

4S Society for Social Studies of Science

Fall 1979

Volume 4 Number 4

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I have been thinking about you and your work for some time.

I am sure that you are doing a great job and I am proud of you.

I hope to hear from you soon and I am sure that you will be successful.

I am sure that you are doing a great job and I am proud of you.

I hope to hear from you soon and I am sure that you will be successful.

PRESIDENTIAL ADDRESS - FOURTH ANNUAL MEETING

"Science and the Polity: Changing Relationships and Their Consequences for Social Studies of Science"

Dorothy Nelkin, Program on Science, Technology and Society, Cornell University, Ithaca, New York 14853

Robert Merton predicted back in the 1950's that the field of social studies of science would thrive as science became defined as a social problem "like war, the perennial decline of the family or the periodic economic depressions." There is no doubt today that science is defined as a social problem. Just when the recombinant DNA controversy has abated, there are rumblings over sociobiology and genetic research. Fundamentalists are increasingly open in their objections to the teaching of evolution in the public schools. The methods of research, the use of human subjects, the role of deception in social science are all in dispute. A recent editorial in Science magazine describes science as "besmirched by technological failures" and notes the resulting "credibility gap." While science is seen as problematic mainly in terms of its social implications, controversies have embroiled scientists in endless debates.

As observers, we must pursue the sources of such conflict. What is the significance of the politicization of questions that scientists have long regarded as technical? A wide variety of explanations have been offered to account for recent disputes:

- Controversies, claim some people, are simply parochial protests against decisions that affect particular interests--but another manifestation of interest group politics.
- Disputes, claim others, reflect traditional anti-science sentiments.
- Objections to science and technology reflect insufficient knowledge, and the resulting uncertainty about questions of risk.
- Or, it is said, science is a problem only because people are poorly informed.

Each of these explanations clearly has some salience. But it seems to me that the persistence of conflict in so many areas of science and technology, and the convergence in the concerns of people with so many diverse social and political perspectives, call for a different level of interpretation.

I will argue that beneath the specific substantive issues at stake in these disputes is a growing ambivalence in the public perception of science as a source of legitimacy --as an authoritative standard of rational decision-making. This represents a significant ideological change, for scientific standards have long had universalistic appeal as a basis of consensus on policy questions.

Yaron Ezrahi has suggested the special affinity between the norms of science and the liberal democratic image of politics. Less blemished than other activities by transcendental, factional, personal or other inaccessible references, science has had special force as a model for political decisions based on homogeneous and rational standards. This image accounts for the long history of proposals to enhance the policy role of science.

In 1932, for example, the physicist Robert Millikan proposed "a scientific jury system" to solve the policy problems of a society in the midst of depression. Policies were to be established and administered by scientific experts who, by virtue of their knowledge, were qualified to judge how to improve the human condition. Democracy, in the view of Millikan and many of his contemporaries (Charles Merriam and Michael Pupin) is founded on an objective, knowledge-seeking scientific approach to the problems of governance. A similar notion reappeared in the recent proposal for a science court that would bring the best judgments of the scientific community to bear on the technical aspects of controversial issues, thereby providing a "defensible credible basis for policy decisions." It has also re-emerged in proposals for a Technical Magistrature and even for a new profession of certified public scientist.

However, the image of science as a compelling basis for fair and objective decisions has been blemished by factional disputes and tarnished by the use of science as a means of political control. Metaphors of "the mandarins," "the priesthood," or "the new Brahmins" are used to describe the function of the "knowledge elite." A recent New Yorker cartoon digs hard at the presumed authority and arrogance of experts. A man on the witness stand, presented with the Bible, asks the presiding judge, "Is this really necessary, your Honor? I'm an expert." When Dr. John Kemeny was asked to justify his lay appointments to the Three Mile Island Commission, he said:

I think it would be wrong for the Commission to consist of a bunch of engineers. We are sort of a national jury on this issue and would you want the jury to consist of experts? I think it would be a mistake; you want the average American to believe what this Commission says.

Science, I am suggesting, has become defined as a "problem" not only in its immediate impact but because of growing concern about its political role.

Let me develop this argument by emphasizing the value issues underlying recent disputes over science and technology and then suggesting their implications for the role of science as a legitimating institution. Conflicts over science and technology reflect far more than technical questions of risk. Issues of equity and distributive justice arise in the siting or development of a technology that may harm a local community while serving a larger region. A number of ethical questions have provoked disputes because of abuse of scientific findings. Science, it is argued, is a "social weapon" that cannot be dissociated from its potential use. Thus research that associates genetically mediated characteristics with human behavior is inevitably controversial.

Still other critics see science as a social problem because of its moral implications: science, it is feared, may change the normal state of nature, alter the genetic structure of man, or threaten deeply engrained beliefs about free will and self determination.

While disputes based on such issues reflect very different values and ideals, there is a striking convergence in their themes. For in each case, the overriding question is "who controls?" Apparently isolated disputes over very diverse issues--genetic research, sociobiology, nuclear power--begin to form a coherent pattern, appearing more as a social movement than as a set of discrete events. They express both an ideological resistance to the reductionism epitomized

by science and a political resistance to the powerlessness implied by the pervasive influence of expertise. They are less against science and technology than against the power relationships associated with them; less against science than against the use of scientific rationality to mask political choice.

In the context of these fundamental political issues underlying disputes, science itself becomes a political resource exploited by various interests to justify their claims. It is a striking and ironical aspect of recent disputes that whatever political or moral values motivate them, technical expertise is seen to be the only way to challenge controversial decisions. Concerns about the freedom to select a cancer therapy tend to devolve into technical arguments about the efficacy of treatment. Moral opponents of fetal research engage in scientific debate about the precise point at which life begins. Even fundamentalists seeking to have Genesis taught in the public schools use scientists to "prove" that "creation theory" is a valid alternative to "evolution theory." Indeed science is less an autonomous source of consensus distinct from political goals than an adversarial tool in political disputes.

This extraordinary confusion of value issues and technical justification in these disputes suggests that controversies over science and technology represent groping efforts to renegotiate the social values and priorities that are embedded in the relationship between science and the polity, and indeed to find a new source of political consensus based on ecological insights or ethical principles.

If this is a reasonable analysis, what does it imply for the social studies of science? Let me very briefly lay out just a few research ideas.

First, controversies suggest that science should be studied not only as an activity of exclusive elites but as a resource. This calls for more research on the pervasiveness of science as an applied activity--studies of technical advice, of national labs, of consulting, of industrial science. Used as a resource in these contexts, what are the implications for the distribution of political power? How is science used to make decisions in the face of technical uncertainty and, indeed, disagreement about risk? What is the professional and political effect of the sheer growth in the numbers of technicians, scientists and engineers? What kind of tensions develop between the corporate or political interests of this growing sector and its traditional professional norms?

Also of interest is the role of science in shaping the assumptions underlying public policy. Critical scientists have posed provocative questions about the relationship between biology and social ideology, and the use of biology as a means of social control, but there is to my knowledge relatively little research on the way biological concepts are used as a guide for social practice in fields such as criminal behavior, health, education, and work. How do biological assumptions affect different concepts of health, of illness or pain? How have such concepts changed over time? How are they expressed in different cultures, religions or age groups? How do assumptions about the nature of biological systems affect concepts of professionalism? And how do they feed back on the epistemological development of science itself?

The controversies over science and technology themselves can be a useful focus for research, raising questions about professional responsibility, about the limits of inquiry, about the external assessment of science. What effects do disputes and

the activities of critical scientists have on the scientific enterprise? How do scientists respond and adapt to external pressures? Are they moving, as some contend, towards politicization and the development of an ideology of their own?

As science has become defined as a social problem it has also come into vogue. The proliferation of television programs and new popular science journals in the past year is remarkable. Even Penthouse is publishing a science magazine. To quote one observer, "Science has become another of life's staples along with food, Saturday night and the Pittsburgh Steelers." The recent efforts to communicate science raises questions about public attitudes and popular notion of science and how they are shaped by political interests, ideologies or social class. Here again, popularization of science, long a neglected field in this country, is ripe for research.

