pollutants on rivers. The conservation movement consisted of citizens' groups and prominent individuals who opposed the "rape, ruin and run" philosophy of those developers and citizens who carelessly exploited America's natural resources with little regard for the side effects of their actions or for the needs of future generations. This movement persisted until the early 1960's when it became a part of the environmental movement.

Despite its alliance against the exploiters, the conservation movement contained two quite distinct schools of thought; the conservationists - who were oriented towards utilitarian "wise use" of natural resources - and the preservationists - whose emphasis was on preserving certain natural systems and beings from use entirely (Hays, 1972:141 ff). The classic confrontation between the two occurred in the early 1900's when conservationists such as Gifford Pinchot, the founder of the United States Forest Service and the forestry profession in the U.S., supported the flooding of the beautiful Hetch Hetchy valley in California's Yosemite National Park so that San Francisco's water supply might be ensured (Hays, 1972:192-195). This project was fought by a number of preservationists led by John Muir, a founder of the Sierra Club and its first president, who argued that the valley's priceless beauty would be irretrievably lost and that equally suitable locations were available to serve San Francisco's needs for water. This battle was finally lost by the preservationists in 1914 when Congress voted to authorize the reservoir although they won other battles such as the fight over whether timber in the national parks should be harvested or not.

Viewed from the perspective of today's environmental debates about the effects of toxic chemicals, the safety of boiling water nuclear reactors, the linear hypothesis about radiation effects on human cancers, the commercial feasibility of solar cells, and the likelihood that rDNA research will have adverse effects on human health, the early environmental issues seem reassuringly simple and straightforward. These first generation environmental issues, share the following characteristics:

1. They had a specific locus in the natural environment. Sometimes this was an area, such as the Hetch Hetchy valley, at other times the locus was one or more species of plant or wildlife such as the song birds which the Audubon Society worked so hard to protect from hunters and the mature trees which the defenders of the Adirondack Park in upstate New York saved from harvesting by the state.

2. The consequences of the environmental threat were relatively immediate and unproblematic. If harvested a forest would lose its mature growth; if overgrazed the land's cover vegetation would be severely damaged; if submerged in a reservoir a valley would be irretrievably lost.

3. The causes of the problems which concerned the conservationists and preservationists were direct and unambiguous, such as: sheep herders overgrazing pastures, sport hunters slaughtering wildlife species to extinction, development interests using resources in such a way that undue waste resulted or that the uses favored by preservationists were foreclosed.

4. For the most part they did not involve controversies between experts nor was a high level of expertise required to understand them. I

say " for the most part" because some of the disputes over resource use in the West did involve knowledge about arid lands and Pinchot's argument for the national forests was predicated in part on the superiority of scientific forestry, through which sustained yields of forest products might be obtained. In these cases, however, the controversies simply involved the application of a new kind of expertise to a situation where no kind of expertise, even a folk expertise, was used to rationalize the existing situation.

5. The conservationists' and preservationists' proposed reforms did not pose a fundamental threat to powerful interests or to the citizenry's way of life. Those threatened by a conservationist victory were either local economic interests such as herders or loggers; hunters or fishermen whose wasteful behavior was becoming obsolete with the advent of game scarcity; or relatively small industrial corporations which lacked the capacity to avoid "wasteful competition" (Hays, 1972:266).

6. The costs of implementing their reforms were relatively small and some of them, such as the government's management of the national forests and fish and wildlife for sportspeople, were economically self-sustaining.

Neither the conservationists nor the preservationists of this time attacked the scientific and technological underpinnings of American society. Indeed,the conservationists wholeheartedly embraced scientific knowledge and were optimistic about technology's beneficience for humanity provided it was utilized through public management and by careful planning. They believed that resource decisions were best made by experts under central direction (Hays, 1972:266) and the professional resource managers in the Forest Service, the Bureau of Land Management, the Tennessee Valley Authority were strongly motivated by a common belief in the superiority of their expertise and the unequivocal social good which would result from their stewardship.

The preservationists, for their part, found this world view arrogant and distasteful since it failed to accord nature any status other than as a source of resources for human use. Inspired by a transcendentalist philosophy, they believed that nature also had values as a source of inspiration and renewal (Fleming, 1972:8). Although many of them felt that modern civilization was too artificial and spiritually arid, the preservationists' critique was not directed against science and technology as such, but against its misuse. In common with their successors, the modern environmental movement, they especially valued the scientific knowledge of biologists and naturalists, many of whom played active roles in the preservationist movement. (3)

As late as the early 1960's sensitive conservationists such as Stewart

Udall, President Kennedy's Secretary of the Interior, could wax enthusiastic about such high technology fixes as oil shale conversion and atomic energy. (4) But Udall also noted: "Ironically... these very successes of science have presented a new set of problems that constitute the quiet crisis of conservation" (1963:175). Even as his book was published, the quiet crisis of conservation was being transformed into the not-so-quiet ecological crisis which was to inspire/instigate a new citizen's movement, the environmental movement. A key event in this transformation was the publication in 1962 of Silent Spring.

II

...science has, indeed, gotten out of hand.

Barry Commoner, Science and Survival (1963:128)

Subscribers to the Book of the Month Club are regularly treated to a "review" of its new selections in the Club's periodical. In October 1962,the selection was the new book by Rachel Carson, a biologist turned nature writer whose earlier books had received wide acclaim. "Certain to be history-making in its influence upon thought and public policy all over the world" trumpeted the review. In this case, at least, the Club's hyperbole was reasonably accurate. Not only was Carson's book a persuasive, eloquent and scientifically respectable (5) indictment of the indiscriminate use of pesticides; it was also a profound scientific-socio-political critique of modern American society's technological Achilles' heel. Carson's graceful prose introduced the reader to the second and third order eco-system effects induced by technology's newfound ability to create substances not known in nature and how these effects altered the complex balance of natural systems. She described how the growth of the scientific specialization after the Second World War resulted in the loss of a holistic understanding of natural systems; how the needs of industry shape scientific research agendas and monopolize expertise; how the persistent pesticides popularity with farmers stems from the same chemical characteristics which cause environmental havoc; and how the Department of Agriculture and the Forest Service ignore the mounting evidence of the harmful side effects of pesticides and herbicides because of their scientific establishment's single-minded devotion to producing more and more despite the fact that the United States at that time was suffering from crop surpluses rather than crop shortages. In order to widen her book's impact, she was careful to link the catastrophic effects chlorinated hydrocarbons have on wildlife with their potential effects on human health and to draw the parallels between radioactive fallout, which was already recognized as a hazard, and the widely dispersed pesticide residues.

Woven through her book is a counter-science based on a biology of whole systems and a respect for nature's balance. Control of insects is needed,

she argues, but control "based on understanding... the living organisms they seek to control, and of the whole fabric of life to which these organism belong" (Carson, 1962:244). The new science of biological control which she commends again and again, offers "The Other Road," an alternative to the smooth superhighway of organic chemistry but, interestingly, one no less scientific nor less peopled with specialists. (6)

Much of Silent Spring appeared in the New Yorker Magazine in the summer of 1962. It immediately stimulated widespread comment by politicians and by the media who were especially sensitive to its message owing to its appearance shortly after the thalidomide tragedy had filled the front pages of the newspapers. By Christmas, three months after its publication, the book had sold 100,000 copies and was a cause celebre thanks to a massive attack on the book and its author's scientific credibility by the pesticide industry (Graham, 1970). As we shall shortly see, the public controversy over pesticide use which it ignited has turned out to be as persistent as the compounds which she sought to warn the public about.

Carson's most important achievement, however, was her success in making the public aware of a new set of environmental problems. Many conservationists were already all too knowledgeable about the peril from pesticides and nuclear testing fallout, and the Audubon Society had tried to publicize the dangers of the former as widely as possible, but thus far, they had been unable to overcome the second generation problems' inherent liabilities as objects for public concern. In contrast to the problems which activated John Muir and Teddy Roosevelt these second generation problems are 1) often not tied to a specific place or species; 2) their effects are subtle and take years or even generations to make themselves manifest by which time it is too late to avoid irreversible harm; 3) their causes require imaginative detective work of the highest technical skill to identify; 4) they often involve intense scientific controversy; 5) their amelioration threatens whole industries and patterns of social life, such as Americans' use of the automobile; and 6) quite apart from the social and economic disruption associated with substantive environmental reform, the price tag for solving these problems is billions of dollars.

Much of Carson's critique was echoed by another biologist-writer the next year when Barry Commoner - who was to become a leading scientist spokesperson for environmental reform in the years to come - published his first book, Science and Survival (7). Commoner's special issue was the threat to human health from radioactive fallout, but his book links this issue to the wider environmental threat and to the question of what scientists' responsibility for guiding technology should be. Given technology's new capacity for catastrophic mistakes, he proposes that scientists are now bound by a "new duty".

> We have the duty to inform, and to inform in keeping with the traditional principles of science, taking into account all relevant data

> and interpretations. This is an involuntary obligation to society; we have no right to withhold information for our fellow citizens, or to color its meaning with our own social judgements. (1963:129)

Carson's and Commoner's writings are models of "informative science". Each identifies an environmental problem and provides a full briefing in lay language of the scientific issues involved. Rejecting the notion of decision making by a scientific priesthood, they place their faith in the wisdom of the public to choose the appropriate mix of risks and benefits offered by modern technology once the public is given access to reliable knowledge. To their mind the public had been misled by half truths and false assurances for too long. As Carson put it:

> It is the public that is being asked to assume the risks that the insect controllers calculate. The public must decide whether it wishes to continue on the present road, and it can do so only when in full possession of the facts. (1962:23)

Commoner's ventures in informative science went beyond his admirably lucid writing. In the late 1950's he was a founder of the St.Louis Committee for Nuclear Information, the first of what became a network of local scientists' groups whose purpose is to identify important issues of public policy that relate to science and to provide reliable information about those issues to local citizens. These groups are the beginning of what has come to be known as public interest science. They interpreted scientific information about fallout and nuclear disarmament and even gathered original information by analyzing strontium-90 in children's baby teeth. Following the nuclear test ban treaty of 1963 the committees substituted "environmental" for "nuclear" in their name and their bulletin, also rechristened, became a reliable source of environmentally oriented scientific information about second generation environmental problems.

Owing to their unanticipated and unintended side effects science and technology were regarded as having "gotten out of hand" by Commoner and Carson. They and the environmentalists they inspired believed that informative science, by communicating the nature of the problem and its seriousness, could inspire citizens to press for reform. Somehow scientists acting as publicists and teachers to the citizenry could, without compromising their scientific integrity, serve as a catalyst for policy change. But once informed, citizens need to make their views known to policy makers in a timely and effective manner if they are to influence public policy. Since farmers, agribusiness and the pesticide industry all had an important economic interest in the continued availability of pesticides and were active in bringing pressure to bear to protect their interests, citizens would have to find a means of bringing counter pressures. How were they to do this? This was one of the questions begged at the time by Carson and Commoner.

III

Sue the bastards!

Vincent Yannacone, Attorney, and co-founder of the Environmental Defense Fund.

It took ten years before DDT was finally removed from use for most purposes. Ironically, the peak year for the application of DDT in the U.S. was 1959, three years before Silent Spring was published, when nearly 80 million pounds were applied (Whitaker, 1976:132). Thereafter its use slowly declined not because of government action but because insect immunity to DDT caused many farmers to turn to other (equally longlasting) pesticides. The chemical industry never let up on its crusade to keep DDT in use, however, owing to DDT's special status as a symbol (progress through chemistry vs. industrial carelessness and arrogance) and as a potential precedent (if DDT went the other chlorinated hydrocarbons in wide use such as aldrin/dieldrin might go too).

The major reason for the final defeat of the chemical industry and its allies in Congress over DDT was the development of an innovative social institution,the environmental law firm, which was able to exploit new forums for scientific debate and to marshall scientific expertise to support the case against DDT in those forums. The modern American environmental citizen groups with their emphasis on lobbying and their use of full time professional staffs also came into being during this time as well. Together the law groups and the lobby groups constitute an answer to the question begged by Carson and Commoner as to how citizens can meet the challenge presented by the second generation environmental issues. Through these groups, environmentalists shifted from the informative to the advocacy use of science. By supporting these groups with their contributions, United States' citizens in effect employ professional advocates who command sufficient expertise to be effective in representing the citizens' interests on these trans-scientific issues.

