

4S Society for Social Studies of Science

Fall 1982

Volume 7 Number 3

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4S *Society for Social Studies of Science*

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A MESSAGE FROM OUR PRESIDENT

It is encouraging to know that, in these times of recession and retrenchment, our Society continues to grow and to flourish. The lively intellectual state of the field of social studies of science is apparent: if 1982 sometimes seems like 1932 in matters economic, it is far otherwise when it comes to our common cognitive concerns. Today the case for social studies of science is accepted by all, and a growing array of monographs and journal articles testify to the intellectual vitality of our field.

If our field of studies is intellectually vital, it does not follow that it is politically secure. Assaults on the behavioral and social sciences have become familiar in Europe as well as in North America, in recent days. The prevailing climate of retrenchment in many universities and colleges can too easily translate into threats to a field without powerful friends at court, or well-recognized departmental support. And 4S itself is still a fledgling organization, feeling its way as to both substance and procedure. There is thus little ground for complacency, but much for hope -- especially as regards the growing strength of 4S, on which I want to report more fully.

The summer 1982 issue of the 4S Newsletter carried vivid testimony to the calibre of candidates willing to stand for the 4S Council, and to the good work of Nicholas Mullins and his nominating committee. The best guarantee of our future as a society is a strong Council composed of individuals of major intellectual reputation, who are willing to work hard for 4S. Equally important is the informed participation of our members by voting for the candidates of their choice: please return your ballot paper at once, if you have not already done so!

The international, interdisciplinary nature of our Society has been apparent in the work of the committee on the John Desmond Bernal Award. The Bernal Award -- first given in 1981 -- is the world's highest honor open to scholars in the social studies of science. Our prize committee is diverse in membership, but it has come in with a unanimous, strong recommendation that the second recipient of the award be Robert K. Merton. Professor Merton's great distinction as sociologist and as historian needs no comment. But it is good to recall that he served as the first President of 4S, and that the award to him at our annual meeting in Philadelphia in October will come just over half a century after he graduated from Temple University and began his career in social studies of science. The presentation on behalf of the Society will be made by John Ziman at the 4S Luncheon on Friday, 29 October: I hope very many of you will be able to be present.

The Philadelphia Meeting promises other rewards. This four society gathering (History of Science Society, Philosophy of Science Association, Society for History of Technology, as well as 4S) will be a gala affair, coinciding as it does with Philadelphia's 300th birthday celebration. Tom Gieryn and his

committee have brought into being an excellent 4S contribution to an extraordinarily rich and diverse program. Details are given elsewhere in this Newsletter, which also contains a reprinting of the pre-registration forms. As well over 1,000 registrants are expected from the four societies and as hotel space will be quickly booked, please register early ... the PREREGISTRATION DISCOUNT ENDS ON 4 OCTOBER. At the Philadelphia Meeting we shall also have to settle our meeting locations for 1983 and 1984, and explore further the exciting possibility of a 4S Meeting in Europe.

The membership and finances of 4S are in a healthy state, thanks to the unflagging work of our Secretary-Treasurer, Lowell Hargens. Equally encouraging are the possibilities for the continued growth and evolution of the 4S Newsletter. That evolution is apparent in this present issue, as the good efforts of Lawrence Stern begin to result in the presence of an authoritative book review section. The eclectic nature of our field makes timely book reviews an especially important medium for the synthesis of work in diverse traditions, and for communication between different schools of interpretation. The further development of the book review section, and indeed of the whole 4S Newsletter, will be the subject of an "Editorial and Publications" Breakfast in Philadelphia, on Friday 29 October. Again, I hope many of you will plan to be present.

Finally, I might note that the very health and vitality of 4S, and the growing profusion of societies and groups concerned with one or another aspect of the meta-science of science, raises questions about the proper social organization of our field. To an historian interested in the politics of knowledge, this question is neither trivial nor straightforward. It is however one to which I hope to return on a future occasion.

Arnold Thackray
Philadelphia
30 July 1982

SEVENTH ANNUAL 4S MEETINGS

Planning is under way for the combined meetings of the History of Science Society, Philosophy of Science Association, Society for the History of Technology, and Society for Social Studies of Science, to be held in Philadelphia, 28-31 October 1982, in the Hilton Hotel and University City Holiday Inn on the campus of the University of Pennsylvania. The meeting will commence on Thursday evening, 28 October, with a reception and cash bar at the Hilton Hotel, and finish at noon on Sunday 31 October.

The program will provide an extraordinary variety of topics to inform and inspire participants. Sessions include "Contrasting Concepts of Sex and Sexuality," "Discovery, Heuristics, and Artificial Intelligence," "American Technology Abroad," "Science and Public Controversies," "Creationism and its Challenge," "Levels of Explanation in Biology," "The Rhetoric of Science," and "Marketing and Employing the Computer." Thomas Kuhn and Mary Hesse will participate in a symposium on "Revolution and Reference." In all, some 70 sessions will be held. Special sessions are scheduled on Saturday afternoon at nearby institutions, including the American Philosophical Society, Drexel University, and the Institute for Scientific Information, and will be followed by receptions for people attending those sessions.

Conference addresses will include presidential addresses by Ernan McMullin (PSA), Brooke Hindle (SHOT), and Arnold Thackray (4S), and the History of Science Lecture by Charles Rosenberg. Society awards to be announced and presented at the conference include the J. D. Bernal Award (4S), the Dexter Prize (SHOT), and the Zeitlin-Ver Brugge and Schuman Prizes (HSS).

Philadelphia is a walking city, and visitors should plan to take time to investigate Independence National Park, the eighteenth-century charm of Society Hill, and the grandeur of the nineteenth-century civic buildings. Museums and libraries abound, including the Philadelphia Museum of Art, American Philosophical Society, Academy of Natural Sciences, Franklin Institute, College of Physicians, Pennsylvania Hospital, and University Museum. And Philadelphia is famous for its restaurants, offering anything from the finest French to the most unusual ethnic fare. In addition, the city is celebrating its 300th birthday, and several Century IV events are scheduled for the week of the conference.

SPECIAL ANNOUNCEMENT

PHILOSOPHY OF SCIENCE ASSOCIATION. The last item available for purchase on the preregistration form is the PSA Proceedings. PSA publishes and distributes the papers for its Colloquia (Contributed Paper Sessions) in advance of the meetings. The sessions of the PSA meetings devoted to these papers are discussions of these papers, not presentations of the papers. These papers are the Proceedings available for purchase on the preregistration form. For the 1982 meeting the volume contains a total of thirty-three papers from the sessions entitled "Discovery, Rationality, and History of Science"; "Causation"; "Scientific Realism and Observation"; "Probability and Statistical Inference"; "Measurement, Verisimilitude, and Decision"; "Philosophy of Physics"; "Tachyons, Temporal Becoming, and Thermodynamics"; "Levels of Explanation in Biology"; and "Philosophy of Science, Past and Future; Metaphor and Play." Purchase with preregistration means the book will be received by the individual purchaser approximately a month in advance of the meeting allowing adequate time to read the papers for the appropriate sessions. Copies will also be available at the meeting.

PRE-REGISTRATION FOR COMBINED MEETING
HSS, PSA, SHOT, and 4S

PHILADELPHIA, 28-31 OCTOBER 1982

NAME _____

INSTITUTIONAL AFFILIATION FOR NAME TAG _____

MAILING ADDRESS _____

Society Membership(s): HSS () PSA () SHOT () 4S ()
(Please check all societies in which you hold membership)

Pre-registration Fees (AFTER OCTOBER 4 REGISTRATION FEE WILL BE \$40)

Member or Program Participant Fee, \$25 Amt _____

Senior/Student/Unemployed Fee, \$15 Amt _____

Non-member, \$30 Amt _____

FRIDAY EVENING RECEPTION, October 29 (free to registrants)

Guest tickets, \$5 ea. No _____ Amt _____

HSS BANQUET, October 30, \$12.75 ea. No _____ Amt _____

\$ 7.50 ea. (vegetarian) No _____ Amt _____

4S AWARDS LUNCHEON, October 29, \$8.95 ea. No _____ Amt _____

\$7.50 (vege) No _____ Amt _____

SHOT AWARDS LUNCHEON, October 30, \$8.95 ea. No _____ Amt _____

\$7.50 (vege) No _____ Amt _____

(Please turn page for additional registration information)

NAME
(Last)
(First)

BREAKFASTS:

HSS Women's Committee, October 29, \$2.50 ea.	Amt _____
SHOT W.I.T.H., October 30, \$5.50	Amt _____
SHOT Museum Interest Group, October 30, \$5.50	Amt _____
SHOT Pelicans, October 30, \$5.50	Amt _____
SHOT Jovians, October 31, \$5.50	Amt _____
4S Editorial & Publications, October 29, \$5.50	Amt _____
4S Social Psychology Sub-Group, October 30, \$5.50	Amt _____
PSA PROCEEDINGS, \$5.00	Amt _____
TOTAL _____	

All checks should be made payable to PHILOSOPHY OF SCIENCE ASSOCIATION

Please send payment in U.S. funds to: Combined Meeting Registration
Philosophy of Science Association
18 Morrill Hall/Department of Philosophy
Michigan State University
East Lansing, MI 48824
USA

PRE-REGISTRATION FORMS MUST BE RECEIVED BY OCTOBER 4, 1982

Check here if you will require child-care facilities_____.

Check here if you are interested in attending one special Saturday afternoon session plus reception:

4S at Institute for Scientific Information_____

PSA at Drexel University_____

HSS at American Philosophical Society_____

SHOT at American Philosophical Society_____

Check here if you are interested in a Sunday afternoon tour of Philadelphia sights of scientific and technical interest_____.

HOTEL RESERVATION FORM

1982 COMBINED MEETING OF HSS, PSA, SHOT, AND 4S (OCTOBER 28-31)
PHILADELPHIA, PENNSYLVANIA

NAME _____

ADDRESS _____

CITY _____ STATE _____ ZIP _____

Date arriving _____ Hour _____ A.M. Date departing _____ Hour _____ A.M.
P.M. P.M.

Please Reserve: UNIVERSITY CITY
HOLIDAY INN

THE HILTON HOTEL OF
PHILADELPHIA

Single(s)	_____ ea. \$38	_____ ea. \$54, 63, 72, 81, 90
Double(s)	_____ ea. \$44	_____ ea. \$68, 77, 86, 95, 104
Triple(s)	_____ ea. \$50	_____ ea. \$80, 89, 98, 107, 116
Quad(s)	_____ ea. \$56	_____ ea. \$92, 101, 110, 119, 128
Suites	_____ ea. \$145	

_____ I wish to share a room with _____.

_____ I wish to share a room with another member of this group selected
by the local arrangements committee.

RESERVATIONS MUST BE MADE BY OCTOBER 4, 1982. RESERVATIONS MUST BE ACCOMPANIED
BY A DEPOSIT FOR ONE NIGHT, REFUNDABLE UPON 48 HOURS NOTICE PRIOR TO EXPECTED
ARRIVAL. YOU WILL BE BOOKED AT THE LOWEST AVAILABLE RATE. PLEASE NOTIFY THE
HOTEL IF YOU PLAN TO ARRIVE AFTER 6:00 p.m. ABOVE RATES DO NOT INCLUDE LOCAL
TAXES. PLEASE ADD 6% TAX.

I enclose a check for \$ _____ (payable to University City Holiday Inn)

OR

Please charge deposit to my _____ Credit Card (Specify BankAmericard or Visa)
Number _____ Expiration Date _____

Signature _____

FILL OUT AND RETURN TO: Reservations Department
University City Holiday Inn
36th and Chestnut Streets
Philadelphia, PA 19104

PROGRAM

SOCIETY FOR THE SOCIAL STUDIES OF SCIENCE (4S)

SEVENTH ANNUAL MEETING

October 28-31, 1982

Philadelphia, Pennsylvania

FRIDAY, OCTOBER 29, 9:00 A.M.

1. Political Economy of the Life Sciences

Chair: Sal Restivo (Sociology, Rensselaer Polytechnic Institute)

Papers: Naomi Aronson (Sociology, Northwestern), "Science and Social Justice:
U. S. Nutrition Researchers and the Wage Problem: 1885-1925"

Deborah Fitzgerald (History and Sociology of Science, Pennsylvania),
"Exporting the Land-Grant Model: The Rockefeller Foundation
in Mexico, 1943-53"

Elizabeth Fee (Hygiene and Public Health, Johns Hopkins), "The
Rockefeller Foundation and the Politics of Public Health, 1914-
1929"

Discussant: Nancy Stepan (History, Yale)

2. Style and Structure of Scientific Papers

Chair: Harry Collins (Sociology, Bath)

Papers: Charles Bazerman (English, Baruch College), "The Emergence of the
Modern Physics Article: A Stylistic History of the Physical
Review, 1893-Present"

Susan Cozzens (National Science Foundation), "Comparing the Sciences:
Citation Context Analysis of Papers from Neuropharmacology and
the Sociology of Science"

Nicholas C. Mullins (Sociology, Indiana), "Form and Function in
Scientific Papers"

M. Callon, J.-P. Courtial, W. A. Turner and S. Bauin (Paris), "From
Translations to Network: The Co-Word Analysis"

Discussant: Harry Collins (Sociology, Bath)

FRIDAY, OCTOBER 29, 9:00 A.M. (continued)

3. Creation and Acceptance of Scientific Innovation

Chair: Ron Westrum (Sociology, Eastern Michigan)

Papers: Peter Messeri (Sociology, Columbia), "Institutional Bases for Early Support of Innovative Thought in Science: The Case of Drifting Continents"

Carlos Kruytbosch and Frederick Betz (National Science Foundation), "The Role of Instrumentation and Analytical Technique in Scientific Advance"

Dorothy Rosenberg (Sociology, West Virginia), "Incorporation of Medical Innovation: Acupuncture"

Gil Peach (Sociology, New York University), "Social Organization and the Manufacture of Statistical Products"

Discussant: Ian Lubek (Psychology, Guelph)

FRIDAY, OCTOBER 29, 12:30 P.M.

4S Luncheon and Awards Banquet

Presidential Address

"The Politics of Knowledge"--Arnold Thackray

FRIDAY, OCTOBER 29, 2:00 P.M.