One thing is clear--defining science as a problem is not necessarily bad for scientists. It has not only attracted popular interest, it has created jobs. As scientists dispute the causes of the Three Mile Island accident, opportunities have blossomed--some \$10 million will have been spent on consulting fees in less than a year. But ironically, as scientists enter disputes, inevitably transforming them into a technical discourse, this only generates further scepticism about their political influence. The more science becomes a reference for political choice, the more it will become a focus of public dispute.

ABSTRACTS OF PAPERS OF FOURTH ANNUAL MEETING

PANEL 1-A: CONTEMPORARY RELATIONS BETWEEN SCIENCE AND LITERATURE

"Should We Be Serious About Science Fiction?"

Mark Adams, 117 Smith Hall, D-6, University of Pennsylvania, Philadelphia, Pennsylvania 19104

Science fiction, whatever its standing with the literary academy, is a potentially rewarding research site for historians and sociologists of science. There is some debate about science fiction's relation to future science and technology, but the genre is a goldmine of past popular attitudes and images relating to science that should be part of any broadly based history of science in the U.S. from the 1930's on. Of particular interest is the so-called "Golden Age" of American science fiction, the period from about 1939 to 1945, which was strongly influenced by John W. Campbell, the editor of the popular magazine Astounding Science Fiction. Among the features and attitudes characteristic of the science fiction of that period are the following: (1) science as explicitly opposed to religion and mysticism; (2) the engineer as hero, valued for his practical skills; (3) "to fight and conquer the environment is the noblest task of mankind" (from a Campbell editorial); (4) science and technology as neutral, having little effect on human nature; (5) change as pervasive, and therefore more interesting than stable utopian states; (6) social problems seen as technical problems with technical solutions; (7) aggressive optimism.

"The Fictional Mode and the History of Science"

June Goodfield, Rockefeller University, 1230 York Avenue, New York, New York 10021

Science is a process engaged in by a group of people who encompass the whole range of human qualities and aspirations. They are all intellectuals, and share a commitment to a methodological ethic, but beyond that they have little in common. There is no one scientific method or style. Thus the importance of understanding the role of individuality in science, and of including personal, cultural, and national factors in one's descriptions of scientists' behavior--as 19th-century novelists did for behavior in general. Wanting to give the proper emphasis to individual creativity in science, I have for the last four years followed closely the work and life of one scientist and her group working in immunology. The results of that study are now complete, and will be published after a year's delay to allow time for outside commentary (which will be published as part of the volume). I am currently at work on four episodes in the history of science in which the scientific outcome was greatly influenced by personal and social factors. My next project, which will allow me to give the widest possible play to individual factors, will be a novel about contemporary science.

"The Gothicization of Science in Modern Literature"

Martin Green, Tufts University

"Gothic" is the word that best describes the qualities of science as it appears in certain works of contemporary literature. Thomas Pynchon's Gravity's Rainbow (1973), widely regarded as the most successful incorporation of science and technology into

the serious literary imagination of our time, is a good example. In Pynchon's complex novel, science is associated in a variety of ways with images which the reader responds to as grotesque, exaggerated, labyrinthine, unreal, secretive, and morbidly sexual. An example of a different kind is Norman Mailer's journalistic study of the Apollo-11 moonshot, Of a Fire on the Moon (1970). Mailer, typically, approaches his subject with Gothic expectations, but finds himself rebuffed by the un-Gothic appearance of the astronauts and the Manned Spaceflight Center in Houston. Mailer partially recovers himself later in the book by focussing on other aspects of his subject.

At the end of the 18th century, the Romantic poets Wordsworth and Coleridge consciously used certain elements of the Gothic in an attempt to restore traditional spiritual values. The modern use of Gothic, rather than hopeful, is by turns desperate and defensively humorous. Many modern writers who would like to loosen the grip of rationalism on their culture feel that science and an attendant rationalism are so thoroughly established that their efforts cannot succeed.

PANEL 1-B: THE SOCIOLOGY OF SCIENCE IN DEVELOPING COUNTRIES:
THE CASE OF LATIN AMERICA

"Foreigners in Their Country: The Brazilian Scientific Elite"

Simon Schwartzman, University Research Institute of Rio de Janeiro

This paper is based on the analysis of the background information of about 70 Brazilian scientists that were responsible for the establishment of the Brazilian scientific institutions of today, in the exact and the biological areas. It takes into account the family of origin, the place of birth, the professional and education career and the generation factor. Most of the subjects are senior scientists that had their academic peak about twenty years ago. In this sense, they make up a generation of pioneers.

In general, the data suggest that these scientists are very atypical and marginal in the Brazilian society. They are often foreign born or children of immigrants, and only a few of them had direct ties with the Brazilian social and political elite. Their scientific initiation was provided through quite unusual and special circumstances of family relations; they had the opportunity to join one of the few active centers of scientific research in Brazil, and to go abroad to advanced studies quite early in their careers.

From this data, the paper discusses the implications of the relative marginality of the Brazilian scientific elite for the institutionalization of scientific research in the country.

"A Study of the Opinion of Scientists in Venezuela about the Role of Science and the Implications of such Opinion on their view of Science Policy"

Yajaira Freytes and Marcel Roche, Department de Estudio de la Ciencia,
Instituto Venezolano de Investigaciones Cientificas (IVIC), Caracas,
Venezuela

Direct interview was conducted on a total of 473 persons employed as investigators in Venezuela in the course of the years 1976-1977. These investigators work in two

public universities and a Government Research Institute located in the metropolitan area of Caracas, the capital. The sample was stratified random and represented 46.4% of the investigators in the metropolitan area and 16.8% of those in the entire country for 1972.

Even though certain characteristics of Venezuelan society, such as: total freedom of publication, possibility of contacts with foreign countries and the country's socio-economic situation, were highly appreciated, in general the research working conditions were considered unfavorable. Even so, 41.1% of the group considered that Venezuela at present is the Latin American country with the best environment for research, followed by Brazil, Mexico and Argentina, in that order.

The beginnings of scientific activity in Venezuela, with few and sporadic exceptions, date from the fifties in this century. The reasons for this late development were seen as being cultural in nature by most investigators, followed by: lack of political and Institutional support, lack of social demand, and underdevelopment in general. In fact, the characteristics of science in Venezuela, revealed by other studies and the present one, suggest that science is oriented toward cultural and academic rather than toward socio-economic aspects. Thus, 51.4% of the group studied stated that they were doing basic research, 45.4% basic and 2.9% development. It may be thought that these figures are influenced by the fact that the interview was conducted in academic environment, but it may be noted that, for 1972 at the national level, 61.9% of the investigators were doing research in academic institutions, according to the Consejo Nacional de Investigaciones Cientificas y Tecnologicas (CONICIT). According to the same source, 43.2% cultivated basic science, 54.1% applied research and 2.9% development. The connections between University research and production are extremely weak in Venezuela.

CONICIT, whose work began in 1969, has drawn support mostly from committees made up of active scientists in order to set up its Science and Technology plans. On the other hand, it does not possess adequate mechanisms to implement such plans and has had its chief success so far in stimulating a favorable academic atmosphere. This could be the reason why most of the interviewed scientists support the present mechanisms of elaboration and implementation of scientific policy, which, as a matter of fact, cannot and does not interfere with academic freedom.

"Social Structure and Productivity in a Scientific Research Institute"

Larissa Lomnitz, Univ. Nacional Auto. De Mexico, AP. Postal 70-325,
Mexico 20, D.F. Mexico

Structural studies of academic communities for the purpose of assessing the influence of social factors on scientific creativity are rarely conducted by anthropologists. A research institute within a university was studied by participant-observation and by other anthropological methods. Data were obtained on: roles (professores, students, administratos), their recruiting attributes and socioeconomic characteristics, relationships between roles, symmetrical and asymmetrical exchange, differentials in prestige and power, formal and informal groups, internal structure of groups, basis of group formation (kinship, friendship, political ideology, research interests), interaction within the group and among groups. An analysis of the internal power structure is attempted, with particular emphasis on director researcher and researcher student relationships. By means of network analysis the ideal model of scientific communications among

institute members and between members of related institutes is compared with the real situation. Cooperation, if any, is based horizontally on friendship and vertically on power differentials. On the basis of these results the following questions are discussed: How and to what extent are scientific careers (area of interest, academic background, research performance) determined by the institute internal structure? What is the influence of social structure on scientific productivity?

