

4S Society for Social Studies of Science

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4S: PROGRESS AND PROSPECTS

Presidential Talk

Scientists and the Social Study of Science: A Research Problem*

by Bernard Barber

I start with a personal problem: During some forty years of interest and work in the social study of science I have often felt dismay and even anger at the widespread ignorance among natural scientists about this field, their seeming denial that in principle it is even possible, their arrogant assertion of all kinds of sociological, political, and psychological generalizations about science without any awareness of the limitations of their impressions, their prejudices, and their common sense knowledge.

Let me try today to turn my personal problem into a sociological problem. Can we move toward defining some research in which not only the beliefs and attitudes of scientists toward the social study of science are systematically determined but also some of the social and cultural sources of those attitudes? I think we can, and as a step along the way, let me offer some impressionistic evidence and some preliminary hypotheses about why there is so much ignorance, negativism, and ambivalence among natural scientists toward the social study of science. Survey research, intensive interviews, and documentary sources could all be used for collecting data.

My impressionistic evidence is not representative of the views of all scientists. I have taken materials from scientists who might be called "the best and the brightest" of their kind, men who are perhaps of special concern for us just because of their articulate and forceful ideologies on this matter. I start with C.P. Snow [The Two Cultures and The Scientific Revolution, Cambridge University Press, 1959], whose views started a universal and heated debate. "I believe," said Snow, "the intellectual life of the whole of western society is increasingly being split into two polar groups. . . . Literary intellectuals at one pole--at the other, scientists, and as the most representative, the physical scientists." By leaving out the social scientists altogether, Snow nullified the possibility that the social study not only of science but of other social phenomena might exist.

From Snow we move to more recent times and to a more ambivalent scientist, John Ziman, a distinguished physicist and scientific administrator [Reliable Knowledge: An Exploration of the Grounds for Belief in Science, Cambridge University Press, 1978]. Ziman is intimately familiar with a considerable amount of the good work in the history, philosophy, and sociology of science. He seems to accept a good deal of it. Yet there is a deep ambivalence in his final views, if we may judge from his conclusions on the last four pages of his book (pp. 183-86):

(183-4) "In this search for 'reliable knowledge' concerning human behavior we thus arrive at a rather disappointing conclusion. There are grave obstacles of categorical vagueness . . . , operational variation . . . , experimental irrelevance. . . , theoretical unprovability . . . and cultural relativism in the way of establishing a general 'science of society' This is not to deny the value of sociological and anthropological research, which uncovers such

*This is a slight re-working of an informal and exploratory talk given at the luncheon of the 4-S Annual Meeting in Washington, D.C., 1979.

richness and variety, such poverty and uniformity, in the lives of all human beings. In default of such research, we should live entirely at the mercy of our own follies, misconceptions and deceptions as to the merest facts of the matter."

(184) ". . . Social commonsense is a very satisfactory guide to social action: 'Everyone is, in a certain sense, a fairly competent social scientist. . . \'" (This last is a citation from R. Harre, Reconstructing Social Psychology.)

(185) ". . . What we live by in social life is not a series of scientific laws, but maxims, whose lack of consistency and consensuality is irrelevant to their practical values."

(185) ". . . The novelist, with his sensible ear and discriminating eye, articulates the universal elements in our emotional lives, and teaches us more about mankind than any formal theory."

Finally, (185) ". . . we cannot learn the art of research by reference to the formal philosophies, sociologies and psychologies of science. . . . We read our Popper and our Kuhn, our Merton and our Polanyi, not for rules and laws and formulae and proofs, but for maxims and insights and understanding. We also turn to the makers themselves---to Newton and Faraday, to Poincare and Einstein, to Darwin and Pasteur, to Freud and Jung, and Durkheim and Piaget, to rediscover the significance of their contributions to the world they have helped create."

This is at best a deep ambivalence, at worst a forbidding negativism about the worth of theoretically derived and empirically tested social study of science.

Freeman Dyson is another distinguished and eloquent physicist. His book [Disturbing the Universe, Harper & Row, 1979] is the first of a series sponsored by the Sloan Foundation as part of its long-standing program to encourage "the public understanding of science." Although the "Preface to the Series" says that ". . . an understanding of the scientific enterprise, as distinct from the data and concepts and theories of science itself, is certainly within the grasp of us all," it gives not a single reference to any social science expertise in the field. As for Dyson himself, while he seems to admit the existence of the social sciences in the following statement, he himself will not have any of them. He says, "My colleagues in the social sciences talk a great deal about methodology. I prefer to call it style. The methodology of this book is literary rather than analytical. For insight into human affairs, I turn to stories and poems rather than to sociology." And, continuing, Dyson rightly recognizes the sources of his preference. "This is the result of my upbringing and background. I am not able to make use of the wisdom of the sociologists because I do not speak their language." But is this language something a man like Dyson couldn't master with not the greatest of efforts? It is, rather, that his heavily classical and literary education in England has always seemed sufficient to him and he felt no compelling reason to take the small effort to read in the works of the social scientists of science.

Part of Dyson's preference may be due to the strong individualistic cast of his view of the world. "To understand the nature of science and its interaction with society," he says, "one must examine the individual scientist and how he confronts the world around him . . . my individualistic bias leads me to listen to poets more than to economists." We see that Dyson is himself aware, as many scientists are not, of his individualistic theory about social institutions and social action. Another assumption of Dyson's that may contribute to his uneasiness with the social

study of science is his unpredictability assumption. "Science and technology," he says, "like all original creations of the human spirit, are unpredictable." This, of course, is an assumption that the social study of science cannot accept.

Looking at one last distinguished scientist, P.B. Medawar [Advice to a Young Scientist, Harper & Row, 1979], we find him not only aware of social science but willing to admit that it might improve upon his "mere" opinion. He says, "I use the word opinion to make it clear that my judgments are not validated by systematic sociological research and are not hypotheses that have already stood up to critical assaults. They are merely personal judgments, though I hope that some of them will be picked up by sociologists of science for proper investigation." We may hope that Medawar represents the wave of the future among scientists in their view of the social study of science.

Now let us look at a series of preliminary hypotheses about why it is that we see so much negativism among scientists about the social study of science. The factor of their socialization and early training, mentioned by Dyson, suggests one hypothesis to be tested by research. This is that scientists are likely, if they are trained at all outside of science itself, to have studied the humanities rather than the social sciences, and especially not the social studies of science, which are still not taught widely or well at most colleges and universities. Very few scientists have had any work as undergraduates in social science, probably not even economics. And there has been no encouragement from their mentors to study social science at some more advanced level. Research should investigate whether generational differences are occurring. Are younger scientists more receptive to the social study of science?

Second, we may offer the hypothesis that the strong values of individualism and autonomy in science lead to resistance to the sociological premise of a considerable measure of social determinism in the creation of scientific knowledge, in the scientific reward system, and in the social organization of science. Do the social explanations of science seem to undermine the values and commitment of scientists as they go about their everyday tasks? This does not seem to happen to the social scientists of science as they go about their researches.

Third, is it a correct hypothesis that all powerful groups tend to resist objective scrutiny by outsiders? Are we all aware that there are dangers in exposing the fact that our values are not fully realized, that much of our performance is routine, that there is even some dishonored deviance? The actual working of institutions and roles is always somewhat different, for specifiable social reasons, from what the official public values and statements of the institution hold it to be. All practicing insiders know about such things, but public visibility is seen as dangerous to the prestige and the interests of the group. Is this one of the reasons why natural scientists are averse to the objective scrutiny of the social scientists of science?

Fourth, do the views of natural scientists on the social study of science come from an inadequate understanding of science? Do scientists hold the simplistic view that "a scientist" can understand anything just because he is "a scientist"? Do they feel that he needs no special knowledge that he can't work up himself and especially for social and political problems? Do natural scientists understand the functions of different technical conceptual schemes for different aspects of reality?

As these hypotheses, and others, are explored and tested, it will be well to explore exceptions to the patterns of ignorance, indifference, and ambivalence I have reported. "Deviant case analysis" can be used in place of controlled experiment, as "natural experiment," as a basis for comparative proof of the social

sources of the predominant patterns. How, for example, did Thomas Kuhn come to recognize explicitly that his theory of scientific change and revolution was sociological as well as philosophical and historical? How did David Edge, who was a practicing radio astronomer, come to look favorably on the social study of science and come to collaborate with Michael Mulkay?

The future progress of the social studies of science will be easier, I think, if there can be more cooperation between natural and social scientists. As we try to understand how to promote such cooperation, we may be able to learn from past failures the conditions under which cooperation does not and does occur. For that reason, research into the attitudes of scientists toward the social study of science looks attractive for its own sake and as a guide to more effective cooperation.

FIFTH ANNUAL MEETING

A reminder to the membership that the 4S annual meeting will be held in Toronto on October 17-19, 1980, in conjunction with three other societies: History of Science Society, Philosophy of Science Association, and Society for History of Technology. In addition to several joint sessions, 4S will sponsor 9 to 12 sessions of its own. Joint sessions tentatively agreed upon are: (1) Science, Technology and Human Values; (2) Women in Science and Technology. Tentatively scheduled 4S sessions include:

- (1) Social Studies of Mathematics
- (2) Social Studies of Astronomy
- (3) Comparative Analyses of Scientific and Technological Controversies
- (4) Social Influences on Research Careers
- (5) Ethnographic Studies of Science
- (6) Public Understanding of Science and Technology
- (7) Quantitative Studies of Science
- (8) Policy Studies

Questions about the program may be addressed to:

Pat Woolf
Department of Sociology
Princeton University
Princeton, New Jersey 08540
Telephone: (609) 452-4541

Dan Sullivan
Dean of Academic Development and Planning
Carleton College
Northfield, Minnesota 55057
Telephone: (507) 663-4301

Questions about local arrangements may be addressed to:

Michael Hammond
Sociology Department
University of Toronto
Toronto, Ontario M5S 1A1
Telephone: (416) 284-3193

The final program will be published in the next issue of the Newsletter.

"Science and Technology Studies: Toronto-80"

Pre-Registration Information for the First Joint Meeting

Plans are well advanced for the October 17-19, first joint meeting of the History of Science Society, Philosophy of Science Association, Society for the History of Technology, and Society for the Social Studies of Science. We urge you to use the enclosed pre-registration form as it will aid our planning and ensure a smoother operation of the meeting. Please note that all societies have common fees. Everyone should send the form to Prof. Peter Asquith, PSA, 18 Morrill Hall, Michigan State University, East Lansing, MI 48824.

Since there is a difference in US and Canadian dollars, we have quoted the fees in both figures and they may be remitted by cheque in either funds.