4. Social Stratification and Scientific Careers

Chair: Steven F. Cohn (Sociology, Maine)

Papers: John Ziman (Bristol), "Research Careers: Specialization and Change"

Jan Vlachý (Prague), "Performance Inequality in Scientific Discipline"

Lowell Hargens and Diane Felmlee (Sociology, Indiana), "Effects of Structural Characteristics of Scientific Fields on Measures of Recognition of Scientists"

James G. Ennis (Sociology, Plattsburgh) and Angela O'Rand (Sociology, Duke), "Stratification in Multiple Networks Among Biomedical Researchers"

Discussant: Jerry Gaston (Sociology, Texas A&M)

FRIDAY, OCTOBER 29, 2:00 P.M. (continued)

5. Technology Indicators: Recent Developments in the Use of Patent Data

Co-Sponsored with the Society for History of Technology

Chair and Organizers: Mary Ellen Mogee (Dept. of Commerce)
Carole Kitti (National Science Foundation)

Papers: Jennifer Bond (National Science Foundation), "The Use of International Patenting Statistics as Indicators of Inventive Activity"

Richard S. Campbell and Claudia E. Thompson (Battelle), "Patent Citation Analysis"

Mark P. Carpenter and Francis Narin (Computer Horizons), "Assessment of the Linkages Between Patents and Fundamental Research"

Discussants: Thomas P. Hughes (Pennsylvania)
Roberta Balstad Miller (COSSA)
Robert Post (Smithsonian)

6. Rhetoric of Science

Chair: Stephen Turner (Sociology, VPI)

Papers: Trevor Pinch (Sociology, Bath), "The Three-Sigma Enigma: On the Negotiation of Error in Physics"

Bruno Latour (Paris), "Inscription Devices in Science"

Michael Altimore (Sociology, Mt. Mercy, Iowa), "The Rhetoric of Scientific Argument: Recombinant DNA"

Steve Woolgar (Sociology, Brunel), "The Scientist as Sociologist: An Application of Attribution Theory to the Social Studies of Science"

Discussant: Donald T. Campbell (Social Relations, Lehigh)

Friday, October 29, 5:15 pm--4S BUSINESS MEETING

SATURDAY, OCTOBER 30, 9:00 A.M.

7. Prosopography and the Study of Modern Science: Problems and Prospects

Chair and Organizer: John W. Lankford (History, Missouri)

Papers: Margaret W. Rossiter (California--Berkeley), "Beyond Collective Biography: The Changing Structure of Scientific Work"

Steven Shapin (Science Studies, Edinburgh), "Prosopography and the Sociology of Knowledge"

Discussants: Daryl E. Chubin (Sociology, Georgia Tech)
Spencer R. Weart (American Institute of Physics)

8. Science and Public Controversies

Chair: Dorothy Nelkin (Science, Technology and Society, Cornell)

Papers: Ronald Brickman (Hoover Institution), "Public Controversies Over Toxic Chemicals in Four National Settings: Science as Cause or Cure?"

Deborah G. Mayo (History, Virginia Tech), "Metastatistics and Public Controversy"

Francis B. McCrea and Gerald Markle (Sociology, Western Michigan), "The Medicalization of Normalcy?: Changing Definitions of Menopause"

Discussant: Robert F. Rich (Woodrow Wilson School, Princeton)

SATURDAY, OCTOBER 29, 9:00 A.M. (Continued)

9. Techniques of Science: Instrumentation and Calibration

Chair: Thomas F. Gieryn (Sociology, Indiana)

Papers: Paul R. McAllister and Francis Narin (Computer Horizons), "Analysis of the Contribution of Scientific Instrumentation to Highly Cited Research"

David Edge (Science Studies, Edinburgh), "Instrumentational Development in Radio Astronomy"

Wouter Van Rossum (Social Welfare, Groningen), "Before Standardization: Temperature as a Social Construction"

Harry Collins (Sociology, Bath), "The Function of Calibration in Experimental Science"

Discussants: Steve Woolgar (Sociology, Brunel)
Derek deSolla Price (History, Yale)

SATURDAY, OCTOBER 30, 2:00 P.M.

10. Can Sociologists and Philosophers Say Anything Useful About Scientific

Discovery Chair and Organizer: Richard Burian (Humanities, Drexel)

Papers: Thomas J. Nickles (Philosophy, Nevada--Reno), "How Discovery is Important to Cognitive Studies of Science"

Karin Knorr (Sociology, Wesleyan), "Scientific Discovery: A Sociologist's Perspective"

Discussants: Larry Laudan (Virginia Tech)
Augustine Brannigan (Sociology, Calgary)

SATURDAY, OCTOBER 30, 2:00 P.M. (continued)

11. Organization of Debates in Biological Research

Chair: Anselm Strauss (Sociology, California--San Francisco)

Papers: Rachel Volberg (Tremont Institute), "From Gene Ecology to
Experimental Taxonomy: A Chapter in the Relationship
Between Ecology and Taxonomy"

Susan Leigh Star (Tremont Institute), "Tactics in the Debate
About Cerebral Localization, 1870-1906"

Elihu Gerson (Tremont Institute), "Confounded Problem Structures
and Debates: The Case of Population Realignment in Biology,
1880-1925"

William Wimsatt (Philosophy, Chicago), "Heuristic Biases and Failures
of Communication in the Units of Selection Controversy"

Discussant: to be announced

12. Money for Science

Chair: Daryl E. Chubin (Sociology, Georgia Tech)

Papers: J. Davidson Frame (George Washington), "Impact of Funding Cuts in
Biomedical Research"

C. Farina (Trent, Ontario), "National Need and Peer-Review: Canada"

Gerald Markle and Stanley Robin (Sociology, Western Michigan), "The
Scientific Ethic and the Spirit of Capitalism: Recombinant
DNA Research for Profit"

Henry Etzkowitz (Sociology, Purchase), "Entrepreneurial Scientists
and the University"

Discussant: Michael Moravcsik (Theoretical Science, Oregon)

SATURDAY, OCTOBER 30, 3:30 P.M.

13. Roundtable: Do Retrospective Citation Indexes Have Applications in the
History and Sociology of Science? (Sponsored by and Held at the Institute
For Scientific Information)

Chair: Morton V. Malin and Henry Smal: (I.S.I.)

Speakers: David Edge (Science Studies, Edinburgh)
Eugene Garfield (I.S.I.)

Moderator: Ronald Overman (National Science Foundation)

SUNDAY, OCTOBER 31, 9:00 A.M.

14. Bibliometrics: Quantitative Analysis of Scientific Literatures

Chair: Daniel Sullivan (Carleton College)

Papers: Marc DeMay (Ghent), "Cognitive Structures and Dynamics of Scientific Specialties: Comparison of Bibliometric and Cognitive Approaches"

Thomas Schøtt (Sociology, Columbia), "Curves of Scientific Literatures, References and Citations: A Mathematical Model of Dynamic Interrelation"

Jane Morley (History and Sociology of Science, Penn), "E. O. Wilson's Sociobiology: A Bibliometric Study of Scholarly Use, 1975-79"

M. Rees and L. Lomnitz (Mexico City), "Development of Mexican Science"

Discussant: Belver Griffith (Humanities, Drexel)

15. Science Indicators: The Quantification of Science and Technology

Chair and Organizer: Robert Wright (National Science Foundation)

Papers: Gerald Holton (Harvard), "Developing Indicators of Quality in Science"

Nestor Terleckej (National Planning Association), "Economic Indicators and Science Indicators"

Denis Johnston (consultant), "Comparisons of Science Indicators and Other Social Indicators"

Discussants: John Holmfeld (Staff, House Committee on Science & Technology)
Donald Buzzelli (Science Indicators Unit, National Science Foundation)

SUNDAY, OCTOBER 31, 9:00 A.M. (continued)

16. Policy Making, Error Correction and High Risk Technologies

Chair: Edward J. Woodhouse (Political Science, Rensselaer Polytechnic)

Papers: Ted I. K. Youn (Public Administration, Hartford), "Decision Making and Error Correction: A Theoretical Perspective"

Joseph G. Morone (General Electric), "Error Correction in the Regulation of Nuclear Reactors"

Edward J. Woodhouse (Political Science, Rensselaer Polytechnic), "Error Correction in the Regulation of Toxic Chemicals"

Discussant: Stanley Carpenter (Philosophy, Georgia Tech)

SUNDAY, OCTOBER 31, 1:00 P.M.

17. Public Images of Science

Chair: Bernard Barber (Sociology, Barnard College, Columbia)

Papers: Aant Elzinga (Theory of Science, Gotesborgs), "Scientists, Romanticism and Social Realist Images of Science"

Jon D. Miller (Northern Illinois), "The Leadership View of American Science and Technology"

John M. Wilkes, Gabriel Haim and Arlene McCormach (Boston University), "Sex Differences in Perceptions of Science and Scientists"

Joseph Haberer (Political Science, Purdue), "The Cartesian Scientist as Political Actor: Einstein's Political and Social Involvement in the Context of Our Time"

Discussant: Bernard Barber (Sociology, Barnard College, Columbia)

SUNDAY, OCTOBER 31, 1:00 P.M. (continued)

18. Fraud in Science

Chair: Harriet Zuckerman (Sociology, Columbia)

Papers: Warren Schmaus (Philosophy, Illinois Institute of Technology),
"Fraud and Negligence in Science"

Thomas A. Shannon (Psychiatry, Worcester), "Fraud: Personal and
Professional Dimensions"

Ray Goodell (Humanities, Massachusetts) "Fraud in Mass Media Presentations of Science: The Case of the Cloning Hoax"

Discussants: Pat Woolf (Sociology, Princeton)
Alexander Kohn (Chemistry, Stanford)

ABSTRACTS OF PRESENTATIONS--SEVENTH ANNUAL MEETING (PUBLISHED ALPHABETICALLY)

The Rhetoric of a Scientific Controversy: Recombinant DNA

Michael Altimore, Mount Mercy College

The dispute over the use of R-DNA techniques comprised several issues: technical (short-term safety), philosophical (e.g., whether scientists would be able to create life, or 'interfere with evolution'), and political (whether the technique would permit genetic engineering or be used for biological warfare).

In this paper I argue that scientists restricted their arguments to narrow technical concerns when speaking to non-scientists (such as members of Congress). This excluded, to a great extent, some of the most important issues involving the relationship between science and the public, such as who should decide how public funds are spent.

Finally, I raise some general questions about that most powerful rhetorical strategy, the promising that tangible social benefits will result from basic scientific research.

Science and Social Justice: U. S. Nutrition Researchers and the Wage Problem 1885-1925

Naomi Aronson

The period 1885-1925 was one of radical transformation in the science of nutrition and in American society. This paper shows how conceptual developments in nutrition research interacted with the social problem of fair wages for American industrial workers. Four episodes in nutrition studies are discussed: 1) the study of caloric requirements (pre-1900); 2) the revision of protein requirements (1904-1907); 3) the transformation: the vitamin concept (1912-1915); and 4) the identification of the deficiency diseases (1916-1925). Prior to the vitamin concept, nutrition studies tended to support the view that wages were adequate, but that workers wasted their money on luxury foods. The vitamin concept drew attention to the inadequacy of wages by demonstrating that certain expensive foods -- such as milk, butter, fruits, and vegetables -- were essential to the diet. The role of external factors, especially World War I, in the egalitarian transformation of American nutrition studies is also considered.

The Use of International Patenting Statistics as Indicators of Inventive Activity

Jennifer Sue Bond, Science Indicators Unit, Division of Science Resources Studies, National Science Foundation

This paper will focus on foreign patenting activity in the United States and on U.S. patenting activities abroad. U. S. patent data will be analyzed by country of origin and product field. Comparisons will also be made of patent trends in other industrialized countries. Factors affecting international patenting activity will be discussed. Preliminary information will be presented on those patents which received a relatively high number of citations from subsequently issued patents. It is thought that analysis of highly-cited patents can provide indicators of patent areas of technical importance.

Public Controversies over Toxic Chemicals in Four National Settings:
Science as Cause or Cure?

Ronald Brickman

The paper explores the role of science in the genesis of political conflict over toxic chemical regulation in the United States, Britain, France and West Germany. In each country, the three preconditions for public controversy are met: the need for scientific input into decision-making paired with the inability of science to provide unequivocal policy prescriptions; the structuring of decision-making into a nearly zero-sum situation where the social advantages of regulation must be weighed against the social costs; and the active presence of powerful social groups having opposing interests in policy outcomes. Despite this similar political context across national settings, one finds that science plays vastly different roles in the development and resolution of conflict. In the U.S., controversy gravitates to and polarizes around areas of scientific uncertainty; in Europe, the likelihood of politicization of the technical component of decision-making is considerably less. These differences are explained with reference to the intervening features of political organization and state-society relations that give variable importance to science as a source of legitimation for regulatory intervention.

From Translation to Network: The Co-Word Analysis

Michael Callon, Centre de Sociologie de l'Innovation, Ecole des
Mines, Paris

Jean-Pierre Courtial, Centre de Sociologie de l'Innovation, Ecole
Mines, Paris

W. A. Turner, Informascience, CNRS, Paris

S. Bauin, Centre de Sociologie de l'Innovation, Ecole, Mines, Paris

How can one identify and analyze the constantly changing relationships between research activity and the general socio-political context without using the classical and now often contested distinction between intellectual and social influences on scientific growth? The notion of "translation" is proposed as a way of answering this question: Recent studies have shown that this notion can be used to study how actors (a) establish the identity of other actors and determine--at least temporarily--their interests and their strategy; (b) define network problems; (c) objective, remodel, and transfer knowledge; (d) establish hierarchies between organizations, groups, and individuals. The paper presented is particularly concerned with point (b) above, that is, translations which have as their goal to define and link together scientific, technical, political, economical, or other types of problems.

We show that words used in a scientific paper are translation operators: they canalize and aggregate interests by their action on the restructuration of a problem network. We then discuss a method--the co-word analysis--which allows us to visualize these networks and to follow their evolution from one period to another, given the constant negotiation among actors of just what is a problem and what is not.

