

Science & Technology Studies

(formerly 4S Review)

Volume 5

Numbers 3/4

ISSN 0886-3040
Fall/Winter 1987

Journal of the Society for Social Studies of Science

Science & Technology Studies

Journal of the Society for Social Studies of Science

Editor

Susan E. Cozzens
Department of Science and Technology Studies
Rensselaer Polytechnic Institute
Troy, New York 12180-3590 USA
(518) 276-6598
Bitnet: USERFP2L@RPITSMTS

Associate Editors

Rachelle Hollander
Ethics and Values Studies
National Science Foundation
Washington, DC 20550 USA

Ned Woodhouse
Department of Science and Technology Studies
Rensselaer Polytechnic Institute
Troy, New York 12180-3590 USA

Contributing Editors

Ditta Bartels
School of History, Philosophy,
and Sociology of Science
University of New South Wales
P.O. Box 1
Kensington NSW 2033
Australia

Howard Gobstein
Office of the Vice President for Research
University of Michigan
4080 Fleming Administration Building
Ann Arbor, MI 48109-1340 USA

Loet Leydesdorff
Science Dynamics Unit
University of Amsterdam
Nieuwe Achtergracht 166
1018 WV Amsterdam
The Netherlands

J. Scott Long
Department of Sociology
Washington State University
Pullman, Washington 99164 USA

Pamela Mack
Department of History
Clemson University
Clemson, SC 29631 USA

Trevor Pinch
Department of Sociology
University of York
York YO1 5DD
England UK

Wesley Shrum
Department of Sociology
Louisiana State University
Baton Rouge, LA 70803 USA

Susan Leigh Star
Department of Information
and Computer Science
University of California
Irvine, CA 94717 USA

Jeffrey L. Sturchio
Archives and Records
Management Services
AT&T Bell Labs/WVA201
5 Reinman Road
Warren, NJ 07060 USA

Williams A. Turner
SERPIA, CNRS
Centre de Documentation
Scientifique et Technique
26, rue Boyer
75971 Paris Cedex 20
France

Book Review Editor:

Edward J. Hackett, Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, NY 12180-3590 USA

Special Editorial Advisor: Daryl Chubin
Computer Consultant: Juan Jewell

Society for Social Studies of Science

President: Arie Rip

Secretary-Treasurer: Wesley Shrum

Past Presidents: David Edge, Nicholas C. Mullins, Arnold Thackray, Bernard Barber, Dorothy Nelkin, Warren O. Hagstrom, Robert K. Merton

Council: (Terms expire Fall 1988) Susan Cozzens, Ronald Giere, Rachel Laudan; (Terms expire Fall 1989) Mary Frank Fox, Ron Johnston, Helga Nowotny; (Terms expire Fall 1990) Adele E. Clarke, Roger Krohn, Steve Woolgar

Publications Committee: Mary Frank Fox (chair), Rachel Laudan, Randall Albury, Thomas F. Gieryn, Marcel LaFollette, James McCartney

Program Committee 1988: Wouter van Rossum, Chair, 104 Sydwende, 9204 KG Drachten, The Netherlands

Local Arrangements 1988: Loet Leydesdorff, Chair, Science Dynamics Unit, University of Amsterdam, Nieuwe Achtergracht 166, 1018 WV Amsterdam, The Netherlands

Information on the Society available from Wesley Shrum, Secretary-Treasurer, Department of Sociology, Louisiana State University, Baton Rouge, LA 70803 USA; E-mail SOWESL@LSUVM

Science & Technology Studies (ISSN 0886-3040), Journal of the Society for Social Studies of Science, is published four times a year. Journal subscription includes membership in the Society. Rates are \$40 institutional, \$30 individual, and \$15 student. To join 4S and receive *Science & Technology Studies*, send a check for the appropriate amount payable to "Society for Social Studies of Science (4S)" to 4S, P. O. Box 487, Canton, MA 02021 USA. Subscriptions in European currencies may be paid through the European Association for the Study of Science and Technology (EASST). For specific rates, contact Elisabeth Crawford, EASST Secretary, Groupes d'Etudes et de Recherches sur la Science, Ecole des Hautes Etudes en Sciences Sociales, 10 rue Monsieur-le-Prince, 75006 Paris France. Change of address should be sent to 4S, P.O. Box 487, Canton, MA 02021 USA. The Society gratefully acknowledges support from the Department of Science and Technology Studies at Rensselaer Polytechnic Institute for the editorial offices of the journal.

SCIENCE & TECHNOLOGY STUDIES

Journal of the Society for Social Studies of Science

Volume 5

Number 3/4

Fall/Winter 1987

Table of Contents

About the Authors	102
-------------------------	-----

ARTICLES

Accounting for the Social Context of Risk Communication <i>Branden B. Johnson</i>	103
--	-----

Reading Repertoires: A Preliminary Study of Some Techniques that Scientists Use to Construct Readings <i>Jonathan Potter</i>	112
---	-----

COMMENTARY

Agenda Battles and Practical Cooperation in Science Development <i>Michael J. Moravcsik</i>	122
--	-----

Science in LDCs: Connectedness versus Universalism <i>Yehouda Shenhav</i>	124
--	-----

"Hidden Agendas," Conceptual Clarity and Disciplinary Awareness in the Study of Science and Technology in the Developing Countries <i>Richard P. Suttmeier</i>	126
---	-----

RETROSPECTIVE

The Emergence and Maturation of the Sociology of Science <i>Bernard Barber</i>	129
---	-----

POLICY REPORT

Funding and Academic Research in the Life Science: Results of an Exploratory Study <i>Edward J. Hackett</i>	134
--	-----

1987 BERNAL PRIZE to Christopher Freeman	148
--	-----

CONFERENCE REPORT: Technology Indicators Conference <i>Alan Porter</i>	150
---	-----

BOOK REVIEW: <i>Rohm and Haas: History of a Chemical Company</i> by Sheldon Hochheiser <i>Reviewed by Basil Achilladelis</i>	151
---	-----

NEWS AND VIEWS

4S News Page	152
Council Minutes	153
Treasurer's Report 1987	155
News Clips	156
Meetings Calendar	163

Information for Contributors	Inside Back Cover
------------------------------------	-------------------

ABOUT THE AUTHORS

Branden Johnson is Research Scientist in the Risk Communication Unit, Office of Environmental Health Assessment, Division of Science and Research, New Jersey Department of Environmental Protection. He conducts original research, oversees contract research, and applies research findings to DEP activities, in such areas as risk communication and risk assessment. He has published extensively on risk perception, hazard management, and environmental policy.

Jonathan Potter is Lecturer in Social Psychology at the University of Loughborough. As well as scientific discourse, his current research interests include accounts of riots and the language of racism. He has published numerous articles and is the author (with P. Stringer and M. Wetherell) of *Social Texts and Context: Literature and Social Psychology* (London; Routledge and Kegan Paul, 1984) and (with M. Wetherell) *Discourse and Social Psychology: Beyond Attitudes and Behaviour* (London and Beverly Hills, Sage, 1987).

Michael J. Moravcsik is Professor of Physics at the Institute of Theoretical Science of the University of Oregon. In addition to his work in theoretical elementary particle physics, he is involved in research in the science of science and science policy, with particular emphasis on the output measures of science and on the building of science in the Third World. He has also been involved in development projects in many Third World countries.

Yehouda Shenhav received his Ph.D. from Stanford University in 1985 and is currently a lecturer at the Department of Sociology, Tel Aviv University. His research concerns gender and ethnic stratification in science and science in a cross-national perspective.

Richard P. Suttmeier is Henry P. Bristol Professor of International Affairs at Hamilton College. He recently completed a year's service in China as the Director of the Beijing Office of the National Academy of Sciences Committee on Scholarly Communications with the People's Republic of China. He has also served as Senior Analyst at the Office of Technology Assessment on a project dealing with technology transfer to China and has written extensively on Chinese science and technology affairs.

Bernard Barber is Professor of Sociology in the Barnard College and Graduate faculties of Columbia University. He has just published *Effective Social Science: Eight Cases From Economics, Political Science, and Sociology* (Russell Sage Foundation, 1988) and is working on a new book, *Knowledge, Culture, and The Social System*.

Edward J. Hackett is a sociologist in the Department of Science and Technology Studies at Rensselaer Polytechnic Institute. He is currently studying ethical and values issues in university-industry research centers and the effects of new technology on organizations and worklife.

Accounting for the Social Context of Risk Communication

Branden B. Johnson
New Jersey Department of Environmental Protection

Abstract

The conveyance of technical risk information from experts to the lay public is unlikely to be successful unless the social context of such messages is addressed. This context includes social networks, economic resources, political rights and responsibilities, histories, and ideologies. Social context can be taken into account by clearly defining desired outcomes, identifying the information desired by citizens, and choosing communicators carefully. Technical and social perspectives on risk communication, incomplete in themselves, can in combination improve management of hazards.

The emerging field of risk communication has emphasized the meaningful conveyance of technical information from risk experts to laypeople. This paper argues that the social context of risk communication is just as important, and sometimes more important, than the technical issues with which the field has largely been concerned. Messages are ultimately conveyed from person to person; what and how risk messages are received will be affected by our relations with the communicator, other humans, and the material artifacts concerned. Although the importance of social context has been "recognized," this recognition has been relatively superficial.

A first approximation of "social context" is compared here to the current emphasis in risk communication. The examples used in this article deal with hazardous waste, hazardous facility siting, and natural disasters, but the arguments made apply as well to other hazards (e.g., radon, occupational safety and health). The focus is on how lay receivers of risk messages are affected by social context, but the influence of social context on the construction of risk messages is also important.¹ Tasks for risk communicators which combine the technical and social context approaches are suggested, and some barriers to the use of risk communication based on social context are briefly noted.

Defining Social Context

A Multi-Dimensional Concept

The social context approach assumes that relationships with other people affect the degree to which individuals attend to, understand, and use technical information about risks. The field is too young to yield

a comprehensive definition of social context, but important factors include social networks, economic resources, political rights and responsibilities, histories, and ideologies. It is important to note that these factors vary in importance in different risk communication situations. For example, the context for siting a hazardous waste facility will be different than that for enhancing occupational safety: workers' apparently voluntary risk assumption in an economic context reduces their ability to act on risk information relative to that of site residents, who wield more political power.² And the statutory differences between low-level nuclear waste and chemical waste management vary the constraints on public and governmental willingness and ability to come to agreement on waste facility siting.³ It is thus critical for success to "know your risk communication problem. . . [and] objectives. . . [and] listen to your audience and know their concerns."⁴

Social Networks

The extent, complexity and membership of social networks can affect the speed and accuracy with which messages about hazards are transmitted. Such networks also affect message credibility, and the definition of the receiver's concerns (e.g., preservation of family or responsibility to the community).

Author Address: Risk Communication Unit, Division of Science and Research, New Jersey Department of Environmental Protection, CN409, Trenton, NJ 08625 USA. The views in this work are those of the author and do not necessarily represent the views of the New Jersey Department of Environmental Protection.

Science & Technology Studies, 1987, Vol. 5, No. 3/4, pp 103-111

At Love Canal those most concerned about the risks were well connected in the neighborhood, particularly the young parents, and became increasingly unwilling to trust official information; the unconcerned were relatively isolated from their neighbors.⁵ In another community, people who identified pollution of the local lake (a Superfund site) as a major community problem were more likely to have intense social interactions and attachment to the community than those who cited unemployment, a much more prominent local problem.⁶ Contrary findings⁷ that attachment to place results in low concern about hazardous waste may be due to offsetting concerns (see below) about economic impacts of waste management.

Ties to place also appear to increase actions (e.g., evacuation) to reduce vulnerability to natural hazards.⁸ Credible sources of evacuation warnings increase with community involvement and social connections,⁹ and discussion of a hurricane threat with neighbors is critical to whether evacuation is actually undertaken.¹⁰ (Where hazard experience is frequent, as in Hawaiian volcanic eruptions, social networks apparently become unimportant in evacuation decisions.)¹¹

Economic Resources

Risk communications may seem to elicit irrational responses if the economic benefits and costs pertinent to message recipients are ignored by or unknown to communicators. Whether a community has a diversified economic base will influence the salience and interpretation of risk messages. Where management of abandoned hazardous waste appears to threaten local economic viability, health risks will tend to be denied.¹² Nuclear power plants or waste facilities which promise to reduce local economic distress may evoke lower risk concerns. However, protecting existing economies (as with abandoned hazardous waste) appears to be more important to local perceptions of facility risks than the chance of enhancing the future economy. The economic vulnerability of a population alters what risk message is received by its members, which is why such vulnerability has been criticized as an unethical criterion for hazardous facility siting.¹³

In the face of natural hazards people may live in clearly dangerous places or refuse to evacuate when warned of imminent disaster. This apparently non-rational behavior has been linked to a lack of the capital and organizational resources that would allow people to avoid or reduce their exposure to hazards.¹⁴ Conversely, the availability of resources can help people reduce their vulnerability to natural disasters,¹⁵ though the expectation of post-disaster assistance (e.g., from government) can reduce hazard preparations despite risk communications.¹⁶

Distribution of Political Rights and Responsibilities

There must be a perceived match between such rights and responsibilities, and the benefits, costs, and risks of a proposed action, if technical risk communication is to be taken at face value. Power—i.e., who makes the decisions about what risks are carried and by whom?¹⁷—is the social relationship at issue here. Power has been identified as a vital issue in siting radioactive and chemical waste facilities,¹⁸ with residents demanding monitoring, design, and closure powers as the price of facility acceptance. Rayner and Cantor¹⁹ use a case study in acceptance of new nuclear technologies to argue that the salient issues are not technical ones like hazard magnitude or probability, but principles for achieving consent to a technology, distributing liabilities, and investing trust in institutions.

Risk communication which ignores these points is likely to fail, unless power is not seen by message recipients as salient to the hazard at issue. Natural hazards usually fall into the latter category. But power can be pertinent where proposed controls to avoid future disaster (e.g., restrictions on floodplain land use) are deemed obstacles to local political and economic opportunities. The perception after a disaster that damages are due to acts or omissions of humans (instead of being due to an act of God) will also make political rights and responsibilities pertinent to natural hazard communications.

History

Perceptions of communicator legitimacy and trustworthiness are affected by the history of the relations between risk communicator and message recipient. The process of risk communication itself becomes part of the continuing experience of communicator and recipient. For example, the experience of states with the Department of Energy over siting high-level radioactive waste facilities has exacerbated rather than resolved conflicts.²⁰ "Good" reputations may be less influential in eliciting trust than "bad" reputations are in losing it: there is some evidence²¹ on low-level nuclear waste disposal that past performance, no matter how good, is no guarantee of acceptance where the hazard is highly dreaded. Mistaken history can be as significant for risk communication as the real thing. For example, officials have been known to delay hurricane warnings, and thus hamper evacuation, for fear of a non-existent "cry-wolf" syndrome from previous mistaken warnings.²²

What makes things more difficult for the risk communicator is that the history of its relations with other groups may be communicated to the message recipient, to the detriment of the communicator's

plans. For example, citizen groups increasingly link those who have already fought local siting of hazardous waste facilities to people in towns which are candidates for such sites. In combination with media tales about unsuccessful waste management, these links make the risk communications of facility proposers less believable. Comparisons of the communicating institution to other institutions dealing with the problem may also be made by message recipients. For example, federal and state agencies were unable to provide risk estimates or cleanup resources for a case of groundwater contamination. Combined with trust in local government, this failure resulted in low concern about the contamination despite official risk communications.²³

Ideologies

The ideologies of communicator and recipient affect their identification of risks and benefits, acceptance of a risk communicator and of proposals for resolving disputes. These ideologies are often concerned with the issue of power discussed earlier, but they can also be concerned with "the state of society" more generally. (Although ideology may seem to be a belief system that transcends particular social contexts, it has been argued that it arises from and is maintained by face-to-face communications with others.)²⁴

Rayner and Cantor argue²⁵ that "the critical question facing societal risk managers is not 'How safe is safe enough? [i.e., technical risks],' but 'How fair is safe enough?'" They suggest that different institutional settings will generate different ideologies. For example, entrepreneurial types will prefer market mechanisms to determine who bears losses (e.g., from new nuclear technologies), assume consent is implied in the allocation of one's money, and express confidence in successful individuals. Egalitarian environmentalists, however, will prefer a strict-fault system of liability, and assume consent to risk is contained only in explicit statements made through trusted participatory institutions (e.g., town meetings). Because both sides' risk communications are structured by their ideologies about liability, consent and trust, to a large extent they end up talking past each other.

The State of the Art and Its Limits

A Technical Emphasis

The social context factors discussed above have been recognized to some extent by people in risk communication. For example, Covello et al.—sophisticated communicators—recently reviewed the literature.²⁶ Among their suggestions were the tackling of "lack of trust and credibility," "resource, legal,

and institutional constraints on analysis, authority, and actions," and "limited [expert and official] understanding of the . . . concerns . . . of individual citizens and public groups"—all valid issues of social context.

However, this review's coverage (and, by extension, that of the field in general) of the social context of risk communication is less than it might be. Relatively little of the review addresses social context issues: some 72% (41 of 57) of its recommendations are concerned with avoiding mistakes in conveying technical information to lay audiences. Examples of these recommendations are

use simple, graphic, and concrete material, avoiding technical language wherever possible. . . identify, acknowledge, and explain uncertainties in risk estimates. . . when people ask personal questions—such as, "Can I drink the water?"—respond in a personal way without minimizing the risks and uncertainties. . . recognize the power of subtle changes in the way information is presented and use such knowledge responsibly. . .²⁷

This technical approach to risk communication is undoubtedly important. Messages must address the problems citizens have in processing scientific information. They must simultaneously acknowledge scientific uncertainties, through such means as presenting data in different numerical or pictorial ways.²⁸ The technical emphasis is also important because "these recommendations may seem obvious, but are nonetheless continually and consistently violated in practice."²⁹ For example, Missouri residents screened for dioxin contamination were sent registered letters informing them that they had abnormal blood and/or urine results. Apparently no explanation of the results—including what "abnormal" meant in the circumstances—was enclosed. A state official agreed that

"it is highly technical [information]. . . But we expect they will find technical support [from family doctors, to explain the results and] to continue with their own health care. . . I wouldn't be alarmed by the letters. . . But then, I'm not one of those people."³⁰

In a world which can produce such risk "communications," a simple emphasis on how to transfer technical information to laypeople should not be lightly dismissed (though the official's response also indicates several problems in the social context of that communication).

The technical emphasis in the risk communication literature is partly due as well to the relative inaccessibility of research on social context. In some cases, presentations of research are so harshly critical

of hazard managers that it is not surprising that institutional communicators dismiss these findings. In other cases, these points appear in literature which is about "risk selection" or "hazard perception" or "environmental politics," rather than explicitly about risk communication. And even when such findings are available, they usually emphasize what happens when the social context of risk communication is *ignored*. This is true of the review by Covello et al., for example; and it is the norm, not the exception. Recognition that the social context of risk communication has an impact is far more advanced than understanding of how positive results can be achieved by taking it into account.

However, the technical emphasis in even state-of-the-art risk communication may also be due to less noble factors than need or lack of alternatives. Risk communicators tend to be either technical risk experts, or employed as their representatives. A technical emphasis is desirable to them because it emphasizes their expertise compared to the lack of it among those to whom they are communicating. Expertise acts as a technical sieve for consensus on facts, but it also labels certain paths to knowledge (and certain pathtakers) as better than others.³¹ Thus the technical emphasis in current risk communication is *both* justifiable technically *and* suspect for its implications for the relative power of groups in hazard management.

Missing Perspectives

Face-to-face communication between two people includes the words spoken, tone of voice, facial expression, and body language. Its content, on all these levels, is constrained by such factors as whether others are present and who they are; the past relationship of the speakers; and whether the agreed-upon text of the talk is that of conversation, interview, debate, interrogation, joking, or gossip. Talk is thus a multi-layered, multi-channeled activity carrying many meanings at once.

Risk communication is an equally rich activity. However, the technical focus noted above is the equivalent of the "words spoken" in a conversation. In some communications—whether general conversation or risk-related—this is sufficient. But in most cases a major, sometimes even dominant, role is played by the non-spoken and subtextual aspects of communication. These aspects of risk messages are carried by their "social context."

Risk communication restricted to conveying technical information will ultimately fail to communicate, because it misses most of what is going on; messages about risks may be sent but will not be received. Furthermore, the current approach threatens to ignore the fact that risk communication is a *two-way*

process. One-way, impoverished risk communication will ultimately become mere ritual, without power to improve management of hazardous activities. This holds true whether it is deployed with good intentions or as a cynical plan to manipulate policy. Emphasis on purely technical communication will prevent its practitioners from perceiving several important aspects of their enterprise.

Unintended messages can be important. The scientific content of messages (e.g., official risk assessments) is important, but is only a small portion of the overall message that is sent. All of the communicator's actions are potential messages, and explicit risk communications may have negligible effects if the rest of "the message" contradicts them or makes them irrelevant. For example, reassurance that water from a municipal facility is safe to drink followed by the facility's closing—even if the shutdown is unrelated to chemical contamination—is unlikely to be a credible risk communication.³² In drought situations, water conservation programs lose credibility if the same utilities which promote voluntary conservation then raise rates to offset the consequent falling revenues.³³

This overall message is also evaluated in the context of others' (agencies, firms, activists) actions, which send similar messages relevant to potentially risky activities. Take the example of opposition to the restart of the undamaged reactor at Three Mile Island. Distrust of the Nuclear Regulatory Commission's neutrality and competence was not the only reason for local rejection of NRC risk messages. Local activists were also affected by such actions as cheating by utility operators and the governor's opposition to restart.³⁴ New Jersey's early easing of restrictions on water use during the 1980-1981 drought threatened the credibility of water conservation programs in neighboring New York City.³⁵ Technical risk communication ignores such influences on message reception.

Risks are not all that is at issue. "Risk" is by no means the only matter of interest in communications between organizations and groups, though the risk communication literature often fails to make that explicit. Decisions of government agencies and firms take into account many other factors as well (e.g., benefits, alternative projects and policies, institutional goals, prescriptions for society). Everyone "knows" this implicitly, but failure to explicitly include these factors in the message threatens its dismissal as irrelevant or self-serving (i.e., disguising political or economic interests as technical matters). The risk focus may also mean loss of an opportunity to directly address (and perhaps resolve) potential conflict on these other issues. For example, local opposition to a hazardous waste facility may be partly due to concerns about noise and property values; an

exclusive focus on safety concerns will overlook these other issues.

This does not mean that bringing policy benefits into the picture will encourage benefit-risk tradeoffs and acceptance of the communicator's proposal. It may do so in some cases, but the larger point is that what is at stake is what institutions and society should do, and messages ignoring this are unlikely to convey conviction. Statutory constraints and professional biases may make it difficult to raise these other issues, underlining the importance of the social context of risk *communicators*, but should not be arguments against making the attempt.

A theory of risk communication should integrate the technical and social context approaches. The ideal theory would be a middle-range one, concrete enough to provide guidance for practitioners but general enough to explain varying message responses across issues and audiences. Our knowledge of how factors in social context interact—much less their relation to technical communication—is too meager to allow for formulation of that theory now. Detailed suggestions about the application of social context to risk communication are also premature. However, some practical options can be suggested.

Communication Tasks Within the Social Context Perspective.

The risk communicator can start to deal with the message recipient's social context by undertaking the following tasks: (1) the *change in recipients' behavior*³⁶ which is desired as an outcome of the risk communication must be clearly defined; (2) the information which the recipient wants, as well as needs, should be identified; and (3) the most appropriate communicator for the situation should be selected, if there is any room for maneuver on this point. Even without definitive information about the effects of social context on reception of risk messages, undertaking these tasks can improve the performance of institutional risk communicators.

Defining Outcomes

Without a definition of desired behavioral outcomes which will shape message content, risk communication is likely to be misdirected or otherwise unsuccessful. This may seem obvious, but as the Missouri example discussed earlier indicates, all too often communicators assume that the conclusions to be drawn from their messages are self-evident. In the "Show Me" state, as elsewhere, "showing" one's data is not enough. The presentation of the data should vary according to whether the purpose of risk

communication is information and education, personal and disaster risk reduction, or joint problem solving.

Information and education. The first category of risk communication listed in the literature review by Covello et al. is "informing and educating people about risks, risk assessment and risk management in general." Although this particular discussion is benign, the lack of specificity of this category makes it suspect. There is often a hidden agenda to such efforts—e.g., encouraging public acquiescence in institutional decision-making. Reassuring messages are certainly far more common than concern-raising ones.

However, simple scientific and risk "literacy" is important for meaningful participation of laypeople in hazard management, and certain steps can be taken to promote useful education. This kind of communication needs to match transmitted concepts to the preexisting concepts and skills of the message recipients.³⁷ For example, if people are concerned about groundwater contamination this might be a base for educating them about harmful effects of unsafe disposal of household chemicals.³⁸ Use of social networks to facilitate transmission and joint learning of the material can be useful where the risk issue is important to local residents. Involvement of recipients in a hands-on application of the information—e.g., in lay risk assessments of possible hazardous waste sites³⁹—can also enhance learning.⁴⁰

Officials and experts may also learn things of value, in addition to their policy preferences, from citizen risk communicators. Citizens can gather data otherwise unavailable to officials because of professional ignorance or the expense of collecting it. For example, using lay volunteers to collect site-specific rainfall and streamflow data to add to National Weather Service data has been proposed to identify specific flood locations in New York State.⁴¹ Citizen risk assessment may identify hazardous waste problems previously unknown to officials.⁴² *Ad hoc* efforts of this sort are already occurring in communities around the country, and in one case⁴³ lay risk assessment allegedly led to an EPA suspension of forestry uses of certain pesticides. Although this trend chagrins experts who dispute the validity of lay assessments, cooperative efforts could avoid invalid results and extend agency resources. The possibility of "data mediation" has also been raised in some quarters.⁴⁴

Personal and disaster risk reduction. Both of these types of risk communication are intent upon affecting individual- and family-level behavior in large populations. The goal is to facilitate coping behaviors by removing constraints on them (e.g., cost,

availability, and relevance), in part by providing information about these factors and seeking to shift the salience of such behaviors.

For example, addressing relevant concerns (e.g., protection of family and conservation of economic resources) may be helped by exploiting cultural images of disaster, like chemical gas clouds,⁴⁵ or fear of hurricane-spawned tornadoes that is greater than fear of the hurricanes themselves.⁴⁶ Although "high-threat or fear campaigns" should be avoided,⁴⁷ accurate, low-emotion presentation of aspects of the hazard pertinent to citizens may be useful. This is particularly true for hazards such as hurricanes, where previous experience with the hazard appears to reduce a person's propensity to evacuate.⁴⁸

Joint problem solving. The arena of joint problem solving and conflict resolution is the part of risk communication most obviously affected by social context, because issues of power and process become paramount here. The conflict over siting nuclear and hazardous waste facilities may require redistribution of political rights and responsibilities.⁴⁹ This possibility raises technical and political questions about where responsibility for such facilities should be located, but it is also likely to arouse opposition from current hazard managers. Cultural (or value) analysis may be helpful in identifying specific decision processes, compensation, incentives, and independent technical capabilities that will overcome distrust of hazard managers.⁵⁰

O'Hare⁵¹ suggests that the "impact statement" approach to agency provision of information is misguided for controversial actions. Since the magnitude of policy or project impacts is among the items at dispute, government should restrict itself to providing general information (e.g., the kinds of impacts expected). Many alternatives should be discussed as early and for as long as possible, with at least some aspects of the policy or project adjustable for a wide range of values. For example, proposing a single waste facility design for a single location at the very beginning of public involvement is to court disaster. Any party should have the resources needed to participate, and be able to propose new alternatives at any time.

O'Hare also suggests that the regulatory agency act as a conduit for information passed between parties. Anyone could ask that a given datum be passed to anyone else. The source of this information would be clearly identified in case the recipient wished to take that into account in evaluating the datum's validity. Deadlines would have to be set for this exchange and debate of information, and for the refinement of policy alternatives. But by fostering mutual development and consideration of information about risk-carrying activities, the chances of joint problem solving may be enhanced.

Identifying Desired Information

The second task recommended to risk communicators is based on the notion that lay people hold different conceptions of pertinent information than do hazard managers.⁵² Yet risk communicators often assume that they know what information is needed by the message recipient. Because "the amount of information in a single message. . . is. . . unique to a single user," with this assumption an agency could only hope to meet all information demands by supplying all data it had; this would overwhelm the analytic capacities of recipients.⁵³ Identifying information desired by interested parties is thus more than just a ploy to placate them. It is necessary to avoid wasted resources in both risk communication and general hazard management. The assumption that experts know lay information needs is likely to be wrong in many cases.⁵⁴ It may also violate norms about the respective rights and responsibilities of institutions and citizens. After all, if citizens cannot be responsible for defining the information they want, what meaningful role can they possibly play in decision-making?

The importance of attending to citizens' information needs—defined by their social contexts—is highlighted by Sharlin's study⁵⁵ of the EPA's failed risk communication program about EDB. In line with professional norms, that program focused on aggregate risks, while citizens were interested in their personal risks. The public relief that followed EPA's ban of EDB emphasizes the earlier point that risk communication encompasses far more than the explicit process institutions label "risk communication."

Providing recipients with the technical and economic resources to produce their *own* information can help risk communicators ensure that all pertinent information is available to all parties, and that the decision-making process is acceptable.⁵⁶ Officials and experts will be concerned about the quality of information provided by other sources, particularly its putative lack of "objectivity." But we will not know whether non-official data can be useful until these institutions re-think their own interests so as to support its provision.

Identifying the Appropriate Communicator

The credibility of communicators will affect reception of risk messages. Appropriate selection of communicators can help to assure the most trustworthy person, group, or institution for the target audience(s) is responsible. But there are limits to the range of available and acceptable communicators. For example, the Department of Energy's general loss of credibility in siting radioactive waste repositories

may be offset by its near-monopoly of pertinent technical knowledge (in conjunction with the equally suspect Nuclear Regulatory Commission and nuclear industry). Politically credible state and local governments may lack technical credibility.⁵⁷ To these problems must also be added social context constraints set by statute, and organizational or professional refusal to relinquish control.

"Trust is perhaps the best-known social context factor in the hazard management field. Yet at the moment it is easier to say that trust is important than to define its components and how it grows, though it is probably multi-dimensional (arguments that it is solely a political or technical issue are likely naive). Furthermore, the presence of distrust has uncertain effects on risk communication, due to the heavy burden of inquiry, complexity and uncertainty it imposes on the distrusting people.⁵⁸ Trust may exist not because one has no reservations, but because the issue is too small or the need for expert action too immediate to behave otherwise.⁵⁹ In short, clearly defining what is meant by "credibility" for communicator selection will require more experience.