Members of two or more societies should check all appropriate boxes. The reception on Friday evening, October 17, is open to all registrants. Your guests are welcome, but there is a fee for each guest ticket (payable either with pre-registration or at the Registration Table at the Meeting). The three society luncheons are each open to all, whether members or not, but tickets must be purchased in advance. No luncheon tickets will be sold on-site.

PSA's Contributed Papers are pre-printed for discussion during the Meeting. This booklet is available to all registrants of the four societies. Pre-registrants desiring a copy should mark the box on the attached form. (Others may purchase the volume directly from Prof. Asquith.)

This pre-registration form does not reserve a hotel room. If you have not already booked a room, you should write directly to the hotel as early as possible. (See information elsewhere in this Newsletter.)

The on-site registration fee for those not pre-registered will be \$5 higher. Further information about arrival in Toronto will be published in the next Newsletter. We look forward to greeting you personally.

Local Arrangements Committee

Peter D. Asquith (PSA)
Michael Hammond (4S)
Richard Jarrell (HSS)
Bruce Sinclair (SHOT)

PRE-REGISTRATION FOR JOINT MEETING

"Science and Technology Studies: Toronto-80"

17-19 October 1980

NAME: _____

ADDRESS: _____

Society Membership(s): HSS () PSA () SHOT () 4S ()

If you wish the PSA Pre-prints mailed to you in advance, check here ().

PRE-REGISTRATION FEES (On-site registration in any category will be \$5 higher.)

	<u>U.S.</u>		<u>Canadian</u>	
Member or on program	\$15	or	\$17	_____
Students/Seniors/Unemployed	10	or	12	_____
Non-members	25	or	28	_____
# reception guest tickets ____ @	5	or	5.75	_____
Luncheon tickets: SHOT, 17 Oct.	13.50	or	15	_____
HSS, 18 Oct.	13.50	or	15	_____
4S, 18 Oct.	13.50	or	15	_____
Total Remitted				_____

Please send cheque (in either US or Canadian funds) payable to PSA,
along with this form to:

Professor Peter D. Asquith, PSA
18 Morrill Hall
Michigan State University
East Lansing, MI 48824

PRE-REGISTRATION CLOSING 15 SEPTEMBER 1980

PLEASE SEE NEXT PAGE FOR HOTEL INFORMATION.

"Science and Technology Studies: Toronto-80"

HOTEL INFORMATION

Headquarters hotel is the Park Plaza, adjacent to the University of Toronto campus. The first 250 room reservations will secure rooms at the headquarters hotel; subsequent reservation requests will be assigned to the Park Plaza II (two subway stops east). Room rates for either hotel are \$40 per night single or double. Somewhat less expensive rooms are available at the Bond Place Hotel about ten minutes away from campus by subway. Rates at the Bond Place are \$26 per night/single or \$30 per night/double. THESE RATES ARE QUOTED IN CANADIAN DOLLARS (APPROXIMATELY 15% LESS IN U.S. FUNDS).

Sessions begin at 9:00 A.M. Friday 17 October and run through part of Sunday 19 October. You may use the attached form for reservations at either hotel. Please note that hotels will not match roommates; you should thus include on this form the name of anyone with whom you wish to share a room. Mail directly to your hotel.

Reservations Manager
Park Plaza Hotel
4 Avenue Road
Toronto, Canada
M5R 2E8

Reservations Manager
Bond Place Hotel
65 Dundas St. East
Toronto, Canada
M5B 2G8

HOTEL RESERVATION FORM

Science and Technology: Toronto-80 (HSS, PSA, SHOT, SSSS)

Please reserve: ☐ single bedroom
☐ twin bedroom for persons
☐ suite

FOR ARRIVAL/ Day Date a.m.
p.m.

(We regret we can hold reservations only until 6 p.m. unless otherwise guaranteed in advance.)

PROBABLE DEPARTURE/ Day Date

NAME Credit Card No.

ADDRESS

CITY OR TOWN Prov. or State

Since my reservation is booked for arrival after 6 p.m. I guarantee payment for the first night should I not arrive and fail to cancel
(initials)

Important Items About Toronto Meetings

A Note to Authors

- An extensive publishers' book display is planned for the Toronto-80 Joint Meeting of HSS, PSA, SHOT, and 4S. A book exhibit not only allows participants to review new and previously available titles, but the publishers' fees also provide added financial support for the Meeting. Thus, for several reasons, it is desirable that as many publishers as possible participate in the Meeting next fall, "Science and Technology Studies: Toronto-80," at the Park Plaza Hotel, October 17-19.

The Local Arrangements Committee itself directly solicits publishers' participation in a variety of forms. They may purchase advertisements in the program, rent display tables, or send individual books for exhibit. Direct solicitation, however, is not as effective as an individual author or editor requesting that his or her book be displayed or advertised at the Meeting. Please take the time to request your publisher to contact Professor Michael Hammond, IHPST, University of Toronto, Toronto, Ontario M5S 1A1 [(416) 284-3193]], to find out how they may make use of this Meeting to advertise your publication. Doing so could lead to increased exposure for your work, a better exhibit at the Meeting, and increased financial support for the societies.

Daycare at the Toronto Meetings

The Local Arrangements Committee for the October Meeting would like to provide high-quality, drop-in daycare on or near the University of Toronto campus if there is sufficient demand for it. This service would be provided on Friday, 17 October, and Saturday, 18 October, during the daytime meeting sessions and lunch hours. There would be an hourly charge for this service.

If there is sufficient demand, full details will be published in the next newsletters of the four societies or by direct mailings to those interested. If you are interested, please send the following information to Professor Bert S. Hall, Institute for History and Philosophy of Science and Technology, University of Toronto, Toronto, Canada M5S 1A1, as soon as possible, but no later than August 1: Your name, address, and telephone; number and ages of the children.

BOOK REVIEW

Bruno Latour and Steve Woolgar, Laboratory Life: The Social Construction of Scientific Facts. Sage Library of Social Research, v. 80. Beverly Hills: Sage Publications, 1979, 272 pages. \$7.95

Bruno Latour, a French philosopher, and Steve Woolgar, a British sociologist, present in Laboratory Life an account of how scientific statements are developed in individual laboratories and accepted as fact by scientists beyond that particular laboratory. Working from the data of Latour's two years' observation of daily activity in the Salk Institute for Biological Studies, the two authors explore how laboratory activities are transformed into published statements through what they call a process of "literary inscription," and then how those statements gain "credit" to emerge as accepted fact out of the many contending statements proposed in the literature of the field. The authors also rely on historical case material concerning the isolation and identification of the chemical structure of Thyrotropin Releasing Factor (TRF) during the 1960's; through this case study they show how TRF only takes on "solidity"--recognizable and accepted identity as a substance--in consequence of statements about the substance being accepted as fact.

The questions Latour and Woolgar pursue hold broad interest; their data are suggestive; many of their observations are perceptive; and some of the analysis is promising. But commitment to a strong epistemological position leads them to construct an unnecessarily comprehensive theoretical scheme, a line of argument that obscures important questions, and an attempt to make the data and analyses say more than they do. Despite frequent denials, Latour and Woolgar speak from a radically subjectivist position which, although not necessary for most of their substantive observations, leads them in their framing argument to make a basic error of conflating fact with the statement of fact. As will be discussed below, their desire to show that fact is only a construct of language and exists only in the scientists' adherence to the statement leads the authors to force the argument at several key points.

Latour and Woolgar begin with what they call an anthropological approach, as they describe the strange activities of the tribe residing during working hours in the Salk Institute. But after they identify the aim of the lab's activity and social structure as the production of written documents, their attention, argument, and methodology turns to language. The data are largely objects of language; the analyses are primarily linguistic; and much of the theory derives from the French blend of philosophy and literary analysis, currently localized in the field of semiotics.

The opening anthropological approach calls attention to several important, but taken for granted, features of laboratory structure. The laboratory is divided into two almost equal areas: in one part are apparatus and people in white coats working with the apparatus; in the other part are books, other written materials, the instruments of language (e.g., typewriters and telephones), and people in office dress working with words. Within the apparatus section, physical materials are labelled, measured, and otherwise transformed into signs; for example, extracts are taken from designated rats, labelled, and then placed in a machine which produces a set of figures. These figures are fed into a computer that produces another set of figures, which is converted into a graph. The graph is then delivered to the office section for further transformation into an article. In this process of literary inscription only the most recent symbolic product is attended to at each stage; all the earlier activities, inscriptions, and transformations seem to be forgotten.

This interpretation of laboratory structure and events establishes the research problems of the remainder of the book. Analysis of the variety of statements produced by the laboratory leads to an examination of how statements of contending laboratories are juxtaposed in the literature until a single claim emerges as a solid fact. In the TRF case study articles are shown to be shaped by the discussion to that point and to shape the possibilities for future statement. The questions the articles attempt to answer, the criteria of judgement for new evidence, and even the nature of the substance being looked at change through time as the literature shapes itself in the emergent field. In this competitively--or in the authors' more precise word, "agonistically"--structured field, scientists make statements strategically to advance their own claims, to undermine, or "deconstruct," the oppositions' claims, and even to eliminate or disqualify certain opponents from the field altogether.

A closer look at the microprocesses of laboratory talk follows, for in that talk decisions are made that determine what goes into the article to be published. An analysis of conversation reveals that the talk anticipates the reaction of an audience within the agonistically structured field. Collaborators discuss what they can get away with stating, given the current facts, criteria and competition in their field. Finally Latour and Woolgar set forth an economic model of how scientists rise to positions where they may indeed produce statements that will be taken seriously, literary credibility being the prime unit of exchange.

The substantive program of the book seems to me a fruitful one. As a teacher and student of writing I have become aware that linguistic (or symbolic) choices transform content, that the process of statement creation influences the statement, that anticipation of audience reaction is a key feature of writing, that prior statements help establish the occasion for the new statement, and that a document once written becomes a feature of the landscape. Although conventions of scientific discourse may attempt to limit some of the factors at play in writing, scientific writing cannot be free of the mechanisms of all language and language production. Thus writing, both as a process and a product, should be treated as problematic, and the archive of science should not be viewed as a transparent conveyor of non-symbolic fact. Understanding the what and how of scientific writing could significantly influence what we understand science to be.

And a revised understanding of science is the aim of Latour and Woolgar. They prepare their vision of science in the polemical opening chapter, and they draw it together in the comprehensive scheme of the last chapter. In order to evaluate that frame, however, we need to become clear on what they accomplish in the middle. There they have done less than they suggest. Claims are not established firmly and precisely, and later claims do not clearly derive from earlier claims. Although the authors offer a wealth of evidence, that evidence is not coherently enough drawn together to support the broad and radical conclusions drawn from it.