Patent Citation Analysis

Richard S. Campbell, Technology Planning and Analysis Section, Battelle
Pacific Northwest Laboratories, Richland Washington

Claudia E. Thompson, Technology Planning and Analysis Section, Battelle
Pacific Northwest Laboratories, Richland Washington

Patent citations provide a unique source of information on the developmental phase of technologies and their evolution. For example, patent citation analysis can detect technological shifts which are the result of market changes or changes in the knowledge base. Patent citations can also indicate technological dominance among firms and countries, measure rates of technological change and identify emerging technologies.

While many of the measures and computational techniques for analyzing patent citation data are directly transferable from the bibliometric study of scientific literatures, there are fundamental differences which should be taken into account.

Assessment of the Linkages Between Patents and Fundamental Research

Mark P. Carpenter
Francis Narin

An assessment of the linkages between patents and fundamental research is described based on the extraction and analysis of the references in seven rapidly growing subclasses of U.S. patents. All of the references in approximately 400 patents in each of these seven subclasses were extracted and analyzed with respect to whether they were to research papers, to patents or to other sources. In addition we looked at the age of the cited research papers, at their subject, and at their level (basic to applied).

The data reported here clearly demonstrate extensive utilization of fundamental scientific literature by patent applicants and examiners. The average patent studied contained 12 references, of which approximately half were provided by the applicant and half by the examiner. Approximately half of these 12 references were to U.S. patents; of the remaining six references, half or 3.3 per patent were to the core of 2400 central scientific research journals. More than half of the journal references in the patents are to applied or basic scientific journals as opposed to engineering and technological journals. Thus these seven rapidly growing subclasses of patents did demonstrate a strong dependence on fundamental scientific literature.

We also found that the cited literature was relatively recent, no older than the literature cited by typical scientific articles. Thus, the literature citation process used by patent applicants and examiners appears to be quite similar to the process of citing of the journal literature by scientists themselves when they are publishing scientific papers.

Comparing the Sciences: Citation Context Analysis of Papers from
Neuropharmacology and the Sociology of Science

Susan E. Cozzens, National Science Foundation

This paper compares the structure of two scientific fields, neuropharmacology and the sociology of science, by comparing the pattern of citations to an influential paper from each area. If scientists in these fields use each other's work in qualitatively different ways, then those differences should be reflected in what they say about each other's work when they cite it. The research therefore utilizes citation context analysis, that is, the close examination of the text surrounding the footnote number, in an attempt to illuminate the structural differences between the fields. Preliminary results of the analysis indicate that the neuropharmacology paper received much more detailed attention and critical scrutiny than the paper in the sociology of science. Correlatively, the sociology paper was more often used in direct support of the citing author's knowledge claims, without any mention of the content of the cited work itself. In addition, changes over time in the character of the citations to the two papers differed. The citations to the neuropharmacology paper showed a striking pattern of unidirectional change: level of detail dropped over time, and the paper was used less often in each citing work as time went on. In contrast, the use of the sociology of science paper shows no clear trend. The use of the paper seems to have been determined more by the dominant research interests in the field at a given point in time than by the length of time since publication. The pattern of change for the neuropharmacology paper thus seem to be cumulative, while the pattern in the sociology of science case does not.

Cognitive Structure and Dynamics of Scientific Specialities: A
Comparison of Bibliometric and Cognitive Approaches

Marc De May, University of Ghent, Belgium

The paper explores several notions of "cognitive structure" currently used in various forms of social studies of science. In particular, we focus upon a comparison between Small and Greenlee's (1980) depiction of cognitive structures obtained by means of co-citation clustering, analyses of the cognitive structure of specialities embodied in the construction of expert-systems in artificial intelligence (AI) and cognitive structures underlying the various interpretative schemes of scientists revealed in the micro-analysis of scientific discourse. Small and Greenlee introduce their results as describing "the mental fruniture" of a researcher in a given area. But is it possible to uncover by means of their approach the tacit knowledge that shapes expectations and that provides directionality to search, aspects of cognitive structure emphasized by "cognitive" orientations in sociology, psychology and AI? We argue that cognitive maps based on co-citation clustering do not capture the generic cognitive structures instrumental in the production of scientific discourse and scientific text. However, a detailed analysis of the pattern and the role of puzzle-solving in the dynamics of scientific specialities will reveal in the principle of heterarchical organization a possibility of relating the various notions of "cognitive structure."

Scientism, Romanticism and Social Realist Images of Science

Aant Elzinga

The paper sets out to contrast three different images of science, related to three rival ideologically based perspectives. The three views and attitudes toward science referred to in the title are images that have existed at least since the French Revolution. The differences and lines of demarcation are most apparent in times of social strife and unrest, when ideological perspectives of society and rival recipes for restoring order bring with them different attitudes and expectations regarding science. Scientism is the general philosophy according to which all aspects of the universe are knowable through the methods of science, and it affirms a principle of progress through science. The romantic image on the other hand tends to emphasize intuition and other (non-scientific) modes of knowing as equal or superior. At the level of political action scientism has been associated with technocratic solutions and romantic images are favoured in some populist movements. Historically these images have had a social base in different sections of national populations. The third public image of science, the one referred to here as social realist, is most evident in parts of the labour movement and derives some of its original formulations from the writings of Marx who opposed both the technocratic images of industrial magnates and the utopian socialism of what he called petty bourgeois strata in the society. Within the Marxist tradition itself, as it was developed in Europe, there exists a tension between technocratic and romantic images of science. This is evident in the advent and decline of the Proletcult movement in Russia directly after the revolution, and more recently in the cultural revolution and its aftermath in China. The paper seeks to make sense of the "pendular" reversals between technocratic and romantic "phases" which occur at certain times, not only in socialist societies.

Stratification in Multiple Networks Among Biomedical Researchers

James G. Ennis

Angela M. O'Rand

Informal contacts among biologists at a summer research institute exhibit a pattern of cliques stratified by age and productivity. These findings support previous analyses of stratification in science, and extend them to the interrelations of network positions. Research, social and community affairs contacts are concentrated within rather than between positions. The center-periphery structure common in other studies was not found in the aggregate, but may exist within local regions within the network. The bases of this structure and its implications are considered.

National-Need and the Peer-Review Process

C. Farina, External Associate, Administrative and Policy Studies Program,
Trent University, Peterborough, Canada

In recent years the demands placed on university research have altered significantly. Government, aware of the potential contribution of innovation to the economy and conscious of the role R&D can play in innovation, has increasingly linked funding increases for university research to programs which encourage research of relevance to national, and in particular, industrial need.

In Canada, the funding of university research in science and engineering is, to a large extent, focussed in the programs of the Natural Sciences and Engineering Research Council (NSERC). Government financing of NSERC declined during the early seventies but, in recent years, has increased significantly. Much of this increase has been absorbed in a new 'strategic grants' program. Although the specific aims of the program appear not to be clearly defined, it is clear that the program is intended to stimulate research in areas of national need such as food/agriculture, communications, and transportation. Operating grants for basic research continues to consume the bulk of NSERC's resources but it is expected that the strategic grants program will represent approximately 15% of the Council's resources at maturity.

NSERC awards its grants through a process of peer-review. This process involves external assessment of proposals submitted by university researchers with a final decision being taken by a committee of experts drawn largely from the universities.

In this paper, a quantitative look will be taken at some of the latent conflicts between the government's changing expectations of university research and this allocation process. Focussing on the granting histories of individual grantees, it will be shown that statistically, an individual's funding level is a function of the period of time over which he has been funded by the council. Studies of other systems of decision making make these findings unsurprising in that they reflect the incremental outcomes of group decisions. However, they do point to the structural problems inherent in a top-down approach to stimulating research in areas of national need within a system involving diffuse focal-points for decision making. If future government funding is to be ensured, it will be important to either clarify expectations in order to take account of the traditional autonomy of research councils or, if need is to be explicitly defined as 'customer' oriented, to explore alternative funding mechanisms.

Exporting the Land-Grant Model: The Rockefeller Foundation in Mexico 1943-1953

Deborah Fitzgerald, History and Sociology of Science, University of Pennsylvania

In 1943 the Rockefeller Foundation, in cooperation with the Mexican government, began a program in agricultural improvement in Mexico. Emphasizing original research into agricultural problems, the Rockefeller team aimed to revitalize Mexican agriculture by creating both a pertinent body of scientific knowledge and an elite corps of agricultural scientists. To this end they provided graduate training for promising Mexican agricultural students, rural demonstrations of improved farm practices, and sophisticated agricultural inputs such as hybrid seed and fertilizer.

In spirit and in practice, the Rockefeller program adopted the styles of two fairly different organizational structures. The first, common to philanthropic organizations, embodied the belief that a massive but short-term dose of money and expertise could not only improve conditions but could infuse the Mexicans with the spirit of progress sufficient to keep the ball rolling after the Rockefeller Foundation departed. The second, most clearly expressed in the land-grant system, reflected the assumption that scientific agriculture should and could be made available to farmers through a multi-level network of research, education, and extension.

This paper will discuss the ways in which these differing approaches were expressed in the Mexican program. I will argue that, unlike earlier programs in philanthropic relief that tended to help those most in need of help, the Rockefeller program more closely resembled the land-grant system which, despite its democratic ideals, tended to produce and disseminate a sophisticated science and technology for more advanced farmers in an effort to improve not the general standard of living, but regional and national productivity.

Confounded Problem Structures and Debates: The Case of the Population Realignment in Biology, 1880-1925

E. M. Gerson

Debates in science often arise around confounded problem structures; i.e., problems which are defined and interpreted in overlapping, but incongruent, fashion by different participants in the research process. This is particularly likely to occur when different research traditions or disciplines address the same family of problems from different perspectives.

Resolution of these debates involves reconstruction of the problems involved, so as to eliminate the confoundings. Reconstruction of problem structure can, in turn, lead to reorganization of the division of labor in a field (realignment) under certain circumstances. Under other circumstances, resolution of debate may simply lead to additional specialization.

This paper develops a model of the relationship between resolution of confounded debates on the one hand, and the disciplinary or programmatic division of labor on the other. The model is illustrated by a case study of the realignment in natural history during the period 1880-1925, which resulted in the modern disciplinary structure of population biology and organism/cell biology.

Fraud in Mass Media Presentations of Science: The Case of the Cloning Hoax

Rae Goodell, Writing Program, Department of Humanities, Massachusetts Institute of Technology

The problem of fraud in science has attracted the serious attention of scientists and scholars and the increasing attention of journalists. In general, the frauds at issue are cases in which it is largely the scientific literature. A related category of deception might be called "fraud in science reporting": cases in which it is largely the lay public that is misled by falsified information about science in the popular press.

Like fraud in science, fraud in science reporting raises important questions about the institution of science and its relationship to the rest of society. Discussion of these questions has frequently been hampered, however, by controversy over whether the popular claims--such as Bigfoot, ESP, the Bermuda Triangle, the Jupiter Effect--are indeed fraudulent or even false. In 1978, however, a striking case arose in which, by means of litigation, the issue of fraudulence was resolved, and some of the circumstances behind the deception were made available for scrutiny. The case was J. B. Lippincott's publication of In his Image: The Cloning of a Man, in which science writer David M. Rorvik claimed that an eccentric millionaire had successfully arranged to have himself cloned, producing a genetically identical offspring. In a suit brought by British scientist Derek Bromhall, the book was ruled a hoax in 1981, and a settlement was awarded in 1982.

In analyzing the conditions that fostered the fraud, one finds three major areas of weakness: (1) a relative lack of professional standards in American book publishing; (2) a tendency among science journalists to avoid criticism of science; and (3) a perception among media professionals, at times justified, at times crippling, that the scientific community exerts too much control over the flow of information about science to the public.

All three of these problem areas can be expected to have significance in understanding not just popular scientific frauds but also the more general failures and frustrations in communication between the scientific community and the public.

Effects of Structural Characteristics of Scientific Fields on Measures of the Inequality of Recognition of Scientists

Lowell L. Hargens

Diane H. Felmlee

Measures of inequality of scientific recognition have been used to study processes of accumulative advantage in science (Allison and Stewart, 1974) and variation in levels of codification across scientific fields (Cole, Cole and Dietrich, 1978). We develop a formal model of the allocation of citations among members of a scientific field in order to show the effects of 1) the growth rate of the field and 2) age variation in the probability that a paper will be cited (the "immediacy effect" - see Price, 1970), on the degree of inequality in recognition that scientists receive. We show that a field's growth rate is positively associated with its level of inequality, and that fields with larger immediacy coefficients have lower levels of inequality. We argue that previous comparisons of levels of inequality in different scientific fields may have reached incorrect conclusions because of their failure to take account of the effects we demonstrate. We also show how much of the variation in inequality of recognition across fields can be attributed to these two structural features in comparison to the effects of variation among individual scientists in their levels of productivity and the average recognition their papers receive.

Scientific Discovery: A Sociologist's Perspective

Karin Knorr-Cetina, Department of Sociology and Science in Society,
Wesleyan University

In this paper, I propose to give a short and of course inadequate outline of some of the main issues in the analysis of scientific discovery from a sociologist's perspective. To be sure, scientific discovery is not seen here as a singular happening of extraordinary inspiration, but rather as the ordinary process of work by which scientists produce knowledge which counts as new. It is also clear that this process is not independent of the so-called context of justification. Arguably, the process of knowledge production represents and illustrates major aspects of consensus formation. In addition, ethnographic observations provide for a version of discovery which confronts the sense in which the latter is both "epistemic" and "transepistemic," community-bound and transcending the scientific specialty. I shall attempt to argue for an internalist sociology of knowledge production which is uncommitted to relativism. To exemplify this perspective, I shall draw upon recent laboratory studies of scientific work.

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The Role of Instrumentation and Analytical Technique in Scientific Advance

Carlos Kruytbosch, Staff Associate, Planning and Policy Analysis,
National Science Foundation

Frederick Betz, Program Manager, Industry, University Cooperative
Research Projects

Listings of major advances between 1950 and 1975 in astronomy, chemistry and the earth sciences were compiled from panels of active scientists in these fields.

The advances are classified according to the principal character of the activity: theoretical; new observational/analytical technique; new observational/analytical equipment; discovery of new object or property; improved measurement.

The prominent place of new instrumentation and observational and analytical techniques in the advancing front of science are described and discussed.

