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Table of Contents

SPECIAL SECTION: 1986 4S MEETING

Preliminary Program	2
Abstracts	7
Registration information	27

POLICY REPORT

Coordination of U.S. Research Programs: Executive and Congressional Roles Albert H. Teich	29
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NEWS AND VIEWS

The President's Page David Edge	37
In Memoriam: Joseph Ben-David Randall Collins	38
Conference Report: Who Needs Needham? John Merson	41
News Clips	43
Meetings Calendar	48
Information for Contributors	Inside Back Cover

Gian-Carlo Rota, MIT: *Prejudices About the History of Mathematics*.

Reuben Hersh, University of New Mexico: *How Some Mathematicians Have Made Some Discoveries*.

Sal Restivo, Department of Science and Technology Studies, Rensselaer Polytechnic Institute: *The Social and Material Foundations of Pure Mathematics*.

Discussant: **Martin Krieger**, University of Southern California

5. National Science Policies

Chair: **Roy MacLeod**, University of Sydney

P. Weingart, **R. Sehringer**, **M. Winterhager**, Science Studies Unit, University of Bielefeld: *Assessing National Strengths and Weaknesses in the Sciences*.

Loet Leydesdorff, Science Dynamics, University of Amsterdam: *On the Measurement of the Effectiveness of National Science Policies in Changing International Environments*.

Andrew Jamison, Research Policy Institute, University of Lund: *National Styles in Science and Technology: A Comparative Model*.

William B. Lacy, **Lawrence Busch**, **Jose de Souza Silva**, **Shripad Deo**, University of Kentucky: *Biotechnology, Agriculture, and Developing Countries: Promises and Contradictions*.

Maurice N. Richter, Jr., SUNY-Albany: *The Comparative Analysis of National Science Policies*.

Wouter Van Rossum, Science Studies Unit/Department of Social Policy, University of Groningen: *The Effects of Policies of Research Councils on Research Priority Fields: Three European Cases*.

6. Women in Science, 1789-1979, I

(Cosponsored by History of Science Society)

Chair: **Ruth Schwartz Cowan**, SUNY-Stony Brook

Dorinda Outram, Department of History, University College, Cork, Republic of Ireland: *Before Objectivity: Wives, Patronage and Cultural Reproduction in Early 19th Century France*.

Ann B. Shteir, York University, Ontario: *Botany in the Breakfast Room: Women, the Family and Plant Science in Early 19th Century England*.

Nancy Slack, Russell Sage College: *Nineteenth Century Women Botanists: Wives, Widows, and Work*.

Joy Harvey, Department of History of Science, Harvard University: *Strangers to Each Other: Male and Female Relationships in the Life and Work of Clemence Royer (1830-1902)*.

Discussant: **Pnina Abir Am**, Harvard University

Organizers: **Pnina Abir Am** and **Joy Harvey**, Harvard University

7. Theories of Language and Science

(Cosponsored by Philosophy of Science Association)

Chair: **Donald Idhe**, SUNY-Stony Brook

Donald T. Campbell, Lehigh University: *Models of Language Learning and their Implications for Social-Constructionist Analyses of Scientific Belief*.

Bruno Latour, Centre de Sociologie de l'Innovation: *A Relativistic Approach of Einstein's Relativity*.

Karin Knorr-Cetina, Universitat Bielefeld: *The Fixation of (Visual) Evidence*.

Discussants: **C. F. V. Quine**, Harvard

H. Collins, University of Bath

Organizer: **Donald Campbell**, Lehigh University

8. Society and Science

Chair: **Michael Sokal**, Worcester Polytechnic Institute

Robert P. Young, University of Texas at San Antonio: *Black Migration and the Decline of Racism in American Social Science, 1910-1940*.

Ubiratan D'Ambrosio, Universidade Estadual de Campinas, Brazil: *On Alternative Methodologies and the Institutionalization of Science*.

Discussant: **Jill G. Morawski**, Wesleyan University

1:00-3:00 pm

9. Science Indicators II

Chair: **Belver Griffith**, Drexel University

Arthur M. Diamond, Jr., Department of Economics, The Ohio State University: *The Empirical Progressiveness of the General Equilibrium Research Program: An Empirical Test*.

James A. Ruffner, Wayne State University: *Citation Rates in Parasitology: A Problem of Systematic Undervaluation*.

V. Cano, School of Library and Information Science, University of Western Ontario: *Publication and Citation Patterns in Mexican Science*.

Lowell Hargens, University of Illinois and **Howard Schuman**, University of Michigan: *Academic Scientists' Use and Evaluation of SCI/SSCI*.

2:30-4:30 pm

10. Women in Science, 1789-1979, II

(Cosponsored by History of Science Society)

Chair: **Joy Harvey**, Harvard University

Ann Hibner Koblitz, Wellesley College: *Careers and Home Life in the 1880s in Russia: The Choices of Sophia Kovalevskia*.

Peggy Aldrich Kidwell, Smithsonian Institution: *Astronomy in the Family: Cecilia Payne-Gaposchkin (1900-1979)*.

Jo Gladstone, Harvard University: *Public Careers and Private Lives: Progressive Age Influences on the Life and Career of Ethnographer Audrey I. Richards*.

Pnina G. Abir-Am, Department of the History of Science, Harvard University: *The Synergy of Disciplinary and Marital Trajectories in the Career of Mathematician-turned-Biologist, Dorothy M. Wrinch, 1894-1976*.

Commentators: **D. Outram**, University College, Cork

11. Communication of Risk

Chair: **Michael Simon**, SUNY-Stony Brook

June Fessenden-Raden and Janet Fitchen, Program on Science, Technology and Society, Cornell University: *Community Perspectives on the Communication of Risk*.

M. Granger Morgan, Department of Engineering and Public Policy, Carnegie Mellon University: *Technical and Psychological Problems in the Communication of Risk*.

Peter M. Sandman, Rutgers University: *The Role of the Media in the Communication of Risk*.

Michael Baram, School of Public Health, Boston University: *Rights, Duties and Dilemmas in Communications about Risk*.

Organizer: **Rachelle Hollander**, National Science Foundation

12. Science Policy and Science

Chair: **Spencer Weart**, American Institute of Physics

Edward J. Hackett, Department of Science and Technology Studies, Rensselaer Polytechnic Institute: *Effects of Funding Changes in the Life Sciences: Project Summary*.

Stephen Hilgartner, Department of Sociology, Cornell University: *Diet, Cancer and the Interpretation of Evidence: Disputes Over The Scientific Basis for Policy*.

H.M. Collins and G.D.L. Travis, Science Studies Centre, University of Bath: *Restricted Funding and the Structure of Research in British University Science Departments*.

Discussant: **Susan E. Cozzens**, Illinois Institute of Technology

13. Reflexivity and Science

Chair: **Augustine Brannigan**, Calgary University

Malcolm Ashmore, Department of Sociology, University of York (U.K.): *The Paradox of Application and its Reflexive Reemergence in its Analytical Presentation*.

Steve Fuller, Department of Philosophy, University of Colorado: *Anti-Reflexivity in the Natural and Social Sciences*.

Andrew McKinley, Psychology Department, St. Andrews University, Scotland: *Meaning, Interpretation and Reflexivity in the Social Sciences*.

Kay Oehler and Nicholas Mullins, Virginia Polytechnic Institute and State University: *Reflexivity in Science: Can It Exist?*

Graham Watson, Anthropology Department, University of Calgary: *Make Me Reflexive - But Not Yet*.

Steve Woolgar, Department of Human Resources, Brunel, The University of West London: *Reflexivity is the Ethnographer of the Text*.

Organizer: **Steve Woolgar**, Brunel, University of West London

Saturday, October 25

9:30-11:30 am

14. Changing Notions of Ownership in Science

Chair: **Susan Cozzens**, Illinois Institute of Technology

Vivian Weil, Center for the Study of Ethics in the Professions, Illinois Institute of Technology: *Policy Incentives and Constraints on Dissemination of Scientific and Technical Information*.

Robert F. Boruch, Department of Psychology, Northwestern University: *Mitigating Conflict Between Research and Enforcement: Technological Promise and Limits*.

S. J. Ceci and J. G. Baker, Department of Human Development, Cornell University: *Scientists' Responses to Disputes Over Access to Data*.

Daryl E. Chubin, Office of Technology Assessment: *Allocating Credit and Blame in Science*.

Discussant: **Harriet A. Zuckerman**, Columbia University

Organizer: **Rachelle Hollander**, National Science Foundation

15. Sociology of Technology I

(Cosponsored by Society for the History of Technology)

Chair: **Joel Tarr**, Carnegie Mellon University

Rachel Maines, School of Management, Clarkson University: *The Vibrator and its Predecessor Technologies, 1752-1930*.