An intuitive indicator that the argument is not under complete control is the almost constant reference backward and forward to other parts of the book, as though the authors were still mulling over the meaning of their evidence and claims, trying out different lights under which to view their material. They frequently try to draw their argument together in passing summaries, but each time the sum of the argument comes out differently (see pages 40-41, 105, 151, 187, 217, 235 and 236-240). Such casting about for meaning and shape is a necessary exploratory procedure and reveals many interesting insights, but it is not the firm basis for strong conclusions.

The kinds of imprecision that prevent the argument from gaining clear shape are evident in the two analyses of written statements at the end of the second chapter. First the authors categorize the publications of the laboratory according to what they claim to be genre, but actually the categories come from audience analysis: lay audiences, scientists outside the field, specialists not current with latest findings, and insiders (pp. 72-73). Although relationships may exist between genre and audience, they are far from the same; more importantly, analysis of each gives rise to a different set of questions. Genre refers to the conventional forms of language and gives rise to questions concerning the role form takes in sorting out and shaping content. Analysis of genre goes directly to the heart of the issue of what is knowable given our forms of expression--an epistemological question that should be important to Latour and Woolgar, given their larger argument. Analysis of audience leads to the interesting, but lesser, question of how the needs and knowledge of readers influence the presentation and appearance of content, leading to observations of how knowledge is distorted by the pragmatics of communicating with less core audiences. This last they explore lightly.¹

After three pages they drop their first analysis to consider what they call "statement types." They present a five point scale of how solid or conditional a statement seems to be. This analysis is based on the shrewd observation that the most persuasive statements are those that do not seem to persuade, but which rather rise above the momentary context of statement to appear as the transparent conveyor of fact. "A text or statement can thus be read as 'containing' or 'being about a fact' when readers are sufficiently convinced that there is no debate about it and the processes of literary inscription are forgotten" (p. 76). Calling attention to the process of inscription, such as mentioning the particularities of a lab or the context of debate in which the statements is made, tend to undermine the statement's claim to general truth. Thus Latour and Woolgar define the objective of laboratory activity as "to persuade colleagues that they should drop all modalities used in relation to a particular assertion and that they should accept and borrow this assertion as an established matter of fact" (p. 81). They then give an account of how some laboratory activities serve this end.

The argument and examples of transformation of statement types are enlightening, but again imprecision makes it appear that more has been accomplished than actually has been. First these types are presented only as the results of an impressionistic examination; no systematic investigation is given to indicate that the typology actually distinguishes texts from each other. Further it is not even clear from the discussion whether these categories are meant to reflect a consistent linguistic practice, or they are meant as ways scientists consider other people's work, perhaps reflected in talk, or they are only a plausible heuristic to help explain some events. Second, once the categories are presented they are used only loosely in the following chapters. The TRF case study, which follows immediately after, although presenting many insights of many kinds, is not a rigorous demonstration of the transformation of statement types as the primary mechanism of scientific argument. The idea of the scheme does remain loosely as an element of discussion, but the five distinct categories vanish. Third, calling the categories "statement types" gives the impression that they offer the major distinction to be made among statements; that is, it appears as though the categories get to the root of the issue of statement form. But the categories actually concern only the relatively minor feature of how heavily couched propositions are. More thoroughgoing and consequential issues concern the form and role of the propositions themselves. Thus I could more readily imagine "statement types" applying to the variety of syntactic relations (defining logical relations) in the many sentences of a scientific report (as Gopnik has studied²), to the variety

of speech acts consummated by the statements (in the tradition of Austin and Searle, now only first being applied to written texts³), or to the variety of roles individual propositions play within the argument of the entire paper. Fourth, no distinction is made here between the many statements made within a single article and the overall statement of the article, if such a univocal statement of an entire text could be said to have definitive existence. This conflation of two levels of statement may be justifiable, but it is certainly problematic enough to require an extensive rationale, missing in this book. In short, Latour and Woolgar do not seem aware of the complexities of the kind of language analysis they wish to pursue: they use pre-contextual, pre-social categories to support large contextual and social conclusions.

Close contact with the evidence of the TRF case study does lead the authors to some striking and admirable observations of how scientific statements are shaped by the conditions of their making, but the neatness of the case study is marred by the fundamental problem of the book. Fact is conflated with statement of fact. To Latour and Woolgar, fact exists only in the minds of scientists who accept particular statements; the authors argue that TRF did not exist prior to its isolation and chemical identification, nor does it exist outside the circle of those familiar with and accepting of its "discovery." I can easily accept that as an item in human consciousness, in the symbolic terms graspable by human consciousness, TRF is constructed, a new item of consciousness; but to ignore that there is a substance out there that precedes our attempt to describe it, to suggest that what we isolate and label are only products of human consciousness, seems a rather naive kind of anthropocentrism. Indeed later Latour and Woolgar present a fancy version of the tree falling unheard in the forest (p. 183), although the authors do feel uncomfortable enough with the argument to qualify its use (see footnote 23, p. 186).

Such a radical position precludes the investigation of one of the more interesting questions about scientific discourse: to what extent and in what way do the conventions and criteria of scientific discourse encourage a grasping towards a reality that exists prior to the words? Or more broadly, in what ways do scientists create a symbolic analogue for nature that moves toward an increasingly good fit between consciousness and that which exists outside consciousness. It would be hard to imagine a science without a commitment to a nature that exists independently of its study, a nature to excite and constrain the imagination, despite science being a human (and therefore a social and historical) activity. Although the human imagination is fertile, would it have sought out, identified, and described TRF unless certain biological and chemical mechanisms suggested the existence of something like TRF? Ludwik Fleck's suggestion of active and passive elements of knowledge offers a much more promising model for the investigation of the commerce between human consciousness and nature, a commerce reflected in scientific texts.⁴

Once having taken the position that science is driven only by the dynamics of its construction with no substantial constraint by things outside consciousness, Latour and Woolgar are moved to show that scientific reality, constructed in daily conversation in the laboratory, is the same as everyday reality, constructed in everyday conversation. After only the sketchiest argument with Garfinkel's distinction between everyday reasoning and scientific reasoning (p. 153), they rest their case on the claim that talk in the laboratory is, like everyday talk, heterogenous. If heterogeneity is to mean anything consequential, it must suggest that the minds and words of all the participants in a conversation are not headed in the same direction at the same time. Heterogeneity could be indicated by differing purposes of different participants, differing interpretations by the participants of the progress of the conversation, shifting planes of

discussion, or a variety of non-harmonious speech acts. But here all Latour and Woolgar show is that laboratory conversation has different referents. At different times scientists talk about four different kinds of things: known facts, activities, theoretical matters, and other researchers (pp. 160-163). The observations that talk in the laboratory seems to revolve around several poles and that these poles are referred to tactically in the course of the communal process of constructing an article are not trivial (although not unexpected; either). But this is far from proving heterogeneity in conversations; by this standard the sentence John recited the second law of thermodynamics. would be heterogenous in itself, because it refers to a person, an activity, and a known fact. The authors have hardly shown that a multiplicity of referents undermines the rationality of the laboratory enterprise. Again the authors have done less than they think they have.

In the final chapter Latour and Woolgar draw together their vision of a science which creates its own reality and of scientists who rise within science. Theirs is a world of making investments, measuring costs, and setting prices, where the currency is all man-made, where the only law of the marketplace is getting away with whatever others will let you get away with. The argument is seductive in the same way a novel is seductive: it begins with a recognizable experience, then gets you so involved in its vision of experience that you no longer notice that experience has been left far behind. Although Latour and Woolgar consider their enterprise part of science, they would not be troubled by this comparison, for they use the language describing fiction to describe both their own work and the work of the laboratory they observe. Their self-consciousness about fictiveness even adds to their abandon, as when they create a fictionalized observer who is allowed to forget things we ordinarily know. Such a tactic may be useful for creating fresh eyes with which to examine the taken for granted things usually overlooked, but at some point the fiction needs to be ended, and the new observations should be set in the context of what we knew originally. But here the fiction is never recalled. Things forgotten are never remembered, and we are lead to the far ends of the fictional observer's speculation--a technique common in the novels of Vladimir Nabokov, who plays with his characters' solipsistic tendencies and who knows the novel is a constructed game to be perpetrated on the reader. Latour and Woolgar make a principle of forgetting in their emphasis on the material left behind at each stage of the process of literary inscription. They do not bother to pay attention to exactly what is carried on to the next stage; they are not interested in defining what traces of reality might remain in the final statement nor what forms the traces might take.

The limited accomplishments of this book do indicate that much can be gained by treating the language and texts of science as problematic, and some of the book's excesses may be forgiven on the basis of the headiness of that discovery. But as the book's shortcomings indicate, great care needs to be taken in applying linguistic and literary analyses to scientific discourse. Most language studies have developed in response to texts and utterances that have little responsibility to reality; novels, poems, literary essays, and even the talk necessary to get through the social world each have their separate commitments, but none requires a representation of nature that goes beyond verisimilitude. Scientific discourse does in some ways resemble other forms of discourse--and those resemblances should be explored--but a commitment to ever more precise descriptions of ever more closely watched realities distinguishes scientific discourse from other forms of discourse. Since language is constructed, in analysing it we must attend to those commitments around which it is constructed. A science of the language of science needs to take seriously the idea that language can create an analogue, no matter how imperfect and mediated, for real things outside the speaker's

imagination. Until stronger evidence than this book comes in, it is plausible to believe that a non-fiction is possible

Notes

I would like to thank Fred Baumann and Larry Stern for their comments and criticisms. I of course take responsibility for all errors and opinions.

¹For a more satisfactory discussion of the effect of various audiences on the shaping of scientific knowledge, see Ludwik Fleck, Genesis and Development of a Scientific Fact, trans. Fred Bradley and Thaddeus J. Trenn, ed. Thaddeus J. Trenn and Robert K. Merton (Chicago: The University of Chicago Press, 1979), pp. 112-125.

²Myra Gopnik, Linguistic Structure in Scientific Texts (Amsterdam: Mouton, 1972).

³J.L. Austin, How To Do Things With Words (Cambridge, Mass.: Harvard University Press, 1962); and John R. Searle, Speech Acts: An Essay in the Philosophy of Language (Cambridge, England: Cambridge University Press, 1969).

⁴Ludwik Fleck, op. cit.

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Bruno Latour and Steve Woolgar. Laboratory Life: The Social Construction of Scientific Facts.

Because I knew that Laboratory Life reflected several years of field work, I ordered it eagerly as soon as I saw it announced. What I received and read, however, was not a research report. It was also not an ethnography, although it claimed to present an "anthropology of science." Laboratory Life is instead a grasping toward a statement of the phenomenological approach to the social study of science. I do not think that this book will be the programmatic statement of the approach: it is too undisciplined to serve that function. But I think that it may turn out to be an exemplar, a conglomeration of theory, method, and observation which will form a rich source of insight, in a fuzzy way, for much future work.