On Inscription Devices

Bruno Latour, Centre Science Technologie Et Societe, Paris

This set of slides taken from various disciplines and various periods, purport to illustrate and also to demonstrate the notion of inscription device. This notion has been developed in sociology of science to approach on a material basis the intellectual and cognitive features of science. It allowed to explain in terms of the sciences of literature, most of the so called "ideas" of scientists. By providing a unified view of the scientific activity it allowed to treat throughout the all production line, facts of nature as well as machines.

Since the first development of the notion, much work has been done in cognitive anthropology, Piagetian psychology, history of print, semiotics and history of scientific and technical drawings and visual displays, to largely confirm the work done by sociologists of science. The set of slides shows the various disciplines that studied the scientific literature, as work on many examples from cartography, to physics, through daily life uses of science or biochemistry and technical engineering.

The slide is not a simple illustration of a theme that could be developed by way of words, it is an essential feature of the demonstration itself. It shows scientific activity as ways of showing, displaying and writing.

The title of the paper could be, if it is maintained into the rhetoric of science paper: "The visual rhetoric of science: A slide show of inscription devices."

Metastatistics and Public Controversy

Deborah G. Mayo, Virginia Polytechnic Institute and State University

Despite the increased public concern over the social consequences of scientific and technological decisions, there has not been sufficient public influence in resolving the controversies to which these decisions give rise. One of the most important contributions that could be provided by interdisciplinary studies of science, technology, and society would be to rectify this situation by providing the means by which the public can understand and critically evaluate controversial policy decisions. In order to accomplish this aim, I argue, tools for clarifying and critically examining the statistical reasoning underlying these decisions must be provided. For example, controversial decisions to ban a practice (e.g., DNA research, nuclear technology) or a substance (e.g., birth-control pills, saccharin, laetrile) cannot be effectively evaluated without tools for assessing the statistical reasoning leading to judging that the risks corresponding to the practice or substance are unacceptable. Finding this reasoning to be flawed provides grounds for rejecting the decision based upon it.

However, there is a great deal of controversy among scientists, statisticians, and philosophers as to whether and how available statistical methods may be used to reach valid statistical conclusions. Hence, assessing policy controversies is closely connected with assessing statistical controversies; and the purpose of this paper is to provide means for doing the former by providing tools for doing the latter. Since such tools function when statistics itself is the object of study, they are tools for doing metastatistics. I illustrate the use of these metastatistical tools by applying them to a case study of the controversy surrounding the risks associated with birth-control pills.

Analysis of the Contribution of Scientific Instrumentation to Highly Cited Research

Paul R. McAllister
Francis Narin

Instrumentation plays a fundamental role in the progress of science and the understanding of the role of instrumentation in scientific advance is a key policy issue. The current research utilized random samples of 100 papers published in 1973 and 1974 drawn from among the top 5% of most highly cited papers in each of the fields biochemistry, botany, organic chemistry, solid state physics and electrical engineering to investigate the contribution of instrumentation to highly cited research.

Overall, these fields were found to be highly dependent on instrumentation, with only 9% of the sampled papers requiring no instrumentation. In the fields of biochemistry, botany and organic chemistry nearly 100% of the papers described research in which instrumentation played a necessary role and these instruments were largely available off the shelf and had been available more than three years prior to publication of the paper. Biochemistry showed a heavy dependence on liquid scintillation counters and organic chemistry a high utilization of NMR spectroscopy.

The fields of solid state physics and electrical engineering showed more diversity in the role of instrumentation and in the range of instruments mentioned. The reported research depended on instrumentation either directly or indirectly in 87% of the solid state papers and in 58% of the electrical engineering papers. No instrumentation was necessary for 13% of the physics and 30% of the engineering papers. Thirteen percent of the electrical engineering described the development of new applications or new types of instrumentation.

The Medicalization of Normalcy?: Changing Definitions of Menopause

Francis McCrea
Gerald Markle

During the past 100 years, the medical community has defined the etiology and problems of the menopause in three different ways. During the Victorian era, menopause was seen as a sign of sin and decay; with the advent of Freudian psychology it was viewed as a neurosis; and as synthetic estrogens became readily available in the 1960s, menopause was treated as a deficiency disease. Different as these views are, they all assume that: (1) women's potential and functions are biologically destined; (2) women's worth is determined by femininity and attractiveness; (3) rejection of the feminine role will bring physical and emotional havoc; and (4) aging women are useless and repulsive. Each medical definition is seen as a product of the physician's power and the woman's stigma. Each definition reflects, and in turn legitimizes, dominant social interests. A fourth definition of the menopause has recently come from outside the medical community. Feminists now view the menopause as normal and unproblematic. This new definition represents the political efforts of oppressed interests to fight off their stigma. As a sociohistorical case study, this research ties together ideas from recent feminist, deviance, and medical literature and shows the antecedents and consequences of the social construction of health and illness.

Institutional Bases for Early Support of Innovative Thought in Science: The Case of Drifting Continents

Peter Messeri, M. Phil. Columbia University

This paper reports on research investigating the nature and extent to which position in the social structure of science influenced individual propensity towards early support of the revolutionary notion that ocean floors and continents have undergone lateral displacement of thousands of miles during the course of geologic history (e.g., continental drift, seafloor spreading). A sample of research geologists and geophysicists (N = 64) were classified by their favorable or critical assessment of the concept as expressed in scientific reports, journal articles and monographs written before 1966, a period of time just prior to widespread acceptance of the idea of a mobile crust. The overall and direct effects of (1) professional age, (2) professional stature, (3) institutional affiliation and (4) research interests on the probability of early support for horizontal movement of the earth's crust were measured using a logistic multiple regression model. Cumulatively these social structural variables were found to account for about one-third of observed differences in rates of early support. Specifically early supporters were drawn disproportionately from the ranks of older scientists, those employed at peripheral institutions and those specializing in paleomagnetic/geomagnetic research. The implications of these findings for general sociological propositions are discussed; in particular the unexpected association between older scientists and early support.

The Leadership View of American Science and Technology

Jon D. Miller, Director, Public Opinion Laboratory, Northern Illinois University, DeKalb, Illinois

This paper reports the results of a 1981 national survey of a sample of non-governmental leaders of American science and technology, the first systematic survey ever conducted for this group. On the basis of 287 interviews of approximately 30 minutes each, it appears that the leaders of organized science generally hold policy views similar to or compatible with the policy views of the attentive and general publics, but in some areas the leadership group displays a higher degree of consensus. The paper will outline the leadership views of the major science policy issues, including the balance between basic and applied research, the relationship of scientific research to economic growth, the relative priority accorded various disciplines, the value of the space program, and the risks and benefits of nuclear power.

Sociobiology in Science, 1975-1979: A Bibliometric Study

Jane Morley

A bibliometric study was generated in order to examine the literature citing Edward O. Wilson's 1975 book Sociobiology: The New Synthesis was indexed by the three ISI citation indexes. Twenty six (or four percent) of the total of 766 citing source items were published in the widely-circulated and highly prestigious journal Science, and these twenty six articles were chosen for in-depth citation and content analysis. The methodology and results are described, conclusions and implications discussed, and a preliminary assessment is made of the sociobiological "paradigm" as revealed in the articles that report original research.

Error Correction and Nuclear Reactor Safety

Joseph Morone, Technology Evaluation Operation

Decision makers faced with complex and uncertain problems usually rely on error correction. But error correction is inappropriate for reactor regulation, where the consequences of error are potentially severe. This paper analyzes how regulators have coped with this combination of complexity (which normally necessitates error correction) and potential for severe consequences (which makes error correction inappropriate).

Reactor regulation assumes that errors in design, construction and operation are inevitable and that therefore, reactors must be designed to prevent errors from triggering severe consequences. That is, regulators handle the combination of complexity and severe consequences by making reactors forgiving of errors. But when have sufficient precautions been taken to prevent errors from triggering serious consequences? When are reactors forgiving enough? This problem, highlighted at Three Mile Island, remains unresolved.

How Discovery is Important to Cognitive Studies of Science

Thomas Nickles, University of Nevada, Reno and Center for Philosophy of Science, University of Pittsburgh

The paper first cites recent philosophical claims concerning discovery and locates them within a longer history of changing methodological conceptions of the relation of discovery (generation) to justification. Next is an attempt to advance the discussion a further step by examining the extent to which generation and justification are dependent or "coupled." Several standard assumptions about science suggest some degree of coupling. Then the importance of a treatment of generation to an adequate theory of scientific inquiry is shown. Finally, and more briefly, sociological and psychological reasons for including generation in cognitive studies of science are examined.

Social Organization and the Manufacture of Statistical Products

H. Gil Peach, New York University

This paper explores the influence of organizational process and interest in the production of applied statistical products. The theoretical approach employs Barry Barnes's emphasis on actual practice, Habermas's concept of cognitive interest, and Karl Marx's perception of work organization. The focus of the paper is on the tension and interpenetration of professional and organizational interests in the creation and acceptance of applied statistics.

Examples are characterized by high-technology: application of formal statistical techniques, computer support, and applied organizational context and are taken from (1) government service (health, housing), (2) private sector (power company), (3) voluntary interest group (public interest research group), and from a private foundation which employs itself in studies to improve the quality and productivity of urban public services.

Theory Testing in Science — The Case of Solar Neutrinos: Or Do Crucial Experiments Test Theories or Theorists?

Trevor Pinch, School of Humanities and Social Sciences, University of Bath, England

This paper is written so as to be of interest to philosophers of science as well as historians and sociologists. It investigates how far the model of theory testing outlined by Karl Popper in the Logic of Scientific Discovery applies to a test of theory drawn from the recent history of physics. The test the paper focuses on is the attempt to measure the neutrino flux from the Sun - an experiment which has been claimed as a crucial test of nuclear astrophysical theory and in particular that the energy source of stars is nuclear fusion. It is argued that Popper's model and a close variant of it are incapable of accounting for this episode. A radical reappraisal of the role of logic in science is called for. It is suggested that a sociology of knowledge will prove to be more successful than a logic of discovery in explaining such episodes.

The Three-Sigma Enigma: On the Negotiation of Error in Physics

T. J. Pinch, Bath University

This paper takes as its reference the well-known discrepancy in physics between theory and experiment which is usually termed the 'solar-neutrino problem'. This problem consists of the mismatch between the detailed predictions of nuclear-astrophysical theory of the flux of neutrinos emitted by the Sun and experimental measurements of this flux. The exact magnitude of the discrepancy between theory and experiment has been subject to dispute. Broadly there are two positions: there are those scientists who argue that the magnitude of the discrepancy is such that it points to the solar-neutrino problem being a major outstanding unsolved puzzle, on the other hand there are those scientists who argue that the discrepancy is a minor problem which may eventually go away. These competing views depend on different interpretations of how theoretical and experimental uncertainties in physics are to be assessed. The various arguments are illustrated with the aid of interview material and it is shown what event that would appear to be a narrow technical statistical issue can be seen to be a product of social negotiation.

Development of Mexican Science

M. Rees

L. Lomnitz

This research aims at describing the evolution of science in Mexico. We hypothesize that as Mexican science consolidates, it develops a national scientific community. This is measured by a relative number of national vs. international citations in publications by Mexican scientists in the Institute of Biomedical Investigation of the National University of Mexico (UNAM). This project presents a network analysis of the bibliographic references in publications by these scientists over a 25 year period. It is further based on ethnographic data of the institution and co-author network analysis.

Incorporation of Medical Innovation: Acupuncture

Dorothy B. Rosenberg, West Virginia University

This paper examines social processes involved in incorporating an innovation into on-going professional work while these processes are occurring. It presents a framework for investigating the response to innovations when they are new and before it is known whether they will be accepted, rejected or ignored by the population to which they become available.

Acupuncture, an innovation in U.S. medicine in the 1970s, provides the empirical basis for this study. Data were collected from 220 physicians attending medical meetings devoted to acupuncture in 1974 by means of self-administered questionnaires, and follow-up mail surveys were conducted in 1974 and 1975.

On the basis of their current use and their plans to use acupuncture, a typology is constructed that represents three stages of incorporation, Pre-decision, Planning, Using. The three groups (Undecideds, Planners, Users) are compared on four underlying dimensions: information-seeking, decision making, knowledge accumulation, social interaction. Factors that implement or impede the acceptance of acupuncture are discussed. Longitudinal data are used to explore the dynamics of the incorporation process.

Fraud and Negligence in Science

Warren Schmaus

Fraud in science has been held to be in violation of a special norm of disinterestedness unique to the scientific profession. In this paper I question Harriet Zuckerman's reasons for regarding this as a moral norm, and then raise the general question as to whether there are any ethical principles specific to scientists. Using social contract theory, I argue that scientific fraud instead represents a violation of a general moral rule which requires everyone to fulfill his or her job or role related responsibilities honestly. In the case of scientists, this general moral rule entails that they meet their obligations as scientists to maintain the highest standards of intellectual rigor.

Negligent or careless scientists are then arguably in violation of the same moral rule as deceitful scientists. Funding agencies are thus encouraged to debar sloppy scientists as well as those guilty of fraud from further research grants. To do so, however, requires having standards regarding what a reasonable and prudent scientist would or would not do. It is suggested that such standards be developed through thinking about the obligations of scientists in terms of their responsibilities as authors. In cases of multiple authorship, a scientist may be found negligent when a co-author has been shown to be guilty of fraud.

Curves of Scientific Literature, References and Citations: A Mathematical Model of Dynamic Interrelation

Thomas Schøtt, Columbia University

A mathematical equation is established interrelating literature-curve, reference-curves and citation-curves, these being the curve of annual literature growth, curves of age distributions of references in articles, and curves of annual frequencies of citations papers received.

Taking previously observed regularities as assumptions, the theoretical implications of the model are derived. In particular it is shown that exponential literature growth and comparatively faster exponential decrease of the average reference-curve implies that the average citation-curve will decline exponentially. The model thus integrates hitherto separate empirical findings and predicts a new observable regularity.

It may also be useful for theorizing about cognitive development, in particular phenomena like obsolescence and obliteration by incorporation. Furthermore, the model can be applied to evaluation of quality of scientific research over time.

Fraud and Deception: Professional and Ethical Dilemmas

Thomas A. Shannon, Department of Humanities, Worcester Polytechnic Institute, Worcester, MA

Instances of fraud, including data faking, are being reported more frequently in the literature and to federal agencies. Deception continues to be used as a method of research into human behavior. This paper examines the moral dilemmas of these problems and considers professional and social responses to them.