Hans Radder, Free University of Amsterdam: *Experiment, Technology and the Intrinsic Connection between Knowledge and Power.*

Michael Mackenzie, Alberto Cambrosio, and Peter Keating, Universite de Montreal: *Third Sector Production of Technological Change.*

Navid Mohseni and William B. Lacy, University of Kentucky: *A Phenomenological Study of Biotechnology: Toward Development of a Theoretical Framework.*

16. Machine Intelligence

Chair: **Lucy A Suchman**, Xerox Palo Alto Research Center

Brian P. Bloomfield, Systems Group, The Open University, U.K.: *The Fifth Generation Computer: Genesis and Development of a Technological Artefact.*

Harry Collins, Science Studies Centre, University of Bath: *The Turing Test - A Sociological Approach.*

Steve Woolgar, Department of Human Sciences, Brunel, The University of West London: *Computer Software as Social Practice: Towards a Sociological Analysis of Copying and Compatibility.*

Organizer: **Steve Woolgar**, University of West London

9:30 am-12:00 noon

17. Sociology of the Biological Sciences

Chair: **Susan Sheets Pyenson**, Concordia University

Edward Manier, Program in the History and Philosophy of Science, University of Notre Dame: *Problems in the Development of Cognitive Neuroscience: Expository Strategies in "Inter-Specialist" and "Intra-Specialist" Texts.*

Philip Rehbock, University of Hawaii at Manoa: *Prosopographical Study of the Nineteenth Century British Naturalists.*

Gail A. Hornstein, Mount Holyoke College: *The Social Construction of Quantification in American Psychology, 1880-1930.*

Chunglin Kwa, Department of Science Dynamics, University of Amsterdam: *The Development of Systems Ecology in the United States.*

Jacqueline Cramer, Science Dynamics, University of Amsterdam, *Mission-Oriented in Ecology.*

Evelleen Richards, Department of History and Philosophy of Science, University of Wollongong: *Robert Knox and the "Resurrection Men" of the Anthropological Society of London.*

1:00-3:00 pm

18. Practice of Mathematics II

Chair: **Lewis Pyerson**, Universite de Montreal

Arthur Jaffe, Harvard University: *Ordering the Universe: The Role of Mathematics.*

Eric Livingston, Los Angeles, California: *The Empirical Object of Theoretical Physics.*

Jean Lave, University of California, Irvine: *Everyday Arithmetic and Advanced Mathematics.*

Martin H. Krieger, University of Southern California: *Adding Up The World: The Culture of the Calculus.*

Organizer: **Martin Krieger**, University of Southern California

19. Sociology of Technology II

Chair: **Roberta Balstad Miller**, National Science Foundation

Trevor Pinch, Department of Sociology, University of York: *Applied Science as Technology: The Case of Health Economics.*

Knut H. Sorensen, Department of Sociology, University of Trondheim: *Deciding Technology.*

Wiebe E. Bijker, Twente University of Technology, The Netherlands: *Tube Contra Bulb: The Social Construction of Fluorescent Lighting and the Role of Economic Power.*

Boelie Elzen, Twente University of Technology, The Netherlands: *The Failure of a Successful Artefact: How the Svedberg Ultracentrifuge Conquered the Scientific World and Also Did Not.*

2:00-4:30 pm

20. Pittsburgh and the Business of Industrial Science, 1886-1896: Studies in the History of Industrial Research

(Cosponsored with the Society for the History of Technology)

Chair: **Jeffrey L. Sturchio**, Center for the History of Chemistry

Ronald R. Kline, IEEE Center for History of Engineering: *Engineering R&D at Westinghouse 1886-1896.*

Bettye H. Pruitt, The Winthrop Group: *Alcoa Laboratories: Impetus and Direction in Incremental Innovation.*

Paul Tiffany, The Wharton School, University of Pennsylvania: *The Origins of Industrial Research in the American Steel Industry: The Development of U.S. Steel Corporation's Research and Technology Department, 1928.*

3:00-5:00 pm

21. Science and Public Policy

Chair: **Lawrence J. Rhoades**, National Institute of Mental Health

Jane C. Kronick (Sociology), and **Robert J. Dostal** (Philosophy), Bryn Mawr College: *Scientific Evidence and Public Policy Decisions*.

Lee Clarke, Sociology, UCLA: *Risk Analysis as Science and Policy*.

Aant Elzinga and **Sven Andersson**, Department of Theory and Science, University of Gothenburg: *Ideals of Science in the Humanities and Their Ethical and Policy Implications*.

David Collingridge, Technology Policy Unit, Aston University: *Why Science is No Use to Policy*.

Discussant: **Lawrence J. Rhoades**, National Institute of Mental Health

22. Medicine and Medical Policy

Chair: **Elihu Gerson**, Tremont Research Institute

Olga Amsterdamska, Department of Science Dynamics, University of Amsterdam: *Clinical Practice and Preclinical Research: The Influence of Diagnostic Laboratories on Research in Bacterial Variation, 1918-1939*.

Kathryn S. Arnow, Washington, D.C.: *The "Ethical Compact" Governing the Investigator-Subject Relationship in Human Experimentation*.

Anja Hiddinga, Department of Science Dynamics, University of Amsterdam: *The Context of Cognitive Change in a Research Programme in Obstetrics*.

Adele E. Clarke, Tremont Research Institute: *The Emergence of Reproductive Science in the U.S., 1910-1940: Conditions and Consequences*.

Sunday, October 26

9:30 - 11:30 am

23. Work Organization in Science and Technology

Chair: **Susan Leigh Starr**, Tremont Research Institute

Susan Leigh Starr, Tremont Research Institute, and **James R. Griesemer**, Department of Philosophy, University of California, Davis: *Linking Concepts with Work Organization: An Analysis of a Natural History Expedition and Its Ensuing Publications*.

Sjerp Zeldenrust, Department of Science Dynamics, University of Amsterdam: *Work Organization and the Research Process*.

Daniel Lee Kleinman, **Jack Kloppenborg, Jr.**, **Leslie Oakes**, **Suzanne Ogilby**, University of Wisconsin-Madison: *Scientific Labor and Monopoly Capital: Managing R&D in the Corporate Laboratory*.

1:00-3:00 pm

24. Scientific Institutions

Chair: To be announced

Charles Bazerman, National University of Singapore: *Literate Acts and the Emergent Social Structure of Science*.

Mario Biagioli, University of California, Berkeley: *Industrial Prizes at the End of the Ancien Regime*.

25. Social Influences on the Development of Scientific Disciplines

Chair: To be announced

Henry Etzkowitz, SUNY-Purchase and **Lois Peters**, New York University: *The Interdisciplinary Origins of Scientific Disciplines*.

Paul K. Hoch, Science Policy & Innovation Research Unit, University of Warwick, England: *Emigres and New Disciplines: The Case of the 1930s Refugee Scholars*.

Joan H. Fujimura, Tremont Research Institute: *Problem Paths: An Analytical Tool for the Social Construction of Scientific Problems*.

Randall Collins, Department of Sociology, University of California, Riverside: *The Sociology of Philosophy: Toward a Triple Level Theory*.

Abstracts

Thursday, October 23

7:30-10:00 pm

Session 1 Handling Scientific Controversies

How Not to Presume: Toward a Descriptive Theory of Ideology in Science and Technology Controversy. Philip T. Shepard, Michigan State University, and Christopher S. Hamlin, University of Notre Dame

Emboldened by Chubin and Restivo's call for science and technology studies (STS) relevant to policy controversy, the paper raises again the problem of ideology and social thought to argue that a descriptive sense of 'ideology' is better suited to organize and guide analysis. STS must take better account of the psycho-social dynamics of controversy if its intervention in policy controversy is not to prolong closure needlessly through further polarization. Indeed, to facilitate mutual understanding of conflicting advocacy frameworks is a pressing goal for STS intervention, which can be met with a descriptive theory of ideology without accepting scientism or acquiescing to distortion and domination. With a descriptive sense STS scholars can avoid pretentiousness, presumptuousness, begging questions, stigmatizing opponents, totalizing culture and society, and privileging either the critic's or the apologist's position, and still methodically treat common elements and complementary processes in establishmentarian and utopian thought in a way that illuminates ideological aspects of language use.

Claims and Resources: An Approach to Scientific and Technical Controversies. Gerald E. Markle, James C. Peterson, Department of Sociology, Western Michigan University
The study of science- and technology-related controversies has emerged as a distinct subfield within the social studies of science. Its further development will require that theoretical frameworks guide data collection and analysis more explicitly than has been true in many of the early case studies. In this paper we propose a framework for the analysis of scientific and technical controversies based on a synthesis of social constructionist and resource mobilization theories.

Social constructionist theory has already demonstrated its utility in studies of the production of scientific knowledge. Limited use of the approach has also been made in studying controversies, but its micro perspective and emphasis on language and interaction have diverted attention from the critical issues of power and political action.

The constructionist approach becomes much more powerful when combined with key elements of resource mobilization theory. Originally developed for the analysis of social movements, resource mobilization theory provides an organizational and political perspective that emphasizes the role of "entrepreneurs" in manufacturing grievances and forming movements.

The Scientific Rhetoric of Anti-Pornography. Michael Altimore, Department of Sociology, Mount Mary College
I propose to examine the scientific rhetoric used in the debate over pornography. It is now taken for granted that pornography causes violence against women and that there is a difficult choice to be made between freedom of expression and saving lives. I will analyze the evidence presented in scholarly journals for such a contention about the effect of pornography and discuss this evidence in the context of other factors in violent crime, including sex offenses, that are entirely obscured, most notably social class.

In this situation, as in the case of alcohol abuse (following Gusfield's important work), science functions as a powerful rhetoric to conceal ideological claims.

Rationality of Science and Social Controversy. Martin Eger, College of Staten Island, City University of New York
The main purpose of this paper is to show that the question of the rationality of science — central in philosophy — is not merely of "academic" interest: Today, it is also an issue at the center of nation-wide social controversies and local conflicts in education.

I examine the cognitive and social links between the creationist controversy and that concerning moral education — in light of contemporary philosophy of science. Apparently, the demarcation criterion relevant to social and educational conflicts is not primarily that between science and pseudo-science but that which deals with natural science and ethics.

Habermas' distinction between the empirical-analytic sciences and hermeneutic disciplines becomes relevant here, but not in its original formulation; and the application of such a distinction to present day concerns raises some serious questions about the educative role of science in society; that is, its role in shaping self-understanding and orientation in the world.

Session 2 Science Indicators I

Procedures for Representing Scientific Processes. Elihu M. Gerson, Tremont Research Institute

This paper presents a strategy for representing the organizational, institutional, and intellectual characteristics of research, based on recent work in database management and artificial intelligence research. The strategy hinges on postponing commitment to particular assumptions about relationships among aspects of science until they can be represented and tested with appropriate data. This means that data must be represented in ways which support many different kinds of analytical strategy, and which do not assign privileged positions to problematical epistemological or methodological assumptions.

