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ARTICLE

**THE SCIENTIFIC MEETING:
AN ANTHROPOLOGIST'S POINT OF VIEW**

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Social scientists customarily describe the structure of a community much as a biologist describes the structure of a living organism. Human societies are populations of individuals who interact by means of exchanges: exchange of goods, of services and of information. The scientific community is just such a population. But as a social group it is peculiar, in that it is diffused and informal. It has no specific territory; no common language; no shared culture. It owes its existence to a system of communication and exchange of ideas, and scientific meetings are a part of this system.

Now, the detailed analysis of how such a community comes about is of considerable interest to social science. Clearly the fact that one's identity as a scientist does not depend on birthplace, residence, national culture, institutional affiliation or political beliefs, indicates that there must be some set of shared symbols, values and norms which represent so to speak the portable habitat or home of this peculiar community. This is what we call a culture. Scientists share a culture; their membership and participation in the scientific community depend as their successive internalization of a common ideology. How does this internalization come about?

Socialization of Scientists

If membership in the scientific community is independent of the accidents of birth or any other external circumstances it must be a consequence of learned behavior. The process of education of scientists has to be crucial in this connection. This process is called "socialization" and it occurs as a prior condition of all occupational specialization in a society. In the case of science, of course, the criterion of membership is rather vague. Consider a physician or other member of the liberal professions: he too is required to go through a formal learning process, but if successful he is awarded membership in his profession by what anthropologists call a rite of passage: in this case, graduation and a formal diploma. There is a formal change of status. The diploma comes with a set of rules of behavior: if any of these rules are broken, the offender may be formally expelled from the profession. In the case of the scientific community, on the other hand, membership is more subtle. It is true that an academic degree is usually a requisite: however, the possession of a degree does not make one a scientist, and some famous scientists (like Cavendish or Mendel) never had a degree. The only strong requisite for membership in the scientific community is participation in scientific activities, i.e. production of knowledge. It should be added that even participation in the production of knowledge does not guarantee membership: participation in reciprocal exchange of scientific information is also required. Results must be published, and publications must be recognized. Conversely, the member must recognize other members' publications in order to deserve recognition. For example, Mendel was not recognized as a scientist until his work was discovered and used by colleagues working in his field: his was a posthumous affiliation to the scientific community. It is easy to understand how the search for recognition is precisely the central motivation of a scientist: his membership in the scientific community depends on it, and so does his status within the community (Hagstrom, 1973).

In science something similar to the socialization of children happens. The science student learns to set bounds to his intellectual universe by his choice of a particular discipline which restricts his field of interest. He then sets about to learn to use the techniques and the scientific language of his discipline. He also learns to distinguish and classify the members of his group, who teach him or whom he teaches, as well as various kinds of colleagues. As he begins working in the field he internalizes a set of rules of behavior which apply to this world of specialization, and he acquires a well-defined ideology. Thus, Jacqueline Fortes and myself have investigated the scientific ideology shared by a group of students in the biomedical sciences. These ideological tenets are not explicitly mentioned in the curriculum, they are not formally a part of the course work, yet the evaluation of students is largely based on their successful internalization.

This is as it should be. For the student who merely learns to handle the tools and experimental methods of a discipline remains a technician, and one who merely acquires the theoretical background of the field remains a teaching assistant or perhaps a historian of science. The practice of scientific research, on the other hand, necessarily requires that one think and act as a scientist. Science is an extraterritorial society, a community without enforceable membership controls. To belong it is indispensable to develop a powerful super-ego, capable of controlling the self and of postponing immediate rewards and gratifications for the sake of a continuing allegiance to the scientific community.

What is this ideology like? Essentially, it is nothing but a system of norms and guidelines of behavior amounting to a personal commitment plus a role identity. Every scientist is rewarded according to his faithful adherence to this ideology: his personal satisfaction depends on this. One might say that the scientific ideology is an invisible passport, which provides a scientist with an identity and a guidance for action. Some basic tenets we have found in our work on scientific ideology are the following:

Nature can be controlled.

Control of nature is possible through knowledge.

The scientific method is the way to attain knowledge.

Knowledge and control of nature is good.

Progress is possible through science.

Being a scientist means living for the search of truth through knowledge of nature.

These and similar tenets constitute an ideology to the extent that they commit a scientist to interpret his activity as a distinctive search for truth, which differs from other systems of knowledge such as religion or magic. A scientist is a kind of priest who must always stand by this commitment and pursue it actively. In order to honor this commitment he must strive to attain certain ideal characteristics. Interestingly, none of this explicitly contained in the curriculum yet the faculty members have no need to reach prior agreement among themselves. They all attempt to impress similar standards on their students. For example, a student learns that the ideal scientist must be self-disciplined in his work and in his thought processes. He must be able and willing to control emotional factors such as subjectivity, frustration, anxiety and envy. A scientist's life is a succession of existential crises, of depressions caused by everyday frustrations and by postponement of social recognition. A scientific career will not develop unless the scientist learns to cope with emotional interferences from this source.

Yet, the construction of a hardy super-ego is not enough. The eventual status of a scientist is not determined by his self-control but by his creativity, i.e. his capacity to innovate. Unfortunately, creativity is hard to define and even harder to teach. The creative impulse actually seems to run counter to self-discipline; it is a self-indulgence, a liberating process. It appears that creativity takes place through a relaxation of discipline which not only affects logical thought processes but even work routines and emotional controls. It thrives on enthusiasm, on motivation, on passion, on subjectivity and on an overblown self-confidence. Once the creative insight has been obtained, all these features of self-critical work, following a rigorous path of experimental testing and discussion, before it can be incorporated into the common domain of scientific knowledge.

Social Behavior of Scientists

Let us suppose that a student has succeeded in internalizing the scientific ideal which the faculty impresses on him at every step along the way to graduation. He may be talented: some professors will venture to say that he has creative potential, that he is promising. But is he a scientist? Not quite. First of all, his internalization of rules of conduct will be repeatedly tested along his career. There are many such rules and their observance is the criterion for belonging to the scientific community. For example, there are written and unwritten rules of cooperation, including rules of scientific probity, without which no scientist could trust the work of his colleagues and the scientific enterprise would collapse. There are rules for deferring to colleagues endowed with higher status and rules for dealing fairly with lower-status colleagues, within a value system which does not necessarily match administrative categories. Most importantly, a scientist must learn the rules of oral and written communication among scientists; no recognition of his work will otherwise be forthcoming.

Now, a community which is essentially nothing but a set of networks circulating information must set great store by the way this exchange of information takes place. Research on problems of specialized interest gives rise to a complicated structure of working groups, institutes, associations, and international unions by disciplines and fields. As a scientist participates in such organizations and groups he evidences in many ways his identity as scientist and his allegiance to the scientific community, because of his active use of communication at different levels, formally at meetings and in publications as well as informally in seminars and laboratory or classroom situations. Every single act of exchange of information is governed by rules and forms of behavior. In addition to the explicit content of the communication there is a distinct ritual; and this ritual serves not just the purpose of communicating information but also other purposes which I now propose to discuss.

Scientific Meetings as Social Rituals

Before I undertake to describe what scientific meetings are about, let me borrow an example from a more exotic society. The Eskimo, a society of hunters and gatherers, is spatially confined to a broad territory which a family group traverses in pursuit of seasonal activities. Thus, in fall the Eskimo hunt the caribou which migrates north from Central Canada; in winter they go north themselves for the purpose of seal hunting, and in spring they return to the south in order to trap game which emerges from hibernation. In summer they are found along the thawing rivers, fishing for trout. Each family moves on its own as it has no fixed abode;

the migration pattern is determined by ecological and technological factors.

How do the Eskimo succeed in maintaining their identity as members of a distinctive society? How can they share a culture which enables them to reproduce by means of marriage exchanges, and to transmit their technology from generation to generation? The Eskimo have a common language, a world view, a system of values, a mythology and an artistic tradition. It is true, of course, that socialization takes place largely in the family group. Parents teach their children to speak to handle basic tools and to behave as members of the Eskimo family. In summer, however, numerous family groups meet on the river banks where hunting and fishing are plentiful, and celebrate communal rites. It is the season for fairs, barter, festivals, exchanges of women, and for the great religious rituals led by elders and by prestigious specialists or shamans. Now is the time for ritual invocations of the tribal gods, for recitations of myths concerning the origins of the Eskimo people, and for affecting a stylized behavior which differs from daily utilitarian behavior. It is different because its purpose is to transmit and reaffirm the ethic and esthetic values, traditions, beliefs, and other ideological formations. The total effect is to reinforce the allegiance of the family to a wider society: in this case, to a nation since the Eskimo society endowed with a territory and with a distinctive culture.

There is a parallel between the life of scientists in a community, and the life of the Eskimo nation. In both cases the society is fragmented into smaller solidarity groups. Each scientific "family" socializes students, who must eventually become members of the scientific community, by teaching them the language, technology, ideology and principles of social interaction of a discipline. The student absorbs the history of the field and of the particular group to which he will belong; the names of heroes and anti-heroes; the crucial experiments; the system of rewards and punishments.

Moreover, since all this is not quite enough for an individual to acquire an immediate and basic feeling of belonging to a community, he is encouraged to participate periodically in scientific meetings. These major tribal get-togethers are ostensibly organized (as among the Eskimo) for the purpose of trading: in this case, the exchange of information and ideas, which is the stock-in-trade of the scientific community. This exchange is endowed with significance through ritual. Thus, competition between groups or scientific "families" is ritualized and conflicts are symbolically expressed and resolved in the interest of the community. Prizes and symbolic awards of various kinds reflect the status of individuals and confirm pecking orders among groups and within each group. The use of symbolic titles and honorific duties emphasizes the social importance of participation in these collective rituals and consolidates the communal values internalized by each individual thus strengthening this sense of identity and of belonging. As we can see, there is more to a scientific meeting than an instrumental exchange of data and information.

Communications presented at a meeting imply a kind of recognition which is not abstract but direct and immediate. The fact of being scheduled on the scientific program is a legitimation of a scientist's membership in the community. Response of the audience to the presentation and even the relative position of a paper on the program represent instant indicators of recognition even though eventually the paper may be published and referenced by others. The latter affords perhaps a more lasting measure of status and recognition: but oral presentations at meetings provide an opportunity of speeding up the turnover of scientific information

and personalizing the exchange of data together with appreciation of the same. It is like a fair, where each producer obtains a direct feel for the demand of his product and for the toughness of the competition, through the response of those in attendance.

At a more general level, and in analogy with the ritual gatherings in certain traditional societies, a scientific meeting is an arena where the hierarchical structure and the symbols of the community are publicly displayed. The meeting starts off with a plenary session, where the authorities officiate in symbolic roles which are intended to express the continuity of the association and its status-awarding power. As the scientific program unfolds, each session is presided by a chairperson who directs the proceedings like a priest in a ritual. Appointments to these various offices also reflects internal rivalries and rifts as each faction competes with others for awards and distinctions: these tensions are resolved each year through the public appearance of the new board of directors and the roster of organizers, chairpersons, invited speakers and award nominees on the program. Official awards are symbolic occasions for recognizing and stressing the values which the community holds up to its new generations, much as among the Eskimo when they bestow public honors upon their best hunters and their wisest shamans. The presentation of an awardee is a compendium of those personal qualities which the community deems worthy of recognition: hard work, ideas leading up to the breakthrough, participation in the general development of the discipline, devotion to students, plus certain personality features such as a recipient's "deep vocation," his "exemplary modesty" and so on. New award winners are publicly compared with previous winners and are thus incorporated to tradition. More than hero worship this is an essential affirmation of the continuity of science and the enduring validity of its norms.

The organizational layout of sessions and symposia also reflects the internal structure of the discipline itself. Scheduling of sessions is related to the status of the topics and may change from year to year; the choice of organizers and chairpersons contains dues and subtle messages of recognition. The order of papers in each session is related to status, as is the distinction between invited papers and unsolicited communications. If several sessions take place simultaneously, room assignments reflect the relative importance of each topic in the view of the organizers. Altogether there are nearly endless opportunities for awarding, withholding or withdrawing recognition in a scientific meeting.

Finally, there is an aspect of scientific meetings which belongs to ritual in spite of its informality. I am thinking of the broad area of personal interaction from whispered comments in a crowded meeting room to buttonholing in corridors and handshakes or slaps on the back in so-called "social events." Octavio Paz has pointed out that parties may be a respectable research topic for understanding a culture. Indeed, partying among scientists provides an endless source of information on life among this fascinating human group.

Turner says that every society requires breaking up its routine functioning by unstructured episodes, which he calls manifestations of communitas. The main feature of these episodes is a temporal suspension of some rules which are normally held to regulate conduct among members of the society. Rules of conduct are essential in any society, as no one could walk by himself on a crowded street or interact with a stranger unless such rules were observed. Chaos and unbearable anxiety would result. Then why discard these rules even for an instant? The reason is that the mingling of people belonging to different status

levels in a relaxed setting, where almost everything goes, serves a purpose to the extent that it fuses personal identification into a community. Ritual drinking and orgiastic socializing provide an experience of the existence of the community which bonds people together in a sense which transcends rules and hierarchies. The community becomes a tangible higher reality which requires our collective allegiance, and we are all levelled in our common acknowledgement of this reality.