"Tactics in the Debate about Cerebral Localization, 1870-1906"

Susan Leigh Star, Tremont Research Institute

Research on the brain expanded widely at the end of the nineteenth century. New surgical techniques, the development of electrophysiological measures for brain activity, and new physiological and anatomical techniques all coalesced to make new kinds of questions possible.

A key aspect of this research was the relationship between the brain and behavior. What kinds of behavior is the brain responsible for? Is there a division of labor in this responsibility? Can we talk about regions of the brain which will be responsible for certain functions? Does this extend to the cerebral cortex, or is it limited to "lower" brain "centers?"

While the answers to these questions are still hotly debated, they emerged during this time period as a debate between two camps: localizationists and diffusionists. In Western Europe and the United States, this debate was primarily carried on by surgeons and doctor-physiologists. Localizationists held that discrete areas of the brain were responsible for single functions, and they ultimately won the debate in the West. Diffusionists held that function could not be localized in one or a few spots in the brain, but rather that functions arise from a complex interaction of forces and processes.

This paper examines the tactics used in the debate about brain localization. The areas covered in the paper include: use of basic research and clinical case material; reconstruction tactics with regard to historical material; the construction of localization as an "inevitable discovery"; ad hominem arguments; idealism and materialism in review of problems; representation of anomalies, artifacts and complications by both sides.

Performance Inequality in Scientific Disciplines

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Regularities in frequency distributions of individual scientific performance are different for major disciplines and specialities. The analysis is based on a 13-year empirical study of several thousand research populations selected from institutional, national and international communities. Suitability of existing probabilistic models and inequality measures is examined against a wide variety of stratification circumstances and long-term time changes.

"From Gene Ecology to Experimental Taxonomy: A Chapter in the Relationship Between Ecology and Taxonomy"

Rachel Volberg, Tremont Research Institute

Since its development at the turn of the century, ecology has existed in uneasy relationship with taxonomy. Ecologists and taxonomists both use and reject one another's work. The basis for this simultaneous competition and dependence seems to be the attempt by both lines of work to construct a classification system for use by the wider biological arena.

Taxonomy and ecology focus on opposite sides of the boundary between the organism and its environment. They are bound to one another by the common problem which they address. But the development of different fundamental units of analysis reflects the different concerns of these lines of work.

This paper examines a striking example of the complex relationship between taxonomy and ecology. A series of reciprocal-transplant and uniform-garden experiments, designed to test the influence of genetic and environmental factors on the evolution of species, were begun by ecologists in the early 1920s. Following the ecologists' lead, a number of taxonomists undertook similar work. By the 1940s, taxonomists had pre-empted this line of experimentation and were scolding ecologists for not following their lead.

Sex Differences in Perceptions of Science and Scientists

John Wilkes, Center for Applied Social Science, Boston University

Gabriel Haim, Center for Applied Social Science, Boston University

Arlene McCormack, Center for Applied Social Science, Boston University

The focus of this paper is on the perception of science by a very important constituency, prospective scientists presently in training at a number of well-known university programs in the Northeastern United States. Their images of science, field by field and as a whole, will affect career decisions and public perceptions because they represent knowledgeable sources of information but still have fairly broad ties from pre-professional days.

Though the emphasis of this report will be descriptive, as it represents work in progress, the study from which it is drawn is intended to test a controversial theory that is the subject of a previous paper. Briefly, the main tenet of our position paper was that cultural stereotypes and cognitive styles interact to have a dramatic and long-lasting effect on the recruitment and career lines of women scientists. Specifically, we posited that the cognitive abilities of the women in science are distributed differently and less optimally across fields than those of the men. (The men's distribution bears a rough relationship to the dominant demands of fields in different stages of development.) By extension, one would expect relatively large proportions of women to find certain less popular scientific roles and the complex research environments represented by less developed fields more attractive than the bulk of their male counterparts do. Just why most of the men and most of the women in the same field might be cognitively mismatched takes one into the theoretical dynamics explored in the earlier paper. However, at base, we posit that a cognitive style associated with the development of a given research style among scientists increases the likelihood that a woman would challenge the prevailing views about a scientific career by embarking on such a career while another cognitive style would reduce the likelihood that a woman would take this course. The result is a disproportionate emphasis on certain cognitive abilities at the expense of others on the part of prospective women scientists. The particular pattern we predict would lead women scientists to be more attuned to other intellectual women (and men) in the humanities than the men in their fields. This would complicate relations with their male colleagues inasmuch as the opposite cognitive style predominates among the men who, as a whole, would tend to think and see things (of a scientific as well as a non-scientific nature) quite differently from the women, but this would have nothing to do with the sex difference per se.

The present data base includes 58 in-depth interviews and 250 questionnaires gathered from men and women presently in graduate programs at one of the better known departments of physics, chemistry, economics or sociology in the Northeastern U.S. Each has been randomly selected to appear in a quota sample, but the quotas approach universal representation for some groups. Data collection is not complete at this time. Ultimately we anticipate having a sample of over 1,000 respondents on the questionnaire portion of the study drawn equally from three cohorts: one group at the point of selecting undergraduate majors (late sophomores/early juniors), one group at the point of undergraduate graduation in a major (which would represent a tentative commitment to a field), and the third group with one to four years of graduate training in their field (representing a serious commitment to the field). The

present data set represents only the first wave of data collection on the last of these three cohorts, so our findings are necessarily of a preliminary nature with regard to the overall hypothesis.

At this stage of analysis what is of greatest interest is to assess the diversity of perceptions of scientific fields and scientists by field, sex, and possibly cognitive style. Later we will interact this material with a richly developed section of the survey instrument centering on self-images and shifting meanings of common terms such as "creativity" in the different subcultures represented by the various fields under study. For now, differing perceptions of the sciences, differing advice that would be offered to different types of students and differing career aspirations by sex and field will be the focus of inquiry. In the absence of the data on the younger students, which is still being collected at this time, the image of science to emerge from these data is primarily that of (proto-) scientists looking at other fields from the vantage point of their own. It is not strictly an informed "public" image of science, such as the undergraduate sample would represent. What one has is an unusually well-informed and critical audience that is in the process of making personal commitments that will shape these fields in the next period. We feel that their perceptions are of particular interest for this reason.

Error Correction in the Regulation of Toxic Chemicals

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Because scientific advice on toxic chemical problems seldom clarifies all technical issues and never answers the many transscientific questions, public policy making necessarily proceeds in part through trial and error. But timely correction of errors requires that damage is not irreversible, that the results of errors are displayed promptly, that observers interpret the negative feedback correctly, and that the political process reacts appropriately. All of these requirements are problematic for toxic chemicals: entropic dispersion of some persistent chemicals is essentially irreversible; long lags before feedback are typical; comprehension of problems is inadequate even by experts; and powerful interests benefit from prolonging errors rather than correcting them.

This paper examines how policy making on toxic chemicals in the U.S. has adapted to these constraints, with primary attention to pesticides, heavy metals, and new chemical substances just entering commerce. The ultimate question, not fully answered here, is whether some policy problems violate the requirements of trial-and-error learning to such a degree that political systems necessarily are incompetent to handle them satisfactorily.

The Scientist as Sociologist: An Application of Attribution Theory to the Social Study of Science

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This paper reviews recent developments in attribution theory and assesses the prospects for its application in the social study of science. Although it owes its genesis mainly to work in experimental psychology, it is argued that attribution theory is especially appropriate to work in the social study of science which portrays the social character of explanation and argument as locally organized context-specific activities. Data from a recent participant observation study of a solid state physics laboratory are examined in the light of attribution theory. It is shown to be particularly useful in understanding the significance for scientific work of scientists' selection and portrayal of social causes of events and actions.

Decision Making and Error Correction: A Theoretical Perspective

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Errors are one of the central problems of theories of choice. Theories of choice generally assume that human decision-making reflect an evaluation of alternatives in terms of anticipative consequences for stable, well defined, and exogenous objectives and that most of what policy making is about can be characterized as making rational choices. Within such a framework, decision processes derive their importance from the choices they generate, information as valued in proportion to the contribution it makes to resolving decision uncertainties, and actions are evaluated on the basis of extent to which they implement prior choices. Our observation of individual and organizational decision making seem to be contrary to such a characterization. Decision making problems are affected by delayed feedback, costly and irreversible consequences. Thus raise a host of problems related to errors in error correction. Policies related to risk technologies and scientific experiments often raise such problems.

This paper is a theoretical expectation of social science series of error and error corrections, and attempts to draw some major theoretical propositions from number of social science fields such as organizational theories, macro-political theory, social psychology and other applied social sciences. A set of theoretical propositions of error corrections will be explored.

Research Careers: Specialization and Change

John Ziman, University of Bristol

Scientists are highly differentiated by their 'specialities'. Yet the advance of knowledge, personal considerations or managerial authority may put them under pressure to change their field of research in mid-career. How do they respond to such pressures? What conditions of education and employment make them more or less versatile, and more or less adaptable?

These questions were discussed informally with very experienced science managers, and with groups of mature scientists (i.e., around age 40) in various British research institutions, including academic, research council, governmental and industrial organizations. This field material has not yet been analyzed in detail, but a number of significant points have become evident.

Education, up to Ph.D. level, does not fix specialities for life. Many scientists move a considerable distance within or beyond their disciplines during their research careers and do not define themselves as closely as the Invisible Colleges to which they contribute.

A mature researcher usually has command of a diversity of skills and interests, which were acquired voluntarily and informally over the years. These skills provide open channels for movement from problem to problem and from field to field.

Experienced scientists take much less time than they anticipated to 'get somewhere' with a novel problem - perhaps because they have acquired a general 'problem-solving' skill. They seldom need formal retraining but are helped by informal contacts and communication of tacit knowledge in exchange visits, conferences, etc.

Individual adaptability may not be simply a decreasing function of age: it depends upon early (i.e., postdoctoral) experience of change, and upon the local institutional environment. The most serious problems arise when a whole institution is forced to increase the tempo of its research programs to take on shorter-term, more specific projects.

Managerial practices relating to promotion, career development, administrative responsibility, institutional organization etc., can have a fundamental influence on individual response to change.

This project is still in an exploratory phase, but is already indicating that the motives, gratifications, and capabilities of mature research workers should be considered significant factors in the social study of science.

ANNOUNCEMENTS

THE CHARLES BABBAGE INSTITUTE. FOR THE HISTORY OF INFORMATION PROCESSING FELLOWSHIP 1983-1984.

The Charles Babbage Institute for the History of Information Processing is accepting applications for a Graduate Fellowship to be awarded for the 1983-1984 academic year to a graduate student whose dissertation will be on some aspect of the history of computers and information processing. Appropriate thesis topics might be concerned with aspects of the history of the information processing industry and its infrastructure. These topics can address specific technological developments in the information sciences, including both hardware and software, especially if they also deal with the economic and organizational milieu of the developments, or with the economic legal or social history of computing.

Residence can be at the home academic institute, other research facility where there are archival materials, the Babbage Institute, or some combination of these. There are no restrictions on the location of the academic institution which will be the venue for the Fellowship. The stipend will be \$5,000 plus an amount up to \$2,500 for tuition, fees, travel, and other research expenses. Priority will be given to students who have completed all course work and have completed all requirements for the doctoral degree except the research and writing of the dissertation. However, even incoming graduate students will be considered. The Fellowship may be extended for a period of one to three years if the Selection Committee concludes such support is appropriate.

Applications should be sent to the Charles Babbage Institute, University of Minnesota, 104 Walter Library, 117 Pleasant Street S.E., Minneapolis, Minnesota 55455, U.S.A. by January 15, 1983. Applications should include biographical data and a research plan or design. Applicants should arrange for three letters of reference, certified transcripts of college credits and GRE scores (or their equivalents abroad) to be sent directly to the Institute.

WORKSHOP REPORT

The Center for the Study of Science in Society at Virginia Tech sponsored a workshop on "The Demarcation between Science and Pseudo-Science" from April 30 to May 2, 1982.

The workshop began with a debate between philosophers about the current status of philosophical demarcation criteria. Larry Laudan (Pittsburgh and Virginia Tech) argued that philosophers were misdirecting their energy by attempting to demarcate science and non-science, and that a better focus for their efforts would be the problem of what constituted well-founded knowledge. This, he stressed, had been the traditional aim of philosophers, and one that had become lost sight of in the late nineteenth century. Adolf Grunbaum (Pittsburgh) retorted that it was premature to announce the demise of the demarcation problem, pointing out that we have no reason not to believe that one can yet be developed, and reminding the audience of the pressing practical needs such a criterion would serve.

The second session was devoted to a couple of historical case studies of episodes at the margins of normal science. Mary Jo Nye (Oklahoma and the Institute for Advanced Studies, Princeton) traced the fortunes of investigations with X-rays, black light and N-rays around the turn of the twentieth century, stressing the observational problems that haunted the investigations. Rachel Laudan (Virginia Tech) claimed that recent descriptions of continental drift as marginal or pseudo-science were mistaken, and went on to suggest that we need a much more sophisticated set of categories -- such as entertainment, pursuit, and acceptance -- in order to understand scientists' changing attitudes toward continental drift.

Attention then turned to anomalous phenomena. Henry Bauer (Virginia Tech) surveyed scientists' reactions to Velikovsky and to the Loch Ness monster hunters, indicating how simplistic the demarcation criteria involved were in most cases. He followed up by suggesting a mixture of social and cognitive tests for demarcating science and pseudo-science. Ron Westrum (Eastern Michigan) gave a lively account of a day in the life of a UFO-ologist, and used this to point out the practical difficulties of carrying out serious investigations of anomalous phenomena. This led naturally into the next session, led by sociologists. Tom Gieryn (Indiana) analyzed the social uses which demarcation criteria serve, stressing that scientists emphasize different characteristics of science depending on the activity from which they wish to distance themselves. Karin Knorr (Virginia Tech) recounted how Heidegger, Habermas, and Garfinkel attempted to set up demarcation criteria.