Two such data representation techniques are proposed: (1) *links*, which carry information about relationships among people, research organizations, instruments, methods, journals, theories and other objects of interest and their properties; and (2) *timelines*, which carry information about sets of objects and their properties over time. These techniques offer several advantages. First, objects

and their properties can be constructed and reconstructed as the course of research requires. Second, the techniques are equally applicable to any data format, numerical or textual. Third, data can be retrieved and manipulated in whatever fashion, and by whatever procedure, seems most useful to an on-going analysis. Fourth, secondary analysis of multiple data sets is greatly facilitated. Finally, storage and maintenance of data are made efficient and (within limits) relatively independent of local hardware and software facilities. The two techniques are illustrated with an implementation for personal computers using a relational database management program. Data for the illustration are drawn from the California Biology Project, a study of biological research in California during the first half of the 20th century.

Obsolescence and Scientific Productivity. Sharon G. Levin, Department of Economics, University of Missouri, St. Louis, and Paula E. Stephan, Department of Economics, Georgia State University

This paper investigates the relationship between scientific productivity and obsolescence, having controlled for the effects of aging. The model developed posits that the arguments in the scientist's utility function are recognition, income and the satisfaction gained from solving the research puzzle. Arguments in the production function for research are human capital, effort, ability and resources. A scientist is said to have become obsolete if his human capital does not contain knowledge of scientific discoveries that have displaced previously held theories in the discipline. Scientists who have become obsolete will engage in less research because their research will be awarded less recognition and generate less income than research embodying currently held scientific truths.

Scientists can, of course, ward off the effects of obsolescence by retooling. The probability of becoming obsolete, however, is not the same for all Ph.D. vintages, where vintage refers to date of Ph.D. Specifically, within a field, there are some fine vintage years for getting a Ph.D. and some exceptionally poor years. A fine vintage occurs if no major change has taken place subsequent to receipt of the Ph.D. A poor vintage occurs if major changes have taken place since receipt of the Ph.D. Scientists from fine vintages are, therefore, at a lower risk of becoming obsolete than are scientists from poor vintages.

The model is tested using panel data from the Survey of the Doctorate. Article counts, adjusted for authorship and quality, are used as the measure of research productivity. The effects of obsolescence are considered by using dummy variables to represent whether or not the scientist was practicing at the time a major change occurred in the field. These variables have been constructed by surveying scientists. Subfields in physics and geology are considered.

A Method for Representing Intellectual Positions in Research Reports. James R. Griesemer, Department of Philosophy, University of California, Davis

A method is presented for representing the intellectual positions of individual scholars on theoretical issues. The method, called conceptual mapping, provides a way to compare positions across publications by a single scholar, across scholars within research programs and across research programs.

Conceptual mapping and a family of related representational techniques (such as co-word analysis) are compared in terms of the research requirements of the Northern California Biology Project being conducted at Tremont Research Institute. The core of these

methodologies is a set of rules for identifying conceptual structures which play a central role in problem formulation and expression. Each makes a number of assumptions about problem formulation. Conceptual mapping, for instance, assumes that novel problem formulations are presented as modifications of preexisting structures (drawn from "the literature") by means of simple problem generating heuristics. A set of problem generating heuristics is presented which may be useful for characterizing the structure of developing theories. A robustness analysis of the assumptions of several techniques is presented and the implications of the results for the utility of this family of methods assessed.

Using the Independent Imperfections of Theories of Science to Design Science Impact Indicators. William N. Dunn, Burkart Holzner, Muhammad Shahidullah, University of Pittsburgh

The National Science Board recently issued *Science Indicators: The 1985 Report*. This report, the seventh in the biennial *Science Indicators* series initiated in 1972, is similar to its predecessors insofar as it "aims to provide a broad base of quantitative information about the structure and function of American science and technology to inform national policy makers as they make judgments about how best to allocate resources to these activities" (National Science Board, 1985:ix). The 1985 report also differs from previous volumes insofar as it recognizes theoretical and methodological problems of providing quantitative information about elements of the science, engineering and technology system. In this context the 1985 report acknowledges that the "sheer description of each element in the system involves many problematic research issues — choice of alternative definitions, ranges of methodologies, and costs and benefits of different approaches. Even more critical are the problems of tracking and analyzing the interactions between the imperfectly measured elements — the dynamics of the system" (National Science Board, 1985:ix).

These and related issues surrounding science indicators ("scientometrics") have been addressed by economists, sociologists, historians, and philosophers of science and by contributors to science policy studies (e.g., Elkana et al., 1978; Machlup, 1980; MacRae, 1985). Typical of these issues are controversies surrounding the demarcation and measurement of science and technology (de Solla Price, 1978), the utilization and misutilization of science indicators by policy makers (Elkana, 1978; Campbell, 1985), the contributions of science to social goals or end-values (MacRae, 1985), and the role of alternative disciplines, theories, and models in investigating the science-society nexus (Thackray, 1978; Holton, 1978; Kochen, 1978; Nelson, 1981).

Session 3 The Social Context of Agricultural Research, 1840 - 1940

Session Abstract: The Social Context of Agricultural Research, 1840-1940. Gregory Sanford, Department of History, Iowa State University

The current state of American agriculture has led to expressions of concern about the role of agricultural research in the farming enterprise. Issues, such as the size and goals of the agricultural research establishment, its use of governmental funds, and the role of public and private institutions demonstrate that agricultural research has a social context. The resolution of those tensions will likely result

in a realignment of social and institutional elements and transform agricultural research itself. This is certainly not the first such shift, and we believe that historical perspective can enhance understanding of contemporary issues.

While the history of agricultural science remains relatively unexplored, historians have marked the contours of the field and framed problems for study. They have examined the Germanic origins of American agricultural experiment stations and studied efforts to organize an agricultural science discipline. Such studies have also recognized the conflicts and tensions which have shaped research institutions. The repeated creation of constituencies, formulation of research policy, and conduct of investigations were attempts to rationalize the disparate ideals and aspirations of farmers and scientists. The historical evolution of agricultural research has involved the institutional reconciliation of various conflicts and tensions seemingly inherent to the enterprise. The session we propose examines those issues over the long term. It sheds light on the social context at different times and upon the ordering of agricultural research. Such a long term view cannot but be helpful in forming an understanding of both the field of agricultural science and the current conditions of agriculture.

Reforming Research: American Perceptions of European Agricultural Institutions, 1840-1870. Gregory Sanford, Iowa State University

The transmission of European knowledge, technologies and institutions to America in the nineteenth century has proven an intriguing avenue for historical research. In the case of agricultural science, historians have documented the significance of Liebig's chemistry and the German model of agricultural research institutions to the formation of an American agricultural research establishment in the 1870s. Yet, Americans had been well informed about European agricultural institutions for thirty years. From the late 1830s, the agricultural press, agricultural societies and state legislatures manifested an awareness of the theoretical and experimental work conducted in England and on the continent. Nevertheless, European models seemed unable to find a constituency among the Americans. Focusing upon New York and Massachusetts, this paper assesses American perceptions of European agricultural institutions.

As part of the effort to improve agriculture between 1840 and 1870, American farm reformers scrutinized European forms of organization and science. Collecting the accounts of travellers, cultivating European correspondents, and commissioning inspectors, agricultural reformers assessed the utility of German, English, French, Russian, Swiss, and Belgian research establishments which included an appreciation of the science, field experimentation and lab work conducted in those institutions. At the same time reform agriculturalists debated the merits of European institutions. Invariably they objected to any direct transfer. Rather, they sought to pick and choose among institutional functions they judged best suited to American conditions. The conclusion argues that certain assumptions about the nature of improvement shaped American perceptions of European institutions. That is, American reformers in New York and Massachusetts evaluated European research efforts by the standard of improvement. Yet that standard reflected commonly held notions about what the farmer ought to be and how to effect his reform. For example, agricultural reformers found the processes of research or observation, experimentation and reflection as significant to their mission as the conclusions drawn from research. Consequently, from the reform perspective, Americans ascertained the strengths and limitations of European research programs. The paper suggests that the agricultural

reformers' notions about improvement were hardly unique and were shared by a wide variety of Jacksonian reformers.

An Awareness of Scientific Community: Wilbur O. Atwater and the Study of Nutrition in the United States. Matthew Rock McCright, Marshall County Historical Society

Co-founder of the first United States Agricultural Experiment Station, first Director of the Office of Experiment Stations at the U.S. Department of Agriculture, identified as one of the earliest investigators to "prove" that certain leguminous plants collected and "fixed" nitrogen from the atmosphere, Wilbur Olin Atwater has been recognized as an influential historical figure. Yet, much of Atwater's work has received relatively little attention. Throughout his varied career, the scientist, academic, and federal bureaucrat carried out extensive investigations in the field of human nutrition.

During more than 30 years of activity in the field, Atwater brought together the research and services of a network of associates from all parts of the United States, and put together large chemical and physical laboratories including some of the most sophisticated apparatus available. An investigation into the interaction between Atwater and a widely dispersed collection of college and university instructors, chemists, physicians, social workers, and others provides insight into what holds such a group together. Focusing on Atwater's extensive manuscripts and publications, as well as numerous state, federal, and commercial publications, this study demonstrates how Atwater's laboratory served as an effective clearinghouse for local and regional nutritional studies around the nation. This was possible because of a shared "understanding" that scientific problems ought to be approached from a particular point of view, from a common perception concerning the study of complex wholes.