The ability of the hazard manager/risk communicator to provide political and economic resources to resolve communication problems is also important, since such resources are needed for appropriate distribution of rights and responsibilities. For example, agencies are unlikely to keep alternatives open (as discussed earlier) for siting of hazardous waste facilities in those states where government enters the process only after private firms have selected sites. In such cases the firm has control—usually by government default—of resources that the agency needs to do this particular risk communication job (e.g., through site negotiation).

Obstacles to Accounting for Social Context

Although integration of the social context approach with the technical approach should dramatically improve risk communication, there are some caveats. Given the obvious problems with a purely technical approach to such hazards as nuclear power and chemical waste, it can be tempting for advocates of social context to argue its primacy in all situations for all hazards. Unfortunately, a "theory" which purports to explain everything—particularly with the multi-dimensional definition of social context proposed here—is no theory at all. Specification of the conditions under which the theory does and does not operate is critical. Some constraints on the influence of social networks, economic resources, and political rights have been noted here, for example, but considerable empirical work will be needed to confirm and extend these speculations.

There are also several practical limits to implementation of a social context approach to risk communication. First, efforts to take into account the social context of message recipients may require more resources than agencies, firms, and professional associations are able or willing to give, even though without them the resources that are spent may be wasted. Second, information credibility may require an on-going relationship between the parties. With such a relationship any given piece of information is harder to reject, because past data from that source have been accepted and future data may be acceptable.⁶⁰ This suggests that "shotgun" risk communications to the general public should be deemphasized relative to targeting of organized groups (e.g., environmentalist or neighborhood groups). However, both officials and these groups may see long-term cooperation as too costly.

Third, it must be re-emphasized that the social context approach is alien to most government and corporate officials. It requires skills they lack and discounts those they have. In some situations it may also call upon them to yield professional and organizational power to ordinary citizens and other institutions. Resistance to such sacrifices can be expected, particularly since the success of social context approaches cannot be guaranteed in their current underdeveloped state.

Ultimately, these obstacles are likely to be overcome, because the current misunderstandings can serve only short-term interests of both the experts and the public,⁶¹ and technical risk communication has been unable to resolve this stalemate. Conscientious hazard managers will make sacrifices and take creative gambles in order to improve unsatisfactory hazard management. They can be helped by social scientists who are willing to make proposals that take into account the constraints on managers' ability to implement the social context approach whole-heartedly. Although this may mean less than satisfactory compromises in the short term, managers who experience success will be willing to try more creative experiments.

Conclusions

Short suggests that antagonists in hazard management may be forced to cooperate "in the search for solutions to mutually threatening conditions."⁶² Risk communication is a means for finding such solutions. But an exclusive emphasis on the conveying of technical risk information will founder on the implicit assumptions that only one kind of information from a single (expert) source needs to (and will) be conveyed. Risk communication is more than just communication about risks; in the end it encompasses the issue of how hazards are defined and controlled.

The social context approach seeks to rectify this problem by emphasizing the role of social networks, economic resources, political rights and responsibilities, history, and ideologies in the reception of risk messages. This approach is no panacea; the slowly growing recognition that technical approaches are insufficient is offset by our limited understanding of the specific impacts of social context and the difficulty of getting hazard managers to implement the social context tasks suggested here. But our need to explore new approaches will eventually overcome these obstacles.

It is important to recognize that no single approach will do. Attention to the conveyance of technical information must go hand-in-hand with attention to the role of contextual factors in affecting its reception, and in raising issues not addressed by the technical information. To assume that social context is all, and there is no objective knowledge that should be communicated, is as blind as assuming that only technical data are salient. Although the desire for generalization is understandable, rules for proper risk communication will tend to be situation-specific. Political issues are much more important than technical information in conflicts over hazardous facilities and hazardous wastes, but that does not mean that technical information is useless. In other situations power becomes less important, and technical data and other social context factors more so.

In short, despite its intellectual and practical challenges (and perhaps because of them), the middle way which respects the roles of both technical and contextual information in risk communication seems the most fruitful. It implies a partnership between citizen and expert in hazard management, which seems particularly appropriate in a society which lauds both science and democracy.

Notes

1. See, for example, Branden B. Johnson and Vincent T. Covello, eds., *The Social and Cultural Construction of Risk: Essays on Risk Selection and Perception* (Dordrecht, Holland: Reidel, 1987), and Branden B. Johnson, "The Social Construction of Science and Hazard Assessment" (Paper presented at the Annual Meeting of the Society for Social Studies of Science, Pittsburgh, PA, 23-25 October 1986).

2. Branden B. Johnson, "Risk Education and Hazard Management in the Workplace: The Role of Workers' and Executives' Social Relations" (Paper presented at the Annual Meeting of the Society for Risk Analysis, Boston, MA, 9-12 November 1986).

3. Branden B. Johnson, "Public Concerns and the Public Role in Siting Nuclear and Chemical Waste Facilities," *Environmental Management* 11 (1987): 571-586.

4. Vincent Covello, Detlof von Winterfeldt, and Paul Slovic, *Risk Communication: An Assessment of the Literature on Communicating Information About Health, Safety and Environmental Risks*, draft preliminary report to the Environmental Protection Agency (Institute of Safety and Systems Management, University of Southern California, Los Angeles, CA, 11 January 1986), p. 35.

5. Martha R. Fowlkes and Patricia Y. Miller, "Community and Risks at Love Canal," in Johnson and Covello, *Construction of Risk*, 55-78.

6. Branden B. Johnson and Bradley H. Baltensperger, "An Empirical Test of Alternative Hazard Perception Models" (Paper presented at the Annual Meeting of the Association of American Geographers, Minneapolis, MN, 4-7 May 1986).

7. John D. Powell, "Assault on a Precious Commodity: The Local Struggle to Protect Groundwater," *Policy Studies Journal* 14 (1985): 62-69; Janet Fitchen, Jennifer Heath, and June Fessenden-Raden, "Risk Perception in Community Context: A Case Study," in Johnson and Covello, *Construction of Risk*, 31-54.

8. See, for example, K. Jill Kiecolt and Joanne M. Nigg, "Mobility and Perceptions of a Hazardous Environment," *Environment and Behavior* 14 (1982): 131-154.

9. Ronald W. Perry, *The Social Psychology of Civil Defense* (Lexington, MA: Lexington Books, 1982).

10. Harry E. Moore, Fred L. Bates, M. V. Layman, and V. J. Parenton, *Before the Wind: A Study of Hurricane Carla*, NAS/NRC Disaster Study Number 19 (Washington, D.C.: National Academy of Sciences, 1963).

11. John H. Sorenson and Phil J. Gersmehl, "Volcano Hazard Warning System: Persistence and Transferability," *Environmental Management* 4 (1980): 125-136.

12. See, for example, David Morell and Christopher Magorian, *Siting Hazardous Waste Facilities: Local Opposition and the Myth of Preemption* (Cambridge, MA: Ballinger, 1982), 35-42; Fitchen et al., "Risk Perception."

13. Johnson, "Public Concerns."

14. See, for example, Paul Susman, Phil O'Keefe, and Ben Wisner, "Global Disasters, A Radical Interpretation," in Kenneth Hewitt, ed., *Interpretations of Calamity* (Boston: Allen and Unwin, 1983), 263-283.

15. Ian Burton, Robert W. Kates, and Gilbert F. White, *The Environment as Hazard* (New York, NY: Oxford University Press, 1978).

16. Sallie A. Marston, "The Political Economy of the Earthquake Hazard in California" (Paper presented at Annual Meeting of the Association of American Geographers, San Antonio, TX, 26 April 1982).

17. Roger E. Kasperson, "Six Propositions on Public Participation and Their Relevance for Risk Communication," *Risk Analysis* 6 (1986): 275-281; see also Peter M. Sandman, *Explaining Environmental Risk: Some Notes on Environmental Risk Communication* (Washington, DC: US Environmental Protection Agency, November 1986).

18. Richard J. Bord, "Public Cooperation as a Social Problem: The Case of Risky Wastes" (Paper presented at Annual Meeting of the American Association for the Advancement of Science, Chicago, IL, 14-18 February 1987); E. Brent Sigmon, "Achieving a Negotiated Compensation Agreement in Siting: The MRS Case," *Journal of Policy Analysis and Management* 6 (1987): 170-179; Johnson, "Public Concerns."

19. Steve Rayner and Robin Cantor, "How Fair Is Safe Enough? The Cultural Approach to Societal Technology Choice," *Risk Analysis* 7 (1987): 3-9.

20. See, for example, Gary L. Downey, "Federalism and Nuclear Waste Disposal: The Struggle Over Shared Decision Making," *Journal of Policy Analysis and Management* 5 (1985): 73-99; Sigmon, "Achieving."

21. Richard J. Bord, Philip J. Ponzurick, and Warren F. Witzig, "Community Response to Low-Level Radioactive Waste: A Case Study of an Attempt to Establish a Waste Reduction and Incineration Facility," *IEEE Transactions on Nuclear Science* 32 (1985): 4466-4471.

22. R. P. Savage, J. Baker, J. H. Golden, A. Kareem, and B. R. Manning, *Hurricane Alicia: Galveston and Houston, TX, August 17-18, 1983* (Washington, D.C.: National Academy of Sciences,

- 1984).
23. Fitchen et al., "Risk Perception."
24. See, for example, Mary Douglas and Aaron Wildavsky, *Risk and Culture* (Berkeley, CA: University of California Press, 1982); James F. Short, Jr., "The Social Fabric at Risk: Toward the Social Transformation of Risk Analysis," *American Sociological Review* 49 (1984): 711-725.
25. Rayner and Cantor, "How Fair," 3.
26. Covello et al., *Risk Communication*.
27. Ibid., 16-18.
28. See, for example, Baruch Fischhoff and Don MacGregor, "Judged Lethality: How Much People Seem to Know Depends Upon How They Are Asked," *Risk Analysis* 3 (1983): 229-236; Harald Ibrekk and M. Granger Morgan, "Graphical Communication of Uncertain Quantities to Non-Technical People" (Department of Engineering and Public Policy, Carnegie-Mellon University, Pittsburgh, PA, October 1986, manuscript).
29. Covello et al., *Risk Communication*, 7.
30. "Dioxin Screening Tests Alarm Missouri Residents," *New York Times*, 21 April 1983, Y-8.
31. Johnson, "Social Construction of Science."
32. Philip Shabecoff, "Uncertainties of a Chemical-Filled World Come Home to a Denver Suburb," *New York Times*, 19 April 1987, Y-19.
33. Karen De Witt, "As Consumers Save Water, Rates Go Up," *New York Times*, 29 October 1979, A-9.
34. Edward J. Walsh, "Challenging Official Risk Assessments via Protest Mobilization: The TMI Case," in Johnson and Covello, *Construction of Risk*, 85-101.
35. Clyde Haberman, "City Not Ready To Start Easing Curbs on Water," *New York Times*, 21 May 1981, B-4.
36. Behavior change should be emphasized in defining communication outcomes, because it is often a precursor (rather than consequence) of attitudinal change; see, for example, Irwin Deutscher, *What We Say/What We Do: Sentiments and Acts* (Glenview, IL: Scott, Foresman, 1973). Behavior can also be easier to change and observe than attitudes, and it provides a more concrete basis for possible negotiation about appropriate hazard management.
37. Joseph D. Novak, *A Theory of Education* (Ithaca, NY: Cornell University Press, 1977); Joseph D. Novak and D. Bob Gowin, *Learning How to Learn* (New York, NY: Cambridge University Press, 1984).
38. Bonney F. Hughes, *Knowledge, Beliefs and Actions of Elmira Water Customers Related to Groundwater, Contamination of Groundwater, and Toxicology*, M.S. thesis, Cornell University, January 1986.
39. Branden B. Johnson, "Citizen Risk Assessment," research proposal submitted to the National Science Foundation (Michigan Technological University, Houghton, MI, 1987).
40. See, for example, Robert Glaser, "Educating and Thinking: The Role of Knowledge," *American Psychologist* 39 (1984): 93-104.
41. Flood Loss Reduction Associates, *Prototype Local Flood Warning Plan* (Albany, NY: Department of Environmental Conservation, 1984).
42. Johnson, "Citizen Risk Assessment."
43. Carol Van Strum, *A Bitter Fog: Herbicides and Human Rights* (San Francisco, CA: Sierra Club Books, 1983), 163-165.
44. See, for example, Donald B. Straus, "Mediating Environmental, Energy and Economic Trade-Offs: A Case Study of the Search for Improved Tools for Facilitating the Process" (Paper presented at Annual Meeting of the American Association for the Advancement of Science, Denver, CO, 20-25 February 1977).
45. Lee Wilkins, *Media Coverage of Quick Onset Hazards: Toward a Definition of Memorable News; The Bhopal Example*, Final Report to the National Science Foundation (School of Journalism and Mass Communication, University of Colorado, Boulder, CO, December 1986).
46. Carlton Ruch and Larry Christensen, *Hurricane Message Enhancement* (Texas A & M University, College Station, TX, 1981).
47. Covello et al., *Risk Communication*, 22.
48. See, for example, Louisiana Department of Public Safety, *Southeast Louisiana Hurricane Study, Evacuation Behavioral Survey*, Final Report (Office of Emergency Preparedness, Baton Rouge, LA, 1984).
49. See, for example, Robert C. Mitchell and Richard T. Carson, "Protest, Property Rights, and Hazardous Waste," *Resources*, Number 85 (1986): 6-9; Johnson, "Public Concerns."
50. See, for example, Rayner and Cantor, "How Fair"; Ward Edwards and Detlof von Winterfeldt, "Public Values in Risk Debates," *Risk Analysis* 7 (1987): 141-158.
51. Michael O'Hare, "Information Management and Public Choice," in John P. Crecine, ed., *Research in Public Policy Analysis and Management*, Volume 1 (Greenwich, CT: JAI Press, 1981), 236.
52. Paul Slovic, Baruch Fischhoff, and Sarah Lichtenstein, "Facts and Fears: Understanding Perceived Risk," in Richard C. Schwing and Walter A. Albers, Jr., eds., *Societal Risk Assessment: How Safe Is Safe Enough?* (New York, NY: Plenum, 1980), 181-213.
53. O'Hare, "Information Management," 236, 240, and 245.
54. See, for example, Ian MacIver, *Urban Water Supply Alternatives: Perception and Choice in the Grand Basin, Ontario*, Research Paper No. 126 (Department of Geography, University of Chicago, Chicago, IL, 1970); Kerry Thomas, Elisabeth Swaton, Martin Fishbein, and Harry J. Otway, "Nuclear Energy: The Accuracy of Policy Makers' Perceptions of Public Beliefs," *Behavioral Sciences* 25 (1980): 332-344.
55. Harold I. Sharlin, "Macro-Risks, Micro-Risks, and the Media: The EDB Case," in Johnson and Covello, *Construction of Risk*, 183-197.
56. In addition to the earlier discussion on information and education, see O'Hare, "Information Management," and Kasperson, "Six Propositions."
57. Johnson, "Public Concerns."
58. Burkart Holzner and Evelyn Fisher, "Knowledge in Use: Considerations in the Sociology of Knowledge Application," *Knowledge: Creation, Diffusion, Utilization* 1 (1979): 223.
59. Sandman, *Explaining Environmental Risk*, 19-20.
60. O'Hare, "Information Management," 243-244.
61. Sandman, *Explaining Environmental Risk*, 22.
62. Short, "Social Fabric at Risk," 718, author's italics.

Reading Repertoires: A Preliminary Study of Some Techniques that Scientists Use to Construct Readings

Jonathan Potter
University of Loughborough

Abstract

This paper analyzes some sense-making practices scientists use when dealing with discourse (reading). The primary data base is a discussion between a group of psychologists of the transcript of an academic workshop which some of them had previously attended. Two procedures are identified. (1) Readings can utilize a device which contrasts what the transcript appears to show from what it actually shows (the "R/A [reality/appearance] Device"). (2) Readings can characterize the transcript as a document of disinterested scientific activities (using the "empiricist repertoire") or characterize it as a document of the working of social interests and personality (using the "contingent repertoire"). These findings are interpreted in a theoretical framework derived from the work of Roland Barthes.

This paper is concerned with the topic of reading: that is, with the ways social scientists make sense of, and account for, spoken and written discourse. This issue has provoked little interest from more traditional social researchers who have applied themselves to finding causal accounts of the nature of scientific belief¹ or to showing the lack of impact the "natural world" has on theory selection.² It is only when we start to consider a systematic analysis of scientific discourse that the central importance of this question becomes clear. On the one hand, discourse analysts have documented the heterogeneous nature of scientific texts.³ On the other, they have shown that divergent systems of terms may be used to account for the same scientific actions and beliefs.⁴

These findings tell us that texts and spoken discourse are far from being a transparent and straightforward medium of communication. Instead they start to show the way discourse, in science just as in other realms, is orientated to actions such as persuading, justifying and blaming.⁵ Given this, I want to ask: what practical procedures do social scientists use to deal with texts and to respond to spoken discourse? How do scientists achieve these interpretative tasks which form such a large part of their professional lives? These questions are particularly interesting given the pervasive variability which discourse analysts have observed in and between scientists' accounts, for their answer will go some way to explaining how this variability is accomplished in such a manner that it does not raise interpretative and interactional problems.⁶

I will explore these questions through examining the way a group of psychologists discuss the record

of an earlier scientific meeting which some of them had attended. The opportunity for collecting these materials was provided by a series of workshops held by personal construct psychologists. One of the workshops was recorded and the tape transcribed. Parts of this transcript were precirculated to participants at a second workshop, at which one session was devoted entirely to their discussion. This, in turn, was recorded and transcribed in full. The latter transcript is ideal for the present purposes because it consists of explicitly made interpretations of discourse from a number of different participants made under the scrutiny of their peers. Reading practices are here exposed for study. Before starting to look at these materials, however, I will examine in rather more detail the notion of reading.

Reading and "Reading"

One central theme of the discourse analysis which has emerged on the Continent is that literary texts should not be seen as direct representations or causal

Author Address: Department of Social Sciences, University of Loughborough, Loughborough, Leicestershire, LE11 3TU UK.

This paper has benefited from comments made at discourse analysis workshops by Andy McKinlay, Michael Mulkay, Greg Myers, Trevor Pinch, Peter Stringer, Teri Walker, Margaret Wetherell, Steve Woolgar, Anna Wynne, and Steve Yearley. The author also thanks Malcolm Ashmore for making detailed comments on an earlier draft of the paper.

Science & Technology Studies, 1987, Vol. 5, No. 3/4, pp. 112-121

products of some extra-linguistic entity. This argument is highly pertinent to the present analysis and has important wider implications for the theoretical conceptualization of scientists' discourse. For the purposes of this paper, however, I have ignored many of its complexities and the discussion is further simplified by focusing on the work of a single literary theorist, namely Roland Barthes.

Traditionally, Barthes argues, both analysts (literary critics and sociologists of literature) and lay people have wanted to explain the structure of texts by reference to a causal system existing "beyond" the text. This might be the intentions or biography of the author, the subtle workings of dominant ideology, or merely the makeup of the world. These resources are deployed to underpin the proper or definitive reading of the text; they legitimate one interpretation and close off competing ones. We can see similar kinds of explanatory moves paralleled in different traditions of meta-science, where scientists' texts are interpreted by making reference to "the facts" (for example, in many school textbooks, some history and philosophy), the psychology of scientists (in some sociology, history, and scientific biography) and the ideological context (in certain sociological perspectives).

Barthes, however, eschews this approach to reading and instead sets out the case for a semiological study of reading practices themselves. That is, instead of trying to produce an ideal reading, Barthes makes the process of production of readings itself the topic of study along with the unstated assumptions about what a reader is and what a text is. I will examine his argument with respect to authors, ideology and the world, in turn.

In the first case, although literary critics have persistently tried to use the intentions or proposals of authors as a way of producing definitive readings of texts, the attempt is doomed to failure. One obstacle is the pervasive and incorrigible problem of deciding what the author's real intentions are. Are they what authors *say* they are; or are they revealed in what authors *do*? If the latter, how are we to avoid inferring intentions from the nature of the work and, circularly, vice versa? If the former, *which* of the varied utterances should be taken literally? Indeed, do we not end up in yet another circle by wanting to use intentions to decide which utterances about intentions are right? There is no reason to suppose that the authors of scientific texts are any different from literary critics in this respect when they read.

More damaging, however, is the possibility, inherent in all texts, of reading them in different ways. To claim that only those ways "authorized" by a writer's intentions are acceptable is, Barthes argues, a moral or ideological claim; for non authorized readings are always available with any text. This is not to

say that versions of author's intentions or psychological makeup are not interesting; rather that such versions should have no *special* legitimizing function. They do not "close off" the possibility of alternative readings. As Barthes puts it:

It is not that the Author may not 'come back' in the Text, in his text, but he then does so as a 'guest'. If he is novelist, he is inscribed in the novel like one of his characters, figured in the carpet; no longer privileged. . . He becomes, as it were, a paper-author: his life is no longer the origin of his fictions but a fiction contributing to his work. . . .⁷

This is, of course, fully compatible with Barthes' general enterprise of producing a semiology of reading practices to replace the flawed interpretative "science" of criticism which is concerned with the goal of producing definitive readings of texts.⁸ Some variations in scientists' readings will be documented below.⁹

The argument about the role of ideology is very similar. Barthes rejects the position, which is represented in his own early work as well as in other places¹⁰, that readings are determined by ideology. For although ideology may be implicated in the formation of texts it cannot determine the way they are read. Take, for instance, Barthes' classic example: the Paris Match cover he sees while visiting the barbers. This shows a young black in French army uniform saluting the French flag. Barthes describes what the cover signifies:

that France is a great Empire, that all her sons, without any colour discrimination faithfully serve under her flag, and that there is no better answer to the detractors of an alleged colonialism than the zeal shown by this Negro in serving his so-called oppressors.¹¹

Yet in the same moment Barthes notes the ideological significance of the text, his own reading deconstructs it. As he dissects the operation of ideological processes he is certainly not a passive victim of these processes. In response to reflexive problems like these, Barthes argues in his later work that ideological processes influence the way texts work to construct the world, but do not prevent alternative readings such as his own.¹² Another way of putting this is to say that readers are not passive victims of the world views expressed in texts but actively contribute to the reading process. Readings are made, not merely received.

Barthes' argument about the relation between texts and "the world" and the way "the world" is used to warrant particular readings is a central part of his semiological perspective. It should be noted, however, that he is not attempting to solve one of the

basic problems of Western philosophy; his is not an epistemological argument for anti-realism. Rather, he is concerned with processes of sense-making in complex texts and the attempt by literary critics to read texts as literal descriptions of (sometimes imaginary) worlds. The most sustained critique of this "literal description" view is found in *S/Z*.¹³ Here Barthes takes an apparently straightforward realist text by Balzac and attempts to elucidate how it makes sense to the reader by splitting the story into fragments and showing how each functions in the complete text.

In particular, Barthes undermines the view that the text's sense is derived through processes of description and denotation. Instead the text acquires its meaning through the reader's bringing to it five cultural codes or accounting systems which embody an organized corpus of background knowledge concerning narrative, theme, character, cultural sociology and symbolism. The text cannot therefore be seen to have a single definitive meaning independent of the specific readings made by readers.

Overall, then, Barthes attacks the idea that the sense of a text is an unproblematic consequence of either the intentions of the author, the ideological processes that may have influenced the text's construction, or of the world that the text is taken to represent. In each case he changes perspective¹⁴ and treats these ideas as worthy of explanation in their own right. The resources drawn on by literary critics and readers for making readings are treated as interpretative procedures to be analyzed, as topics for study.¹⁵ Thus, although Barthes does not himself see authors' intentions, and ideology, as grounds for definitive textual exegeses, he nonetheless sees as illuminating the study of attempts to use them as grounds; and instead of treating "realism" as a product of acute and literal description, he treats it as a linguistic effect. The traditional question of how accurately a text describes is therefore replaced by a prior analytic question which asks how the organization of discourse within a text achieves the *effect* of merely describing.¹⁶

Resources and Repertoires

Barthes' argument has important implications for the study of social scientists' readings of their own discourse. Most basically, it suggests we look at scientists' readings as a topic in its own right rather than attempting to recover the literal meaning of scientific texts. And it shows the pertinence of the following questions: do scientists draw on certain resources to achieve and sanction readings? And if they do, what might these resources be? In other words, what are the similarities between the reading practices of scientists and the reading practices

which Barthes identifies as typical of literary critics and sociologists? To make these questions even more specific, I will concentrate on the way readings are produced by using the "empiricist" and "contingent" repertoires to which I now turn.

Recent research on scientists' discourse has shown that there are highly recurrent variations in the way scientists describe their own and others' actions and beliefs. There are differences, for instance in the way true and false beliefs—that is, beliefs the speaker claims are true or false—are characterized.¹⁷ True beliefs are depicted as straightforwardly arising from the experimental evidence, while errors are accounted for by referring to the intrusion of extra-scientific factors. Or take accounts of the role of criteria such as simplicity or testability in the selection of theories.¹⁸ Scientists describe criteria as determinate and clear-cut when characterizing their own theory choices; but when describing those of their competitors they treat criteria as socially contingent and open to strategic manipulation. Discourse analysts claim that these regular differences are constructed through the use of different linguistic repertoires. Briefly, the empiricist repertoire roughly corresponds to traditional conceptions of scientific rationality. Data are seen as arrived at by way of standardized, impersonal routines and are taken to provide a clear-cut criterion for selecting theories. The contingent repertoire, in contrast, recognizes the importance of a variety of social influences and takes facts to be dependent on fallible interpretative procedures.¹⁹

What has not so far been examined is the way these interpretative repertoires are utilized by scientists for dealing directly with discourse. Such utilization is implied in some of the studies that have looked at the use of these systems; clearly scientists do not have unrestricted and persistent access to each other's lives and workplaces, and even if they did, their understanding would be textually mediated. Thus spoken and written texts will inevitably play a central role in scientists' construction of versions of others' research, the history of their discipline, and so on.²⁰ Yet for the most part scientists' accounts take the form of direct descriptions of these things and make no reference to the texts and talk through which they are made available. By analyzing transcripts in which different participants are discussing in detail a single set of discursive materials it is hoped that some of the interpretative procedures used to construct readings will be made more explicit.

In the examples that follow the differences I will be examining are predominantly lexical, and centre on the alternative procedures that participants use to manufacture versions of actions and beliefs from the transcript. Construct psychologists at the workshop

may take a particular section of transcript to reveal actions which are, for instance, concerned with the disinterested development of theory. I will use the term *empiricist readings* to refer to this kind of construction. Alternatively a section of transcript may be taken to embody actions which are directed toward more personal, self interested goals rather than neutral scientific ones. I will call this kind of extract *contingent readings*. It is not the readings *themselves* which are contingent or empiricist, then, but rather these are attributes adduced by readers from the transcript; the text is read either as empiricist or as contingent. For example, statement X is read either as a description of an experimental technique or as an attempt at intellectual legislation by a dominant orthodoxy.

Now, it is clear that not all accounts describing the text in personal or social terms can be called contingent; for in certain situations personal or social processes may be quite separate from any issues of scientific relevance. For instance, if there is a break in the discussion for an argument about seating positions this might be totally separate from scientific questions of any kind and it would therefore be wrong to class it as contingent. Yet perhaps such an argument was a struggle for psychological supremacy which crucially influenced the outcome of the next hour's theoretical debate. The point is that there is no straightforward way for the analyst to decide. To resolve this difficulty, I have chosen to examine accounts in which participants characterize the *same* section of transcript in contrasting ways; and in particular where some participants offer an empiricist reading while others offer a contingent reading. In this paper, therefore, I will use the term *contingent* in a restricted sense to refer to those accounts which recharacterize empiricist versions using terminology from the contingent repertoire.

Analytic Materials

Given the perspective on reading described above, the focus of analytic interest is not some elusive inner experience that the reader has of the text. Rather, it is the readings explicitly displayed in accounts: a book review exhibits a reading of a book and referees' comments exhibit a reading of a submitted research article. In discourse of this kind recurrent and explicit reference is made to identifiable anterior texts.

The analytic materials for the current study consist of a transcript of a single session of a three day residential workshop of personal construct psychologists²¹ held at a South Coast seaside resort. The transcript records a loosely structured exchange among eleven participants which lasts about one and a half hours and runs to just over ten thousand words. This session was devoted to the discussion

of a section of another transcript from a previous, similar workshop. This transcript had been precirculated to participants accompanied by a letter suggesting that they might like to read it in preparation for a discussion. A number of the participants were common to both construct workshops: Dennis, Ian, Jonathan (the current author), Janthia, Mike, Neil and Richard. Some participants, however, were at the second workshop but not at the first: Ann, Frank, Chris, Shirley; and vice versa: Alan, Carol, Sue, Grant, James.

To simplify exposition, throughout the analytic section, the precirculated transcript will be referred to as *the transcript* and the commentary on that transcript as *the commentary*. To assist the identification of particular sections of extracts, each sentence is numbered; 2.4, for instance, refers to sentence 4 in extract 2.

Example One

To start with I will examine a comparatively straightforward empiricist reading of part of the precirculated transcript. In it the speaker is responding to two earlier speakers who have identified certain themes. One of these themes concerns the nature of the processes actually occurring within the group of psychologists, and the other concerns the ideal group which would be implied by construct theory (a "Kellian group" - named after construct theory's founder, George Kelly). The speaker, Mike, takes the previous speaker to be seeing these things as equivalent, and disagrees with this equation.

Commentary Extract 1

Mike. 1Um, I think that these two things are different and they are playing out against each other. 2I mean we are using the group to think about the nature of a Kelly group, and then we are [laughs] using the nature of a Kelly group to think about how the group was operating. 3I mean they are, I thought it was working—I mean, reading it—it works quite nicely. 4We are both using the direct shared experience of everybody who has been there and yet trying to produce a more generalized view of what, what Kelly thinking would do to group dynamics. (Commentary, 8-9)

In sentence 1 of the commentary Mike formulates the two themes—the processes occurring in the group and the nature of a Kellian group—as separate and suggests that there is a tension between them. This tension, however, is positive: "it works quite nicely" (3). Each is being used to throw light on the other: the participants' "shared experience" of the group is being used as a resource for explicating the nature of

a Kellian group, and their theoretical understanding of what a Kellian group would be like is used to elucidate their specific group interaction (2 and 4).

In this extract, therefore, the speaker takes the transcript to reveal what is going on in the previous workshop. Three sorts of activities are identified:

(A) *Using*—the actual group, a hypothetical notion of a group;

(B) *Thinking*—about the nature of a Kellian group, about the activities of the particular group;

(C) *Producing*—a more generalized idea of the application of Construct Theory to group dynamics.