As I understand the phenomenological approach, it claims that a scientist's work is in essence a process of symbolic interaction with nature and with other scientists, the purpose of which is to make claims about the former. The organizing question of Latour and Woolgar's work is "How do scientists come to label certain of their claims about nature as facts?" Their specific concern is with how this labelling happened in the Salk Institute in several cases. In the course of their discussion, the authors make it clear that most of the claims about nature advanced and discussed by the scientists they observed were not treated as facts, but only as potential facts. For observations to become facts, a process of consensus-formation had to take place within and outside the laboratory. The consensus thus formed through interaction was also dissolved in the same way in certain instances. The authors

claim that these observed aspects of the lives of Salk Institute scientists are general throughout science, and they imply that paying more attention to them will help us to understand science better.

I agree. The phenomenological approach provides a view of scientific activities which is different from that provided by the analysis of the institutional structure of science and by studies of the development of scientific specialties. Since every institutional structure consists of an aggregate of individual decisions about their actions, the microviewpoint helps us understand both individuals and structures by making sense of those decisions. The phenomenological approach specifically provides an analytic technique for addressing the questions of perception and misperception which inevitably arise in the analysis of individual action. I feel that my own understanding of citation data has been improved by the discussion of the symbolic uses of references (1, 2); I assume that other researchers will find that a reconsideration of their problems and methods in interactional terms will help their work reflect scientific activity better.

By reading Laboratory Life, however, one might be led to believe that macro and micro analyses of science are contradictory. The authors' tone is often polemical, leading to the impression that they would like to replace other forms of sociological analysis with the phenomenological perspective, rather than expanding and informing the others. Clearly, I feel that this position is too narrow. It is justifiable for Latour and Woolgar to remind other sociologists that "scientific facts" are made and unmade by people. It is also justifiable, however, to ask Latour and Woolgar to remember that the existence of social institutions (e.g. specialty groups, graduate schools, journals, the National Science Foundation) are a taken-for-granted aspect of laboratory life. These institutions are continually created, maintained, and dissolved through the actions of people. How do interactions among scientists come to be patterned, and how is the perception of "institutionalization" of patterned interactions maintained? These questions should be as interesting to a phenomenological sociologist of science as the questions Latour and Woolgar address.

Just as Latour and Woolgar overstress the contradictions between their approach and others at the theoretical level, I feel that the epistemological position of phenomenologists has been exaggerated. I do not agree with Bazerman (in his review in this issue of the Newsletter) that Latour and Woolgar are radical subjectivists, although their phrasing in the book is at several points consistent with that view. "Phenomenological bracketing" is an observational technique which calls on the researcher to suspend judgment temporarily about the truth or falsity of the subject's statements, not to withdraw belief in reality completely. We are asked to be agnostics, not atheists. Without phenomenological bracketing, we run the risk of trying to explain scientific activity by invoking the character of the object scientists study: scientists act the way they do because nature has to be studied that way. According to the authors, this way of thinking about science makes it difficult to grasp the ongoing cognitive dilemma of scientists, the precarious status of their claims about how to conceive of and study reality. Latour and Woolgar say that scientists themselves recognize this dilemma, but that students of science have by and large missed this point. The latter claim may or may not be true, but certainly should not be.

Despite my basic sympathy with the phenomenological approach, I was disappointed in Laboratory Life for the reasons implied in my opening paragraph. Field work will undoubtedly be the major methodological tool of phenomenological sociology of science, but Latour and Woolgar do not make serious use of it in this book. The authors draw on neither the long tradition of field work in sociology nor the

anthropological tradition for methodological models. The field data are not presented in any detail. Instead, the arguments in the book lean heavily on other forms of data, on historical materials and publication and citation data. Both of these are used naively, without attention to their methodological strengths and weaknesses. This is surely ironic. As a result of this seeming contempt for empirical matters, Laboratory Life ends up saying more about how to think about science than about how to study it, and even less about the character of science itself.

The book's finest quality was its sense of humor. Phenomenology can be fun, because it encourages us to see familiar situations in unfamiliar ways. If the approach is to have serious implications for the sociological understanding of science, however, a clearer statement than this one as to its purposes, methods and research problems needs to be made.

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"Science Indicators and Science Policy"

H. Brooks, 226 Aiken Computation Laboratory, Harvard University,
Cambridge, MA 02138 (USA)

The relation of science indicators to science policy raises several questions. First is the definition of the system, whether just academic science or the entire system of technical innovation from research through manufacturing and marketing or government policy and operations. Second is society's expectations from science, and how its health can be measured against expectations whose realization depends more on political social factors than on science itself. Third is how the output of research can be compared with the inputs into it if there is no norm other than comparative international performance.

"Science Indicators and Policy Analysis"

H. Averch, Office of Scientific, Technological, and International
Affairs, National Science Foundation, Washington, D.C. 20550 (USA)

This paper discusses the use of science indicators by public policy analysts with limited time and resources. Using the example of innovation policy, it describes what kind of policy relevant propositions can be extracted from science indicators and shows the inherent limits of indicators as instruments for policy making. It shows how science indicators can and must be combined with other sources to construct alternative strategies for decision making. It closes with a discussion of the use of science indicators in constructing consistent lines of argument and reasoning for making policy and for checking past policy.

"Indicators of Science: Notes and Queries"

H. Zuckerman, Department of Sociology, Columbia University,
New York, New York 10027 (USA)

R.B. Miller, Social Science Research Council, 1755 Massachusetts
Avenue, N.W., Washington, D.C. 20036 (USA)

Some science indicators can be found in Social Indicators 1976 as well as Science Indicators - 1976, but the coverage of science is limited. Neither volume contains data on cognitive aspects of science and technology or on their social consequences. The authors make suggestions for then-and-there assessments of cognitive advance in science and for prospective and retrospective checks on the validity of these assessments.

"International Indicators of Science and Technology: How Does the U.S. Compare?"

R. McCulloch, Department of Economics, University of Wisconsin,
1180 Observatory Drive, Madison, Wisconsin 53706 (USA)

Because the basic determinants of innovative success are poorly understood, the data in SI-76 cannot support an unambiguous summary assessment of U.S. science. While some nations now rival the U.S. in relative expenditure for R&D, U.S. absolute expenditure still dwarfs that of any nation except the U.S.S.R., and the U.S. remains preeminent by most measures of technological capacity. However, the technology gap continues to narrow, bringing both costs and benefits to the U.S. Advances abroad threaten the U.S. position in some markets and exacerbate the nation's trade adjustment problems. But the nation may also benefit substantially from new opportunities to import as well as export advanced technology.

"Comments on 'International Indicators of Science and Technology'"

E. Mansfield, 202 Plush Mill Road, Wallingford, PA 19086 (USA)

Science Indicators - 1976 has been prepared with a great deal of skill and is a valuable document. The fact that it reflects the unsatisfactory state of basic knowledge of the ways in which science and technology affect, and are affected by, various economic, social, and political variables of interest to policy makers is no fault of its authors. Nonetheless, in handling some topics, the report might have gone further in indicating the limitations of the measures used. Also, several topics omitted from the report might be considered for inclusion in subsequent editions.

"Indicators of the Impact of R&D on the Economy"

R.B. Freeman, Department of Economics, Harvard University, Cambridge,
MA 02138 (USA)

This paper reviews the literature on the economic effects of R&D and then examines the gaps in our knowledge. While most micro-studies show that R&D raises economic growth, existing knowledge of the mechanisms by which R&D affects productivity and output is sparse, and it is unclear whether the micro-studies can be generalized to the national economy. The paper concludes by examining some possible consequences of the reduced R&D effort by the United States.

EXTRACTS FROM PAREX NEWSLETTER

CONFERENCE REPORTS

Conference on Anthropology, Ethnomethodology and Sociology of Knowledge
in the Study of Science "Fury Conference"

Bad Homburg, 4th-7th January 1979

by

Steve Woolgar

Following the first annual conference of the Society for Social Studies of Science, held at Cornell in November 1976, a number of participants decided to organize a much smaller meeting in which some of the more novel approaches to the social study of science could be discussed in detail. The idea for this follow-up meeting was born during a journey from Cornell in a Plymouth Fury, hence the meeting's designation. A group of fifteen braved the European winter to assemble at the Werner Reimers Stiftung at Bad Homburg on the outskirts of Frankfurt.

The theme of the meeting was "Anthropology, Ethnomethodology and Sociology of Knowledge in the Study of Science." Papers were presented by seven of the participants:

Bruno Latour (CNAM, Paris): What does it mean to study Science?

David Bloor (Edinburgh, Scotland): An Anthropological Approach to Industrial Scientists: An Empirical Test of Mary Douglas 'Grid and Group Theory'.

Harry Collins (Bath, England): Understanding Science.

Steve Woolgar (Brunel University, England): What can Ethnomethodology tell us about Science as a Topic?

John Law (Keele, England): Ethnomethodology and Theory in the Sociology of Knowledge.

Karin Knorr (Pennsylvania, USA): The Political Knowledge of Truth.

Richard Dolby (Kent, England): Controversy and Consensus in the Growth of Scientific Knowledge

The restricted size of the meeting and the comfortable facilities provided an environment conducive to close consideration of the issues at hand. Indeed, the success of the meeting is perhaps reflected by the gradual realisation that there were a number of significant discrepancies amongst the positions of participants previously thought to be essentially similar. It became clear, for example, that gross categorisations of ethnomethodology conceal many different empirical strategies. Thus, the difference between the position of Steve Woolgar (who argued for an optimistic attitude towards the potential of ethnomethodologically inclined analysis of science) and the position of John Law (who argued that sociologists cannot afford to adopt the ethnomethodological critique) appears to hinge on somewhat discrepant accounts of what ethnomethodology actually looks like in practice. By far the most marked division which emerged, however, was between those who presented papers, the majority of whom advocated a so-called "micro" approach to the study of scientific activity, and those other participants, who were largely unclear about the benefit of this strategy

for their own longstanding macro-sociological research interests. In reaction to this (referred to in "conferenceese" as that between the "microbes" and the "examiners") the first group (those who presented papers) closed ranks in order to portray the advantages of a "micro" approach. This, in turn, had the unfortunate effect of detracting from the extent to which the "microbes" could usefully work out their differences. Nonetheless, some steps were taken towards clarifying the vast potential of the "micro" approach. The empirical (rather than the currently programmatic) contributions which will be published in the near future argue the point persuasively. This work will undoubtedly benefit from discussion in an environment such as that provided by the Bad Homburg meeting.