Episodes of communitas characteristically produce a feeling of well-being, a special euphoria, among participants. This is because the temporary suspension of status distinctions provides a breathing space, which allows relationship between individuals to fall into place. Conflicts and tensions arising from the hierarchical organization of social roles are allayed or resolved through personal interaction. Thus, students talk to recognized authorities in the field, an opportunity which confers an intimation of membership in the scientific community. In other words, eventual formal recognition as a scientist is preceded by a number of informal occasions where fragmentary recognition is achieved at different levels. There is a wealth of information in these episodes of communitas: social connections are made across hierarchical lines, and scientists occasionally may be rewarded by social recognition in a way which is more immediate and nearly as effective as the formal recognition which may be forthcoming later in their scientific careers. The formal rigidity of structure which provides the setting of scientific meetings is effectively balanced by the informality of "mixers," social events and spontaneous parties where people run into each other and often make interesting contacts and exchange relevant ideas.

The scientific community renews itself in such meetings, and reaffirms its loyalty to some of its more fundamental principles. A meeting is a public statement of mutual recognition of our identity as scientists: this is a far more portentous event than a mere exchange of ideas or even a show of colors or a display of legitimacy and hierarchy. A scientific meeting is an arena for all these purposes: new knowledge is discussed and hierarchically organized, new scientists are critically tested and incorporated, recognition is awarded and withdrawn. But it is also a ritual of tribal allegiance, an episode of communitas.

References

- Hagstrom, Warren. 1975. *The Scientific Community* (Carbondale, IL.: Southern Illinois University Press).
- Paz, Octavis. 1976. "The Flight of Quetzalcoatl and the Question of Legitimacy." Pp. ix-xxii in J. Lafaye (ed.), *Quetzalcoatl and Guadalupe* (Chicago: University of Chicago Press).
- Turner, Victor. 1977. "Symbols in African Ritual." Pp. 183-194 in J. Dolgin et al. (eds.), *Symbolic Anthropology* (New York: Columbia University Press).

REVIEW ESSAY**CASE STUDIES: A PROMISING WAY TO ASSESS TECHNOLOGICAL IMPACTS?****A Review Essay of**

Barry M. Casper and Paul D. Wellstone, **Powerline: The First Battle of America's Energy War**. Amherst: University of Massachusetts Press, 1981. And,

Adeline G. Levine, **Love Canal: Science, Politics and People**. Lexington, MA.: Lexington Books, 1982.

In recent years there has been a considerable increase in the general level of awareness about potentially serious side effects of technological development. Hence, it is not surprising that technological disasters and siting disputes have become an area of interest to social scientists in a variety of disciplines. In the past, most examinations of accidents and proposed projects have been carried out by involved technologists or analysts contracted to produce mandated impact statements. While the environmental and public health portions of these studies, which draw on clear bodies of technical expertise, have started to reach a reasonable level of sophistication, the other social impact sections are generally quite crude. They receive scanty attention during the review process in any case. It is therefore a noteworthy development that professional social scientists have begun to look into this subject through strategic case studies involving well known incidents. Fairly detailed monographs are at last appearing which explore and report on the issues involved. Two such books, sociologist Adeline Levine's study of Love Canal and political scientists Casper and Wellstone's account of the siting of a Powerline in rural Minnesota will be reviewed here.

In keeping with the traditional interests of social scientists, each account focuses on the development of a politically significant social movement at the grassroots level. The cases are similar in that each resistance movement was sparked by a perceived threat to the homes of those directly involved. The accounts both trace the process by which solid or average citizens become politicized and gradually learn how to effectively mobilize and use influence effectively. There the similarities end, however, as the groups involved and the settings are very different. One group wins, the other loses, one group is resisting the creation of a potential threat while the other people find out that they are victims after the fact.

Powerline involves a direct action resistance movement by rural farmers who are led to slowly set aside their law abiding instincts to mount an all out effort to stop the proposed construction of a high voltage D.C. powerline across their cropland. They come to see the line as a direct assault on their way of life, and move to greater and greater extremism after imminent victory at the county level. They watch their means of legal recourse dwindle and finally disappear. Ironically, the state procedures had been reformed to encourage citizen participation via mandated public hearings. Unfortunately, the narrowly construed grounds for possible objection to the project had the effect of making the matter a technical decision, and put it effectively beyond the control of those most affected. The fury of the farmers takes them from open debate and mass demonstration to passive resistance and then harrassment. Finally there are open confrontations with the police and outright acts of

sabotage. At one point half of the state police force had to be mobilized to control the hundreds of farmers who awaited the survey teams each morning. The inventive obstruction of the locals was quite impressive even before they figured out how a few men could topple the towers quietly in the dark. In a final desperate effort to take back control of the apparatus that seemed to be oppressing them, they ran a candidate for governor--and garnered a third of the statewide vote. Though they failed to stop the line, they so altered the social and political atmosphere in the state that the authors consider it unlikely that another such line could be built in Minnesota during the remainder of this century.

The accounts of the encounters between the authorities and the farmers are drawn with vivid imagery. The inclusion of bibliographic sketches of key resistance movement figures that bring one up to the point that they enter center stage is one of the highlights of the book. Maintaining the participants' subjective reinterpretations of the events in which they star, amidst revealing personal life histories, gives the account special shock value and integrity. The shock comes from finding out that leading farmers totally committed to preserving the land as a sacred trust against urban people who don't understand how immoral the line is, are actually technically trained individuals. Their job histories include sojourns at places like Raytheon and the Bureau of Mines and some have advanced degrees. These people are actually technical dissidents and dropouts who are raising a challenge to the way of life they left behind.

Love Canal involves the residents of a working class neighborhood in Niagara Falls who reluctantly awaken to the fact that their neighborhood school is built on top of an abandoned chemical dump "sold" by the Hooker Chemical Company to the city for a dollar and a waiver of responsibility. Subsequent construction of houses, roads and sewers had perforated the original clay cap and a witches brew of toxic chemicals was leaking through the neighborhood underground. It appeared in various forms, ranging from smelly black ooze on the cellar wall or a swimming pool, to exploding rocks and a miscarriage rate several times the national average. On finding that they may already have received serious exposures to carcinogens, as well as suffered a range of immediate physical discomforts, the residents organize an increasingly media wise political campaign to get enough of an indemnity from the state to allow them to abandon their contaminated and worthless homes and leave the area. The main portion of the account involves the process by which they organized themselves to effectively mobilize resources and press their demands on a political apparatus greatly concerned about setting precedents that would involve the state in a quagmire of open-ended legal and financial commitments.

The homeowners association led by Lois Gibbs receives the greatest attention of any group in the book. It moved gradually from peaceful demonstrations and information meetings to open confrontation, culminating in a mediagenic episode in which two EPA officials are taken hostage. The incumbent Governor, under the intense pressures of a reelection campaign, finally made the unprecedented promise to buy their homes. This was far from the end of the story, however, as it still had to be determined just whose homes were affected and which governmental bodies would actually pay for what. There was also the question of what to do to halt the slowly moving chemicals. Initially, 239 and ultimately more than 500 families were relocated as of 1981, (with hundreds of applications still pending). Total cost to all levels of government combined exceeded \$68 million.

Given the dramatic nature of these events it is not hard to see how they attracted the attention of social scientists. The cases gave them ample opportunity to explore classical sociological and political themes ranging from specialized topics in organizational behavior and the impact of the mass media, to the subtleties of selective perception and the sociology of knowledge. All this is in addition to the obvious questions about resource mobilization, social movements, stratification issues and the distribution of power. The role of expert authority in legitimating given courses of political action also received considerable attention in both accounts.

Others will undoubtedly consider in detail the potential contribution of studies focusing on the social costs of technology to the social sciences and carefully enumerate all of the potential connections with classical sociological theory cases such as these offer. However, the two extended case studies under review in this essay are of interest from another standpoint as well. They present a golden opportunity to assess the promise of classical social science methods and perspectives for the study of technology. What the potential contribution of qualitative case studies, such as Powerline and Love Canal, is, or can be, for the emerging interdisciplinary field of technology studies, will be the focus of the remainder of this essay.

There are two main reasons for considering the cases primarily from this standpoint. One is that it gives me an opportunity to briefly review the state of the field in technology studies and issue a timely warning to social scientists who want to follow the lead of these authors and study technological incidents. There is a danger in simply assuming that resource and power perspectives current in the social sciences can be routinely extended to cover technology disputes. Both of the present case studies did so and it worked out quite well in the Love Canal case, though it was not fully appropriate in either of them. A serious problem of interpretation which arose in the Powerline case can be attributed directly to the inappropriate extension of a power framework to a sequence of events that can be explained another way. Further, the alternative interpretation would be more in keeping with the broader technology literature. Of this more will be said later, but for now, let me note that it is unfortunate that the structure of Casper and Wellstone's account does not provide much latitude for post-hoc analysis of issues not anticipated by the authors. Levine's account is much more complete and satisfying in this respect, and its value as a policy relevant study is enhanced as a result.

The other main reason for taking a technology perspective on these cases is methodological. That Levine's account escapes many of the criticisms to be made of Casper and Wellstone's is attributable in large part to her methodological care. Case study analysis is part of a long standing tradition of fieldwork in sociology. In political science, case studies are traditionally more likely to involve the analysis of watershed legal cases or campaigns which operate in a more structured social context. Levine's greater familiarity with the canons of qualitative analysis, and the observer's role, along with her self-conscious application of cross validation techniques, helped her to produce a more balanced account of the incident in its social context. Casper, who we later find out became a full participant in the countermovement against the powerline by running for state office on the movement's party ticket, was not as careful in this regard. The result is that Levine produces a more credible and objective account, despite the fact that both Casper and Levin side with the citizen's groups in the end and both focus on their respective movements

tactics and successes.

Whether the clear difference between the accounts' in completeness and balance is primarily due to the fact that Casper was a participant observer, while Levine was an outside observer, or derives specifically from the different conceptual orientations and methodological sophistication each brought to the study is an important question. The answer to it will determine the range of applications case study analysis might appropriately have in the study of technology-society issues.

There has been a strong tendency in this field to avoid the use of case studies when assessing the impact of proposed technology projects and accidents such as those explored by the studies under review. The reluctance to use this technique stems from several sources, but two are particularly worthy of note in this context. The important but controversial role insider exposees have played in the technology literature and the high stakes involved when impact studies are undertaken as part of a legally mandated impact statement make "objectivity" an extremely important issue in the field. The prevailing view, which is particularly strong in the policy relevant impact studies, is that qualitative case studies cannot be done so as to avoid unacceptably high levels of subjectivity and the nature of such studies makes it hard to review the data base and assess the degree to which bias has been involved in the drawing of conclusions.

This harsh view is understandable when one takes into account that the most common example of a case study account in this area typically appears as part of an exchange between the dissidents in a field or organization and its defenders. The public is not often in a position to know about such matters because only well placed people with the right technical training are privy to them. Hence, there is a special value attached to "whistleblowing" exposees by knowledgeable insiders who allege faculty judgement or wrongdoing on the part of former employers or colleagues that endangers the public or the future of the field. On the other hand, these are by their very nature untrained participant-observers' accounts in which the reporter is openly partisan and typically has a narrow range of experience. The degree of balance and objectivity that can be achieved under such conditions is a hotly debated point in the field. It is also a matter of special fascination to concerned scientists and technologists, who often go to extreme lengths to produce balanced and credible accounts, and those administrative superiors and legislators who are called upon to evaluate their accounts. In one recent example, Daniel Ford used the Freedom of Information Act to force the NRC to release the private files of an in-house gadfly to document his case against the atomic establishment (see the Cult of the Atom, D. Ford, Simon and Schuster, 1982). People in this field hope that the social scientists who are starting to show an interest in technology disputes will teach them something about how a fully involved observer can produce a creditable objective account, and how one can tell if an account deserves to be taken seriously.

The strength of feelings about the need to minimize subjectivity in this field is most evident in the area of impact statements. Despite widespread dissatisfaction with the narrow purview and general irrelevance of social data typically offered in federally mandated impact studies, there is a strong sense that it is better to know less, but to be sure of what you do know (and have the relevant data base be subject to review) than to incorporate insights of a

far more penetrating nature (on the basis of a complex, diverse, unsystematic body of information) and risk legitimating someone's subjective interpretation as part of an officially sanctioned impact statement. Hence, impact statements tend to be heavy with the trappings of statistical analysis. But the census data, economic projections and sometime survey data featured so prominently in these reports are generally too crude to elucidate many of the most relevant issues. Those issues that are subtle, such as the quality of community life, or global, such as the cultural milieu or public trust, are the hardest to get at in this way. Even gross effects that are not amenable to statistical analysis, such as the development of interest groups and shifts of power and political involvement, tend to be mentioned in passing when noted rather than integrated into the body of the analysis. Though the available statistical materials are not usually very revealing in isolation, they have an air of objectivity and are easier to defend on procedural grounds.