In addition the transcript is described in more global terms as showing the activities in A and B successfully “playing out against each other” (1). Overall, then, this speaker distinguishes between the theme concerning social processes occurring in the group and the theme concerning the content of the group’s discussion. And he sees a successful and reflexive interaction to be taking place between them.

It is also interesting to note that at the point where the speaker appears to be making a direct evaluation of the group’s success he corrects himself and emphasizes that his understanding is derived from *reading* the transcript (3). In a preliminary way this speaker formulates a “gap” between the transcript and the *actual* actions of participants. He thereby starts to raise the possibility that the actual actions might be other than they appear from the transcript, and in so doing points to the fallibility of his interpretation. In extracts below we will see this idea more elaborately formulated.

Example Two

In the next extract the interpretation of the transcript is more complex. Again the speaker characterizes the actions which the transcript reveals happening at the previous workshop. This time, however, two different versions of these actions are produced. The transcript is described as *explicitly* saying one thing which hides a more significant *implicit* message.

Commentary Extract 2

Dennis. 1Yes, I mean, reading it again I got a sense that some of the tension in it is to do with, er, [laughs] to do with the way in a sense you can cheat slightly and we do. 2That is, you can, you can be actually making a remark about the behaviour of other people in the group, in the sense of not wanting it, or regretting its being there, but you can frame that into saying

that an ideal Kellian group would be like this. 3And the unsaid part is: “but not like what you’ve been.” 4And a couple of times you, and I think I, and I don’t know if anyone else as well, referred to other groups we had been in. 5Er, and that was quite interesting, because it sort of again it [Mike laughs] was said: “isn’t it sort of interesting, I have been in a group that works like this.” 6But you could see behind that the possibility of saying “and why haven’t you buggers!” (Commentary, 9-10)

The first speaker in this passage, Dennis, starts by alluding to the “tension” mentioned by the previous speaker (extract 1). However, in this case *tension* is given a rather different meaning. The previous speaker treats tension as a creative product of the theoretical analysis and the group process being used to illuminate each other. In contrast, Dennis treats tension as a consequence of the fact that participants can and do “cheat slightly” (1). This cheating consists of using points which appear to be to do with the nature of Kellian groups to make points which are actually critical comments of a personal nature on the behaviour of other group members (2). These implicit points are characterized as a sort of hidden speech, or subtext. Thus a comment that “an ideal Kellian group would be such and such” is depicted as conveying the implicit message “but not like what you’ve just been” (3). Dennis gives two different examples to illustrate this dichotomy between what is actually said and what is apparently said (sentences 2 and 3, 5 and 6). In both cases, the ostensive meaning is given along with a translation which provides the real meaning.

Dennis thus formulates two versions of the transcript being discussed. One is what it *appears* to say and the other is what it *really* says. The real message of the speech is seen as implicit. Yet it is clearly thought to be understood by at least some of the other participants—Dennis is not describing a solipsistic joke on his part. I will refer to this discursive technique as the R/A (reality/appearance) device.²²

Although Extract Two uses the R/A device to separate a warranted from an unwarranted version, while Extract One hardly raises the possibility of competing versions, in each case versions are constructed using interpretative repertoires commonly available in science. Thus, with Extract One, the empiricist repertoire is deployed to produce a reading in terms of standard scientific narratives of theoretical development and increased understanding. The repertoire acts as a code for sense-making in the same way that the five cultural codes allows Barthes’ reader to make sense of Balzac’s story. Furthermore, just as Barthes stresses that reading is a radically constructive activity, where sense is produced from

texts, we see social scientists constructing widely different readings from the very same section of transcript. Thus, in Extract Two, we see the contingent repertoire deployed to produce a reading in terms of personal motivations and group dynamics. Again, the repertoire can be seen as a culturally available code for constructing readings.

It would overburden the present analysis to engage in a detailed comparative study of these readings and the transcript they refer back to. It would also force a discussion of the important but complex issue of reflexivity²³ which would be tangential to current concerns; for I would need to discuss the differences between my readings and those of the participants.²⁴ However, the sections of transcript which make up the topic of Extracts One, Two, and Three are reproduced in Appendix A. This allows readers to inspect the relation between the commentary and the precirculated transcript.

Example Three

I will now examine a third reading which uses the R/A device. Again both contingent and empiricist versions of the actions revealed by the text are formulated. In this case, however, the R/A device is used to display an empiricist reality behind a contingent appearance. This is the inverse of the previous example, and in many ways is a rather unexpected occurrence. Both scientists and sociologists of science more commonly claim that what *looks* properly empiricist is really the veneer over some more contingent process (expressions of ideology, bids for promotion and so on). The appearance of examples such as the following in scientists' discourse attests to extreme interpretative flexibility which is facilitated by the combination of two basic repertoires and the R/A device.

In the example that follows, Ian suggests that one of his points *appears* to be a self-interested contribution to the psychological processes going on in the group. Yet, he claims, it is *really* concerned with questions of a theoretical nature.

Commentary Extract 3

Ian. 1I think there's a triple sort of complication there. 2I mean I look at myself saying that there and think, well I just, I chose a very silly example. 3You know, what I was trying to do was to say something about theory and about what construct theory has to say about groups. 4And then chose as an example something from the here and now, which is of course bound to end up in a spiral of infinite complication. 5And I think Richard is quite right to say keep it away from that. 6I mean, one should, one should choose examples, I mean,

if you choose an example from the here and now it obviously becomes a complicating factor, doesn't it. *Jonathan.* 7So you read Richard there as saying "keep it away from the here and now?" *Ian.* 8Well, I was certainly not wanting to get into the here and now. 9I was wanting to get some ideas about how construct theory in groups, you know, at a, a theoretical level. 10I chose an example from the here and now because, you know, you think "oh well, it's nice and clear and everyone can see it." 11 [laughs] But, of course, everybody can't see it, because it is part of the very engine that is going on at that very moment. 12It's clear isn't it, it's nice, you see what four, my first bit on four B is basically talking at the level of theory. 13OK. 14And in line four I take and James as an example. 15OK. 16Then Richard takes that as being not interesting, but that, I wasn't particularly interested in it; only as an example. 17I wasn't interested in it as a piece of interpersonal business to be sorted out. 18So I think you were right to do that. 19And then I think I go along with what Richard is saying in practice, because on five A I am actually continuing to talk theory. 20And not interested in getting into the examples either. 21I think that that statement there of mine is what I was trying to say in a nutshell: that, you know, that goes back to an earlier distinction and that is the distinction between "up there" and "right here." 22And what I am trying to say about groups is that it is a very concrete business. 23And that to try and make out that it is some kind of floating thing up in the ceiling is, is um, something I wanted to disagree with at the time. 24Does that clarify it? (Commentary, 12-14)

In his use of the R/A device Ian's point is the inverse of Dennis' (Extract Two). It suggests that one of his statements which has been read as, and *appears* to be, contingent is *really* empiricist. Ian brings out his contrast most strongly in lines 12 to 20. He identifies his speech in the precirculated transcript and says that it is "basically" concerned with theory (12). Furthermore, he characterizes his point in that transcript about the dispute between himself and another participant, James, as an "example" of his theoretical point; he is not interested in it for its own sake (15). Ian then turns his attention to the response to his transcribed points from Richard. He characterizes Richard as reading them as "not interesting" (17) because they are a "piece of interpersonal business" (18) which has to be sorted out. Yet he claims in the discussion that his earlier transcribed self is also not interested in them for this reason (17-18); he is only interested in them "as an example" which illustrates his theoretical point. And he goes on to give

a gloss which depicts a particular piece of speech as summarizing the whole "in a nutshell" (22-26).

Overall, then, Ian contrasts what he sees as Richard's interpretation with his own "proper" understanding of the transcript. It is interesting to note, however, that he goes to some lengths to make Richard's misreading accountable. He points out a number of reasons why Richard should (mis)read his points as an attempt to contribute to the "interpersonal business" rather than as a contribution to the theoretical discussion. For instance, he says that he was "silly" to choose an example from the here and now to illustrate a theoretical point, because it was "bound to end up in a spiral of infinite complication" (4-5). And he says that Richard is "quite right" in saying that they should not get involved in that (6), a claim he reiterates in sentence 19. So although he does not totally endorse Richard's reading—which he sees as encouraging him not to use examples from the processes in the present group; he depicts it as both understandable and having a positive effect. Ian identifies Richard's misreading as one which could be made by any reader not in possession of specialized knowledge or prepared for the degree of reflexivity which Ian has introduced.

Reading and Reality

In the course of the analysis I have discussed a number of ways in which participants characterize text. In particular I have examined the use of what I have termed the R/A device and the way these social scientists draw on the contingent and empiricist repertoires when describing discourse. I will discuss these topics in turn.

Participants have the option of treating any particular section of discourse either as a kind of transparent medium which provides direct access to the actions of participants or as an opaque medium which can conceal these actions. In the former case, a speaker will simply list or describe the acts which may be "seen in" the transcript. Extract One is of this type. The speaker uses a number of act and action categories to describe what the transcript reveals is "going on" in the earlier workshop—for example "using" a hypothetical notion of a group. Such an account is not merely a reformulation of what is said, it also starts to describe what the saying is for, what is done by what is said. The utterances discussed in Extract One are taken by the speaker to be *doing* certain sorts of theory development.

An alternative way in which participants treat the discourse is to see it as concealing those actions which do occur. In this case the text will appear to embody actions; yet these are not the actual actions of participants. It is this form of accounting

I have described by the term *R/A device*. Such accounts appear in Extracts Two and Three. In these cases two versions of the actions embodied in the text are proposed. One of these is depicted as obvious to the reader or as the version which would be arrived at by taking the text at face value. The other is depicted as concealed by this obvious version: it is implicit but not yet apparent. Extract Two shows how this works. The speaker characterizes a section of transcript as appearing to embody a statement about the ideal form of group predicted on theoretical grounds. However, the speaker goes on to say that the text actually embodies criticism of the behaviour of other members of the group. This device thus suggests superior knowledge or skills on the part of its user. It implies that the user is able to penetrate the appearance to reveal what actions are in fact occurring.

Clearly such a device presupposes two readers: the speaker and some notional or implied reader. In the different instances of use of the R/A device, above various notional readers are suggested. For instance, in Extracts Two and Three the notional reader who recovers only apparent actions is equated with a generalized reader who is without special knowledge and reads the text innocently. In Extract Three, however, a specific reader is also introduced. Here Ian characterizes Richard's readings as only recovering what the text appears to say (only those actions which the text appears to embody) not what it actually says (the actions it actually embodies).

What, then, might be the role of the R/A device in scientific discourse? I have only looked at a small number of instances here; but, assuming these are indicative of a more widely drawn-upon device, what functions might it serve? In the first place it is likely to be used in the construction and rejection of versions of events, particularly in instances where the speaker is dealing with a version of events taken to be obvious or natural. In such cases simply disagreeing with the version, or simply producing a competing version, might well be ineffective, for this response would not deal with the question of the alternative's obviousness. At best such an approach would succeed in producing a viable competitor. However, the R/A device works by undermining one version in tandem with the production of an alternative. Moreover, it addresses the issue of the obviousness of the competing account. Instead of trying to contradict this obviousness, it is accepted. The R/A device, in its formulation of a reality which lies "behind" appearances, distinguishes obviousness from correctness. Indeed, it specifically formulates the obvious as equivalent to incorrectness.

A further important feature of the R/A device is the flexibility in accounting which it facilitates. In

the examples analyzed above there are typically important differences between the two versions formulated; one version is contingent and the other is empiricist (although I do not wish to suggest that the device is restricted to opposing contingent and empiricist versions). The actual degree of flexibility will be a practical issue which will depend on each specific context of use. However, it does seem that the R/A device is suitable for dealing with highly disjunctive versions. There is no reason to suppose, of course, that such accounting will necessarily be successful; other participants may not be convinced of the reality implied by the use of the R/A device. Further work might usefully compare those occasions when the R/A device is effective with those when it is not. However, its use does seem to be one practical, if not omnipotent, solution to certain interactional problems.

Finally, the device provides one way in which the problem of competing versions can be resolved in practice. For instead of discussion and dispute amassing increasingly large numbers of competing versions of events, versions are continually rejected as being of appearances only. The notion of a real version which may or may not lie behind appearances is reinforced by the use of the R/A device. It thus maintains the idea of a definitive truth while explaining away the existence of a multitude of inconsistent versions of it. In this sense, the R/A device can be seen as a procedure for factual accounting, a standard discursive form used in the production of "descriptions."²⁵

Apart from its practical importance to participants, the R/A device is theoretically significant to analysts of science discourse. One of the central findings of detailed work on scientists' talk and documents is that they are highly fragmented and variable²⁶, and yet this variation has a highly regular organization to it. Scientists draw on different repertoires depending on the requirements of the interpretative context; for example, the empiricist repertoire is used for warranting one's experimental claims²⁷; the contingent for accounting for error²⁸ and so on. One of the predictions of this theoretical approach is that difficulties are likely to emerge when two inconsistent repertoires are used together and that special techniques may be available for resolving these difficulties. In the above analysis we see how the R/A device can be used for resolving potential contradictions between versions, thus providing further confirmation of the utility of the discourse-analytic perspective.²⁹

I will now return to the issue with which I introduced this paper, namely that of resources being used to construct definitive versions of actions embodied in texts. As we saw, Barthes has suggested that readers and critics draw on particular kinds of resources when producing readings of literary texts.

In the analysis above, I have concentrated on the use of two specific resources: the contingent and empiricist repertoires.

In what way can these repertoires be seen as resources in the sense outlined by Barthes? As I have noted, the participants generally took the pre-circulated transcript to be a document of specific acts and actions. Giving a reading therefore involves providing an interpretation of what acts and actions are performed by or through what is said. Frequently such interpretations involve specifications of acts using the contingent and empiricist repertoires. For example, talk might be interpreted in empiricist terms as a development of theory (Extract One) or in contingent terms as an interpersonal criticism couched in the language of theoretical commentary (Extract Two). The participants draw upon the two repertoires to give definitive readings and close off alternative readings.

Perhaps the most precise analogy is with the cultural codes described in *S/Z*.³⁰ Each of these consists of an organized corpus of background knowledge about, for instance, character and personality. Through the use of these codes the reader makes sense of the text; indeed the text cannot be properly considered to have a meaning outside of these constructive acts of sense making. The empiricist and contingent repertoires can similarly be considered to be codes or semiotic systems for making sense of transcript and other forms of scientific discourse. It is these codes which allow the reader to construct actions and complete acts from discourse. They are resources for accomplishing readings. Although at times participants appear to treat this move as natural and unproblematic, the production of competing readings exposes its interpretative nature. What emerges from this analysis, then, is the close similarity between the reading practices of scientists and traditional literary critics and the relevance of recent developments in literary theory³¹ to the study of scientists' discourse.³²

More generally, these interpretative repertoires can be seen as linguistic resources out of which the distinctive forms of scientific interaction and sense making are constituted. Other studies show how the interactional use of these systems provides the distinctive organization to, for example, Nobel Prize speeches³³ or conference question time.³⁴ In the current study we see how actors reproduce the social world of science—complete with motives, causes and social processes—from scientific texts. The repertoires are resources which enable this reproduction and provide its recognizable form.

Overall I have attempted to illustrate a novel approach to the study of scientists' reading. This is not concerned with revealing some inner experience of the text by the reader. Instead I have suggested that

insofar as scientists are explicating, interpreting, and commenting upon written texts they are publicly displaying techniques of sense-making in the production of versions of the text's meaning. It is these versions which I have called "readings." There is probably no hard divide between readings of this kind and more general interpretative practices. In each case what is available for analysis is a sequence of texts. It is convention only which leads us to call one text which appears to refer back to another a reading of that other text.³⁵ The analytic topic becomes the way these texts are constructed and the differences between them. In this paper I have concentrated on the way readings construct empiricist and contingent versions of activity and the role of the R/A device in these constructions.

Notes

1. B. Barnes and S. Shapin, eds., *Natural Order: Historical Studies of Scientific Culture* (London and Beverly Hills: Sage, 1979); and D. Bloor, *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976).
2. H. M. Collins, ed., "Knowledge and Controversy," special issue of *Social Studies of Science*, Volume 11 (1981), and H. M. Collins, *Changing Order: Replication and Induction in Scientific Practice* (London and Beverly Hills: Sage, 1985).
3. C. Bazerman, "What written knowledge does: Three examples of academic discourse," *Philosophy of the Social Sciences* 11 (1981): 361-387; J. Gusfield, "The literary rhetoric of science: comedy and pathos in drinking driver research," *American Sociological Review* 14 (1976): 16-34; G. Myers, "Texts as knowledge claims: The social construction of two biology articles," *Social Studies of Science* 15 (1985): 593-630; S. Woolgar, "Discovery: Logic and sequence in a scientific text," in *The Social Process of Scientific Investigation*, ed. K. Knorr-Cetina, R. Krohn, and R. Whitley (Dordrecht: Reidel, and S. Yearley, "Textual persuasion: The role of social accounting in the construction of scientific arguments," *Philosophy of the Social Sciences* 11 (1981): 409-435.
4. G. N. Gilbert and M. Mulkay, *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse* (Cambridge: Cambridge University Press, 1984); and B. Latour and S. Woolgar, *Laboratory Life: The social construction of scientific facts* (London and Beverly Hills: Sage, 1979).
5. J. Heritage, *Garfinkel and Ethnomethodology* (Cambridge: Polity Press, 1984).
6. A. McKinlay and J. Potter, "Model discourse: Interpretative repertoires in scientists' conference talk," *Social Studies of Science* 17 (1987): 443-63; E. R. Fuhrman and K. Oehler, "Discourse analysis and reflexivity," *Social Studies of Science* 16 (1986): 293-307; J. Potter, "Discourse analysis and the turn of the reflexive screw: A response to Fuhrman and Oehler," *Social Studies of Science* 17 (1987): 171-7.
7. R. Barthes, *Image - Music - Text* (London: Fontana, 1977), 161.
8. J. Culler, *The Pursuit of Signs: Semiotics, Literature, Deconstruction* (London: Routledge and Kegan Paul, 1981).
9. For a fascinating examination of the way scientific discoveries are constructed through reading, see also G. Myers, "The Adenovirus story: Narratives of the discovery of split genes" (Paper presented at 7th Workshop on Scientific Discourse, University of Bradford, 22-3 April, 1987).
10. Barthes is not explicit, but is probably referring here to the work of Goldmann and Lucacs. See C. Slaughter, *Marxism, Ideology and Literature* (London: Macmillans, 1980).
11. R. Barthes, *Mythologies* (London: Paladin, 1972.)
12. See in particular R. Barthes, *S/Z* (London: Jonathan Cape, 1975).
13. Ibid.
14. S. Woolgar, "Changing perspectives - A chronicle of research development in the sociology of science," in *Sociology of Science Conference Budapest 1977*, ed. J. Farkas (Budapest: Publishing House of the Hungarian Academy of Sciences, 1979).
15. Compare with the ethnomethodological move from resource to topic: D. Zimmerman and M. Pollner, "The everyday world as a phenomenon," in *Understanding Everyday Life*, ed. J. Douglas (London: Routledge and Kegan Paul, 1971).
16. The analogy between these theoretical developments and moves in ethnomethodology has not gone unremarked. See C. C. Lemert, "De-centred analysis: Ethnomethodology and structuralism," *Theory and Society* 7 (1979): 289-306; J. Potter, P. Stringer, and M. Wetherell, *Social Texts and Context* (London: Routledge and Kegan Paul, 1984).
17. M. Mulkay and G. N. Gilbert, "Accounting for error: How scientists construct their social world when they account for correct and incorrect belief," *Sociology* 16 (1982): 165-183.
18. M. Mulkay and G. N. Gilbert, "Opening Pandora's Box: a case for developing a new approach to the analysis of theory choice in science," *Knowledge and Society* 5 (1984): 113-139; and J. Potter, "Testability, flexibility: Kuhnian values in scientists' discourse concerning theory choice," *Philosophy of the Social Sciences* 14 (1984): 303-330.
19. These interpretative repertoires are more extensively documented in Gilbert and Mulkay, "Accounting for Error"; J. Potter and M. Mulkay, "Making theory useful: Utility accounting in social psychologists' discourse," *Fundamenta Scientiae* 3/4 (1982): 259-278; J. Potter and M. Wetherell, *Discourse and social psychology: Beyond attitudes and behaviour* (London and Beverly Hills: Sage, 1987); and S. Yearley, "Vocabularies of freedom and resentment," *Social Studies of Science* 15 (1985): 99-126.
20. Compare with D. Smith, "Textually mediated social organization," *International Social Science Journal* 36 (1984): 59-75.
21. Personal construct psychology is often considered a slightly marginal area within academic psychology, although a significant number of researchers are advocates of the approach. It is somewhat more established within the field of clinical psychology where a number of the leading figures are placed. This workshop took place in a magnificently situated Lakeland hotel over a period of three days. There was no fixed agenda. Instead a number of themes were proposed at the start and discussion loosely organized around them. One figure, a key person within UK personal construct psychology, described it thus: "The whole workshop remains in my mind as a fantastic experience. I have never before been involved with a number of people with a common interest in such an unstructured situation for such a long period of time." Each session was tape recorded, with all participants agreeing that their talk could be used for research. From this tape a verbatim transcript was made of about 130,000 words.
22. A similar distinction is made in P. Elgin, "Resolving reality disjunctures on Telegraph Avenue: A study of practical reasoning," *Canadian Journal of Sociology* 4 (1979): 359-375.
23. See M. Ashmore, "A question of reflexivity: Wrighting sociology of scientific knowledge," unpublished D.Phil. Thesis, University of York, UK, 1985; M. Mulkay, *The Word and the World* (London: Allen Unwin, 1985); P. Stringer, "You decide what your title is to be and [read] write to that title," in *Issues and Approaches in Personal Construct Theory*, ed. D. Bannister (London: Academic Press, 1985); and S. Woolgar, "Irony in the Social Study of Science," in *Science Observed: Perspectives on the Social Study of Science*, ed. Knorr-Cetina, K. D. and Mulkay, M. (London and Beverly Hills: Sage, 1983).
24. A discussion of this kind using data similar to that explored in this paper can be found in J. Potter, "What is reflexive about

discourse analysis? The case of reading readings," in *Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge*, ed. S. Woolgar (London and Beverly Hills: Sage, 1987).

25. See Heritage, Garfinkel, and D. Smith, "K is mentally ill: The anatomy of a factual account," *Sociology* 12 (1978): 23-53. Another way of looking at the R/A device is as a technique for resolving potential reality disjunctures and maintaining the primacy of mundane reason: See M. Pollner, *Mundane reason: Reality in everyday and sociological discourse* (Cambridge: Cambridge University Press, 1987).

26. Gilbert and Mulkay, *Pandora*, and Latour and Woolgar, *Laboratory Life*.

27. G. N. Gilbert and M. Mulkay, "Warranting scientific belief," *Social Studies of Science* 12 (1982): 382-408; M. Mulkay and G. N. Gilbert, "Scientists' theory talk," *Canadian Journal of Sociology* 8 (1983): 179-97.

28. Mulkay and Gilbert, "Accounting for Error"; Potter, "Testability, flexibility."

29. See also Gilbert and Mulkay, *Pandora*, chapter 5.

30. Barthes, *S/Z*. See also C. Belsey, *Critical Practice* (London: Jonathan Cape, 1981).

31. Stringer, "You decide."

32. Potter et al., *Social Texts and Context*.

33. M. Mulkay, "The ultimate compliment: A sociological analysis of ceremonial discourse," *Sociology* 18 (1984): 531-49.

34. McKinlay and Potter, "Model Discourse."

35. Potter et al., *Social Texts and Context*.

Appendix A

The following passage is the topic of Extracts One and Two.

Dennis. I have worked regularly in a group that meets fortnightly, and one thing [] I have noticed is that we are developing and getting very adept at—and it is to do with, I think, essentially with the notion of people's experiments—is quite quickly as it were one or other will think of a form for a quick experiment, you know. And it can be thrown in, and there almost now seems to be an agreement that you never resist a form, you know, even if that doesn't particularly (*Mike.* Um, um.) attract you; that is not an issue.

Mike. I think that is interesting, because I think the group made a decision not to go along with that form. I mean, I consciously had decided I was not going to name my form (*Dennis.* Yes.) yesterday evening [laughs].

(Discussion, 6/1(i)/1-2)

The following passage is discussed in Extract Three.

Richard. I think I saw this group in the terms in which I have seen other groups, and *would* want to see other groups in the future, which is simply as being a set of constructs. OK. And these constructs are not in any simple way the constructs that are held by each individual. They are a set of constructs that are, as it were, pushed up into the air—I mean, I think of it very physically—by people talking to one another. [] I mean the—this is my perspective, admittedly—it's success as a Kellian group in the way in which I would want to see it is that people do take away that set of constructs and identify it, for the sake of economy, or sort of cognitive processing and all the rest of it, as the set of constructs that were thrown up by the construals of particular people in a particular place in a particular time.

Ian. Yeah, that's the sort of thing; I mean the typical sort of thing that you get in human behaviour, or whatever you want to call it, is that in different contexts people will do different things. Now, it might be that me and you, James, argue about something—OK—and you are defending so and so and I am attacking it. It might be that next week somebody is attacking your position and I will come in and I will really defend your position. [*James.* Um.] You know, because I have taken away the entire set of alternatives that has been generated by this group and I then use it. And I have become bigger as a result of being with this group.

Richard. I am not interested—sorry, just to come back in an egocentric way—that does not fit what I said. I am not terribly interested in what happens to you or what is happening to you and that's why I am just going back to where we started in this bit of the discussion; you and James is really not terribly interesting.

Ian. Yes, well, I was just trying to illustrate what you are saying.

Richard. Well it doesn't illustrate what I am trying to say from the point of view of a Kellian group. [*Ian.* Um.] Because the set of constructs that constitutes a Kellian group for me is at a totally different level from what is happening between the two of you. That's why I think it is very unprofitable to start talking about a Kellian group in terms of what's happening between two people, because one very quickly gets locked into that way of seeing the group.

Ian. I don't see how you can separate them; you are talking about group constructs, or whatever you want to call them, as being up there in the air, you literally went like that [gestures] and I don't think they are up there; I think they are between us; I think they are very concrete; they are acts.

Richard. Yes, but they are not between you and James.

Ian. That was just an example.

(Discussion, 6/1(i)/5-6)

Agenda Battles and Practical Cooperation in Science Development

Michael J. Moravcsik
University of Oregon

The following three articles continue the discussion in the pages of this journal about an appropriate research agenda on science and technology in developing countries. First, Michael Moravcsik responds to Stephen Hill's critique of his original proposal, which appeared in the 4S Review in 1985. Then Yehouda Shenhav offers empirical evidence suggesting which perspective has been put into practice in less developed nations (LDCs). Finally, Richard P. Suttmeier points out the incompleteness of both Moravcsik's and Hill's agendas, and proposes heightened awareness of the contributions of economics and political science to research on this topic.

This is in response to Stephen Hill's commentary *Science & Technology Studies* 4 (1986): 29], which speculates on the existence of a "hidden agenda" in my article *4S Review* 3 (1985): 2-13], presents Hill's own conceptual agenda, and proposes topics for study in the area of science and technology in the developing countries. My response will be in two parts: a conceptual discussion and a commentary on a practical, programmatic level.

To start with the conceptual part, I have no hidden agenda. I have always been quite an open person, who has written a lot on science development (too much, perhaps), has spoken frequently on the subject (sometimes too much for my own good), and has been eager to respond to questions. In general, it is not productive to speculate on my hidden agendas: What I believe on the conceptual plane is all laid out in the open.

The problem of being attributed with a hidden agenda is that the list of things I do *not* believe is very long. But to clear the air—and as an example—let me say explicitly that I do not, and never did, believe in "cargo cults." Cargo cults are the worst form of one-dimensional thinking, because they assume that one action is a *sufficient* condition for a desired result to happen. As somebody who has preached multi-dimensional thinking for some time, I could hardly have a "secret one-dimensional agenda."

On the positive side, however, I do believe that the existence of a functional scientific and technological infrastructure is a *necessary* (though not *sufficient*) condition for science and technology to have an impact in a country. If you do not have something, you cannot apply it. This is hardly a far-out assumption; yet it does not, in my view, receive sufficient attention.

Let me now turn to Hill's own conceptual scheme. It is a venerable one, frequently heard, and subscribed to by many "development agencies," nationally and internationally, including the CASTASIA activity cited by Hill. This latter is a formalistic exercise on the part of people who often hold considerable power in their own countries but who are, both by background and by their daily activities, far removed from the realities of science and technology and of the links of these with the various other aspects of these countries.

The Hill framework has no secret agenda either. Its assumptions are articulated explicitly and frequently. For our purposes, some of these are as follows:

- 1) Among the effects of science and technology, the only one of importance in developing countries is the economic one.
- 2) Of the economic effects of science and technology, one should consider only the short term ones, in view of the great present and burning material problems the developing countries have.
- 3) The problem with science and technology in developing countries is that the science practiced there is irrelevant, "basic," untargeted science pursued in subservience to "Western" science, and not the useful, targeted, applied science that is "relevant" to the country.

Author address: Institute of Theoretical Science, University of Oregon, Eugene, Oregon, 97403 USA.

Science & Technology Studies, 1987, Vol. 5, No. 3/4, pp. 122-123

As to the first two of these, they represent simpleminded, narrow, one-dimensional thinking. Though they are related to *some* of the truth, this segment of truth is so far from the *whole* truth that in isolation it represents untruth. The point of view is also (unintentionally) quite condescending to the developing countries. But most importantly, since it is a simpleminded picture of a complex, multidimensional problem, it is also ineffectual in practice. In particular, this view perpetuates the dependence of the developing countries on the countries that have the know-how and hence the influence.

As to the third of the above points, it is a superstition not substantiated by facts. In reality, as far as the *input* of science and technology is concerned, in virtually all developing (or developed) countries the lion's share (80-90%) of the resources is devoted to targeted, *applied* scientific research and technological development. In the developing countries, these funds usually go into large governmental laboratories, explicitly proclaimed to be institutes for applied research and development, and loudly "targeted" for "useful" and "relevant" projects. Interestingly, there is hardly ever a meaningful evaluation of the *output* of these institutions, perhaps because it is believed that the establishment of such huge institutes leads *automatically* to the targeted results (cargo cult).

As to the question of "Western" science vs. "Third World" science, that is also something that has been debated often. Much depends on whether one claims to have different sciences according to epistemological content, according to external linkages, according to organizational structure, according to cultural ties, etc. In summary, however, I have yet to see or read a clear and practical exposition of what this different, non-Western science is supposed to be, and hence how one could attain it—or, for that matter, why the science we have is "Western." The descriptions I see tend to discuss the *use* of science and technology rather than science and technology itself. In that respect, of course, there can indeed be some differences, even though considerable universality exists and differences appear mainly in detail.