S.W.
Brunel University

COMMENT ON THE "FURY CONFERENCE"

by

Peter Weingart

The enigmatic term "FURY" goes back to the closing night of the conference at Cornell in 1976 when a few of us rode from Ithaca to Syracuse in a Plymouth Fury to catch a plane. It was then that the idea for the conference was born as I had been impressed by some programmatic papers devoted to anthropological and ethnomethodological approaches. Having come home, I asked Wolf Lepenies to join me in organizing the meeting and we immediately agreed on a structure according to which the seemingly new approaches to the study of science were to be confronted to criticisms from traditional sociologists, sociologists of science, ethnomethodologists and anthropologists in order to test their merits and shortcomings. This in my view was a way to strengthen them, to give them support and recognition. Also, much time was to be reserved both for the preparation of papers and for their discussion at the meeting itself.

My fascination with the anthropological and ethnomethodological points of view came from their promises to provide a radical solution to the problem of overcoming the "internal/external" debate. This debate and its unsatisfactory dichotomies had haunted the sociology of sciences even after it had become what we hesitatingly called the "cognitive sociology of science."

When the meeting finally took place more than two years after the initial idea, most of the original expectations were disappointed and some new surprises and insights came up. The papers held no surprises as they remained on a programmatic level. This perhaps did not reflect the actual state of work in the field, but simply corroborates the experience that no matter how much lead time one gives for the preparation of a conference, the writing of papers will assume priority in the work organization of the individual scientist only two months or so before the very last and final deadline.

The first surprise came with a paper by Bruno Latour which in a very polemic fashion put down all previous approaches in the study of science as insufficient and attempted to offer the anthropological/ethnomethodological perspective as a panacea. The surprising fact about this paper was that it represented the programmatic statement of a new generation in the sociology of science which forced those of us who still thought of themselves as belonging to the "new," i.e. "post-Mertonian" generation, to realize that their time had come up. Generations

in the sociology of science, then, seem to have a life-time of half a decade. This is short enough to keep the memories of "revolutionary coups" alive among the old and the new generation and thus to subject them to self-reflective analysis. Much of this meeting was characterized by demonstration of self-assertion by a yet uncertain community, seeking to demarcate itself from the traditionalists. The critical comments which admittedly were ironical responses to polemic challenges in part, were taken to be unfavorable to the new approach and triggered some very emotional reactions. Both Kuhn and Lakatos would have loved to watch!

The second surprise was the obvious disparity between the programmatic goal of the "revolutionaries" and their traditional implications. The goal or rather the claim is that the new approaches will overcome the internal/external dichotomy by studying science in the making, i.e. as a social process, the product is shown to be inherently social. Strangely enough, all the analyses in this context become very "internalists," preoccupied with micro-processes of interaction, scientific work processes, etc.

Only a paper by K. Knorr made the attempt to link this level of analysis to categories on a macro-analytical level, and she is perhaps the only one yet to admit that the internal/external dichotomy will return through the back door again. To a German it is an additional surprise to observe that the English and the French scholars put forth the anthropological/ethnomethodological approaches as radically new. As Wolf Lepenies pointed out in his comment, much of this is quite familiar to anyone who has grown up where a hermeneutical tradition has always been much stronger. Some of the new work seems to be a vulgarization and most of it takes no account of the methodological problems that come along with the tradition. This does not of course, serve as an argument against these approaches and I want to make clear that contrary to my colleagues' impressions, I hold their work to be interesting and fruitful, though not beyond criticism. In the light of these impressions the conference proved the point that science (at least our social study of science) is a social process, or rather a "self-exemplification of science." Now who was it who made this point?

P.W.

University of Bielefeld

SYMPOSIUM ON THE SOCIOLOGY OF SOCIOLOGICAL KNOWLEDGE, Maison des Sciences de l'Homme, 30-31 March 1979.

This meeting was sponsored by the research committee on the sociology of knowledge of the Association Internationale des Sociologues de langue française, and organized by Bernard-Pierre Lécuyer and Gérard Namer. A critical commentary of some of the papers delivered appears in PAREX, #4 (Autumn, 1979), written by Lécuyer. The titles and substantive areas covered were:

Gérard Namer, "Machiavelli and Rousseau as the sources of the sociological discourse"

Bernard-Pierre Lécuyer, "Empirical social research in France at the dawn of industrialization: its institutions and research works from 1780 to 1850"

Janina Markiewicz-Lagneau, "Sociology, its classics and the national reconstruction of Poland from 1919 to 1939"

Juan Corradi, on the "Present state of the art of the sociology of knowledge in the United States"

Diana Pinto, on "The relation between sociology and society in Italy from 1945 to 1978"

Elisabeth Crawford, on the "Deprofessionalization of the sociological craft in the United States and France and its consequences"

Jacques Maître, on the "Sociology of Catholicism in France"

Daniel Friedman on "Fake-healers and their patients"

Gerard Lagneau, on "The consequences of the 'Cox dilemma' for the sociological study of advertisement and advertising agencies"

MEETING OF THE SECTION "SCIENCE STUDIES" OF THE GERMAN SOCIOLOGICAL ASSOCIATION, Berlin, 18-20 April 1979.

CHAIRMAN: Peter Weingart, Bielefeld

Papers presented centered around the different aspects of Science and Technology policy. Comments on some of these papers by Georg Kamphausen appears in PAREX, #4 (Autumn, 1979):22-23.

Papers read at the Congress

I - TECHNOLOGIEPOLITIK und SOZIALER WANDEL

K.W. Deutsch (Harvard University/Wissenschaftszentrum Berlin): Technik und sozialer Wandel.

II - STAATLICHE WISSENSCHAFTS- und TECHNOLOGIEFÖRDERUNG: EMPIRISCHE ERGEBNISSE

K. Pavitt (University of Sussex): Government Support for Industrial Innovation: The Western European Experience

P. Papon (Ecole de Physique et Chimie Industrielles de la Ville de Paris): Scientific Expertise between Administrative Power and the Politics: Science Planning in France.

O. Keck (Universität Ulm): Überlegungen zur politischen Ökonomie der Grosstechnologie am Beispiel der Schnellbrüterentwicklung.

III - PROBLEME TECHNOLOGIEBEGLEITENDER SOZIALFORSCHUNG

F. Naschold (Wissenschaftszentrum Berlin): Probleme sozialwissenschaftlicher und technischer Forschung im Bereich neuer Produktions- und Fertigungstechnologien

M. Széplábi (Bundesministerium für Forschung und Technologie): Sozialwissenschaftliche Technologieforschung: Aufgaben und Widerstände.

J. Reese (Gesellschaft für Mathematik und Datenverarbeitung, Bonn): Die Herausforderung der Sozialwissenschaften durch neue Technologien: Das Beispiel der Informationstechnologie.

T. K. Karlsen (Universität Kassel): Ungenutzte Potentiale von Technik und Technologieforschung am Beispiel der Büroinformationstechnik in der öffentlichen Verwaltung.

J. Janshen (Wissenschaftszentrum Berlin): Neue Kommunikationstechnologien im Alltag.

IV - WISSENSCHAFTLICHE EXPERTISE und POLITIK

M. Gibbons (University of Manchester): Problems in directing Science to National Need: An Empirical Study of the Science Research Council, 1964-1975.

H. Meier (Wissenschaftszentrum Berlin): Vom Nutzen und Nachteil anwendungsorientierter Sozialwissenschaft: Kritische Anmerkungen.

G. Bechmann (Gesellschaft für Kernforschung, Karlsruhe): Sozialwissenschaftliche Begleituntersuchung als planerisches Instrument der Technologiepolitik am Beispiel des Einsatzes informationsverarbeitender Technologien.

Projektgruppe Automation und Qualifikation (Institut für Automationsforschung): Integration von Technologiepolitik und Entwicklung beruflicher Handlungsfähigkeit als Problem gewerkschaftlicher Strategie.

W. D. Webler (Universität Bielefeld): Begleitforschung und politische Entscheidung: das Beispiel der Neuregelung der Juristenausbildung.

INNOVATIONS AND OBSTACLES TO INNOVATION IN THE APPLIED SCIENCES, Maison des Sciences de l'Homme, 28-31 May 1979.

This meeting was organized by the Foundation Maison des Sciences de l'Homme and the Group d'Etudes des Méthodes de l'analyse sociologique (GEMAS) with the collaboration of the DGRST. A summary of the main themes and orientations which emerged at this meeting appears in PAREX, #4 (Autumn, 1979): 25-26, written by Michael Bauer.

LIST of the COMMUNICATIONS:

M. Bauer (France): La production d'ingénieurs chimistes en France; enjeux et déterminants d'une politique d'enseignement.

Pr. Blume (Great Britain): Structures and values as barriers to innovation-oriented research in Britain.

O. Grandstrand (Sweden): Technological change, subcultures and conflict; a study with specific reference to conditions of industrial R and D in Sweden.

Dr. Haber (Great Britain): On the progress of applied chemistry in Britain between the wars.

Pr. Kurucz (F.R.G.): Industrial research in the Federal Republic of Germany; a sociological perspective.

Pr. Layton (U.S.A.): Industrial and academic research: barriers to collaboration, the case of hydraulic turbine in America.

G. Meyer-Thurow (F.R.G.): The industrialization of invention. The origins, organization and role of industrial research in the German chemical industry up to 1914. A case study.

Pr. Y. Rabkin (Canada): Chemicalization of petroleum refining in the United States: the role of cooperative research, 1920-1940.

T. Shinn (France): The genesis of French industrial research 1880-1960.

Pr. Torstendahl (Sweden): Industrial research and researchers in Sweden, 1880-1940.

RESEARCH IN PROGRESS

WEST GERMANY

UNIVERSITY OF BIELEFELD, Research Projects
Universität Bielefeld, USP Wissenschaftsforschung Postfach 8640

· I - DEVELOPMENT of RESEARCH OUTSIDE the UNIVERSITY SYSTEM (ENTWICKLUNG der AUSSERUNIVERSITÄREN FORSCHUNG)

In the centre of this research project the main question at issue is the political problems which have forced the government to establish additional research capacities besides the traditional and established scientific (academic) system. Which factors can be evaluated as relevant to the fact that the universities were not thought to be able to take over these new tasks? Were the new establishments able to fulfill the expectations of the government and under which conditions (organizational, institutional embeddings) did they operate?

Besides the description of the development of government research in Germany, the project aims mainly at two complexes, reflecting the major topics of interest of the Science Studies Unit:

- (1) The analysis of the genesis of government research (Ressortforschung) at the end of the nineteenth century.
- (2) Conditions of the foundation of government research laboratories and the development of "Big Science" after the war.