ROUNDTABLE DISCUSSION C

"Receptivity to Unusual Ideas"

John Arnold, Thaddeus Flynn and James Feeny, Worcester Polytechnic Institute, Worcester, Massachusetts 01609

Discussion centered upon the results of the two small research projects done by undergraduate students at WPI under the advisorship of the session organizer. Their significance lies in the way they connect ongoing research into anomalies and fringe science with the study of cognitive styles in science. The authors hoped to shed light on Kuhn's interpretation of the process of scientific change by relating cognitive and personality characteristics to the credibility ratings given unusual scientific theories or reports of anomalies by members of the scientific and technical community.

Kuhn never really developed his ideas about how or why some people would become early adherents to paradigm challenging theoretical frameworks. However, he implied that such commitments would be made on highly subjective grounds and that vested interests would account for the greater weight of evidence needed to convince some members of the research community than others. The issue is important because it is an essential aspect of his theory that some be converted early and work to develop the theory into a viable paradigm contender capable of convincing the bulk of the field. Hence, the qualities of these early adherents are of great interest for our understanding of the process of scientific change.

Arnold and Flynn presented 31 senior science and engineering students with eight (8) questionable or strange ideas (from a variety of sources) involving issues in physics, biology, mechanical engineering, and computer science. The specifics ranged from an argument suggesting the existence of sea serpents to the claim of an invention capable of tripling the gas mileage of any car. These reports were evaluated individually on the basis of their credibility and then compared with one another by students majoring in the fields involved as well as in the unaffected fields. Measures of cognitive and personality characteristics believed to be relevant based on earlier research were also administered.

The results indicated that two of the personal factors, a personality dimension called "independence" and a cognitive characteristic known as "differentiation" ability, could be combined to yield a good predictor of the respondents' overall receptivity scores (based on their reactions to the whole group of unusual ideas). "Tolerance of Ambiguity" also made a small difference, but it did not matter whether the phenomena involved one's own field of expertise or not.

Since all of the ideas clearly contradicted prevailing theory in those fields, the vested interest and marginality explanations offered by Kuhn to explain differences in receptivity are placed under suspicion by these findings. Nevertheless, the results fit quite well with prior studies demonstrating the influence of cognitive style on choice of fields (Wilkes, 1977) and the criteria emphasized most in the evaluation of research reports by peer reviewers (Wilkes and Neumann, 1978).

Feeny's study began as a straightforward attempt to generalize these findings to practicing scientists while taking into account the respondents' self images and perception of the state of their fields. It developed in new directions because of his decision to concentrate on the "insider" judgements so crucial to the process of scientific change and interview his respondents in some detail about the grounds for their evaluation of new theories. Hence his sample consisted of 22 academic physicists on the faculty of Harvard, MIT, and Brandeis who were asked to evaluate a proposed theory of "random electrodynamics" with paradigm breaking potential. The theory, which was developed by Boyer of CCNY, contains no quantum assumptions yet promises to explain the same electrodynamic phenomena covered by established quantum theory. The same idea had been presented to the student sample in a simplified form as one of several unusual ideas.

Initial field work revealed that the physicists could not or would not respond as anticipated with a clear opinion. They did not consider themselves insiders to the implied theoretical conflict and protested an inability to pass judgement. Yet the interviewer sensed that they really could not even take the idea seriously enough to object to it. Feeny redeveloped his questionnaire to find out if this was the case by including three "strange" claims to be evaluated relative on one another as was done in the student study.

The resulting evaluations indicated that the physicists did think much less of the idea than the sample of students had and far less of it than the key comparison question involving the likelihood that "Ultra Intelligent Machines" (with powers of intellect beyond that of humans) would be developed. In this assessment they differed from the students who split evenly on the UIM's and rated the new physical theory slightly more likely than the development of super-intelligent machines.

It is not yet clear whether the personal factors associated with receptivity among the students will be operative among the physicists, as Feeny's analysis was not complete at the time of reporting. However, these findings set off a spirited discussion that moved briskly from just what Kuhn's theory of paradigm change really implied at the personal level, to the broader implications of these findings and the validity of Feeny's initial subjective interpretation of how the physicists he interviewed were making their evaluations of a new theory they had never heard of before.

ROUNDTABLE DISCUSSION D

"Autonomy of Scientists"

Simon Marcson, Sociology Department, Rutgers University, New Brunswick,
New Jersey 08903

Ten persons attended the roundtable on autonomy of scientists. I had asked two sociologists to say a few words after my introduction involving largely a list of questions about the topic. Jerry Gaston emphasized the importance of differentials in autonomy for different organizations. Maurice Richter emphasized the importance of differences in autonomy for science and scientists in different societies, such as China and Russia.

The discussants fully participated and touched on: differentials in autonomy by discipline organization and individual scientist; problems in autonomy arising in different societies, and political environments; consequences for scientific accomplishments and productivity occasioned by differentials in autonomy. The brief time allotted was hardly sufficient to get into many of the aspects of autonomy.

ROUNDTABLE DISCUSSION E

"Annotated Bibliography of Studies of Scientific Disciplines"

Carlos Kruytbosch, Room 423, Office of Planning and Policy Analysis,
National Science Foundation, Washington, D.C. 20550

A draft of an annotated bibliography of studies of scientific disciplines was circulated at the 4th Annual Meeting of the Society for Social Studies of Science, November 2-4, 1979 in Washington, D.C. The bibliography, prepared in the Office of Planning and Policy Analysis at the National Science Foundation, is intended for use by science administrators who need a guide to literature on social and organizational issues in specific disciplines, and by scholars engaged in the social studies of science.

A final version of the bibliography is planned for publication in early 1980. We encourage suggestions, contributions, and criticisms of the draft to be directed to Diane Weisz or Carlos Kruytbosch, Room 423, Office of Planning and Policy Analysis, National Science Foundation, Washington, D.C. 20550. (Phone (202) 634-1664) (A few copies of the draft bibliography are still available from that office.) Those who provide contributions and comments will receive copies of the final version.

PANEL 2-A: HISTORICAL AND PHILOSOPHICAL SCIENCE STUDIES: IMPLICATIONS FOR SCIENCE POLICY

"Philosophy of Science, Science Policy, and Science Education"

Michael J. Zenzen, Department of Philosophy, Rensselaer Polytechnic
Institute, Troy, New York 12181

Can one discern symbiotic relations among philosophy of science, science policy and science education? The paper argues that philosophy of science in its original forms served the ideological function of confirming science in its

self-sufficiency and relieving it of the burden of reflexivity. A partial consequence of this is the conflation of science and progress and legitimation of the scientific management of social problems.

I suggest some revision in science education and then turn to science policy and science policy studies. The central theme here is that if science policy studies attribute too much rationality to science policy then SPS will serve the ideological "masking function" of protecting science policy from the sort of inquiry which questions the rationalization of power.

The paper concludes by considering the recent "turf war" between FDA and NIH over government policy on nutrition and suggests possible lines of inquiry for SPS.

"Math Blocks: A Sociological Explanation and Some Proposed Solutions"
Randall Collins, Department of Sociology, University of Virginia,
Charlottesville, Virginia 22903

It is proposed that math blocks are produced by the method of teaching mathematics through a graduate sequence of particular problems. The method selects students for their mathematical intuition, and fits the self-image of mathematicians as creatively finding their own way to higher understanding. For students who lack innate intuition, however, the method is experienced as the most authoritarian part of the school curriculum, tying them to a tightly controlled progression and allowing no overview of nor reflection upon the more general ideas and methods that students are expected to learn. Historical changes in the structure of schooling have made rejection of the mathematical curriculum especially common in twentieth century American education. A number of suggestions are offered for overcoming math blocks by revising teaching methods in high school and college mathematics.