Table 1 summarizes the post-Silent Spring policy struggle over the future of DDT. The story is quite complex owing in large part to the United States' federal system of government, where states have considerable powers and where at the national level there is an intricate balance of powers among a bicameral legislature, the executive and the courts. Ultimate victory on this issue boiled down to securing a decision by the relevant regulatory agency of the executive branch of the government to remove DDT from its list of registered pesticides. Under the legislation in force such a decision had always been within the discretionary power of the relevant administrators (first in the Department of Agriculture and later, after the enforcement duty was transferred, in the Environmental Protection Agency) but they had chosen not to make it because, in their judgement, DDT's benefits outweighed its environmental harm.

Substantive progress towards the ultimate goal began only when the matter was brought to court. (8) Vincent Yannacone, a Long Island resident and attorney, brought suit against the Suffolk County Mosquito Control Commission's local use of DDT for mosquito control. Yannacone enlisted the enthusiastic technical aid of some respected biologists who worked at local educational and research institutions and financial support from the Audubon Society's Rachel Carson Fund. In a series of cases on Long Island and in Michigan he was successful in persuading the courts to hear the case thereby gaining public forums where the scientific evidence relevant to the DDT controversy could be aired in depth through an adversarial proceeding. Yannacone and the scientists soon formalized their association by founding the Environmental Defense Fund (EDF) in 1967.

In 1968 they used another type of adversarial fact-finding forum - a hearing in Wisconsin before an expert examiner who was to determine whether DDT should be declared a pollutant according to state law. This celebrated six month hearing pitted the combined expertise of the pesticide industry against the expertise of Yannacone's scientific colleagues and the many scientists who they were able to persuade to testify in their behalf. The very high quality of the EDF scientists, their meticulous preparation for the case and Yannacone's exceptional ability to draw out scientific evidence through cross-examination put the industry position on the defensive. EDF also publicly questioned the effectiveness of federal regulation since some federal regulators were called on the stand to testify about the basis for their continued registration of DDT. This case received considerable publicity and even before the favorable verdict was announced in May 1970 a number of states passed laws which restricted the use of DDT.

EDF next moved to the federal level. A few years earlier two very important court decisions had liberalized the rule of standing making it possible for citizen's groups to seek judicial review of administrative decisions even though they lacked an appreciable economic interest in the matter. (9) EDF was accordingly granted standing for its challenge to the USDA and, subsequently, to EPA. As shown in Table 1 the court eventually held that since EDF had raised substantial questions about DDT's safety it was not sufficient for EPA informally, behind closed doors, to decide the evidence was insufficient and to refuse to hold cancellation hearings. The court therefore ordered EPA to institute formal cancellation proceedings. In the court proceedings and in the six month long EPA public hearing before an examiner EDF again marshalled its scientific anti-DDT road show and debated its industry and Department of Agriculture adversaries. Despite an adverse recommendation by the examiner, whose conduct during the hearing allegedly showed his strong prejudice for the pro-DDT position, and strong pressure from powerful agriculturally-oriented Congresspeople one of whom presided over EPA's budget requests in the House of Representatives, the EPA Administrator

ultimately decided to remove DDT's registration some seven years after EDF began its litigation campaign. The determining factors behind the Administrator's decision against DDT seem to have been: 1) the strong campaign by the environmentalists who were EPA's major constituency and 2) what appeared at the time to be strong evidence that DDT was a carcinogen. The Mrak report had suggested that this was a strong possibility.

While the victory over DDT was precedent setting, it did not clear the way for quick decisions about the other chlorinated hydrocarbons which had been indicted in the Mrak report of 1969, or for the other pesticides and herbicides in use for which there is evidence of carcinogenicity. For example,EDF's litigation to ban aldrin/dieldrin on the grounds that they composed "an unreasonable risk of cancer in man" took a total of five years before the pesticide was banned in 1974. This battle like its predecessor involved prodigious use of legal and scientific resources. The thirteen month long administrative hearing pitted seven lawyers for the manufacturer and three lawyers from the Department of Agriculture against two EPA and two EDF attorneys.

IV

> The feverish pitch of Earth Day was to pass. By 1976 the environmental movement had not gone away but,... had begun to succeed, at least to be institutionalized...
>
> John C. Whitaker, Nixon administration environmental official (1976:25)

By 1979 the United States environmental movement has become well institutionalized. At the national level (10) the principal environmental lobbies consist of two types: the organizations stemming from the earlier conservation movement, such as the Sierra Club, which had made the transition into environmental lobbies (11) and organizations such as Environmental Action which were specifically founded to address the second generation issues. EDF has been joined by a second environmental law group, the Natural Resources Defence Council (NRDC), and by litigation organizations associated with two of the largest environmental lobbies, the Sierra Club and the National Wildlife Federation (NWF).

These national groups, which are listed in Table 2, enjoy widespread public support. According to a public opinion poll I conducted in 1978 16 percent of adult Americans consider themselves to be "active participants" in the environmental movement while a further 44 percent are sympathetic to the movement (Mitchell, 1978b). Less than one percent actually belong to the national groups, however,but this amounts to one million people (taking into account the overlapping memberships since many people support several groups). Between member contributions, donations from wealthy individuals, foundation grants, and sales of publications these groups' income in 1976 amounted to 67 million dollars.

The range in income varies enormously as shown in the table where the National Wildlife Federation's 22.6 million dollar income is seen to be 61 times greater than Environmental Action's.

By commanding such financial resources the groups are able to employ full time professional staffs to work on policy issues. Normally the salaries paid are less than those paid by government or industry for someone with comparable skill and experience, but they range upward to above $30,000 for experienced attorneys. The growth in number of full time paid professional staff is one of the major developments in the movement since the early 1960's. (12) It is only by having such staffs that the groups are able to have a significant influence on how regulatory agencies such as EPA, the Forest Service, the Bureau of Land Management, the Food and Drug Administration, the Consumer Produce Safety Commission and the Department of Energy carry out the responsibilities entrusted to them by legislation. Volunteers - members who donate time to the organization - are vital to the operation of an organization such as the Sierra Club and are important, to a lesser extent, in most of the other groups.

With the exception of the two independent environmental law groups and the Massachusetts Audubon Society (a large state environmental group which is independent of the National Audubon Society), the environmental group staffs include very few professional scientists (13) or economists. (14) Massachusetts Audubon has always stressed the importance of science in its work and its scientific staff consists of a full time biochemist and a half-time physicist. EDF and NRDC are staffed by lawyers and scientists with a ratio of about two lawyers to each scientist for a total of some 10 full time staff scientists and economists whose specialities range from biochemistry to geology and whose policy areas are principally toxic substances, nuclear power and energy policy more generally. The two organizations have evolved a remarkable similar way of utilizing their in-house scientific capability considering the fact that EDF was selfconsciously founded as a scientists group with litigative capacities while NRDC was founded as a law firm which drew on scientific expertise.

In addition to their educational work, which creates a basis of public support and awareness for their cause, the national groups engage in three other forms of activity: lobbying, participation in administrative decision making, and litigation. Lobbying involves efforts to influence legislation in Congress and the states. Ten years ago there were perhaps two or three full time environmental lobbyists in Washington while today there are twenty-five to thirty. Participation in administrative decision making refers to a wide range of activities by which the environmentalists seek to influence the implementation of the many important environmental laws which have already been passed. These include: critiquing environmental impact statements,commenting on proposed regulations, participating on scientific advisory committees, providing information to the agencies, testifying at administrative hearings, and petitioning for

regulatory action. Since many of the environmental laws leave a great deal of discretion to the regulators in setting standards and enforcing them and since administrative law in the United States has been moving towards a system of interest representation (in no small part due to court victories by environmentalists) whereby agency bias towards the regulator is overcome by the provision of representation to all interested parties (Stewart, 1975) this has become an especially important arena for policy influence.(15) Litigation is intimately related to administrative decision making since the courts are the environmentalists' last resort when they are unable to get an administrative agency to fulfill what the environmentalists consider to be its legal duty.

The range of issues about which environmentalists are concerned is vast. In addition to pesticides and other toxic substances such as asbestos and lead, the second generation issues include problems associated with chemical fertilizers, water pollution, air pollution, solid wastes, nuclear energy, recombinant DNA research and drinking water. (16)

The context of the policy debate is typically one in which private interests such as business firms, manufacturers and farmers seek to minimize environmental regulation of their activities. These interests sometimes enjoy the support of labor when labor believes that the environmental regulations may affect jobs. Environmentalists often refer to their opponents as "the enemy" and view themselves as underdogs in the struggle to influence policy because of the greater resources commanded by their opponents.

The technical questions in the contemporary environmental debates center on benefit/cost issues. For example, in the DDT debate these included: What is the effect of DDT on wildlife? How much of a threat is it to human health and to the biosphere? How reliable are its substitutes? What will be the losses to the U.S. cotton production if DDT is given up? Among the technical issues treated at length in the Scenic Hudson hearing (the issue before an examiner concerned a proposed pumped storage electric facility on the Hudson River which threatened to diminish the considerable natural beauty of the area) were: How much electric generation capacity did the utility require in the future? How much could the utility rely on other utility systems through interconnections to meet its peak power needs? What is the appropriate method of determining the cost of electricity? (Sax, 1970:133)

Underlying these technical questions are fundamental assumptions about the nature of the good society. It is important to note that for the most part environmentalists and their opponents appeal to the same set of values. For business these include: equity (regulation leads to higher costs which especially affect those with lower incomes), individual freedom (regulations restrict individuals' freedom of choice), efficiency (government interference leads to waste and threatens free enterprise),

security (from shortage and recession) and prosperity. Environmentalists also appeal to equity (large corporate interests shape policy to meet their needs not those of individuals, the interest of future generations are ignored by industry's short term profit prospects), individual freedom (from being exposed to toxic substances without one's consent), efficiency (in the sense of the conservation of resources for their highest need), security (from unnecessary risk to health and well being) as well as beauty. Finally, while there is a small segment of the environmental movement which harbors fundamental doubts about the validity of scientific knowledge and believes that debate on the scientific factual level ignores important aspects of reality to the point of distorting the issue (Devall, 1979), the national environmental groups share with industry the view that scientific knowledge can lead to conclusions which are useful in evaluating the benefit/cost of modern technologies. Both accept the methodology of science, have a basic respect for the institution of science, and believe that disputes can be illuminated if not resolved by the introduction of the best possible data. This is not to say that environmentalists view scientific knowledge as the only source of truth or that they uncritically accept all aspects of the current institutional manifestation of American science. It is to say that they respect science sufficiently to argue their case on scientific grounds on issues which have a scientific/technological dimension.

Thus for the most part the differences between environmentalists and their opponents are not about values so much as about how those values can best be realized. Only on the question of the desirability of rapid economic growth do environmentalists differ from their opponents on something approaching the value level with environmentalists being inclined towards a conserver society - where technology's adverse side effects are minimized through careful assessment prior to implementation - and slowed economic growth, while their opponents prefer a high growth economy whose shape is determined by conventional market forces and where technological innovation is affected by a minimum of regulation.

V

> If you want to define government, it's decision making with inadequate data.
>
> Governor Jerry Brown of California when pressed on a question about energy by Sierra Club members, May 7, 1977.

We now come to the specific questions cited at the beginning of this paper which asked how environmental groups acquire and use scientific (17) information and expertise. Let us begin by making a distinction between the informal and formal use of data.

By the informal use of scientific and technical findings I refer to the

"Give me a number" or "Have you got a study that shows..." syndrome which is commonly encountered in the lobbying situation. In this kind of situation the data are typically undocumented, and taken out of context to legitimate positions rather than to justify them. (18) These data, however abused, must not be fabricated or grossly misleading else the lobbyist is likely to lose credibility. The informal use of data occurs often in the final days before a vote on a piece of legislation when lobbyists on both sides and their congressional champions engage in a thrust and parry of isolated facts which purport to show this effect or that cost.

The formal use of data, in contrast, involves the documented systematic use of studies to justify a position. Its practice requires expertise although not necessarily formal scientific training. Occasionally the legislative process permits the systematic use of data by competing interests as when hearings are held in preparation for drafting a bill, but the administrative decision making process and litigation require it. In what follows I will restrict my discussion to the formal use of data in the United States' administrative forums as this presents the greatest challenge to public participation in the formulation of trans-scientific policy.

The formal use of scientific information in these policy arenas is similar to scientific discourse in many respects but there are two important differences. (19) First, since the decision maker usually has a deadline by which a decision has to be made only the information which is available by that time will be of use even if it is partial and if competent experts cannot reach full agreement on its reliability and/or significance. As the quote from Governor Brown suggests, this situation is frustrating to the decision maker as well as to policy oriented scientists. Second, the terms of reference of the legislation will define what data are relevant and irrelevant studies, however high in quality, cannot be considered. For example, under the Delaney Clause carcinogenic substances cannot be added to food in the United States. However much we may know about a substance's other harmful qualities they are irrelevant in a proceeding based on the Delaney Clause. Conversely, studies which do consider carcinogenicity, however third rate they may be, are highly relevant.