Experimentalism and Reform: Genetics in Agricultural Research, 1900-1920. Barbara A. Kimmelman, University of Pennsylvania

Historians of science addressing the rise of experimental biology from c. 1880 have focused almost exclusively on zoological researchers trained and employed at major research universities. But botanical research was itself increasingly experimental at this time; and if we examine the social context of experimental botanical work in the United States, we must acknowledge the vast institutional apparatus assembled by the federal government for agricultural research. Agricultural research was intensely interventionist and explicitly experimental at the turn of the century. Seed agents employed by the USDA travelled worldwide to bring back seeds to be cultivated under special conditions in the United States, or to be crossbred by horticulturalists, botanists, and plant physiologists at the USDA and state agricultural colleges and experiment stations. The emphasis on scientific research, part of a general movement to reform agricultural life and production, called for new disease resistant, better-yield varieties and new culture methods. Many agricultural researchers, with this production-oriented research imperative, greatly valued interventionist experimental techniques.

Mendelism is a special case that can be addressed within the larger context of agriculture and Progressive reform. The conception of Mendelism crystallized as the science of genetic improvement, associated equally with plant and animal improvement and the genetic improvement of humankind. Mendelism could thus contribute to, and benefit from, both the technical and social aspects of the agricultural reform movement. I will discuss the extraordinarily positive response to Mendel on the part

of American agriculturalists and the rapid institutionalization of Mendelian work at agricultural research centers, a process that produced national associations, a widely-distributed genetic curriculum, undergraduate and graduate genetics clubs, published research papers, and three new research departments devoted exclusively to genetic work by 1915. I argue that the extraordinary success of genetics in the United States must be seen as partly the product of the enthusiasm and strengths of the agricultural reform movement.

Agricultural Science and the Politics of Research: The Case of Hybrid Corn. Deborah Fitzgerald, Harvard University
In the annals of American agriculture, the development of more efficient technologies and more productive seeds and grains are considered as the ultimate and inevitable results of enlightened scientific research programs at the land-grant colleges and large agricultural firms. One of the most applauded innovations was hybrid corn, the product of intense research in the 1910s and 1920s. When it was commercially introduced in the 1930s, its advantages over traditional open-pollinated corn were cast by promoters as self-evident: its potential yield per acre was more than twice that for open-pollinates, and it was often bred for resistance to numerous pests and diseases. Yet there were disadvantages as well. Hybrids carried these traits for only one season, obliging farmers to purchase new seed each year, a practice that ran counter to both conventional wisdom and practical experience. Second, hybrids were temperamental, delivering their high yields only under very particular field conditions: a hybrid developed in central Illinois, for instance, would often perform less well than traditional open-pollinates in southern Illinois. Third, this physiological sensitivity paved the way for the introduction of other agricultural "necessities," most notably chemical agents, which were perceived by farmers as vital components of a farming program that was designed to nurture the centerpiece crop, hybrid corn.

In this paper I will argue that hybrid corn, and the attendant operating procedures it entails, was not the inevitable result of corn improvement research, but that other options were considered and, ultimately, rejected. For example, shortly after Donald Jones discovered a way to make hybrids commercially viable to commercial producers, breeders at Minnesota and Nebraska land-grant colleges developed corn strains called "synthetics" which, while yielding a bit less than some hybrids, allowed farmers to use the same seed for three or four years. Synthetics were something of a compromise between hybrids and open-pollinates. Yet despite continued research on synthetics in America, and at least one brief consideration for their use in developing countries, they have not been produced commercially. Another alternative to the contemporary hybrid corn arrangement, first suggested by land-grant breeders, was to provide farmers with the single-cross seed for a particular hybrid combination, letting the farmer do the crossing and seed increase on his own farm, an effort that would reduce the seed cost. While this idea gained even less currency than synthetics, it again reflects the alternatives that breeders faced in devising strategies for corn improvements.

My object in this paper, then, is to explore a few of the "roads not taken," attempting to discern both the scientific and social constraints on corn research and development, and the rationale for choosing one alternative over another. Hopefully this discussion will be instructive to not only historians of agricultural science, but also those historians interested in problem and development choices scientists make in other fields.

Friday, October 24

9:30-11:30 am

Session 4 Practice of Mathematics I

Prejudices About the History of Mathematics. Gian-Carlo Rota, MIT

Historians of mathematics have often tacitly assumed that the history of mathematics can be presented in much the same way as the history of civilization. We shall exhibit some examples to show that this is not the case. More precisely, the history of mathematics presents greater discontinuities than the history of other scientific subjects.

How Some Mathematicians Have Made Some Discoveries. Reuben Hersh, University of New Mexico

A few anecdotes, from the personal experiences of the speaker and others, will illustrate some of the ways that mathematical discoveries have sometimes been made. An effort will be made to show how these anecdotes are linked to a certain style and philosophy associated with Richard Courant and his "school," meaning some of his colleagues in pre-Hitler Göttingen and some of his students in post-war New York.

The Social and Material Foundations of Pure Mathematics. Sal Restivo, Department of Science and Technology Studies, Rensselaer Polytechnic Institute

Sociological principles, formulated within a broadly conceived cultural and historical materialism, are applied to the study of pure mathematics as a social product and social construct. The guiding premises of my approach are: (1) references to mental events, activities, and processes are references to social phenomena; and (2) as a social activity becomes more specialized, and more autonomous in relation to other social activities, it becomes increasingly focused on its own products as objects and tools of social production and reproduction. The "internal" social structure and material reality of the mathematical community are determinants of mathematical knowledge, as much so as "external" social and material realities. Instead of "pure" logical and mental facts beyond the limits of "external" forces, we find more social and material causation and construction. Even the mathematician is a social structure. These ideas are illustrated in discussions of the history of pure mathematics (including comparisons of Sumerian, Babylonian, Greek, Arabic, and modern mathematical activities), the ideological function of pure mathematics in modern societies, Boole's notion of "laws of thought," and metamathematics.

Session 5 National Science Policies

Assessing National Strengths and Weaknesses in the Sciences. P. Weingart, R. Sehringer, M. Winterhager, Science Studies Unit, University of Bielefeld

The paper will present preliminary findings of a study of German basic R and D based on ISI data, in particular the 1984 co-citation indicators. This is an attempt to identify gaps and weaknesses as well as strong points on the sub-field level which will later provide the basis from which to

undertake in-depth institutional analyses to identify causes of bad or excelling performance in research.

On the Measurement of the Effectiveness of National Science Policies in Changing International Environments. **Loet Leydesdorff**, Science Dynamics, University of Amsterdam

It will be argued that from an information science point of view national science policy efforts come down to deliberate attempts to change the selections which (institutionally defined) research groups make with respect to the international literature over time. These selections can be either active (publishing and citing) or passive (being cited, coted, etc.).

The study examines the development over time of two highly comparable Dutch science policy programmes (one on 'wind energy' and one on 'solar energy') in terms of parameters which can be associated with such selections. The different outcomes of the two programmes are analyzed in terms of the emergence of new journals in both areas. When the development of journals is taken as an operationalization of the international scientific environment, it can be shown—using methods which are described in an earlier article—that the scientific outcome of these science policy programmes can be accounted for in terms of the developments in the international environment. Some implications for the elaborations of a model and for science policy will be discussed.

National Styles in Science and Technology: A Comparative Model. **Andrew Jamison**, Research Policy Institute, University of Lund

The paper explores the notion of national styles in science and technology. It begins by tracing the emergence of the concept in some of the early writings in the history, philosophy and sociology of science (Merz, Duhem, Veblen), as well as its ideological distortion in the search for national differences in science and technology in the interwar years. With universalism declared to be an institutional "norm" for scientific activity, national components of scientific knowledge were marginalized after the second world war, and it is only in recent years that the subject has come to be broached once again.

In this paper, an historical comparison of scientific and technical development in Sweden and Denmark is presented. It depicts national components of scientific knowledge on three interrelated levels - a metaphysical (or cosmological), an institutional, and a technical-economic.

The paper then outlines two areas of research in the social study of science where a comparative perspective - and a focus on national styles - can be enlightening. The one concerns technology policy-making, and in particular the search for national strategies in microelectronics. The second concerns public participation in scientific production, and in particular the knowledge strategies of environmental movements. In both areas, the author is currently carrying out comparative research in Scandinavia.

Biotechnology, Agriculture, and Developing Countries: Promises and Contradictions. **William B. Lacy, Lawrence Busch, Jose de Souza Silva, Shripad Deo**, University of Kentucky

Agricultural biotechnology has been viewed as a major force for improving the social well-being of developed nations as well as a lucrative commercial opportunity for multinational corporations. Recently, the perceived opportunities of this new technology have spread to the Third World with concomitant investments of human and economic capital. This paper focuses on the development

of agricultural biotechnology in the Third World. This technology is situated in the broader social, economic, and historical context for science, technology and agricultural development. Although traditionally the technology has been assessed from an economic cost-benefit perspective, this analysis also utilizes a general social assessment strategy. Issues of state, university, and industry relationships in agricultural science and biotechnology, activities, transfer of technology, control and access of agricultural technology, and the potential social and economic consequences of the technology will be addressed. Examples will be drawn from Brazil, India, UN Industrial Development Organization's two biotechnology centers (Trieste, Italy and New Delhi, India), and the International Agricultural Research Centers.

The Comparative Analysis of National Science Policies. **Maurice N. Richter, Jr.**, SUNY-Albany

Comparisons among science policies of different countries are complicated by the fact that government policies which affect science are often not classified as "science policies," and by the fact that science is affected by practices that are policy-determined in some countries but not in others. This paper seeks to develop concepts which will help us to cope with these complexities in comparative science policy studies.

The Effects of Policies of Research Councils on Research Priority Fields: Three European Cases. **Wouter Van Rossum**, Science Studies Unit/Department of Social Policy, University of Groningen

European governments follow different policies as to the way research councils are involved in their efforts to initiate and stimulate research in fields that are supposed to be societally relevant.

In this paper the policies of three countries are compared, in which the research council respectively (a) executes the national research priorities (Switzerland); (b) is involved with national programmes executed by other bodies (Sweden); (c) has no institutional relation with the national programmes (FRG, in this case the DFG).

For two fields in particular, *viz* microelectronics/semiconductor research and health research, the consequences of the different positioning of the research councils are assessed.