PANEL 2-B: THE SOCIOLOGY OF APPLIED RESEARCH

"The Social History of SIDS Research in the United States, 1945-1974"
Karl G. Hufbauer and Michael P. Johnson, Department of History, University
of California, Irvine, Irvine, California 92717

During the last decade sudden infant death syndrome (SIDS) emerged as the subject of a national research mission and a SIDS research community formed among medical scientists. As in other fields of medical research, scientists, the National Institutes of Health, and Congress participated in these interrelated developments. But the key group in putting SIDS on the nation's medical research agenda and in insisting upon institutional and material support for a SIDS research community were the parents of SIDS victims. How bereaved parents interacted with pediatricians, researchers, NIH, and Congress is the central theme of the social history of modern SIDS research. The interactions formed three basic stages: first, from 1945 to 1961 the results of pioneering research on sudden unexpected infant deaths convinced some parents that an undetermined cause, rather than their neglect, was responsible for these deaths; second, from 1962 to 1969 parents fostered NIH support for a SIDS research mission at the University of Washington and developed a compelling rationale for a national research mission; third, from 1970 to 1974 parents persuaded Congress to establish SIDS as a top priority mission of NIH and to provide the emerging SIDS research community with ample resources. In this instance the

consumers of medical research overcame their isolation, disorganization, and weakness in the scientific hierarchy to have a major impact on scientific developments.

"Cosmopolitanism in Basic versus Applied Research"

Susan E. Cozzens, Institute for Scientific Information, 3501 Market Street,
University City Science Center, Philadelphia, PA 19104

In recent years, sociologists of science have been seeking a Holy Grail: The Relationship between Cognitive and Social Structure. The exercise presented here is a small part of that search. In this presentation, after reviewing the concepts and issues on which the research is based, I will compare one basic and one applied research area in physics, chemistry and biology. Using survey data, I will attempt to relate variation in intellectual structure to three dimensions of variation in the pattern of communication in those areas. Finally, I will try to draw the implications of this analysis for future research.

PANEL 3-B: SOCIAL PROCESSES OF SCIENTIFIC INVESTIGATION

"The Scientist's Cognitive Limitations: Descriptive and Prescriptive Implications"

David Faust, Division of Child and Adolescent Psychology, University of
Minnesota, 420 Delaware Street S.E., Minneapolis, Minnesota 55455

Major advances in the development of knowledge are often accomplished by the design of methods for cross-validating intuitive constructions of reality. Science is one such method. And yet prescriptive scientific methodology is largely based on philosophical-intuitive systems of knowledge that have not been subjected to empirical test. However, since prescriptive scientific methodology makes implicit assumptions about the human capacities of the scientist and about means-ends relationships, assumptions that have real world referents, certain of these assumptions can be tested empirically. Researchers studying science can potentially conduct such critical tests. Findings providing possible disconfirmation of philosophical prescriptions especially merit careful consideration and can be of major importance. Although the study of science has not thus far provided a clear disconfirmation of a philosophical prescription, recent findings in cognitive psychology show definite promise for accomplishing this. A relatively large body of research has demonstrated that human judgment is far less accurate, and far less capable of managing complex decision tasks, than was previously assumed. Although this research has not been conducted directly with scientists in the actual process of scientific investigation, it appears likely that results will generalize to this domain, and that we will find that scientists are far less capable of many of the judgment tasks required of them in most prescriptive philosophical programs. For example, the judgments prescribed for theory evaluation may be performed inadequately due to cognitive limitations. Studies of the scientist's judgment capacities, then, should be a high research priority, given the possibility for providing the first clear empirical disconfirmation of assumptions of prescriptive scientific methodology. Such empirical tests of philosophical assumptions should be a central thrust in the study of science, and may eventually further elevate the genius of the scientific method.

"The Other Side of the Coin: Occult Belief"

Barry Singer, Psychology Department, California State University, Long Beach
6101 E. Seventh Street, Long Beach, California 90840

My preliminary remarks will outline my background and interest in paranormal belief. My background includes a number of experimental studies on occult belief, teaching courses on the topic, and a book in press. My basic question has been why paranormal beliefs are so strong and so prevalent in scientifically advanced societies. Another interest in examining paranormal belief is to highlight and understand opposing scientific approaches to belief and through contrast to differentiate them from intuitive or non-rational approaches.

The bulk of the paper will briefly outline some of the factors which we hypothesize account for paranormal belief: media bias; lack of scientific rebuttal; prevailing cynicism about social institutions and about science in particular; current misinterpretations of philosophy of science; inadequate science education; narrow socialization concerning the validity of science; inadequacies of probabilistic reasoning; dependence upon intuitive or even superstitious approaches under conditions of low risk and/or high uncertainty; personal needs to believe; social unrest and devaluing of conventional meaning systems.

Finally, I will pose the questions of how science avoids or compensates for the above factors, and ask whether the compensation is complete. That is, do processes of irrationality which are highlighted by extreme and bizarre beliefs hint at vestiges of such processes in science? Due to time considerations, I will pose this question without addressing it; but, if appropriate, will address it during the discussion period.

PANEL 5-A: SCIENCE INDICATORS FOR RESEARCH AND POLICY

"A Quantitative Analysis of American Chemistry, 1876-1976"

Robert F. Bud, P. Thomas Carroll, Jeffrey L. Sturchio and Arnold Thackray*

*Department of History and Science, University of Pennsylvania, EF Smith Hall D,
Philadelphia, Pennsylvania 19174

The authors have examined the history of American chemistry over the past century using the technique of science indicators. Among the trends discussed in this report of their findings are the growth of chemistry as an occupation, the early importance and subsequent decline of chemistry degree conferrals relative to other fields, and the entrenchment of chemistry within American higher educational institutions. Prosopographical analysis of the careers of American Chemical Society presidents further elucidates the changing nexus among occupation, profession, and discipline.

"Demography of Problem Areas in Astronomy, 1950-75"

Thomas F. Gieryn, Department of Sociology, Indiana University,
Bloomington, Indiana 47405

Analysis of the growth of radio and x-ray astronomy in the 1960s suggests that future reductions in the size of entering cohorts of new doctorates in astronomy may lengthen the time needed to exploit future innovations or breakthroughs. This

may well lead to slower rates in the advancement of astronomical knowledge. Most of the scientists making up the early growth of these two problem areas had recently earned their PhD's, and, it was found, the probability of initiating research in radio or x-ray astronomy declined with the age of the scientist. Since smaller entering cohorts of new doctorates would imply an overall aging of the astronomical community, the rate at which scientists will move in to exploit future innovations will be slower than during the period of peak growth during the 1960s.

PANEL 5-B: COMPARATIVE PERSPECTIVES ON THE FORMULATION AND
IMPLEMENTATION OF SCIENCE POLICIES

"Policymaking for Science in China and Japan"

Richard P. Suttmeier, Rd 1, Griffin Rd, Clinton, New
York 13323

A comparison of policymaking for Chinese and Japanese science, the paper opens with an assessment of the levels of effort devoted to basic research, and the role of basic research support in the national science policies of both countries. Second, the author examines the institutional structures for basic research--who makes policies for it and who performs it. Third, he considers the participation of scientists in policymaking, and the strengths and weaknesses of the national systems supporting basic research. The paper concludes with the presentation of five hypotheses regarding the impact on science of bureaucratic competitors, political elites, the predictability of support, approaches to technological innovation, and the international environment.

"Policymaking for Science in Canada and the Soviet Union"

Yakov M. Rabkin, IHSPS, University de Montreal, Montreal H3C 3J7
Canada

The comparison of Canadian and Soviet science policies begins with a review of the historical circumstances of, and expectations linked to, the transfer of modern science into each of the two countries. Resulting patterns of science organization are then discussed, with particular emphasis on the role of the central governments. This is followed by an analysis of the priorities in science policies. Express attempts to foster science in order to catch up economically with older industrialized nations are characterized and their impact assessed. Finally, the contributions of scientists to the formulation of science policies for their countries are analyzed and linked with the prevailing patterns of employment of Canadian and Soviet scientific manpower.