In order to effectively influence administrative decision making on issues which involve technical and scientific questions environmentalists need both legal and scientific capabilities. To this end, the environmental law groups have developed full time lawyer-scientist teams and have assumed the major role among environmental groups in working on the administrative implementation of the high technology issues of toxic substances and energy. In this way American environmentalists have been able to institutionalize a "counter-science" capability.

The type of science employed by these environmentalists is what I will

call advocacy science. Those practicing advocacy science marshall scientific evidence in support of a particular policy conclusion such as banning the use of fluorocarbons in aerosol spray cans or the adoption of a particular numerical limit for the number of porpoises killed by American tuna fisherpeople and then actively press those claims in the relevant policy forums. The fundamental aim of the advocacy is to ensure that technology is not permitted to inflict serious harm on the environment or human health. Since those who benefit materially from a given technology may be counted on to promote and defend it in administrative and judicial settings environmentalists tend to find themselves in adversary relationships with industry scientists as each seeks to discredit the scientific evidence and arguments used by the other.

Stepping back for a moment, let us contrast this model with that of Rachel Carson. Informative science as practiced by her is in the advocacy mold - up to a point. Silent Spring is clearly a brief for a particular position and casts aspersions on the efforts of industry scientists in defense of DDT. But instead of presenting her case in a policy forum she presented it first to the public. Practitioners of advocacy science tend to do the reverse: they devote most of their energies to preparing presentations for policy makers, not the public. For example, EDF's toxic chemical program has a list of 200 publications which its staff has produced over the past five years, almost all of which are comments, statements, and testimony prepared exclusively for policy forums. When they seek to enlist public support they usually do it through the news media. (20)

The full time practitioners of advocacy science include both scientists and non-scientists, most of whom are lawyers. The non-scientists are able to work on topics which require a high level of expertise to understand as long as the topics are routine in the sense that the nature of the problem is clearly defined and it does not require original analysis. For example, as of this writing EPA is shortly to make a decision on allowable air emissions from coal-fired plants. What is at issue is the level at which the plants will be allowed to emit sulfur dioxide (one proposal is for 1.2 pounds per million BTUs, averaged over 30 days) and the extent to which the EPA will insist on the use of scrubbers to remove sulfur dioxide. These issues, while involving a number of complex questions of fact regarding utility technology, the distribution and use of coal with varying levels of sulfur content in the U.S. and the effect of sulfur dioxide on human health, are fully within the competence of the EDF staff attorney handling the issue. The issues are clearly defined and have been in that debate for some time. The latest challenge to EDF's preferred policy in the form of a study by the National Coal Association, can be rebutted by reference to other data available from EPA (Environmental Health Letter, May 15, 1979:4-5).

The non-scientist advocates often work with one of their organization's staff scientists but this is not always the case. When not working with a staff scientist the non-scientist usually relies on an informal network of cooperating scientists for assistance. Skill in development and using these networks is a prerequisite for success in influencing policy for both the scientist and non-scientist advocates. On the whole, environmentalists have been successful in developing and using these networks of outside scientists although the task is hindered by several institutional features of American science:

- Including economists, non-industry scientists are rewarded for doing pure not applied science and can spare time for public interest work only at the risk of their career. Unless a scientist is already a specialist in the area that an environmental group is working on there would be a considerable time investment for the scientist to become competent in it. Non-industry scientists who already are active in these areas are few in number and heavily committed. For these scientists the environmental law groups have found that the more specific their request the greater the chance of gaining the scientists' help.

- In some applied areas such as radiation and drinking water standards the field is monopolized by a more or less closed circle of experts who adhere to a particular viewpoint and bring informal pressure to bear on colleagues who might challenge that viewpoint (Nader in Epstein, et al., 1977:20-26).

- Outside scientists rarely have the policy skills needed for effective advocacy science, and environmental groups' overburdened staffs usually lack the time to help them acquire these skills. Charles Wurster, the biologist co-founder of EDF once described this problem at a conference on public interest science:

> The forum of the scientific journal is a very different forum from that of the courtroom, the agency hearing, the Congressional hearing or press conference. The relevant aspects of an issue and the language to express them may be quite different in each of these. Most scientists, accustomed to only one of those forums, may shoot right past the target. Recently, for example, I was reading testimony that had been presented in some administrative hearings. I was struck by how much of it was irrelevant. It was absolutely correct and read like a scientific journal, but in ten pages there was one relevant sentence. Administrators or decision makers at one point must sort all of this material into one of two baskets labeled "benefits" and "costs". If in ten pages of testimony they only retrieve one sentence to throw into a basket, and all the rest are

> irrelevant, then this scientist has functioned as a scientist but has gone right past the target in terms of relevance. Yet relevance is the commodity with which administrators must deal. (Epstein, et al., 1977:82-83).

The full time scientist staffers are an important source of expertise for their groups. The ones who I have interviewed are both highly competent scientists, commanding the respect of other scientists, and skilled policy advocates. They are able to identify new problems which need to be placed on the policy agendas, to conduct crucial experiments, to critique research reports, to compile research reports of their own, to check the scientific accuracy of the reports written by the non-scientist staffers, to cooperate with the scientist staff of the regulatory agencies and to serve on government science advisory committees. Their scientific training makes it especially easy for them to draw on the expertise of fellow scientists who are sympathetic to the environmentalist cause.

While staff scientists can be extremely useful to the environmental group working on second generation issues in the administrative and legal arenas, finding the right kind of person is not an easy task. The environmental law groups pride themselves on the quality of their professional staffs. Many of their attorneys have graduated with honors from leading law schools and more than a few have acquired other distinctions such as clerkships and Rhodes scholarships. They prefer to hire only attorneys that have already acquired post-law school experience, as they do not have the time to provide in-house training, and they are able to indulge this preference, such is the demand to work for the environmental law firms. Moreover, they can safely assume that the new attorneys will be skilled in policy advocacy since public policy is part of an honored tradition in the legal profession and advocacy in an adversial setting is at the heart of the law. As we have seen, these skills are not part of the repertoire normally acquired in the course of winning a Ph.D. in the hard sciences. Thus the number of people who are both well-trained scientists with several years of post-graduate experience and who are skilled in public policy advocacy is small indeed.

But even if a possible candidate is identified there is a further problem. Since environmental group lawyers practice law in the normal course of their work their future careers are directly enhanced by the experience gained in environmental advocacy. The environmental group scientist, on the other hand, is removed from the lab with potentially disastrous results for his or her future career as an academic scientist. Environmental science policy work requires the scientist to be a generalist and range across specialities and even disciplines in the course of his or her work. No matter how competent the scientist may be, after only a couple of years away from the lab, he or she loses touch with his or her former research speciality.

For the scientist who is interested in environmental policy, however, the environmental groups have much to offer. The scientist enjoys a freedom to choose issues and pick forums which would not be available if he or she worked as a staff scientist for a regulatory agency. The challenge of learning new areas of science sufficiently to deal with policy questions can be exhilarating. As a member of a respected organization the scientist has a platform and status which has come to command respect in the press and in the regulatory agencies. One sign of this respect is the increasing number of invitations to environmental group scientists (and lawyers) to serve on one or more of the governments regulatory agencies' numerous scientific advisory committees (21) and the special National Academy of Sciences committees. (22)

Although environmental groups enjoy a reasonable degree of financial stability their financial resources are limited and far less than those of their industrial adversaries. In a situation of limited resources and small staffs much of their work is defensive - criticizing proposals, attacking research reports submitted by industry to document its position, hounding regulatory agencies to conduct needed research, and seeking increases in the federal budget for approaches which they favor such as integrated pest management and solar energy. Much of this activity has amounted to fighting an endless series of brush fires and, while necessary, does not contribute to reforms which might have a more fundamental effect on America's ability to mitigate technology's harmful side effects. In the last few years, however, the environmental movement has developed a greater capacity for anticipatory activities.

For example, as the groups have acquired expertise in a particular area, they have occasionally practiced an offensive form of advocacy science by conducting studies which show the worth of alternative ways of accomplishing social objectives. Some examples:

- NRDC, according to John Adams its executive director, is spending between $100,000 and $200,000 on studies of various alternative air pollution control plans for New York City.

- The Bonneville Power Authority currently has plans to build 35 new coal and nuclear power plants to meet the future needs of its area. NRDC did a full analysis of the potential for conservation as an alternative to this plan and concluded that only five to six new plants would be needed if this potential were utilized.

- EDF conducted an economic study of the Pacific Gas and Electric Company in California which they submitted to the California Public Utilities Commission. W.R.Z. Wiley, EDF's staff economist, used an innovative computerized technique to compare what the utility must invest to produce an equivalent amount of utility from coal and nuclear power, or from five alternative sources. His conclu-

> sions were that PG & E could replace 90 percent of its proposed coal and nuclear units with a combination of alternatives and earn $472 million more in net earnings over an 18 year period.

As a result of the regulatory agencies' need for help with the flood of rule making required by the new environmental legislation, the environmental groups have also been able to make an input into rule making far earlier in the process than before. By anticipating major regulatory decisions they have been able to prepare and submit what amounts to "shadow" criteria documents and proposed regulations to the regulatory agencies. For example, Massachusetts Audubon's Ian Nisbet wrote a 750 page report which was used by EPA as the basis for the proposed effluent standards for polychlorinated biphenyls (PCBs) and A. Ahmed Karim of NRDC spent two years on a literature survey on the health effects of fine particles in air pollution hoping to have a similar effect on EPA's fine particle standard.

Occasionally the groups' staff scientists conduct experiments or studies to document new problems. For example, EDF scientists analyzed water from the Hudson river to document the failure of municipal sewage plants to clean carcinogenic substances from industrial waste water. Samples were taken from two industrial outfalls and the outfalls of two municipal plants which treated water from industrial plants. The findings were published in a major report (Environmental Defense Fund and New York Public Interest Research Group, 1977). Robert Harris, another EDF scientist, conducted two studies investigating the relationship between drinking water quality and cancer mortality rates for the period 1950-1969 by parish in Louisiana (Harris, 1974 and Page, et al., 1977). His results, indicating a statistically significant association between cancer mortality rates and populations which received drinking water from the Mississippi River, were an important stimulus to the passage of the Safe Drinking Water Act in 1974.

VI

> The public interest is in seeing that the best possible decision is reached. The reason for having citizen participation is so that the conglomeration of private interests, people that have their own particular viewpoints, are thoroughly aired so that a decision-maker does not - being ignorant of an important point - make a decision which turns out ultimately to have been the wrong one.
>
> Anthony Z. Roisman, attorney, in testimony before a Congressional Subcommittee (U.S. Congress, 1977:83)

Thus far I have described the development of a particular institution in America - the environmental law and lobby groups - and have shown

how they have achieved considerable success in overcoming the obstacles facing citizens who wish to influence public policy on second generation environmental issues cited at the beginning of this paper. At this point I should emphasize that this "success" is relative and precarious. Each environmentalist victory still requires considerable expenditure of time and money, and while the Carter administration has elevated many environmentally-oriented individuals to policy making positions, industry has stepped up its efforts to make its case before the agencies. These efforts are abetted by the Administration's and Congress' concerns about the costs of regulation in these inflationary times.

Although my examples have been primarily from the toxic substances debates, the same science advocacy approach by professional environmentalists characterizes much of the environmentalist involvement in the rDNA regulation, energy policy, and nuclear power debates as well. Let us briefly consider the assumptions regarding science and technology, risk, and the American political system which underly it. These assumptions may suggest how applicable this public interest group model may be for other countries.

ASSUMPTIONS UNDERLYING THE ENVIRONMENTALISTS' SCIENCE ADVERSARY APPROACH

Science and Technology

1. Regulatory issues are trans-scientific. They involve both scientific facts and questions of value which must be resolved by the political process.

2. Scientists' interests influence the questions they ask and how they interpret their data. This is the case for scientists in universities (23) as well as for scientists in industry.

3. The present economic system has a bias towards introducing new technologies which provide short term benefits without regard to possible long term risks. Large profits may be made from the new technologies whereas manufacturers are subject to few penalties if it is discovered that the technologies they introduce produce long term risks.

4. Any given solution has a problem. Technical fixes may in the end create problems worse than those which they were created to solve.

5. Nature knows best. Technologies which employ natural processes are far preferable to those which interfere with natural systems or use human-made substances not known in nature.