II - "THE NOBEL PRIZES in PHYSICS and CHEMISTRY at THE END of THE NINETEENTH and THE BEGINNING of THE TWENTIETH CENTURY"

Research Project in cooperation with Dr. E. Crawford (Paris) under the auspice of PAREX and financed by the Stiftung Volkswagenwerk, Hannover.

Short description of the Research carried out by the Science Studies Unit in Bielefeld:

The history of the natural sciences at the turn of our century could, without doubt, be best characterized by its radical changes concerning the traditional mechanical worldview and the following new conceptual and theoretical developments based on new experimental achievements. In this time of revolutionary changes, especially in Physics and Chemistry, fell the testament of Alfred Nobel and the award of the first Nobel Prizes. Since then, these prizes have constituted the most prestigious international award for discoveries in the three fields of science: Physics, Chemistry and Medicine (Physiology) - the development of which they were set up to promote.

To get some insights into the interesting relations between the institutional and cognitive developments of Physics and Chemistry and the awarding of the Nobel Prizes in these fields, it will be necessary to analyse carefully the records of the Nobel Committees and the prize-awarding institutions. Based on the accessible material in Stockholm, i.e. the minutes of the meetings of the Nobel Committee (Protokoll: K. Vetenskapsakademiens Nobelkommitte för Fysik), the typescript copies of letters or other documents (Handlingar rörande Nobelpriset i Fysik), the minutes of the sessions of the Academy and the meetings of third Klass (Physics) and fourth Klass (Chemistry) (Vetenskapsakademiens protokoll angående Nobelarenden), the volumes of the in-coming letters (brev) concerning both, Physics and Chemistry and further sources available in the archives, we want to throw light on several important questions in the history of science. Some of these questions will be as follows:

Who are the nominators and the nominees? Are the discussions in the Committee about Nobel's will related to some extent to the selection of nominators and the awards of the prizes?

Where do the nominators come from (institutional, disciplinary background)? Does the analysis of the proposals and refusals refer to the actual and general developments in Physics and Chemistry? To which factors could the timelags between scientific inventions (new theories) and the award of the Nobel Prizes be reduced, and many other questions concerning mainly the analysis of the disciplinary histories of Chemistry and Physics versus the topics, awarded by the Nobel Committee. For these reasons we are interested in all materials available in Stockholm for both Chemistry and Physics.

Regarding the fact that most of the early Nobel-Laureates have been Germans, we have started our survey in studying the expansion and differentiation of the disciplines of Physics and Chemistry, analysing the structure of the German University system, the growth of research capacities and the history and new creation of research laboratories.

Beside this institutional background, a rational reconstruction of the cognitive development in Physics and Chemistry, i.e. the unsolved problems, the proposed solutions and the objectives of Physics and Chemistry in general may help to investigate the close relationship between the history of science and the Nobel Prizes. The communicational networks between the scientists, measured by their correspondence and the proposals made to the Committee as reported on the Nobel archives will constitute another major point of view in the reconstruction of problem areas in Physics and Chemistry, related to the outstanding scientific works honoured by the award of the Nobel Prize.

III - DEVELOPMENT of SCIENCE INDICATORS

The quantification of science, i.e. the development of science indicators have meanwhile grown up to a respectable research field and have become a topic of general interest in the Social Study of Science. It was first in the United States that the discussions were pushed forward about the limits and possibilities of Science indicators as a tool for Science policy; they deeply influenced some considerations about the structure of governmental support for Science and Technology and new evaluations of priorities in decision processes. Critiques of the used indicators (Manpower, SCI, National budgets and various other input and output indicators) touching both methodological and theoretical implications will be taken up in this project and thought over for their general usefulness for Science policy in general and the specific situation in Germany. On the basis of these critiques, it is planned to develop new indicators for more detailed problem areas. Empirical tests of the indicators in use and the new developed ones will probably illustrate problems and insufficiencies of government and Science statistics and thus an effort will be made to work towards a change of the common data production.

T. H. DARMSTADT, RESEARCH PROJECT THE SCIENTIFICATION of OBSTETRICS

A case study about the relation of commonsense knowledge and scientific knowledge:
Planned Research Project at the T. H. Darmstadt.

by B. ADLOFF and G. BÖHME.

Notwithstanding the impressive successes of medicine in Obstetrics (concerning security of birth and decrease of mortality) there are problems within the actual situation. The doctors try to level up the so far neglected psychological and social aspects. The midwives have to face a reorganization of their job, which brings an enhancement of medical-technical competences associated with a loss of independence. Women articulate a critique of the technicalization and rationalization of Obstetrics together with the request for better integration of birth into the context of life.

The theoretical background of the project is given by a differentiation of two types of knowledge, lifeworld knowledge and scientific knowledge, the project being related to other investigations in the scientification of particular areas of life and experience. For the case of Obstetrics, a theoretical construction of these types of knowledge will be performed. The supposition is that the social agents of these knowledge types are the doctors and the midwives respectively. Midwife knowledge, then, in short words is to be characterized by the integration into the context of the lifeworld, by tradition, by the unity of knowledge and personal maturity, by empathy and self-experience. Medical knowledge in Obstetrics, in contrast, is determined by the paradigm of natural science and hence can be characterized through objective experience, as a type of knowledge based on explanations, serving to avoid trouble, which ultimately allows men to direct processes under well defined conditions. Of course, such a constructed type of midwifery knowledge is to be presupposed only for a hypothetical primordial state, that is before there was any explicit contact between midwifery and medical knowledge. Since such a contact exists, that is in singular cases since the Renaissance, institutionalized since the eighteenth century, a scientification of Obstetrics has been going on. In its historical part, the project will examine the process of scientification. Main points of interest are at the origin of medical thematization of birth under the auspices of pathology, of the obstetrical clinic, the history of countermovements against the main stream of the development of medicine, the shift of Obstetrics out of the hands of women into the hands of men, the development of midwifery, and the change from supporting Obstetrics to programmed birth. The historical investigation finds its end and continuation in the analysis of the actual state. This analysis will serve the explanation of the actual problems and the further development of Obstetrics. The actual state will be analysed according to three main questions. First the tension between certain wants and norms will come under scrutiny, i.e. the tension between medical necessities, the organizational constraints of the clinic and the wishes of the women concerned. Second the knowledge and the competences of the doctors, the midwives and the pregnant women are to be investigated under the perspective of possible complementarity and division of labour, and the actual chances of realization of these competences under given conditions. The third point of interest is the actual change of midwifery and its consequences for the performance of Obstetrics. These questions will be investigated with the help of methods of empirical social science (interviews).

As for the approach of the actual state, the project is restricted to the situation of the FRG with some comparisons to other European countries; for the historical investigation it is restricted to the development since the Renaissance in German speaking areas. The project will be performed by a team of four researchers and will have its institutional base at the Institute of Philosophy of the T. H. Darmstadt.

B.A. & G.B.
T. H. Darmstadt

COUNTRY REPORTS

Impressions of the Situation of Science Studies in the Netherlands

by Arie Rip, State University of Leiden

Science studies in the Netherlands are still a rather heterogeneous field--as it is everywhere--I would think. The heterogeneity of the field is combined with a solitary character of the individual activities: often a certain specialism or tradition is represented by one or a few isolated individuals only, who have found their reference groups in the international community sometimes but have no "peers" within the Netherlands. I shall briefly touch upon the situation in the different disciplines contributing to the field to illustrate my general point.

In the history of science, the Dutch have produced some eminent scholars (E. J. Dijksterhuis, R. Hooykaas, also R. J. Forbes) but they have left little in the way of successors. Hooykaas' chair in the Free University of Amsterdam has been, ably, filled by an Englishman, M. J. S. Rudwick, and it may be significant that as research-fellows he has had to take English or American scholars also. There is a growing interest in the history of technology, with a working party and some nuclei for growth in the universities (e.g. A. J. van Schelven in Twente and C. P. Bertels in Leiden).

Sociology of science is not a separate specialization in the Netherlands: individual research workers, and occasionally graduate students, come to it from other sociological areas, or sometimes from other disciplines (natural science, via science and society activities, see below). The research being performed at the moment has very little international visibility; only W. van Rossum is well-known in the sociology of science community.

For psychology of science, similar things can be said, but here fewer persons are involved. What interest there is, is centered on cognitive psychology of science (M. de Mey, now returned to Ghent, Belgium) and some work in the border zone between epistemology and psychology.

A survey of the philosophy of science in the Netherlands up till circa 1970 has been given by C. A. van Peursen and R. J. A. van Dijk, Z. f. allgem. Wissenschaftstheorie 3, 1973, pp. 372-379. They note a trend towards a more dynamic and genetic conception of the sciences, allowing methodology to be taken up in a broader framework, that of strategies of human action (ibid. p. 377). This trend has, since then, become more manifest, notably because of the impact of neo-Kuhnian philosophy and sociology of science on Dutch philosophers of science. A new journal, Kennis en Methode (Knowledge and Method), appearing since 1977, testifies to this. The circles around this journal have also taken the initiative to found a Dutch Society for the Philosophy of Society (spring 1979). It is probably typical for the Dutch situation that the charter of the new Society specifies among the goals also "the advancement of the study of the relations between science, technology and society, and the possibilities (opportunities and limitations) of science policy."

The study of science and technology by economists often proceeds separated from the social and reflexive studies of science treated up till now. This is also the case in the Netherlands. Innovation and research management studies are undertaken in the Delft Interuniversity Business School and by staff from the government organization for applied scientific research (TNO). The latter cooperate with the Sussex Science Policy Research Unit; the nature of these studies is application-oriented (government, industrial concerns). A few

economists pursue studies encompassing the role of science and technology, so A. Heertje on the role of technical development.

Political studies of science and science policy studies are almost absent excepting some in-house feasibility studies of proposals for science policy organization. But there is a lot of discussions about science policy, which includes bits and pieces of analyses (cf. also science and society activities, below). Political scientists, such as A. Hoogerwerf and H. J. Aquina, pay some attention to science policy, but only as part of broader studies of government policy.

Because of the interdisciplinary and often problem-oriented nature of science studies, it is necessary to complement the disciplinary approach with one in terms of problem-areas and trends. Very important in the Netherlands is the issue of science, technology and society (cf. the remarks under philosophy of science and science policy studies). In the last decade, teaching programmes in science and society have been started in nearly all departments of science and technology, and in a few other disciplines; they increasingly include a research component (for the background of the teaching programmes, see Egbert Boeker and Arie Rip, Social Studies of Science 5, 1975, pp. 457-484).