"Power and Influence in Australian Science Policy From 1968 to 1978: An International Comparison"

David Bennett, University of Oxford
Peter Glasner, Department of Sociology, Polytechnic of North London,
Highbury Grove, London, England N5 2AD

The paper traces the development of the policymaking machinery for science and technology in Australia during the last decade. The institutionalization of science

policy advisory structures is emphasized, in contrast with several other industrialized nations. A discussion of the Australian Science and Technology Council suggests that Australia has followed an unusual path in the institutionalization of scientific advice to government. The first generation of science policy advice was primarily ad hoc and concerned with "science-for-policy" while the second generation has focused more on "policy-for-science." The unique nature of the Council's membership, with its heavy commitment to advice from industry, reflects a reaction to the peculiar nature of the Australian government's expenditure on science as a whole.

THOUGHT AND OPINION

Notes on the Fourth Annual Meeting

Martha Chappell Dean
Syracuse University

Science policy was the theme of the Fourth Annual Meeting of 4S, which was held in Washington this November. Although the small group size was conducive to much positive social and intellectual interaction among the attenders, the meeting could have benefited from and would have interested many 4S members and potential members who were absent this year.

There were several issues and studies presented at this meeting which made it well worth attending. For example, the panel on science indicators, which included papers by Thackray, Gieryn, and Griffith, was particularly valuable in providing quantitative evidence for an emergent state of crisis in certain scientific specialties. Thackray and his coauthors pointed to an enduring trend in which the proportion of doctorates granted in chemistry is decreasing relative to the other sciences. Gieryn presented evidence suggesting that the rate at which problems in astronomy are solved will decrease with reductions in the rate at which new PhD's are produced. Griffith discussed the use of cocitation cluster analysis as the most epistemologically relevant quantitative tool available to science policy researchers; however, he cautioned that the use of such bibliographic indicators for policy decisions is in a state of transition and could possibly generate an "unamiable" form of policy if done prematurely. As the discussant for the science indicators panel, Holmfeld raised pointed questions and stimulated lively discussion among the panelists and the audience. Quite clearly, there is a practical problem concerning the best way for policy makers to make use of science indicators. One obviously important issue not mentioned in that discussion is the great risk of corruption of indicators following their use in policy decisions. Decision makers must be extremely careful in making funds and jobs contingent upon the level of a specific indicator, because as soon as scientists become aware of that contingency they may do whatever they can to alter the measured indicator artificially; this deliberate alteration may or may not substantially change the underlying process of which the indicator is a measure.

Another interesting panel focused on the future of academic science. Again, most of the panel presentations pointed to a rather bleak outlook, if current trends continue. In one outstanding paper, Meyer showed that the ratio of high-quality physics researchers to absolute number of new physics PhD's remains constant, whether that number is increasing or decreasing. Gieryn and Meyer, then, appear to have arrived independently at the conclusion that declines in the production of PhD's may have a negative effect on the quality if not the quantity of research in particular specialties.

There were other sharply defined papers, such as Cozzens' description of cosmopolitanism in basic versus applied science, and Arnold and Flynn's roundtable presentation on receptivity to unusual ideas. Probably many papers which I did not hear were also quite good, and the fact that they are not discussed here is no indication of their value.

Although there were informational and social benefits from this year's meeting, there are several opportunities for improvement in future programs. First, the organization of the meeting around a dominant theme such as science policy may have discouraged attendance by some people who have little or no interest in that theme. Perhaps a better plan would be to organize multiple thematic panels which continue from year to year. Aside from attracting more registrants, such an organization might serve to improve the quality of the papers. Authors of on-going research would feel less pressured to present it prematurely at their theme meeting if they knew that their theme panel would likely be available again the following year.

A second area which can be improved is the program description mailed in advance of the meeting. Individual authors should be aware that they have some influence over people's decisions to attend the conference. In next year's advance program, more precise and informative titles should be available, and perhaps very short abstracts should be included. Such a program description will stimulate more enthusiasm.

It may be that the lack of precision and definition in this year's titles reflects a genuine uncertainty about the appropriate content and method of 4S research. Some attenders expressed concern that the number of well-defined quantitative studies appears to be decreasing rather than increasing. If our specialty is to progress fully, quantitative researchers must be encouraged to participate more actively in future meetings.

The relative lack of quantitative research to support problem structuring and theory building may be a reaction to the admittedly choking constraints of early positivism. However, we seem to be committing a reverse error by implying that problem-solving research of an empirical nature is by definition of lesser interest than problem-structuring exercises. We were fortunate that the meeting was richly endowed with carefully constructed protocols and caveats for researchers; however, too few of these authors were further inclined to provide us with empirical tests of their structuring efforts. In view of the current crisis surrounding science, which was so poignantly described by William Carey in his keynote address, problem-defining and problem-solving activities should have equally important roles in 4S.

PARADIGMS LOST? A Personal View of the 4-S Conference

Peter Glasner
Polytechnic of North London

Arriving at the 4-S Conference by futuristic Metro-rail, being deposited in a concrete cavern which passes for a station, and decanted into a total environment from which there appeared, at first, to be no escape to the fresh air outside, set the scene for three of the more bizarre days of my academic life. The L'Enfant Plaza Hotel rises organically from a body of offices rooted in an underground cavern of shops and eating-places. It is rumoured that only a week previously, the hotel had hosted the announcement by a Republican of his presidential candidacy. Politics must be an expensive business. Eating and drinking at the L'Enfant Plaza certainly is for the British academic. At least there is a bar located in each

hotel room to provide succour for the weary traveller, especially after tracking from the television end to the bathroom end. Unfortunately two of the miniature bottles in the bar cost the same as a whole bottle in the off-license (bottle shop) located in one of the straggly roots of this sprawling utopia. I only know because I needed something to calm my shattered nerves after the fire alarm went off at 2:00 a.m. one morning and made me realise just how frightening a total environment can be.

The Conference unfortunately lived up to its setting. The focus was, of course, Science Policy; the locale was Washington, D.C.; the keynote speaker was from the American Academy of Science; and many of the participants were civil servants. Bernard Barber was elected President to succeed Dorothy Nelkin and both spoke to the assembled group after meals, whose quality seemed to vary in inverse relation with their cost. We were informed that the membership now extended to an impressive 23 countries; we also heard little or nothing more from outside the Americas for the rest of the Conference. We heard about the problems of being accepted as sociologists of science (again), but very little from the active researchers in the field about the results of their latest work. We heard (yet again) that science is a public issue seen as problematic by society at large, and we wondered at the lack of any real political dimension in the papers presented. We were told about the problems engendered by advances in modern technology, but saw little of its impact on the style or presentation of the papers, and only a few of us thought the hotel itself posed just those kinds of problems. In short we participated in building a mighty edifice of words within an almost medically sterilised environment which provided us with a conceptually cosy (but airconditioned) negative utopia, and shielded us as effectively as the physical surroundings from the fresh air of real science outside.

Once or twice the bells rang, but as in the world of the hotel, they were probably false alarms. Two philosophers from Canada began the task of laying out the philosophical relationships between the philosophy of science and science policy. Unfortunately they had to condense a 40 page paper into a 15 minute slot with the resultant lack of satisfaction by both speakers and audience. For the rest, there seemed to be a certain degree of self-congratulatory complacency represented by one group who insisted loudly that they were "the only ball game in town." However, another group was more critical, and one individual clearly resented "coming 430 miles and paying \$30 (did he get a cheap rate?) to hear good old . . . present a bunch of doggerel." He viewed the whole affair as sub-standard, and it certainly appeared that the degree of dissatisfaction seemed to vary directly with the distance travelled, if our Australian colleagues are to be believed.

In conclusion, the quality of papers, sessions, and presentation was much the same as any other international conference. Academics seem to have long lost the battle to make conferences interesting and useful, except at the level of renewing or making friendships. Certainly Koestler's "call girls" were much in evidence, en route from one International conference to another. What was most disturbing for the European-based sociologist of science at this conference was, on the one hand, the concern with problems which had been faced a decade before elsewhere (has no one heard of Habermas in the U.S.A.?), and, on the other hand, the stultifying effect of the intellectual and physical environment within which the conference took place. How is it possible for anything so artificial to produce knowledge about the real world of science without recognising the artificiality? Perhaps the focus and locale were wrong, given the current state of the social studies of science. Certainly I would not want to buy a used science policy from this conference.