Risk

1. If the public is to be subjected to any risk, it should be justified by a real public benefit. Environmentalists are highly sceptical of the real worth of many benefits. (24)

2. Risks involving potentially catastrophic events with low probabilities of occurrence should not be imposed on people. Human fallibility is such that the probability of such events occurring is higher than the experts realize.

3. An ounce of prevention is worth a pound of cure. Products should be proven safe before their introduction into the marketplace rather than the reverse. Once a new technology is absorbed into the system, a large measure of human control over it is lost.

4. The costs involved in rejecting new technologies which are really harmless are far lower than those involved in accepting new technologies which turn out to be harmful. "Letting a dangerous chemical slip by may entail social costs a hundred times greater than restricting an innocent one." (Harris and Page, 1977:7).

The American Political System

1. Decisions about science and technology must not be left to the experts alone. Their biases tend to blind them to the public's real interests.

2. Government regulatory agencies cannot be trusted to represent the interest of the public because they tend to be overly influenced by those whom they are set up to regulate. They therefore need input from environmental groups to balance industry's input. Only if public and private interests are able to freely compete on an equal basis can the public interest be served. (25)

3. Watching the regulators requires full time professional environmental advocates.

VII

Considering all relevant data and viewpoints is essential to good decisions.

Judge David L. Bazelon (1979:280)

We now come to the question of how the public interest is served by advocacy science as practiced by the environmental groups. If the plura-

listic model of government decision making is a reasonable description of current policy making in the United States, it can be argued that thus far the public has been well served by the environmental groups' activity on its behalf. Without violating the norms of science they have effectively represented in regulatory forums an important point of view which has hitherto been underrepresented.

This was certainly the case with DDT. The NRDC's challenge to the Nuclear Regulatory Commission over the environmental effects of the uranium fuel cycle, particularly the disposal of nuclear wastes, is another case in point. (26) According to David Bazelon, the judge in this case who ruled in NRDC's favor only to be overturned by the Supreme Court on procedural grounds, "only after extensive prodding by environmental and citizens' groups did the industry and regulators show any awareness of waste disposal as a problem at all." (Bazelon, 1979:279). Bazelon declared that the Nuclear Regulatory Commission exhibited "an almost cavalier manner" towards the uncertainties surrounding the issue and the "apparently substantial criticisms" brought by the environmentalists.

Other issues about which environmentalists have presented scientific evidence in support of a position which it appears would not otherwise have been represented are the Alaska Pipeline, DDT, dioxin, Tris (a chemical added to children's sleepwear which has been identified as a carcinogen) and asbestos linings in hair dryers. In each of these instances environmentalist prodding in the face of industry opposition led to regulatory action. In the current debate over the benefit/cost of environmental regulations, environmentalists have attempted to provide studies to counter the agencies' tendency to emphasize quantifiable factors to the exclusion of environmental and human values which may be no less important despite the difficulty of reducing them to dollar figures.

In the scientific controversies which I have studied, I have found no substantial confirmation for accusations by some that environmentalists distort issues by exaggerating risks or that they intentionally misrepresent scientific information. While they are more risk adverse than industry scientists, thus far the use of science by environmentalists may be said to be "responsible" in the sense that: 1) the interest they represent is direct- their view of what is best for the public and 2) they observe the norms of science in reporting their data and documenting the references supporting their arguments.

There are several important institutional factors which promote the groups' maintenance of the norms of science. First they are held accountable for the accuracy of their work by their board of directors, (27) their science advisory committees (which they appoint to further their legitimacy and to help them obtain use of the volunteer expertise of the eminent scientists who comprise these committees), and the reviewers to whom they send their materials. Second, the environmental groups' staff scientists are subject to peer pressure from the scientific community to some extent.

Third, in the adversary setting in which they work their arguments and data are made public and are subject to attack by their adversaries.Since they work in a situation where their credibility is one of their important resources, the groups believe strongly and,as far as I can determine, act on the belief that anything less than good quality scientific work on their part will reduce their effectiveness.

While they may be said to represent the public interest in the pluralistic sense described above it is important to inquire further and to ask who they represent and how they do this. Is the "underrepresented" interest which they represent the preserve of a narrow segment of the public or is it more widely spread? Put another way, who are the citizens who share the environmentalists' concern for environmental protection and their assumptions about science, technology and risk?

As mentioned earlier, polls have shown that a large proportion of the American public agree with the environmental advocates on these matters and these people cut across social groupings to a considerable extent. Only a very small percent of their ultimate constituency, the public, say that they are unsympathetic to the environmental movement.

The environmental groups also have the strong support of their members. With the exception of the Environmental Policy Center, all the groups listed in Table 2 have thousands of members whose monetary support accounts for a good share of their income. Some of the groups, most notably the Sierra Club, have procedures for member participation in governance and policy development. A number of others - including the two environmental law groups which are most actively involved in advocacy science - do not give their members any direct voice in the organization as their boards of directors are self-perpetuating or, in the case of Environmental Action, all policy decisions are made by the staff acting as a collective. Nevertheless, while the members of these groups do not have a direct voice they may be said to possess an indirect voice through their contributions. If a member becomes dissatisfied with the groups' activities he or she can simply decline to renew their support. Evidence from my research on the members of these groups suggests that at the present time almost all members find this arrangement satisfactory. (28)

Advocacy science needs to be balanced by informative science if it is to retain its base of public support, however. There is a danger that the environmental groups' success in playing the role of government's technological conscience may tempt their overworked staffs to overlook the equally important task of educating the public about the issues. The greater their staffs' ability to participate in esoteric discussions of criteria documents and the more their advice is sought by government agencies, the greater the likelihood that these advocates may become yet another elite group which has lost touch with their supporters.

The challenge of informative science is all the more imperative when the scientific illiteracy of even well educated Americans is taken into account. According to Norman Birnbaum,

> the scientists and technologists have discovered the public interest at an historical moment when the educated public (about science) cannot yet be said to exist. (1979:8)

If this is the case there is a serious danger, for example, that the continuous stream of statements announcing the carcinogenicity of one substance after another may engender among a significant portion of the supporters of environmental groups (to say nothing of their sympathizers among the general public) the fatalistic belief that everything causes cancer and a corresponding lack of enthusiasm for doing something about it. Or the debate about whether the ozone standard is too high or too low may seem meaningless to a people who have no understanding of the standard setting process, the evidence about the health effects of different levels of ozone, and the question of the possible synergistic effects of ozone and other air pollutants.

There are some signs that environmentalists are aware of this danger and are taking steps to revive informative science. For example, NRDC publishes a periodic newsletter for its members which carries long analyses of the issues which NRDC is working on. EDF has just published a book on cancer causing agents in the environment (EDF and Boyle, 1979) which was written by a professional writer who worked with EDF's toxic chemicals program staff. One myth which the EDF scientists were particularly interested in countering is the "everything causes cancer" myth. A very similar book to EDF's by a scientist-activist was recently published by Sierra Club Books, The Politics of Cancer (Epstein, 1978).

Informative science appears to be especially alive and well in the energy field where those contesting the present government policies have not yet gained entre into the policy making process in the way that environmental scientists working on toxic substances have done. Amory Lovins' article in Foreign Affairs (1976) and subsequent book, Soft Energy Paths (1977), bear comparison with Silent Spring in many ways not the least of which is the "road not taken" metaphor. These publications, particularly the first one, have given Lovins legitimacy as a scientifically respectable advocate for alternative energy and he has now used this legitimacy to at least get his foot in the door of the policy making inner sanctums where he has pushed his approach in an advocacy manner. Barry Commoner's most recent works have been on energy (1976,1979). In the anti-nuclear movement there has been a relatively close fit between informative and advocacy science although most often the practitioners of the latter (e.g. Henry Kendall, Arthur Tamplin, Thomas Cochran, Anthony Roiseman, Daniel Ford, David Comey) have not been the authors of popular books.

Whether the same institutions which have been so effective in practicing advocacy science can and should develop major informative science efforts is an open question. Rachel Carson was essentially a freelancer who received encouragement from the conservation organizations but who was not organizationally affiliated with them. Perhaps the best the advocacy groups can do is to experiment with new ways to inform their members about the scientific principles underlying the issues they work on and the questions of fact about which policy judgements are being made and to encourage those members of their staffs who are so inclined to devote some of their time to informative science. For groups whose resources are limited this is a large task which should have high priority, but obviously it will not be sufficient to overcome the general public's scientific illiteracy on the most crucial issues. Other institutions and programs, a description of which lies beyond the scope of this paper, will also be needed.

These comments should not be taken as a recommendation that the time has come for the environmental groups to make a wholesale shift from advocacy to informative science. While Silent Spring played and important role in creating a climate of opinion favorable to the evolution of the advocacy groups, by their uncompromising advocacy they, in turn, facilitate the possibility of genuine public debate on important issues concerning technology, risk, and environmental values which might otherwise be ignored. The continued practice of effective advocacy science by these groups is required to ensure both that the environmental perspective receives a full hearing by policy makers and that the questions about risk and technology retain the urgency they deserve.

TABLE 1

DATE	INITIATIVE BY ENVIRONMENTALISTS	OFFICIAL OPEN FORUM FOR DEBATE	REPORT	POLICY ACTION	COMMENTS
Summer-Fall 1962	Publication of *Silent Spring*				
By December 1962				Over 40 pesticide control bills introduced in state legislatures.	Such bills as were passed did not result in a substantial change in the use of DDT.
May 1963			President's Science Advisory Committee (PSAC) report echoed Carson's criticisms and made various recommendations including the "orderly reduction in the use of pesticides." Said their elimination should be the goal.		
Spring 1963		Senator Ribicoff's Subcommittee hearings on environmental problems including pesticides. Carson testifies as does the industry spokespeople.	Ribicoff's report takes a critical look at both industry and various government agencies. Published in 1966.		As Ribicoff's committee had no jurisdiction over USDA it had no leverage to push for change.
1964				Congress amends the Federal Insecticide, Fungicide and Rodenticide Act (FIFRA) to make the manufacturer of a pesticide prove its safety before registration by USDA for use.	This law had virtually no effect as USDA continued to grant pesticide registrations as a matter of course.
1965			Congressman Whitten instigated a Congressional study into pesticides and the accuracy of their critics. Report, released in 1965, castigates *Silent Spring* and exonerates pesticides.		Whitten's views had considerable influence since he was chair of the committee which did have jurisdiction over USDA.
1966	Yannecone brings suit against Suffolk County (New York) Mosquito Control Commission to ban the further use of DDT.	State courts.		State Supreme Court issues a temporary restraining order which remained in force a year and a half pending legislative action.	The Court provided a forum for a complete review of the evidence against DDT and the publicity led to the county's prohibiting its use.
1967	The Environmental Defense Fund filed suits in Michigan to restrict local use of DTT.	State courts.			Lost some cases but publicity and court orders resulted in shift to other methods of control.

DATE	INITIATIVE BY ENVIRONMENTALISTS	OFFICIAL OPEN FORUM FOR DEBATE	REPORT	POLICY ACTION	COMMENTS
late 1968 - first part of 1969	Two Wisconsin environmental groups petition state Dept. of Natural Resources to declare DDT a pollutant according to Wisconsin state law. EDF organized the presentation of the case against DDT.	Hearing before an examiner lasts six months and produced 2500 pages of testimony. National forum for the DDT debate.		May 1970 the examiner declared DDT and its analogs an environmental pollutant.	A number of states in 1969 and 1970 took effective action against DDT partially in response to the airing of the controversy by EDF in these state forums.
September 1968			Highly critical GAO report on USDA enforcement of FIFRA.		
December 1969			Mrak Commission Report on pesticides to Secretary HEW. Recommends elimination within two years of all uses of DDT (with minor exceptions) and calls for "corrective action" on other chlorinated hydrocarbons.		Report was influential in legitimizing the anti DDT position.
May-June 1969		Fountain Congressional hearings to examine how USDA was discharging its pesticide regulation responsibilities.	Report highly critical of USDA.		
July 1969			NAS-NRC advisory report recommended more effective steps to stop unneeded release of persistent pesticides.	USDA bans some uses of DDT.	DDT could still be used in agriculture, however, which accounted for 2/3 of its use.
October 1969	EDF and four other environmental groups petition the Secretary of Agriculture to suspend all uses of DDT.			No answer from agriculture.	
November 1969				Secretary of Agriculture issues notices of cancellation for almost all uses of DDT.	Since he did not suspend its use industry could continue to use it during the involved and lengthy appeals process mandated in FIFRA. Furthermore its major use in cotton was not covered.
December 1969	EDF files suit to have USDA ordered to suspend all uses of DDT.	U.S. District Court of Appeals.		May 1970 Court order Secretary of Agriculture to suspend DDT or to show cause why not.	Suspension is stronger than cancellation because it would cancel use during appeals process.
December 1970				Nixon transfers pesticide regulation program from USDA to EPA.	Delay in dealing with court order.