Every superstition has, however, some roots in facts, and Hill's is no exception. If one looks at the *output* of science and technology in developing countries, one can easily get the impression that applied science and technology are neglected, since the applicable and applied output of the above mentioned 80-90% of the input is minuscule. This is one of the key problems in the developing countries: the poor *quality* (in spite of the large *quantity*) of applied scientific research and technological develop-

ment. The reasons for this are multitudinous and complex, though to a large extent known.

So much for conceptual frameworks. It would appear from the above that since Hill and I are far apart on the conceptual level, we would also come up with quite different practical directives. Nothing could be farther from the truth.

Indeed, one of the things that keeps the world going is that people with very different ideas on the abstract and conceptual planes can often come to substantial agreement on what needs to be *done*. They may want to do these things for different reasons, but that is unimportant in practice. This is certainly so in this case. I find much of value and interest in Hill's list of research topics, particularly in the direction of studying the links of science and technology with other aspects of the developing countries. Indeed, a year ago when I worked for the Thailand Development Research Institute for three months to shape a research program for this institution, a good portion of our plans pertained to the study of such links.

I also know, partly from that experience, that when one pursues research along those lines, it does not take long (only a few *why?*s in succession) before one realizes that many of the questions pertaining to the scientific and technological infrastructure itself that were listed in my original article form an indispensable part of the solution to these linkage problems.

On the whole, I am in favor of including Hill's list in the *practical* agenda for research on science and technology in the developing countries. Whether this is feasible in the context of our 4S society brings me to my last point. My original list was constructed with the particular audience in mind. It is my guess that 4S consists mainly of people who are interested in acquiring knowledge about the workings of science and technology and their links, and much less of people who are action- or policy- oriented. Thus, if we want to create enthusiasts for this research topic, we have to appeal to people on their own grounds, which is more epistemological than operational.

In summary I can honestly say that while Hill and I look at science development in very different ways, I consider those differences unimportant from the point of view of cooperation in practical research projects. Hence, I am looking forward to such collaboration with him or with anybody else who is getting interested in the nascent area of studies of science and technology in the context of the developing countries.

Science in LDCs: Connectedness versus Universalism

Yehouda Shenhav
Tel Aviv University

Moravcsik's unshakable faith in science and Hill's reserved view about the utility of science to national development (the "cargo cult of science") seem incompatible in their present forms. While Moravcsik (1985) advocates increased scientific assistance to developing nations on the underlying assumption that science enhances their ability to capitalize on their own resources, Hill (1986) considers scientific research to be only marginally integrable with LDC development.

Although on the face of it the two scholars' viewpoints appear to be divergent, they share a theoretical affinity. The "connection" hypothesis is central to Hill's argument: "... [N]o matter how 'good' the science is, it simply is unlikely to connect with the surrounding user environment as it can in ... advanced nations." (1986: 30) Therefore, Hill states, "science is but a marginal add-on to the knowledge flows that transform their [the LDC's] social and economic life." (1986: 29) Thus if science is connected with its surrounding environment and is in harmony with the social and economic context within which it is generated, it follows that Hill's thesis could be reconciled with Moravcsik's contention.

Since Moravcsik and Hill allude to the same social context (that is, of developing countries) their views remain contradictory only if they share a common conception of the meaning of science. Science, however, does not have one single meaning and may refer to a wide variety of activities and fluid definitions as Hill himself has testified elsewhere (1977: 221). Nevertheless, it is implied that Hill chiefly addresses the Western self-centered outlook of science, to which he overtly so opposed in the context of the LDC. This version is presented in its extreme form by Price (1982: 167). The main ingredient in Price's definition is the refereed publication. It is based on the conception that science is considered as such only when it is registered in the Western monitors of science, i.e. scientific journals.

Such a view of science leads to the syndrome described by Hill in the "connection" hypothesis. The correlation coefficients between the number of national scientific publications¹ in 1973 and two economic measures (GNP per capita and energy

consumption per capita) in 1980 are -.05 and -.01 (respectively) in the LDC countries, and .24 and .56 (respectively) in the developed countries. Furthermore, the LDCs are less capable of converting publications knowledge into technological applications, a necessary step toward economic development: the correlation coefficients between the number of publications and the number of patents is .34 in the less developed countries, and .94 in the developed countries. These figures, regarding the relevance of western science to the social and economic context of LDCs, speak for themselves.

An alternative definition of science (Moravcsik and Exell, 1978) includes more down-to-earth problem-solving activities (where the distinction between science and technology becomes blurred) and hence less visible on the Western monitors of science. Concrete examples of such science, labelled by Moravcsik himself as "barefoot science," are presented elsewhere (Moravcsik and Exell, 1978). Whereas the implications of Price's definition of science support Hill's view of the situation, the alternative definition undoubtedly has different consequences for LDC economies. In all likelihood, the adoption of such a definition of science would generate less debate between the two contenders about the relationship between science and economic development.

Despite this possible theoretical resolution of the two different perspectives, both scholars know that in reality there is no guarantee that LDCs will conduct "connected" science. This probably stems from the rationalized belief in one world-wide universal and context-free system of science to which all scientists belong. This view is congruent with the ideology of science which is reflected in the norms of "communality" and "universalism." Ben David (1962), for example, has translated such ideology into science policy. Advocating one broad international scientific system, he suggests that smallness

Author Address: Department of Sociology, Tel Aviv University,
Ramat Aviv, 69978 Tel Aviv, Israel.

Science & Technology Studies, 1987, Vol. 5, No. 3/4, pp. 124-125

and provincialism impede the evolution of a scientific critical mass in a country and hinder communication with competent colleagues unless they belong to the international system. As mentioned above, I question the utility of such a strategy for the economic development of poor countries.

The hard core of the problem is that scientists in peripheral countries face a severe dilemma: whether to develop a local authentic orientation and become alienated from the international scientific community or to maintain their international orientation and become alienated from their national contexts. This dilemma reflects a gap between individual rationality, which suggests that links to core science are desirable, and a collective rationality, which suggests that knowledge-producing systems should not be governed by Western criteria according to which problems such as solar energy, heating, local health issues, and food research would be neglected. The conflict is apparent in the following statement made by a physicist from the University of Nairobi: "There is little glory or recognition for improving a 'Jiko'² for instance compared to work in the area of nuclear physics. It is obvious, however, which of these efforts has more direct applicability for the people in Kenya."³

It is ironic that countries which enjoy 3000-4000 sunshine hours per year have solar energy systems only in the museum of science. Moreover, targeted research contradicts the Western imperatives of science which are based on Polanyi's (1951) statement that the invisible hand of theoretical need regulates the advancement of knowledge. The international scientific community believes that

such processes are facilitated only when absolute autonomy is guaranteed. As long as this ideology remains dominant, LDC science is likely to resemble its Western counterpart and be detached from its surrounding environment. In so far as this is the case, Hill's argument is likely to be borne out by LDC reality while Moravcsik's thesis remains wishful thinking.

Notes

1. In natural science, medicine, and engineering.
2. A cooking stove which is suitable for the local climate.
3. *Development and Cooperation* 1985: 26.

References

- Ben-David, Joseph. "Scientific Endeavor in Israel and the United States." *The American Behavioral Scientist* 1962: 12-16.
- Hill, C. Stephen. "Contrary Meanings of Science—Interaction Between Cultural and Personal Meanings of Research in a Developing Country Scientific Research Institutions." In S. S. Blume, ed., *Perspectives in the Sociology of Science*. New York: John Wiley & Sons, 1977, 195-230.
- Hill, C. Stephen. "The Hidden Agenda of Science Studies for Developing Countries." *Science & Technology Studies* 4 (1986): 29-32.
- Moravcsik, Michael. "Science in the Developing Countries: An Unexplored and Fruitful Area for Research in Science Studies." *4S Review* 3 (1985).
- Moravcsik, Michael and R. H. B. Exell. "Third World Neweds 'Barefoot' Science." *Nature* 276 (1978): 315-316.
- Polanyi, Michael. *The Logic of Liberty*. Chicago: University of Chicago Press, 1951.
- Price, Derek J. de S. "The Parallel Structures of Science and Technology." In Barry Barnes and David Edge, eds., *Science in Context*. Cambridge: MIT Press, 1982.

"Hidden Agendas," Conceptual Clarity, and Disciplinary Awareness in the Study of Science and Technology in the Developing Countries

Richard P. Suttmeier
Hamilton College

Professors Michael Moravcsik and Stephen Hill have performed an important service by reminding us, again, of the important issues surrounding science and technology in the developing countries. They have also called our attention to the possible roles of science studies in these issues. Moravcsik has reminded the sciences studies community of the possible theoretical and practical payoffs from attention to the developing world, and Hill has opened up the possibility of expanded dialogue on the subject in his constructive critique of the Moravcsik statement.¹

On the face of it, there appear to be some rather fundamental differences between Moravcsik and Hill; at least so it appears to Professor Hill. Hill believes that Moravcsik's views are both wrong and inappropriate given current realities. The use of "hidden agenda" in Hill's title is indicative of his belief that Moravcsik's view of the role of science in development is fraught with problems. Hill also sees Moravcsik's agenda for research as one which accords more with the latter's own values, rather than the concrete conditions of the developing world.

While there is more questioning of Moravcsik's motives and ideological stance than suits my taste, Hill does usefully call our attention to the importance of underlying assumptions, and the world views of investigators, in this area of research. The problem is, he merely adds an alternative set of assumptions to Moravcsik's; unfortunately he comes nowhere near exhausting the set of relevant possibilities.

The Need for Distinctions

The Moravcsik and Hill pieces usefully challenge us to discussion. The advance of our understanding from these discussions, however, requires that we be far more attentive to the concepts we use and to the relevant disciplines from which insights might be drawn. We need at the outset to set boundaries on our understanding of science studies. Moravcsik tends to take a narrower view of what the science studies exercise entails, and the research agenda he proposes is one which would be readily understandable to those who would self-identify as doing

"science studies." Hill, on the other hand, is more expansive in his implicit bounding of the field. Hill rightly recognizes that the issues in the developing world are often more technological than scientific and that these cannot be understood apart from an understanding of the international "knowledge flows" which have come to characterize world affairs.

There is a rich literature to be consulted if we accept the Hill conception of a research agenda. For many years students of economic development, political and administrative development, and technology transfer have been trying to figure out what factors make possible the effective assimilation of foreign technology in developing countries, and what factors explain the development of indigenous innovative capabilities.² More recently, students of international business have begun to shed light on the technology transfer experiences of firms with international operations, a subject of considerable relevance for understanding the prospects for scientific and technological development in the developing world.

In keeping with the theme of the need to make distinctions, this literature indicates that the motivation for and performance of technology transfer varies greatly by type of industry and by individual corporate strategy. Indeed, the very concept of technology transfer is not likely to be the same for firms in different industries.³ Those conventionally understood as doing "science studies" normally have not been trained in these areas, and normally do not consult the relevant literature on development. Unfortunately, students of development often are oblivious to the work of the science studies community.

The Moravcsik and Hill pieces thus present us with a basic methodological choice. Do we maintain a narrower conception of the exercise, as implied by Moravcsik, or do we broaden our domain, as implied by Hill? Moravcsik, of course, in other places,

Author Address: Department of Political Science, Hamilton College, Clinton, New York 13323, USA.

Science & Technology Studies, 1987, Vol. 5, No. 3/4, pp. 126-128

has demonstrated his awareness of the broader developmental issues, but has insisted in these earlier jeremiads that indigenous scientific capabilities are prerequisites for ultimate national technological capabilities. Other students of development, especially economists, have tended to dismiss the importance of indigenous scientific capabilities, regarding them as a misuse of scarce resources. Hill's position, it seems, is not incompatible with this latter position.

There are good reasons for accepting the Moravcsik position of a narrow definition of the exercise. In principle, it leaves us with a far more manageable research domain (although the execution of the Moravcsik program would require extensive field work by sensitized investigators, the numbers of which, alas, are not great). Furthermore, as argued below, coming to understand the scientific enterprise in developing countries is an important issue, both theoretically and practically, as Moravcsik suggests.

However, if we wish to understand the development of technological capabilities more generally, we must expand our domain, as Hill implies. But if we wish to go this route, then the expansion must be further than Hill himself suggests.

Again, there is a need for distinctions. Any expanded science studies agenda for research on developing countries must at the outset recognize the great variety within the set we refer to as "developing countries." Nations clearly vary by culture and language, political and economic systems, former relations with colonial powers, factor endowments, size, and geography more generally. They also vary by levels of economic development and national aspiration. These factors combine in the decisions leaders make about development strategy, and understanding these development strategies, as well as the other distinguishing features of a country's uniqueness, is a required first step for the expanded science studies agenda.

Development Strategies

For instance, the development experiences of such countries as the Asian newly-industrialized countries (NICs), India, Brazil, China, and Japan offer a complex array of strategies involving mixtures of self-reliant indigenous scientific development, reliance on technology and science transfers from abroad, import substitution, and export promotion. A look at the development experiences of these and other countries also suggests the importance of size and national aspiration. The bigger countries have clearly been far more attentive to the development of indigenous scientific capability. The smaller states, which in these cases have also focused development activities on export promotion, have on the other

hand downplayed the importance of developing an indigenous capability for original research and have tended instead to seek congenial commercial relations which include technology transfer. These cases suggest that a commitment to *either* Moravcsik's narrow approach, or to Hill's expanded approach would be a mistake. Clearly, what is necessary is an approach which recognizes both the importance of the indigenous scientific development issues (Moravcsik) and the international technological linkages (Hill).

The recent evolution of these countries all the more requires that we not be locked into the either/or choice with which Moravcsik and Hill leave us. The Asian NICs, for instance, are increasingly being forced to confront the issues of indigenous science, while the larger countries, especially India and China, are engaged in a profound reorientation towards international technology flows.

The experiences of these countries also point to a topic of great importance for scientific development which does not figure centrally in either the Moravcsik or Hill formulations. This is the role of military R&D and national prestige "big projects" such as space and nuclear programs, in the shaping of a nation's science and technology development. Unfortunately, this topic is often a difficult one to study since data is less available than one would like; but the experiences of China, India, Brazil, and pre-WWII Japan suggest a profound influence on S&T from military programs and/or "big projects."

Mechanisms of Social Choice

A consideration of ambitious, government-proposed military research and "big projects" points to the need for a more focused attention in science studies—those oriented towards the advanced countries as well as the less developed countries—on what might be called "mechanisms of social choice." In comparative analyses of the "science systems" of different countries, it is difficult to escape common issues involving the fundamental institutional matrices affecting the choice of direction for scientific development. The issue can be stated most simply as follows. To what extent can market forces be counted on to stimulate S&T development? It is an issue which is perhaps most obvious when one considers the S&T reform efforts of countries such as China and the Soviet Union (which involve the activation of market forces after decades of eschewing them), but it is present in all countries.

The asking of this question necessarily takes one into theories of welfare economics and public choice; these have not been widely applied to science studies. Nevertheless, students of science policy should have little trouble recognizing the significance of the issues. Whether it be Reagan's Washington,

Takeshita's Tokyo, Teng's Beijing, or Gandhi's Delhi, a fundamental issue in strategies of S&T development is the role to be played by the state and the role to be played by the market. Far more needs to be known about the "public goods" characteristics of science and technology, and how these affect the motivations and behaviors of government and non-governmental decision makers, of scientists, and of engineers.

It is likely that a better understanding of the mechanism of social choice will shed light on, but probably not explain, the patterns of institutions and behaviors which contribute to the acquisition of indigenous technical capabilities. For this, mechanisms of social choice will have to be understood in the context of diverse cultural matrices, some of which seemingly predispose some countries towards successful "learning by doing" more than others. Increasing evidence points to the importance of this propensity for "shop floor" learning as a necessary condition for the acquisition of national technical capability.⁵ Neither of the programs proposed by Moravcsik and Hill, as stated, would logically call our attention to this important phenomenon.

The International Environment

Finally, the issues raised by Moravcsik and Hill cannot be adequately addressed without clarifying further some of our underlying assumptions about the nature of the international environment in which a nation's S&T development efforts occur. Hill, in my view, takes a useful first step in addressing this question, but it is only a beginning. What should be our view of the international milieu as we approach an analysis of the problems of scientific development in the less developed countries? How autonomous are national S&T development efforts relative to the international system? Is that system one which biases third world S&T development towards a pattern of dependency, as Hill implies? If so, how do we explain the apparent escape from dependency which seems to be occurring in East Asia? Should we adopt more of an interdependent view? Are we in fact witnessing the early emergence of an entirely new international political economy in which S&T are better understood as truly trans-national phenomena, the capturing of which in our concepts and theoretical formulations has only begun?

My own sympathies are with the latter view, but I think the important points are: 1) that the question of our understanding of the problems of science and technology in the developing countries increasingly must include explicit attention to the ways in which national experiences are related to international phenomena; and 2) that we approach the

acquisition of such understandings of the international environment free from sophomoric, ideologically based views of subordination and superordination.

Conclusion

Moravcsik and Hill have raised important questions for the science studies agenda. Contrary to first appearances, the positions of the two need not be incompatible as Hill would have us believe; there is an "apples and oranges" quality to Hill's response to Moravcsik which precludes logical inconsistency. However, choosing between the Hill and Moravcsik research agendas does involve a major methodological (*not* ideological) choice affecting the scope of inquiry.

The interesting point in comparing the Moravcsik and Hill pieces is thus not their incompatibility, but rather their incompleteness as prescriptive research programs. Both seriously beg the question of which disciplines and disciplinary literatures are relevant to studies of science and technology in the developing countries. This omission leaves us with proposed research agendas which are unnecessarily atheoretical, *ad hoc* and insensitive to the findings and insights of established social science disciplines in their treatment of development issues. This, of course is a peril in any interdisciplinary work in the social studies of science. It can be overcome, one hopes through heightened awareness of the work of the disciplines and through the iterative process in which we are all engaged. May the discussions continue.

Notes

1. Michael J. Moravcsik, "Science in the Developing Countries: An Unexplored and Fruitful Area for Research in Science Studies," *4S Review*, 3 (1985); Stephen Hill, "The Hidden Agenda of Science Studies for Developing Countries," *Science & Technology Studies*, 4 (1986).

2. For a recent effort to summarize this literature and highlight lessons learned, see Aaron Segal (ed.), *Learning by Doing: Science and Technology in the Developing World*, (Boulder: Westview Press, 1987).

3. See for instance, Geoffrey Oldham and Alyson Warhurst, "Technology Transfer to the Chinese Offshore Oil Industry," (Sussex, England: University of Sussex, Science Policy Research Unit, unpublished report, n.d.).

4. Michael J. Moravcsik, *Science Development - The Building of Science in Less Developed Countries* (Bloomington, Indiana: Indiana University, The PASTITAM Program, 1976).

5. The "shop floor" learning concept is developed by Nathan Rosenberg. See the discussion in Segal, *Learning By Doing*, 7ff.

The Emergence and Maturation of the Sociology of Science

Bernard Barber
Columbia University

Editor's note: This contributed essay was prepared in July, 1987, as preface to a Chinese translation of Science and the Social Order.

I am gratified that my colleagues, Mr. Gu Xin, and his associate in the Institute of Policy and Management in the Academia Sinica in Beijing, Mr. Zhao Leijin, have undertaken the translation and publication of my *Science and the Social Order* in order to make it more available to the scholarly and science policy communities in the People's Republic of China. The book was originally published in the United States in 1952 and has since then appeared in a British edition (1953), two American paperback editions (1962 and 1970), an American hardcover printing (1978), and in Japanese translation (1955). This is, of course, the first Chinese translation.

Because, fortunately, there has been a good deal of progress in the sociology of science and in the science policy field during the thirty five years since 1952, *Science and the Social Order* is not up-to-date with respect to data, especially those provided in the intervening years by empirical studies employing sophisticated survey research techniques and the newly-invented science citation method, nor is it up-to-date in that intensive scrutiny of the substance of scientific ideas which has lately come to characterize a growing part of the sociology of science. Scholarly work carried out beginning in the 1960s and since has provided new data, new research techniques, and new intensive scrutiny of the actual development of scientific ideas. As I shall explain more fully below, I view all of this work with admiration and approval. The sociology of science has emerged and matured since 1952.

Despite all this progress, however, *Science and the Social Order* still has a great deal to offer the reader. The basic character of the book and its continuing usefulness will become quite clear in the account I give below of its intellectual origins. The purposes I had in mind in 1952 are still important for the sociology of science. The admirable progress made in the sociology of science since 1952, like all scientific progress, has often been one-sided, in this case one-sidedly microscopic in its analysis of science. It has often been neglectful of the macroscopic features of science as a major social-structural and cultural institution in society. Further, it has often not been

sufficiently comparative in its approach, looking to the similarities and differences in science in different societies and in different historical periods. The social-system, institutional, and comparative presuppositions are all basic to *Science and the Social Order*, and they are still essential for a multidimensional, synthetic analysis of the social character of science.

This preface provides me a valuable opportunity to make two new statements—one, about the intellectual origins and purpose of the book, and, two, about the welcome emergence and maturation of the sociology of science as a regular and recognized specialty in the social sciences.

Intellectual Origins and Purpose

It is often difficult to think ourselves back to the origins of a scientific specialty. Once established, a specialty and its practitioners are more concerned with everyday, ongoing problems and work than with its origins. So it is with the sociology of science. It is difficult to realize now that in 1952 there was no such specialty as the sociology of science. True, the British "scientific humanists" (people like Bernal, Hogben, Soddy, etc.) and others in America and Russia had earlier written about the nature, problems, and uses of science, but they did not think of themselves as sociologists of science or as working in a social science specialty. Bernal and his fellow "scientific humanists" were natural scientists concerned for the troubled world of the 1930s worldwide Depression and also for how the condition of society might be improved by the effective reorganization and greater application of science for human welfare. Their interests were primarily practical policy, not scholarship for its own sake. Those were the typical interests of the time for almost everyone writing about science and science policy.

The only professional sociologist writing about science in the 1930s and 1940s was the then-young (only in his twenties and thirties) Robert K. Merton,

Author Address: Department of Sociology, Barnard College, Columbia University, New York, New York 10027 USA.

Science & Technology Studies, 1987, Vol. 5, No. 3/4, pp. 129-133

whose classic *Science, Technology, and Society in Seventeenth Century England*, which had been his doctoral dissertation in the new Sociology Department at Harvard, was published in 1937 under the sponsorship of George Sarton, the founding father of the history of science, in the History of Science Society publication, *OSIRIS*. In the early 1940s, for both intellectual and social reasons (the Nazi repression of science in Germany), Merton published his also classic papers on the normative structure of science. Note that Merton did not publish either his book or his papers with a view to establishing the sociology of science as a social science specialty. Then and now, some fifty years later, Merton's primary and over-riding interests as a sociologist has been in the development of social theory. Although his book, his early papers, and his later papers, which were produced only from 1957 forward, make Merton a founding father for the sociology of science, in the 1930s and 1940s he was writing about science only because thereby he could develop theoretical ideas about the role of "ideas" or "culture" in social systems and about the consequences of ideas for stability and change in social systems. In the wholly "materialist" social theories that were frequent then, ideas were only "superstructure," only epiphenomenal, not also partly independent forces in society. Merton's thesis in his early book and papers—namely, that religious and normative ideas were independently effective in social action, both in principle and practice (e.g., the seventeenth century scientific revolution in England)—was intended as a contribution to social theory, a counter-weight to "materialist" determinism.

As an undergraduate student of Merton's in the late 1930s, and as his associate and close friend thereafter on a continuing basis during the 1940s and 1950s, I heard nothing of the sociology of science as a specialty. Indeed, my own purpose in the late '40s and early '50s in setting out to write *Science and the Social Order* was quite similar to Merton's in his work on science: the development and application of social theory. We had the same purpose, but a different source for it.

As an undergraduate and graduate student at Harvard University (with time out for four years to serve in the U.S. Navy during World War II), I had studied with and come to admire very much the contributions to social theory of Professor Talcott Parsons. I admired not only his revolutionary general theory of action but also his development of social systems theory and his applications of that theory, in course lectures and journal articles, to the analysis of the functions, variable structures, and processes of change in social structural and cultural systems in society. I was especially attracted by Parsons' emphasis on and exemplification of the uses of comparative historical and societal materials for the testing

and development of social systems theory. Parsons invited his Harvard colleagues who were specialists in a wide variety of societies and historical eras to give lectures in his course on comparative institutions. The basic purpose of the course as a whole and of the individual lectures by Parsons and his guests was always the development of social systems theory.

My ambition and purpose, then, in undertaking my own first major sociological work was theoretical, to follow Parsons. I wanted to accomplish my purpose in two ways. First, I wanted to write a book that would present in concise and somewhat abbreviated form the substance of the major social structural and cultural components of the theory of society as a social system. I intended to analyze the functions of each sub-system component within the larger social system and also its functional relations with each of the other sub-systems. I intended, further, to analyze systematically the various but limited number of structural alternatives which could subserve these several social structural and cultural functions. And finally, I intended to show dynamic processes in these sub-systems, how they maintained their stability under some conditions and how they changed under others. All this was to be accomplished in a single volume with rich historical, anthropological, and contemporary empirical exemplification. I did write several chapters for a book of this kind and I did develop both an introductory and an advanced sociology lecture course in which my purpose was tentatively worked out, but no published book ever resulted. I have long regretted that fact and still think such a book would be invaluable for sociology as a science. It could provide a focus for an increase in the integration of sociology as a discipline.

The second way in which I wanted to accomplish my goal of developing and applying social systems theory now seems incredibly grandiose. But at that time in sociology, it seemed possible. I intended to write a separate book for each and every one of the major social structural and cultural sub-systems of society. Having put aside the first—the overall, the general-book, and being familiar from my work with Parsons, Merton, and Sarton with the scattered work in the social aspects of science field, I wrote *Science and the Social Order* as the first in the series of separate books I had in mind. In the late 1950s, I wrote a second book also intended both as a separate project but still a part of my overall purpose to run through all the sub-systems of the theory of society as a social system. That was my *Social Stratification: A Comparative Analysis of Structure and Process* (1957). (Note the sub-title.) After that, I gave up my grandiose project.

Further evidence that until the 1960s the sociology of science did not exist nor was even conceived of as

a specialty in sociology can be found in *Science and the Social Order* itself. Although I hunted for, and found, scores of books and articles that I could organize in that book with my social systems theory into the first comprehensive treatise on the sociology of science, when I recently examined those references I found that very few of them refer to any one of the others. We now know from recent work on the nature of specialties in science and on the invisible colleges and formal groups making them up that an essential characteristic of such specialties is the intensive cross-references of publications to one another, the co-citations, that constitute them. Specialties consist of dense webs of cross-references. By this criterion for the existence of a specialty, my book proves that no specialty existed then for the sociology of science.

A glance at the chapter titles of *Science and the Social Order* will quickly show how largely and directly indebted I am to Parson's social systems theory and to his work on comparative institutions. I start with a chapter on the nature, functions, and changing character of science as the essential embodiment of human rationality in social action and social systems. It should be noted that my discussion of the normative structure of science derives from Parsons' analysis of what he called the "pattern variables" as well as from Merton's classic papers on that subject. I then present comparative materials from earlier historical periods and from modern society, including the variability of science in modern society as between what I called the "liberal" and "authoritarian" sub-types of modern society. Incidentally, at a time when my view was contrary to the received opinion, I argued, *directly on the basis of my social system analysis*, that Russian society was probably strong in at least many branches of science. I made this analysis at a time when one American biologist had just written a book on "the death of science in Russia." I should note that, on the basis of my analysis of Russian science, I was approached by a representative of a major U.S. Government intelligence organization and asked if I would come to Washington to organize a unit for the study of Russian science. I did not go, but I am sure that the organization in question, as well as other government organizations and academic groups, now keeps up with the activities of science in all major Powers, including of course, China. That science is a major source of national power is now recognized by all.

Other chapters in my book discuss organizational structures in science, first in general, and then specifically in American government, industry, and university organizations. In the chapter on the social process of invention and discovery, I analyze the dynamic processes in science, concentrating on that perpetual theoretical problem in sociological theory, the relations among individual, culture, and society.

The chapter on the social control of science treats of the complex, changing, and sometimes conflictful relationships between science as a social institution and other social institutions, especially, at this time and in this case, the political institution. Finally—and here I was very much influenced by Parson's liberal optimism—I devote the last chapter to an optimistic view of the nature and prospects of the social sciences. Thirty five years later, I still hold to that view. We may not have the theoretical integration that would in many, but not all, respects be desirable for the future progress of the social sciences, but we do see a hundred flowers blooming. We are far from where I came into sociology fifty years ago. (Incidentally, when I came into sociology fifty years ago, I had never heard the word before. How lucky I was to fall in, as a student at Harvard College, with people like Parsons, Merton, Sorokin, and L. J. Henderson, from all of whom I learned a great deal about social theory in general and about science in particular.)

Emergence and Maturation

This is not the place to do more than sketch some of what has happened since World War II, both intellectually and in the larger societal world, to cause the emergence and maturation of the sociology of science as a scientific specialty. In World War II, science proved, perhaps more visibly than it ever had before, its enormous social consequentiality. The development of the atomic bomb and of antibiotics, to mention only two examples, made governments see that they must give large and continuing moral and financial support to all forms of science. Postwar competition among the Powers on the industrial and military fronts led to new institutions for the support of science and to the emergence of organizations and studies in science policy in the hope that they could more effectively guide such support. It is a hope which has not been fulfilled as well as some had originally wished. Nevertheless, large government support of science by all societies that can afford it is likely to be with us permanently.

On the intellectual front, a number of important developments, beginning in the late 1950s, contributed to the development of the sociology of science and science policy. At this time, Robert Merton returned from some of his intervening interests to an intensive interest in the sociology of science and encouraged a new generation of graduate students to become specialists in that field. Jonathan Cole, Stephen Cole, and Harriet Zuckerman were students of Merton's who have continued to specialize in the sociology of science. These scholars were particularly proficient in the use of survey research techniques; they thus brought new quantitative data

to the sociology of science. They also made valuable use of the science citation indices that had been invented as an information retrieval device by Eugene Garfield in the 1960s but now were creatively used by sociologists to illuminate essential aspects of the social structure and processes of science.

The major part of my own work at this time concentrated on the sociology of biomedical research. In collaboration with a group of able postgraduates and graduate students (Daniel Sullivan, John Lally, and Julia Makarushka), I did two large survey studies on the problems of ethics in biomedical research. Our explicit theoretical focus was what we called "the dilemma of science and therapy." In our analysis we used our survey data, citation data, and network data. Because of the rapid heightening at this time of national policy concern for these problems of biomedical ethics (informed consent and the risk-benefit ratio), I became involved in a series of social policy activities and had some small influence on national social policy in this field. On the basis of that experience I have recently reflected, as a sociologist of science, on the relations between empirical social research and social policy. Using my reflections as a model for other social scientists whose empirical social research also has had policy consequences, I have collected eight cases of this kind from economics, political science, and sociology in a recently published book, *Effective Social Science* (1987). All eight cases address themselves to the same thirty-odd sociology of science questions. The cases provide some interesting tentative generalizations about the relations between empirical social research and social policy.