It is easy to see why a cautious attitude would prevail, given the political context of this type of policy analyses and the high stakes involved. But, given the nature of the conceptual problems plaguing this area, it is clearly time to reconsider the potential contribution of qualitative case study analysis. In depth study of strategic cases has much to recommend it to a field where the challenge of complexity and interaction effects is great. A broad open-ended approach is needed to allow one to capture and incorporate diverse phenomena documented in various ways and yet the scope of inquiry must be limited to make the task manageable. Case studies are actually one of the few options left when social scientists are faced with subtle phenomena that operate at several levels of analysis simultaneously with dauntingly complex interrelations to one another.

Given the dearth of alternative ways to capture the range of the phenomena that should be under study, the two case studies before us will be scrutinized in terms of the promise they hold out. They are effective ways of combining the requisite subtlety of analysis with an acceptable degree of balanced objectivity and rigor. In the following pages, I shall demonstrate that, while each study has flaws in one area or another, the two cases in combination suggest that this qualitative approach under review, at its best, can fulfill the stringent requirements for policy related study of technological impacts. A broader commitment to studies of this type would almost certainly enhance our understanding of the social challenges facing a technological society. The potential contribution of such studies to inform the policy process is also great. It is now time to return to the case studies and document these comments and conclusions in detail.

Tradeoffs Between Balance and Depth

As noted earlier, one thing that makes these case studies of interest to technologist and others in the multidisciplinary field of technology studies, is that they are done by professional social scientists. The degree of depth they can achieve as outsiders, the types of phenomena they deal with and the degree of objectivity they are able to achieve as they get involved in these disputes are all matters of great interest to those assessing the promise of this analytical technique.

The Casper and Wellstone account of Powerline represents a mixed performance as judged by these criteria, since it amply illustrates both the special virtues and potential flaws of the participant-observation approach. Their account achieves deep insights in that it is revealing about key motivational issues

associated with the meaning and feeling of participation in the resistance movement. These are essential to understanding the movement, but not sufficient to evaluate the controversy as a whole. In short, the Powerline case takes one deeply into the subjective realm of the resistance movement and conveys the depth of feelings stirred when a community is disrupted and symbolically violated by having its values scorned and its way of life challenged. This is valuable as it means the account captures dimensions often lost in statistical treatments. Further, it is expressed in a powerful way using compelling rhetoric that only a participant, who is personally stirred by these things, would be likely to use.

Toward the end of the book, far too late actually, the reader finally learns about the actual extent of the lead author's involvement with the movement. He ran for public office on behalf of the movement in their last ditch effort to stop the powerline. This degree of involvement exacerbates the potential problem concerning the objectivity of the account. They literally ignore the other point of view in their account, except to offer a partisan interpretation of the opposition. This image results in a caricature of opposition representatives, primarily lawyers, with whom the farmers come into contact while whole chapters are devoted to portraying key people on the movement side. There must now be open concern about the extent to which the overall interpretation of the issues is merely an expression of the movement's partisan debate position. The last sections do read more like a last statement to the jury about how unfortunate the outcome was rather than an effort to understand the implications of the movement's ultimate failure.

Since the authors are not particularly conscious of methodological considerations they clearly do not achieve the level of objectivity and balance theoretically possible in a participant observation study. These limitations do not totally discredit the study which still remains a superior account of the movement viewpoint and the motivations of the participants. From this standpoint the biases in the report can actually be considered a virtue. Overall the study still serves a useful purpose as a model of how to get at subtle, relevant phenomena. It broadens our understanding of what factors affect the meaning attributed to this project by those citizens most directly affected and documents how important those matters of interpretation are for the public reception of technology.

Levine is far more self-conscious about the methodological side of her study and makes strong claims about the value of systematic observational and analytical techniques. If anything, she overdoes this and seems a bit defensive about the subject. She admits that her greatest contact was with the residents but that she also "tried to find out as much as possible about all sides and understand the perspectives of people in various positions."

Levine stops just short of being a participant in the full sense, but remains an interested observer. Still, her personal involvement is such that the most striking success of her account is that despite taking sides, she succeeds in achieving a balanced understanding of the position and the rationales of (all) the main parties to the dispute. The overall context of action and decision, and the ways in which the antagonists react to one another, is laid out with unusual completeness. Though her main focus is on the Homeowners Association and in particular, its leader, Lois Gibbs, the result is that one

still has a good feeling for the movement's interpretation--and much more, since there is something with which to compare it. Levin admirably lays out the context of decision, a task that takes several chapters since three interacting levels of government (local, state and federal), various governmental agencies, a local activist group or two and several independent scientists are involved. It appears that she has really come to understand the outlook and "worldview" of the government representatives as it affects their interpretation of the situation almost as well as that of the resistance movement.

This is not a result of uninvolvedness, a non-judgemental attitude or personal gifts of empathy on her part. She has worked at achieving a balanced, overall view and distinguishes carefully what she can see and knows from what the actors knew at key decision points. There have been repeated interviews, and media coverage has been carefully cross validated against other sources before being accepted as sufficiently factual to use. Public statements are compared to perceptions and private statements. By remaining an outsider and depending on carefully screened and validated second hand reports from various perspectives and vantage points, Levin achieves an admirable overall understanding of how things fit together. Context, tactics of the movement and bureaucratic maneuvers are laid out in relation to one another. The price she pays in terms of losing some of the feeling and meaning of participation in the resistance movement, and the rich character sketches that are the highlights of Casper and Wellstone's treatment of Powerline, is evident but acceptably small. Though Casper and Wellstone do it more often and more systematically, Levine commonly manages to hint at the meaning of events as seen by the movement through the use of numerous interview quotes as flashbacks following recollections of events she has juxtaposed with the public record. There is other evidence of her close continuing contact with key individuals on the movement side of the dispute--such that a few individuals do emerge at the end having been revealed, bit by bit, throughout the account. In any case, the losses in detail of this kind are more than made up by the comprehensive nature of the picture that results from Levine's methodology.

From the standpoint of policy study, Levine's account has several virtues but a few are worthy of special note due to their political significance. First, though the subject is victims and the clumsy efforts of an unresponsive bureaucracy to help them, the treatment is not maudlin or overly sympathetic. Indeed, Levine has a knack for telling you what movement representatives think is going on, or say they have learned, without accepting what they say as the truth. At several points she consciously elaborates on their views to fill in the larger picture they cannot appreciate. The result is that the local often come off as unreasonable or naive and occasionally more cynical than is warranted. Second, while Levine is slow to judge she does not ultimately withhold judgment but takes sides with the residents for reasons that are fully explained. In essence, Levine concludes that exigencies have so twisted the official logic that the authorities are losing touch with reality. Her stance is credible and the open critique lends power to the account. Finally, even after taking sides, Levine retains a sympathetic understanding of the situation of the officials and documents a second unnecessary tragedy in addition to that created by the oozing chemicals. By way of warning to officials faced with similar situations in the future, she documents how the officials and the people they are honestly trying to help become estranged from one another and move from cooperation, to mutual suspicion and finally open antagonism. Then she explains how the resulting situation effectively sabotaged the scientists' efforts to collect

data systematically and analyze it objectively, further politicizing the whole situation as decisions had to be made on the basis of questionable and incomplete information.

- The root cause of this deteriorating situation, according to Levine, is the unwillingness or inability of the state officials to confide in the affected citizens the nature of the legal fiction they had decided to use to get federal aid on their behalf. Why the federal government officials in charge of emergency or disaster assistance saw the situation as they did is a story in itself, as is the decision of the state to accept these interpretations rather than challenge them, given the precedent setting nature of the situation. None the less, injustice and dangerous actions involving the handling of the chemicals flowed from that decision, and the residents soon came to question both the competence and goodwill of the officials assigned to them.

Since the key participants were out of touch with each other on this crucial matter of why things were being done as they were at Love Canal, the state found itself in a position of publicly denying certain realities of the situation to those who were living it. Forced to defend their interpretation against the confused questions and later the outraged attacks of residents (who increasingly tried to develop independent sources of information) the officials hardened their position and even had to actively suppress the resident's efforts to document and disseminate their version of the situation, for fear of jeopardizing the possibility of getting outside aid. Soon the state was saying inconsistent things at different times and to different audiences and the officials' credibility collapsed. With the loss of trust in those who controlled their fate, the residents lost their patience and turned to overt pressure tactics, in a desperate attempt to save themselves from financial disaster and the looming threat to their health. The state could not respond to these demands without vastly increased resources, and to get them the fiction had to be maintained. As a result the escalation process soon got out of hand.

In this hostile atmosphere of bizarre actions and a war of interpretations, scientific research of an objective nature was either a threat or a political resource--and both sides buffeted the researchers associated with the other interest group. Efforts to do research proved to be both futile and dangerous, as Levine's accounts of the fate of different scientists amply demonstrated. The best developed instance of this involved Paigen, the toxicologist who befriended the residents, helped them interpret the information they were given by the state representatives, helped them organize their own study, and who developed an interesting hypothesis about what affects the leaking of chemicals through the earth. This was based on residents' observations and reports about what the area was like in the past. Unfortunately, a hearing for her theory would indirectly legitimate the claims of the homeowner's association and cast further doubt on the competence and motives of the state department of public health. Paigen's reward was harassment at the state level and to get a hearing she had to raise the stakes by seeking forums at the federal level where the state was applying for funds. The threat was obvious and retribution by the state, which indirectly employed her, was rapid and severe. Ultimately panels of reviewers convened by the state were induced to proclaim that there was never clear evidence of a health threat at Love Canal after all, but by then political action had led all the residents to be moved out.

Clearly the overall account is a very disturbing one filled with a variety of lessons. However, from the standpoint of technology study, the fascinating thing is how Levine managed to be relatively objective, sympathetic and take sides all at the same time. In effect, she has belied the conventional wisdom that suggests a case study cannot be objective, particularly if the person producing the account is deeply involved or takes sides. Given that insider exposees are an important genre of literature in this field, and provide information unavailable from any other source, the possibility that a report with the character of an exposee critique can be done so as to be complete, balanced and credible is important news.

The next logical question is whether this feat requires full fledged sociological training or whether careful, motivated technologists and scientists with a model to follow can do the same. If they can, the work is of great potential significance to the whole field. If not, there is a lot of work to be done in this area by social scientists.

Though Levin emphasizes methods in her explanation of what gives the case study truth value, actually much more than that went into it. The question is not whether practicing technologists will be able to gather and cross validate accounts from various sources. Clearly they can. What is not clear is whether they would be familiar with the theoretical perspectives and concepts that led Levine to ask the questions that she did in the way that she did. Probably not without some training in the social sciences. However, is that an essential ingredient or would other questions do as well? Actually, the answer seems to be that the sociological perspective that enlightened Levine's study is a double edged sword. There is evidence that it can as easily be wielded in such a way as to obscure important issues involving the control of technology.

Theoretical Issues with Empirical Implications

The traditional stance of social science regarding technologies is to construe them as passive social factors, tools or resources to be understood in relation to stratification and power issues. In short, their impact is to be understood in terms of the aims or interests of the groups, organizations or individuals ostensibly controlling their development and use. It is not yet clear, however, whether or not it is appropriate to extend the traditional power and resource perspectives current in the social sciences to technology studies. At the minimum they will need to be carefully reconceptualized in light of some of the special challenges involving the control of technology. At the extreme, technology studies will end up radically reshaping social science itself as efforts are made to accommodate its potential status as an independent social force of increasing potency and importance.

Actually there are important theorists among both those optimistic and pessimistic about the social impact and implications of technology that question this interpretation. Few specializing in this field today accept the idea that technologies are neutral. The question is whether to view them as independent social forces possessed of an internal logic that impose themselves on technologists and society in general or as subtle influences on men's thinking, of a cultural nature. In any case, technological developments and applications probably cannot be understood entirely in terms of those who attempt to use them for their own ends. Technologies are complexes of men and machines that seem

to develop an internal logic or mindset with a deep hold on those most involved with them. The issue of control is greatly complicated by this fact and studies exploring this phenomenon are urgently needed.

The upshot of this is that when a technologist writes an expose^e about his or her field, and criticizes superiors' or colleagues' judgement, the issue may not really be competence or collusion at all but a call to break free of that special form of "groupthink" to which technologists and scientists seem to be prone, before disaster overtakes everyone involved.

When a narrow logic captures a field or organization entrusted with the public safety and becomes a hotly defended conventional wisdom or seeming obsession with those in charge, how should one weigh the few discordant reports of those who cannot or will not conform to the prevailing view? When unforeseen side effects accumulate along with the development of a technology on which society is increasingly dependent and a small band of dissidents says it needn't be so if the logic of the system were not so twisted and irrational, how should the critics be treated? When should one or a few outsider critiques be taken seriously when they are arrayed against the vast majority of qualified experts who see no insuperable problems and assume that the logic of development in their field is as rational as it is self evident? How can the dissident avoid being written off as a hysteric or an incompetent? Surely the concerned technologist wants to know how to approach this problem and the responsible administrator, who suspects that they are dealing with a kook but wants to stay the hand of professional retribution long enough to examine the odd unfair charges leveled against his or her organization, is keenly interested as well.