Within the broad area of science, technology and society, issues of science and public decision-making are attracting a lot of attention, which is not limited to the science and society teaching programmes. From the side of science policy, as well as decision-making in general, the issue keeps cropping up, members of Parliament inquire what science studies have to offer them, etc. Obviously, recent controversies as the environment, nuclear energy, recombinant DNA, have contributed to the popularity of this issue. Public participation may have a good chance in the Netherlands because of recent history (cf. Dorothy Nelkin, Technological Decisions and Democracy, European experiments in public participation (London: Sage, 1977) and in view of the willingness to experiment with "science shops" (Dorothy Nelkin and Arie Rip, "Distributing expertise: A Dutch experiment in public interest science," Bulletin of the Atomic Scientists 35 (5), May 1979, pp. 20-23 and 54).

As in other Western industrialized countries, appropriate technology is fashionable in some circles, but also rather concrete work in this field is done in the technological universities and some independent units in both soft technology and technology for developing countries.

Institutionalization of the field of science studies is growing slowly. Since autumn 1975 there exists a Group Science Researchers (Wissenschaftsforscher), supported by the Interuniversity Institute for Social Science Research(1). Communication and mutual criticism to improve the quality of the research have been the main goals; at the moment attempts are made to experiment with judging research proposals, with a view of getting a more official policy status. There is a general move towards the formation of formalized peer-groups (Werk-gemeenschappen) in the Netherlands this last year: impending science-policy reorganization make it probable that research money, apart from that directly given to the universities, will be distributed in this way for all specialized disciplines.

Another interesting and potentially significant development lies in the fact that the Science Policy Advisory Council (the top advisory council to government) has brought out a report on science studies, noting the fragmented state of the field in the Netherlands and proposing the formation of a research unit specifically devoted to the study of the dynamics of science and its policy implications. The Science Policy Ministry has shown interest and, if money can be found, some steps will probably be taken during the next few years.

(1) Secretary to the Group is P.A. Koefoed, c/o SIWO, O. Z. Achterburgwal, 128 Amsterdam.

ANNOUNCEMENTS

Report on Conference

Innovation and Continuity in Science - Vrije Universiteit Amsterdam 12-14 December 1979.

Professor Martin Rudwick organised this international conference to mark five years of work by the Unit for History and Social Aspects of Science at the Vrije Universiteit. Ten papers were read over three days, and each was formally discussed and followed by a question and answer period. Innovation was presented in five contexts with two papers per context. These were:

Technology: Hans Braun, Bochum; Ranier Randolph, Santos Dumont, Brazil.

The Scientific Community: Steven Shapin, Edinburgh; Henry Brookman, Amsterdam.

The Small Group: Gerald Geison, Princeton; Harry Collins, Bath

The Individual: Frederick Holmes, Yale; Howard Gruber, Rutgers

Religious Beliefs: Martin Rudwick, Amsterdam; Eileen Barker, L.S.E.

Participants felt that the format worked admirably, particularly for the three middle sessions where the progressive narrowing of perspective brought unity and some genuine cross-fertilisation of ideas between sessions. Standing right in the middle, I found myself being relieved of prejudices about the role of historical and psychological studies.

The conference was enjoyed by all the participants and the level of hospitality was outstanding making certain that exchanges and discussions did not cease at the ending of the formal meetings.

Amended versions of some of the papers from the middle sessions are to be published in a special edition of History of Science in December 1980. This should make it possible for this all-too-rare example of successful cross-disciplinary collaboration to give rise to further work.

H.M. Collins
University of Bath

Report of Meeting

New Perspectives in the History and Sociology of Scientific Knowledge,
University of Bath, 27-29 March 1980.

This meeting was a continuation of the "New Perspectives" series of conferences run by the British Society for the History of Science--in this case in collaboration with the BSA Sociology of Science Study Group. The meeting was organised by Harry Collins and Steve Shapin and the 25 papers were precirculated amongst the 80 participants (for details of the programme see 4S Newsletter Volume 5 No. 1). The currency of the theme of the conference was indicated by the fact that several potential participants had to be turned away due to organisational constraints--even so, several "gate-crashers" could be found furtively trying to avoid the conference organisers! The work schedule of the conference turned out to be demanding with, on one occasion, eight papers being presented and discussed over one morning. Perhaps this was overly ambitious. However because the papers had been precirculated and because the discussants' remarks were of a uniformly high quality any exhaustion on the part of participants reflected two and a half days of stimulating discussion. It is hoped that revised versions of some of the more sociological papers will be published in a special edition of Social Studies of Science.

The reactions of three participants to this meeting of sociology and history are presented below:

I

A traditional characterisation of the history and sociology of science runs as follows: Historians are empirical internalists soberly documenting historical facts, whereas sociologists are theoretical externalists boldly (or rashly) mapping intellectual developments in science onto the changing structure of the ambient society. The most obvious achievement of the conference was to demonstrate how poorly this traditional model represents the current intellectual complexities in the history and sociology of science.

As far as the sociology of science is concerned, the conference demonstrated the extent to which this has become like other areas of sociology; that is, highly differentiated along theoretical and methodological lines. An earlier solidarity, crystallising around the "post-Kuhnian programme" in response to philosophical hostility, has given way to an increasing diversity of approach. This diversity was captured by the Conference, where most positions were represented: marxian and non-marxian, normative and interpretive, internalist and externalist, materialist and idealist and so on.

In this situation, the sometimes limited communication between adherents of different positions was more a reflection of the state of the art than of the rigours of the timetable. Indeed, there seems a real possibility that current pluralism may end in fragmentation and the emergence of several self-contained sociologies of science.

While the various theoretical positions entail quite different notions of what a sociological explanation should look like, most of them are at least united in challenging the idea that there is any really important distinction between the history and sociology of science. The conference demonstrated that there remains a legacy of past intellectual practice in the form of a division of labour

in which sociologists use the "empirical" work of historians as raw material for their more "theoretical" endeavours. However, this particular distinction is likely to become less marked and, conversely, the number of researchers who find it impossible to describe themselves as either historians or sociologists is likely to increase. This local trend reflects a wider convergence between sociology and history.

The task of future conferences would appear to be two-fold: to help counteract tendencies towards fragmentation within the sociology of science and to encourage a continuing convergence of sociological and historical interest.

Dave Robbins
Department of Sociology and Social
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The University College of Wales
Aberystwyth

II

For many of the historians of science present, the greatest advantage of this very enjoyable conference was the opportunity it gave to meet a particular group of sociologists of science, and to hear them discuss their work. While few historians find much stimulus or interest in the citation-crunching variety of sociology of science, at Bath we were confronted with a quite different approach to the analysis of modern science. However familiar we may have been already with the published work of--particularly--the Bath and Edinburgh "schools," it was much more stimulating to hear it expounded "life" (this is a well-known feature of all kinds of wissenschaftlich communication, which the citation-crunchers should perhaps ponder more carefully). What I found most striking about this work was the extent to which it is in fact historical in all but name, even though it deals with the science of the 1960's or 1970's. This comment is not meant to herald a take-over bid by historians, but simply to underline the community of interest between sociologists and historians of science, when members of both groups become seriously concerned with the relation between the processes of social construction of scientific knowledge and the content of the claimed knowledge itself. Indeed the greatest irony of the conference was the extent to which many of the overtly historical papers were less closely concerned with the detailed content of science than the sociological ones, in the sense that they focussed attention more on the broader underlying metaphysical foundations of past scientific work. In this respect I think that historians of science could well learn from the sociologists who were present at Bath, to bring their analyses of the social component of science much closer to the everyday activities of the scientists they study. All in all, a most successful conference, and I hope there will be more like it!

Martin Rudwick
Vrije Universiteit
Amsterdam, The Netherlands

III

Some 20 years ago, I observed a party of ladies being guided around an exhibition of "Young Contemporaries" at the Witney Museum in New York. Their guide discoursed fluently on the successive blobs, smudges and squiggles--until he came to a canvas on which there was recognisably a human figure. He took a deep breath: "Here, ladies, we have a figurative painting. To understand such paintings, one must know about Greek sculpture, Renaissance portraits, Impressionism, and things like that. That's the trouble with such paintings"

The "trouble" with some of these new sociological and historical perspectives was that they found they really had to know and understand both aspects of their subjects. It turned out to be an important socio-historical fact that to be an Irish Tory in the 1820's was to be on the side of progress; it was likewise realised to be a controversial question whether an opinion taped in an interview is more revealing and/or reliable than the unsolicited evidences of a personal letter that happens to have been preserved. What was heartening was more than the civility of the dialectics and the remarkable degree to which everybody (especially the official discussants) had done their homework on the pre-circulated papers; it was how much all participants had learned and were learning from each other. That's the sort of "trouble" we should all be in.

Although the scrutiny of fine details is thus becoming more realistic, meta scientific insights can best be captured with a wide-angle lens. A lot of other relevant aspects of view points were missing. I can think of a number of philosophers who could have given much--and taken much--in these debates. Somebody referred to "internalism," but only as if to a temptation of Satan. I had scribbled "themata, surely" on half the preprints. How instructive it would have been to have had a real live working taxonomist to comment on the various papers on classification. 4S is broader, and more eclectic--but it, too, is scarcely aware of the multidisciplinary but coherent conception of science that is now emerging from these "troubled" waters.

John Ziman
University of Bristol

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Trallement

Selon les normes de la convention collective en vigueur.

Date d'entrée en fonction

Le 1er janvier ou le 1er juin 1981

Date de clôture des candidatures

Le 1er septembre 1980

**Les candidats sont priés d'envoyer
leur curriculum vitae à:**

Mme Brigitte Schroeder-Gudehus
Directeur
Institut d'histoire et de sociopolitique
des sciences
Université de Montréal
C.P. 6128, succursale A
Montréal (Québec)
H3C 3J7

AAAS PROFESSIONAL ETHICS PROJECT
Preliminary Progress Report

Increasingly, scientific and engineering societies are becoming involved in addressing ethical aspects of their members' professional activities. Many professional societies affiliated with the American Association for the Advancement of Science have developed guidelines for professional conduct that not only monitor and respond to errors in professional judgment and abuses of professional rights; several societies also provide support for members upholding the code and sanctions against members violating the code. These rules and practices constitute an important response by the scientific and engineering professions to value conflicts associated with the development and use of science and technology. As such, these efforts are part of a larger set of institutional forces that influence the work environment and behavior of scientists and engineers. In general, however, the professional societies have not developed active programs addressing the ethical implications of their members' work. These are among the preliminary conclusions of a Professional Ethics Project conducted by the office of the AAAS Committee on Scientific Freedom and Responsibility, with the sponsorship of the National Science Foundation and the National Endowment for the Humanities.