Comments on the 4th Annual Meeting

Howard Gobstein
Beltsville, Maryland

A request that I comment on the 1979 meeting from a "policy perspective" prompted me to give considerable thought to how meetings might better integrate some of the diverse interests of 4S members. I wish to make some suggestions hoping that if they are good, they may be adopted, and if bad that they are criticized.

I recall the closing, rather heated discussion of the 1978 conference in Indiana. A few dozen people were spread across a large auditorium, arguing whether a policy perspective was taking over the Society. There was substantial concern that the 1979 meeting, especially because of its Washington, D.C. location, would be overwhelmed by some evil government pragmatism.

Well, I work in Washington, D.C. in some non-field referred to as science policy, and I attended panels of the 1979 meeting on some notion of "policy relevance." Had I written these comments within the week following the 1979 meeting, my message would have reflected my initial disappointment that the 1979 policy orientation did not go far enough. With some noteworthy exceptions (particularly Bill Carey's keynote address) I emerged from sessions with the questions--what does all this mean? How is this applicable to science policy?

My disappointment vanished within that first week after further thought on why I joined the Society, and what I think its purposes should be. I did not join this organization for its emphasis on policy analysis--that is done by other societies. In 4S I have the opportunity to reflect on more basic social science research on science. What do we know, what intrigues us about how and why science is done? This type of research, while so necessary for science policy (but unfortunately often left out of analysis) should not be driven by policy considerations. The future utility of this basic research could be minimized if research questions were narrowly framed to suit today's issues.

The Society should continue to encourage diversity in panel topics. We should include reports on social studies of science as well as social studies of the science policy process. Perhaps the best description of the objectives of our meetings should be akin to "multidisciplinary" as defined by the Rossini and Porter paper. That is when "the disciplinary components [individual projects in this case] are performed independently and joined externally through appropriate editorial linkages [as in meeting programs]." This is as opposed to an interdisciplinary meeting where components would be linked internally and substantively. I am not arguing that individual research projects should be of one discipline, but that all projects presented at a meeting should not have the singular objective of being policy relevant. As a whole, we should strive for the "patchwork quilt" of a multidisciplinary meeting rather than the "seamless woven garment" of interdisciplinary work (Rossini and Porter analogy).

A multidisciplinary approach to a meeting offers potential pitfalls, especially with next year's plans to meet jointly with other, more basic research oriented societies. How can the Society avoid the occurrence of a basic research vs. policy relevant schism? Obviously this is a simplified dichotomy but nonetheless describes different approaches to research.

The strength of the Society lies with the range of interests of its members which spans the basic questions of how science is done to the applied questions of how the government ought to treat science. The researchers and practitioners along this continuum of interests have much to contribute to one another's work. We should seek ways to provide opportunities at meetings for interested people to explore the links among the various kinds of research.

I have four related suggestions for future Society meetings pertaining to the above views. First, individual sessions should have more time for questions and discussions. I recognize that there are tradeoffs between presenting more papers vs. fewer papers in more depth vs. fewer papers with more discussion. Most panel sessions which I attended at this past meeting had about five minutes left for questions. This unfortunately does not leave sufficient time for the attendees to trade alternative perspectives. Second, when appropriate, panel organizers should consider including a discussant from an alternative perspective. For example, a policy oriented panel could benefit from a discussion of how the policy issues might be addressed or reframed using the findings from other research on science, and what this policy discussion suggests for further basic social science research. Third, recognizing that for some participants, there will never be sufficient time to discuss some issues in the scheduled session, round table discussions should be convened immediately following sessions to allow a continuation of debate. Rather than reconvening the next day when the momentum of the discussion is lost, round tables could be regularly available following each session. Finally, at the close of the meeting there should be either round table discussions or some other scheduled sessions to attempt to link various research findings together. These sessions should be somewhat structured to focus discussion. Perhaps rapporteurs could be selected at the beginning of the meeting to raise issues and questions at the closing session. An example of such questions from the 1979 meeting might be:

How do funding norms of agencies affect research in various fields? Johnson and Hufbauer mentioned in their panel presentation the concept of "ripeness" of a field. They described this as a function of the number of proposals and willingness of scholars to do research in a field. This suggests many questions such as:

- How is research funded in fields which are not yet ripe? How does this affect scientists' interest in working in these fields? What of the age distribution of scientists in ripe vs. non-ripe fields?
- How does ripeness relate to utility or relevance? Would more money in a particular field speed up the ripening process, particularly if there is a societal need for the research?
- If there is an increase in industry/university joint research projects would this have an effect on the ripening of fields for research? Would this create a broader range of research opportunities due to different concepts of funding of government and industry?

THE LOYAL OPPOSITION, THE COURT JESTER, AND OTHER CRITICAL IDEALS
--unaddressed issues at the 4S Annual Meeting, November 2-4

Lynn M. Lindholm
University of New Hampshire

The institution of the loyal opposition has a long and distinguished history in democratic societies. But, neither the idea nor the institution has a very respectable place in the history of intellectual conflict--at least not since Enlightenment radicals exposed them as mechanisms which destroyed intellectual integrity in the hands of scholastics and religious apologists. Ideas must be criticized, it seems, disloyally: without regard to the possibly destructive effect of the criticism on the idea or the intellectual system of which it is a part. But, we have learned a lot about both social theory and rational standards since the Enlightenment--particularly a lot about the weakness of radical ideals, so it is not clear to me that the Enlightenment demand that criticism be made without deference to an established framework fits intellectual life as we now understand it.

Nevertheless, when the editor of the newsletter asked me to review the annual meeting of the Society, the one complaint I wanted to voice most loudly was the complaint that this Enlightenment ideal of unrestricted criticism had been replaced--pervasively, without critical debate, and with extremely damaging results--by a framework which demands "loyal" opposition. That is, the question, should (or can) a responsible critic ever abandon the position of loyal opposition and criticize "destructively" is, I have found, very difficult and disturbing. Yet, on many levels and in many ways during the annual meeting, this difficult and disturbing question was presumed to be settled in the negative--in favor, that is, of the theory that responsible and meaningful criticism must come from within the established framework, from a loyal opposition.

The major invited addresses illustrated so well the unfortunate results of a loyal opposition policy that I plan to elaborate my complaint against it by comments on them alone. I mean no personal criticism or disrespect to the major speakers; they are the most visible and paradigmatic--not necessarily the most deserving--targets. Throughout the events I recorded, I felt the powerful influence of the view that events--even intellectual activities such as the delivery of an academic paper or address--serve significant social and professional functions distinct from any individual's intellectual purposes. But, can the social and professional functions be served if the intellectual purpose is not? Is all well when this happens? Although such questions are neither stated nor discussed, an implied "yes" answer was pervasive at the meetings as it is in the literature. I intend the criticism which follows to discourage this complacency.

William Carey, an editor of Science from the AAAS, gave a keynote address which recalled a political speech in its righteous but vague and indistinct appeals for academic scientists to criticize the scientific policymakers. A hollowness in these appeals was illustrated, it seemed to me, by the obvious but unaddressed disparity between the framework in which they were set and his audience: Leaving aside the call for harsh or determined criticism, many social scientists who study science would shrink from the idea of criticizing science, science policy making, or any behavior of scientists, at all--rather than describing or "observing" them in safe empirical style. The usual line is that social scientists--like good

historians but unlike philosophers--aim to show science "as it is" not "as it should be." Discussions by social scientists which show this concern with wider issues must, it seems to me, remain equally superficial until this conflict of basic aims is confronted more straightforwardly than it has been; such a confrontation, however, necessarily risks the established framework in social studies and so is not loyal opposition.