The final session dealt with parapsychology. Seymour Mauskopf (Duke) outlined the fortunes of the attempt to produce experimental evidence for parapsychological phenomena during the twentieth century, paying particular attention to the difficulties involved in replicating the results. I. J. Good (Virginia Tech) presented a series of provocative suggestions about the possible causes of parapsychological events, and ways in which these might be tested. The workshop

ended with a brief concluding session. There was general agreement that the search for an all-purpose demarcation criterion between science and pseudo-science was unlikely to succeed, and that further analysis of the different purposes a demarcation criterion might serve was needed. Some reservations were expressed about the consequent tendency to argue that a demarcation criterion was working but a rhetorical device, since it seems unlikely that scientists could fool all of the people all of the time if there were not some substantial cognitive advantages in scientific procedures. All in all, however, the workshop proved an environment conducive to mutual interaction between scientists, historians, philosophers and social scientists in the attack on a shared concern.

RENSSELAER POLYTECHNIC INSTITUTE. The School of Humanities and Social Sciences at Rensselaer Polytechnic Institute announces the formation of a Science and Technology Studies Division. This Division is an outgrowth of and replaces the Center for the Study of the Human Dimensions of Science and Technology, established in 1974. The Center's M.S. Program in Science, Technology, and Values will continue to be offered in the STS Division. The Division will also offer the M.S. degree in Public Archaeology, formerly granted by the Department of Anthropology and Sociology. The STS Division will continue to reflect the Center's commitment to values as integral components of deliberations on, studies of, and education in science and technology. Members of the Division are currently developing curricula in science and technology policy at the undergraduate and graduate levels. Shirley Gorenstein has been appointed Director of the Division; David Ellison will be Acting Director for 1982-1983. Effective September 1, 1982, Abbe Mowshowitz will join the RPI faculty as Research Director and Professor of Science and Technology Studies. The other members of the Division are Hetty Jo Brumbach, Linnda Caporael, P. Thomas Carroll, Richard Conviser, Deborah Johnson, Duane Orlowski, Sal Restivo, David Starbuck, John Schumacher, Edward Woodhouse, Richard Worthington, and Michael Zenzen. Information about the Division can be obtained by writing to Professor David Ellison, Science and Technology Studies Division, Rensselaer Polytechnic Institute, Troy, New York, 12181.

BOOK REVIEWS

- Jack Morrell and Arnold Thackray, Gentlemen of Science: Early Years of the British Association for the Advancement of Science. Oxford: Clarendon Press, 1981, xxiii + 592 pp.

This book may well win some awards. It is a major scholarly achievement. One might have predicted that it would be. Both Morrell and Thackray already have published notable research on the social context of nineteenth-century British science, and an ideal subject for such a study is the British Association for the Advancement of Science -- an organization created in 1831 which met annually in different cities, bringing together men and women of different backgrounds, different geographical areas, different degrees of eminence, and different interests in science.

A principal feature of the book is the strong foundation of unpublished evidence on which it is based. It stands to reason that many men planning annual meetings over a couple of decades would have generated an enormous correspondence. It is to Morrell and Thackray's considerable credit that they have tracked down some 5,000 relevant letters, and they have used them generously. They have arranged for the separate publication of 300 of the most important ones.

Events threw the main responsibility for organizing the BAAS's first meeting in York into the, as it turned out, politically capable hands of William Vernon Harcourt, son of the Archbishop of York, Oxford graduate, clergyman, and organizing force behind the new Yorkshire Philosophical Society. A perceived decline in British science had promoted bitterness as men of science leveled blame at one another and at existing institutions. Charles Babbage attacked the Royal Society of London, and hostility flourished between Oxbridge and the Scot, David Brewster. The many-faceted Harcourt managed to transcend such bickerings to establish a quickly successful association. In 1831 he not only brought together large numbers of his fellow provincials and a fair number of men of science for the first meeting in York, but also elicited commitments from key men of science for a second meeting which took place the next year at Oxford. Traveling first to university cities to consolidate university support and next to several industrial cities, the BAAS attracted thousands of participants each year and accumulated large sums of money. It became a "cultural resource," which could then be used for various purposes.

Long-term characteristics of the BAAS emerged from the initial meetings. Harcourt's talent in steering safely through troubled waters persisted in the BAAS's continuing concern to avoid extraneous controversy. Moreover, the "Gentlemen of Science" in London and in the universities took control. Members of provincial scientific societies, responsible for much of the first meeting's success, had slipped deferentially into subordinate roles by the second and third meetings. Harcourt recognized that the BAAS could only succeed if university and metropolitan scientists were in charge, and these men wanted to be in charge. Especially important were William Whewell and others from Cambridge who, for example, in taking control, replaced Harcourt's Baconian version of empiricism with a more mathematical Newtonian version as the dominant methodological view within the BAAS. The methodology strengthened the BAAS's commitment

to a Newtonian hierarchy of sciences with mechanics and astronomy at the top.

The Gentlemen of Science tended to be liberal Anglican in religion and Whig or Peelite Conservative in politics. They brought their broad and moderate views to their reform of science. Establishing the BAAS was moderate reform. Morrell and Thackray convey their interpretation of that reform in chapters entitled "The Ideologies of Science," "The Politics of Science," "The Utilities of Science," and "Making Knowledge." By emphasizing the objectivity and limitations of natural knowledge, the Gentlemen of Science sought to avoid political and religious controversy, thereby bringing as many people with as many views as possible under their umbrella of an organization. Though avoiding political controversy, they nevertheless had to be politicians within the BAAS and to become involved with national politicians. They could only control the BAAS's destiny if they were, in fact, in charge, and they practiced an appreciable amount of power politics as they turned the BAAS into a parliament of science with themselves as the cabinet. From that position of strength, they could use their "cultural resource" to give financial support for research they wanted done and to lobby the London Parliament for the causes of science. The BAAS had other "utilities." It provided a forum for publicizing the drawbacks of British science compared to that of other European countries. Local people and organizations hosting BAAS meetings used them to promote their local causes or to further their own careers. The BAAS helped "make" knowledge by providing a ready forum for men from different regions and different views and, unlike most other scientific societies of the day, by encouraging debate of the issues. They may have tried to avoid political and religious controversy, but they fostered heated discussion of, for example, competing theories of light and different views of geological strata.

The centerpiece of Morrell and Thackray's interpretation of the BAAS lies in the chapter "Ideologies of Science." They seem to be claiming that modern scientists regard scientific knowledge as objective and value free because of events during the early years of the BAAS. The leaders of the BAAS invented and used the ideology of objective science to ensure the success of their organization. In turn, the BAAS's overwhelming success guaranteed that the socially determined ideology would become the natural way of conceptualizing scientific knowledge.

Defining, demarcating, and delimiting science in ways satisfactory to its audiences, its patrons, its performers, supporters, and hangers-on was essential if the BAAS was to succeed. Only if science were separated from politics and theology could members of opposing social or religious groups unite for its advancement. Only if science were linked to progress and cut off from the controversial inquiries of statisticians and phrenologists would its appeal be authoritative and clear. Only if science were rendered attractive to various constituencies would it serve as an instrument of social expression and social integration. The demonstration of science as a popular, benign, peaceful, and desirable resource was necessary if the Gentlemen of Science were to claim the political fruits they coveted.

... The ideological categories into which natural knowledge was then cast remain familiar today (p. 224).

This may be a hard thesis to sell and, in some quarters, probably impossible. From the evidence presented, it would seem possible that the Gentlemen of Science were tapping into a set of ideas widespread among men of different political and religious views, ideas already established independent of the BAAS. On that interpretation, men of different persuasions would have been joining the BAAS not because the Gentlemen of Science succeeded in concealing controversial matters involving science, but because they agreed with the Gentlemen of Science that natural knowledge, for the greatest part, did not bear directly on political and religious controversies and that, even when it did, the scientific side of issues could usually be discussed separately from the political or religious.

That view would not deny that the problem of the objectivity or autonomy of scientific (or historical) knowledge is more thorny than many believe. Nor would it deny that the men who led the BAAS constituted within early Victorian culture a particular faction, defined in social, economic, religious, and political terms. Nor would it deny the possibility that scientific objectivity is, in fact, a socially determined notion. It would just question this particular theory of its social determination. If the Gentlemen of Science were expressing views already shared by others, then perhaps one could say that the BAAS was an intellectually determined social entity.

Though some may dissent from a few of Morrell and Thackray's broadest conclusions, all should agree that they have deeply penetrated the social context of Victorian science. With their massive evidence, they thoroughly explore the intricate societal matrix which surrounded and influenced the BAAS. Clearly, in many ways, religion, politics, love of truth, and love of power were all important in shaping the BAAS's early years. Morrell and Thackray's narrative and their interpretation of it will receive, and deserve to receive, wide attention.

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Donald A. MacKenzie. Statistics in Britain, 1865-1930: The Social Construction of a Virile Science. University of Edinburgh Press. Edinburgh, 1981.

If there is any life left in the "Weissstorch theory of knowledge," this book will help to extinguish it. The stork didn't bring us the Student's t-test, it was a product of Gossett's studies of "the behavior of beer" in a Guinness brewery. But statistics is not merely a social product; it is, MacKenzie argues, like all knowledge constitutively social. It is perhaps too extreme to conclude on this view that the tetrachoric correlation coefficient, for example, is racist and elitist because it served eugenic interests. But Pearson's correlation coefficients did reflect his view of correlation as a measure of "the strength of heredity" and was used as a tool for evolutionary and eugenic prediction. Earlier, Galton had developed regression and correlation in connection with his eugenic program. (Galton referred to eugenics as a "virile creed;" the intimate relationship between statistics and eugenics documented by MacKenzie prompts me to label statistics a "virile science." It is not incidental that the Eugenics Review regularly endorsed conventional notions of masculinity and femininity.)

The "error theorists" who preceded Galton were not, MacKenzie persuasively contends, "on the verge" of a simultaneous discovery. The major reason is that the error theorists and Galton had different goals and different views of variability. MacKenzie's view is that statistics are tools; and while a tool's construction will reflect the tasks it was designed for, the tool is not always limited to those tasks. Thus "our statistics" and "Pearson's statistics" must be evaluated in terms of their own contexts of construction, development, and use. More generally, MacKenzie views science as an activity of invention, oriented to the general goal of improving the human capacity for prediction and control. The pursuit of particular goals is sustained by social interests located either within science or the wider society. Societal interests can reflect the overall (e.g., class) or fine (e.g., social institutions) structure of society, or the relative influences of overall and fine structures.

MacKenzie places the lives and works of Galton, Pearson, and R. A. Fisher in their social contexts, and analyzes them as key factors in the development of statistics as a scientific specialty. This development conforms to the Griffith-Mullins network model, a model, incidentally, that MacKenzie suggests was anticipated by V. I. Lenin in his theory of the "party" and revolutionary change. Fisher's work, according to MacKenzie, extended the contributions of Galton and Pearson, his bitter disputes with Pearson notwithstanding. Fisher's biological research was clearly tied to his eugenic concerns. But the close relationship between statistics and eugenics evident in the works of Galton and Pearson is not found in Fisher's case. His "metastatistics" focused on refining the practices and theories of the earlier generation of statisticians. MacKenzie, however, does not make the organizational basis of Fisher's metastatistics explicit enough. "Metastatistics" was just what we would expect

from someone working in a field that had become a viable specialty with problems bequeathed directly from one professional generation to the next. The strengthening of that organizational imperative underlies Neyman's argument that "confidence limits" were more rigorous than Fisher's "fiducialism," a controversy that takes us beyond the 1930 boundary of MacKenzie's story. Fisher's work, of course, was not entirely divorced from extra-statistical concerns. MacKenzie notes that it was closely tied to agricultural research; it also reflected a new role for statisticians in scientific research (especially in the areas of agricultural and industrial production) that had begun to emerge in the 1920s.

According to MacKenzie, studies of science and ideology abated in reaction to the Nazis' "Aryan physics" and the Stalinists' "proletarian biology." He is intent on resurrecting such inquiries, but not in order to argue that modern science is "bourgeois," or that statistical theory is "fascist." Instead, he concentrates on how social interests influenced the form, content, and development of statistical theory. His general thesis is that advocacy of and (less frequently) opposition to eugenics motivated statistical work and affected the content of statistical knowledge during the period in which statistics developed out of Galton's work and Pearson's institution-building into a scientific specialty. Eugenics was one manifestation of the social interests of a rising professional middle class in late nineteenth-century British capitalist society. Eugenists argued that their policies (which stopped short of the "lethal chamber" but included "segregation") were based on scientific naturalism, and more specifically on Darwinian evolutionary theory. MacKenzie contends that in fact the eugenists "read" class structure onto nature. (It is ironic that Darwin, who is called on to do battle in "monkey trials" and to vanquish Creationists, was linked to the surrogate religion of eugenics (see below) by blood and argument. He gave inheritance the edge over environment in individual development; Galton was his cousin; and his son, Major Leonard Darwin, was President of the Eugenic Education Society from 1911 to 1929.)

Much of post-Mertonian sociology of science has a neo-functionalist slant that ignores or obscures conflict, elitism, and issues of power and social control. MacKenzie, however, places conflict and power at the center of his story: biometricians led by Pearson versus Mendelians led by Bateson; Pearson versus Yule; Pearson versus Fisher; ideas used as weapons within science and in the relationships between science and society; scientists seeking dominance over other scientists; and scientists pitted against other professionals in an attempt to establish science as "the sole arbiter of rational belief." On the latter issue, I wonder that MacKenzie didn't pay more attention to the role of the British Association for the Advancement of Science, which supported some eugenics research and stressed "scientific method" in its public relations efforts on behalf of science and scientists. This would have, for one thing, rooted Pearson's emphasis on method and the internal struggles among statisticians over methods in an important organizational context.