DATE	INITIATIVE BY ENVIRONMENTALISTS	OFFICIAL OPEN FORUM FOR DEBATE	REPORT	POLICY ACTION	COMMENTS
1971	EDF again files suit to have EPA suspend DDT uses.	U.S. District Court of Appeals.		Jan. 1970 Court orders EPA to cancel all DDT uses and to consider whether there is a basis for suspending DDT's use immediately. A week later EPA complied with the order to cancel which the manufacturers appealed.	First step against its major use on cotton. Use continues during lengthy appeals process, however.
August 1971 - June 1972		Public cancellation hearings lasting six months on EPA's decision to cancel DDT's registration. EDF participates.	April 1972, Hearing examiner recommends against the cancellation of DDT's registration.	*Administrator of EPA announces the banning of DDT effective by the end of 1972.*	Major national forum for airing the scientific questions. Examiner discernably unsympathetic to environmentalist position. Nevertheless DDT finally is effectively banned for all but "emergency" use.
Second half 1972		U.S. District Court where appeal to the EPA decision by manufacturers is heard and rejected.		December, DDT ban goes into effect.	

SOURCES:
EDF and Boyle, 1979; Graham, 1970; Whitaker, 1976; and, especially, Prinack and Von Hippel, 1974.

ABBREVIATIONS NOT NOTED IN TABLE:
USDA - United States Department of Agriculture;
EPA - Environmental Protection Agency;
GAO - General Accounting Office;
HEW - Department of Health, Education & Welfare;
NAS-NRC - National Academy of Sciences - National Research Council

TABLE 2

MAJOR UNITED STATES NATIONAL ENVIRONMENTAL GROUPS
ACRONYM, DATE OF FOUNDING, 1977 MEMBERSHIP AND 1976 INCOME

Name	Acronym	Date of Founding	1977 Membership	1976 Income (thousands of $)
Older				
Defenders of Wildlife	DEF	1959	35,000	1,269
Izaak Walton League	IWL	1922	60,000	ca. 500
Nature Conservancy	NC	1951	55,000	15,620
National Audubon Society	NAS	1905	373,000[a]	10,537
National Parks & Conservation Association	NPAC	1919	42,000	871
National Wildlife Federation	NWF	1936	620,000[b] (Associate members)	22,588
Sierra Club	SC	1892	178,000	6,474
The Wilderness Society	TWS	1935	68,000	1,862
Younger				
Cousteau Society	Cousteau	1975	150,000	2,262
Environmental Action	EA	1970	16,000	367
Environmental Policy Center	EPC	1972	no membership	200
Friends of the Earth	FOE	1969	19,000	674
Union of Concerned Scientists	UCS	1971	45,000	ca. 600
Environmental Law Groups				
Environmental Defense Fund	EDF	1967	45,000	1,788
Natural Resources Defense Council	NRDC	1970	35,000	2,037

[a] Membership figure is for "members" where family memberships, which cost more, are counted as two members.

[b] NWF counts 3,500,000 members in all membership categories.

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(1) These groups use private donations to support full time staffs of professionals to seek "public goods" of various kinds. Such goods are available to everyone in society without exclusion and include such things as clean air, safer automobiles, a fairer income tax system. For a discussion of why people contribute to such groups when their individual contributions are relatively inconsequential and they could "free-ride" on the contributions of others see Mitchell, 1978b.

(2) The major work on these groups is Burton A. Weisbrod's Public Interest Law: An Economic and Institutional Analysis.

(3) One of the early manifestations of the conflict between naturalists and professional resource managers was the difference of opinion in 1897 between Gifford Pinchot and Charles Sargent over the report of the National Forest Commission on which both served. Sargent, who was professor of aboriculture at Harvard and one of the eminent of American experts on forests, was inclined to emphasize the preserving of forests, whereas Pinchot, the forester, desired the fullest legitimate use of their resources (McGeary, 1960:38-43).

(4) Indeed, support for nuclear power by some environmentalists continued into the early 1970s although it was seldom expressed with the kind of enthusiasm which Udall evidences in the quote at the beginning of this section. For a fascinating statement in support of nuclear power see Denovan, 1970.

(5) She sent her chapters to specialists in the field for review and drew on many scientific papers and reports, the full list of which takes up thirty pages in the paperback version of her book. Although her attackers were many, the scientific errors in her book were relatively few (Graham, 1970:94).

(6) "Specialists representing various areas of the vast field of biology are contributing - entomologists, pathologists, geneticists, physiologists, biochemists, ecologists - all pouring their knowledge and their creative inspirations into the formation of a new science of biotic controls" (Carson, 1962:245).

(7) For a perceptive review of Commoner's and Carson's place in the environmental movement which pays special attention to their role as scientists see Donald Fleming, "The New Conservation Movement" (1972).

(8) In the late 1950s a group of Long Island citizens led by a well known ornithologist tried to obtain a court injunction against DDT local spraying, but despite appeals that went all the way to the Supreme Court, no relief was granted. The best short account of the court battles on DDT in the 1960s is Primack and Von Hippel (1974).

(9) The first involved a suit brought by environmentalists against the Federal Power Commission, Scenic Hudson v. FPC 354 F.2d 508 (2d Cir. 1965), cert. denied, 384 U.S. 941 (1966). The second landmark standing case was Office of Communication of the United Church of Christ v. FCC 359 F.2d 994 (D.C. Cir. 1966). This legal development played a very important role in the evolution of the United States environmental movement because it enabled the groups to challenge certain administrative actions. As a result, the views of the environmental groups are taken more seriously by government agencies than they otherwise might be.

(10) The environmental movement is very active at the state and local level in the United States. Two of the major national organizations, the Sierra Club and the National Audubon Society, have a large number of active local chapters and groups, and the National Wildlife Federation has over one million affiliate members who belong to local organizations - typically outdoor sports clubs - which are members of the Federation's state organizations. In addition there are numerous ad hoc groups of various kinds. This paper is primarily concerned with the national groups. It should be pointed out that local groups regularly draw on scientific expertise especially in siting controversies (Nelkin, 1975). A major source of expertise are local university faculty.

(11) Meaning by this that they have added the second generation problems to their agenda and maintain a Washington office some of whose professional staff actively seek to influence environmental policy. Lobbying in the strict sense is only one of the policy influencing techniques used by these groups and until the 1976 recent revision of the tax laws it was one which some of them were reluctant to employ for fear of losing their 501 c (3) non-profit tax status.

(12) It wasn't until 1952 that David Brower was hired by the Sierra Club as its first full time executive director. A decade later the first Sierra Club Washington office was established with one staffer. The major growth in the Club's Washington office occurred in 1972 when its staff expanded from two to its present seven to eight. Nationally the Sierra Club has a total of 85 full-time staff including support personnel. Friends of the Earth, with a far smaller membership, expanded its staff (including support personnel) from seventeen in 1972 to thirty-seven i 1977 (Mitchell and Davies, 1978:16-17).

(13) By scientists I refer principally to individuals trained at the Ph.D. level in physics, chemistry, biology, nuclear engineering, and those related scientific disciplines which are of greatest relevance to the second generation environmental problems. In June 1979 the new president of the National Audubon Society announced that the Society will add several scientists to its staff in the near future.

(14) I include economists in my discussion of scientists here and below because technical economics involves a level of specialized knowledge not easily acquired without advanced training and because economic expertise is an ever increasing ingredient in the environmental policy debate.

(15) For a full discussion and critique of public participation in the administration policy process see Frank, Onek and Steinberg 1977.

(16) For an overview of policy issues as defined by a group of environmental leaders see the Task Force Report sponsored by the Rockefellers Brothers Fund, (Barney, 1977).

(17) If it isn't already apparent to the reader I should like to point out that in the interest of stylistic economy I often use "scientific" to refer to what might more properly be termed "scientific and technical".

(18) Here is an example of the production and use of data in the informal mode to gain media coverage for a point of view. James Flug is the director of Energy Action, a public interest consumer group. Recently he wrote a brief article in the Washington Post in the form of a diary of his activities for a week. The entry for Tuesday has him reading the morning papers: "Spell broken by Post, Times "spec" stories on the president's coming energy speech. Both say a quick decontrol is centerpiece. We release a comment for stories on decontrol: "a fraud on the American people, huge price increases and no benefits." It's picked up by the wires and radio networks - two quickest avenues of public education. Wires and networks begin pressing for numbers. What will this cost the average family? Energy Action's one-man research department, Ed Rothschild, and I quickly come up with conservative figure: Oil decontrol alone will cost the aveerage family $270 more in first year. Figure hits wires, used on Cronkite with attribution, next morning on "Today" without attribution." The Washington Post, April 15, 1979, p. B.3.

(19) For an excellent elaboration of the differences between academic and policy research see Coleman, 1972 and also Revelle, 1975:1101.

(20) The groups regularly issue press releases and, whenever possible, hold press conferences. It has become customary for reporters to call environmentalist contacts for comments on new developments in environmental legislation or regulation.

(21) These committees are set up to provide the agencies with outside expertise and points of view. At the end of 1975 more than 45 agencies employed 1,267 advisory committees with 22,256 positions (Frank, et al., 1977). In response to abuses Congress passed the Federal Advisory Committee Act in 1972 which required their meetings to be open, their written records to be made available to the public, and the memberships "to be fairly balanced in terms of the points of view represented and the functions to be performed by the advisory committee" (5 U.S.C. App. I, 5(b)(2)). The regulatory agencies of most interest to environmentalists use a large number of "technical" advisory committees which "generally contain persons from academia and industry, but often do not include persons from interest groups" (Ibid.:615). For environmentalists this was particularly the case with advisory committees in the Departments of Agriculture and Interior. In recent years environmental scientists and lawyers have found these committees somewhat more open to them. The small number of environmentalist experts is an important limiting factor on the groups' use of this mechanism for input to the policy process. For an analysis of consumer input (which includes environmentalist input) to the FDA advisory committees, see Friedman, 1978.

(22) These committees, which make up half of the government's science advisory establishment (800 committees and 7,000 scientists in 1973), are not covered by the Federal Advisory Committee Act (Epstein, et al., 1977:45), although the Academy seems to feel it has at least to appear to meet the same requirements.

(23) A number of academic scientists especially microbiologists were incensed at the environmental groups' continuing efforts to have the federal government tighten up on safety measures which apply to laboratory experiments using rDNA techniques. According to Nobel Prize Winner J.D. Watson, "We never expected ... that we could be branded as polluters by the environmental movement. For until recombinant DNA came along, we always thought we were on their side". (Watson, 1978). Watson then went on to impugn the motives of the environmentalists: "... such groups thrive on bad news, and, the more the public worries about the environment the more likely we are to keep providing them with the funds that they need to keep their organization growing". (Ibid.) Environmentalists reply that the experts always feel that they know what they're doing. "The biomedical community is getting its first taste of public policy review and not liking it any more than the auto industry, the coal companies or the pipeline builders did when it happened to them". (Marshall, 1978:1269).

(24) This is particularly the case with consumer products such as hair dyes which do not serve what they consider to be fundamental social needs; and technologies, such as nuclear power, for which there are substitutes. For an interesting discussion in this vein of the question, do we need rDNA?, see Cavalieri, 1978.

(25) Richard B. Stewart calls this view the principle of interest group representation. According to him this principle has been warmly endorsed by commentators ... Indeed, litigation on behalf of widely-shared "public" interests is explicitly defended as a substitute political process that enables the "citizen to cast a different kind of vote (which) informs the court that ... a particular point of view is being ignored or underestimated" by the agency. Its ultimate aim is seen as "a basic reordering of governmental institutions so that access and influence may be had by all". Richard B. Stewart, 1975:1760, footnotes omitted.

(26) Natural Resources Defense Council v. Nuclear Regulatory Commission, ed. Rep. 2nd Ser., vol. 547, p. 633, (D.C. Cir. 1976), reversed sub nom.

(27) It should be pointed out that much discretion is accorded the professional staff of environmental groups. Nevertheless, boards are kept informed about program activities and in cases where controversy arises, as with the groups' position on rDNA research, the Boards take an active role.