Clearly it is more than her method that allows Levine to beam a ray of hope to those who want to demonstrate how a given logic can distort the effort to effectively address a problem, it is I would argue, her perspective drawn from her sociological training. One of the traditional resource concept of technology to describing the official reality, yet it is not clear that she saw this as a technological issue. Faced with the phenomenon sometimes known as the "technological mentality" and a powerful technical mindset as well, she construed it as an expert attitude and an organizational reality. Using these familiar concepts she discovered that they played a key role in the drama unfolding before her. She had no idea about the special significance her descriptive materials would have for the debates about the nature of technology and the circumstances under which it can get out of control to produce bizarre effects. Hence, the sociological perspective both guided her and blinded her, but the completeness of her account assured that all the essential elements were considered one way or another. Still, her work stands as a model for technologists struggling against techne-logic as well as those who pierced through it and are trying to mount a credible critical analysis. Their ability to express and understand the multiple perspectives of participants will then become the mark of a critique worth special consideration.

Casper and Wellstone, were not so fortunate, since their account is less complete and they did not anticipate this type of issue. Serious problems of interpretation result when strange things, such as obsessive behavior and irrational decisions occur. From a traditional "power struggle" perspective they can only account for such phenomena by positing new outside actors influencing the decisionmaking-essentially, a conspiracy theory. One finds that, although the authors did not collect interviews reflecting the rationale and interpretation of the utility, they are really quite interested in the logic

of the people who initiated the powerline project. One cannot reconstruct the decision context with the information they offer, so they are forced to indulge in considerable indirect inference, and post hoc analysis as well as outright speculation in trying to ferret out the meaning of the following sequence of events. First, an innovative plan to use local coal of low quality on site and transport the energy is devised. However, a rural coop utility with special access to low cost federal loans is undertaking the project, though most of the power is not for its service region and will simply go into the grid. The contractor selected to mine the soft lignite reneges on the initial deal and more than doubles the quoted price. The utility goes ahead with the deal anyway, though it is now cheaper to ship in higher quality coal by rail than proceed with the experimental mine and DC powerline project. Major resistance to siting the line is overcome at great cost with severe delays and still the project is not canceled. The financial position of the utility is severely effected by the whole process and it is necessary to sharply increase the electric rates of those in the region that resisted the line most strenuously, to pay for it. A concerted effort to take back control of the co-op company is provoked, and it is incredible that any responsible decisionmaker could be so shortsighted and stubborn.

After tracing the relationship between the co-op and several regionally powerful utilities in MAPP (the local grid), the authors conclude that a gross error has been made in pursuing this project given the lower priced alternative of shipping coal. They can only account for this type of error as due to influence peddling or some larger ulterior motive, so they conclude that the whole thing is probably a scam to get public funds to finance something that will benefit private companies. Portions of this analysis are convincing and represent good detective work, however, the whole thing is an untested notion that sounds like a countermovement conspiracy theory. Actually, such a narrow view and severe miscalculation is an unworthy of a clever conspirator as of a competent administrator. No, the whole thing has a flavor to it that is very familiar to experienced technology watchers, and is far more interesting than it seems.

By failing to explore and describe the context of the decision from the utility side at the point of project initiation, in the balanced fashion illustrated by Levine. Casper and Wellstone have passed up a golden opportunity to look into the extent to which a "technological mentality" or organizational mindset is operative. This is truly unfortunate in their case, as the central theme is implicitly the social control of technology; the powerline project's tendency to develop momentum and seemingly get out of control is what they are documenting. Hence, they fail to deal with one of the most interesting and far reaching theories about how technological means become ends in themselves and become autonomous, or self-justifying at a certain point in their development, and leave their whole interpretation open to criticism.

Examining the case from that standpoint for a moment, it is not hard to develop an alternative scenario for the seemingly irrational, self-destructive and obsessive behavior of the utility, during the course of the decision making process. Imagine a situation in which the inhouse technologists, faced with a management concerned about rising energy costs, are convinced that the wave of the future is to use low quality fuels in bulk at the site of the mine and transport the product. They know that the key problem in doing this is the severe transmission losses and that at high power levels D.C. transmission, though rare and experimental in this country, minimizes those losses. They

want to pioneer doing this at the level of a commercially viable system, but not commit the whole system to it, so that a relatively small member of the consortium, that can and has been carried by other utilities in the system should the project fail, takes on the project with the encouragement of its partners. It's a technically sweet project, and those involved develop a deep psychological commitment to making it succeed. The inhouse group is convinced what they are doing is important, nay necessary-the hope of the future. Such an attitude was clearly evident in the early days of nuclear power.

Under such circumstances the project cannot be understood simply as a means to the immediate end of producing cheap electricity to the utility-but a new exciting concept that extends the state of the art and requires the creation of a special complex of men and machines capable of developing and supporting the new technology. This is largely formed or committed well before anyone thinks about where the construction will occur. It is assumed a site for the necessary facilities will be found. Simpler alternatives are set aside, careful and expensive plans are made, and then a key contractor develops doubts and tries to withdraw by announcing massive price increases. Though these are painful, they are absorbed, because too much has happened now and the economic investment, though large, is nothing compared to the psychological investment of the organization at this point. Given the assumption that energy costs will rise, and this source is cheap and abundant, the project will simply be a somewhat longer term investment. But because it is new all it really has to do to justify itself is break even.

This is probably what the several political insiders were aware of, the logic and momentum of the project before it was announced, that led them to tell the activists that it was too late to stop the line-it had gone too far. To the activists, noting that no ground was broken yet and the project was questionable on cost effective grounds given other alternatives, this sounded like politics, like the fix was in and some powerful person might suffer a loss of some kind if it didn't proceed. But surely it was not too late to decide not to build it. When they committed themselves to blocking construction they seem to have been thinking of themselves in a battle with powerful background figures. Since to them the project was wrong and not cost effective, they could conceivably obstruct it long enough to run the economic and political cost to unacceptable levels and the elite behind it would withdraw the idea rather than take the loss or be forced to do so by the others who would.

Only a few of the activists, particularly the mysterious Verlyn Marth, who sparked the rebellion and seems to have been involved in past environmental controversies, knew what they were really up against. However, these few experienced activists were not thinking of the present any more than the utility. It was the symbolic point that this was not the way to go, and they were willing to serve in the immediate cause knowing that blocking this line was probably hopeless. Since it is doubtful that the rank and file would have gone along under these conditions, the conspiracy theory of the movement serves an important motivating function since it holds out hope prevailing in the end due to the reasonableness of their cause and willingness to use direct political action. The Power-line countermovement is no less subject to the logic of the legal fictions and tactical maneuvers it has decided to use than the officials representing the state of New York at Love Canal. It is simply more excusable and understandable when they lose touch with reality. The great value of the account is the detail and precision with which the authors lay this worldview out for us. The great

flaw is that they were so captured by it that it became their reality too.

Without Levine's counterpoised sets of beliefs and interpretations, how is the chronicler, the participant, or in this case both, to recognize the elements of his or her own mindset, much less evaluate them? Hence the real cost of the single perspective analysis is that it leaves the analyst, particularly the participant-observer analyst, in the grip of the mindset, or in this case the counter mindset, forged in the heat of conflict with its opposite and hardened by the encounter. This is even more serious than the incidental effect that one thereby deprives the field of examining the process of decision-making holistically enough to look for clues relevant to the autonomous technology debate. By sacrificing the opportunity to examine one's own beliefs against the background of the other side, sympathetically understood, one cannot grapple very well with the implication of either logic.

When this occurs the stage is set for a relatively severe case of selective perception which casts doubt on the fragments of evidence that can be gleaned post hoc to support one or the other side's interpretation. This has an interesting implication in that it suggests that such an account by an outsider such as a reporting social scientist has less potential truth value than that of a technical person with no training in the social sciences who, from the vantage point of an insider, came up with a similar interpretation. The reason this is so, is that an insider who has grappled with the prevailing definition of the situation enough to reject it personally and openly challenge it publicly is closer to having the necessary set of checks and balances to produce a credible case study analysis of this kind.

In summation, it would seem that the prospects for achieving the promise these two case studies hold out to the field of technology-society studies in terms of dealing with complexity, getting a grasp on relevant subtle phenomena, combining diverse types of data and improving the objectivity and balance in the "whistleblowing" reports that are a staple of the field are fairly good. The problems that limit the value of the Powerline case study do not seem to be inherent to the participant observation technique and could have been rectified to a great extent by the cross validation procedures and conceptual approach employed so successfully by Levine in her account of Love Canal. Producing studies of this calibre does not seem to be beyond the capabilities of concerned scientists and technologists if they take the time and put themselves through the exercises necessary to achieve conceptual balance by learning the logic of the other side. They may even have some advantages in doing so.

There is no magic in the traditional social science perspectives that Levine stressed, though it was clearly important to her in her case. The concepts she brought to the study, such as culture, worldview and groupthink, that are well known to social scientists have parallels in phenomena such as "mindsets," "mentalities" and even "established paradigms" with which many scientists and technologists are familiar. Indeed, social scientists entering this field will have to be on their guard to avoid certain aspects of their own mindset, particularly as it applies to the interpretation of technology as passive or active in the situation. Though the matter is still under debate the evidence for an active or interactive role different from the traditional view of technology as a social and political resource, though not inconsistent with it, is quite persuasive and case studies that address this issue are going to be far more valuable than those which do not. Studies involving the challenge of controlling technology that do not deal with it may be quite worthless.

Those social scientists who see the challenge and promise of technology studies and want to contribute to this field will find the literature interesting and full of familiar phenomena with unfamiliar names attached to them. While it is unwise to assume that they are precisely what has been encountered before in the study of different cultures and the literature on movements and organizations, the parallels are strong and the opportunity to extend the insights of social science into this realm is great. Those who are careful and make an effort to be complete may well make large contributions to two fields, as Levine has done with her account of the Love Canal case. Others will make far more of a contribution to their own field than that of technology studies, as Casper and Wellstone have done, but even so the results are well worth reading and have their place in this complex and challenging field of study.

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BOOK REVIEWS

Dorothy Nelkin and Michael Pollak. **The Atom Besieged: Extraparliamentary Dissent in France and West Germany.** Cambridge, MA.: MIT Press, 1981.

The resolution of technological controversies can challenge the ability of democratic institutions to negotiate the relationship between the individual and society, the citizen and the state. Civilian nuclear power has proven to be an issue that has highlighted many of the difficulties involved. Western democracies have been confronted with decisions about nuclear power which entail complex and inter-related scientific, technological, social, political, economic and moral choices. The resolution of these issues is made even more problematic by the well known but controversial association between nuclear power and nuclear weapons. Discussion often increases uncertainty, ambivalence, and polarization, not only among the public, but among the political institutions that serve them. The role of the public in the resolution of these issues is far from self-evident, and is continually being shaped by old and new mechanisms of decision making. Under these circumstances, the failure to achieve quick and permanent solutions does not necessarily reflect a lack of effort by the various participants, but rather, that individuals are confronting difficult decisions which they see as having significant impacts upon their lives.

The 1979 accident at Three Mile Island (TMI) highlights the difficult societal decisions which nuclear power engenders. A recent review of TMI stated, "Extraordinary efforts are needed to help the public think out the difficult value issues that permeate nuclear power decisions, to come to terms with risk and equity considerations, and to assure itself of the honesty and openness of the safety guardians" (Kasperson, et. al. 1982, p. 43). Since TMI there have been no new orders for nuclear plants in the United States and there have been numerous well-publicized cancellations and delays. At best, nuclear power can be characterized as being in a "holding pattern." Its status in Europe has varied.

Dorothy Nelkin and Michael Pollak attempt to account for the different status of nuclear power in France, where the antinuclear movement has had little impact, with that of West Germany, where a moratorium has been called, by referring to the different political structures of these countries. They argue that important cleavages in political institutions have significantly limited their representative role with regard to nuclear policy. By contrasting the different political traditions and institutions of France and Germany (as well as a briefer discussion of other European countries), Nelkin and Pollak account for the different levels of impact of the opposition movements. The successful impact of opposition groups is seen to depend upon political and administrative arrangements: the receptivity of the court system, opportunities for citizen participation in administrative decisions, and the openness of political organizations to dissident popular demands. When conventional channels lacked responsiveness to antinuclear demands, public concerns became channeled through new forms of organization and modes of political action. The authors see the comparatively decentralized decision-making apparatus in Germany as providing more opportunities for activists to create divisions within the political establishment and to thus exercise influence on public policy decisions, whereas the lack of these conditions in France enabled the government program to continue with little change. Public protest

was as pervasive in France as in other countries, but neither the courts, the parties, nor the local governments could channel protest to the highly-centralized policymaking level.

- In establishing the broad context for their comparative analysis, Nelkin and Pollak systematically consider the political conditions, the "nuclear establishment," and the antinuclear movement. They achieve a useful balance between a consideration of historical-structural factors such as long-standing political traditions, legal and administrative relationships, and the specific policies and issues in the current nuclear debates. After providing a careful review of the history, organization, policymaking procedures, and ideology of the nuclear establishment, they then examine the antinuclear movement. Here they consider the social and economic factors which contributed to its development, its social base, tactics and strategies, use of scientific expertise as a political resource, discourse and ideology, patterns of political conflict, and influence. In two especially informative chapters, they analyze the role of the courts - considered the "decisive factor determining the outcome of the nuclear debate" (166), and the government response.