Based upon interviews with officials involved and written materials, the paper indicates especially the effects of the variations in the position of research councils upon the translation process from problem area to actual research.

Session 6 Women in Science, 1789-1979, I

Before Objectivity: Wives, Patronage and Cultural Reproduction in Early 19th Century France. **Dorinda Outram**, Department of History, University College, Cork, Republic of Ireland

This paper examines the input of wives into the social organization and vocational identity of elite science in France in the post-revolutionary period. In spite of the enormous attention this period has traditionally drawn from historians of science, no study has even been given to the impact of marital strategies on the scientists of the time, whether as individuals or as a group.

I begin my inquiry into these points by establishing as my sample the scientific membership of the Institut de France in this period. The number of bachelors was

high; later than usual marriage was common, numbers of children produced were lower than for other elite groups. Given the strong contemporary disapproval of marriage for savants, what benefits were to be reaped from marriage by those who refused to contemporary warning on this score? Part of the answer must lie in the nature of the women themselves. These wives were drawn from all social classes; they tended to be much younger than their husbands and often were productive writers, translators, or artists in their own right. A picture of their marriages emerges as one in which the production of children, or the achievement of financial or social advancement is not stressed. It is argued that we abandon a model of scientific patronage which involves only (male) patron and (male) protégé and substitute for it a 3-point model which would involve patron, patron's wife and protégé in which the patron and wife perform quite different tasks which reinforce the patronage networks of the successful scientist.

I conclude by questioning contemporary arguments about the marriage of savants and by applying a more chronologically precise view of the evolution of this idea. A pervasive ethic of virtuous happiness insisted on marriage as that social institution which was the nearest to nature and the most likely to reconcile 'nature' and 'the world' for the savant. It thus emerges as the social institution which, as this period progresses, poses least threat to the (male) savant in terms of his vocational self-image.

Botany in the Breakfast Room: Women, the Family and Plant Science in Early 19th Century England. Ann B. Shteir, York University, Ontario

The science of botany in England developed from natural history and from a base in amateur pursuits, and women were prominent and enthusiastic participants in the activities of amateur plant study from the mid-18th century on. They were not central to scientific discovery within botany, nor did they have a public hand in shaping the main botanical institutions. Women's place in the history of botany until the later 19th century was as cultivators of science, helping to spread botanical ideas and to encourage the botanical work of others through their teaching, their writing, and their example. Much work of pre-professional British botany took place within networks of family relationships. It is particularly the case for women's contributions to late-18th and 19th-century botany that family lines affect the development of scientific interests and the directions which that interest may take. This is not surprising for the decades before botany became a formal subject in schools and universities open to female students.

My paper will trace links between women's amateur work in 18th and 19th-century England and their family lives. I will discuss women as botanical helpmeets. I will show how family affiliation helped and hindered kinds of botanical work. Family circumstance was historically a spur to writing popularizations of botany, as examples will demonstrate. I also will describe the place of family as initiators into popular science: here it will be important to sketch the place of botany in 19th-century general education.

There has been very little work on the social history of British botany, and research is only just beginning from a feminist perspective. In my essay "Linnaeus's Daughters" I sketched women's presence as cultivators of science and showed how it helped to be a botanical daughter or wife. In the paper projected here, I would like to use diary and autobiographical material to get at biography more closely,

and I want to use a broader notion of family to explore how women came to learn their botany and how botany fit into daily life. Examples will include the life and work of Elizabeth Blackwell (c.1700-1758), Priscilla Wakefield (1751-1832), Pleasance Smith (1773-1877), Maria Gray (1787-1876), Elizabeth Twining (1805-1879), Jane Loudon (1807-1858), Mary Kirby (1817-1893), Lydia Becker (1827-1890), and Phoebe Lankester (1825-1900).

Nineteenth Century Women Botanists: Wives, Widows, and Work. Nancy Slack, Russell Sage College

Botany was thought an appropriate and edifying subject for young women in 19th century America. It was taught from 1820 on in many academies and female seminaries, not to train women to be professional botanists but as part of their training to become better and more spiritual mothers. There were no professional positions open to women until the 1870's with the opening of the women's colleges. Nor were there sources of training for these women beyond the secondary school level in the medical schools where male botanists received their training. No graduate instruction in botany existed for women in American universities until the 1890's. Yet one source finds 500 women botanists in the 19th century!

The explanation is that the great majority of women in botany were not professionals by any current criteria. Many were the traditional "helpers" of their fathers or husbands. In the mid-nineteenth century for example, William Sullivant's wife, Eliza Wheeler Sullivant, was an acknowledged assistant in his important work on mosses. Doubtless many other botanical wives went unrecognized.

Among the better known women who published in their own names all possible family patterns are to be found: single, married, widowed, with and without children. The lives and work of several of these women botanists will be explored in this paper. These include: Almira Phelps Lincoln, Kate Curran Brandegee, Elizabeth Knight Britton, and Wanda Kirkebride Farr. Widowhood, by the sudden removal of the constraining traditional role and the need to supplement income served as a spur to increase botanical activity. The most common strategy for recognition in botany for a woman born just before the twentieth century was the decision to remain single. Modern women scientists as well as historians may gain insight from choices and conflicts of these nineteenth century women botanists.

Strangers to Each Other: Male and Female Relationships in the Life and Work of Clemence Royer (1830-1902). Joy Harvey, Department of History of Science, Harvard University

Outside of a few major literary figures in the nineteenth century, few women in France produced writings as closely linked to their female roles as Clemence Royer, best known for her French translation of Darwin's *Origin of Species*. This translation with her own dramatic and startling preface was only one of a large body of writings on social evolution, economics, animal intelligence, anthropology, 'natural morality' and even a historical novel.

Even in the preface to her first edition of Darwin (Darwin's third edition), she signaled the social consequences of unthinking selection in human marriage, expressing her fear that the continued choice of beauty in women over intelligence would have a disastrous effect on the human species. Ten years later she complained in an unpublished memoir that "woman was the one animal in all creation about whom man knew the least."

Clemence Royer's life was intimately tied to her public pronouncements. She had, as a child, seen the effect of a tyrannical if loving father, and been a witness to many ar-

guments between two strong-minded parents. She rejected marriage as an option for herself, but formed a life-long alliance with a man who had spent many years in exile, to return to a public life only for his last ten years. This alliance was not an easy one, since she found herself in economic straits, but it resulted in a son who was to become a colonial officer, never entirely at ease with his own life. She was strongly influenced by these male relationships in her personal life. In her public life, except for the first lectures she gave on science and philosophy, she found herself the only woman in male scientific societies. At the end of her life she was regarded by the women who surrounded her as an almost legendary representative of the closed world of men's science. Yet the French middle class feminists who rediscovered her gave her a new opportunity to speak directly to women through regular columns in the French journal *Le Fronde* edited by Marguerite Durand.

Session 7 Theories of Language and Science

Models of Language Learning and their Implications for Social-Constructionist Analyses of Scientific Belief. Donald T. Campbell, Lehigh University

Recent presentations of the social-constructionist and social-relativist analyses of scientific belief have correctly invoked the emphasis on the underjustification of both "fact"-establishment and theory choice in post-positivist philosophies of science. Accompanying this is an emphasis on the role of language in forming the categories and causal scenarios of perception and interpretation, for ordinary perception, folk-classifications, and science. Examples of the latter for use in this paper will come from four sources: Knorr-Cetina & Mulkay (1983), Barnes (1983), Bloor (1983), and Collins (1985).

Not at issue are the social-constructivist points that belief in scientific facts and theory go beyond what has been empirically and logically proven, that this indeterminacy is practically important as well as logical (in that it allows for social processes of persuasion, group formation, belief traditions, and extra-scientific sources of preferences, all to influence belief choice). At issue is only the matter of degree. Employing a deliberately oversimplified epistemological short-hand, Collins has identified the dimension effectively: "... the natural world has a small or non-existent role in the construction of scientific knowledge" (1981, page 3). This dimension has at the other pole the extreme or naive scientism: "the natural world totally determines scientific belief." This ideal type is prevalent in science education and in popular faith in science. Among some of the philosophers of science who identify their position as "scientific realists" something close to this extreme may be endorsed: "In mature sciences like physics and chemistry, 'the way the world is' is the predominant determinant of scientists' consensus on which 'facts' are 'established' and which theory 'best fits' them." The impressive research of the strong, social constructivist, relativist programmes in the sociology of science can properly be argued to reject both of these positions. There remain, however, degrees of 'natural world' contribution to belief choice that are well worth arguing, even be it only that range between "little" and "nothing" offered by Collins. Such arguments will not be able to employ "proof," empirical or logical, and will have to make use of unproven presumptions. They will have to

use coherence arguments for belief-choice even were they to retain "correspondence" with "physical reality" as a goal. But this, after all, is where we all live in ordinary life as well as in science.

This paper argues that our sociological model of language learning will affect the plausibility of the belief that the nature of the natural world has any effect at all on belief-choices in science. More than that, it will argue that one plausible model of language learning has implications as to which, among alternative social systems of mutual persuasion, would be more likely to maximize the role of 'the natural world' in influencing the scientific belief-choice. Such a hypothetical comparison of social systems of belief retention and change implies the possibility of a modest, fallibilist, presumptive "sociology of scientific validity" (Campbell, 1985).