(28) In 1978 I surveyed a random sample of the members of five national environmental groups. Only 4 percent of the EDF membership said that they are dissatisfied "with the opportunities provided to you by EDF to have a voice in its affairs ". Eighty-nine percent, in answer to another question, said they think of EDF as "representing my views on environmental/conservation issues to the government". On the specific question of whether it is "important for environmental/conservation organizations to become involved in the debate about recombinant DNA" those EDF members who agreed outnumbered those who disagreed by a 3:1 ratio. Results similar to these were found for the Sierra Club, the Wilderness Society, Environmental Action and the National Wildlife Federation.

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SCIENCE AND SOCIAL CONTROL, CONTROVERSIES OVER RESEARCH ON VIOLENCE*

Dorothy Nelkin, Cornell University
Judith P. Swazey, Boston University School of Medicine

INTRODUCTION

Dr. Martin Arrowsmith, the researcher-physician immortalized by Sinclair Lewis, doubtless would feel a stranger in a strange land if he materialized today. The reverential public attitudes towards scientific and medical research, reflected and in part created by earlier 20th century fictional works such as Arrowsmith and "nonfiction" accounts by writers such as Paul de Kruiff, have changed, and many areas of research have come under critical public scrutiny. In fiction, Arrowsmith's visions of science and technology are supplanted by images of the Andromeda Strain, The Terminal Man, and The China Syndrome. In real life, research projects are stopped or delayed as groups external to science, and in some cases scientists themselves, question the moral implications of research activities, their immediate impacts, and their long range social consequences. In the late 20th century social assessment of science, questions are raised about the limits of scientific inquiry, and efforts are made to redefine and in some cases sharply delimit its boundaries (1). Is there some research so threatening to the basic values of certain groups or so potentially risky to human subjects that it should not be done at all? Who are, or ought to be, the "experts" in decisions about the nature and governance of research? Is the traditional freedom of scientists to define and control their own research still reasonable given the expanded possibilities of modern science in areas such as human biology and behavior?

For multiple reasons, the very process of carrying out scientific investigation is now suspect. Experiments involving the human fetus threaten the values of right-to-life groups; techniques of recombining DNA mole-

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cules, it is feared, may produce new and dangerous forms of infectious microorganisms or lead to an Orwellian future of genetic engineering, Concerns are voiced about the conduct, methods, and objectives of research with human subjects, especially in institutional settings such as prisons or mental hospitals, where concepts such as informed, voluntary consent are particularly difficult to realize.

Some research is challenged because of the potential abuse of the knowledge it may generate. Studies of the relationship between genetics and IQ, research on genetic manipulations, and theories of sociobiology often are attacked on methodological grounds, but the basis of concern is their potential for sociopolitical misuse. Biology, critics claim, is a "social weapon" that provides not only beneficial knowledge and applications but also scientifically accessible means of social control (2).

Critics of science often evaluate research less in terms of its internal merits than its perceived social implications. Moral concepts of fairness, human dignity, and human rights enter the social assessment of science; and these concepts far outweigh criteria such as "scientific importance", the excellence of design, the qualifications of investigators - the sorts of parameters by which researh is assessed (and funded) within the scientific community.

Givent the increasingly critical social assessment of science, it is not surprising that research bearing upon the identification, the reduction, and the control of violent behavior, often linked with criminal acts, has been especially vulnerable to critical scrutiny.

The Projects

The three projects relating to the study or control of violent behavior that we have examined illustrate many of the forces and issues that converge in the social assessment of science. The first project began in May 1970 when Harvard child psychiatrist Stanley Walzer initiated a chromosome screening study of all male newborns at the Boston Hospital for Women to identify and prospectively study the development of those with an XYY karyotype. Walzer's project grew out of work he had done with Harvard geneticist Park Gerald, including a 1965-69 chromosome survey of randomly selected, phenotypically normal newborns that was focused chiefly on identifying XYY karyotypes. Criticisms of various facets of the XYY study's design and execution began to be voiced early in 1974. In May 1976, although various Harvard review committees continued to approve the study, Walzer stopped the newborn screening phase of the study.

Project two also had its inception in 1972, when the Neuropsychiatric Institute of the University of California at Los Angeles proposed to develop a Center for the Study and Reduction of Violence. This interdisciplinary center planned to establish research and demonstration programs to study the causes of pathologically violent behavior, to identify "violent predispositions", and to develop techniques for preventing violence and for

treating criminal offenders. The proposed projects included studies of disturbed adolescents and their families, the use of a new drug (cyproterone acetate) for the treatment of rapists, the chromosomal abnormalities related to violent behavior, and the relation between violence and minimal brain damage. The researchers sought funding from the California Department of Mental Hygiene and the federal Law Enforcement Assistance Administration (LEAA). By 1974, after more than a year of protest, both the State of California and the federal government withdrew earlier promises to support the Center and plans for its creation were abandoned.

During the same period, several experimental behavior modification programs in prisons, that comprised the third project we studied, also lost their funding from LEAA. These included the Special Treatment and Rehabilitation Training project (START) in Missouri,and projects at federal facilities in Butner, North Carolina and Patuxet, Maryland. These prison programs used various behavior modification techniques including group therapy, Synanon-type therapy, electronic techniques of aversive conditioning, and operant conditioning. The prisoners enrolled in the projects had been unable to adjust to regular prison routines because of consistently "maladaptive" and "unmanageable" behavior. The projects were intended to help change the attitudes and behavior of these recalcitrant prisoners so that they could return to regular institutional programs.

The research and demonstration work in these three projects raised sensitive questions about the relationships of genetically mediated characteristics to human behavior, about the morality of modifying behavior by medical or psychological means, about the power of social control offered by new biomedical and behavioral technologies, and about the power relationships inherent in situations and settings where freedom of choice to participate in research cannot be taken for granted.

Biomedical and behavioral research covers a spectrum from "basic" to "applied" and from "experimental" to "therapeutic" research (3). The three projects can be arrayed, with some overlap, on different loci in the basic-applied and experiment-therapy spectrum. Their location on the spectrum, in turn, helped to shape the nature of the controversy that each project generated.

The XYY study sought to combine basic epidemiological research in behavioral genetics in the newborn screening portion of the study, with a longitudinal follow-up of "experimental" (XYY karyotype) and "control" (normal karyotype) groups that included therapeutic interventions within a research context. The second, multifaceted project, the UCLA Center for the Study and Reduction of Violence, involved various combinations of research and treatment-demonstration projects. The third enterprise, the LEAA-funded behavior modification programs in prisons, primarily involved demonstration projects designed to test the use of various behavior-change principles and techniques.

These projects, in their own right and as paradigmatic cases, are open to quite different interpretations depending on one's ideological perspective. Is behavior control a way to bring recalcitrant individuals into a less socially deviant and more personally or socially productive life? Or is it a means to better maintain social order at what may be enormous political and personal cost to certain socioeconomic groups? Will research into possible genetic bases for antisocial or criminal behavior enable us to ameliorate or prevent such behavior, or will it only facilitate social control by means of "genetic labelling"? Is a medical-behavioral approach to criminally violent behavior a humane route to reduce the need for punishment, or is it a sinister diversion of attention from the social and environmental factors that help to provoke such behavior?

Underlying all three controversies are different ideological perspectives on the definition of violence and its sources, and on the consequences of the emerging techniques of predicting and manipulating behavior. Our analysis of the disputes over these projects thus focuses on the perceptions of the protagonists, the sources of support and opposition, and the tactics of opposition and defense; our intention is to demonstrate how different ideological and political convictions shape the evaluation and the acceptance or rejection of scientific ideas and their applications.

The Actors

The Researchers

Except in the judgement of the actors themselves, the scenarios of the three projects do not have "heros" and "villains". In our view, neither researchers nor their critics had pernicious intentions. The researchers for the most part sincerely sought to advance science and to bring what they perceived as the benefits of science to the resolution of a social problem, and their opponents, with equal sincerity, sought to expose and stop what they perceived as a misuse or abuse of scientific hypotheses and techniques. In some cases, especially in the XYY study, the scientists identified their study as basic research that should be evaluated by their peers according to norms for judging the intrinsic merit and validity of such research. In addition, they saw practical benefits accruing from their work. It was hoped that if a correlation between the extra Y chromosome and a predisposition to aggressive behavior was established, this would facilitate the development of remedial and therapeutic interventions to prevent antisocial behavior.

At UCLA, investigators sought to develop diagnostic and predictive methods to reduce or prevent criminally violent behavior. They regarded their work as a form of public service that would benefit victims of violence, potential criminals, and society as a whole. Comparably, those involved in the LEAA prison projects perceived their work as a means to enhance

institutional order within prisons and to permit alternatives in incarceration; they claimed to be using science to seek humane solutions to problems that usually are handled punitively.

Blinded by long-standing assumptions about the objective, value-free, and apolitical nature of science, the researchers in every case failed to perceive the critical ideological and political questions raised by their work. Moreover, socialized in the tradition of scientific autonomy and professional dominance that characterizes the role of scientists and physicians, they were stunned by the opposition and the vehemence of the political challenge to their endeavors, and responded with moral outrage. The nature of the scientific review system compounded the reluctance with which they comprehended opposition. Prior to funding, research projects as a rule are reviewed by an institutional board to assure that the risks to subjects are outweighed by potential benefits and that informed consent is obtained by appropriate means. Proposed projects also are reviewed by a funding agency's study section or comparable peer review group to assess scientific design and merit. The existence of such procedures, most researchers feel, are more than adequate controls over the quality and conduct of their work. The fact that the major sources of funding for these three projects came not from the usual sponsors of basic research, but rather from the sources as the NIMH's Center for Studies of Crime and Delinquency, the LEAA, the Bureau of Prisons, and the California Commission on Mental Health -- agencies that are less concerned with scientific validity than with finding solutions to social problems -- did not alert the scientists to the controversial aspects of their work.

Thus, in the XYY study, the researchers defended their project, in part, by citing the multiple reviews which their protocol had undergone within Harvard and the National Institutes of Health. They also pointed out that their sources of funding - the NIH's Center for Studies of Crime and Delinquency - was not one to which they had applied directly because they perceived a linkage between their work and the Center's. Rather, they had submitted a proposal to NIH, and it had been routed to the Center by the Institute's central grant processing office, the Division of Research Grants. The sources of funding, XYY researcher Park Gerald feels, was misleading. For, he affirms, "at no time has the research that we've been involved in been related to violence". Similarly, Stanley Walzer maintains that "we did not start the study with the assumption that XYY is related to violence, because by the time we began, the literature increasingly was showing that it is not related."

The Critics

Proposals for research funding, and the conduct of research itself, usually are routine and closed matters, handled within their scientific community. But these projects relating to violent behavior produced an extraordinary, indeed a violent, response from quite different groups.

The critics of the Boston XYY study were mainly members of Science for the People, a group of scientists socialized during the Vietnam War and sensitive to social and racial inequities and to the potential misuse of scientific knowledge. Their concerns with XYY research grew out of their broader ideological opposition to work in behavioral genetics. They took issue with this specific project on the grounds of its methodological design, the problems of obtaining valid informed consent for research with newborns, and the implications of "labelling" its subjects as chromosomally "abnormal", possibly setting into motion a "self-fulfilling prophecy" about their behavioral development.

At UCLA, the political climate in the early 1970s was still charged by the events of the late 1960s. Indeed, only several years earlier university research had been a target of student protest because of its links to military objectives. The opposition to violence research thus grew out of the student movement. But critics of this as well as of the LEAA programs also included the Black Panthers, the NAACP, the United Farm Workers Organizing Committee, the California Prisoners Union, NOW, the Mexican American Political Association, and other groups concerned with ethnic and social equity. Perceiving prisons as a major pillar of institutional racism, such groups often regard inmates as "political prisoners". In this context they regarded the research on violence and the use of behavior modification techniques as efforts to prevent political dissent, to divert attention from unrest among the poor, and to create complacent prisoners for the benefit of guards or complacent children for the benefit of teachers (4). Despite the diversity of the critics--students, scientists, and political groups concerned with minority rights--they shared similar interpretations of the methodological, moral, and political problems inherent in the research.

The Sources of Opposition

Critics of the XYY studies at both Harvard and at UCLA argued that basic methodological problems distorted the research and precluded the gathering of valid data. On scientific grounds alone, therefore, they held the research to be unethical (5). Studies of the frequency of the XYY genotype were based on an inaccurate demographic picture of the study population and inadequate control groups. The retrospective studies to identify men with the XYY chromosome used sources such as inadequate military data and often anecdotal records. Prospective studies of the development of the XYY individual violated basic methodological principles by relying on behavioral descriptions of children by parents. How can a researcher assure that information on the chromosomal abnormality would not bias parental attitudes, thereby influencing the very behavior that is being studied? How in such cases can one distinguish the effect of a genetic aberration from the results of parental attitudes?