The authors characterize their approach as "eclectic" (ix), which they justify on the basis of their broad focus, as well as their analytical depth. They utilize a wide range of research methods and materials, including interviews with officials and activists, participation in electoral meetings and discussions of antinuclear groups, and the review of official documents, antinuclear literature, previously-collected statistics and survey data. Their own indepth knowledge of the two countries also appears to have provided them with an important asset. They caution the reader, however, that in some cases their analysis is only a first step, raising more questions for further research.

Nelkin and Pollak view the resolution of the various nuclear controversies as efforts by those in power to maintain legitimacy and preserve social order. They argue that government and industry policymakers, in oligopolistic arrangements, present a unified front to minimize the potential for public influence. Long-term investments create an inertia which precludes policy flexibility. Though opportunities for public influence may often be illusory, policymakers recognize the importance of maintaining an image of democratic decision making. They are skeptical of the public's ability to exercise soundness of choice and make decisions in its own best interests, and find it difficult to even imagine external control. The need for administrative and political support in both countries becomes reflected in opportunities for public involvement and consultation, but these are seen by the authors as gestures designed to win public acceptance for the siting of nuclear reactors and other facilities. The lack of separation between the responsibilities of promoting and regulating nuclear power which, as in the United States, has been criticized as an inherent conflict of interests, is also noted to be another aggravating condition.

Less concerned with democratizing decision-making, policymakers are more interested in creating the impression that choice exists. Despite their claims of neutrality and expert service in the "public interest," they reveal assumptions and biases which Nelkin and Pollak consider an ideological justification of nuclear power. Convinced of the "rationality" of their choices, policymakers are portrayed as avoiding discussion of the logical implications

of the values reflected in nuclear programs. The French, for example, promoted nuclear power as a means of achieving greater national independence - a stated value - yet eventually utilized American technology, seemingly contradicting this goal (21). This policy change, the authors suggest, favored economic internalization more than independence. Thus, they conclude that the concept of "independence" had more of an ideological than analytic content. I would only emphasize that an ideology based on such values as national independence, progress, and an image of the future related to societal growth, is a testimony not only to the policymakers' convictions and the depths to which the current controversies have stirred them, but also to the strategic importance of energy in all societies, which makes it amenable to association with such grandiose themes.

The antinuclear movement also has an ideological component which is considered the key to its broad appeal. It is related to broad economic, political and cultural concerns, as is the ideology of the nuclear establishment. The movement has a broad base of social support from diverse constituencies with different political agendas. The authors state that they do not wish to "establish the validity nor to point up to the fallacies of this ideology (140)," and in a footnote argue that, "By analyzing the antinuclear ideology, we start from the fundamental contradiction established by linguistic analysis that discourse is not the same as reality and that political discourse is intended to persuade and does not necessarily seek analytical clarity" (221). Yet for both the nuclear establishment and the antinuclear movement, it would be useful as a next step to examine the relationship between their ideology and more "nuclear specific" concerns. How does their assessment of the economic need for nuclear power, for example, relate to their assessment of the risk to human health which nuclear power poses?

To what extent does ideological conflict account for all of the "real" points of contention? Nelkin and Pollak see ideological differences as the primary reason for conflicting assessments of nuclear power. I would suggest, however, that the role and consequences of these ideological differences remain to be specified further. Economic incentives and vested interests may indeed account for the policy preferences of the nuclear establishment. On the other hand ideology may be considered more of an after-the-fact value-preference-justification (cf. Barber, 1971). In addition, ideology may not sufficiently explain the nuclear preferences of some segments of the antinuclear constituency. The role of ideology as a means of temporarily mobilizing sympathizers and as a tactical device is important to understand, but it should not preclude efforts to delineate its specific role in particular disputes, its inhibiting impact on achieving societal consensus, and its linkage and support for other more specific nuclear-related concerns on both sides. If nuclear power is opposed because it entails economic and industrial concentration, why aren't other industries which embody these values opposed with equal fervor by the same groups? This is not to suggest that ideology does not play an important role, but that perhaps, the characteristics of nuclear power specifically have not received sufficient emphasis by Nelkin and Pollak.

In order to examine the relationship between ideology and other actual points of contention, it is important to consider the relationship between the "antinuclear movement" and the "general public," an admittedly vague and amorphous entity, but one whose opinions policymakers frequently assess in the course of rendering decisions. This distinction - between the movement and the

general public - may serve a useful heuristic purpose. It is somewhat obscured, however, because of the author's broad definition of the antinuclear movement: "a form of intermittent social organization that oscillates between a small core of committed activists and a mass organization temporarily mobilized for specific events" (130). In this connection, Mazur has suggested that the demographic and motivational characteristics of active participants in a controversy and passive members of a wider public, who may express their views in a public opinion poll, or attend a demonstration, should be differentiated (Mazur 1981, p. 43). The activists' opinions are likely to be embedded in an ideological framework of beliefs which is clearly distinguishable from, and more politically extreme, than those of the general public. The degree to which the broader public, as distinguished from the small core of permanent activists, share and/or are motivated by these ideological concerns must be empirically established, and not implicitly inferred from an analysis of movement literature, or from the sympathy which the public has with the movement's goals. The broader social and ideological issues may be weighed and negotiated differently by the general public. The conflict between industrial centralization and its impingement on democratic values, for example, may be "resolved" by the public one way, and by active opponents, in another.

Such a distinction has been used in research which has focused on government and nuclear industry "decision makers" in the United States and their perception and evaluation of "public opinion" (Cohen, 1982). In this case study it was reported that decision makers were very cognizant of what they perceived to be important differences between the "antinuclear movement" and the "general public." The movement was considered an important influence on, though clearly distinguishable from, the general public. Compared to members of the public, antinuclear activists were seen as younger, better educated, more liberal, and expressing more extreme opinions with greater intensity. Activists were perceived to be more ideologically motivated, less open-minded, and less willing to "balance" different and often competing considerations. Decision makers perceived activists' opposition to nuclear power more as a means to create a different type of society. Nuclear power was seen as a "surrogate" issue for activists, whereas for the public it was seen as an end in itself. Furthermore, antinuclear leaders were seen as lacking accountability to the general public, which had no formal mechanisms of assuring that the movement "represented" or "spoke" for it. Consequently, nuclear industry decision makers challenged the movements' claims to legitimate influence, and attempted to restrict its role in various policymaking forums, such as the adjudicatory proceedings of the United States Nuclear Regulatory Commission.

These findings suggest that Nelkin and Pollak's analysis could be informed by further inquiry into the relation between the antinuclear movement and their broader publics in Western Europe. Activists' confrontation and negotiation with the policymakers may create the (correct) impression that their debate contains an "ideological stonewall." When more typical members of the "public" are involved, or considered in the analysis, however, we should be encouraged to specify the role of ideology. This line of inquiry is even suggested in opinion poll statistics cited by the authors: in both countries there are similar divisions of opinion on nuclear issues among the general public (about 40% are opposed). Seen in this context, it is more difficult to conclude that the political machinery is not "representative." It may not be "responsive"

to the antinuclear movement, but to conclude that policymaking institutions have lost their "representative" character, requires some conception of the relationship of the movement to some broader public. Is the movement "representative" of some broader constituency (relevant public), and if so, in what particular ways? The controversy may be "ideological," but for whom?

Of the thousands who attended antinuclear rallies, how many are concerned primarily with ideological issues such as economic centralization or the spector of a police-state which nuclear power implies to the activists, and how many are principally concerned with the specific health, safety, and environmental consequences of nuclear power - for which activists' ideology may be a sufficient "magnet," though not "mirror"? Nelkin and Pollak write:

While the authorities, especially in France, focused almost exclusively on questions of risk, antinuclear activists saw these risks as a surrogate for larger social, political, and economic concerns. Indeed, even if technical consensus could have been established, this would have had little effect on antinuclear attitudes (181).

While a technical consensus may have little effect on the activists' attitudes, we cannot be certain that antinuclear sympathizers would have responded similarly. Though the ideology may faithfully reflect the essence of activists' concerns, it may have less correspondence to those of a sympathetic public, to whom policymakers presumably address themselves as well.

Much of the recent discussion has recognized that nuclear power is not only a scientific or technological controversy, but also one over social values (cf. Del Sesto 1979). Perhaps, to this degree, the antinuclear activists have performed a useful service to society by clarifying the value of referents of the debates. "Extraparliamentary dissent" may be a process of social discussion which is more effective in identifying and explicating the risks and benefits of technology, than the formal means of societal negotiation are (Mazur 1981, p. 127). Since nuclear decisions entail other value considerations, we would expect that various individuals would resolve these value conflicts differently.

Nelkin and Pollak have made an important contribution to our understanding of the nuclear energy controversies. By collecting and presenting a vast amount of information, and interpreting it in relation to theoretical issues in the sociology of science as well as political sociology, they have laid the groundwork and provided other students with a useful point of departure for further empirical and theoretical inquiry. Nevertheless the book is not without its limitations. Their eclectic approach is perhaps more useful on substantive than on theoretical grounds. It would have been useful if they provided a more detailed and systematic discussion of their interviewees, research materials, and secondary sources. Though they successfully illustrate the general theoretical perspective of German sociologist O. Rammstedt, who sees a social movement as a social process which develops in a situational context, i.e., its organization, ideology, and tactics adapt to the environmental and political circumstances in which it evolves, they do not systematically test or develop his model. The same is true of their occasional references to the literature in social movements and political sociology (i.e., Gerlach and Hines; Habermas). To this degree, their work acquires the character of an ad-hoc application of theoretical relevances, rather than a tightly-knit empirical analysis generated by a cohesive or consis-

tent theoretical framework. One also senses that Nelkin and Pollak are personally more sympathetic to the antinuclear position, and perhaps more desirous of influence for active opponents, than, say, passive supporters. But these objections notwithstanding, their work remains a major accomplishment and welcome addition to the field.

References

- Barber, Bernard. 1971. "Function, Variability, and Change in Ideological Systems." Pp. 244-262 in Bernard Barber and Alex Inkeles (eds.), *Stability and Social Change*. Boston: Little Brown.
- Cohen, Robert Leslie. 1982. "The Perception and Evaluation of Public Opinion by Decision Makers: Civilian Nuclear Power in the United States." Unpublished Doctoral Dissertation, Department of Sociology, Columbia University.
- Del Sesto, Steven L. 1979. *Science, Politics, and Controversy: Civilian Nuclear Power in the United States, 1946-1974*. Boulder, Colorado: Westview Press.
- Kasperson, Roger, C. Hohenemser, J. X. Kasperson, and R. W. Kates. 1982. "Institutional Responses to Different Perceptions of Risk." In David L. Sills, C. P. Wolf and Vivien B. Shelanski (eds.), *Accident at Three Mile Island: The Human Dimensions*. Boulder, Colorado: Westview Press.
- Mazur, Allan. 1981. *The Dynamics of Technical Controversy*. Washington, D. C.: Communications Press.

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Patrick Grim (ed.). *Philosophy of Science and the Occult*.
Albany, NY.: State University of New York Press, 1982.

According to the General Introduction, this collection of short essays integrates two aims: first, to serve as an introduction to philosophy of science through an examination of the occult; second, to examine the occult rigorously and seriously. By itself, the book can hardly achieve either of its stated aims; but as a reading list it can certainly serve both aims through stimulating interest.

I commend the notion of introducing students to philosophy of science by capitalizing on the wide-spread interest in such subjects as astrology, parapsychology, UFOs, and ancient astronauts. That approach has met with some success in the teaching of biology (1), of physics (2), in science education generally (3) and in history (4); no doubt there are many more examples of which I am not aware (5). But I found myself wanting to know whether the editor of this book had actually tried out on students this collection of essays. I suspect not, for the Introduction fails to say that it has been useful in practice and rather argues, at some considerable length, how and why the collection accomplishes its goals. I found this explication tiresome, irritatingly self-congratulatory, and not particularly convincing. I suggest it be left unread, and that one go direct to the essays themselves.

The first of five sections in the book deals with astrology, through five essays or extracts. "Objections to Astrology" is the lead: The public statement of 1975 by "186 leading scientists" cautioning the public against giving credence to horoscopes and the like. Some of the deficiencies of this statement are pointed to in later pieces, but there is hardly a full discussion of it. That illustrates, I think, the strengths and weaknesses of the whole book. A host of major and minor issues is raised, explicitly in the introductory sections as well as more implicitly in the essays; that is good. But there is no full-fledged, coherent addressing of those questions. In my view, that is good in a book intended to stimulate students and to serve as subsidiary reading in a course of lectures or a series of seminars; but it does contradict in some measure the claims made for the book in the General Introduction. At any rate, "Objections to Astrology" could be criticized not only because it is a dogmatic argument from authority, but also because of its ineptness in pretending to authority. The qualifications of any of the signers to discuss astrology is not evident, a point mentioned but not well developed in Feyerabend's essay (much better is Westrum's discussion) (6). The division of "leading scientists" into Nobelists and others would lend itself readily to satire: if the Nobelists count for more, why include the others at all? Or were the others necessary because the Nobelists included no astronomers? Why exactly should we listen, about astrology, to Nobelists in economics or chemistry? And how about some more numbers than just that "186": how many Nobelists were asked to sign but refused? How many non-Nobelists?