A Relativistic Approach of Einstein's Relativity. Bruno Latour, Centre de Sociologie de l'Innovation

This paper presents a development of the notion of immutable mobiles and of centers of calculation (B. Latour and J. de Noblet, eds., *Les 'Vues' de l'Esprit*, (B. Latour "Visualization and Cognition," H. Kulkick, editor, *Sociology of Knowledge Science and Art* - forthcoming) and explores it in the case of Einstein's general relativity theory. One of the main problems of Einstein is to decide how the equivalence of all observers' reports may be maintained even though these observers are sent into frames of references that are accelerated or go near the speed of light. Instead of pursuing an externalist sociology—replacing Einstein's notions inside his family and cultural background, this article, that uses semiotic tools, explores an internalist sociology. The more one sticks to the text, the more it is easy to see a proliferation, in Einstein's stories, of characters and actors with whom (with which) the author has to struggle. This proliferation raises many "management problems," so to speak, for Einstein if he wants to keep the displacement of information without deformations or transformations. The drama of Einstein's text is then analysed by studying what the author is ready to abandon and what he wants to maintain at all costs. This internalist sociology can be studied with classic sociological tools, except that, in this case, it is applied to semiotic characters. The advantage of this approach is to show how the content of an abstract theory brings the analyst closer to the context. The general claim (but it is still a claim!) is that the more abstract and mathematised is a theory, the easier it is to study it in a sociological fashion.

Session 8 Society and Science

Black Migration and the Decline of Racism in American Social Science, 1910-1940. Robert P. Young, University of Texas at San Antonio

Racist theories of black inferiority dominated American social science until the late 1920's. The intellectual ascendancy of scientific racism in the United States resulted, in part, from the fact that one of the dominant scientific paradigms of the late nineteenth century, Darwin's theory of evolution, seemed to suggest a persuasive explanation of American race relations during the late nineteenth and early twentieth centuries: Black social inferiority was due to black biological inferiority. Before this racist doctrine could

be successfully assaulted, two preconditions had to exist. First of all, a rival paradigm, cultural anthropology, had to be presented to the scientific community which explained racial differences in terms of factors other than innate racial characteristics and equaled the Darwinian paradigm in its aesthetic appeal and explanatory power.

Secondly, the social conditions had to exist which would allow racial equality to be demonstrated empirically. These intellectual and social preconditions were not present in the United States until the 1920's, but once they were, racist explanations of black social inferiority were discredited.

On Alternative Methodologies and the Institutionalization of Science. Ubiratan D'Ambrosio, Universidade Estadual de Campinas, Brazil

The idea of universality of Science comes under challenge as recent research, mainly by anthropologists, show evidence of practices which are typically of a scientific nature, such as observing, counting, ordering, sorting, measuring and weighting, but which are carried out in radically different way than what is practiced in educated or scholarized circles. This is particularly true in mathematics, and has encouraged research on what has been called *Ethnomathematics*. In this paper we discuss basic issues which lead to some critical remarks on current methodologies for History of Science and Epistemology. The approach lies on the borderline between History of Science and Cultural Anthropology and has implications on the analysis of the institutionalization of Science throughout History.

1:00-3:00 pm

Session 9 Science Indicators II

The Empirical Progressiveness of the General Equilibrium Research Program: An Empirical Test. Arthur M. Diamond, Jr., Department of Economics, The Ohio State University
The empirical progressiveness of the general equilibrium research program is examined by analyzing the articles that cite the leading general equilibrium theorist, Gerard Debreu. Of a random sample consisting of 88 articles that cite Debreu, only 5 are even marginally empirical. To test for an indirect effect on empirical research, a subsample of 10 of the theoretical articles citing Debreu was randomly selected. Of 79 articles citing the 10, only 5 were empirical, indicating that the indirect effect is not large.

Citation Rates in Parasitology: A Problem of Systematic Under-valuation. James A. Ruffner, Wayne State University
Indicators derived from *Science Citation Index* must be adjusted for each discipline and field, but practice lags. A case study highlights some problems and opportunities.

Few parasitic systems can be maintained under controlled laboratory conditions. Half the work is concentrated thereby on about two dozen "fashionable" genera. Various circumstances, including strictly local problems of human and animal welfare, lead to slower paced research on hundreds of "special interest" genera. Citations in one research specialty are shown to change by a factor of two as work shifts between these categories. The effect is strongest for the highest "impact" authors. These differences hold generally for the discipline and affect authors

and journals alike. Accordingly, significant work and sources may be undervalued by normal citation standards. The results temper notions of quality or progress and raise fresh doubts about the utility of impact indicators without considerable background analysis.

Publication and Citation Patterns in Mexican Science. V. Cano, School of Library and Information Science, University of Western Ontario

Scientific output of Less Developed Countries (LDC) has mostly been studied using ISI data-bases as data sources. Such studies have presented a biased view of local scientific activities of LDC due to cultural and language bias of ISI coverage. This study proposes to analyze the local and international scientific activity of Mexico using BIBLOS database as data source. BIBLOS presents the first data-base with comprehensive coverage of scientific output published locally and abroad by Mexican based researchers.

Two samples will be randomly selected. Sixty papers published in Mexico constitute the first sample. Significant differences are expected in the age, language and nationality of citations on both samples.

A full bibliography of members of two research institutes in Mathematics will be compiled. Papers will be classified as theoretical, applied or experimental. Publication and citation patterns will be analyzed. An abundance of theoretical papers in proportion to applied or experimental papers published abroad is expected.

Academic Scientists' Use and Evaluation of SCI/SSCI. Lowell Hargens, University of Illinois and Howard Schuman, University of Michigan

Survey data for U.S. academic sociologists and biochemists show that both groups make extensive use of citation index information. We examine patterns of use of citation index data both for bibliographic purposes and in evaluations of individuals' scholarly contributions. We also analyze variation between and within the two fields in individuals' evaluations of citation count data. Such variation can be explained in terms of Festinger's theory of social comparison processes, and it shows the influence of a "frogpond effect" wherein the average level of citations to colleagues in a scholar's department affects his or her evaluation of citation counts in addition to the effect of the level of citations to his or her own work.

2:30-4:30 pm

Session 10 Women in Science, 1789-1979, II

Careers and Home Life in the 1880s in Russia: The Choices of Sophia Kovalevskaja. Ann Hibner Koblitz, Wellesley College
The mathematician Sofia Kovalevskaja (1850-1891) had the good fortune to grow up in the 1860's, at a time when the educated classes of her native Russia were engaged in multi-faceted debates about social and political reform. In her youth Kovalevskaja espoused nihilism, and remained an adherent of the philosophy all her life. This paper argues that nihilism, with its faith in science, belief in the total equality of women, and attempts to transform the patriarchal foundations of marriage, the family and society, was an aid to Kovalevskaja in her later struggles to blend home life and a career as the first woman professor of mathematics in modern times. Because of her nihilist convictions,

Kovalevskaia perhaps was more easily able to reject the constraints imposed by traditional conceptions of woman's place and role than were many other women scientists in her time and later.

Astronomy in the Family: Cecilia Payne-Gaposchkin (1900-1979). Peggy Aldrich Kidwell, Smithsonian Institution

Cecilia Payne-Gaposchkin was a woman of enduring loyalties. As an undergraduate at Newnham College, Cambridge, she decided to become a research astronomer. Barred by her sex from a research position in England, Payne emigrated to the United States in 1923, did outstanding graduate work at the Harvard Observatory, and remained there for over fifty years as a postdoctoral fellow, staff member and, ultimately, professor of astronomy.

One friend who recommended Payne when she first went to Harvard commented that she was so interested in astronomy it was unlikely she would leave the field after a few years training in order to get married. Indeed, during her first years at Harvard, the Observatory staff offered Payne much of the companionship and support of a family. Then, on a 1933 trip to Germany, she met Sergei Gaposchkin, a Russian expatriot who had recently been dismissed from his job at a German observatory because of his nationality. Gaposchkin told Payne of his long struggles to become an astronomer and, deeply moved by his plight, she arranged for him to come and work with her at Harvard. Some months later, when it seemed that Gaposchkin's temporary job probably would not be renewed, he and Payne "eloped" from the Observatory. Their unexpected marriage became a longlasting if stormy union that combined collaboration at the Observatory with the raising of three children.

Payne-Gaposchkin's unconventional decision to combine duties of astronomer, wife and mother brought with it a host of difficulties. How much should her research plans be adjusted to Sergei's interests? Who would take care of the children and, later, how much should she intervene to encourage their astronomical studies? How should she respond to invitations that included her but not her husband? Examining the answers Payne-Gaposchkin found reveals both her own loyalties and the assumptions of mid-twentieth century American society.

Public Careers and Private Lives: Progressive Age Influences on the Life and Career of Ethnographer Audrey I. Richards. Jo Gladstone, Harvard University

The influence of family structure, class background, generational and national affiliation upon the beliefs and attitudes of prominent women intellectuals has been no less powerful than it has been on the men. In examining the early socialization pattern and the responsive academic chord touched on in the long life and career of Audrey Richards the effect of a separate sphere avocation of the women of her generation in Britain must be born in mind.

Intellectual role models, many of them male, a few female, progressive Tory family traditions, the crucial academic influence of Graham Wallas and other Fabians brought her as a trained scientist and social worker into the heart of the Functionalist movement in British social anthropology at the age of thirty. Her empirical ethnography, her single marital status, the changing topics of her studies, and the degree to which her own studies were or were not 'gender free' will be examined against the separatist British female sphere and the predominantly male one in which Audrey Richards' life was so successfully embedded.

The Synergy of Disciplinary and Marital Trajectories in the Career of Mathematician-turned-Biologist, Dorothy M. Wrinch, 1894-1976. Pnina G. Abir-Am, Department of the History of Science, Harvard University

Dorothy Maud Wrinch, "Dot" to her friends and "Delta" to her correspondents, pursued an active career as a researcher in mathematical physics and philosophy of science in the period between 1918-1932. A wrangler in Cambridge University's mathematical Tripos, she held research fellowships at Girton College, taught at Imperial College in London and tutored at the five women's colleges in Oxford. In 1929 she became the first woman ever to receive a D.Sc. from Oxford.