Early in the decade of the 1960s, in *Little Science, Big Science*, Derek Price's highly mathematized publication measures of growth processes in science further promoted quantitative approaches in the sociology of science. With his revival of the seventeenth century term *invisible college* to refer to the importance of network processes in science, Price also stimulated the use of network measures of scientific processes and structures. Other important users and developers of network measures have been Diana Crane, Nicholas Mullins, and Henry Small. Price was a passionate advocate of what he called "the science of science." His brilliant and metaphorical style made his books attractive to a wide readership.

One of the factors impeding the development of the sociology of science before the 1960s was the resistance to it by the history of science and the philosophy of science, both of which were relatively well established scholarly specialties. In the form of an emphasis on the "internalist" approach to science, there was strong resistance in these specialties to the "externalist" assumptions of the sociology of science, particularly the Marxist version but even the more

main-line, liberal version. In 1962, Thomas Kuhn's *The Structure of Scientific Revolutions* caused a creative revolution in the relations among philosophy, history, and sociology of science. Apparently not himself entirely aware of the fact at that time, Kuhn used concepts and materials from all three of these specialties in his seminal work. The study of science as a social phenomenon has never been the same since. Inter-specialty cooperation and discussion is now frequent and fruitful. There is no longer any talk of "internal" and "external" aspects of science. Scientific ideas, organizations, and processes are now clearly seen by all scholars as sub-systems inter-related with other social and cultural sub-systems in society.

Finally, very happily, and for social and intellectual reasons which it would be most interesting to investigate as an exercise in the sociology of science, there has been another creative revolution, this one coming primarily from Great Britain. There, philosophers, sociologists, natural scientists, and science policy specialists have combined, especially at the Science Studies Unit of the University of Edinburgh led by David Edge but also at Sussex, York, Bath, and elsewhere, to provide valuable studies of the actual substance of scientific ideas. Barry Barnes, David Bloor, Harry Collins, David Edge, Roy MacLeod, David Mackenzie, Michael Mulvey, Steven Shapin, Richard Whitley, and Steve Woolgar have been the leading British figures in this revolution. Their close-in, micro, empirical studies of the actual development of scientific ideas and organizations are an important contribution to our understanding of science as a social phenomenon.

Like many revolutions, however, this one has its one-sided tendencies. It tends, or so it seems to me, to be relativistic on the ontological aspects of science and rationalistic on the social side. Contrary to this "school," I do not think that the natural and social worlds are entirely a construction of scientific ideas about them nor do I think that scientific work is solely motivated by "interests," as several members of this school assert without ever giving a satisfactory theoretical definition of that term. Science is driven by science, by norms, by interests, and by the "real" world, all four.

Science has some degree of autonomy. It is to some extent *independent* of other social structural and cultural sub-systems as well as being *interdependent* with them. The difficult empirical problem is to work out specifically the amounts and types of independence and interdependence. The task is difficult, but this is no reason to shirk it.

The sociology of science has by now become an international scholarly specialty. Frenchmen such as Bruno Latour, Austrians such as Karen Knorr-Cetina, and Dutchmen like Arie Rip all are valuable workers

in this specialty. The sociology of science now has its own international professional society, the Society for Social Studies of Science, and it has two professional journals, *Social Studies of Science*, founded and edited by Edge and MacLeod at Edinburgh, and *Science & Technology Studies*, the infant journal of the Society for Social Studies of science, edited by Susan Cozzens.

The sociology of science has been born and grown up since *Science and the Social Order* was published in 1952. It is my hope that this translation into Chinese will further the progress of the sociology of science and of science policy.

New York
July, 1987

Funding and Academic Research in the Life Sciences: Results of an Exploratory Study

Edward J. Hackett
Rensselaer Polytechnic Institute

Abstract

Research funding has become an important part of science yet we know little about how funding agencies, scientists, and university departments respond to changes in the funding system. The present study uses face-to-face interviews with a small sample of scientists, department chairs, and government officials to explore how scientists accommodate their work and careers to the funding environment. The results are organized into five themes: the role and prospects of scientific marginals, relations between principal investigators and their staffs, organizational consequences for universities, accommodations to scientific change, and quality of science. The paper closes with questions for policymakers.

Money is the fundamental instrument of science policy. By changing the amount of support for an area of research, policymakers attempt to steer the course of U. S. science. Increases and decreases of support are expected to direct science in one way or another and, in these days of fiscal constraint, tight budgets are expected, through competition, to drive out mediocre science and leave only excellence.

But the scientific research system—including scientists, their employing organizations, and funding agencies—is loosely-coupled and complex, so changes at one level may have unintended consequences at other levels. Funding shifts must be transmitted from the policy level through the organizational filters of the funding agencies and employing organizations (universities) to the scientists (Noll in Cozzens, 1986). In this transmission signals may be distorted, amplified, or diminished by the interests and interpretations of the organizations. Moreover, the various actors in the science system have diverse interests, goals, and environments. While they all share some commitment to the overarching goal of increasing certified knowledge, other goals (such as organizational survival, career advancement, and self-actualization) and other contingencies enter into the calculations of university departments and scientists. Therefore it may not be easy to predict how science will be affected by a shift in research support.

Policy decisions intended to reshape science often assume a straightforward "stimulus-response"

model: the stimulus of increased support for an area will attract able scientists and produce good science. And conversely, the competition engendered by decreasing support for an area will weed out the weaker scientists and leave only the most able. Yet we know very little about how scientists obtain research support in a highly competitive funding environment and how university departments may act to help or hinder scientists in that endeavor. Thus, policymakers who would use funding priorities to redirect science or to improve its efficiency might

Author Address: Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, New York 12180-3590 USA. Earlier versions of this work were presented at the Annual Meetings of the American Association for the Advancement of Science, February 1987, and the Society for Social Studies of Science, November 1986.

This research has been supported by the National Science Foundation under Grant No. PRA85-14061 and by a grant from the Paul Beer Trust at Rensselaer Polytechnic Institute. Any opinions, findings, conclusions, or recommendations expressed in this paper are those of the author and do not necessarily reflect the views of the National Science Foundation. Earlier versions of this paper benefited from discussions with Stuart Blume, Michel Callon, Tom Carroll, Daryl Chubin, Jean-Pierre Courtial, Sharon Harlan, Loet Leydesdorff, Sal Restivo, and Ned Woodhouse. I am also indebted to the scientists who readily and generously talked with me about their research lives. Finally, I am very grateful to Susan Cozzens for her advice and encouragement throughout the project.

Science & Technology Studies, 1987, Vol. 5, No. 3/4, pp 134-147

benefit from a study of how scientists, university departments, and funding agencies reconcile their goals and functions to the available research support.

This study examines the responses of funding agencies, university departments, and scientists to changes in the level of research support by comparing areas of research with different recent funding histories. In this paper I am chiefly concerned with the behavior of scientists in one specialty, developmental neuroscience. I will also draw on interviews with scientists working in the area of immunology concerned with lymphokines. My purpose is to discover how the changes in the funding environment, particularly changes that increase competition for resources, might influence scientists' careers, work patterns, and the science they produce.

Other work on the effects of funding has taken a more aggregated perspective, examining the consequences of policies for an entire research area over time. Such studies typically use historical and bibliometric data to show how funding patterns are related to measures of growth and development in coherent areas of science (e.g., the papers in Cozzens, 1986; Rip and Nederhof, 1986). This paper works at a lower level of aggregation, asking how scientists reconcile their research and career needs with the funding environment.

Data

This paper is based on face-to-face, taped interviews with scientists in their offices. The interview schedule was loosely structured, asking specific questions while allowing time for elaboration and exploration of unique details of a scientist's research interests, employment circumstances, and career. Interviews lasted from one to three hours, averaging about ninety minutes.

Respondents were chosen according to several criteria. One population of potential respondents was identified through lists of grant recipients provided by NIH or retrieved from the Federal Research in Progress data base. These lists were then restricted geographically to the east and west coasts and by "quality" of the department where the scientist worked when the award was made. Quality was determined by the rating of a school's doctoral program in cellular and molecular biology according to an evaluation conducted by a National Academy of Sciences committee (NAS, 1982). I relied on two measures: the scholarly quality of faculty, based on a reputational survey, and the influence of recent publications, determined by counting the publications and evaluating the quality of the journals in which they appeared. Middle-rank schools were within one standard deviation of the mean on these measures; top-rank schools were at least one (and

sometimes two) standard deviations above the mean. Geographic restrictions were imposed to keep travel costs and inconvenience reasonable while insuring some diversity. Imposing these quality criteria on the sample allowed the study to focus on active researchers while extending its scope beyond the most prestigious departments.

A second, smaller population of respondents was obtained by asking NSF program managers to send letters requesting an interview to scientists whose proposals were declined funding in one of the three most recent panel meetings.

Respondents also were chosen to provide diversity in academic rank (ranging from full professors through recent tenures to marginal jobs), years of support (ranging from 25 continuous years to first-time investigators), and sex (to avoid under-representing women, especially as their positions and problems may differ from men's).

This paper is based on interviews with 23 scientists, 2 department chairs, and 4 agency officials. Of the scientists, 15 were in developmental neuroscience, 8 in lymphokines. Five were in low-level, academically marginal positions; 6 were assistant professors, 5 were associate professors with tenure, 7 were full professors (although one of these does not have tenure and her career prospects are very similar to the marginals, despite her rank and accomplishments). They are evenly divided between medical schools and academic departments. Their educational backgrounds are generally similar in "quality" of the Ph.D. department—most came from very good schools—but are quite different in baccalaureate origins, ranging from the most prestigious schools to the most obscure.

Their publications and accomplishments varied substantially, as one would expect from a group so diverse in professional age and rank. But even the least accomplished had made noteworthy contributions, judged by the usual measures of publication and citation counts. For example, one scientist whose employment circumstances are marginal was recently the first author of a paper published in *Science* and her work has received over sixty citations in the 1984 volume of SCI.

Results

The findings are presented as five themes, grouped into two sets. The first set of themes has to do with the consequences of funding competition for the social organization of science; the second is concerned with effects on science as an intellectual enterprise. In considering social organization I begin with a discussion of marginal positions, then consider relations between principal investigators (PIs) and their staffs, and finally consider some consequences for the academic research and educational enterprise.

Major universities employ many Ph.D.s in academically marginal positions. Variouslly termed the "unfaculty" (Kerr, 1963), unequal peers (Kruytbosch and Messinger, 1968), and the "nonfaculty" (Teich, 1982), these scientists populate an academic "never-never land" made possible by the availability of research support and made miserable by the difficulty of obtaining such support and by their ambiguous status in the institution. These scientists might have such titles as "Assistant Research Anatomist" or "Research Fellow." However, in some schools there is a well-defined career track outside the usual academic ranks for these people, so it is possible in some institutions to attain the rank of full professor (or its equivalent), although usually without tenure.

The numbers of marginal scientists have been growing rapidly in recent years. According to National Research Council data, academic employment for doctoral scientists in nonfaculty positions (*other than postdocs*) grew at an annual rate of 7.8% between 1973 and 1979; in contrast, faculty employment grew 4.1% per year during that period (NRC, 1981:69). Contract researchers in British universities, a comparable category, grew roughly 65% between 1976 and 1984 (Pearson, 1987; see also Roberts, 1984).

The origins of such positions lie in the history of the university, but their growth and differentiation may be fueled by the dynamics of the academic research system. In recent years the number of available faculty positions in universities has been far smaller than the number of new doctoral scientists (NRC, 1981; Phillips and Shen, 1982). For a time the surplus was absorbed into postdoctoral fellowships and fellows were extending their stays in such positions as they waited for faculty jobs to become available (Coggeshall and Norvell, 1978; NRC, 1981). In the basic biomedical sciences, for example, the number of postdoctorals grew rapidly from 1974 through 1982, then leveled off (although the number of Ph.D.s awarded in those years grew only slightly; Institute of Medicine, 1985).

In the case of developmental neuroscience, for example, intense scientific interest has attracted many more graduate students to Ph.D. programs than there are faculty jobs to employ them. These scientists write grant proposals to support themselves while they wait in the queue for a job, thereby maintaining and developing their research skills, and to build the sort of track record many departments now expect of their new faculty. Since research money is somewhat more available than faculty jobs (and as these scientists are well-trained, capable researchers), they may succeed in supporting themselves in these

marginal positions. In effect, their status is an extension of the postdoctoral fellowship, itself a waiting state in which scientists acquire skills, "prove" themselves, and await faculty openings.

These people certainly are a significant scientific resource. The importance of their research can be documented from their publication and citation records. Moreover, the work done in these marginal positions also contributes to the productivity of other scientists and indirectly assists academic search committees by providing a longer "track record" with which to evaluate job candidates. But their time in these marginal positions also has some costs which may be less apparent. For example, the conditions of employment in such positions are often undesirable. The salaries are usually lower than academic salaries and the positions may not provide full-time pay (although they may demand full-time work). There is also little job security: employment usually terminates when a project ends. Moreover, such positions have limited academic "rights," such as claims on laboratory and office space or access to seed money and equipment. In one instance the scientist was not allowed to submit an independent research proposal, although she was permitted to have a new investigator award and an independently-run subcontract from a project at another institution. Underlying this prohibition were the department chair's inability to assign space to the scientist and his desire to push the fledgling scientist out of the (doctoral and postdoctoral) nest. Whatever the motivations, these are difficult circumstances in which to work as a scientist.

These positions also entail substantial vulnerability, both within the institution and outside. Within the university such marginal scientists are dependent on others to provide part-time teaching or research employment to complete their salaries, and thus are indebted to those who bestow such favors. Even the allocation of lab space may be contingent on having a working arrangement with a more senior scientist and thus may be terminated without notice or recourse. In relations with the larger scientific community occupants of marginal positions are at a competitive disadvantage because they do not have an established laboratory. They may also be more likely to have their ideas misappropriated from abstracts, papers, and proposals simply because they are less credible complainants and because they haven't yet staked their intellectual claim in the literature.

Perhaps most important, such positions typically offer poor career prospects, even for those scientists who accept the lesser salary and prestige and the inferior working conditions. Seldom are there clearly-defined career paths, and some who choose to remain are driven out as "too senior" to occupy

such posts. And when there is a career path it is often not an enviable one.

For example, one scientist holds the rank of full professor, without tenure, and a rather high salary at a prestigious institution. Through several years of very productive work she had been promoted through the ranks. But her case serves to underscore the marginality of that career path: precisely because of her experience, rank, and high salary it will be very difficult for her to move to another institution or to patch her salary together from a variety of sources. Not occupying a tenured position, she faces the prospect of leaving science before reaching 50 years of age if she is unsuccessful in getting her grant renewed.

The scientists I spoke with in this status are quite capable by most measures: Ph.D.s from very good schools, several publications, their work well-cited, and supported by the NIH and NSF. It should be stressed that these people are contributing to science, they are performing, not merely showing potential. Thus the *positions* are marginal to academia and perhaps also to the social organization of science (e.g., study sections might be concerned about their access to laboratory space and consider their grant proposals somewhat differently) but as *researchers* they are not marginal to the intellectual enterprise of science.

Yet they are acutely aware of their precarious status. One said, "I had a feeling when I was looking for a job that I could have fallen off the bottom . . . it's a big jump from a postdoc to getting a job. That's the crunch time. Are people staying forever as postdocs? That must be pretty weird."

He is grateful to have a tenure-track job now, although he must provide 75% of his salary from research grants. And this is not without its stresses: "Because I'm in this . . . position I have a longer row to hoe than most—I have to be more perfect." Tellingly, he closed with, "I think it's the same position a woman might find herself in," drawing a direct parallel with another category of marginal in science.

Some also experienced extreme bitterness and feelings of powerlessness. As one said, "I'm disgusted. I feel totally that I don't have any power and it can only hurt me to complain." Others mentioned that the system was unfair (their ideas had been stolen and now they were paranoid; they were evaluated on nonscientific criteria approaching sexism); that their educations were inadequate (leaving them unprepared for the grant-writing required and for the cut-throat character of research as they perceived it); that the necessary mentorship was withheld from them (but available to others).

This vulnerability is real, not merely "felt." Two respondents had had their ideas stolen from proposals or papers under review but had no

recourse. One couldn't persuade a senior colleague to take action, the other chose to do nothing because she feared a complaint would annoy the study section and perhaps trigger a reprisal. In contrast, their more senior colleagues usually admitted only some "wariness" of competitors and one full professor said he found meetings a wonderful place to work out his speculative new ideas.

Some marginal scientists expressed a weakened attachment to science. One accomplished researcher in a marginal position said, "There is a very real sense that I might have to change careers entirely at the end of the next renewal period."

Why so pessimistic, I asked?

It's that whole lottery aspect. Doing good work and working very hard are necessary but not sufficient conditions for getting your grant renewed.

Perhaps the most important liability of such positions is their effect on the young scientists' access to collaborators. In developmental neuroscience and other highly collaborative areas of the life sciences these marginal scientists are caught in a double bind. Despite serious impediments to collaboration they need to work with others in order to be productive. But if they do develop a productive collaboration with a colleague at their current institution then eventually succeed in obtaining a "regular" job elsewhere, then their collaborations are undone when they depart.

The problems marginals face in arranging collaborations are well-described by one scientist who had been in such a position. For him, the difficulties are

partly institutional, partly self-perception: you're so busy scrambling on your own research that you don't have time to take a breath. One of the nice things about getting a job [as an assistant professor] was that I knew I really didn't have to do anything for two years. It was expected I would have a dead time, and really I exploited that dead time to work on [his next area of research, a collaborative endeavor with a colleague at the institution]. . . . But I don't think you have that kind of prerogative, psychologically or positionally, to set up those things quite as intensely if you don't have a job. For one thing, you may not be there long enough. The guy you're collaborating with has an investment too—we're both going to be here five years and it's worth our while to sink some time into it. If you're a postdoc or an overgrown postdoc everybody knows you're trying to get your ass out of there—you *should* be trying to get your ass out of there.

So in the highly collaborative neurosciences these marginal scientists are likely to have a hard time

finding collaborators. But even if they succeed in developing collaborations there is a difficulty, for such attachments substantially increase the career costs of moving to another institution.

One scientist had been successful in finding collaborators and put down scientific roots so deep that the cost of moving would be very high. Her next research project, for which she was preparing to gather pilot data, depends upon a difficult saliva assay and the use of nuclear magnetic resonance. Both are available in her current institution, which has a top-flight NMR facility and the world's expert in the required assay. To leave would mean abandoning the line of research because virtually no other institution could provide these resources and, since the project uses human subjects, it would be exceedingly difficult to conduct from a distance. Others reported substantial but less dramatic reliance on collaborators and resources at their current institutions, sometimes as mild as the "halo" effect of a senior lab director and the connections associated with the position. In general, it is perfectly natural to expect a maturing researcher in a highly collaborative science to establish such connections—indeed, scientific research as well as the scientist's success probably depend on it.

These marginal positions may be important to academic science for several reasons. Most plainly, these are scientifically productive positions, occupied by energetic young people who spend virtually all their time on research. These positions also offer training and skill development—an opportunity to acquire the tacit knowledge of the craft, about which more below. Less obviously, these positions comprise a proving ground that assists in the assessment of new faculty and allows a measure of slack in the labor force dynamics of science. For all these reasons the positions are very valuable. Yet the employment insecurity they carry is embittering, the career path tenuous, and the uprooting that will follow most successful job searches has high costs to the individual and the research system.

The Relationship between PIs and Their Research Teams

Scientists acknowledge that obtaining preliminary data is essential to getting a project funded and launching an academic career. There is also general agreement that junior people must "win their spurs" by doing independent research. Despite this, there is evidence that tight budgets with little discretionary money and sharp competitive pressures (with high productivity standards for remaining supported) are impeding young investigators' movement from their work in others' labs to their own independent careers. Thus the interests of postdocs and students—who need some time to do their independent work under another's supervision—are not al-

ways in harmony with the interests of the senior investigators who employ them (and who need a pair of skilled hands to keep the lab's output high).

Here are some specific instances:

- Three scientists relied heavily on the experience and talents of senior postdocs to carry out the day-to-day work of their labs. In fact, some carefully chose very experienced post-docs to be sure that the requisite managerial skills were there. (In one instance, "very experienced" meant twelve years of postdoctoral experience.) Yet one senior investigator fired a long-term postdoc at the urging of a funding agency that said the postdoc had been in that status too long. Thus the PIs are dependent on the advanced skills of postdocs in getting the lab's work done, without much provision for advancing the scientists' careers. Moreover, it is in the nature of these positions that they don't offer any advancement opportunity (in the academic sense, though one might be promoted from assistant through associate to "full" research anatomist in some places) and, increasingly, the PIs cannot offer their charges the scientific freedom and resources that might help them get a faculty position. On the other hand, funding agencies appear to have implicit timetables for scientists to leave these relatively dependent statuses for independence (or another career). One PI was explicitly directed to remove a postdoc from a project when the funding agency excised the portion of the research that had employed him.
- Another PI once allowed his staff much latitude to do their own work in his lab and to participate in decisions about his projects, but now he doesn't. Instead he feels obliged to squeeze out all the effort he can on his own work, often replying to their ideas, "That's very interesting, but don't try it on my time." He had been scared by his experience on an NIH study section, where he saw senior people with long-term projects lose their awards. He doesn't want that to happen to him.
- An assistant professor who also was trained by eminent scientists experienced a similar problem. When asked how much latitude he allowed his staff he replied,

Not very much. I used to do that during my first grant period. As a result I was accused by the reviewers of my scientific endeavors not being too focused, going in too many directions at once.

So he now runs a tighter ship.

When asked what were the costs of running a tighter ship he said that "it delays [the grad students'] educations in essence because I would say that the directed approach [makes] them . . . more like technicians. Probably the value of their education is their ability to master techniques rather than

necessarily to find out how to think, how to formulate their own problems . . . they will presumably do that at the postdoc level or as first-time faculty members."

- Others simply said they gave no thought to their postdocs' career development; that was left to the individuals.

Organizational Consequences

Underlying these first two observations is a painful irony: on the one hand educational institutions and their members are increasingly dependent on federal research money to pay the bills, yet on the other hand there is an increasing divergence between educational and research values in academic science.

At the aggregate level this dependence has been documented in budget analyses, studies of indirect cost rates, and the pleas and actions of university administrators (e.g., allocations of money outside the peer review system for buildings; lobbying for mega-projects and research centers). Yet it is also expressed in the actions of scientists and their employing departments.

- One assistant professor was told that his NIH project would not be renewed. He said, "At that time I already knew that the tenure decision was only one year away—and the chairman made it clear that without a grant it would be difficult to recommend me staying at this institution." For him the message in his department is clear: "The more grants you get the greater, speedier promotions one obtains, more space one obtains, more prestige." Conversely, "If you don't do that, you're out."
- After eight years of having charged between 65 and 100% of her salary to external sources another had to stand up to her department chairman, although she was untenured at the time, and insist on reducing her effort on an NIH project below 65% because her other duties simply prevented her from spending that much time on the project. Her request met with substantial resistance. Apparently her institution expected its tenure-track faculty to provide most of their salaries from external sources with relatively little concern for the actual allocation of time and effort.
- One young scientist, who provides 75% of his salary from research grants, put the matter succinctly: "Everybody knows you'd better have a grant or more."

Postdocs and graduate students are both "research labor" and students, providing a skilled workforce while they learn. Yet as they're moved from training programs onto research budgets (as training programs end and university fellowship funds are strained), the emphasis shifts from their education to

their research contributions. This raises the prospect of their being trained for short-term research purposes or industrial needs, not the longer-term requirements of the discipline.

Even those postdocs supported by independent training grants within departments are viewed by faculty as a departmental research resource. Only individually-obtained, external postdoctoral fellowships are viewed as truly independent.

And graduate students may experience the same sorts of pressures to obtain external support as faculty. In one case a faculty member whose research support temporarily ended was provided with a grad student paid for by a departmental teaching assistantship. When the scientist succeeded in getting a project supported he was told to assume the grad student's cost, despite having no funds for that purpose in the grant. He used supply money to pay the student for one year and told her to get an external fellowship to support the balance of her graduate training. In his words:

I kind of gave her a deadline, saying that if she's not able to find her own funding she will have to accelerate her research tremendously in order to graduate very quickly because I can support her for only one year, not two. . . . Trouble is, graduate students, especially at her level, take a very long time to generate any data and I didn't feel it quite right to pay her from the grant when she would presumably not generate enough results to justify subsequent renewal. . . . But it worked out fine from her point of view. [She got two fellowships.]

Another professor insists that his students seek independent support for their training:

As soon as they come into my lab I say, "I want a grant [proposal to NSF or NIH for a fellowship] from you, pronto!" I tell them, "You know, I have to be funded. If I'm not funded I'm out of my job. If you're not funded you're out of a job, so better get a grant. . . . That's the name of the game if you want to do anything in science now."

Yet another made the same point in somewhat different terms. He urges his graduate students and postdocs to "compete for funding—fellowships—as soon as possible because if they plan to be academics this is what their careers will be like."

This dependence can also be seen at the departmental level. One department hired several neuroscientists a few years ago because the area seemed to be "hot" and fundable. When these faculty had difficulty obtaining support over the years the department chairman rethought his strategy and moved back to a department with a more traditional mix of research problems. The upshot is that those young neuroscientists appear on their way out of that department.

So much for the dependence of universities on external support. Now for the irony: value conflict between teaching and research.

The tension between academic and research values was explicit in two departments. One department has a policy that forbids postdocs to work with scientists who do not have research grants, and considered a similar policy for graduate students. The rationale is that without external research funds a scientist is doing no work that could responsibly involve a postdoc or student. The department has extended this "realpolitik" principle to its educational program, offering a seminar in proposal writing as part of the graduate program. (The course includes a mock panel meeting with open criticism of students' proposals.) Getting research support is an integral part of academic science. In fact, the pressures to seek outside support are so great that some tenured faculty "continually submit research proposals without success because it is expected of them."

In contrast, a second department acknowledges the role of research funding in academic science but sees a different problem and has taken a contrary stance. Here the faculty is trying to establish a policy for allocating graduate student support by some principle that will offset the attraction of outside research support. Their rationale is that the academic subject matter and its representation in scientific research should be independent, and that it is essential to insulate the discipline from current research trends.

These departments have resolved the conflict between academic and research values in opposite ways. We should be careful not to side with either position too hastily. As cold as the first policy sounds, it does prepare its students for the competitive funding climate. And to the extent that "grantsmanship" contains tacit knowledge that can only be acquired through experience, this department attaches its students to mentors with proven skills. On the other hand, as reasonable as the second policy seems, it does perpetuate specialties that may be out of date and makes the allocation of students vulnerable to the internal politics of the department.

Some faculty members experience this ambivalence internally. For example, one newly-tenured associate professor found himself caught between the academic values instilled in him during his training (with an eminent scientist) and the requirements of research. Whereas formerly he valued the quality of scientific work above all else (and was so slow to publish that it cost him his contract renewal at a prestigious institution), he says his motto now is "I don't want it done well, I want it done Tuesday."

As internal and external support for grad students is diminished they will be supported by research grants (and contracts) at increasingly early stages of

their education. The potential effects of this are a *reduced* capacity for independent work (because they become important parts of a scientist's research effort early on) and the possibility for premature specialization as they acquire the restricted range of knowledge and techniques required by a research project. The pressure to specialize is intensified by intellectual change in the sciences, particularly the broad incursion of molecular biology into the neurosciences, and by the tight (and relatively unrewarding) academic job market, which makes work in the drug industry look more attractive and thus induces graduate students to acquire the most marketable skills while in school. Often this means intensive study of the technical skills of molecular biology at the expense of broader, substantive courses that present the classic themes and problems of the discipline.

Social Organization: Conclusions

To sum up the consequences for social organization in more sociological language, there is evidence that competition for funding may cause alienation, a weakening of norms and rules (or, perhaps equally powerful, a *perception* that such norms and rules are weak), and structural change in research universities. Consider each in turn.

First, some scientists, particularly marginals, expressed a degree of alienation from their work and from one another. To some degree this alienation takes the form of feelings of powerlessness or malaise, but the most worrisome form is *structural* alienation: a growing separation between categories of scientists (e. g. , those who have secure jobs and those who don't; between PIs and staffs); and between scientists and science because their commitment is weakening.

Second, scientists are also experiencing anomie, a weakening of norms and rules that guide behavior. The ambivalence between academic and research values is a good example, for it suggests inconsistent norms at work, and there are others:

- One scientist had under review at NIH a training grant proposal and a center proposal, both with priority scores of about 120—very fine scores indeed. Neither may be funded because of policy decisions to reduce training expenditures and a feeling that there are "enough" such centers already established. The effect of these decisions is to call into question, in this scientist's mind, the funding system's rationality and the fairness of its evaluation criteria. Others view the grant-award process as a lottery, or as an unreliable system upon which they depend. Importantly, these are NOT scientists to whom the system is unknown; rather, these are people who have served on NIH study sections and understand their inner workings.

- Similarly, scientists in marginal positions observe a weak connection between their performance (which has been quite good) and the rewards they've received (meagre). In their minds the rules that govern the allocation of rewards are askew.
- And the young can't rely on their elders to socialize them, to teach them the rules, for the rules are in flux. As one said:

• My biology professor grew up in the glory days of easy money. . . . What I don't think anybody in my generation knows is that the rules are totally different now. Those 60s rules that the old guys play and the middle-aged guys play don't apply to us. There's no easy money and there's no toleration of eccentricity. In other words, you can be brilliant but a geek and still get money in the 60s—by that I mean you can have great ideas but write a bad grant—you'd still be given money. When I was a grad student the older generation would sit around and talk about which schools in northern California they would consider working at. That mentality gets transmitted down to the succeeding generations, but those rules don't apply. I've known people who've gotten screwed, including myself, because we've had those expectations subliminally, even though rationally we know they can't be true. . . . Now you have to be perfect, you have to write perfect grants—it has to be well-written as well as brilliant.

- Ultimately, some experience a weakened connection to science. One excellent young scientist, when asked her plans should a pending proposal be denied support, described a business venture she would undertake. Another told me of a promising researcher whose support ended and who is now running a health food store. In both instances we will have lost a talented and highly trained scientist (each had done at least two years of postdoctoral work). Yet another, senior, scientist fully expects to leave science in two or three years when her current funding expires.

Third, academic research is undergoing structural changes which are expressed in several ways, including the nature of the scientific apprenticeship, the division of labor in research teams, career mobility patterns, and in the organization and composition of departments. Each of these deserves brief elaboration.