Feyerabend, in the book's second essay, aptly criticizes the religious nature of "Objections." From my viewpoint unfortunately, he then displays a total lack of discrimination in what he is prepared to call "scientific studies," referring the reader to Lyall Watson's *Supernature*, for example. Feyerabend also reports that "variations in the rate of standardized chemical reactions"

can be ascribed to "changes of the structure of water used in the experiments"; he does not tell the reader how primitive were those experiments on reaction rates, nor that more careful measurements have not revealed such effects. But the reader is told that "the chemical bond in water is about one tenth of the strength of average chemical bonds"--a repetition, from Watson, of an apparent confusion between inter- and intra-molecular bonding. Feyerabend also refers to S. W. Tromp as W. W. Tromp, miscites a 6th International Congress as the 5th, and frequently (but not entirely consistently) writes Velikowski for Velikovsky. Hardly a good example of taking care in making one's case.

Perhaps the best piece in the whole book is Edward James', "On Dismissing Astrology and Other Irrationalities." James shows that no quick dismissal of astrology as irrational is possible, since no single criterion is available: Popper's falsifiability does not work, nor does Kuhn's analysis of the methods of science; nor does a quick examination of the content of astrologers' claims. James makes two points that ought to be kept in the forefront by those who examine "fringe" subjects (as I prefer to describe what the book calls "occult") and by those who wish to discredit "pseudo-science" (as they characterize fringe subjects). First, "To show that astrology is irrational. . . is to tell a long story"; James illustrates the analytical dissection that can demolish astrological writings in detail, but warns that one must read the astrological literature and stand ready to do battle in an indefinitely long series of analyses. Second, that individuals can find excellent reasons to reject astrology, to be disbelievers, is not in itself a warrant "to say that no one today should adhere to astrology"; that distinction is almost never made in the literature about anomalous phenomena and claims, yet it is crucially necessary if there is to be any sense to the arguments, let alone any hope of resolving them. Indeed, the authors of several of the other essays in this book could profit from an understanding of the cogency of James' critique.

The case for something like astrology is made by Michael Gauquelin, reporting his statistical correlations of planetary positions with the births of subsequently successful people. The section on astrology then concludes with a review, by I. W. Kelly, of three approaches to astrology: the traditional, the "cosmobiology" of Gauquelin, and the "humanistic." There is also a short list for suggested further reading.

This set of essays does not amount to a rigorous discussion of the claims and status of astrology. The important questions are raised; indeed, explicitly posed in remarks introducing the section: "Is astrology a prime example of pseudoscience, and if so why? Must we distinguish traditional astrology from "Humanistic astrology" and "cosmobiology" in order to answer such a question? . . . What is the relation between correlation. . . and claims regarding causal relations between those things that are correlated?" But the essays do not explicitly grapple with these questions. So we have a collection of pieces that are interesting and provocative to varying degrees, and which could serve as an introduction to reading about astrology or to reading about philosophy of science.

My reaction is the same to the other sections of the book: Science or Pseudoscience?; Parapsychology; UFOs and Ancient Astronauts; Other Approaches to the Occult. There are sufficient good essays and sufficient different topics and viewpoints that most readers will find something useful and worthwhile for them; and this reading list might indeed be a good one to use in an

introduction to philosophy of science. But I do think the book should be seen as that, as an interesting reading list, and not as the definitive and integrated exposition and discussion that the General Introduction seems to promise.

Other than that Introduction, the book's greatest shortcoming is its failure to provide clues to the affiliations of the authors. I should have liked to have been able to satisfy my curiosity, whether a particular author is a philosopher or a sociologist, for example. And since I particularly enjoyed the piece by Edward James, I was curious to know what else he has written; the book gives me no help toward discovering that, no address for James, no professional identification. An odd oversight in a collection, it seems to me.

But these are, after all, minor shortcomings compared to the enjoyment and stimulation offered by the book. The pieces are all commendably short: if one does not appeal, turning of a few pages brings a different viewpoint. The choice of main themes is surely a sound one, and the individual pieces within the sections are also, on the whole, appropriate choices. I hope the book gains a wide audience; I shall also be interested to hear how well it has served its intended and commendable pedagogic purpose.

Notes

1. Richard W. Greene, a course entitled "Biological Problem Solving" in the Unified Science Program for non-science majors at the University of Notre Dame; personal communication, 1979.
2. Thornton Page, "Education and the UFO Phenomenon," Chapter 1 (pages 3-10) in Carl Sagan and Thornton Page (editors), *UFO's--A Scientific Debate* (Ithaca and London: Cornell University Press, 1972).
3. Michael Martin, "The Use of Pseudo-Science in Science Education," *Science Education*, 55 (No. 1), 53-56, 1971.
4. H. E. LeGrand and Wayne E. Boese, "Chariots of the Gods? and All that: Pseudo-History in the Classroom," *The History Teacher*, VIII (No. 3), 359-370, May 1975.
5. I would appreciate being told about other instances.
6. Ron Westrum, "Scientists as Experts: Observations on 'Objections to Astrology'," *Zetetic Scholar*, 1 (No. 1), 34-46, Fall/Winter 1976; Paul Kurtz and Lee Nisbet, "Are Astronomers and Astrophysicists Qualified to Criticize Astrology?" *Zetetic Scholar*, 1 (No. 1), 47-52, Fall/Winter 1976; Ron Westrum, "More on Astrology," *Zetetic Scholar*, 1 (No. 2), 107-112, Spring/Summer 1977; reply by Kurtz and Nisbet, pages 112-117.

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Aaron Wildavsky and Ellen Tenenbaum. The Politics of Mistrust: Estimating American Oil and Gas Resources. Beverly Hills: SAGE, 1981.

Wildavsky and Tanenbaum (henceforth W&T) have compiled an interesting and, at times, insightful analysis of the relation between estimates of oil and gas resources and the formulation of American energy policy. The resultant book has much to recommend it. The Politics of Mistrust is the first major social scientific analysis of an important subject: the ramifications of the inherent uncertainty that characterizes natural resource estimation. Theoretically, the book provides an important contribution to the analysis of policy controversies by transferring insights from the social study of science to a new context. In particular, the book draws heavily upon Kuhn (1962) and others who have demonstrated the linkage between theory and observation in science to argue that an individual's interpretation of the accuracy of a resource estimate is largely determined by his ideological beliefs.

In essence, W&T argue that, due to the inherent uncertainty of the estimation process, the interpretation of specific resource estimates by particular interested parties as either "high" or "low," "correct" or "incorrect" does not (and cannot) reflect an objective assessment. Instead, such interpretations are a function of the interested parties' pre-given ideological commitments and the current price of oil and gas on the market. This leads W&T to provide a structural model that predicts how various parties will interpret and use resource estimates under specified economic conditions.

Although the model harmonizes with the "history of mistrust" they present, the history W&T provide is both selective and poorly documented. More significantly, their model leads to an outright contradiction when asked to account for certain closely related and less subjectively presentable phenomenon. For example, W&T claim that when prices are high, the oil and gas companies favor high estimates because "significant new supplies can be found if prices are high" (Table 12.1, p. 300). If this is the case, how do W&T account for the fact that in 1974, immediately following the massive price increases associated with the Arab oil embargo, the industry was both actively arguing and producing estimates which suggested that the current government estimates were several hundred billion barrels too large (Gillette 1974, West 1974).

The answer to this question lies in an understanding of the particular range of phenomenon W&T have examined: they addressed the question of how people interpret and utilize resource estimates and avoided the related question of how such estimates are produced. As a result, W&T should be chastized for much the same reason that they should be praised. While bringing epistemological insights from the social study of science to policy analysis, W&T have failed to fully realize the implications of these insights. As Mulkay (1979) has shown, these insights imply an ability to subject the content of scientific knowledge (as opposed to its method of production) to social scientific analysis. Having not done this, it is my opinion that their structural model is seriously oversimplified.

The policy implications of a model that encompasses both the production and interpretation of natural resource estimates are markedly different from those contained in the Politics of Mistrust. For example, W&T argue that the con-

flict over U.S. energy policy requires a political agreement upon a basic policy framework. While the necessity of a political settlement to such fundamental policy problems can hardly be denied, W&T argue that the primary characteristic of such a policy framework should be a radical downgrading of the importance placed upon resource estimates as a criteria for evaluating policy alternatives. This view is predicated upon the belief that the uncertainty in oil and gas estimation results from "poor" science, i.e. the techniques for making such estimates are not well developed. Analysis of the social factors influencing the production of natural resource estimates, however, shows that a majority of the uncertainty in natural resource estimates comes from non-scientific causes (Bowden, 1982). Such research opens up the possibility of an interpretational consensus among policy makers predicated upon an understanding of how such non-scientific factors generate uncertainty about the size of remaining resources.

One other flaw deserves special mention. On a technical level, the book conflates "reserves" and "resources." Although the authors clearly understand the difference between the concepts, the method by which their arguments are deployed often forces the reader to transfer conclusions justified on one level to the other. Since many geologists (e.g. McKelvey 1974, Sheldon, 1975) view such classificatory confusion as the primary source of uncertainty in resource estimates, this conflation of concepts seriously weakens the analysis.

In short, while many of the specific policy conclusions of W&T are laudable (e.g., their call for a tie between policy goals and data collection and their critique of recent government policy vis a vis reserve estimation), their overall suggestion for the resolution of energy policy conflicts is fundamentally flawed. Estimates of remaining oil and gas resources are too important in the assessment of certain policy alternatives to be totally exorcized from the decision making process. At some point the economically recoverable petroleum resource of an area must become exhausted. Forecasting when this will occur provides information that is essential to policy decisions involving shifts to alternative energy sources or shifts in the geographical area of search. Thus, an effort should be made (a) to understand the various sources of uncertainty, both scientific and non-scientific, that affect the process of resource estimation and (b) to restrict the utilization of resource estimates as an evaluative criteria to those decisions for which such information has utility. This alternative to W&T's recommendation both minimizes the inherent uncertainty of such estimates and allows the best possible information to be utilized in those situations where that information acts as a legitimate criteria for assessing policy alternatives.

References

- Bowden, Gary. 1982. "Organizational Interests, Political Economy and Historical Change in the Estimation of U. S. Crude Oil Resources." *Pacific Sociological Review* 25(4):419-448.
- Gillette, Robert. 1974. "Oil and Gas Resources: Did the USGS Gush Too High?" *Science* 185(29):127-130.
- Kuhn, Thomas. 1962. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

- McKelvey, Vincent. 1974. "Concepts of Reserve and Resources." Methods of Estimating the Volume of Undiscovered Oil and Gas Resources, American Association of Petroleum Geologists Research Symposium, Stanford University.
- Mulkey, Michael. 1979. *Science and the Sociology of Knowledge*. London: Allen and Unwin.
- Sheldon, Richard. 1975. "Estimates of Undiscovered Petroleum Resources--A Perspective." Pp. 11-22 in *1975 Annual Report of the United States Geological Survey*.
- West, Jim. 1974. "U. S. Oil Policy Riddle: How Much Left to Find?" *Oil and Gas Journal* 72(37):25-28.

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Social Interests and Professional Strategies in American Social Science,
1918-1940

Richard Gillespie (University of Pennsylvania) The Psychopathology of
Industrial Life: Elton Mayo and the Prehistory of the Hawthorne Experiments

James Capshew (University of Pennsylvania) Muzak for the Masses: Psychology
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Psychosurgery to Return the Nation's Mentally Ill to Active Employment, 1942-1955

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Participants:

Charles Bazerman (CUNY) **How Physicists Read**

Greg Myers (University of Texas at Austin) **How Biologists Write**

Katherine McCain (Drexel University) **How Macroeconomists Cite**

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Organizer: Ryan D. Tweney, Bowling Green State University

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ISSUES IN THE COMPARATIVE ANALYSIS OF SCIENCE-RELATED CONTROVERSIES: Panel

Organizer: James C. Petersen, Western Michigan University

Participants:

Diana Dutton (Stanford University)

Jerry Markle (Western Michigan University)

Allan Mazur (Syracuse University)

Arie Rip (University of Leiden)

NUCLEAR WEAPONS AND PUBLIC POLICY

Organizer: Linda Lubrano, American University

Participants:

Jerry Markle (Western Michigan University) **Minutes to Midnight: Scientists' Resistance to the Bomb in the 1940's**

Michael Altimore (Mount Mercy College) **Policy Perspectives in Nuclear Weapon Decision-Making**

Peter Stein (Newman Laboratory, Cornell) **Current Opposition to U. S. Nuclear Policy: The Views of Scientists**

Discussants:

Judy Reppy (Cornell University)

Spencer Weart (American Institute of Physics)

**ARE KUHNIAN THEORIES OF SCIENTIFIC CHANGE RELEVANT TO
TECHNOLOGY ?**

Organizer: Rachel Laudan, Virginia Polytechnic Institute and State University

Edward Constant (Carnegie-Mellon University) **Kuhnian Communities and
External Social Structure: The Dissemination of Petroleum Engineering
Knowledge**

Rachel Laudan (VPISU) **Cognitive Change and Social Structure in Science
and Technology**

Peter Weingart (University of Bielefeld) **The Structure of Technological
Change: Reflections on a Sociological Analysis of Technology**

SELECTION AND DRIFT IN THE DEVELOPMENT OF IDEAS

Organizer: William Snizek, Virginia Polytechnic Institute and State University

Participant:

David Hull (University of Wisconsin-Milwaukee) **Editors as Gatekeepers--
31 years of Systematic Zoology**

Discussant:

William B. Lacey (University of Kentucky)

**INSTITUTIONAL INFLUENCES ON SCIENTIFIC RESEARCH:
KEPT SCIENCE?**

Organizer: Daryl Chubin, Georgia Institute of Technology

Participants:

Tony Smith (Iowa State University) **How University Research Supports
Vested Interests**

Gary Bowden (University of Calgary) **The Politics of Oil Estimation**

Nicholas Tilley (University of Minnesota-Duluth) **Responsibility, Agency,
and Acid Rain**

FUNDING AND KNOWLEDGE GROWTH

Organizer: Susan E. Cozzens

Participants:

Steven Cohn, **Algebraic and Geometric Topology**

Thomas Gieryn and Joe Tatarewicz, **Planetary Astronomy**

C. Steward Gillmor, **Ionospheric Physics**

Karl Hubauer, **Sudden Infant Death Syndrome Research**

Henry Small and Edwin Greenlee, **Collagen Research**

CAN PHILOSOPHY OF SCIENCE BE NATURALISTIC?