Beyond a compilation of existing codes of ethics, the professional societies' ethics activities previously have not been documented comprehensively or subjected to systematic evaluation. At a time of increasing concern about the moral and legal rights and obligations of scientists and engineers and the status of professional self-regulation, such an assessment was selected as the basis for the AAAS project. A Workshop on Professional Ethics and a Survey of Scientific and Engineering Associations are the major components of the AAAS Professional Ethics Project conducted by co-directors Rosemary Chalk, Staff Officer for the AAAS Committee, and Mark S. Frankel, Director of the Center for Study of Ethics in the Professions at the Illinois Institute of Technology (formerly of Wayne State University). Sallie Chafer coordinated the project workshop. The 12-month project results will highlight significant ethics activities within the societies, and the project data and findings will also contribute to an evolving resource base useful for further study of professional ethics issues.

The Survey of Scientific and Engineering Associations polled 241 scientific and engineering societies affiliated with AAAS and received responses from 178 societies, a 74% response rate, at the time of the Workshop. The survey elicited previously unavailable information about the ethics activities of professional societies. Survey data provide information indicating the range of societies that have adopted rules of conduct (approximately 75 percent of the responding societies' aggregate membership are affected by such rules); the formation of committees or staff offices within the societies to review ethical concerns or individual complaints submitted to the societies; and the use of sanction and support mechanisms to implement and enforce the societies' codes. The project team has prepared a preliminary framework to classify these activities for further analysis.

Over 80 representatives of professional societies, academic and private research institutions, government, public interest groups, and the press met last November at the two-day Workshop on Professional Ethics in Washington, D.C., to review the survey results, identify criteria for classifying and evaluating professional ethics activities, and consider appropriate roles for professional societies seeking

to encourage ethical professional conduct. The workshop focused on: the results of the survey; changing attitudes toward professional rights and obligations; ethical issues in selected professions; and ethical issues in the development and use of scientific and technical knowledge. Wide-ranging discussions by workshop participants highlighted important topics for consideration by professional societies. For example:

--Workshop participants noted the general lack of consistent attention by professional society members and staff to ethical concerns about the use of scientific and technical knowledge. Survey data suggested that although 64 societies reported adoption of ethics codes or guidelines, a smaller number cited standing committees concerned with ethics (57 societies), staff offices on ethics (24 societies), full-time staff officers (7 societies), or special funding (23 societies) for ethics activities.

--Workshop participants discussed the difficulty of formulating appropriate guidelines for individual members on ethical issues, noting that societies often deal with such issues on a general or abstract level. Specific issues of concern include: the lack of common definitions in reviewing individual cases among societies; the difficulty of accommodating the needs of a membership that may have widely varying interests and professional responsibilities; and the determination of appropriate goals and functions for ethics guidelines.

--Workshop participants and the survey data indicated that when complaint adjudication procedures are available within a professional organization, the society frequently does not publicize or actively enforce the procedures. Survey data identified 105 societies with some form of available sanctions against members violating ethics guidelines--but only half of them had imposed sanctions at any time since 1970.

--Workshop participants emphasized the importance of support mechanisms for society members involved in conflicts over ethical issues. Yet, the survey data indicated such support mechanisms are currently in a primitive stage of development in societies, with very few groups providing counselling and referral services or financial and legal assistance. For example, survey data revealed that although 11 societies provide financial or legal assistance, only 3 societies have provided services on 17 occasions.

--Workshop participants noted that greater emphasis should be directed toward informing the society members and the public of activities related to ethics guidelines and procedures. Such notification is particularly important in light of public concerns over the development and use of science and technology, the right of consumers to be informed, and the increased cost of professional services.

--Workshop participants expressed concern over the currently ambiguous relationship between legal standards and professional ethics guidelines. This concern is manifested most clearly in the area of employer-employee relations as well as in procedures involving the use of humans or animals. Although some societies are developing employment guidelines, very few groups have clarified the legal rights of their members in disputes with employers, clients, research subjects or others, nor have the societies clarified the legal status of their rules of conduct in supporting members who have experienced retaliation as a result of taking actions in compliance with their professional codes of ethics.

A final report on the AAAS Professional Ethics Project will be published in mid-1980 and will include comments from workshop participants and major conclusions. The final report will represent a unique collection of information on professional ethics and should stimulate further research into and greater understanding of the importance of professional ethics to science and technology, and to developing a better definition of the role of professional societies both in promoting responsible conduct by their members and in supporting members who act responsibly.

Further information is available from the Committee office at AAAS. (1515 Massachusetts Avenue, Washington, D.C. 20005)

PROJECT ON PUBLIC UNDERSTANDING OF SCIENCE AND TECHNOLOGY

MODERN TIMES, INC., a non-profit organization devoted to developing theater and media programs to further the public understanding of science and technology, has received a grant from the Maryland Committee for the Humanities to develop a play on the Three Mile Island incident. Under the auspices of MODERN TIMES, a dramatist, a physicist and an historian are collaborating to produce a script, film sequences, and discussion format for local production at colleges, schools, science centers and community theaters. For further information contact MODERN TIMES, INC., 2522 N. Calvert Street, Baltimore, Maryland 21218
Tel: 301-366-4604.

INTERVIEWING SPACE SCIENTISTS/ADMINISTRATORS. A program to interview selected eminent space scientists and administrators is being undertaken by the American Institute of Physics' Center for History of Physics, under a contract from the National Aeronautics and Space Administration. Payment and expenses for part-time work are offered to historians with some knowledge of NASA/space science work and with some interviewing experience, who wish to conduct a few interviews. Send letter and vitae to Spencer Weart, AIP, 335 East 45th Street, New York, New York 10017. AIP is an equal opportunity employer.

PHYSICS CITATION INDEX FOR THE 1920's

The Institute for Scientific Information has recently completed (under National Science Foundation support) the compilation of a citation index for the physics literature for the period 1920 to 1929. A limited number of copies of this index in printout form will be distributed to libraries and archives or other appropriate repositories. Individuals interested in utilizing this index or having their local institutions acquire a copy should contact Henry Small, Institute for Scientific Information, 3501 Market Street, Philadelphia, PA 19104.

THE CHARLES BABBAGE INSTITUTE FOR THE HISTORY OF INFORMATION PROCESSING

by

Pamela Gullard, Charles Babbage Institute

The Charles Babbage Institute for the History of Information Processing (CBI) was founded in 1977 by Erwin Tomash, Chairman of the Board of Dataproducts Corporation, to conduct and otherwise promote historical research in the field of computers and information processing. Tomash says: "Modern society has found it increasingly important to process ever larger amounts of information at ever faster speeds. We are becoming an information-dependent society and the data processing industry itself has become a major economic force. The impact of the information revolution on the contemporary individual can hardly be understated. It is time to systematically study the revolution."

The Executive Secretary of CBI, Paul Armer, spent most of his career at the RAND Corporation where he was head of the Computer Sciences Department. In early 1978, Armer opened a temporary office at 701 Welch Road in Palo Alto, California 94304.

An Advisory Committee was soon formed. This committee met in April 1978 and formally endorsed the idea of CBI. The Committee members agreed that the two most important tasks facing the Institute were to (a) build a rich data base of historical information and (b) encourage scholars to use that data base. Following this meeting, the Advisory Committee was expanded into the Institute's Board of Trustees, which includes industrial leaders, professionals in the field of information processing, and historians of science and technology.

The program of the Institute is broadly-based, encompassing not only technical but also socioeconomic aspects of the history of information processing. The program is international and maintains a standard of inquiry which continually probes behind personal reminiscences to the broader questions which concern the professional historian, such as: How did the economic environment of the times affect the development of the computer?

CBI expects to select a permanent location at a major university by May 1980. Meanwhile it has begun a fellowship program, annually offering a grant to an outstanding graduate student working in the field. CBI has also launched an oral history project, including a series of interviews with individuals associated with the late George Forsythe in establishing the Stanford University Computer Science Department. In addition, historical materials are being collected on an emergency basis until CBI has the facilities to expand its archival activities. The Institute publishes a Newsletter* and in other ways has begun to further communication among those working in the field.

CBI, a non-profit organization, is supported by the community it serves. The American Federation of Information Processing (AFIPS), an organization which through its constituent societies represents over 250,000 individuals in the field, is contributing \$50,000 per year. In addition, industrial organizations have contributed a total of over \$100,000 and CBI has received financial support from over forty individual Founders and Associates.

*If you would like to be on CBI's mailing list, write to: The Charles Babbage Institute, 701 Welch Road, Suite 224, Palo Alto, CA 94304. There is no charge for the Newsletter. Membership in the Associates of The Charles Babbage Institute, which includes a subscription to the Annals for the History of Computing published by the American Federation of Information Processing Societies (AFIPS), is \$40.00 per year.

CBI's impact on the field is indicated by such support and by other expressions of enthusiasm for the program. For example, when accepting CBI's first fellowship, doctoral fellow William Aspray, Jr. wrote: ". . . your support . . . stimulates important research and a healthy realignment of the priorities in the history of science. . . ."

APPOINTMENT ANNOUNCED

MARK H. ROSE has accepted appointment as associate professor in the Department of Social Sciences at Michigan Technological University, Houghton, Michigan 49931.

CALL FOR PAPERS

CITIZEN PARTICIPATION IN SCIENCE POLICY. The Journal of Voluntary Action Research will be publishing a special issue in 1981 on the topic of citizen participation in science policy. This issue will cover a wide range of citizen activities including participation in hearings & boards, involvement in public service science centers, and protest activity such as the anti-nuclear protests. The Journal of Voluntary Action Research is an official publication of the Association of Voluntary Action Scholars and has recently joined the Transaction Periodicals Consortium.

If you wish to submit a paper for this special issue, please write me as soon as possible indicating your interest and briefly summarizing the focus of your paper.

Jim Petersen
Deputy Editor, JVAR
Department of Sociology
Western Michigan University
Kalamazoo, Michigan 49008

FORTHCOMING CONFERENCES, MEETINGS AND COLLOQUIA

There will be a session on psychology of science at this year's American Psychological Association meeting on 5 September 1980 in Montreal, Canada. The session is being organized by Leslie H. Bean, Ohio State University (Room 242, 164 West 19th Street, Columbus, Ohio 43210). Participants include John Wilkes, Arthur Elstein, Howard Gruber, and Clifford Mynatt.

LOREN GRAHAM will present a paper on "Soviet Views on Biomedical Ethics" at the International Conference of Slavic Studies in Garmisch, West Germany, September 30 to October 4, 1980.

4S Society for Social Studies of Science

The 4S Newsletter is published four times each year at the Department of Sociology, Southern Illinois University, Carbondale, Illinois 62901, and sent to all members of the Society for Social Studies of Science. Membership is on a calendar year basis. Membership dues (\$15 for professionals, \$5 for students) and institutional subscriptions (\$25) should be sent to: The Secretary/Treasurer, 4S, Department of Sociology, Indiana University, Bloomington, Indiana 47401.

Editorial Assistant: Beverly Morber

4S

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