- We might view graduate study, postdoctoral appointments, and marginal positions as part of the scientific apprenticeship program. As in more traditional apprenticeships, this program serves to train new workers in the craft, certify them for membership in the guild, and socialize them in the tacit knowledge and values of their chosen career. But apprenticeships also serve to control the rate of entry into a craft, slowing growth to avoid

crowding the labor market and lowering wages, and to shape the characteristics of entrants, permitting guild members to pass along advantages to family and friends and to exclude undesired categories of workers.

Viewing marginal positions in science as apprenticeships raises several issues. For example, such positions may not provide the freedom and resources necessary to become properly apprenticed as a scientist. The constraints, measured in terms of both budget and autonomy, are often so great that the appropriate skills are not acquired. Similarly, some have suggested that the socialization is not adequate—that the right rules are not taught—thus failing at another function of the apprenticeship. (The story about the two departments that adopted opposite policies for allocating postdocs and students to faculty neatly captures the ambivalence experienced on the teachers' side of the relationship: What values should be taught?) Moreover, the scientific apprenticeship may not lead as surely to employment as does its more traditional counterpart.

More serious questions can be raised when we think of the scientific apprenticeship as a means of restricting the flow and shaping the characteristics of new workers entering a craft. For in science the hiring of people into such positions (and their dismissal from them) are less formal, with fewer safeguards of individuals' rights, than hiring into faculty jobs. And whatever the formal guidelines for the treatment of such people on the job, it is very likely that informal norms and practices have substantial power. Thus, affirmative action laws and institutional policies may have less influence on hiring decisions and work relations in this apprenticeship system than they do in the open workforce. The recent growth in litigation about letters of reference may further harm the delicate relationship between master and apprentice.

- The relationship between PIs and their staffs may have taken on some of the characteristics of the relationship between capitalists and workers. Especially if the PI is a full-fledged member of an academic department, he or she controls (but, like a corporate executive, does not own) the productive resources of science: laboratory space and equipment, research and travel budgets, and employment. In contrast, research staffs are dependent on these resources to make their livings and must trade their labor for access to them. There is even an analog to the notion of surplus value and its expropriation in that PIs may "expropriate scientific credit" by attaching their names to publications produced in their labs on which they have done very little work. (Interestingly, labs are often referred to as "shops," as in, "that's a paper from Gilhooley's shop.") Of course, this framework would predict

the alienation discussed at length in the early part of this paper.

- There has always been downward mobility, on average, from the Ph.D. institution to the first job. But a shortage of academic jobs, expectations of significant research experience before the first (tenure-track faculty) job, and the availability of support for such research might combine to increase the waiting time between Ph.D. and first job, especially to increase the amount of time spent doing significant, independent work as a marginal. (After all, that's often required to get the job.) As much of this work is collaborative, scientists will put down roots while waiting, and these attachments will increase the cost (and disruption) of leaving. In effect, the academic career is being restructured in a self-defeating fashion, removing some autonomy and many of the rewards from the early career while building a career change in at the wrong time.

Yet there is a dilemma here, for scientists recognize that it doesn't "look good" to remain at the institution where one did graduate or postdoctoral work because it detracts from one's image as an independent investigator. There is also a more tangible cost to remaining at the same institution: these stayers often "inherit" space from their postdoctoral work, usually without formal agreement, and are not provided with the seed money and other resources to initiate a research career given to someone who has been recruited. Thus there are strong incentives to leave yet significant career costs to doing so.

- Finally, departments are the organizational embodiments of the compromise between academic and research values. Research fields move on but departments are frozen by their tenure and hiring decisions. This becomes especially critical in periods of slow growth or stability, where a department could be "frozen out" of the exciting new trends because it was successful in hiring and tenuring the stars of an earlier era who, locked in while young, remain a major component of a department that now lacks faculty in the latest hot areas. This may be happening now to departments that cannot hire people with training in molecular biology and are "frozen" with first-rate faculty educated in the cellular and systemic traditions. And this tension may contribute to the proliferation of research centers and institutes on university campuses, for these new organizations sidestep the intellectual rigidities of traditional departments and allow more flexibility in hiring and promotion standards.

Accommodating scientific change

Two intertwined aspects of scientific change should be considered. The first occurs when scientists change their research directions in order to remain funded. Some of this change may have been intended by policymakers and program managers ("dirigisme," to Rip and Nederhof, 1986), but some may be unintended, undertaken by scientists as they try to reconcile their career needs with the resources available in their environments. The second sort of change occurs in the science itself, in the form of new theories, methods, findings, or standards of proof to which scientists' work must conform to remain accepted by the community. The advent of molecular biology in the neurosciences offers a good example of this. These two sorts of changes are interconnected, of course: it is through the actions of scientists that science changes, and changes in science present challenges to some scientists and opportunities to others.

Researchers who needed to alter their interests to remain funded often chose to move into research areas with health applications that had been identified for policy emphasis. These scientists tailored their proposals to the Aging or Eye Institutes, for example, in order to increase their chances of support. They typically did this reluctantly, intending to return to their more "basic" interests as soon as circumstances permitted. Here are some examples.

One assistant professor, in dire need of research support, decided that the Heart Institute would be a good target for a proposal. He succeeded in securing support, although the resulting project isn't entirely satisfying to him because it is

... essentially a measuring type of science ... let's take an animal with high blood pressure and an animal with low blood pressure and measure as many parameters as we can and see whether anything is different ... that type of research I don't particularly like and I proposed it simply for the reason of obtaining money to do research.

Another young scientist redirected her work from basic developmental neuroscience to aging, with some success, but said, "development is still where my heart is." Both plan to move back to their basic research interests when possible.

In contrast, a tenured associate professor who had been without research support for several years (after a well-funded early career) "looked around and said there's the NIA and maybe I should get into aging. . . [But] I believe if I do what I want the money will be there from some source or I'll get it done anyway. . . I would rather do this project with no money than do something else with more money. Right now this

is where my heart is." Clearly, the ability to follow one's heart depends in large part upon having a secure job that allows risk-taking.

Others chose a different strategy. Judging that the future of the neurosciences relied on molecular biological approaches, they chose to redirect their work to that lower level of analysis.

Molecular biology. The introduction of molecular biology has had profound effects on developmental neuroscientists, creating opportunities for scientists with the requisite skills or "intellectual mobility" to redirect their work and driving others from lines of inquiry that were once judged productive (because the standards for demonstrating causality had changed). Perhaps this is not a classic Kuhnian revolution—it is not driven by the accumulation of anomalies, for example—but it certainly qualifies as a substantial change in the paradigm.

Virtually everyone I spoke with has been affected in one way or another by the advent of molecular biology in the neurosciences, so scientists' preparation for the shift of investigation to the molecular level was a recurrent theme in the interviews. Very senior scientists with long research records and funding histories were just as concerned as their younger counterparts. Some of the established scientists expressed frustration ("I can't keep the terms straight.") and tried a variety of strategies to accommodate the trend. Some relied on getting collaborators or postdocs with the needed skills into their labs. Others tried to "bootleg" some molecular work on current projects, thereby obtaining preliminary data, establishing credibility as investigators in the area, and gaining familiarity with the methods and results. Their motives for doing this varied.

Some felt that their research areas were "ready" for molecular approaches, and were driven to the molecular level by desire to contribute. But others noted that study sections "wouldn't even look at proposals" that didn't contain molecular biology, or felt more subtle pressure, as did one scientist who felt his proposal was not getting "as favorable a hearing [as it deserved] because it was not molecular enough. . . . One of the major criticisms of it was that it was still [after repeated revisions] too descriptive and wasn't getting at causal mechanisms. . . . [N]obody said, 'You'd better start doing molecular biology on your system' but I think that's what they were getting at." His perception may well be accurate, for two scientists who served on study sections and were comfortable with molecular biology said plainly that they pushed strongly for such work in their evaluations of proposals, and an agency official explained the steps he took to insure that molecular biology was represented among his panel and reviewers.

Some scientists even presented "five year plans" to move their work from its cellular version to the

molecular level. For example, one young scientist has decided to shift his research to the molecular level because that is what twentieth-century biology will be remembered for and he wants to be a part of it. According to his plan,

ion channels are being cloned, and once they're cloned there are probes for them, that 3 or 4 or 5 years down the line, when I'd played out the electrophysiology to the single-channel level and I knew how the channels were changing, there would be molecular probes that I could use.

Although this path is taken by choice it is not without substantial risk. There are dangers in making the transition to molecular work, and he knows of labs that dedicated themselves to cloning but were so overwhelmed by technical difficulties that they become unproductive. To avoid this, he intends to "parasitize" his current grant, using some of the time and resources it provides to learn the technique and establish himself in the new area. He also hopes to produce an adequate stream of publications from the existing project to keep his career afloat. Yet here also lies danger, for as he directs his attention to the molecular level he feels other labs catching up on him in his "bread and butter" area. In the section on quality, below, other manifestations of risk-taking in science will be discussed.

But not all this movement of intellectual interest was by choice, effected in an orderly manner through a plan that keeps the scientist funded and productive during the transition. Other scientists went without external support, in one case for a period of five years, as they tried to retool and reorient their work.

In other words, the need to remain employed and funded and to keep up with new developments are important contingencies in a scientist's research life which help shape their "research trails." As with occupational change, it is one thing to say that in the aggregate one class of occupations is being replaced by another; it's a quite different matter to be a candidate for replacement oneself.

Effects on the quality of science

The competition for funding and the level of available support influence the quality of science in two ways. The first and most direct way is through the quality of work performed at the lab bench, including ability to produce preliminary results, replicate experiments, and extend findings to a broad array of substances or specimens. I will discuss this briefly and give some instances of these effects. The second sort of influence occurs through scientists' willingness to take risks and their ability to absorb the consequences of risky ventures that turn out unfavorably. This is a more complicated process that will be detailed in the longer portion of this section.

The "bench quality" of science. Much of the bench quality of science—the care that goes into the results produced—depends upon the availability of slack resources. Preliminary data and replications, for example, serve to make more convincing arguments to others and to oneself. Yet tight budgets and competitive pressures remove some of the resources needed to do this, the former by eliminating sources of discretionary support, the latter by redirecting money into more "productive" (i.e. publishable) channels.

Problems arising from the absence of seed money were cited by two scientists, one a marginal scientist whose position lies outside the usual academic hierarchy, the other a tenured associate professor. The marginal scientist's research plans led her to a study that would require the use of NMR to compare two different categories of patients. To strengthen the proposal, she hoped to run ten cases in each category, showing that the predicted differences are in fact observed. An NMR facility is available at the institution and she was able to negotiate a reduced rate for her pretest. Despite this, funds were not available to do the pretest and the proposal will include only a single NMR image, a token demonstrating that the equipment is available and that the proposer can interpret the image, but a far less persuasive argument that could be made by comparing a small sample of cases. The tenured scientist changed research interests in mid-career and wrote a proposal to work in the new area that made an analogical argument but lacked any data at all. He realized that preliminary results would have strengthened his case but, lacking seed money, was forced to advance the weaker argument. His proposal was funded, although its priority score was very near the borderline, and he has been very successful in his new area.

These cases are important for two reasons. First, they indicate that scientists may be unnecessarily in jeopardy of not being supported because they are unable to conduct preliminary studies that would strengthen their proposals. Second, the quality of their proposals suffers in a very concrete sense, for a proposal supported by preliminary research can be more cogently argued, more insightful, and better-planned (because there are fewer uncertainties) than a proposal written without preliminary work. Taking the funding agency's point of view, a project supported by preliminary work faces fewer unforeseen difficulties and has better prospects for working smoothly.

Risk. "Quality" in science means more than the straightforward quality of bench work described above; it also includes more speculative flights of imagination, intellectual leaps which, at the time taken, were hardly assured of success. Changes in the funding environment may also threaten this type

of quality by reducing scientists' willingness and ability to undertake risky projects. This might happen if the costs of failure become so great that few can afford to take a chance on an idea that may be sound but does not assure success. Moreover, this aversion to risk may become institutionalized, resulting in study sections and agency officials unwilling to support a project that has some chance of failure. (The presentation of preliminary results, discussed above as an element of the quality of science, serves to allay scientists' concerns about the soundness and workability of their proposals. In the limit, some say, it is ideal to complete the entire study, or at least its first year, before submitting the proposal.) Thus the quality of science depends in some measure on scientists' ability to take risks and absorb the consequences of failure. Sometimes these risks may be taken openly, in a proposal, while other times they may be part of a speculative series of experiments conducted behind closed lab doors. For their part, scientists carefully assess their risks, albeit without benefit of calculations, and can talk in detail about the riskiness of their work.

One facet of risk is the risk of coming up empty-handed in a project. For example, one scientist regretted posing so technically difficult a problem in her recent NIH proposal, for she was having trouble getting good results. She was at the point of cutting her losses and resigned herself to publishing an approximate solution, based on data that were not very good in order to salvage some "yield" for her efforts. Because of this she felt her grant renewal was in serious jeopardy.

Too little risk also can be dangerous. One scientist had been very conservative in his past work:

I was doing experiments where I was very confident they would work out and fairly confident what the answer would be. . . . It was always sort of good, solid work—sort of a good round steak—but then after a while people got bored with round steak and actually I did too. The work has held up but it just hasn't been the breakthrough stuff. It hasn't had the impact.

The work *was* solid enough to earn him tenure at a good university which in turn has given him the resources to shift, with very little external research support, to an area where "for the first time I really don't know what the answer is. . . . I'm now chasing after this enzyme and it's possible that it's not involved at all—it may be epiphenomenal. I don't know. I believe it is understandable and the chase is fun."

Over the years he has developed an approach to the problem that blends experiments having a high probability of success with some that are quite risky. The latter experiments are technically very difficult yet, if successful, will have a very high payoff. He

has repeatedly submitted proposals to do this work without success, but is confident that he has developed enough preliminary data and experience to convince the agency to support him.

Another tenured associate professor's story is strikingly similar: his early work avoided risk, trying to follow closely on his or another's research. He avoided "proposing things that are highly speculative . . . [it is] hard enough to get stuff funded that isn't at all speculative, let alone put yourself out on a limb." After being awarded tenure he has had to ride out a lengthy spell without external research support, relying on internal distributions of seed money and graduate students to keep his lab working. His plan is to start with a small grant and parlay it into a full-blown research program. To increase his chances of obtaining support he has chosen to work in an area of the brain that is currently of substantial interest to others in the neurosciences and to give his proposal an explicit connection to health concerns so it will appeal to clinical interests within NIH.

Unsurprisingly, most scientists try to balance their risks by choosing problems that allow them some "bread and butter" publications while holding out the possibility for the "breakthrough stuff" or by selecting a research strategy that gets at the fundamental scientific question while allowing substantial productivity. One assistant professor sketches this second strategy as he warns scientists about

Trying to go after a difficult problem which, if attacked directly, one might come up empty-handed. For example, had I remained in tissue-culture and tried to attack [my research problem] from that point of view . . . I might have come up empty-handed. [But by] attacking the problem in a somewhat more indirect way I can generate data of interest to the scientific community. Nevertheless, even if I do succeed and characterize very well the appearance of the different receptors at different stages I still don't solve the basic mystery of the question I've set out. . . I will not discover the reasons why those synaptic connections are formed.

In other words, by choosing an approximate rather than a direct approach to his problem he is certain to be productive, to make a series of incremental contributions to knowledge, although this approach will not yield the definitive solution.

Study sections and panels may not be prepared to evaluate strikingly new ideas and results. One scientist had made a breakthrough and in fact had a report of that work in press at a very prestigious journal. Yet he decided not to mention those results in his renewal proposal but to rely instead on proposing to do "more of the same" for he feared that the study section would not believe his work and not support his proposal. His strategy was successful.

Others have mentioned that the grant proposal must be "perfect" and "flawless," that a proposal raising the least doubt in a reviewer's mind is likely to fail.

One researcher was very pessimistic about the prospects of getting a grant renewed. When asked why, she said, "I guess because I've been on a study section. I look at the grant [proposal] and I see mistakes in the grant, and I might make mistakes in the competing renewal and somebody might notice them." And any flaw would be fatal. This view is convincing on arithmetic grounds alone, for as priority scores and ratings crowd the top end of the scale, a very few mediocre scores will be sufficient to put a proposal out of the funded region.

Others point to past difficulties they've had in convincing federal agencies to support them while they begin work in a new area. Scientists are faced with a dilemma: their work must be sufficiently innovative to generate enthusiasm but tame enough to promise results. Risk may be a necessary element of good science yet the system that evaluates proposals is risk-averse and their employment circumstances are intolerant of failure. And they must somehow manage to keep up with technical changes in science while remaining funded, although funding agencies are reluctant to support people as they enter a new area.

This extended account from a scientist captures the frustration well.

I think the funding environment from the government doesn't give you much elbow room. If I had written a grant on that [topic], they would have probably come back and said, "he doesn't have any experience, any track record, in this line of work, and so I don't think we can give him funds." And probably since I hadn't been doing it as well, I probably would have made some little error, maybe big error, in writing the grant proposal. And the system for reviewing grants at the federal level is extremely criticism-sensitive: you get any kind of criticism that potentially calls the research into question at some level, it knocks your score down and hurts very badly." Even if the core idea is very good, and many times the criticism comes not because the idea is incorrect but because the reviewer hasn't understood what you've proposed. In fact, that happened to me on the last go-round. . . The study I proposed was designed to get around one particular problem that always creeps up in these kinds of quantitative studies, and we did it in a way to exactly avoid that problem but it didn't come across to the reviewer. And it was about the only substantial criticism . . . and it got dinged for that reason. Fortunately it survived . . . by the skin of my teeth, I feel.

Our research system may have adapted itself to having slack resources which permit scientists to explore areas other than those proposed, to conduct a portion of their future work on current support, and to integrate their research tasks with teaching responsibilities by providing post-docs and graduate students with a measure of autonomy. But that may be changing. Higher standards of productivity, closer accountability to funding agencies (e.g., in the form of NSF's evaluations of recent project products as a part of proposal review and closer attention by NIH study sections to whether a project has met its objectives), and tighter budgets combine to reduce the latitude available to scientists.

University policies recognize the importance of putting discretionary resources into scientists' hands by returning a portion of charged-out salary or overhead to the school, department, or investigator. Agencies recognize this too, most notably in the form of NIH's Biomedical Research Support Grants (BRSGs), which provide institutions with a small amount of money (calculated as a percentage of NIH-sponsored research underway at the university) to be distributed to faculty for travel, equipment, and seed money for new research. Yet as overhead rates are reduced (both through negotiations between the federal government and individual institutions and, perhaps, through more sweeping policy changes) and academic salaries on research grants are limited, universities are ending programs that pass discretionary money along to investigators. At the federal level, the BRSG program is perennially threatened with extinction. Thus investigators can expect to have less discretionary money in the future and, in the specific sense described above, the quality of science is likely to suffer.

Conclusion

Let me close by drawing from the preceding themes four sets of researchable questions that policymakers should consider.

First, on the matter of academic marginals.

1. How many marginals are there and how do the numbers vary by state of the field? For example, are marginals proportionally more prevalent in growing or declining fields than in fields that are relatively stable? What are these scientists' contributions to science and how do they differ from those of their colleagues in more traditional positions? Why are there differences in the numbers of marginals across fields and what purposes do they serve? In other words, how is science served by sustaining people in these ranks? Or do such positions merely provide transitional employment, absorbing the overproduction of our universities?

2. Is the number of marginal positions growing? Do we wish it to grow? In what ways? Especially in

a period of projected stability or decline in college enrollments, universities might be reluctant to increase their ranks of tenure-track faculty. But research priorities may create demand for doctoral scientists in some fields and universities may respond by hiring scientists into these non-faculty ranks. The trend might be further fueled by the growth of research centers that respond to industrial and governmental research needs which may be more intense and of shorter duration.

3. Given the answers to the above, should universities take steps to limit employment in marginal positions (and the articulation of categories of marginal employment: some schools are developing very fine gradations within this category) or should such positions be encouraged and institutionalized? And, of course, should funding agencies endeavor to support people in such positions?

Second, on the related themes of alienation and the divergence of educational and research values.

1. What should be the relationship between teaching and research? Education and research are diverging in many senses, ranging from fundamental values through organizational forms (such as institutes, centers, and departments) to career paths and employment contexts. If there is such a divergence what purposes are served by it? What harm is done by it? If we wish to integrate education and research, how can this be done with minimal cost to the objectives of both?

2. Are we restructuring the academic research environment in a way that trains young scientists to be incapable of independent work? The needs for substantive specialization and technical competence in research project staffs encourages PIs to train their graduate students and postdocs rather narrowly. This tendency is exacerbated by the demand for technical sophistication in the private sector.

3. How will the social organization and quality of scientific research be influenced by the growing divergence of interest between senior investigators and their staffs? This is especially important for those senior scientists who rely most heavily on their junior colleagues to do the day-to-day laboratory work and for junior scientists who rely on their employers to provide training, advice, and sponsorship.

Third, how do funding policies interact with changes in the content of scientific research areas?

1. Which scientists are most sensitive to changes in funding priorities and how do they respond? How enduring are the accommodations scientists make to changes in the funding environment? How enduring would we like them to be? What are the consequences of these shifts in research directions for the research destinations and the scientists at work within them?

2. How do funding policies affect the structure of academic departments and research areas? Are

those policies affecting evaluation criteria and the distribution of specialties within departments? Are they causing undesired intellectual migrations of scientists or appropriately reallocating talent and resources?

3. How can we provide for the retooling of senior scientists without unduly disrupting their research and the careers of those working with them? This is especially important in such areas as developmental neuroscience which are richly-joined to the findings and techniques of other fields of science because the complexity of work at the laboratory bench increases with every change in any of the connected fields.

Fourth, what are the implications of new funding policies for the quality of scientific research?

1. Are high levels of accountability and close budgeting compatible with high-quality science or do scientists depend on financial flexibility and discretionary resources to do their work well?

2. It seems we are trading redundancy and robustness for efficiency and accountability. Is this a good bargain? At what point might it become a bad bargain?

3. Does risk-taking contribute to the quality of science? How? Does the current perception that the funding system is intolerant of errors substantially reduce the risk scientists are willing to take?

References

Cozzens, Susan, ed. "Funding and knowledge growth." Theme section of *Social Studies of Science* 16:1 (1986).

Institute of Medicine. *Personnel Needs and Training for Biomedical and Behavioral Research*. Washington: National Academy Press, 1985.

Kerr, Clark. *The Uses of the University*. NY: Harper and Row, 1963.

Kruytbosch, Carlos and Sheldon Messinger. "Unequal peers: The situation of researchers at Berkeley." *American Behavioral Scientist* 11 (May-June 1968): 33-43.

National Research Council. *Postdoctoral Appointments and Disappointments*. Washington: National Academy Press, 1981.

Pearson, Richard. "Researchers living on the fringe." *Nature* 327 (1980): 86.

Phillips, Don and Benjamin Shen, eds. *Research in the Age of the Steady-State University*. Boulder, CO: Westview, 1982.

Rip, Arie and A. J. Nederhof. "Between dirigisme and laissez-faire: Effects of implementing the science policy priority for biotechnology in The Netherlands." *Research Policy* 15 (1986): 253-268.

Roberts, Stephen, ed. *Academic Research in the United Kingdom: Its organization and prospects*. London: Taylor Graham, 1984.

Teich, Albert. "Research centers and non-faculty researchers: A new academic role." In *Research in the Age of the Steady-State University*, edited by D. I. Phillips and B. S. P. Shen, 91-108. Boulder, CO: Westview, 1982.

Christopher Freeman: Economic Analysis of R&D

Citation read by David Edge, President, at the 4S Banquet, 20 November 1987.

The winner of the 1987 John Desmond Bernal Prize is Emeritus Professor Christopher Freeman, the first Director of the Science Policy Research Unit (SPRU) at the University of Sussex. The award is given in recognition of his contributions to the social studies of science—in particular, for his work on the economic analysis of R&D, and for the way in which both he and SPRU have advanced the theory and practice of science and technology policy.

Chris Freeman was born in 1921. He served in the second world war in the 1st and 5th Battalions of the Manchester Regiment, and was educated at Sandhurst Royal Military College and the London School of Economics, where he graduated in 1948 with First Class Honors in Economics. After several years of teaching and research (particularly commercial market research), in 1959 he joined the staff of the National Institute of Economic and Social Research, in London. There he started the distinguished and pioneering series of research projects into expenditures on R&D, R&D manpower studies, and surveys of research and innovation in a succession of industries. This led, in 1966, to his appointment as the first Director of SPRU, a post that he held until 1982. He is still actively involved in science and technology—latterly on problems of technology and employment—and the fact that a longstanding commitment in Sweden means that he cannot be with us tonight (a fact that he deeply regrets) is itself testimony to his continuing popularity as a lecturer and consultant.

Chris Freeman's publication list contains no fewer than 149 major items—many of them seminal books and papers of wide influence. He has, for instance, been central in the recent revival of interest in "Kondratiev Long Waves." But to confine one's assessment of his career to his publications would be to miss its central point: for Chris is committed to a strategy in which his *research*, and that of his colleagues, is related to *practice*, by effective personal advocacy. By my count, over a third of Chris's publications list consists of specially commissioned reports, and of named (and prestigious) lectures. This reflects the heart of Chris's life work: he was in *demand* as an adviser, and widely respected and influential in this role. Here the appropriateness of the award of the Bernal Prize is more marked.

Chris has recently written a moving appreciation of Bernal's work, and of its influence on him.¹ He writes:

As a student at the London School of Economics I was fortunate enough to hear Bernal lecture several times before, during and after the war, on the *Social Function of Science* and related topics. These lectures were not part of the regular curriculum; they were events organised by various student societies. But I learnt more from them than from any lectures in the regular curriculum with the possible exception of some by Harold Laski.

One lecture in particular stands out in my mind. It was in 1947 and as an ex-service student I was dissatisfied with much of the regular economics curriculum which seemed to me and most of my fellow students to be even more remote from the real world than when I started economics in the 1930s . . . It was . . . extraordinarily refreshing to hear Bernal talk about the role of scientific research in the economy and in war from firsthand knowledge of government, the armed forces and industry . . . But even more stimulating was his discussion of the future role of scientific research in the civil economy . . .

And, later, Chris says of Bernal that . . .

He believed profoundly that the interactions between science and the economy were mutually beneficial and his main criticism of contemporary social science was that it was too much of an abstract "spectator" subject and was not sufficiently involved in policy-making and real world testing.

It will be clear that Bernal would have recognised Chris Freeman as a social scientist after his own heart! Warren Hagstrom and his colleagues on the 1987 Bernal Prize Committee are to be congratulated on the precision of their judgement.

Perhaps it will be by the existence and influence of SPRU that history will best remember Chris Freeman. Again, Bernal himself would have nodded. Chris has written, of Bernal's book *The Social Function of Science*, that . . .

It was seminal in the full meaning of the word: it not only give rise to many new ideas in the social sciences and a new subject—the "social studies of science"—but also to new policies and indirectly to

1. C. Freeman, "Bernal and the 'Social Function of Science'," draft chapter for book on Bernal, edited by Francis Aprahamian (mimeo, amended 1985). I have taken all my quotations from this mimeo draft.

many new institutions both inside and outside government. To go through the contents list at the beginning of the book is to draw up the agenda of most of the debates on policies for science and technology since the second world war.

Similar reflections run through one's mind on reading any of the recent SPRU Annual Reports. For SPRU is indeed an institution in the Bernal tradition: and it was, in important respects, Chris Freeman's creation.

SPRU sets the agenda, even for those of us who profess a disdain for "policy-related research." In 1966, when it was clear that Chris was going to Sussex, and I was going to leave the BBC to go to Edinburgh, I arranged to meet him (for the first time) over lunch at a restaurant near the National Institute. We fell to discussing what the research agendas of our two Units might look like. Chris talked of having the measure of Bernal's problem of assessing the *inputs* to scientific research—but how could one measure the *output*? You won't believe this (and Chris has probably forgotten it)—but much of our lunchtime discussion that day concerned *the strengths and weaknesses of citation methods*! Some things haven't changed much in twenty years—and SPRU has played a not insignificant role in keeping that topic (among a host of others) on our agendas!

So it is with the greatest pleasure that I, on behalf of the Society, nominate Christopher Freeman for the 1987 John Desmond Bernal Prize.

David Edge
Edinburgh

The Prize was accepted on Freeman's behalf by Diana Hicks. Her remarks follow.

Last year Michael Mulkey accepted the Bernal Prize with a multivoice, new literary form acceptance speech. This year I find myself in the interesting position of a junior researcher in a multivoice acceptance speech. In this role I circumvent the usual dependency of such voices in these texts because I am a truly different person. Although all this is tremendously interesting, if I explore the potential inherent in this situation I would be hijacking the text, which I do not wish to do. Therefore I will unreflexively accept the Bernal Prize for Professor Freeman.

In so doing, I would only like to add to David's praise the perspective of the Science Policy Research Unit. For it must be said that, although Chris no longer teaches, all SPRU staff and students are truly students of Chris. We have read *The Economics of Industrial Innovation* and are guided not only by his vision but by

the intellectual rigor and humanity evident in his work and in his life. Such an intellectual debt to one man is surely rare, especially when that man is so fundamentally modest and self-effacing and recognized by all who know him as a wonderful person.

When Chris is due to speak, one would never identify him as the speaker among the crowd of important-looking people milling about beforehand. Yet after hearing him speak, one never forgets. As Chris could not be here tonight, for those who have not yet heard him speak, that pleasure remains. I can only read a statement he has prepared.

Diana Hicks
Science Policy Research Unit
Sussex

Acceptance Statement

Through my colleague, Diana Hicks, I would like to express my warm appreciation of the honour of the award of the Bernal Prize. I regret very much that I was unable to attend the conference myself but I am delighted that she is able to say a few words on my behalf. She is one of a group of colleagues at SPRU who are working very much in the spirit of Bernal in attempting to measure and map characteristic features of the scientific community and its work. I value the award as a tribute to their work and that of my other colleagues as well as my own, since the Unit has always tried to work as a collective group and anything I have been able to achieve in the last 20 years has been intimately associated with the work of the Unit as a whole.

So far as I am concerned personally, I am particularly happy that the Society for the Social Studies of Science has designated this award as the "*Bernal Prize*." I was directly inspired by Bernal to enter this field of work when I first heard him talk about "*the Social Function of Science*" in 1939. Much later, at the time when SPRU was set up in the 1960s, he gave us every possible encouragement. Personally, I believe that the two parts of his book: *What Science Does* and *What Science Could Do* remain the basic agenda for all our work. I also believe that his Utopian, long-term, humanistic vision of science contributing to the elimination of poverty, under-development, and war remains an inspiration. For that reason especially, I appreciate the honor of this award.