In his luncheon address, Bernard Barber declared himself to be examining the question of scientists' hostility to the social studies of science. One potential explanation of that hostility which was very noticeably absent from the agenda of the sociological investigation into the matter which Barber recommended was the theory that scientists considered the work of social scientists on science substandard and uninteresting. Thus, their hostility is the reasonable reaction to the rational (and perhaps even correct) conclusion that there had been considerable waste of effort, time and money. Why was this option missing? Does a sociological study of an enterprise eliminate or preclude the possibility that the actions within it may be the result of rational decisions? Barber's approach follows the established sociological tradition which institutionalizes this weakness: when one studies the social setting of an enterprise, the place of rational action based on rational deliberation becomes extremely ambiguous and mysterious. Is it replaced, precluded, included in a different framework? These questions, and the problems which arise from ignoring them, intensify into major faults when the social enterprise examined is an avowedly intellectual one--e.g. science. Barber gave a clear indication that within the framework of sociological investigation he proposed, one could not discover that intellectual, rather than sociological, forces explain the phenomena. Is this acceptable? Scientists--social observers like Barber will have observed--often joke about the tendency of social scientists to invent self-serving irrefutable research programs; perhaps an intellectual rather than a sociological examination of such jokes is appropriate. But, does the established sociological framework allow it?

Dorothy Nelkin's presidential address was graciously short, and it concerned the mingling of politics and science nowadays. It is hard to be more definite. For, as speakers on political issues tended to do, she sketched problems so loosely and developed discussions programmatically and indefinitely; speakers in later sessions also continually referred to the fact that research on recombinant DNA, pollution control, or nuclear energy had reached the public forum but without development of a problem situation around the cases. Nelkin said outright that science experts are used today on both sides of political and legal battles. Is this bad? She said no. Yet, if the tentativity in the results of science--which makes the fact that they are variously interpreted and debated not only unremarkable but necessary--was not the problem, it was not clear what was. Talking to initiates and to members of the loyal opposition--especially in an overly empirical tradition--has always discouraged the proper regard for problems and their tight formulation.

Sophisticated social scientists certainly do not have to readopt the radical view that true intellectuals need no frameworks in which to present their theories and their criticism. They can even argue with great persuasiveness that criticism set within an established framework is easier for all parties--including those who disagree with the framework--to understand and judge. Modern sociologists also have a valuable advantage over our Enlightenment predecessors in the insight that activities may have social functions that can not be reduced to or explained in terms of any individual's rational purposes. But if these advances are taken to

imply that responsible, fruitful criticism can only come from a loyal opposition which concedes the basic value of the criticized idea or framework, they bring a negative return.

Let me be clear. The framework which limits criticism to loyal opposition actively discourages attacks on the worst intellectual performances: if a performance is bad enough--particularly if its weaknesses reflect deficiencies in the scholarly tradition of which it is a part rather than personal inadequacies--any criticism threatens to be disloyal. Critics sensitive to the demand for intra-framework loyal opposition, abstain. They are particularly encouraged to abstain, I think, when they can sooth themselves in a muddled way with the notion that presentations are performing significant social functions even if they are not performing any noteworthy intellectual one. But, concerned readers should notice that once the poor performance is publically accepted, its sociological status may well change: From an innocuous if empty social ritual, the poor performance becomes part of the established intellectual tradition. As such it must be loyally acknowledged; colleagues (particularly young ones) are then pressured by editors and reviewers (in a sociological scenario well understood by this time) into setting criticism and new approaches in "loyal" terms that maintain continuity with the publically recorded presentations. Thus, intellectual standards sink. Intellectuals, even sociologists, replace the ideal of autonomous standards for intellectual work with the institution of the loyal opposition at their peril.

ANNOUNCEMENTS

Obituary

It is with deep regret that we report the untimely passing of 4S member Tyler Thompson who died September 21, 1979. According to Tyler's wishes, anyone who cares to make a contribution in his memory may send it to:

Tyler Thompson Research Fund
Sloan-Kettering Cancer Center
Box E, 1275 York Avenue
New York, New York 10021

Addendum

In Vol 4, No. 2 of the Newsletter we announced the publication of Countermovements in the Sciences edited by Helga Nowotny and Hilary Rose, but neglected to mention the publisher, which is: D. Reidel, Dordrecht - Holland.

Call for Papers

COMPARATIVE PERSPECTIVES ON SCIENCE POLICIES. If you have written, or would like to write, a paper on the formulation and implementation of science policies in at least two countries, please contact Professor Linda L. Lubrano, School of International Service, American University, Washington, D.C. 20016. She is interested in identifying potential contributors to a book on comparative science policies. The central focus of the book will be on the participation of natural scientists in the formation of national policies that affect areas of fundamental research. It will also compare the roles of scientists in the administration of science policies. Each contributor would be expected to present an integrated comparison of policymaking for science in at least two countries.

A New Newsletter

The newsletter Parex Informations, initially set up by the Parex organization (Parex is a conflation of Paris-Sussex), is undergoing marked revamping and extension. The intention is to provide "a forum for the lively international exchange of opinions in the European Sociology of Science." As such the newsletter will publish information about developments in the history and social studies of science in Europe; items include: details of research projects and thesis work; book reviews and bibliographies; meeting and colloquium reports; news of research funding.

In its revised format, it is hoped that Parex Informations will achieve the same kind of success in Europe that the 4S Newsletter enjoys in North America.

The editors of both newsletters are agreed that inter-newsletter exchange can only work to the general good, so it is hoped that future newsletters will carry excerpts from the other newsletter. Readers who would prefer unexpurgated versions of the European newsletters are encouraged to contact the editor direct:

Georg Kamphausen

Editor

Parex Informations

Hauptstrasse 68

4800 Bielefeld 14

West Germany

Recent Meeting

The Social Process of Scientific Investigation -- Steve Woolgar, Department of Sociology, McGill University, Montreal

A small weekend meeting, organized by Roger Krohn and Steve Woolgar, was held at the Department of Sociology, McGill University, Montreal, 19th-21st October 1979. Papers were presented by sociologists, historians and psychologists with a common interest in focussing on the fine details of scientific work. In the course of two days useful discussion, three main points of importance emerged. Firstly-- and perhaps not unrelated to the unusual interdisciplinary composition of the audience--there was a feeling that the range of factors and categories traditionally

employed by sociologists to explain the work of science was overly restricted: explanations in the social studies of science should additionally consider, for example, psychological and historical variables. More significant, perhaps, was the suggestion that in our attempts to come to terms with science at the level of practice we should give much more weight to the use of such variables, models and other concepts by the scientists themselves. Secondly, the importance of in situ, contemporaneous study of scientific work was reaffirmed. Participants at the meeting represented the recent completion or ongoing conduct of no fewer than five separate studies involving what might be broadly termed participant observation of laboratory practice (Knorr, Lynch, Latour and Woolgar, McKegney, Zenzen and Restivo). The paper by Michael Lynch contained a powerful overview of the assumptions and perspectives embodied by these recent studies, as well as pointing to some new directions and issues to which this kind of study of scientific practice should now address itself. Thirdly, the close involvement in these studies of observers with their subjects generates a reflexive awareness which might be fruitfully exploited as an analytical resource. For example, our understanding of scientific practice may benefit from the explicit comparison between the strategies and techniques adopted by the social researcher and those employed by scientists. Or--as exemplified in the paper by Zenzen and Restivo--the relationship between observer and scientists in participant observation, and the dynamic influence of this relationship both on the observer's report, and on the scientific research in progress, may provide important clues for our further understanding of science.

The papers at this meeting, together with those presented at a meeting in Bielefeld in June, are destined for probable inclusion in Sociology of the Sciences Yearbook (Volume 5) edited by R. Krohn, K. Knorr and R. Whitley and to be published by Reidel in January 1981.