The institution Galton and Pearson constructed became inevitably dominated by what MacKenzie, following Becker, refers to as "side bets," that is, other interests besides eugenics. MacKenzie's story has its own side bets: (1) reading his account of the eugenics movement in England prompts one to wonder about Nazism as an extreme version of a civilizational racism instead of a Teutonic aberration; (2) professionalism was so closely tied to eugenics that it seems reasonable to conjecture that it was an alternative eugenic strategy,

a social genetics for the controlled selection, socialization, reproduction, and nourishment of a "perfected" elite. A third side bet, the relationship between statistics and religion, is especially intriguing. MacKenzie shows that the micropolitics of the biometrician-Mendelian controversy paralleled the decline of religion. Increasingly, intellectuals defended their social and political views on the authority of Nature, not of God. The opposing factions led by Pearson and Bateson constructed different biologies, and used them to defend different social arrangements. I view this as more a matter of a translation in religious space than one of the decline of religion. MacKenzie himself suggests a translation by noting that eugenics was a surrogate religion for Galton and Pearson. Nature replaced God - but as a new god; as Gauss said (quoting from King Lear), "Thou, nature, are my goddess, to thy laws my services are bound." As early as 1738, DeMoivre attempted to free probability from its association with gambling, and to establish its theological relevance in a doctrine of a "divine order" exhibited in "the regularity of statistical ratios." MacKenzie in fact discusses "social Newtonianism" and the framework it provided for much of the development of probability theory in eighteenth-century England; Bayesianism, for example, has its origins in the quest by the Reverend Thomas Bayes "to confirm the argument taken from final causes for the existence of the Deity." (Pearson exhibited an awareness (if not necessarily a self-awareness) of the theological connection when he observed that statistics were a religion for Florence Nightingale; "the Passionate Statistician," as she was sometimes referred to, had been J. J. Sylvester's most distinguished mathematics student in the 1850s.)

This book is a significant contribution to the constructivist perspective in social studies of science. MacKenzie clarifies, if he does not entirely resolve or dispose of, the problem of understanding knowledge as "constitutively social." Implicitly, he also provides resources for a psychological theory that treats knowledge as "constitutively affective." He uses sociological theory to penetrate about as close to the social heart of mathematical knowledge as anyone has managed to get (albeit in a case that is not so recalcitrant when compared to Greek geometry or Cantor's invention of transfinite numbers). No doubt, critics will interpret his effort as another example of the poverty of interest theory and of explanatory sociology in general. Without in any way devaluing such criticisms, I would argue for the primacy of concern for self-criticism among interest theorists and constructivists. Let me close with some puzzles for a self-critical theorist who likes to sing with MacKenzie:

It's interests and not truth that make science grow,
We do invent and not discover what we know; And even
for statistics it's the case, that origins are never
pure but base.

I do wonder why MacKenzie waffles between determinism and voluntarism (while generally striving, rhetorically, to give voluntarism the edge), and (less emphatically) between description as pure and as inevitably theoretical. He defends bold conjectures, but he throws up so many qualifications that he transcends due caution and distracts himself and his reader from precisely the sort of daredevil theorizing we need if we are going to develop a critical instead of a worshipful understanding of science. And is there, after all, a role for truth and causality in this inquiry (and its constructivist and relativist relations) hidden by a soft determinism and a surrogate causal rhetoric?

Finally, it is important that those of us who affirm MacKenzie's perspective and conclusions, as well as those who oppose them, consider whether and how MacKenzie's interpretation of the social construction of statistics has been influenced by his evaluation of statistical methods. This is a study, remember, that eschews the individualist, empiricist, statistical approach and adopts a sociology of knowledge perspective influenced by Lukacs and Goldmann. Following MacKenzie's lead, we can expect to solve some of these puzzles by examining his use of rhetorical resources to persuade readers (especially those readers who are not experts in statistics and who make up an important part of the intended audience for this book) about his viewpoints. And we can trace the interests that lie at the root of why he finds it "useful" to consider certain conjectures "reasonable."

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June Goodfield. Reflections on Science and the Media. Washington, D.C.: American Association for the Advancement of Science, 1981, 113 pgs., \$9.00.

The mass media may be the public's major source of information about science. Yet examinations of that media role have been sparse. Few researchers have delved into the area. And still more rare are the books giving us some perspective on the issues involved in the public understanding of science via the mass media.

That's why June Goodfield's book, Reflections on Science and the Media, is such a welcome addition. Essentially "commissioned" by the American Association for the Advancement of Science, this slim volume attempts to address three major elements of the communication process: (1) the roles and obligations of scientists as sources; (2) the proper role of journalists who cover science; and (3) the "duty" of the public to become informed about science.

Goodfield certainly has the credentials to write about science communication. Trained initially in zoology, with a doctorate in the history and philosophy of science from the University of Leeds, Goodfield is both a scientist and a communicator. Among her numerous books are Playing God: Genetic Engineering and the Manipulation of Life and, most recently, An Imagined World: A Story of Scientific Discovery. She respects both science and journalism, and that lends credibility to her criticisms of both fields.

The book makes no pretense of being comprehensive. It is not designed to offer the latest in research findings within science communication. It has no intention of telling you "how to" become a science journalist or "how to" increase your chances of being more accurately quoted as a source. Instead, as Goodfield notes, she was charged with the responsibility of writing "a critical essay about the mutual and often uneasy relationship between two distinguished professions (pg. ix)."

The resulting essay, while brief, is indeed thought-provoking. It is divided into two main sections. In the first part, Goodfield addresses the three actors in the communication process -- scientists, journalists, audiences -- and discusses factors that affect each actor's participation in the process itself. Then, in the second part, the author introduces four case studies as illustrations of points made earlier.

While I found the entire essay to be literate and readable, I do feel that it falls short in two ways. First, it gives uneven attention to the three actors, concentrating primarily on the journalists to the detriment of the other two. And secondly, its emphasis on the case studies (the majority of the text is devoted to them) is at the expense of more in-depth discussions of the really substantive topics raised in the first part of the essay.

Let me address the "unevenness" criticism first. Early in the text, the author notes that she will examine sources, journalists and certain aspects of the audience. But the resulting essay really addresses primarily the media, with somewhat lesser attention paid to scientists as sources and very short shrift given to audiences. Certainly Goodfield fulfills her stated goal of dealing with the "duty" of audiences to become informed about science. But the very basic question of who attends to media science materials is a critical one, since the whole media enterprise is based on the assumption that someone, somewhere is ingesting the material for some functional reason. Goodfield devotes only a paragraph or two to an identification of the audiences for science communication (pp. 8-9), and that identification is overly simplistic.

In the first part of her essay, Goodfield does deal with factors that can affect scientists' interactions with the mass media. And her comments are illuminating. But the case studies, with one exception (the Asilomar conference), really tell us very little about sources and what motivates them to do what they do in varying circumstances. Rather, the case studies are used primarily to examine the performance of the media. Thus, while examination of the role of scientists is a primary goal of the essay, I don't think that role has received the attention it deserves.

The case studies do make for lively reading. Goodfield offers four of them in this essay: media coverage of the "painted mouse controversy" involving a researcher at Memorial Sloan-Kettering Cancer Center in the mid 1970s; coverage of the now-famous Asilomar conference during the same period; media treatment of the publication of David Rorvik's book The Cloning of a Man in 1978; and the coverage (or attempts at coverage) by the British media of the thalidomide disaster and the resulting court cases during the 1960s and 1970s.

But as interesting as they are, I think the case studies divert us from the most important part of the book: Goodfield's discussion of the kinds of constraints under which both scientists and journalists operate when they interact with one another. It is here that the author had an opportunity to break new ground, to make a significant contribution to our understanding of the news production process. But the chapter, while touching on a number of important constraints, does not go into the kind of depth necessary to make that contribution. Nor does this chapter make any reference to some of the important new research by sociologists such as Gaye Tuchman¹ and Herbert Gans,² who have written about the routinizing elements of news work that influence information selection. None of these researchers has examined science reporting per se, but their findings tell us a great deal about the daily functioning of media organizations, within which science reporters necessarily must work.

Empirical investigation of the kinds of constraints under which scientists are operating when they interact with reporters is just getting underway.³ But again, Goodfield is brief, offering a few tidbits (such as the suggestion in pages 31-32 that involvement in the public communication of science is not rewarded within the social system of science) but generally not venturing into new territory. Aspects of the social system of science may have a profound effect on how scientists perceive the media and on their behaviors when communicating with journalists. But the brevity of Goodfield's treatment prevents her from providing some real insight into the problem.

That said, I must reiterate that I like the Goodfield book for what it is: a brief, provocative essay that raises more questions than it answers. To be brief and provocative was Goodfield's intent, I believe. So my criticisms of the volume are directed more to the initial goals of the work rather than to how well the stated goals were realized.

And within her essay, Goodfield makes a number of excellent points. She urges both scientists and journalists to learn more about the processes (and subsequent constraints) governing the other and to utilize that knowledge to facilitate interactions rather than to avoid them. She calls for more advocacy reporting of science by the media; such reporting usually makes sources quite uncomfortable and is almost nonexistent in the major U.S. media. And at the end of her essay, Goodfield remains realistically unsure about the ability of the mass media to adequately inform anyone about scientific and technological issues. The question of how well the media can provide the kinds of information needed for informed public decisions is an important one, but it remains essentially unanswered in our society.

References

1. Gaye Tuchman. Making News (New York: The Free Press, 1978).
2. Herbert J. Gans. Deciding What's News (New York: Vintage Books, 1979).
3. Sharon Dunwoody and Byron T. Scott. "Scientists as Mass Media Sources," Journalism Quarterly 59: 52-59, Spring 1982. An older study that also offers insights is Roy E. Carter, Jr., "Newspaper 'Gatekeepers' and the Sources of News," Public Opinion Quarterly 22: 133-144, Summer 1958.

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Everett Mendelsohn and Yehuda Elkana (eds.). Sciences and Cultures. Sociology of the Sciences, Vol 5, (1981), D. Reidel, Dordrecht, Boston and London, pp. xvii and 270.

One of the strengths of social studies of science is its interdisciplinary character. Philosophers, historians, sociologists and anthropologists all contribute, and the dialogue is not always that of the deaf. The present volume exemplifies this cross-disciplinary tradition and, in particular, the application of anthropological methods to historical data. Given the backgrounds and the interests of the contributors it is inevitably very diverse. This is not a criticism. Indeed, it is a strength. It does, however, pose problems for the reviewer who wishes to discuss the volume qua volume. I shall accordingly consider the articles individually, and reserve a few general comments until the end.

The middle part of the book is a fine series of essays in which theoretical interests and empirical material are deftly interwoven. I shall return to these in a moment. At the beginning and end there are two essays that are, one presumes, supposed to provide a framework for the whole. To start with there is Yehuda Elkana's magisterial and expressly programmatic survey of the prospects for an anthropology of knowledge. And at the end, there is Wolf Lepenies "sociology of sociology and anthropology." To my mind both of these essays misfire. Lepenies' essay, which is rather slight, muses on recent advances in the sociology and anthropology of knowledge, and makes some rather impressionistic suggestions about the social factors conducive to such developments. I can see little merit in the essay, least of all in the context of the present volume.

Elkana's paper also raises problems, though it does at least address the relationship between science and culture, and raises serious points of substance. His basic thesis is that culture as a whole, including science, has to be understood as a hermeneutic circle -- in the West science is the most important part of that circle. From this he draws a number of conclusions. The first is that both science and social studies of science adopt (or should adopt) "think" description -- there is a multiplicity of complex cultural structures which influence and (analytically) muddy the distinction between observation and theory. Second, the complexity of the hermeneutic circle may be broken into for the purposes of analysis by treating scientific change as a function of three factors: (a) a body of knowledge; (b) socially determined "images of knowledge;" and (c) values and norms included in ideology but not directly dependent on these images of knowledge. The third is that there are no basic differences, though many differences of emphasis, between science and the rest of culture. Elkana's aim is to offer some new tools for analysis (for instance the three levels of the hermeneutic circle mentioned above) and he claims that he has solved a number of problems (for example the nature of the relationship between science and non-science). However, despite these claims, I judge his article

to have misfired. On the one hand, I don't think that it is as novel as he seems to think it is. Surely an "interpretative" approach to science and its context is by now fairly widespread. On the other hand (and this is the more serious criticism), my belief is that it is argued in insufficient detail to persuade anyone who is not already converted. Thus, instead of concentrating on a single topic in depth, he chooses to fire in all directions simultaneously, and accordingly misfires.

Although the essays of Lepenies and Elkana fail for lack of a definite object, the same is not generally true for the other, more empirical contributions. Peter Wright's paper is an attack on Whig conceptions of science and, in particular, on the distinction between internal and external factors which he detects as reemerging in the work of such writers as Barnes. Science and its boundaries with non-science are very much contingent social constructions. Noting that it is often argued that "science" (the term is anachronistic in this context) emerged in Seventeenth Century England, he shows that its institutionalization was a function of contingent social factors, and in particular the attempts of Royalists and moderate Parliamentarians to secure a future for the "Royal" Society after the Restoration. This is elegantly done and would certainly require serious consideration by any remaining Whig historians. My only reservation attaches to his reading of Barnes which I believe to be unsympathetic -- but this is a minor matter.

A similar story, retold as a more or less unadorned historical narrative, is offered by Peter Buck. He writes about the emergence of science as a social force in post 1911 China where it was seen, at least initially, as a safe agent for national social cohesion and a change in a fissile society subject to external imperialist intervention. What the early Chinese scientists and studies did not at first realize was that the science made available to them, primarily by the United States, was (a) very particular in conception, and (b) directed by a series of quite explicit (though not necessarily simultaneously held) imperialist views of the proper position of China and its relations with the United States. Buck mentions, in particular, U.S. concerns to prevent Boxer-like internal rebellions, to promote morality as a counterweight to the effect of industrially generated individualism, and to build up links with future Chinese leaders by means of scholarships. Overall, Buck's piece satisfactorily complements that of Wright and together they argue persuasively that "science" should be seen as a cultural or social creation, not a naturally occurring category.

The papers of Caneva, Kleinman and the Goods enter into the content of scientific culture itself. Caneva's analysis of the responses to an anomaly -- the discovery of electromagnetism by Oersted -- uses Mary Douglas' notion of grid and group to good effect. High grid, high group German scientists such as Schweiger and Muncke adopted a policy of "monster adjusting" -- of fitting the anomaly into a new place in existing theory. French scientists, identified as low grid and high group, turned out, as the theory suggests, to be "monster barriers" -- they rejected the anomaly out of hand -- except for the isolated Ampere (high grid, low group) who embraced the theory by generating radical new theory about it, a "monster embracer." Finally, a later generation of Germans in a changed low grid, low group social structure, assimilated the monster by reworking theory

in a comprehensive manner ("monster assimilation"). Caneva's essay is stimulating, as he himself agrees, for raising as many questions as it answers. In particular there is the question of the measurement of grid and group in differentiated societies where different parts of social structure vary in their characteristics, even for the same individual as he/she goes through daily life. This is not a question that can be resolved a priori. Rather it requires further empirical investigation of the kind that has been attempted in a new series of articles edited by Mary Douglas under the title The Sociology of Perception (Routledge, 1982). However, one thing is, certain. Caneva's work, along with that of the Bloors, shows that Mary Douglas' notions of grid and group cannot be dismissed, as some anthropologists have suggested, as amusing but ultimately inconsequential.