The Harvard XYY researchers, in retrospect, recognize that they were "frighteningly naive" about how their work might be perceived. Had they been more attuned to the sociopolitical climate surrounding behavioral genetics in general, and XYY research in particular, they might have learned much from the fate of the Maryland's aborted screening study of boys in juvenile jails and from underprivileged minority families. That study's design and objectives, its critics held, made it "a parody of clinical research" and a "science of devil's marks"; it was seen as epitomizing "medical totalitarianism" and the use of "fraudulent science" to support sociopolitical views.

Issues of freedom of choice and justice, however, were far more fundamental in shaping the dissent. Each of these projects raised questions about the validity of informed, voluntary consent. The federal guidelines designed to protect participants in human experimentation, based on principles established at the Nuremburg trials, seek to promote and protect the autonomy of subjects and guard them against untoward risk. They require that research using human subjects must make an important contribution to science, that the benefits of the study must outweigh its risks, and that subjects must give their informed, voluntary consent. But what constitutes adequate information? Cannot many subjects be subtly coerced to participate in experiments? Informed consent and voluntary compliance are ideals not easily realized in the coercive environment of a prison, or in other settings where many forms of overt or covert leverage can be placed on potential research subjects or on their guardians. The parents of a disturbed or hyperkinetic child who is referred to an experimental program by his school are hardly in a position to give "voluntary" consent (7). Inmates in a prison, where participation in an experiment may be linked to a system of favors or to existing power relationships are hardly in a position to avoid coercion. Women given consent forms while in labor are hardly in a situation to assimilate information about a newborn screening program.

Critics of the LEAA behavior modification programs questioned the meaning of procedural guidelines in a setting where discretionary power is held by authorities whose primary goal is maintaining order and cooperative behavior among the prisoners. Indeed, "voluntary participation" in a prison may be effected by fear of punishment and, above all, by the need to behave in ways that will maximize chances for parole.

Several prisoners described the behavior modification experiments, not as positive reinforcement, but as an abuse of their human and constitutional rights, and as cruel and unusual punishment. Some complained of humiliating "token economies" in which routine necessities such as toilet paper had to be earned, other complained of the use of aversive conditioning in which drugs were used to control behavior. One prisoner wrote of "the Nazification of the prison system with the enthusiastic cooptation and co-criminality of the academics...prisoners are literally fair

game for the mad surgeons, the shrinks, and the social engineers..."(8). The conditions of research in a prison, contend its critics, simply preclude rational discourse and independent choice.

Moreover, they argue, the risks of such research far outweigh the usefulness of the knowledge, even assuming that valid results could be obtained. Given the sources of funding and the compelling social pressures to develop rapid technologies for reducing violence, could the projects maintain stringent ethical standards concerning the recruitment of subjects and the conduct of the research? Scientific techniques that promise to reduce or prevent violence have such public appeal that concepts of human dignity or individual rights may be given low priority. Opponents of the research had little faith that scientists could resist such social pressure, for, after all, their own careers were involved.

Critics also feared the long term implications of research that relates human behavior to genetically mediated characteristics. XYY research brought forth images of eugenics, and fears of generating knowledge that would be used to perpetuate damaging social class and racial biases (9). Do we really want to know if there is a genetic basis to social behavior? Would not such knowledge effect our belief that individual citizens should be treated with equal respect? Would this not allow the development of pernicious mechanisms for social control, particularly of racial minorities and the poor? In the case of UCLA's proposed center, critics did not miss the fact that the Los Angeles schools to be used for the study of troubled adolescents were in Black and Chicano areas. Similarly, those who drew critical attention to the proposed 1970 Maryland XYY chromosome screening study pointed out that half of the test groups consisted of children from predominantly poor, black families enrolled in a free medical care program at Johns Hopkins, from whom the researchers had not planned to obtain consent. Studies of the families of aggressive children, the critics observed, mainly focus on the poor, if only because the middle class is better able to conceal its problems. Assessing the research in such political terms, critics attacked it through political action -- demonstrators, petitions, referenda and various "media events".

The Tactics of Opposition and Defense

> We're here to cure your troubles and fill your life with hope.
> We'll treat your criminal tendencies with lobotomies and dope.
> Dope to make you feel you're drowning, dope to give your pain.
> Dope to keep you quiet while we're cutting out your brain.

In the UCLA and LEAA projects, the critics saw scientists as "collaborators" with prison officials, and their rhetoric left little to the imagination. They called researchers "racists" and "Nazi butchers", and related the research on violence to "Fascism", "eugenics", and "genocide". The scientists were accused of "sprinkling the perfume of scientific

legitimacy over the stench of experimentation on prisoners". (One cartoon shows a fiendish scientist at UCLA converting prisoners into robots by running electric wires through their heads.)

They attacked the political backing of the projects, the sources of funding and, at UCLA, the support of Ronald Reagan and other conservative California politicians. They searched for vulnerable points -- especially the aura of secrecy which seemed to envelop the projects. The UCLA Center proposed at one point to locate some of its experimental programs at a NIKE missile base that had been abandoned by the Army. This isolated site was far away from residential neighborhoods, and was selected in order to avoid community opposition to the experiments. But the plan backfired as critics saw this choice of location as a means to maintain secrecy: Just what would be done at this isolated laboratory? Suspicions were reinforced when investigators hesitated to distribute information on controversial aspects of the program. At first, this probably was out of habit; scientists, after all, regard their work as technical and not for public distribution. Later materials, however, were withheld out of self defense. Critics interpreted this inaction as an effort to avoid criticism, and when material was distributed they then publicly accused scientists of "sanitizing" it in response to criticism.

They also seized on the vague nature of the research guidelines. How would prisoners be selected? What were the research protocols? Scientists may leave such specifics open for several reasons. The UCLA Center was conceived as a coordinating base for existing research and there was little point in limiting the research to be included prior to funding. Moreover, flexibility and lack of early specification about research is advantageous, allowing autonomy and flexibility in its actual implementation. For critics, however, the open ended nature of the proposal allowed the possibility of psychosurgery, brainwashing, and other unethical activities. And lack of specification reduced public accountability. Indeed, the UCLA plan resembled the loosely structured university research centers that had so easily adapted to military research during the Vietnam War.

The point of the critics' dramatic frontal attacks on the projects was to arouse public concern and thereby to indirectly apply pressure on public agencies and scientific institutions. The critics of science see no direct way to implement their views through legitimate institutional channels. The peer review panels, the university committees, and even the institutional review boards are perceived as mechanisms to foster the interests of the professional community. And this perception is often reinforced, as in the case of the critics of the XYY project who first, unsuccessfully, tried to work through Harvard's Review Committees. Accordingly, tactics of public protest are seen as more effective. And indeed they worked. There were, however, costs.

In many protests, "stormtroopers" appear who take up the cause, extending the tactics to a point which is often embarrassing to the leadership of the opposition. The personal harassment of Stanley Walzer, the principal investigator of the Harvard XYY research, and his family was not intended by those who initiated the criticism of the project. Walzer, in turn, felt defenseless against the stormtrooper tactics of personalized attacks, and profoundly relieved when, in May 1976, he halted newborn screening. "If someone has set out to destroy you and would use any means to do so, in their belief that you are inherently evil and must be stopped, you will be destroyed. There is no defense against an assault in which any means will justify the end". Even for those scientists who were not subjected to personal harassment, public criticism of their work was hard to accept, and they responded by hardening their opposition to the "intrusion" of those who raised difficult questions.

The scientists felt vilified by the attacks and retaliated by characterizing their critics as "professional character assassins", "irrational" and "hysterical" people, a "small number of self-interested, politically motivated people who wish to see the prison system of this country destroyed". They accused their critics of spreading "false propagandistic horror stories"; they felt harassed and intimidated by "witch-hunters". To defend their autonomy they invoked the norms of science as an apolitical, value-free activity, even where their research was clearly more applied than basic, more therapeutic or corrective than experimental. And they rallied the support of their peers, pointing to the fact that the research had been reviewed and approved, although by those very institutions rejected by critics as totally inadequate to assess the research.

The Context of Mistrust

What was it about these projects that aroused such often volatile controversy? The research sought ways to predict and control anti-social behavior, but the disputes framed basic problems in the very definition of anti-social behavior, in the assumptions concerning the cause of violence, and in the selection of appropriate solutions. What, in fact, constitutes violent behavior? Sometimes this is clear cut, but the propriety of some forms of behavior may rest entirely on debatable social norms and value judgements. Behavior may be defined as violent because it threatens existing power relationships; "incorrigibles" and "radicals" are often grouped together as "troublemakers". People who fight against dehumanizing conditions, or who organize political or religious groups that threaten the power structure of a prison, may be defined as "maladapted". Similarly, children who respond to chaotic conditions in disorganized classrooms or to overly controlled situations by hyperactive behavior may be defined as maladapted or even "brain damaged".

Explanations of the sources of violence also vary: is violent behavior primarily a problem of the individual offender, or a social problem that can be attributed to injustice, repression or exploitation? Quite different perspectives divided the scientists from their critics. Investigations of genetic mediators of violent behavior or the use of behavior modification by definition focus on the individual as a present or potential offender. They define violence as a problem of deviance and seek to adapt the individual to the social system. Opponents of this research focus their attention on the social environment that produces violence:

> Violence is primarily a social reaction to situations in which individuals believe no other means can remove intolerable conditions. In the United States the most oppressed groups are the ones most likely to react violently ...To provide the government with a medical tool for combatting violent behavior offers yet another possible weapon for repression without dealing with the root causes of violence. (11)

By dwelling on the individual offender, contend the critics, scientists divert attention from the social injustice, the poverty, and the many other problems responsible for violence. Science thus legitimizes the prevailing neglect of social problems, allowing those in power to deny their share of responsibility and to avoid searching for social solutions. Holding such views, critics regarded research on violence as a political activity,a bio-medically-grounded technological means to manage socially disruptive behavior in order to maintain social control. Such differences in perceptions of violence and its sources are not just academic; they have operational consequences, and it is these consequences for the prison system, for offenders, and indeed for scientists that contributed to the intensity of the dispute.

For the scientists the fundamental question raised by these disputes was, who controls and evaluates research? There is no consensual basis to measure the effectiveness of medical or psychiatric techniques for resolving problems of criminal violence. Professional evaluations or research in this area can easily be discredited. Acceptance must rest on trust in professionals and acceptance of the social relationships involved in the research process. In these projects trust clearly did not exist.

In part, the declining trust in science and its governing authorities to represent public values is part of a larger syndrome. A Harris poll for example, found that between 1966 and 1973, the proportion of the public expressing a great deal of confidence in the leadership of institutions declined as follows: Federal Executives, 41% to 19%; Congress, 42% to 29%; major companies, 55% to 29%; higher education, 61% to 44%;

medicine, 72% to 57%. Anti-professional attitudes are widely expressed in the demands for greater accountability and participatory control; deception, manipulation, loss of autonomy, and lack of choice are ubiquitous complaints (12). This is the social climate of the disputes over violence research -- a climate in which the policies and procedures governing science increasingly are subject to a democratic process that includes active political debate.

Historically, the social applications of science often have been legitimized by their association with basic research, and basic research justified by appeal to its potential social utility. These linkages between science and its social applications, coupled with unease about and distrust in institutes and "experts", contribute to present demands for a renegotiation of society's contract with scientists and their work, that will involve them more in democratic governance procedures. Another contributory factor is the realization that in many areas of science there is little to distinguish that which its practitioners label "research" from other activities normally subject to the political process (13). In these three projects, for example, only the XYY study could be appropriately labelled "basic" research; the others, while called research, involved active interventions or social policy. Their sources of funding, the compelling pressures for quick solutions to the problem of violence, and the entrepreneurial character of the project leadership, all indicate their policy orientation.

In this context, it is hard to believe that the scientists were entirely taken by surprise. The campus protests of the late 1960s had just abated. And the rhetoric of rights--womens rights, patients rights, consumer rights, and indeed, prisoners rights-- was in the air. Yet the reaction of scientists was one of moral outrage and defense as they sought to maintain their autonomy and avoid political engagement.

As conflicts bring science and technology into the realm of pluralist politics, subject to the claims of diverse moral views and political interests, key questions about the control of research and its applications are being addressed, revolving around the issue of "who should control?" If there is conflict between scientific goals and public values, can one rely on researchers to assess the implications of their own work? Given the professional norms of science, the general political inexperience of researchers, and their distaste for political involvement, can they be trusted to perceive the political and social implications of research? And will they publically surface problems that might jeopardize their own careers, particularly if they feel that they or their colleagues have been or will be burned by the controversy that "going public" can entail? "When things begin to get too hot", one such researcher has declared, "the academic context probably is not the place to look for many profiles of courage". The actions of scientists most closely involved with recombinant DNA research in drawing public

attention to the potential risks of their work shows that scientists can engage in and act upon a social assessment of science; but the "going public" of the recombinant DNA researchers was a relatively rare event, as was their decision to call for a moratorium upon their field of research (14).