Starting in 1932, Wrinch participated in the meetings of an avant-garde group of scientists and philosophers - the Biotheoretical Gathering - who sought to extend the role of logic and mathematics, formerly proven so successful in physics' two revolutionary theories (the theory of relativity and quantum theory), to the yet undeveloped theoretical biology. Following a sabbatical spent on becoming acquainted with opportunities in theoretical biology, Wrinch switched her entire research effort to this yet unestablished field. Soon, she made a major contribution by advancing the first theory of protein structure in the mid- and late-1930s, a time when this problem was thought to constitute the "secret of life".

Despite her world fame in the late 1930s, Wrinch lost her position in the post-WW2 period when the scientific community came to believe that her once original theory had been refuted, even though in the mid-1950s the supposed refutations themselves collapsed. Eventually, Wrinch became a tragic paradigm of a woman in science who almost reached the pinnacle of world theoretician only to be pushed to the fringes by male opponents who enjoyed and abused greater institutional power.

Though many different factors are needed to explain Wrinch's rise and fall in the scientific community (such as the general difficulty for women to secure positions and independent scientific credit; the sheer complexity of her ideas in theoretical and mathematical biology; her move from Britain to the US, the home ground of her most ruthless opponent; or her own confrontational strategy in scientific disputes) an aspect frequently omitted pertains to her consistent strategy of operating on both the career and the marital fronts. Unlike many Victorian-born educated women who believed that "no woman can serve two masters" and thus felt compelled to choose between a career or a marriage; Wrinch felt she was well suited to score great successes in both of these, presumably exclusive, arenas. This paper will explore the interaction between her career trajectory of disciplinary switch from mathematics to biology and the two marital choices she made, in 1922 in Britain and in 1941 in the US.

Session 11 Communication of Risk

Community Perspectives on the Communication of Risk. June Fessenden-Raden and Janet Fitchen, Program on Science, Technology and Society, Cornell University

Effective risk communication is an interactive process. From our research in communities with toxic contamination problems, we found that being unaware of community perspectives can result in misperceptions, even rejection, of risk information - no matter how accurate. Facts presented to the community are filtered through culturally shaped perceptions, through the local context in which the risk

is embedded and by individual experiences. Our research indicates that trust in local institutions and in agencies managing the risk situation enhances the credibility of the information provided. Also, while risk information usually, of necessity, is presented in aggregate terms, we found that people tend to ignore it unless they can relate it to their specific locale and themselves as individuals.

Technical and Psychological Problems in the Communication of Risk. **M. Granger Morgan**, Department of Engineering and Public Policy, Carnegie Mellon University

The results of modern quantitative risk analyses can be complex. There are problems in adequately conveying such results, both to technically sophisticated analysts, and to semi-technical and non-technical people. Some of these problems arise from biases introduced by the heuristics that people use when they think about uncertainty. Some result from the difficulties of adequately conveying complex results in two dimensional representations. In communication to semi-technical and non-technical people, additional complications are introduced by unfamiliarity with the formal concepts of probability. This paper will provide a brief status report on what we now understand about these problems and will report on several pieces of research in progress.

The Role of the Media in the Communication of Risk. **Peter M. Sandman**, Rutgers University

Media treatment of risk is an important aspect of risk communication. Important dimensions of media performance include: (a) the tendency to cover politics, cost, and other factors more than risk; (b) the tendency to cover alarming aspects of risk more than reassuring ones; (c) the tendency to cover extreme positions on risk more than moderate ones; and (d) the tendency to cover any position on risk more than the data supporting it. Examples of these and other aspects of media risk communication can be drawn from issues ranging from hazardous waste facility siting to indoor radon contamination. Patterns of risk coverage are explicable in terms of traditional journalistic standards.

Rights, Duties and Dilemmas in Communications about Risk. **Michael Baram**, School of Public Health, Boston University
This paper provides an overview of the legal doctrines governing risk communication and the value conflicts and legal issues which remain unresolved. It covers the following subjects:

- (1) The industrial "duty to warn" consumers and users of hazardous products;
- (2) The worker's "right to know" hazard information and to gain access to other industrial information on workplace risks;
- (3) The community's "right to know" accident hazard information from industry;
- (4) The industrial duty to report to government agencies the hazard attributes of company products, processes and wastes;
- (5) The responsibility of government agencies to provide for "freedom of information" in responding to inquiries by private parties about agency-held information on hazards;
- (6) Some unresolved issues including the protection of trade secrets, the implications of risk communication for concepts of responsibility, and whether risk communication should be premised on a "right to know" or "need to know" basis.

Session 12 Science Policy and Science

Effects of Funding Changes in the Life Sciences: Project Summary. **Edward J. Hackett**, Department of Science and Technology Studies, Rensselaer Polytechnic Institute

This study examines how changes in federal policy, expressed through changes in the levels of funding for scientific research, are interpreted, enacted, and transformed by agency officials, scientists, and university departments. Increasing federal support in an area of science is generally expected to increase the volume and quality of research by attracting more and more able scientists to the area. Similarly, decreasing support for an area should signal scientists to work elsewhere. But such changes in policy must filter through funding agencies, universities, and scientists before they have any effect on research itself, and these intermediaries may alter policies in the transmission process.

For example, a funding agency's management of new funds will be shaped by its history, goals, interests, and constituencies. A large increase in support for research in an era of science must accommodate such preexisting influences; a decrease could be buffered by them. University departments must provide a stable and productive environment of equipment, colleagues, and other resources in the face of changes in scientists' interests and funding agency priorities. Departments may choose to soften the impact of declining support by providing "bridging" support to a scientist between grants or they may choose to provide seed to those willing to seek support in an emerging area. Or consider scientists who may have to reconcile their enduring research interests to the current funding climate by abandoning areas no longer "fundable" and acquiring new interests that link their "research trails" to emerging prospects for support.

Diet, Cancer and the Interpretation of Evidence: Disputes Over The Scientific Basis for Policy. **Stephen Hilgartner**, Department of Sociology, Cornell University

Using a social constructionist framework, this paper analyzes policy disputes over dietary recommendations intended to reduce cancer mortality. The policy discourse is viewed as a drama played out in various social arenas (e.g. scientific advisory panels, federal agencies, the mass media, etc.). In an ongoing struggle over the interpretation of evidence, actors in the controversy attempt to define the scientific basis for policy. To accomplish this, actors select items from a corpus of evidence and recombine them. By selecting, juxtaposing, undermining, bolstering, weighing and packaging pieces of evidence, participants in the controversy attempt to construct authoritative summaries of the state of scientific knowledge, while deconstructing opposing views. The paper will document how various actors develop strategic constructions and present them in public arenas to shape nutrition policies.

Restricted Funding and the Structure of Research in British University Science Departments. **H.M. Collins and G.D.L. Travis**, Science Studies Centre, University of Bath

The British Economic and Social Research Council has recently funded a Science Policy and Social Studies of Science initiative. Among the grants awarded was one to the University of Bath for a study of 'The Structure of Science in Britain and Peer Review'. The object of this research is to examine the balance between high risk innovative research and work which extends an existing 'paradigm'.

The paper reports the first results of fieldwork in two large, well-known, university departments. It presents a typology of innovatory research activity, and goes on to explore the hypothesis that lack of resources restricts the opportunities for innovatory research.

Session 13

Reflexivity and Science

The Paradox of Application and its Reflexive Reemergence in its Analytical Presentation. **Malcolm Ashmore**, Department of Sociology, University of York (U.K.)

This paper describes the phenomenon labelled 'the paradox of application': to be usable in a practical context, knowledge appears to have to undergo a transformation involving a radical reduction in its distinctive content; yet to be counted as 'knowledge' it must be seen to possess a distinctive content. Thus the (social) process of practical application achieves the paradoxical effect of destroying the very entity - the knowledge - that is supposed to be the 'subject' of the process. And thus the conventional (and very powerful) naturalistic idea of the application of knowledge is shown to be illusory. This paradox is developed using materials from the author's (and M. Mulkey and T. Pinch's) current research project on the practical application of health economics which is funded by the U.K. ESRC as part of its initiative on science studies and science policy. It is in this context that provides for the reflexive (and ironic) reemergence of the paradox: what would be the applicability in the context of a science policy of these findings (knowledge) about the (non) applicability of knowledge.

Anti-Reflexivity in the Natural and Social Sciences. **Steve Fuller**, Department of Philosophy, University of Colorado
I start by introducing the term "cryptoscience" to describe activities that are, in principle at least, susceptible to a scientific treatment (especially in the sense of being able to obtain empirical generalizations), but which, for various reasons, are prevented from being subject to such treatment. I then argue that both natural and social scientists are cryptoscientists about their own activities, which amounts to rejecting a reflexive application of the scientific method.

The natural scientific rejection of reflexivity often stems from the "free market" and "open society" rhetoric of Popper, von Hayek, and even Feyerabend, who connect the "realism" of scientific findings to the fact that they remain robust, in spite of attempts by would-be "scientists of science" (that is, methodologists) at regulating the scientific enterprise. It is noted that none of the opponents of reflexivity for natural science has an answer to the claim recently advanced by Gernot Boehme and the German "finalizationists": namely, that while a regulated natural science would not be "progressive" in a sense amenable to Popper et al., it might be more socially beneficial. The social scientific rejection of reflexivity, voiced with varying degrees of sophistication since Mannheim's program for a sociology of knowledge, rests on the fear that social scientific results would be vitiated if they were themselves relativized to their social contexts of validation. This fear is legitimate, however, only if the social scientist either secretly aspires to the socially detached standpoint of the classical natural scientist or mistakenly assumes that his results could be valid relative to just one (or a few) social contexts.

Both the natural and social scientific rejection of reflexivity are based on premature conclusions about what the sciences will discover which would then be reflexively applied: the natural scientist suspects the possibility of universal determinism destroying the free spirit of inquiry if he is treated as just another natural phenomenon; the social scientist suspects a lack of universal social determinism but suspects that each society is subject to its own laws, and that would undermine the significance of whatever he discovered. In short, reflexivity would threaten the natural scientist's motivation for doing anything and the efficacy of whatever the social scientist did. It is noted that these fears of reflexivity are justified only insofar as the characters of natural and social science turn out to be as just indicated—which is rather unlikely.