Kleinman and the Goods discuss medical meanings and the way in which these are generated as a result of social and cultural negotiations, interests and preferences. Kleinman's paper, which takes the form of a rather unassimilable high speed review and justification of a "meaning-centered approach" to medical phenomena will not be discussed here. The piece by Bryon and Mary Jo del Vecchio Good is more leisurely, more empirical and thoroughly consistent with that of Kleinman. Their argument is that illness realities are semantic, being produced and reproduced discursively in the course of social and political encounters. The biomedical model of illness, though in and of itself just another hermeneutic reality, attempts to deny the holistic nature of sickness by abstracting and interpreting certain features in isolation from the patient and his/her meanings. Finally, they suggest that semantic network analysis -- an attempt to relate and thereby give meaning to the cultural sickness terms used in a given context -- might provide an appropriate method for the study of illness-realities. Their argument is illustrated by two fieldwork cases, one from Iran and the other of an Iranian student in the United States, in which negotiations and contradictions between Western and Iranian illness realities are exemplified. This is a most attractive paper, is thoroughly consistent with interpretive history of science, but offers the promise of a new method -- that of semantic network mapping. I look forward to reading more of their work.

There is one other paper, that of Robert Anderson, on the necessity of fieldwork studies of laboratories. Though I was looking forward to this -- I attempt laboratory ethnography myself -- I must confess that I found it disappointing. Yes, they are necessary. Yes, they offer us a chance to understand the relationship between broader social factors and work in the laboratory. But like Elkana, Anderson does not attempt to drive any particular point home. It is another rather broad survey -- and although Anderson is obviously sitting on a large amount of interesting data, he uses it here to rather little effect.

Overall, then, the papers are diverse, directed by different concerns and on different subject-matters. This, as I noted earlier, is not a complaint. In diversity there is strength. What, if any, unity is there behind this diversity? Other than the obvious sciences and cultures concern, each reader will detect his or her own favorite theme. For me that theme is the importance of social interests for the growth and direction of scientific knowledge. Put in this way, this is scarcely a discovery. But turned around it raises some crucial problems. If social interests are central, then what is their status? How are they imputed? Authors such as Wright and Buck, painting on a broad historical canvas, locate them in class or economic concerns, and they do it convincingly. Caneva takes a description

of social structure very much for granted, and is concerned, as I have noted, with the way in which interests are advanced in certain types of structure. Anderson's image of interest is more ad hoc, institutional and personal as well as economic, and the Goods' is largely implicit, at least outside the domain of the personal. One doesn't have to be an advocate of a general theory of interests to note that there is diversity and a fair degree of unselfconsciousness in the way in which they are imputed. Perhaps more thought about their nature and imputation would be desirable. In particular it would be useful to know how personal motives and concerns are seen as relating (if indeed they do) to social structural categories. Another related but very important question concerns the stability of interests. Do the authors take these to be relatively fixed or not? If not, then how do they change? If fixed, then why and how are they stabilized? Again, I am in no position to legislate an answer. But with studies in the culture of science of the empirical sophistication that we now have, an attendant theoretical focus on structure and interests might pay dividends.

Overall this is a good book, and many of the empirical pieces are excellent. Mendelsohn and Elkana are to be congratulated for the collection.

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Alfred Kelly. The Descent of Darwin: The Popularization of Darwinism in Germany, 1860-1914. 1981. University of North Carolina Press: Chapel Hill, 185 pp., \$18.50.

D. F. Bratchell. The Impact of Darwinism: Texts and Commentary Illustrating Nineteenth-Century Religious, Scientific and Literary Attitudes. 1981. Humanities Press: Atlantic Highlands, N. J., 140 pp., \$9.95.

Kelly has written a scholarly work about the popularization of Darwinism in Germany between the publication of the Origin and the First World War. Although popular Darwinism was slow to get started in Germany, rising literacy and falling publication costs enabled popular Darwinism to "ride to crest of the great reading wave that swept through all social classes" (p. 23). Initially, only the middle classes participated in this movement, but by the 1880's they were joined by a small but respectable readership among the working classes. Several of the popularizers of Darwinism were themselves professional scientists, e.g., Ernst Haeckel, Ludwig Büchner, and Carl Vogt, but the most successful popularizer of all was Wilhelm Bölsche. In his panegyrics to nature, everything shed light on everything else.

At the outset Kelly is confronted by two methodological problems -- what is to count as a "popularization" and what as "Darwinism." Kelly finds the first cut surprisingly easy to make. The distinction between popular and professional works was fairly sharp at the time. Popularizations all but wore their identity on their sleeve. The second difficulty proved to be much more formidable. Darwinism was all things to all people. Kelly remarks on the "infinite malleability of Darwinism" (p. 44) as if this were a characteristic peculiar to popular Darwinism, but the spectrum of views exhibited in the professional Darwinian literature was hardly less varied. A fact of life with which intellectual historians must live is the heterogeneity and multiplicity of ideas that can cluster under the same label. In attempting to set out this range of diversity, the historian should be careful not to be taken in by word magic. The appearance of the name "Darwin" in the term "Darwinism" is liable to imply little more about the meaning of this term than does the appearance of "meteor" in "meteorology." In Germany at least, Darwin and his writings played only a minor role in the intellectual movement that bore his name. As Kelly describes it, Darwinism was roughly equivalent to the popular science tradition in Germany. But what about Darwin? Calls to put Darwin back in Darwinism are not only beside the point, they are sure to have about the same effect as repeated cries to put Christ back in Christmas.

With tedious regularity, commentators on Darwinism remark that it arose in a competitive, semi-laissez faire society and that all Darwin did was to read the contingencies of his own society into the framework of the living world. Such claims sound plausible enough, but after reading the wide variety of sources for and implications of Darwinism that Kelly details, I have my doubts. People in the last part of the 19th century in Germany were as adept at picking and choosing what suited them from Darwinism as religious fundamentalists are in their selective reading of the Bible. Some viewed evolution as a progressive force that would lead to the overthrow of established governments; others viewed it as supporting the status quo. Some emphasized the competitiveness of evolution; others mutual aid. Some revolutionaries liked it because it implied that societies

could change through time, while others opposed it because it implied that social revolutions were unnecessary, and on and on:

Perhaps it is more accurate to speak of several popular Darwinisms than of one universal system of ideas. Almost anyone could and did appeal to Darwin's authority. Materialists, idealists, aristocrats, democrats, conservatives, liberals, and socialists, as well as protagonists of virtually every shade of religious opinion, all staked out their claims in Darwinian territory (p. 7).

Kelly's book has a lesson to teach intellectuals. Because ideas are so important to us and because we have some commitment to rational connections between ideas, we tend to read these two convictions into society as a whole, often with the effect of distorting intellectual history beyond recognition. Numerous authors of a variety of persuasions have recently voiced their fears about the implications of sociobiology for social policy. The assumption is that policy makers actually take the content of scientific theories seriously and are liable to put the implications of these theories into practice. I am afraid that I am a good deal more cynical than these authors. Until I see some evidence that such connections have characterized the past, I am not about to read them into the future. Certainly Kelly provides no evidence for these assumptions.

Kelly deals with five main themes, devoting a chapter to each -- the climax of popular Darwinism in Bölsche's erotic monism, the teaching of Darwinism in secondary schools, the conflict between organized religions and Darwinism, social Darwinism, and the connection between Darwinism and Marxism. Although few Germans today are liable to remember his name, Bölsche was the best selling writer of nonfiction in Germany prior to 1933. Although his early works were not especially popular, his three-volume Love-Life in Nature: The Story of the Evolution of Love (1898-1901) was a sensation. In these books, Bölsche proclaimed that sexual love "was the unifying principle of the universe, the engine of evolution." In Bölsche's hands, "Darwinism was changed from a tale of bitter struggle to an erotic monism or paneroticism, a lyrical celebration of love" (p. 42). According to Bölsche, the human body was nature's most exquisite work:

To Bölsche, every part of the body, even the anus, was worthy of admiration. He effusively praised the harmony and beauty of the male penis, mocking those who found it obscene. But his greatest encomia were saved for the female derriere: "Let us not forget," he said in all earnestness, "that the backside of woman belongs among the most alluring art forms of the entire cosmos." The female backside represented a grade ideal toward which the universe was evolving (p. 49).

And this at a time when biology textbooks totally omitted discussion of the reproductive system. One wonders what fun the critics of sociobiology might have had at the expense of their adversaries had they been aware of this German precursor.

The story of the teaching of Darwinism in German secondary schools shows remarkable parallels to the story in the United States of America fifty years later. To begin with, very little science of any kind was taught in German secondary schools and what there was was poor. Herman Müller, a biology teacher in the small Westphalian city of Lippstadt, was an exception. Not only was he a very successful teacher, but also he taught Darwinism. However, a newspaper took

exception to Müller's corrupting the loyal youth, and soon the politicians became involved. The controversy took several turns, but in each case, Müller tactfully turned aside official censure, promising to be more careful in the future. However, the politicians and popular press were unrelenting. They proclaimed that the school authorities would "bear the responsibility when there grows up within our fatherland a generation whose confessions are atheism and nihilism and whose political philosophy is communism" (p. 63). Christ was not an ape who died for the sins of other apes!

The end result of all this furor was the general mistaken impression that the government had prohibited the teaching of Darwinism in public schools. The controversy even resulted in a play by Max Dreyer in 1900 entitled "Der Probekandidat" (the probationary teacher). Almost nothing was taught about evolution in German secondary schools prior to the Müller affair; almost nothing was taught on the subject thereafter. The same story can be told for the famous monkey trial in the United States. Biology classes in our high schools remain to this day primarily exercises in cutting up worms -- at best. Certainly evolutionary theory is rarely discussed, but I suspect not because it is evolutionary theory but because it is a theory. Biology teachers take the evolution of species for granted, but do not explain much about the evolutionary process because they do not know very much about it.

Although it is currently fashionable to play down the conflict between religion and science that Darwinism brought to a head, Kelly is forced to tell the familiar story all over again. However, he contradicts popular wisdom when he turns to social Darwinism. As Bannister (1979) has shown in the United States, Kelly documents in German; there really was not much that one could call social Darwinism. The ranks of those who thought that Darwin held the key to understanding human societies "were actually rather small and their reading audience very limited" (p. 103). The great influence of social Darwinism was a bugaboo invented by later commentators. Yes, Darwinians were racists; so was everyone else at the time. Strangely enough, the issue of anti-Semitism was hardly ever raised. Yes, there was a Jewish problem, but for such Darwinians as Buckner, it was the same as the Christian problem -- how to do away with all religions (p. 118).

Kelly also challenges the common wisdom that Nazism had its roots in Darwinism. He finds no evidence for such claims. After all, when the Nazis did come to power, they proscribed the literature of popular Darwinism (p. 122). As Kelly sees it, viewing early German Darwinians as precursors to the Nazis is just another instance of "precursorities," an intellectual disease that is no longer socially acceptable among intellectual historians. Marxists are also liable not to like what Kelly has to say about the social consciousness of German workers at the time. There was none. They were much more interested in making more money than in socialism. In fact, what passed for Marxism among the workers at the time was actually Darwinism. The gripping question at the time was "Moses or Darwin?" not "Moses or Marx?" (p. 138).

By now, it should be apparent that I found Kelly's book full of thought-provoking information. For the readers of this publication, Kelly's greatest strength is his realization that he needs to do more than just read the literature of the period if he is to assess influence. "It is possible to analyze in detail the content of popular books, but to assess the effect of these books requires a leap of faith" (p. 6). He does the best he can,

ferreting out old surveys, comparing sales of books and readership in workers' libraries, and so on. When he can find it, the evidence is extremely helpful. It is one thing to be told that Bölsche was the most popular writer of non-fiction at the time. It is much more striking to be told that the combined sales of Bölsche's books by 1914 was 1.5 million copies, three times the circulation of Haeckel's works (p. 37). Instead of relying solely on intuitive responses, Kelly uncovers a questionnaire distributed to 5,000 workers in 1891 indicating only 5.9 percent could count as being "intellectuals" (p. 130). Even when one attempts to study the masses, one is forced to study the "elite" or nothing at all. One can go on only so long about the books that the majority of people did not read, the ideas they did not discuss, the events of which they were not aware.

My one complaint of Kelly's exposition is that it tends to be repetitive. He frequently spoils a later story by mentioning its outcome several times before he gets around to telling it. The one conclusion that he avoids telegraphing is the fate of popular Darwinism in Germany. The only thing more striking than the length of its success is its sudden and total demise. Popular Darwinism has completely vanished from the German scene. Kelly does not note, however, that Darwinism as a scientific movement remains. Could it be that old-fashioned philosophers of science have a point when they claim that science is to some extent cumulative and progressive?

There is much less to say about Bratchell's book. It is primarily a narrative interspersed with long quotations from primary sources. As representatives of Darwin's predecessors, Bratchell quotes from the works of Erasmus Darwin, Charles Lyell and Robert Chambers. After quoting from the Origin of Species, Bratchell chooses as representative commentators, T. H. Huxley, Samuel Wilberforce, Richard Owne, Archbishop Whateley, William Hopkins. The religious controversy and the reflections of this controversy in the fiction of the period are represented by quotations from the writings of Charles Kingsley, Alfred Lord Tennyson, Arthur Hugh Clough, John Henry Newman, George Meredith and Samuel Butler. A Darwin scholar is not likely to learn anything new from Bratchell's texts and commentaries, but the book could be used successfully in an undergraduate course on Darwinism.

Reference

Bannister, Robert C., 1979, *Social Darwinism: Science and Myth in Anglo-American Social Thought*, Temple University Press, Philadelphia.

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