Papers given: Dr. Edward J. Hackett
(Center for Applied Social Science
Boston University
270 Bay State Road
Boston, Mass. 02215)

"Discovery and Problem Formulation in Friend Virus Research"

Dr. David Faust
(Division of Child and Adolescent Psychology
University of Minnesota, Mayo Memorial Building,
420 Delaware Street S.E.
Minneapolis, Minn. 55455)

"A Cognitive Model of External Factors and Scientific Beliefs"

Dr. Michael Zenzen & Dr. Sal Restivo
(Human Dimensions of Science Center
Rensselaer Polytechnic
Troy, New York 12181)

"The Mysterious Morphology of Immiscible Liquids: The Discovery and Pursuit of an Anomaly in Colloid Chemistry"

Dr. David Galaty
(Department of Social Sciences
University of Wisconsin
Green Bay, Wisconsin 54302)

"Reductionist Physiology and the Students of J. Mueller"

Prof. Howard Gruber
(Institute for Cognitive Studies
Rutgers University
Newark, New Jersey 17102)

"The Evolving Systems Approach to Creative Scientific Work:
Networks of Enterprise"

Dr. Michael Hammond
(Department of History
University of Toronto
Toronto, Ontario)

"The Sacrifice of the Neanderthals in French Paleontology"

Dr. Karin Knorr
(University of Pennsylvania
Faculty of Arts & Sciences
Department of Sociology
3718 Locus Walk CR
Philadelphia, Pennsylvania 19174)

"The Scientist as Analogical Reasoner on the Conservatism of
Scientific Innovation"

Dr. Michael Lynch
(Centre of Criminology
University of Toronto
John P. Roberts Research Library
130 St. George Street
Toronto M5S 1A5)

"A Study of Technical Work in a Brain Research Laboratory"

Mr. Doug McKegney
(Department of Communication
Simon Fraser University
Burnaby, B.C., V5A 1S6)

"The Relationship between Formal and Informal Communication in
Scientific Research"

Secretary/Treasurer's Report for 1979

A. Minutes of 4S Council Meeting, Washington, D.C., Nov. 2, 1977.

Present: Council members--J. Gaston, L. Hargens, E. Layton, I. Mitroff, D. Nelkin, E. Skolnikoff, A. Thackray, P. Woolf. Council members elect--B. Barber (President-elect), D. Chubin, B. Schroeder-Gudehus, H. Small, A. Teich

I. President Nelkin called the meeting to order at 4:00 p.m. She outlined some of the main activities which occurred during 1979, including a successful membership drive, and announced that the number of the preregistrations for the 1979 annual meetings was larger than for any previous 4S Convention.

II. The council discussed the possible publication of selected papers from the 1979 4S meetings in a "yearbook" volume organized around one or more of the themes of the convention. It was resolved that:

The President (Nelkin) and Organizer of Conference Sessions (Teich) shall explore the possibility of publishing a volume containing selected proceedings of the 1979 4S meetings. They shall also be empowered to take whatever steps they deem necessary for preparing such a volume--such as appointing an editorial board, etc.

The motion was approved. (see also item X)

III. The council discussed the question of increasing 4S membership dues to maintain the society's real income. It was resolved that:

Beginning in 1980:

Yearly professional dues be increased to 15 dollars.
Yearly institutional dues be increased to 25 dollars.
Yearly student dues be maintained at 5 dollars.

The motion was approved.

IV. Various means of increasing the participation of the Society's European members were discussed; including holding joint meetings with affiliated groups in Europe, the inclusion of more European news in the 4S Newsletter, and the publication of 4S news in Scientometrics. It was resolved that:

4S shall begin to find representatives of various groups in Europe who will be correspondents for the 4S Newsletter and who will build networks to expand European membership and encourage further European participation in annual meetings.

The motion was approved.

- V. The possibility of obtaining support for 4S activities from various foundations, and multinational corporations with interests in the work of 4S members was discussed. It was resolved that:

Ian Mitroff be appointed to head a committee charged with exploring such arrangements.

The motion was approved.

- VI. Further discussion of sources of support beyond existing membership dues lead to the resolution that:

A new category of membership, "supporting member", be created for benefactors who contribute \$100 or more per year to the 4S.

The motion was approved.

- VII. The council discussed the status of the 4S affiliation with AAAS. Ian Mitroff ends his term as the 4S liason to AAAS as of January, 1980. It was resolved that:

Daryl Chubin be appointed as the new liason with AAAS, and that the 4S play a more active role as an affiliate of AAAS (such as sponsoring sessions of future AAAS meetings).

The motion was approved.

- VIII. The Secretary-Treasurer reported on 4S membership and financial status as of Nov. 1, 1979. (see below)

- IX. The Committee on Future Meetings (P. Woolf, Chair) reported on current arrangements for the 1980 4S meeting to be held in Toronto, October 16-19 in conjunction with the meetings of the Society for History of Technology, the History of Science Society, and the Philosophy of Science Association. In addition, the Committee proposed that the 1981 meeting be held in Atlanta. It was resolved that:

The 1981 meeting be held in Atlanta, Georgia, with Daryl Chubin serving as head of the Local Arrangements Committee.

This motion was approved.

- X. The Council further considered arrangements for possible publication of a volume containing selected papers from the 1979 4S meeting. It was resolved that:

The Society provide up to \$1000 for possible costs associated with the editing and typing of the manuscript, and other incidental expenses. Furthermore, that the editors seek in their negotiations with publishers to have all royalties go to 4S, and to obtain a preferential price for 4S members.

The motion was approved.

The meeting adjourned at approximately 7:00 p.m.

B. Minutes of the 4S Business Meeting, Washington, D.C., Nov. 2, 1979.

- I. President Nelkin called the meeting to order at 12:45 p.m. She announced the resolutions of the Council meeting of Nov. 2.
- II. The Secretary-Treasurer reported on 4S membership and finances as of Nov. 1, 1979.
- III. The motion was made and seconded that the Society extend its deepest appreciation to Linda Lubrano and Albert Teich for their excellent arrangements of a fine meeting. The motion was passed by acclamation.
- IV. The motion was made and seconded that the Society thank its President of 1978-79 for her valuable service and energetic performance of her role. The motion was passed by acclamation.

There being no further business from the floor, the meeting adjourned at 1:10 p.m.

C. Results of Election of New 4S Officers, August 31, 1979.

President elect--Bernard Barber

Council members elect--Daryl E. Chubin
Brigitte Schroeder-Gudehus
Henry Small
Albert Teich

D. 4S Membership Data for 1979

<u>Domestic</u>	<u>Number as of 12/31/78</u>	<u>Number as of 12/31/79</u>	<u>% 1978 Members Not Renewing</u>	<u>% New Members</u>
Professional	287	323	29%	41%
Student	61	44	30%	2%
Institutional	16	19	6%	31%
<u>Foreign (including Canadian)</u>				
Professional	111	94	25%	10%
Student	9	9	0%	0%
Institutional	12	14	17%	33%
TOTAL	496	503		

E. Budget Report for 1979

<u>EXPENSES</u>	<u>AMOUNT BUDGETED</u>	<u>EXPENSES</u>	<u>BALANCE</u>
Newsletter	3,400	3,980.00*	-580.00
Annual Meeting & Membership Drive	500	4,210.44	-3710.44
Duplicating and Postage	600	250.00*	350.00
Secretarial	1,000	99.57	900.43
Telephone	100	-----	100.00
Miscellaneous	<u>200</u>	<u>-----</u>	<u>200.00</u>
TOTAL	5,800	8,540.01	-2,740.01

*Amount Projected from Current Expenditure

<u>INCOME</u>	<u>EXPECTED INCOME</u>	<u>ACTUAL INCOME</u>
Membership Renewals	4,000	3,895.00
New Memberships	500	1,485.00
Sale of Mailing Labels	-----	362.60
Annual Meeting	-----	6,616.00
Balance from 1978	<u>1,800</u>	<u>2,018.83</u>
TOTAL	6,300	14,377.43

INCOME	14,377.43
EXPENSES	<u>8,540.01</u>
BALANCE	5,837.42

4S Society for Social Studies of Science

Vol. 17

Volume 4 Number 4

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Editorial Assistant: Beverly Morber

EXPENSE	UNITED STATES	AMOUNT	BALANCE
Postage	100.00	100.00	100.00
Printing and Stationery	100.00	100.00	100.00
Telephone	100.00	100.00	100.00
Miscellaneous	100.00	100.00	100.00
TOTAL	400.00	400.00	400.00

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