Christopher Freeman
Science Policy Research Unit
Sussex

Workshop on Indicators of International Technology Transfer

22-25 June 1987, organized by the Center for the Interdisciplinary Study of Science & Technology (Northwestern University) and the National Science Foundation (NSF).

NSF gathered some 35 diverse professionals in Evanston to grapple with tech transfer (T^2) indicators. And grapple we did— T^2 is only one aspect of a complex technology development process. That process can be conceived as a "technology delivery system" with many component processes (and no set definitions). New technology can come from invention or from T^2 . T^2 is meaningless without technology absorption (tech mastery to enable effective utilization). Another perspective considers the application of technology in terms of innovation and diffusion processes. These require more than technology understanding—David Teece emphasized the role of "complementary assets." Finally, much of the interest in T^2 stems from eventual implications for economic competitiveness in technology-intensive products. Further complexities arise when one notes that "technology" may pertain to either production processes or final products. The workshop touched on all of these aspects of technology development.

Given such a complex system, how does one measure what is happening with an eye toward policies to facilitate national interests? Workshop presentations discussed available "indicators" from the perspective of the analyst (secondary statistics, none designed to tap T^2 per se), particularly in the keynote presentation by conference organizers Radnor, Wad, and Schlie; also by Porter. Others discussed issues as seen by the providers of such statistics (e.g., Anandkrishnan re UN data bases; Martinez re Latin American data). Moge presented an in-depth consideration of what NSF might do to compare innovation processes. Many presenters raised conceptual issues concerning what indicators are indicating (e.g., Barton on changing forms of foreign investment; Dosi on the evolving "Multipolar" US roles). Others, undeterred by the serious data problems addressed by many, drew on impressionistic data to foretell what is happening to T^2 (e.g., Gellman on emerging trends; Dedijer on the increasing importance of "intelligence").

Discussion of a number of specific indicators pointed toward real limits to "face validity." The implications of "Foreign Direct Investment" for T^2 become muddy when one ponders the US moving from the greatest donor to the greatest recipient in a few years. The measures of licensing of technology—Technological Balance of Payments/Receipts—appear to relate to different stages in the technological de-

livery system: payments as an "input" indicator; receipts as an "output" measure. Multinational corporation (MNC) sponsored R&D in a country other than its base may entail transfer of technological knowledge into and/or out of that country (e.g., consider Japanese investment in R&D in the US). Tech transfer may not neatly displace invention; rather, some evidence suggests that T^2 is most effective where indigenous R&D is greatest.

This complexity presents dilemmas—one needs micro level measures to understand tech development processes, but macro level indicators for policy considerations. One suggestion was *impact indicators*—aimed at understanding the effects of T^2 , not just its occurrence. Another was to not demand that macro indicators be explanatory—as Dosi pointed out, the price index does not explain inflation. T^2 takes place in many, rapidly evolving forms (one of the five working groups noted over 20). The Workshop representation highlighted another complexity—some technology watchers are interested in what is happening in the NICs (the Newly Industrializing Countries), a non-homogeneous group including such small Asian nations as Singapore and larger, partly industrialized nations such as Brazil; others are concerned about the LDCs; and, unfortunately, the best data pertain to the OECD countries.

To monitor such complex processes, an easy recommendation would be to have many, multiple measures. However, as emphasized by Cozzens and Hill, users such as Congressional staffers and *USA Today* want a simple picture. Both quantitative indicators (hard to develop new ones) and qualitative indicators merit consideration.

The Workshop raised questions about the utility of certain concepts—e.g., high vs. low technology; product cycles. On the other hand, it postulated value in considering T^2 in terms of the "regimes of appropriability" of given technologies in given settings. Participants also supported a 2×2 framework of receivers and senders by governments and enterprises. The sense of the Workshop was that T^2 indicators should not be used to justify control actions by governments because we do not understand the impacts of T^2 sufficiently. The Workshop wrapped up with a series of specific suggestions for NSF. We will look for their realization in the future. In the meantime, look for a Workshop Proceedings sometime (contact Jason Christian at NSF, Science Indicators group) and the 1987 *Science and Engineering Indicators* (without a separate International chapter) in January, 1988.

Alan L. Porter
Georgia Institute of Technology

Rohm and Haas: History of a Chemical Company, by Sheldon Hochheiser (University of Pennsylvania Press, 1985)

Corporate histories are extremely valuable documents for, among others, the student of business management, economics and history of science. When studied in isolation, their usefulness depends very much on the importance of the corporation they describe. But when a set of such histories of companies within an industrial sector are studied systematically, they provide far more important insights to the development of an industry. When—as in the case of chemicals—the industry is “science intensive,” they also become valuable tools for the study of the history of the science. Thus the Rohm and Haas Company must be commended for supporting the project which led to the publication of this book, and one hopes that their example will inspire other corporations to follow suit and enrich in this way our understanding of the history of chemistry and of chemical technology.

The strength of the book is to be found in Hochheiser's description of the technological innovations that Rohm and Haas has introduced over its 75 year history. The company's roots in Germany and dependence during the first thirty years of its history on the advances made there by Dr. Otto Rohm and by other German chemical companies are very well described as is its gradual emancipation, which resulted from Otto Haas's conviction that research should be the company's driving force.

The technological tradition of acrylic acid chemistry comes out most strongly. Traced back to Rohm's 1901 doctoral dissertation, it developed by a number of short and long steps into a near monopoly for a wide spectrum of products. The technology was unrelentingly supported by Haas throughout his lifetime, while the products that were marketed resulted sometimes from luck (Plexiglas sheet), from dogged research over long periods of time (emulsion paints), of from agents external to the company (Acryloid hydraulic fluids). Markets were found where the technology led.

Another aspect that comes out clearly is the importance and characteristic of the individual innovator. For example, the description of Herman Bruson, the discoverer of Acryloid additives, reminded me of a virtually identical description of the creative individual given 15 years ago by Heinrich Hopff, Professor of Chemical Technology at the ETH in Zurich and one of the research directors at BASF.

However important the individual and the technology, both develop within an economic and business environment. The book does not provide adequate descriptions of that environment, so that some of the actions of its leading actors seem to occur in a

vacuum. For example, the 1950s was an extremely fruitful period for innovation in the chemical industry. Yet, for Rohm and Haas it was not, as can be seen from the relatively few new products introduced during that period and from the company's failure to enter sectors like synthetic fibers or to pursue with vigor its advantage in pesticides. In the absence of any explanation, the reader is left to conclude that the reasons were founder Haas' age and the Plexiglas tradition's strength.

The chapter on Rohm & Haas' attempt to diversify into synthetic fibers in the 1960s provides excellent insight into the attempts of a company to break with tradition. In this particular case there was little chance of success, as the competitors had started much earlier, had already established their positions in the marketplace, and Rohm and Haas did not possess a new product which could revolutionize the status quo.

The chapter on “A new class of concerns” gives an account of the company's experience with the manufacture of chemicals which proved to be carcinogenic. Although the chapter provides a detailed description of the events that took place before and after the discovery of cancer among workers exposed to bis-chloromethyl ether, the author seems to believe that such incidences should not be given widespread publicity. Unfortunately our political and administrative systems are shaken into action by the publicity rather than by the significance of such events, and until this attitude has changed and preventive rather than corrective measures are taken for possible environmental and health hazards, there will be a role for the Ralph Naders and Rachel Carsons to play.

The last two chapters describe the challenges that Rohm and Haas, and indeed most chemical companies, have faced since the mid 1970s because of the adverse effects of mature technologies and of a prolonged world economic recession. Emphasis on profits rather than sales, increased R&D budgets, exit from commodities, emphasis on specialties, rationalization of production, selective geographical retrenchment or expansion, are some aspects of the strategies adopted by chemical companies for their recovery. As can be seen from its performance in the 1980s, Rohm and Haas is one of the companies which found a formula that worked. Reflecting on the history of the chemical industry makes one wonder, however, whether these strategies will prove adequate for the longer term, or whether the solution will come from the chemical companies exercising their ability in introducing radical new technologies which will open a new era of achievement and of profits.

Basil Achilladelis
University of Oklahoma

The 1988 Program Committee has begun its work. Deadline for abstracts for individual papers was 1 February and for session abstracts 29 February. Contact Wouter van Rossum, Sydwende 104, 9204 KG Drachten, Netherlands with your program ideas. Telephone (0) 5120-31767; office (0) 50-636500. Other members of the program committee are listed in the Council Minutes (this issue, 152-3).

Local arrangements for the 1988 Amsterdam meetings are in the hands of Loet Leydesdorff, Wetenschapsdynamica, University of Amsterdam, Nieuwe Achtergracht 166, 1018 WV Amsterdam, Netherlands. Write to Loet to be on the mailing list for registration information, if you have not already sent back the reply slip from the "windmill" leaflet. Loet's telephone is (0) 20-525-6595; E-mail U00223@HASARA5.

The 4S Dance Group, which met for the first time in Cahoots at the Worcester Marriott, has asked the Amsterdam Local Arrangements Committee to locate a suitable spot for another gathering in 1988. If you like to dance, watch for an announcement at the meeting. Long live rock!

The 1989 meetings will be in Irvine, California. The Program Committee is chaired by Adele E. Clarke, 136 Whitney Street, San Francisco, CA 94131 USA. Local arrangements are being made by S. Leigh Star, Department of Information and Computer Science, University of California at Irvine, Irvine, CA 94717 USA. Why not volunteer to help?

Future 4S meetings could be at your institution. To volunteer, contact Ron Johnston, Center for Technology and Social Change, University of Wollongong, P.O. Box 1144, Wollongong, NSW 2500, Australia.

Publications Committee reports that negotiations are beginning with several commercial publishers to handle the Society's journal. The next issue you receive should be the first one with a new publisher!

Science & Technology Studies welcomes a new staff of associate and contributing editors for 1988. (See in-

side front cover.) **Nominations, including self-nominations** are being accepted for these positions for 1989. In particular, the journal is looking for an Associate Editor in the history of science or technology with ties to the European community. Nominations should be sent to Mary Frank Fox, Chair of the 4S Publications Committee, 3042 Survey Research Center, Institute for Social Research, The University of Michigan, Ann Arbor, Michigan 48106-1248 USA.

Symposium proposals for the AAAS 1989 meeting are due at AAAS 15 March 1988. Contact Ned Woodhouse (4S Liaison Chair) for help in preparing and submitting your proposal. Ned is at the Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, New York 12180-3590 USA. **Political scientists and sociologists** will want to know that sections on science and technology have recently been started (by 4S members) in their professional societies. Ned has further details. Members of other societies can draw on these organizing experiences by calling or writing Ned.

The 4S Membership Directory is in process. If you have not filled out your form, request one from P. Thomas Carroll, Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, New York 12180-3590.

Nominations, including self-nominations, for 4S offices are being accepted by Helga Nowotny, European Centre for Social Welfare Research and Training, Bergasse 17, A-1090 Wien, Austria.

The 1988 Bernal Prize will go to a distinguished individual chosen by a 4S committee. The committee is chaired by Ronald Giere, Department of Philosophy, 315 Ford Hall, University of Minnesota, Minneapolis, MN 55455. Contact Ron with nominations.

4S Membership information is available from the Society's new Secretary-Treasurer: Wesley Shrum, Department of Sociology, Louisiana State University, Baton Rouge, LA 70803-5411 USA. Telephone (504) 388-1645; E-mail SOWESL@LSUVM.

Minutes of the 4S Council Meeting (with additional notes from the Business Meeting)

Worcester, November 18, 1987

Present: David Edge (President); Arie Rip (President-Elect);
Thomas F. Gieryn (Secretary-Treasurer)

Council Members: Susan E. Cozzens, Mary Frank Fox,
Ronald Giere

Past Presidents: Dorothy Nelkin

Committee Representatives: Marcel LaFollette, Loet Ley-
desdorff, Sal Restivo, Wouter van Rossum, Ned
Woodhouse

David Edge called the meeting to order at 2:30 P.M.

Acceptance of Minutes

1. Minutes of the 4S Council meeting at Pittsburgh on October 23, 1986 were unanimously approved.

Secretary/Treasurer's Report

2. The Treasurer's Report was approved by the Council, showing a balance of \$10,224.16 October 31, 1987, a decrease from \$11,646.23 on January 1, 1987. The decrease resulted in part from higher costs of publishing *Science & Technology Studies*, as summarized below. The total cost includes: typography, printing, alterations and postage.

Volume 4, Number 1, Spring 1986	34 pages	\$1972.51
Volume 4, Number 2, Summer 1986	48 pages	2844.71
Volume 4, Number 3/4, Fall/Winter 1986	56 pages	3899.96
Volume 5, Number 1, Spring 1987	48 pages	3629.19
Volume 5, Number 2, Summer 1987	52 pages	3549.94

During 1987, the Society took in \$11,062 from Membership Dues, received a refund of \$1362.12 from the 1986 Pittsburgh Meeting, and earned \$357.74 in interest. The Society spend \$11,076.09 on its publications, \$1054.23 for subscription services (Academic Services, Inc.), \$1500 for preparations of the Amsterdam 1988 meeting, and \$537.61 for supplies, equipment, and telephone.

3. Tom Gieryn did not prepare a budget for fiscal year 1988 because of continuing uncertainty surrounding the Society's publications.

4. Tom Gieryn reported a current membership of 342 (272 individuals, 39 institutions, 31 students). Only those who have paid their dues for 1987 are included in these figures. Our membership at the end of 1986 was 332: of those, 321 have renewed their memberships for 1987, and we have added 21 new members during the year.

5. Tom Gieryn announced the results of the 1987 4S Council Elections. The following people will serve on Council through the 1990 annual meetings: Adele Clarke, Roger Krohn, and Steve Woolgar. Council thanked its outgoing members: Ruth Schwartz Cowan, Bruno Latour and Steve Shapin.

6. The Council approved President Edge's appointment of Wesley Shrum as the new Secretary/Treasurer of 4S. The Council thanked Tom Gieryn for his efforts in this office.

Publications Committee

7. Mary Frank Fox (Chairwoman) thanked Susan Cozzens for her fine work in editing *Science & Technology Studies*. The Council recognized that Susan had invested extraordinary amounts of her time and energy in service of the Society's publication.

8. The Council appropriated up to \$7000 for the production of the next double-issue of *Science & Technology Studies* (Volume 5, Number 3/4, Fall/Winter 1987, to appear in February 1988.)

9. The Council deliberated at length the future of the Society's publications. The Society has already committed itself to the commercial publication of an official 4S journal, starting with Volume 6 (1988). At this moment, the preferred option is for the Society to assume editorial control of *Science, Technology and Human Values*, with John Wiley, Inc. continuing as its publisher. The principal advantage for us is that such an arrangement would connect the Society to an established and high quality journal, and it would send the official 4S publication to about 600 institutional and 500 individual subscribers.

However, before 4S can begin negotiations with John Wiley, Inc., it is necessary for both Harvard University and Massachusetts Institute of Technology to agree to transfer ownership of *STHV* to Wiley. The negotiations among Harvard, MIT, and Wiley have been protracted, and as of this writing, no agreement has been reached. Continuing delays would create serious problems for the Society: Susan Cozzens justifiably reported her reluctance to continue being responsible for arranging the typesetting, printing, and mailing for Volume 6 (1988) of *S&TS*. Moreover, the Society's treasury may—after the publication on Volume 5, Number 3/4 of *S&TS*—shrink to as little as \$3000, yet we cannot sent out renewal notices for 1988 until some final arrangement for publishing the journal is secure.

In light of these concerns, the Council set two deadlines: (a) if the 4S has not entered into negotiations with John Wiley, Inc. before December 31, 1987, then the Society will enter into serious negotiations with a number of commercial publishers about publication of *S&TS*; (b) if the 4S has begun negotiations with John Wiley, Inc. before December 31, 1987, the Society will negotiate exclusively with Wiley until February 1, 1988; if no agreement has been reached by that time, the Society will begin negotiations with a number of commercial publishers (which may include continuing negotiations with Wiley). The Council deferred a decision about a possible increase in membership dues until serious negotiations begin with a commercial publisher.

10. Susan Cozzens reported that she had received several inquiries from commercial publishers (among them: Transaction, Beech Tree, Sage) offering attractive arrangements for the commercial publication of *S&TS*.

11. The Council thanked Mary Frank Fox for the report, and also thanked Marcel LaFollette (her predecessor) for continuing efforts in finding the best publications arrangement for 4S.

Program Committee

12. The Council thanked Sal Restivo and his Committee for a high quality and innovative program for the Worcester meetings.

13. The Program Committee for Amsterdam 1988 is: Wouter van Rossum (chair), Aant Elzinga, Tom Gieryn, Ed Hackett, Baudouin Jurdant, Loet Leydesdorff, Helga Nowotny, and Steve Woolgar. Wouter van Rossum and Loet Leydesdorff raised the possibility of a contract with Elsevier for commercial publication of the Proceedings of the Amsterdam meeting. The matter was discussed both at the Council meeting and at the Business meeting, and there was some support for a volume of abstracts similar to that produced for the Ghent meetings.

14. The Council thanked John Wilkes and his Worcester crew for their successful efforts. At the 4S Business Meeting, it was suggested—and widely endorsed—that at future annual meetings, the Local Arrangements Committee should make available at the start of the meeting a list (with addresses) of all participants. A list of participants at Worcester will soon be made available for individual purchase from John Wilkes.

15. David Edge gratefully accepted a gift of \$500 from Worcester Polytechnic Institute to support the inclusion of student research reports in future 4S publications. The 4S expressed an interest in continuing the "student paper competition," although discussion of how best this might be implemented was left for future meetings.

Future Meetings Committee

16. In Ron Johnston's absence, David Edge reported that Leigh Star has offered to host the 1989 annual meetings at the University of California, Irvine. After some discussion, the Council tabled the question of preferred dates for that meeting.

17. The next "multiple-society" meeting (involving perhaps 4S, History of Science Society, and the Philosophy of Science Association but probably not the Society for the History of Technology) is tentatively scheduled for Seattle in Fall 1990.

The Council noted the liabilities in having two successive meetings on the American West Coast.

18. No bids have yet been received for hosting the 1991 meeting. Aant Elzinga has offered Goteborg, Sweden as a site for the next 4S/EASST joint meeting in 1992.

Bernal Award Committee

19. The John Desmond Bernal Prize for 1987 was awarded to Christopher Freeman, University of Sussex, U.K. The Council thanked Warren Hagstrom and his Committee for a fine choice. The Council also thanked Eugene Garfield and the Institute for Scientific Information for their continuing support of this award.

Laison Committee

20. It was reported that a new professional society had recently come into existence: "Science, Technology and Society." The Council decided that this organization seemed to have both aims and audiences that differed from those of 4S; we wish them well.

Nominations Committee

21. The Council thanked Ron Giere and his Committee for arranging a fine slate of candidates for the 4S Council elections. Giere noted that future Nominations Committees must—in creating slates for Council elections—take into account not only the disciplinary, national, and gender diversity in the Society, but it must also find candidates who are willing to invest considerable "sweat equity" in the 4S.

Old Business

22. Ned Woodhouse showed a beautiful mock-up of the 4S Membership Directory, which should be available in Spring 1988. The Council thanked Ned and Tom Carroll for their efforts.

23. At a dinner following the 6:00 P.M. adjournment of the meeting, Council discussed a report by Steve Fernandez and Brian Winter of Worcester Polytechnic Institute on the prospects for 4S "computer conferencing."

24. The Council thanked David Edge for two vigorous and successful years as President of 4S, and the baton was passed to Arie Rip.

Thomas F. Gieryn
Secretary/Treasurer

ATTENTION

Your 4S membership and subscription to *Science & Technology Studies* have expired if the last four digits in the first line of your address label are 8612. See subscription information (inside front cover) to renew.

The Field of Science & Technology Studies: Discussions on Our Present and Future

Dear Colleague:

Science & Technology Studies is initiating an inquiry into the current state and future directions of the field of S&TS, including the subfield of Ethics and Values Studies (EVS). We invite you to participate. We hope to sponsor one or more roundtables on this topic at the Amsterdam meetings, with follow-through sessions in later years as appropriate. In addition, we will begin to publish relevant products of the inquiry in *S&TS* as they become available. Written contributions to the endeavor could include, among others:

1. A *proposed map of the overall domain of S&TS and the subdomain of EVS*, developed with as much rigor as a scholarly article. Do the subjects typically covered at a 4S (or SHOT or HSS) meeting actually cover the full range of concerns that might usefully be addressed regarding science and technology as social phenomena? How would anyone know? Can we choose problems intelligently without an overall picture of the potential alternatives?

2. A *critique of a sizeable chunk of the overall domain*, such as a subdiscipline (e.g., 20th century history of technology), problem area (science funding), or conceptual approach. The critique should review the literature and place it within the larger S&TS field, note major shortcomings and omissions, and point out opportunities.

3. *Research agenda defenses*. Why is a particular subject or approach more worthwhile than a multitude of others? In problem choice, we detect a widespread casualness that professional social scientists would never accept in executing their research projects. We challenge you to mount a serious intellectual defense of any set of research problems. This task is especially important since our domain is large and the available resources small.

Our goal for this ongoing discussion is to clarify the scope, structure, and central problems of the field, by defining the relations among its subareas and its relationships with the disciplines it draws on. In doing so, we will help shape editorial policy for *Science & Technology Studies* and directions for the Society for Social Studies of Science. Please contact us with your contributions, for Amsterdam and beyond.

Susan, Rachelle, and Ned

Treasurer's Annual Report: Status of the 4S Treasury, December 31, 1987

OLD BALANCE	December 31, 1986	\$11646.23
INCOME	Membership Dues	11101.00
	Interest	433.11
	Refund from 1986 Pittsburgh Meeting	1326.12
	Refund from 1987 Worcester Meeting	500.00
	TOTAL INCOME	\$13360.23
EXPENSES	Publications (<i>Science & Technology Studies</i>)	11076.09
	Academic Services, Inc. (Subscription Mgt.)	1322.54
	Telephone	10.98
	Postage	443.61
	Photoreproduction	123.40
	Supplies and Equipment	45.21
	Start-up Expenses—Amsterdam 1988 Meeting	1500.00
	Presidential Expenses	202.05
	1987 Bernal Plaque	47.10
	Calligraphy	41.00
	TOTAL EXPENSES	\$14811.98
NEW BALANCE	December 31, 1987	\$10194.48

Networks

Sociologists working on issues of minority participation in science met at the meetings last summer of the American Sociological Association. Addresses and research interests were exchanged. For a copy of the list, and to add your name to it, contact Willie Pearson, Jr., Department of Sociology, Box 7808 Reynolda Station, Winston-Salem, North Carolina 27109; (919) 761-5495.

Discount

Late-breaking news prevents us from using this space to announce a group discount subscription price for 4S members to *Science, Technology, & Human Values*. Instead, we advise 4S members not to renew their ST & HV subscriptions until a new editor is announced, indicating that the future of the journal is settled. When that happens, 4S members may subscribe for \$30 rather than \$37 for 1988. Contact Periodicals Division, John Wiley & Sons, Inc., P.O. Box 836, Bound Brook, New Jersey 08805, and identify yourself as a 4S member to receive the discount rate.

Awards

The History of Science Society has established a new prize for a book on the history of women in science. The 1987 award went to Dr. Regina Markell Morantz-Sanchez (University of Kansas) for *Sympathy and Science: Women Physicians in American Medicine* (Oxford University Press, 1985). The new prize was established by the long-active HSS committee on women in history of science. For more information on the award, contact Dr. Michael Sokal, Executive Secretary, HSS, 100 Institute Road, Worcester, MA 01609 USA; telephone (617) 793-5363 or 793-5385.

The annual AAAS Scientific Freedom and Responsibility Award was established to honor scientists and engineers whose actions, often taken at significant personal costs, have served to foster scientific freedom and responsibility. For information, contact Linda Valentine at the AAAS Office of Scientific Freedom and Responsibility, 1333 H Street NW, Washington, DC 20005 USA; (202) 326-6791.

Positions

The Center for the History of Physics at the American Institute of Physics seeks applicants for the position of Post-doctoral Associate Historian. The person

should hold a recent Ph.D. or a virtually complete dissertation in some area of the history of physical science. Appointment will be for one year, renewable up to three years. The historian will help conduct and administer the Center's programs in oral history interviewing of physical scientists, the preservation and cataloging of their documents, and the use of such historical materials for scholarly and educational purposes, with ample opportunity to pursue personal research interests in related areas. The AIP offers excellent salary, benefits, and support services. Employment is expected to begin in the latter half of 1988. Anyone qualifying for the position is urged to send a letter, *vita*, and names of three references to Spencer Weart, American Institute of Physics, 335 East 45th Street, New York, New York 10017 USA.

Rensselaer Polytechnic Institute's Department of Science and Technology Studies sometimes has temporary openings at the lecturer or instructor level to teach courses in history, political science, sociology, or anthropology as well as science and technology studies. These appointments have been full time for a year or a semester. Interested persons should send a *curriculum vitae*, with names of two references familiar with their teaching, and a list of courses they are able to teach to: Shirley Gorenstein, Chair, Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, NY 12180-3590. RPI is an equal opportunity/affirmative action employer.

Fellowships and Grants

NSF's *Ethics and Values Studies* (EVS) and *History and Philosophy of Science* (HPS) programs are being linked in a new program called *Studies in Science, Technology, and Society* (SSTS). The two existing panels will continue to review proposals in their areas, and proposals which fall outside the scope of EVS and HPS but within SSTS will be appropriately reviewed with ad hoc procedures. SSTS is located in a new Division called "Instrumentation and Resources" within the Directorate for Biological, Behavioral, and Social Sciences.

NSF's *Sociology Program*, however, wishes to stress that proposals in the Sociology of Science and Technology are still being reviewed and funded there, just as proposals to study science or technology from the viewpoint of other social science disciplines (e.g., political science, economics, geography) are being reviewed through the programs for these disciplines in the Division of Social and Economic Sciences (SES).

The closing date for submitting preliminary proposals to Ethics and Values Studies (EVS) is 1 May 1988. EVS supports research and related activities examining ethical or value issues of current significance to U.S. science and engineering. Results are to be accessible to persons interested in professional ethics, impact of technology, and uses of science in decisionmaking. Contact Rachelle Hollander, EVS, National Science Foundation, Washington, DC 20550 USA.

Information about support available from the National Endowment for the Humanities is given in the *Overview of Endowment Programs*. The *Overview* is published twice yearly, in January and July. Free copies may be obtained by writing or calling *NEH Overview*, Room 409, 1100 Pennsylvania Avenue NW, Washington, DC 20506 USA; telephone (202) 786-0438. If you mention that you saw the overview announced in *Science & Technology Studies*, NEH will also send a complimentary copy of *Humanities*, NEH's bi-monthly magazine.

The History of Science Society's Committee on Independent Scholars seeks to aid scholars trained in the history of science who are unemployed, unaffiliated with any institutions making use of their training as historians of science, or employed either part-time or without prospects of continuation or renewal. The History of Science Society's Committee on Independent Scholars seeks to aid scholars trained in the history of science who are unemployed, unaffiliated with any institutions making use of their training as historians of science, or employed either part-time or without prospects of continuation or renewal. The committee funds an Associate Scholars Grants Program, providing grants-in-aid of up to \$2,000 for research travel or other research expenses. Persons interested in applying for such grants should send eight copies of a proposal consisting of a curriculum vitae a proposed budget, a brief (three pages or less) statement of the proposed research project, and one letter of recommendation (dealing with the intellectual merits of the research) to Michael Sokal, Department of Humanities, Worcester Polytechnic Institute, Worcester, MA 01609 USA. Proposal submitted by 15 April 1988 will be considered for support starting 1 August 1988, although the committee will try to respond expeditiously to emergency requests.

The Center for History of Physics of the American Institute of Physics has a program of grants-in-aid for research in the history of modern physics and allied sciences (such as astronomy, geophysics, and optics) and their social interactions. Grants can be up to \$2000 each—double the amount formerly offered. They can be used only to reimburse direct expenses connected

with the work. Preference will be given to those who need part of the funds for travel and subsistence to use the resources of the Center's Niels Bohr Library in New York City or to microfilm papers or to tape-record oral history interviews with a copy deposited in the Library. Applicants should either by working toward a graduate degree in the history of science, or show a record of publications in the field. To apply, send a *vitae* plus a letter of no more than two pages describing your research project and including a brief budget showing the expenses for which support is requested. Send to Spencer Weart, Center for History of Physics, American Institute of Physics, 335 East 45th Street, New York, NY 10017 USA. Deadlines for receipt of applications are June 30 and December 31 of each year.

The American College of Obstetricians and Gynecologists-Ortho Fellowship in the History of American Obstetrics and Gynecology carries a stipend of \$5,000 to be used to defray expenses while spending a month in the ACOG historical collection and other medical and historical collections in the Washington DC area, in order to continue research into some area of American obstetric and gynecological history. Applications for the 1989 award will be accepted until 1 September 1988. For more information contact Gay Takakoshi, Librarian, Historical Collection, American College of Obstetricians and Gynecologists, 600 Maryland Avenue SW, Washington, DC 20024 USA; telephone (202) 638-5577.

The next deadline for research grants from the American Philosophical Society is 1 April 1988 for a written decision by 30 June. These grants can cover necessary foreign and domestic travel at the lowest available charter or economy rate, living costs while away from home up to a maximum for \$30 per day, and microfilms, photostats, photographs, and the like. The maximum grant that will be made is \$3,500, and this amount will be approved only in exceptional cases. The maximum grant for a full professor is \$2,500. These grants may supplement other research grants that do not cover travel. For application forms and further information, send a brief description of your research project and proposed budget in a letter to the Committee on Research, American Philosophical Society, 104 South Fifth Street, Philadelphia, PA 19106 USA.

The National Aeronautics and Space Administration History Office announces an interest in funding the research and writing of publishable journal-length essays or small monographs on NASA-related aerospace history. Subjects need not be limited to aerospace science and technology but may include topics in aeronautical and space policy, law, and R&D man-

agement. Bibliographical and historiographical studies will also be considered. Up to \$9,500 may be awarded, depending on the nature and scope of the project proposed; awards will be contingent upon the availability of funds. Proposals will be considered three times annually and should be submitted by 15 June, 15 October, and 15 February. Interested parties should discuss possible proposals with the director. Contact Sylvia D. Fries, Director, NASA History Office, National Aeronautics and Space Administration, Washington, DC 20546 USA; telephone (202) 453-8300.

Publications

Technological Innovation and Human Resources is a new series of books published by Walter de Gruyter (Berlin and New York). It brings together research, critical analysis, and proposals for change concerning technological innovations and how they affect people in the workplace. *Technology* here includes but is not limited to computers, information systems, telecommunications, computer-aided design and manufacturing, artificial intelligence, and other related forms. Inquiries regarding contributions should be sent to Urs E. Gattiker, School of Management, The University of Lethbridge, Lethbridge, Alberta, T1K 3M4, Canada; or Laurie Larwood, School of Business, State University of New York at Albany, Albany, New York 12222 USA.