Organizer: Thomas Nickles, University of Pittsburgh

Ron Giere (Indiana University) **Toward a Unified Theory of Science**

Andrew Lugg (University of Ottawa) **Marginality in Science**

Sal Restivo (Rensselaer Polytechnic Institute) **The End of Epistemology?**

TAKING SCIENTIFIC PRACTICE SERIOUSLY

Organizer: Karin Knorr-Cetina, Wesleyan University

Sharon Traweek (Massachusetts Institute of Technology) **Laboratory Life in High Energy Physics**

Karin Knorr-Cetina (Wesleyan University) **Studying the Laboratory**

THE NATURE OF SCIENTIFIC KNOWLEDGE

Organizer: Sal Restivo, Rensselaer Polytechnic Institute

Participants:

Nehemiah Jordan (George Mason University) **Plato and Epistemology**

Harry Collins (University of Bath) **Scientific Knowledge and Science Policy-- Some Foreseeable Consequences**

IS THERE A SOCIOLOGY OF TECHNOLOGY?

Organizer: Ron Westrum, Eastern Michigan University

Ron Westrum (EMU) **The Old Sociology of Technology**

Wiebe Bijker (Technische Hogeschool Twente) **Yes There Is! A Unified Approach to the Sociology of Science and Technology: The Sociology of Artifacts**

Trevor Pinch (Technische Hogeschool Twente) **Yes There Is! A Unified Approach to the Sociology of Science and Technology: The Sociology of Facts**

Joop Schopman (University of Utrecht) **Semiconductor Development in the Netherlands Up to 1955: A Study of Its Determinants**

SCIENCE AND INSTITUTIONAL SUPPORTS

Organizer: Tom Gieryn, Indiana University

Participants:

Stephen Turner (University of S. Florida-St. Petersburg) **Patronage and Trust in Science**

Thomas F. Gieryn (Indiana University) **Does Sociology Belong in the National Science Foundation? Historial Inquiries, 1945-1960**

Donald Fisher (University of British Columbia) **The Emergence of Applied Social Science: Rockefeller Philanthropy and the Social Science Research Council**

CAREER CONTINGENCIES AND STRATEGIES

Organizer: John Ziman, Imperial College of Science and Technology

Susan Cozzens (National Science Foundation) **The Internal Structure of Problem Sets: A Closer Look**

Ian Lubek (University of Guelph) **Augustin Hamon and "Publish or Perish"**

John Ziman (ICST) **Problem Choice as a Career Contingency**

John Law (University of Keele) **On the Tactics of Social Control: Theory of the Actor-Network**

FUTURE MEETINGS COMMITTEE REPORT

9TH ANNUAL MEETING

RECOMMENDATION TO BE MADE TO COUNCIL

Get Ready for Ghent!

The Future Meetings Committee of the 4S, consisting of Nicholas Mullins, Chair; Arie Rip, Daryl Chubin, and Roland Clevinger, will recommend to the Council that the 1984 meeting of the Society occur in Ghent, Belgium. An invitation from Marc de Mey and Communication and Logic, an interdisciplinary group based in the Department of Logic and Epistemology of the University of Ghent, to organize the annual meeting of the 4S for November 15-18, 1984, has been accepted by the Committee, subject to approval by the full Council at the 1983 meeting. The meeting will be joint with the European Association for the Study of Science and Technology (EASST). Preceding the 4S conference will be the ERISS II meeting, a sequel to the Casenovia conference of June, 1981.

The meeting will be held in the Cultural Center of the University, a restored monastery. Meeting rooms of different sizes are available and there is coffee shop service during the day. Three or four parallel sessions will be held. More details on this meeting will follow in later newsletters.

1985 and Beyond

Contact with the PSA, SHOT, and History of Science Society on the 1986 meetings has been made, and sites for the four society joint meetings are being discussed. Currently, Washington is the primary site being considered. The 1985 meeting site, a meeting of the 4S by itself, is still being considered. We have one preliminary invitation, but no action has yet been taken. Any group of 4S members who would like to propose a site for 1985 should send a letter of inquiry to: Nicholas C. Mullins, Department of Sociology, BH744, Indiana University, Bloomington, IN 47405 USA.

Cooperative Meetings

Daryl Chubin, the 4S representative to AAAS, Section S, has proposed that we use the sessions we can put together as members of Section S at the New York meeting of AAAS, in May, 1984. He will be reporting later on programs and sessions. Anyone with a proposal for a session for that meeting should contact Daryl.

Submitted by
Nicholas Mullins, Chair

LOCAL ARRANGEMENTS COMMITTEE
8TH ANNUAL MEETING
--ANNOUNCEMENT--

The Local Arrangements Committee promises attractive accommodations and beautiful countryside to accompany the outstanding program offered for the 8th Annual Meeting in Blacksburg.

Blacksburg is located on a plateau between the Blue Ridge Mountains and the Alleghenies in southwestern Virginia. It is at 2,100 feet elevation, so November evenings can be chilly.

The community is served by Roanoke Airport, and there are both direct and connecting flights daily to and from other major cities, including Washington, Atlanta, New York and Chicago. The airport is approximately 35 miles from Blacksburg.

Regular limousine service is available to and from the Blacksburg Marriott Hotel for most flights. Blacksburg is easily accessible by ground travel from the airport via the Interstate system (I-81). Avis, Hertz, Budget and National car rentals are available at the airport. Cab service is available, but expensive.

The Local Arrangements Committee will survey all pre-registrants in late summer to assure that ample limousine service is available for arrivals on Thursday afternoon, November 3, and Friday morning, November 4, and for departure to return to the airport immediately after the conclusion of the meeting on Sunday, November 6. Those who are not pre-registrants by the end of August, and those whose plans are for later arrival or earlier departure, will be asked to make their own ground transportation arrangements.

PRE-REGISTRATION INFORMATION

and

HOTEL RESERVATION FORMS ON FOLLOWING PAGES

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4S ANNUAL MEETING

NOVEMBER 4 - 6, 1983

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 INDIANA UNIVERSITY
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THE SOCIETY FOR SOCIAL STUDIES OF SCIENCE

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NOMINATING COMMITTEE REPORT

CANDIDATE PROFILES

The following candidate profiles were solicited from candidates for the offices to be decided by election this summer. They are listed alphabetically by office. All listed have agreed to stand for election, but some profiles were not available as this 4S REVIEW went to press.

President**Jerry Gaston**

BIOGRAPHY: Jerry Gaston received the Ph.D. in 1969 from Yale University where he specialized in the sociology of science. From 1969-1981 he was in the Department of Sociology at Southern Illinois University; in 1981; he moved to Texas A&M University. He is the author of several articles and two books, and has edited and co-edited two books. His current research interests are in ascriptive/achievement status in science and the status of U.S. science policy.

GOALS FOR 4S: "I believe that 4S has come a long way, and that its general direction is appropriate. We have some difficulties ahead with our efforts to produce a scholarly journal, and the precise directions those efforts will take are still not resolved to everyone's satisfaction. That will take negotiation, persuasion, and hard work on everyone's part. In the near future, we should try to devote time and energy on charting out the important intellectual questions that deserve special attention."

Nicholas C. Mullins

BIOGRAPHY: "I was trained at Deep Springs, Cornell, and Harvard Universities, receiving the Ph.D. (Social Relations) in 1967. I have taught at Vanderbilt, Dartmouth, and since 1971, at Indiana University, Bloomington, where I have risen to the rank of Professor of Sociology. I have written several books, including Theory and Theory Groups in Contemporary American Sociology, Science: Some Sociological Perspectives, and Social Communications among Biological Sciences and a number of research articles. I am currently writing a book on scientific specialties and conducting new research on large laboratory organization. I have been active as a student of, and advisor to, federal science agencies.

I have been active in 4S activities since the original Organizing Committee and the first Council of the 4S. Most recently I have chaired the Future Meetings Committee and I am a member of the Publications Committee."

GOALS FOR 4S: (1) To develop a strong intellectual dialog through meetings and publications which will assure our position as an international and interdisciplinary organization. I support cooperative efforts with other societies to develop joint meetings, and the strongest support for the 4S REVIEW.

(2) To engage the policy and knowledge components of our field in mutual education. I support recruiting the participation of decision makers as

well as students of science in all our activities.

(3) To support through publicity and consultation all education efforts in the social studies of science. I support curriculum exchanges, workshop development and publicity, and program consultation through or partly sponsored by the 4S.

None of these activities represent new departures, but rather the further implementation of earlier initiatives. They do constitute a coherent and practical program for a scientific society. The President of the Society must provide leadership. The willingness of the volunteer officers, committee members and other participants to keep the Society moving is its real strength.

Secretary
(One Nominee)

J. Scott Long

BIOGRAPHY: J. Scott Long is an Associate Professor of Sociology and Statistics at Washington State University. In 1977 he received his Ph.D. in Sociology from Cornell University, where he remained for an additional year as a post-doctoral fellow in the Program for the Social Analyses of Science Systems. His research in the sociology of science has focused on processes of stratification in science and issues related to the measurement of scientific productivity. His publications in this area include: "Productivity and position in the academic career" (1979), "Entrance into the academic career" (1980 with P. D. Allison and R. McGinnis), "Organizational context and scientific productivity" (1981, with R. McGinnis), and "Cumulative advantage and inequality" (1982, with P. D. Allison and T. D. Krauze). These papers were published in the American Sociological Review.

GOALS FOR 4S: As secretary, my primary goal would be to actively contribute to the continued growth of 4S as an interdisciplinary and international society. To this end, I would work to: (1) expand membership by publicizing the activities of 4S; (2) improve communications with related professional societies; (3) extend 4S's past success in providing an interdisciplinary forum for the study of science; and (4) insure the smooth operation of business associated with 4S.

Treasurer
(One Nominee)

Lowell Hargens
[Incumbent]

Council

Henry H. Bauer

BIOGRAPHY: My education was in chemistry (Ph.D. 1956, University of Sydney; postdoctoral 1956-58, University of Michigan). I taught at Sydney (1958-65), Michigan (1965-66), and the University of Kentucky (1966-78); my research was largely in electrochemistry. During the 1970s, I became fascinated by the discrepancy between the actualities of scientific practice and the views of that practice held by scientists, the public, humanists, and social scientists, and I began to study the implications of that discrepancy as revealed in controversies about subjects on the fringes of science. My analysis of the Velikovsky controversy is now being brought out by the University of Illinois Press (scheduled for publication in fall 1984); a book on the controversy about the Loch Ness monsters will be finished this summer. My desire to work in Science Studies rather than Chemistry led to a decision to seek an academic position not tied to chemistry; since 1978, I have been Dean of the College of Arts and Sciences at Virginia Tech (Virginia Polytechnic Institute and State University, formerly VPI). Here I have supported the establishment and development of the Center for the Study of Science in Society.

GOALS FOR 4S: The time is right to develop an accurate description of scientific activity and its role in contemporary culture. The insights so far offered by historians, philosophers, scientists, sociologists, and others are partial descriptions of limited validity; the "whole elephant" is still to be characterized, and I see 4S as a means for working toward that (and I see the existence of 4S as evidence that others share my view). The most immediate task is for practitioners of different disciplines to learn to communicate with one another. The meetings and publications of 4S should be designed to arouse the interest and participation of people whose training and disciplinary biases are quite various. To make that slightly more than a platitude: sessions of 4S meetings should focus on specific questions with invited speakers from the range of disciplines (by contrast to individual sessions for philosophers, for historians, and so forth); cross-disciplinary critiquing should be supported--let the philosophers, say, explicate their dissatisfactions with the various sociological approaches, and vice versa; I should like to see active participation in 4S by science writers--at least, and perhaps initially, as invited speakers. In brief, I should like to see 4S as a meeting ground for students and explicators of scientific activity who want to reassess and modify the dogmas by which we live within our traditional disciplines.