Without the discomforting actions of opposition groups, then, who will raise questions about research? For a variety of reasons, as we have suggested, researchers and their peer reviewers are unlikely to voice social assessment typed of concerns, and indeed often fail to deal adequately with questions about the intrinsic validity of research proposals. If not the critics, who will raise these questions, and in part speak for the usually "voiceless" subjects of research? In these three projects, not atypically, the development and evaluation of proposals never involved the subjects or consumers of the studies -- the prospecitive parents, minorities, the poor, or prisoners.

These questions about the governance of research are addressed by the three controversies over projects on the prevention and control of violent behavior that we have examined. In retrospect, and we would argue prospectively, the projects had certain characteristic features that made them potentially volatile undertakings. The controversies that did in fact erupt may be interpreted essentially as a struggle for power over new techniques of social control that are opened up by scientific research (15). The idea that research projects such as these should be stopped or, at the very least, scrutinzed and controlled by groups outside the scientific community, illustrates the growing awareness of the political power inherent in the control of science and technology.

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4. Many of them expressed their views in hearings before the California Council on Criminal Justice, July 27, 1973.

5. Critics of the XYY study's design, for example, cited the Nuremburg Code's third provision, which states that "The experiment should be so designed and based on the results of animal experimentation and a knowledge of the natural history of the disease or other problem under study that the anticipated results will justify the performance of the experiment".

6. See G.J. Annas, L.H. Glantz, and B.F. Katz, Informed Consent to Human Experimentation: The Subject's Dilemma (Cambridge, MA.: Ballinger, 1977).

7. Issues of informed consent, as well as social control, also were central to another research controversy that errupted in Boston in 1972, concerning the effects of psychotropic drugs on learning difficulties and behavioral disorders in children. The coalition of community groups and organizations opposing this project included the Massachusetts Advocacy Center, which also became involved in opposition to the XYY study. See "MBD, Drug Research and the Schools", The Hastings Center Report 6 (June 1976), special supplement.

8. Letter to Dr. Willard Gaylin, n.d.

9. See George Annas, "XYY and the Law", Hastings Center Report, 2:2, April 1972; and Richard Roblin, "The Boston XYY Case", Hastings Center Report 5:4, August 1975, 5-8.

10. Daily Bruin (UCLA student newspaper), January 11, 1974. Much of the diatribe against the project appeared in this newspaper throughout 1973 and 1974, and in petitions and other ephemera.

11. Daily Bruin, February 25, 1974.

12. D. Nelkin, ed. Controversy: Politics of Technical Decisions (Beverly Hills: Sage Publications, 1978).

13. See discussion by H. Green in "Law and Genetic Control: Public Policy Questions", in Marc Lappe and Robert Morison, Ethical & Scientific Issues Posed by Human Uses of Molecular Genetics, Annals of the New York Academy of Sciences 265, (1976).

14. See Biotechnology and the Law: Recombinant DNA and the Control of Scientific Research. Southern California Law Review 51 (Sept. 1978).

15. S. Chrorover, "The Pacification of the Brain: From Phrenology to Psychosurgery," in T.P. Morely, (ed.) Current Controversies in Neurosurgery, (Philadelphia: W.P. Saunders, 1976), p. 758.

CONFERENCE PROGRAM

Wednesday 20.6

17.00 Welcome address: Anders Omholt, Director General of the Norwegian Research Council for Science and the Humanities

Jean-Jacques Salomon, President of the International Council for Science Policy Studies

SESSION I: PERSPECTIVES ON SCIENTIFIC EXPERTISE AND THE PUBLIC

Chairman: Hans Skoie

James S. Coleman: Conflicts Between Policy Research and Decision Making

Commentator: Georges Ferné

Discussion

20.00 Dinner

Thursday 21.6

09.00 SESSION I: PERSPECTIVES ON SCIENTIFIC EXPERTISE AND THE PUBLIC (continued)

Chairman: Everett Mendelsohn

Yaron Ezrahi: The Professionalization and Deprofessionalization of Science in Democracy

K. Guild Nichols: The De-institutionalisation of Technical Expertise

	Commentators:	Sverker Gustavsson Radovan Richta
	Discussion	
12.00	Lunch	
13.00	SESSION II:	SCIENTIFIC EXPERTISE AND DECISION MAKING: THE CASE OF NUCLEAR POWER
	Chairwoman:	Dorothy Nelkin
	Robert Olby:	The Case for Nuclear Power Examined
	Helga Nowotny:	Experts in a Participatory Experiment: The Austrian Debate on Nuclear Energy
	Per Ragnarson:	Impacts of the Nuclear Debate on Safety Experts and Safety Engineering
	Brian Wynne:	The Rationality and Ritual of Nuclear Decision Making
	Commentators:	Jon Elster Peter Weingart
	Discussion	
18.00	Dinner	

Friday 22.6

09.00	SESSION III:	SCIENTIFIC EXPERTISE IN GOVERNMENT
	Chairman:	Francis Sejersted
	A.W.Coats:	The Economist's Role: An International Perspective
	Commentators:	Gudmund Hernes Janos Farkas
	Discussion	

11.00 SESSION IV: INTEREST GROUP INVOLVEMENT IN SCIENCE AND TECHNOLOGY

Chairman: Roy M. Macleod

Kenneth Green: Trade Unions and Technical Expertise - the Control of Asbestos Dust in British Workplaces

Robert Cameron Mitchell: Since Silent Spring: Science, Technology and the Environmental Movement in the United States

Judith P. Swazey: Science and Social Control. Controversies over Research on Violence

12.30 Lunch

14.00 SESSION IV: INTEREST GROUP INVOLVEMENT IN SCIENCE AND TECHNOLOGY (continued)

Commentators: Elisabeth Helander
Arie Rip

Discussion

15.00-17.00 Panel discussion: Scientific Expertise and the Public - a new Relationship?

James S. Coleman, Everett Mendelsohn, Abdur Rahman, Jean-Jacques Salomon (chairman)

LIST OF PARTICIPANTS

Aubert, Vilhelm	Professor, Department of Sociology, University of Oslo Norway
Bettum, Berit	Economist, Member of the Board of the Norwegian Research Council for Science and the Humanities Norway
Blegen, Hans Marius	Deputy Director, Norwegian Research Council for Science and the Humanities Norway
Bye, Tove	Member of the Investigative Committee on the Safety of Nuclear Power Norway
Coats, A.W.	Professor, Department of Economic and Social History, University of Nottingham England
Coleman, James S.	Professor, Department of Sociology, University of Chicago USA
Dahl, Helmer	Fellow, Department of Applied Physics, Chr. Michelsen Institute Norway
Eriksen, Viking Olver	Director, Norwegian Institute of Atomic Energy Norway
Ezrahi, Yaron	Associate Professor, Department of Political Science, Hebrew University of Jerusalem Israel
Farkas, Janos	Research Fellow, Institute of Sociology, Hungarian Academy of Sciences Hungary

Ferné, Georges	Head of Social Science Policy and University Research Section, OECD, Directorate for Science, Technology and Industry France
Forbes, Eric G.	Professor, Department of History, University of Edinburgh; Secretary, International Union of the History of Philosophy and Science, Division of History of Science Scotland
Green, Kenneth	Associate Professor, Department of Liberal Studies in Science, Manchester University England
Gronow, Terttu Luukonen	Staff Member, The Academy of Finland Finland
Helander, Elisabeth	Research Director, The Academy of Finland Finland
Hernes, Gudmund	Professor, Department of Sociology and Political Science, University of Bergen Norway
Kant, Horst	Research Fellow, Institut für Theorie, Geschichte und Organisation der Wissenschaft, Akademie der Wissenschaften der DDR German Democratic Republic
Kröber, Günter	Professor, Director, Institut für Theorie, Geschichte und Organisation der Wissenschaften, Akademie der Wissenschaften der DDR German Democratic Republic
Lagadec, Patrick	Ingénieur de Recherches, Laboratoire d'Econométrie de l'École Polytechnique France
Landberg, Hans	Secretary General, Council for Planning and Coordination of Research Sweden
Löppönen, Paavo	Staff Member, The Academy of Finland Finland
Macleod, Roy M.	Professor, Science Education Department, Institute of Education, University of London England

Malecki, Ignacy	Professor, Committee for the Science of Science Polish Academy of Sciences Poland
Mendelsohn, Everett	Professor, Department of History of Science, Harvard University USA
Mikulinski, S.R.	Professor, Institute of History of Science and Technology, Academy of Science of the USSR USSR
Mitchell, Robert Cameron	Senior Fellow, Resources for the Future, Washington D.C. USA
Nelkin, Dorothy	Professor, Program on Science, Technology and Society, Cornell University USA
Nichols, K. Guild	Staff Member, OECD, Directorate for Science, Technology and Industry France
Nowotny, Helga	Executive Director, European Center for So- cial Welfare Training and Research Austria
Olby, Robert	Associate Professor, Division of History and Philosophy of Science, University of Leeds England
Olsen, Tore	Professor, Department of Physics, University of Oslo Norway
Olszewski, Eugeniuoz	Professor, Committee for the Science of Science, Polish Academy of Sciences Poland
Omholt, Anders	Director General, Norwegian Research Council for Science and the Humanities Norway
Rahman, Abdur	Professor, Centre for Study of Science, Tech- nology and Development, Council for Scientific and Industrial Research India

Ragnarson, Per	Research Fellow, Division of Heat and Power Engineering, Institute of Technology, University of Lund Sweden
Richta, Radovan	Professor, Institute for Philosophy and Sociology, Czechoslovak Academy of Sciences Czechoslovakia
Rip, Arie	Research Fellow, Department of Chemistry and Society, Gorlaus Laboratoria der Rijksuniversiteit te Leiden Netherlands
Ruge, Mari Holmboe	Head of Division, Sub-Council for Social Sciences, Norwegian Research Council for Science and the Humanities Norway
Sabet, Adel A.	Under-Secretary of State, Ministry of Scientific Research Egypt
Salomon, Jean-Jacques	Professor, Head of Science Policy Division, OECD, Directorate for Science, Technology and Industry France
Sandvand, Ole Johan	Staff Member, Institute for Studies in Research and Higher Education Norway
Schreiner, Per	Director General, Planning Department, Ministry of Finance Norway
Schroeder-Gudehus, B.	Professor, Institut d'Histoire et de Sociopolitique des Sciences, Université de Montréal Canada
Sejersted, Francis	Professor, Department of History, University of Oslo, Chairman of the Advisory Board of the Institute for Studies in Research and Higher Education Norway
Sirevåg, Torbjørn	Director General, Science and Research Department, Ministry of Education Norway

Skoie, Hans	Deputy Director, Institute for Studies in Research and Higher Education Norway
Spiegel-Rösing, Ina	Professor, Abteilung für Wissenschaftsforschung, Universität Ulm Federal Republic of Germany
Strømholm, Per	Research Fellow, Department of the History of Ideas, University of Oslo Norway
Svensson, Roger	Staff Member, Council for Planning and Coordination of Research Sweden
Swazey, Judith P.	Professor, Department of Socio-Medical Sciences and Community Medicine, Boston University School of Medicine USA
Tranøy, Knut Erik	Professor, Department of Philosophy, University of Oslo Norway
Vaa, Mariken	Staff Member, Institute for Studies in Research and Higher Education Norway
Vangsnes, Sigmund	Director, Institute for Studies in Research and Higher Education Norway
Wàlentynowicz, B.	Research Fellow, Polish Academy of Sciences Poland
Weingart, Peter	Professor, Forschungsschwerpunkt Wissenschaftsforschung, Universität Bielefeld Federal Republic of Germany
Wittrock, Björn	Associate Professor, Department of Political Science, University of Stockholm Sweden
Wynne, Brian	Associate Professor, School of Independent Studies, University of Lancaster England

Zacher, Lech W. — Professor, Institute of Philosophy and Sociology, Polish Academy of Sciences
Poland

Zahlan, A.B. — Visiting Fellow, Science Policy Research Unit, University of Sussex
England

Østerud, Øyvind — Assistant Professor, Department of Political Science, University of Oslo, Member of the Board of the Norwegian Research Council for Science and the Humanities
Norway