Science in Context, a new semi-annual journal from Cambridge University Press, is "devoted to the study of the sciences from the point of view of comparative epistemology and historical sociology of scientific knowledge. It will not segregate history, philosophy and sociology of knowledge from each other. . . . This integrative approach will demand comprehensive (and often longer) articles, many of them commissioned. Several alternative approaches to the same problem will often be presented together." Editors are Robert S. Cohen, Yehuda Elkana, Gideon Freudenthal, and Simon Schaffer. For subscription information contact Cambridge University Press, The Edinburgh Building, Shaftesbury Road, Cambridge CB2 2RU, UK; or in the USA or Canada, Cambridge University Press, 32 East 57th Street, New York, NY 10022 USA.

Social Epistemology, a new quarterly journal from Taylor & Francis Ltd., will publish critical syntheses, interviews, book reviews, and "provocations," in order to encourage the recognition and negotiation of boundaries between disciplines "for purposes of developing a more integrated view of knowledge production." Editors are Steve Fuller and Stephen Downes. Subscription information is available from

Taylor & Francis Ltd., 4 John Street, London WC1N 2ET UK.

Knowledge in Society, a new quarterly publication from Transaction Periodicals, is devoted to the development of an interdisciplinary science of knowledge in society. Contributors investigate practical problems of our information-intensive society as well as new theories, methods, and strategies for understanding and improving the critical communication role of scientists and professionals in contemporary society. Editor-in-Chief is William N. Dunn. For subscription information contact Transaction Periodicals, Rutgers—The State University, New Brunswick, NJ 08903 USA; (201) 932-2280.

On TOP (On Those Opposed to Pseudoscience), a newsletter, distributed its first issue for Fall/Winter 1987. The newsletter intends to acquaint academics, administrators, policy makers, and other professional dealing with issues relating to pseudosciences with each other and to describe the nature of their respective interests. For further information, write to the Department of Sociology, Anthropology, and Social Work, Box 19599, 205 University Hall, The University of Texas at Arlington, Arlington, Texas 76019 USA.

The *Professional Ethics Report*, the newsletter of the American Association for the Advancement of Science Committee on Scientific Freedom & Responsibility Professional Society Ethics Group, has issued its first number. The newsletter is devoted entirely to publishing timely information on professional ethics issues and activities which affect a wide range of professions. Its "field of vision" will focus primarily on those professions whose members are engaged in scientific research and its applications, but it will from time to time include items on other professional fields. The newsletter will report on news and events, describe programs and activities, review various resources, and in other ways foster interprofessional dialog on professional ethics. Interested colleagues are encouraged to write to the Committee if they would like to be added to the mailing list. Contact Mark S. Frankel, AAAS, 1333 H Street NW, Washington, DC 20005 USA.

An *Annotated Bibliography on Values and Ethics in Organization and Human Systems Development* is now available. Containing 178 items, it is intended to serve as an educational and reference tool for OD-HSD teachers, students, researchers, and practitioners and clients as well as persons in related behavioral sciences and in the applied and professional ethics field. Both descriptive and normative material are included. Copies may be obtained from the American Association for the Advancement of Science, Marketing De-

partment, 1333 H Street NW, Washington, DC 20005 USA. Cost is \$4 for AAAS members and \$5 for non-members, postage pre-paid.

The publication, *Resources for the History of Computing: A Guide to U.S. and Canadian Records*, has been printed and is now available for \$9 from the Charles Babbage Institute, 103 Walter Library, 117 Pleasant Street SE, University of Minnesota, Minneapolis, MN 55455 USA.

The National Governors' Association and the Conference Board have jointly prepared a national report which discusses the attitudes of state, industry, and university officials toward economic competitiveness. The report is entitled *The Role of Science and Technology in Economic Competitiveness*. The project was conducted with the support and participation of the National Science Foundation. An executive summary can be obtained free from: National Science Foundation, Forms and Publications Unit, Room 232, 1800 G Street NW, Washington DC 20550 USA.

Lecture Series

The Forum for Independent Research in Science and Technology Studies (FIRSTS), an interdisciplinary lecture series and related programs, invites interested scholars from the U.S. and Canada to give lectures on their current work. The Forum meets in the Boston area. Travel expense and honoraria are available to those who are currently in temporary positions. Interested parties should send a title, abstract, and two preferred dates, as soon as possible, to the coordinators, P. G. Abir-Am and K. E. Duffin, Department of the History of Science, Harvard University, 235 Science Center, Cambridge, MA 02138 USA; telephone (617) 459-0582 and 661-4689.

Calls for Papers

The *Eighth Annual Conference of the Association for the Advancement of Policy, Research, and Development in the Third World* will be held 20-25 November 1988 in Kingston, Jamaica. Paper abstracts, panel proposals, and roundtable suggestions are solicited. Submit a one-page proposal and a biographical professional statement indicating areas of professional and geographical competency to Mekki Mtewa, Executive Director, Association for the Advancement of Policy, Research, and Development in the Third World, P.O. Box 70257, Washington, DC 20024-1534 USA. (Deadline: 26 February 1988)

Prior to the joint 4S/EASST meetings in Amsterdam, the Science Studies Unit of the University of Leiden will sponsor a workshop dedicated to the develop-

ment and applications of science and technology indicators, under the title "Science Indicators: Their Use in Science Policy and Their Role in Science Studies." The dates are Monday 14 and Tuesday 15 November 1988. Those interested in participating are urged to contact Anthony F. J. van Raan, Science Studies Unit/LISBON Institute, University of Leiden, Stationsplein 242, 2312 AR Leiden, The Netherlands; telephone: 31 71 273909; telex: 39427 burul nl. A second announcement, in March, will give a preliminary program and deadline for further contributions.

The History of Science Society program chairs are actively looking for ideas for sessions for their 1988 meeting. Proposals for regular sessions and for work-in-progress papers must be received by 15 March 1988. Please send copies to: Joan L. Richards, Box N, Department of History, Brown University, Providence RI 02912 USA; or Shirley A. Roe, Department of History, U-103, University of Connecticut, Storrs, CT 06268 USA.

The Society for History of Technology is calling for papers and session proposals for its annual meeting, to be held at the Hagley Museum and Library in Wilmington, Delaware, 20-23 October 1988. The committee seeks proposals in all areas of the history of technology; especially welcome are submissions from minority scholars and from scholars in neighboring disciplines. The deadline for receipt of proposals by the program chair is 1 April 1988. Proposal must include a 150-word abstract and a one-page curriculum vitae. Please send four copies of each proposal to Larry Owens, Department of History, University of Massachusetts, Amherst, MA 01003 USA; telephone (413) 545-2223 or 549-4773.

The Technology and Society Division of the American Society of Mechanical Engineering has requested papers for presentation at the winter annual meeting in Chicago, 28 November-2 December 1988. Papers on the following topics are especially solicited: technology assessment, energy and environmental assessments, assessments of alternative energy sources, models of engineering ethics, appropriate technology for developed and developing nations, sociotechnical programs and ethics courses in engineering education, emerging technologies, legislative and legal problems relating to technology, and specific aspects of the interactions between technology and society. Two copies of completed manuscripts should be sent by 30 April 1988 to A. M. Dhanak, Department of Mechanical Engineering, Michigan State University, East Lansing, MI 48824 USA.

Papers are needed for a conference on *Explorations in Feminist Ethics: Theory and Practice*, to be held 7-8 October 1988 at the Radisson Hotel, Duluth, Minnesota,

USA. Send two copies of 10- to 12-page papers, including an abstract of 150 words maximum, by 30 April 1988 to Eve Browning Cole, Department of Philosophy and Humanities, 369 A. B. Anderson Hall, or to Susan Coultrap-McQuinn, Institute for Women's Studies, 209 Bohannon Hall, both at the University of Minnesota, Duluth, 10 University Drive, Duluth, MN 55812 USA.

The IUHPS Committee on History of Women in Science, Technology, and Medicine is planning to hold several special sessions at the XVIIIth International Conference on the History of Science. (See Meeting Calendar for general information on the Conference.) For further information on the special sessions, contact Margaret Rossiter, History Department, 451 McGraw Hall, Cornell University, Ithaca, NY 14853 USA.

On the occasion of the two hundredth anniversary of the French Revolution, the Society for Philosophy and Technology invites papers on the theme "Technology and Democracy," for a meeting to be held in June 1989 in Bordeaux, France. Submissions are due 15 July 1988. For information on format and other requirements, contact Langdon Winner, Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, NY 12180-3590 USA.

Meetings Past

The Center for Law and Technology at Boston University Law School held its inaugural symposium in March 1987 on *Major Industrial Hazards: Liability, Insurance, and Risk Management*. A second symposium was held in April 1987 on *Expert Systems: Liability and Regulation*. The Center is actively engaged in activities focused on risk analysis, communication, and management. For further information contact Professor Michael Baram, Director, Center for Law and Technology, Room 866, Boston University Law School, 765 Commonwealth Avenue, Boston, MA 02215 USA; (617) 353-5294.

In September 1987, the AAAS-American Bar Association National Conference of Lawyers and Scientists hosted a workshop intended to examine the nature and scope of *scientific fraud and misconduct* and the issues that have surfaced in dealing with such problems. Close to forty participants discussed what constitutes scientific misconduct, how extensive it is, what standards should prevail, how to protect the rights of involved parties, factors which precipitate misconduct and how it might be prevented, and current policies adopted by the National Institutes of Health and the National Science Foundation. A report on the workshop will be published by AAAS in early

1988. For more details, contact Al Teich, Head of the Office of Public Sector Programs, AAAS, 1333 H Street NW, Washington, DC 20005 USA.

A Symposium on *Science and Technology in the Public Interest: The National Bureau of Standard in the Post-War Era, 1945-1985* was held at the National Bureau of Standards in Gaithersburg, Maryland 17-18 September 1987. For information on the conference proceedings, contact Professor Stuart W. Leslie, Department of the History of Science, Johns Hopkins University, Baltimore, MD 21218 USA; (301) 338-7501.

An *International Conference on High Technology in the Less Developed Countries* was held in the Asian Institute of Technology, Bangkok, Thailand, 21-24 January 1988. Information is available from Professor Manas Chatterji, School of Management, SUNY-Binghamton, Binghamton, NY 13901 USA; (607) 777-4886.

The Third National Science, Technology, Society Conference on *Technological Literacy* was held in early February, jointly with the *inaugural meeting of the Society for STS*. The Society is formed "in response to the needs of professional from a wide spectrum of disciplines and livelihoods, who share a concern for reflection, education, or action in any of the myriad areas where technology and science affect our society." For further information, write to: STS Program, The Pennsylvania State University, 128 Willard Building, University Park, PA 16802 USA; telephone (814) 865-9951.

The AAAS 1988 Annual Meeting in Boston was the site of several events related to *professional ethics matters*, including a two-session symposium on science, engineering, and ethics and a workshop organized by the AAAS Office of Scientific Freedom and Responsibility. Both assessed recent developments in research and education and produced an agenda of research priorities and mechanisms for implementing research in science, engineering, and ethics. For further information contact Amy Crumpton at AAAS, 1333 H Street NW, Washington, DC 20005.

Meetings Future

The American Philosophical Society Library will sponsor a symposium titled "*The Grand Enterprise: Modern Physical Science in America, 1920-1960*," to be held 18-19 March 1988. The symposium honors John Slater Clarke (1900-1976), a leader in quantum mechanics and solid state physics. Those interested in attending should contact Dr. Edward C. Carter II, Librarian, American Philosophical Society, 105 South Fifth Street, Philadelphia, PA 19106-3386 USA; (215) 627-0706.

A conference to examine *the ways that cities have developed and attracted new technology industries* is planned for 23-25 March 1988 in London. Questions the conference will address include: How can technology industries reverse the decline of cities? What is the nature of the partnership with the private sector? What roles can the public and educational sectors best play? For further information, contact Patty Dempsey, Technology Innovation Program, University of New Mexico, 1920 Lomas NE, Albuquerque, NM 87131 USA; telephone (505) 277-5934.

Boston University's Center for Law and Technology is sponsoring a *Symposium on Legal Obligations of Transnational Corporations to Disclose and Communicate Risk Information*, 24-25 March 1988. Theme is new policies and requirements in the U.S. and European Community for risk communication between industrial firms, agencies, and persons at risk. Speakers include government and industrial officials, attorneys, social scientists, and risk analysts. Contact Professor Michael Baram, Director, Center for Law and Technology, Boston University Law School, 765 Commonwealth Avenue, Boston, MA 02215 USA; (617) 353-5294.

The Seventh Annual Luncheon of the *Planning History Group* will be held on Saturday 26 March 1988 at Noon in Bally's Hotel, Reno, Nevada. The luncheon is being held in conjunction with the meeting of the Organization of American Historians. Carl Abbott, Portland State University, will present a paper entitled, "New York of the South or Paris of America? Economic Strategies in Washington, D.C. since 1890." For additional information contact: Blaine A. Brownell, College of Social and Behavioral Sciences, University of Alabama at Birmingham, Birmingham, AL 35294 USA; telephone (205) 934-5643.

The *relationship between gender and science and technology* will be the focus of a major conference to be held 9 April 1988 at the University of California, Davis. Conference panels will address such issues as the barriers and challenges women face entering scientific and technical fields; the impact of technology on high-tech industries; the role of technology in women's health; and the reflection of science and technology in literature by women, in the news media, and in computer technology and video games. Some panels will discuss women as a force for change in the peace movement and in Third World countries. There will be a plenary session of talks devoted to reproductive technologies, including an assessment of the legal implications of surrogacy. Advance registration (\$20 for faculty and employed, \$10 for students or unemployed) must be made by 28 March through Diane Lardelli, UCD Campus Events and Information, Uni-

versity of California, Davis, CA 95616 USA; (916) 752-1920. For other information, contact Kathryn Johnson or Patricia Bailey at the UCSD Women's Resources and Research Center, (916) 752-9918 or 9919.

"The Chomskyan Turn: Generative Linguistics, Philosophy, Mathematics, and Psychology," an international symposium, organized by the Institute for the History and Philosophy of Science and Ideas, Tel-Aviv University and the Van Leer Jerusalem Institute, will be held 11-14 April 1988. *"50 Years of the 'Merton Thesis',"* an international workshop organized by the same, will be held 16-19 May. Both will take place in Tel-Aviv and Jerusalem at the locations of the sponsors, as part of the Bar-Hillel Colloquium for the History, Philosophy, and Sociology of Science. For information, contact Edna Ullmann-Margalit, Colloquium Coordinator, P.O.B. 4070, Jerusalem, Israel.

The *XIIth British Congress in the History of Medicine* will be held at the University of Bath on 15-17 April 1988. There will be sessions on the themes of provincial practitioners, disease in rural communities, specialist medicine, accidents and emergencies, the sharing of knowledge, and self-help. Enquiries to Dr. J. R. Guy, Postgraduate Medical Centre, Yeovil District Hospital, Yeovil, Somerset, England.

The History of Genetics, Eugenics, and Evolution: A Commemorative Conference in Honour of Bernard Norton (1945-1984) will take place on 7 May 1988 at the University of Leicester. The program includes talks by Nils Roll-Hansen, David Barker, Robert Olby, Peter Bowler, Jonathan Hodge, and John R. G. Turner. Further details may be obtained from Wing Commander G. Bennett, 31 High Street, Stanford-in-the-Vale, Faringdon, Oxon SN7 8LH England.

The New York Academy of Sciences and the Philosophy and Technology Studies Center of the Polytechnic University in Brooklyn are sponsoring a conference on *Ethical Issues in Military Research* 18-20 May 1988. The purpose of the conference is to bring scientists and engineers representing both sides of the political-moral spectrum in the SDI and related debates together with philosophers and others who can help place such debates within a larger context. To be considered are: conceptual distinctions between pure and mission-oriented research; the historical and sociological background of current debates; the kinds of ethical arguments employed in the articulations of different positions; and the moral options open to persons on various sides of the issues. The conference will be held at the New York Academy of Sciences, 2 East 63rd Street, New York, New York 10021 USA; telephone (212) 838-0230.

A course in "Sociology of Science--Moral and Political Issues of Science in Society" will be held 9-20 May 1988 at the Inter-University Centre of Postgraduate Studies, Dubrovnik, Yugoslavia. The course will explore different conceptions of the social responsibility of scientists within a broader framework of moral and political issues facing contemporary society. Inquiries should be directed to the Secretariat of the IUC at Frana Bulica 4, YU-50 000 Dubrovnik, Yugoslavia; telephone (050) 28-666. Telegraphic address: INTER-UNIVERSITY

The *Pasteur Institute* is organizing an international colloquium on its history to be held in Paris 7-10 June 1988. For further information contact Michel Morange, IBM, Batiment Jacques Monod, Institut Pasteur, 25 rue de Dr. Rous, 75015 Paris, France.

The twentieth annual meeting of *Cheiron: The International Society for the History of Behavioral and Social Sciences* will be held at Princeton University on 15-19 June 1988. For information contact the program chair, Raymond E. Fancher, Department of Psychology, York University, North York, Ontario M3J 1P3, Canada. The *Second Latin American Congress on the History of Science and Technology* will be held 30 June to 4 July 1988 in Sao Paulo. For information contact the Organizing Committee, 2.o CLA/HCT, Caixa Postal 6063, 13.081 Campinas-SP, Brasil.

A conference entitled *Promoting Ethics, Values, and Interdisciplinarity in Higher Education*, sponsored by the Alaska Pacific University, Anchorage, will be held at Pembroke College, Cambridge, England, 8-12 August 1988. The conference will consist of small work group discussion, the sharing of papers and materials, and the presentation and publication of group papers. Participation is strictly limited to 75. Contact Shirley Paolini, Professor of Humanities, Alaska Pacific University, 4101 University Drive, Anchorage, AK 99508 USA; (907) 561-1266.

The Institute for Advanced Studies in the Humanities, University of Edinburgh, will hold a conference on *Technology, Communications, and the Humanities*, 18-21 August 1988. The main themes of the conference will be technology and decisionmaking, technology and the dissemination of information, technology and the acquisition of knowledge, technology and creative

design, and technology and daily life. Inquiries for more information should go to: Director, Institute for Advanced Studies in the Humanities, University of Edinburgh, Hope Park Square, Edinburgh EH8 9NW, Scotland, UK.

An international summer school in history of science has been set up to meet annually for two weeks in Bologna, Uppsala, and Berkeley in rotation. The school's purpose is to bring together specialists and advanced aspirants to consider topics in history of science and technology deemed interesting, timely, and appropriate to the location. The first school, devoted to *eighteenth century science*, will be held in Bologna 19 August to 9 September 1988. For application forms and further information write to one of the organizers: Tore Frangsmyr, Office for History of Science, Uppsala University, Box 256, S 751 05, Uppsala, Sweden; J. L. Heilbron, Office for History of Science and Technology, University of California, 470 Stephens Hall, Berkeley, CA 94720 USA; Giuliano Pancaldi, Universita degli Studi di Bologna, Dipartimento di Filosofia, Via Zamboni, 38, 40126, Bologna, Italy.

What do we really know about editorial peer review in science? Not much, claims the American Medical Association, in planning the *First International Congress on Peer Review in Biomedical Publication*. Scholars in non-biomedical disciplines, such as social scientists and historians of science are urged to participate so that peer review can be examined in the context of the overall scientific enterprise. The Congress will be held in Chicago 10-12 May 1989. Research protocols should be developed now; future mailings and journal announcements will call for abstracts. To be on the mailing list, contact Ms. Sharon Iverson, *Journal of the American Medical Association*, 535 North Dearborn Street, Chicago, Illinois 60610 USA; telephone (312) 280-7123.

Erratum

On the Table of Contents of Volume 5, Number One, the source of the cover illustration was incorrectly identified as the American Philosophical Association instead of the American Philosophical Society. Our apologies.

18-19 March 1988. Philadelphia, Pennsylvania. The Grand Enterprise: Modern Physical Science in America, 1920-1960. Contact Dr. Edward C. Carter II, Librarian, American Philosophical Society, 105 South Fifth Street, Philadelphia, PA 19106-3386 USA; (215) 627-0706.

23-25 March 1988. London, England. Conference to examine the ways that cities have developed and attracted new technology industries. Contact Patty Dempsey, Technology Innovation Program, University of New Mexico, 1920 Lomas NE, Albuquerque, NM 87131 USA; telephone (505) 277-5934.

24-25 March 1988. Boston, Massachusetts. Symposium on Legal Obligations of Transnational Corporations to Disclose and Communicate Risk Information. Contact Professor Michael Baram, Director, Center for Law and Technology, Boston University Law School, 765 Commonwealth Avenue, Boston, MA 02215 USA; (617) 353-5294.

9 April 1988. Davis, California. Conference on the relationship between gender and science and technology. To register, contact Diane Lardelli, UCD Campus Events and Information, University of California, Davis, CA 95616 USA; (916) 752-1920. For other information, contact Kathryn Johnson or Patricia Bailey at the UCSD Women's Resources and Research Center, (916) 752-9918 or 9919.

11-14 April 1988. Jerusalem and Tel-Aviv, Israel. The Chomskyan Turn: Generative Linguistics, Philosophy, Mathematics, and Psychology. For information, contact Edna Ullmann-Margalit, Colloquium Coordinator, P.O.B. 4070, Jerusalem, Israel.

15-17 April 1988. Bath, England. XIIth British Congress in the History of Medicine. Enquiries to Dr. J. R. Guy, Postgraduate Medical Centre, Yeovil District Hospital, Yeovil, Somerset, England.

15-17 April 1988. San Jose, California. Celebration of the 350th anniversary of Descartes' *Discourse on Method*. Some travel funds available for graduate students; contact Michael Sokal, History of Science Society Executive Secretary, Department of Humanities Worcester Polytechnic Institute, Worcester, MA 01609 USA.

7 May 1988. Leicester, England. The History of Genetics, Eugenics, and Evolution: A Commemorative Conference in Honour of Bernard Norton (1945-1984). Contact: Wing Commander G. Bennett, 31 High

Street, Stanford-in-the-Vale, Faringdon, Oxon SN7 8LH England.

16-19 May 1988. Jerusalem and Tel-Aviv. 50 Years of the "Merton Thesis." For information, contact Edna Ullmann-Margalit, Colloquium Coordinator, P.O.B. 4070, Jerusalem, Israel.

18-20 May 1988. New York, New York. Ethical Issues in Military Research. Contact: New York Academy of Sciences, 2 East 63rd Street, New York, New York 10021 USA; telephone (212) 838-0230.

27-29 May 1988. Melbourne, Australia. Annual Conference of the Australian Association for the History, Philosophy, and Social Studies of Science. Contact: Richard Gillespie, History & Philosophy of Science Department, University of Melbourne, Parkville, 3052, Australia.

7-10 June 1988. Paris, France. International colloquium on the history of the Pasteur Institute. Contact Michel Morange, IBM, Batiment Jacques Monod, Institut Pasteur, 25 rue de Dr. Rous, 75015 Paris, France.

13-17 June 1988. Poughkeepsie, New York. Symposium on the history of modern mathematics. Some travel funds available for graduate students; contact Michael Sokal, History of Science Society Executive Secretary, Department of Humanities Worcester Polytechnic Institute, Worcester, MA 01609 USA.

15-19 June 1988. Princeton, New Jersey. Twentieth Annual Meeting of Cheiron: The International Society for the History of Behavioral and Social Sciences. Contact: Raymond E. Fancher, Department of Psychology, York University, North York, Ontario M3J 1P3, Canada.

30 June - 4 July 1988. Sao Paulo, Brazil. Second Latin American Congress on the History of Science and Technology. Contact: Organizing Committee, 2.0 CLA/HCT, Caixa Postal 6063, 13.081 Campinas-SP, Brasil.

8-12 August 1988. Cambridge, England. Promoting Ethics, Values, and Interdisciplinarity in Higher Education. Contact: Shirley Paolini, Professor of Humanities, Alaska Pacific University, 4101 University Drive, Anchorage, AK 99508 USA; (907) 561-1266.

18-21 August 1988. Edinburgh, Scotland. Conference on Technology, Communications, and the Humanities. Contact: Director, Institute for Advanced Studies

in the Humanities, University of Edinburgh, Hope Park Square, Edinburgh EH8 9NW, Scotland, UK.

7-8 October 1988. Duluth, Minnesota. Explorations in Feminist Ethics: Theory and Practice. Contact: Eve Browning Cole, Department of Philosophy and Humanities, 369 A. B. Anderson Hall, or Susan Coultrap-McQuinn, Institute for Women's Studies, 209 Bohannon Hall, both at the University of Minnesota, Duluth, 10 University Drive, Duluth, MN 55812 USA.

20-23 October 1988. Wilmington, Delaware. Society for History of Technology Annual Meeting. Contact: Larry Owens, Department of History, University of Massachusetts, Amherst, MA 01003 USA; telephone (413) 545-2223 or 549-4773.

14-15 November 1988. Leiden, Netherlands. Science Indicators: Their Use in Science Policy and Their Role in Science Studies. Contact Anthony F. J. van Raan, Science Studies Unit/LISBON Institute, University of Leiden, Stationsplein 242, 2312 AR Leiden, The Netherlands; telephone: 31 71 273909; telex: 39427 burul nl.

16-19 November 1988. Amsterdam, Netherlands. 4S/EASST Joint Meetings. Contact: Loet Leydesdorff, Wetenschapsdynamica, University of Amsterdam, Nieuwe Achtergracht 166, 1018 WV Amsterdam, The Netherlands; telephone (020) 525-6595; E-mail U00223@HASARA5.

20-25 November 1988. Kingston, Jamaica. Eighth Annual Conference of the Association for the Advancement of Policy, Research, and Development in the Third World. Contact: Mekki Mtewa, Executive Director, Association for the Advancement of Policy, Research, and Development in the Third World, P.O. Box 70257, Washington, DC 20024-1534 USA.

28 November-2 December 1988. Chicago, Illinois. Technology and Society Division of the American Society of Mechanical Engineering Winter Annual Meeting. Contact: A. M. Dhanak, Department of Mechanical Engineering, Michigan State University, East Lansing, MI 48824 USA.

1-12 May 1989. First International Congress on Peer Review in Biomedical Publication. Contact: Ms. Sharon Iverson, *Journal of the American Medical Association*, 535 North Dearborn Street, Chicago, Illinois 60610 USA; telephone (312) 280-7123.

11-15 July 1988. Manchester, England. Joint Meeting of the British Society for the History of Science and the History of Science Society (USA). Contact: Ronald L. Numbers, Department of the History of Medicine, University of Wisconsin, 1300 University Avenue, Madison, WI 53706 or John Pickstone, Department of Science & Technology Policy, The University, Manchester, M13 9PL, UK.

1-9 August 1989. Hamburg and Munich, Federal Republic of Germany. XVIIIth International Congress of the History of Science. General theme: Science and Political Order. Information available from Prof. Christoph J. Scriba, Institut für Geschichte der Naturwissenschaften, Bundesstr. 55, D-2000 Hamburg 13, F. R. of Germany.

Science & Technology Studies welcomes information about forthcoming events of interest to its readership. Submission by electronic mail is encouraged, to USERFP2L@RPITSMTS. Other submissions to News Editor, *Science & Technology Studies*, Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, New York 12180-3590 USA.

Information for Contributors

Science & Technology Studies is a multidisciplinary journal which publishes research, commentary, and reviews. As the official journal of the Society for Social Studies of Science, it seeks to foster exchange and communication among a variety of individuals and groups concerned with the development and dynamics of science and technology, including their relationships with politics and society.

Science & Technology Studies publishes articles of the following types: research articles, representing the best methods and most important problems in the field of science and technology studies; theoretical essays, stretching the current limits of understanding; critical state-of-the-subfield reviews; policy essays, exploring an area where citizens and governments face choices with consequences; and author-meets-critics reviews. Contributions may also take the form of a letter to the editor or commentary on a published article.

Contributors should submit three copies of their manuscripts, double spaced throughout including footnotes. The review process is double-blind: neither author nor reviewer identity is revealed. Authors are responsible for removing all signs of their authorship from their manuscripts before they are submitted. Each manuscript will be sent to at least two reviewers, one in the specialty area of the article and one outside.

Contributions will be reviewed for interest to a multidisciplinary audience concerned with science and technology, for communication with that audience, and for quality of data and dependability of information. Verbatim comments of all reviewers will be forwarded anonymously to authors.

In deference to the many disciplinary writing styles represented in the Society, our referencing policy is pluralistic. Either numbered notes or author-date references may be used. In format, the journal follows the *Chicago Manual of Style*. With numbered notes, examples are:

1. Jarlath Ronayne, *Science in Government* (London: Edward Arnold, 1984).

2. Richard S. Westfall, "Science and Patronage: Galileo and the Telescope," *Isis* 76 (1985): 11-30.

3. Paul Forman, "Weimar Culture, Causality and Quantum Theory, 1918-1927," in *Historical Studies in the Physical Sciences*, ed. R. McCormach, Vol. 3 (Philadelphia: University of Pennsylvania Press, 1971), 1-115.

If author-date references are used, the bibliography should be constructed as follows:

Latour, Bruno and Steve Woolgar. *Laboratory Life: The Social Construction of Scientific Facts*. Beverly Hills: Sage, 1979.

MacKenzie, D. and B. Barnes. "Scientific Judgment: The Biometry-Mendelism Controversy." In *Natural Order*, edited by B. Barnes and S. Shapin, 191-210. Beverly Hills: Sage, 1979.

Roll-Hansen, Nils. "The Death of Spontaneous Generation and the Birth of the Gene: Two Case Studies of Relativism." *Social Studies of Science* 13 (1983): 481-519.

Authors are urged to consult recent issues of the journal for further examples.

Submission of accepted articles on diskette is encouraged, but not required. If you plan to use a word-processor to produce your manuscript, please contact the editor about the prospect of providing your copy on diskette.

Manuscripts may be submitted to any of the Contributing Editors or to:

Susan E. Cozzens
Department of Science and Technology Studies
Rensselaer Polytechnic Institute
Troy, New York 12180-3590 USA

Please mark "S&TS submission" on your envelope.

ATTENTION

Your 4S membership and subscription to *Science & Technology Studies* have expired if the last four digits in the first line of your address label are 8612. See subscription information (inside front cover) to renew.

Science & Technology Studies
Journal of the Society for Social Studies of Science
Department of Science and Technology Studies
Rensselaer Polytechnic Institute
Troy, New York 12180-3590 USA

NONPROFIT ORG.
US POSTAGE
PAID
WASHINGTON, DC
PERMIT NO. 2832

26960-STS-8712
STEPHEN ZEHR
DEPT OF SOCICLOGY
INDIANA UNIV
BLOOMINGTON, IN 47405