Richard M. Burian

BIOGRAPHY: Head and Professor, Department of Philosophy and Adjunct Professor, Center for the Study of Science in Society, Virginia Polytechnic Institute and State University. Ph.D., Philosophy, University of Pittsburgh, 1971. Teaching posts: Brandeis University 1967-76, Drexel University 1977-1983; visiting positions at Florida A&M, University of Pittsburgh, University of California, Davis; research fellow (1976-7) at the Museum of Comparative Zoology; Harvard, while on an ACLS Study Fellowship. Organizer, 1983 Summer Conference on History and Philosophy of Biology; Assistant Director, 1982 Summer Institute on Philosophy of Biology; Organizer, 1982 4S Meeting Symposium, "Can

Sociologists and Philosophers Say Anything Useful About Scientific Discovery?" Current research interests include interrelations among history, sociology, and philosophy of science (esp. biology), role of national and disciplinary traditions in the history of genetics, the interplay between social and cognitive structure of scientific disciplines. Articles include: "More than a Marriage of Convenience: On the Inextricability of History and Philosophy of Science," "Why Philosophers Should not Despair of Understanding Scientific Discovery," "Human Sociobiology and Genetic Determinism," "The Social Structure of Science."

GOALS FOR 4S: The main goal of 4S must remain the forging and strengthening of the professional base for an "interdiscipline." This requires not only strengthening of the new 4S REVIEW and other science studies journals, but the organizing of symposia, conferences, and meetings in which the various disciplinary (and national) perspectives represented in 4S are forced to confront common problems. It is, after all, only in genuine interaction between disciplinary perspectives that science studies will transcend the old disciplinary boundaries, only by means of international contact that we will break the limits of national traditions, and only by forming an interdisciplinary and international community that we will overcome the fragmenting forces built in to our enterprise and the present climate.

Daryl Chubin

BIOGRAPHY: Associate Professor and Director, Technology and Science Policy Program, School of Social Sciences, Georgia Institute of Technology, where he has taught since 1977. Author of Sociology of Sciences: An Annotated Bibliography on Invisible Colleges, 1972-1981 (Garland, 1983) and co-author of The Cancer Mission: Social Contexts of Biomedical Research (Sage, 1980). Co-editor of special issues of Technological Forecasting and Social Change (1981) and The American Sociologist (1982). Current research on peer review and fraud, controversies in biomedicine, and interdisciplinary research processes. Has served on various 4S Committees and is presently Society liaison to Section L, AAAS. Former collaborating editor of Social Studies of Science, Scientometrics, and The American Sociologist. Current consultant to offices within NSF and NIH, respectively.

GOALS FOR 4S: The quality of life in 4S is determined by its publications and meetings. Now that the Newsletter has evolved into the 4S REVIEW, it should be developed further under a commercial publisher into a full-fledged scholarly journal. A place to start is the commissioning of review articles and periodic annotated bibliography. As for meetings, I share Arie Rip's aversion to "American-style big conferencing," and in addition to THE annual meeting, would like the Society to sponsor--either alone or in conjunction with other interest groups--regional workshops/conferences on specific themes. Such meetings would decentralize the life of the Society even more, highlight works-in-progress, and increase the face-to-face interaction that an annual meeting provides in only a superficial way.

Harry Collins

BIOGRAPHY: Harry Collins is Director of the Science Studies Centre of the University of Bath. He is an editorial advisor to SOCIAL STUDIES OF SCIENCE and to 4S REVIEW. His research is in the sociology of scientific knowledge and he has done empirical studies of areas of physics and of parapsychology. Books include FRAMES OF MEANING (RKP 1982) and a collection of source articles in the sociology of scientific knowledge. He also edited a special issue of SOCIAL STUDIES OF SCIENCE, entitled KNOWLEDGE AND CONTROVERSY.

GOALS FOR 4S: The chief function of the 4S should be as a unifying organisation. The 4S has been a tremendous success in forging an international character for Science Studies. The extent of its success - at least as seen through British eyes - is the growing redundancy of purely national forums for discussion. The 4S annual meeting has become THE forum in spite of what were seen as deep intellectual divisions in the early days of the Society. 4S must continue in this role - perhaps in concert with its new sister EASST. The other vital function for 4S is to help unify the discipline of Science Studies. While it is important that each of the sub-fields retains its disciplinary rigour, 4S must exploit the areas where there are common interests and a common focus. The growing together of philosophy, history and sociology that has been seen during very recent meetings must be nurtured.

Derek J. de Solla Price
[Profile not available]

Sal Restivo
[Profile not available]

Albert H. Teich

BIOGRAPHY: Albert H. Teich is Manager of Science Policy Studies at the American Association for the Advancement of Science. His principal responsibilities are in the R&D Budget and Policy Project. In addition, he is AAAS staff officer for the Committee on Science, Engineering, and Public Policy. Prior to joining AAAS, Teich was associate professor of public affairs and deputy director of the Graduate Program in Science, Technology, and Public Policy at George Washington University. Between 1969 and 1976, he served in a number of research, administrative, and teaching positions at Syracuse University Research Corporation and the State University of New York. Teich is the author of many articles and the editor of three books, Scientists and Public Affairs (MIT Press, 1974), Technology and Man's Future, 3rd. ed. (St. Martin's Press, 1981), and Science, Technology and the Issues of the Eighties: Policy Outlook (Westview Press, 1982). He holds a B.S. in physics and a Ph.D. in political science, both from M.I.T.

GOALS FOR 4S: I would like 4S to become the central professional association for persons interested in social studies of science--a goal which, I believe, is in sight. In order to do this, it must run an annual meeting of consistently high quality; it must offer sufficient tangible and intangible benefits to its members to attract and retain a membership comparable in size to the community of scholars it purports to represent; and it must structure its publications and activities so as to define common grounds among the diverse disciplines of its members. I believe the 4S Council must lead the society in this direction, and as a member I will do my best to see that it does.

Ron Westrum

BIOGRAPHY: I have been a member of the 4S since the first meeting and have been active in the sociology of knowledge and science for the last eleven years. I have specialized in the sociology of anomalous events and especially in scientists' reactions to these events. My major contribution to the 4S has been the newsletter SOCIAL PSYCHOLOGY OF SCIENCE, which I have edited for the Social Psychology Subgroup for the last three years, and which has allowed many researchers to know of each other's work.

GOALS FOR 4S: As a council member I would work to make the 4S more internally connected, much as I have done with the Social Psychology Subgroup. I would try also to get more involvement of the 4S with the scientific community generally, so that the 4S might have an impact on science as well as an understanding of it.

**Nominating Committee Report
Submitted by
Bernard Barber, Chair**

ANNOUNCEMENTS: FEDERAL AGENCIES

**NATIONAL SCIENCE FOUNDATION
DIVISION OF PLANNING AND POLICY ANALYSIS**

A new publication on social studies of science, **Studies of Scientific Disciplines: An Annotated Bibliography**, summarizes and indexes 285 studies in thirteen broadly defined science and engineering disciplines, with the aim of making the findings and conclusions more accessible to science administrators, policymakers and practitioners.

Copies of the document are available from the National Technical Information Service, Document Sales, Springfield, Virginia, 22161 using publication number PB83-159954. The accession number for microfiche copies available from NSF is NSF/PRM 82006. The current price is \$17.50 (\$4.50 for microfiche). A limited number of individual copies are available from NSF Division of Planning and Policy Analysis.

For further information, please contact Ms. Diane Weisz or Dr. Carlos Kruytbosch, Division of Planning and Policy Analysis, National Science Foundation, 1800 G Street, N.W., Washington, D. C. 20550.

ANNOUNCEMENTS: FUTURE CONFERENCES/SYMPOSIA

**NATIONAL SCIENCE FOUNDATION
DIVISION OF POLICY RESEARCH AND ANALYSIS
COLLOQUIUM ON**

"Funding Sources and the Direction of Science"

September 23, 1983

9:30 - 12:00

How do scientists come to study what they study? How do their sources of support influence problem choices? If the interests of research sponsors are a force pushing new knowledge in certain directions, are there any counterforces? If so, how do they operate, and how strong are they?

The National Science Foundation is holding a colloquium to sum up knowledge on these questions and point the way to further studies. For information, contact:

Susan E. Cozzens
Division of Policy Research and Analysis
Room 1229
National Science Foundation
Washington, D. C. 20550
(202) 357-7826

**AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE
SECTION ON HISTORY AND PHILOSOPHY OF SCIENCE--L**

Anyone wishing to organize or participate in a symposium at the Los Angeles meeting of the AAAS, May 23-28, 1985, in Section L (History and Philosophy of Science) should contact the secretary:

David L. Hull
Department of Philosophy
University of Wisconsin-Milwaukee
Milwaukee, WI 53201

The deadline for applications is August 1, 1984.

ANNOUNCEMENTS: POSITION AVAILABLE

**THE FRAUNCES TAVERN MUSEUM
RESEARCH HISTORIAN**

The Fraunces Tavern Museum, New York City, has an opening for a Research Historian. Special project on the history of public health in early America to culminate in a public exhibition and scholarly publication. Strong research and writing skills, working knowledge of archival and artifactual collections, and ability to travel are required.

Advanced studies in American history, history of medicine, public health, or technology; Ph.D. preferred. Competitive salary. Position available for 12-18 months commencing September 1983 (dependent upon grant funding).

Submit letter of interest, resumé, references and any relevant reprints to:

Curator of Collections
Fraunces Tavern Museum
54 Pearl Street
New York, New York 10004

ANNOUNCEMENTS: INTERNATIONAL DEVELOPMENTS

**UNIVERSITY OF LUND ANNOUNCES
NEW RESEARCH PROGRAMME**

The Research Policy Institute, University of Lund (Sweden) has a new research programme on technological change and technology policies in the Third World. A programme description is available free of charge from the Research Policy Institute on request. If you are interested to know more about the programme, please send your name and address to:

Technology and Development Group
Research Policy Institute
Magistratsvägen 55:N
S-222 44 Lund
Sweden

Your name will then also be put on the mailing list for future publications within the programme. Please give also the names of others who you think should receive the programme description.

ANNOUNCEMENTS: FROM OTHER JOURNALS

Social Studies of Science

The August 1983 issue of Social Studies of Science (Volume 13, Number 3) will contain the following papers:

The Disciplinary Stake: The Case of Chronobiology
Alberto Cambrosio and Peter Keating

Naturwissenschaft und Sozialismus: Tendenzen der Naturwissenschafts-
Rezeption in der deutschen Arbeiterbewegung des 19. Jahrhunderts
Kurt Bayertz

Uses of Knowledge and Values in Technical Controversies: The Case of
Nuclear Reactor Safety in the US
Steven L. Del Sesto

Historical Distributions of Multiple Discoveries and Theories of Scientific
Change
Augustine Brannigan and Richard A. Wanner

Anything Goes (Discussion Paper)--Denise Russell

Nature and Nurture (Essay Review)--Jonathan Harwood

ANNOUNCEMENTS: NEW APPOINTMENTS**VIRGINIA TECH****and****CORNELL****ANNOUNCE NEW FACULTY/VISITING APPOINTMENTS**

RICHARD M. BURIAN, currently at Drexel University, has been named Head of the Department of Philosophy, with concurrent academic appointments as Professor of Philosophy and Adjunct Professor in the Center for the Study of Science in Society. The Department of Philosophy is newly established through separation into two of the pre-existing Department of Philosophy and Religion; the newly established Department has a faculty of 12 people. Burian has been associated with Biology, History, and Philosophy Departments at the University of California at Davis, the Department of History and Philosophy of Science at Pittsburgh, and the Museum of Comparative Zoology at Harvard; he has been an associate dean at Brandeis University, and was assistant director of the 1982 Summer Institute on Philosophy of Biology sponsored by the Council for Philosophical Studies and the National Endowment for the Humanities.

DARYL CHUBIN, Director of the Technology and Science Policy Program at Georgia Tech, will be Visiting Associate Professor in the Program on Science, Technology and Society and the Department of Sociology at Cornell University for the 1983-84 academic year.

LARRY LAUDAN has accepted an appointment as Professor in the Center for the Study of Science in Society and Adjunct Professor in the Department of Philosophy. Laudan was the first chairman at Pittsburgh of the Department of History and Philosophy of Science, and was director of the Center for Philosophy of Science at Pittsburgh before coming to Virginia Tech as visiting professor in 1981. He is the author of Progress and Its Problems (1975), Science and Hypothesis (1981), and editor of Mind and Medicine (forthcoming). He is currently working on a book on values and scientific change, with support from an NSF-NEH Sustained Development Award.

RACHEL LAUDAN has accepted appointment as associate professor of Science and Technology Studies and History. A historian of geology and technology, Laudan has taught History of Science and Technology at Carnegie-Mellon University and was Assistant Director of the Center for Philosophy of Science at Pittsburgh. She has published on 19th century geology as well as on the plate-tectonic revolution, and is currently working on the relevance of models of scientific change for technology studies and on a monograph on the history of geology.

ANNOUNCEMENTS: ERRATUM

In Volume 1, Number 1 of **4S REVIEW**, we announced a new book edited by Harry M. Collins, entitled **Sociology of Scientific Knowledge: A Source Book**, and showed the contents of the book with authors' names and contributions. Harry writes that another paper should have been included in the original publicity. The additional contribution is:

D. Robbins and R. Johnson, "The Role of Cognitive and Occupational Differentiation in Scientific Controversies"

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