I end by defending, against Harry Collins, reflexivity as a methodological tool for integrating the sciences. Collins has suggested that disciplines are typically non-reflexive in order to keep the point of their activities in focus (much as one does not apply the rules to a game beyond the field of play). I argue that taking reflexivity would force the scientist, whether natural or social, to account for the world such that he could live in it. Indeed, the negative reaction of scientists to the reflexive move shows just how far we are from that goal.

Meaning, Interpretation and Reflexivity in the Social Sciences. **Andrew McKinley**, Psychology Department, St. Andrews University, Scotland

Critics of the strong programme (SP) have argued that it leads to a radical scepticism about the meaningfulness of social actions. SP proponents have responded to this by appealing to notions of local criteria of meaningfulness such as interests.

The present paper formulates a radical critique of this response, suggesting that SP mistreats the relationship between sociologists' data and their explanations, by underestimating the reflexivity of sociology of knowledge practice. It mistakenly claims that interests are recovered through empirical analysis and are thus explanatory (analyst's) hypothetical constructs used to make sense of participants' local activities and accounts. In opposition to such a claim, the present paper argues that accounts are internally related to interests and activities, and thus a correct understanding of how such accounts are managed just is a correct understanding of such interests and activities. It is concluded that both SP and its opponents have mistaken the nature of the reflexive 'problem'.

Reflexivity in Science: Can It Exist? **Kay Oehler and Nicholas Mullins**, Virginia Polytechnic Institute and State University
We argue that reflexivity must be adequate from a science studies perspective encompassing scientific as well as sociological practice. We find that the use of "experimental literatures" in sociology of science has little relevance to scientific literature.

We ask, is it helpful or useful to transfer the goal of sociological reflexivity, which may or may not be advanced by "experimental literatures," to the realm of scientific work? Even though science is seen as mostly non-reflexive, examples of a long tradition of "experimental literatures" put forward by natural scientists are discussed. Even within sociology, traditionally oriented sociologists have written parodies, reversal ceremonies, and ironic humor. We argue that these forms of expression are most useful to the extent that they are anomalous forms of expression in science. If such is a case, such practices have not advanced reflexivity (defined in this way at least) in all sciences.

Finally, we explore alternative interpretations of reflexivity. We argue, in particular, that while "experimental literatures" may be a reasonable form for reflexivity to take in sociology, the ideal form of reflexivity varies from scientific subculture to subculture.

Make Me Reflexive - But Not Yet. **Graham Watson**, Anthropology Department, University of Calgary
There is much muddled thinking about reflexivity in the writing of members of the interpretive school of anthropology. The muddle consists, in the first place, in paying lip-service to relativism while stubbornly clinging to realism, and, in the second place, in claiming to confront reflexivity while merely managing it. The root of the trouble lies in anthropologists' failure to acknowledge that reflexivity is not merely something which one 'does', such as engaging in self-reflection, but is, rather, an essential and inevitable property of all discourse. The fact that reflexivity is essential is potentially subversive of the authority of the author; hence numerous strategies for managing it. Essential reflexivity is ignored, characterized as an irrelevance or as a distraction, or 'dealt with' by turning it into a topic for investigation or by transforming it into something non-threatening like the problem of validity. In these ways interpretive anthropologists defuse essential reflexivity and invigorate the realist genre they declare abandoned. Woolgar's proclaimed confidence in them is misplaced.

Reflexivity is the Ethnographer of the Text. **Steve Woolgar**, Department of Human Resources, Brunel, The University of West London

The social study of science has recently benefitted from a number of "laboratory studies," the hall-mark of which is the ethnographic study of the culture of the laboratory. With reference to discussions of the history of anthropological method, this paper examines the argument that ethnographies are best understood as textual accomplishments and asks what is to be gained by drawing attention to the textual character of reports about science. The famous anthropological concern with the exotic is identified as the root of some current complacency about the character of ethnographic reporting. It is argued that an understanding of the textuality of reporting more profitably proceeds by stressing the *uncertainty* of ethnographic study rather than the *exoticism* of its subjects. An attempt is made to exemplify this argument by way of ethnographic observations of this paper's own construction.

Saturday, October 25

9:30-11:30 am

Session 14 **Changing Notions of Ownership in Science**

Policy Incentives and Constraints on Dissemination of Scientific and Technical Information. **Vivian Weil**, Center for the Study of Ethics in the Professions, Illinois Institute of Technology
A number of recent public policy decisions affect research in technical fields. New sorts of proprietary arrangements, especially applications of patents and trade secrets, influence the availability of research results. In the interest of national security, the government is acting to con-

trol dissemination of scientific information by the use of prepublication review agreements with grantees, contractors, and employees, the classification system, and the application of laws such as the Espionage Act.

New proprietary arrangements engender debate about the extent to which patent policies promote openness as intended and the extent to which a policy of valid trade secret contracts encourages secrecy. Where there is public funding or government action, there is concern about the scope and basis of first amendment protection. Debate arises about whether scientific information ranks as fully protected speech and about the extent of the government's control of information in which it has a proprietary interest. Implications for researchers and the research enterprise of the capture of technical information as private property or as government property will be explored.

Mitigating Conflict Between Research and Enforcement: Technological Promise and Limits. **Robert F. Boruch**, Department of Psychology, Northwestern University

The paper will discuss problems engendered by government agencies with two potentially conflicting missions: enforcement and legitimate social research. The relevant agencies include the Internal Revenue Service, the Center for Disease Control, the Federal Aviation Administration, Food and Drug Administration and others. Part of the potential conflict lies in the idea that individuals (or institutions) are unlikely to candidly provide information, designated for research purposes, to an agency with enforcement powers. Citizens are wary of admitting tax cheating to surveys supported by the IRS, for instance. Some solutions to problems are also discussed.

Scientists' Responses to Disputes Over Access to Data. **S. J. Ceci and J. G. Baker**, Department of Human Development, Cornell University

In 1983 a colleague and I proposed that all forms of data (behavioral and physical) that have been gathered with public support (e. g. federally funded or funded by tax exempt foundations) should be made available to researchers other than the original P.I.s, provided the latter were allowed the exclusive use of the data for a limited period of time. The first part of my presentation will be devoted to a brief description of this proposal, its legal, pragmatic, and ethical roots, and some of its procedural features. The remainder of the presentation will be devoted to a description and analysis of scholars' reactions to the proposal.

Allocating Credit and Blame in Science. **Daryl E. Chubin**, Office of Technology Assessment

Contemporary cases of misconduct in research (so-called scientific fraud) can be seen as a violation of shared standards of propriety in a research community. Although we can only infer how widely shared these standards are, it is clear that the allocation of blame for some impropriety falls off the continuum of "normal" expected behavior. Such "deviance" identifies the transgressors; seldom are the punishments meted out without reference to the seniority and reputations of the researchers involved. Simply put, career capital neutralizes the charge and/or embarrassment of deviance. The junior investigator is blamed and usually pays the extreme penalty of exile from science. That this occurs is not surprising; science is a political institution. Analysis of the occurrence, however—be it the case of Spector, Soman, Darsee, or others—raises anew questions of the scientific

ethos: its expression, transmission, and effectiveness in discouraging impropriety in research.

Allocating credit and blame is the "reward (and punishment) system" of science. This paper asks: how does the incidence of misconduct in research clarify the status of the scientific ethos? Are there competing institutionalized norms, operational in particular local settings, that blur the community standards of proper research behavior? How is "tacit knowledge" made explicit in the recognition of credit and blame for research misdeeds? Finally, is the notion of "intellectual property" useful for explaining this process?

Session 15 Sociology of Technology I

The Vibrator and its Predecessor Technologies, 1752-1930. Rachel Maines, School of Management, Clarkson University

The electromechanical vibrator, introduced as a medical instrument in the 1880s and as a home appliance between 1901 and 1903, represented the convergence of several older medical massage technologies, including manual, hydriatic and mechanical. This paper examines the evolution of the vibrator as a medical appliance mainly in the context of treatments for women's complaints, particularly hysteria, neurasthenia, chlorosis, and disorders of menstruation. I shall argue that some forms of gynecological treatment for these conditions employed and/or prescribed by physicians from Aretaeus Cappadox (c. 150-200 AD) through the early decades of this century were fundamentally sexual in character and may, in some cases, have resulted in orgasmic response by the patient. This claim is supported chiefly on medical sources, but also with data from advertising, popular magazines and books, engineering literature, and patient accounts.

Experiment, Technology and the Intrinsic Connection between Knowledge and Power. Hans Radder, Free University of Amsterdam

The paper starts with an analysis of similarities and differences between experimentation and technological production. It is argued that both imply an active intervention in nature and society, but that the cognitive and social aspects of experiment and technology may display considerable differences. The theoretical analysis is illustrated by means of examples taken, in particular, from the technology of nuclear energy production. Next, it is shown that, from the viewpoint of the present paper, the technology conception prevailing in public debates on technology and in technology policy is inadequate; again, the case of nuclear energy serves as an example of this point. Finally, it is concluded that, and in which way, scientific or technological knowledge and social power are intrinsically connected.

Third Sector Production of Technological Change. Michael Mackenzie, Alberto Cambrosio, and Peter Keating, Université de Montreal

It is a general observation that the boundaries between universities and industry, "free" scientific knowledge and industrial or "intellectual" property have become increasingly blurred especially in the biotechnology area. Although there have been models relevant to this process, such as the "New Technology Systems" concept of Freeman et. al., these can address the process only in terms of scientific innovations and knowledge inputs and

outputs. The institutional context for such innovations, the roles of scientists, and the complex distinction and relationship between some knowledge described as "free" and other claimed as proprietary remain unmodelled and are usually described and understood as conflictual—with the innovation process requiring accommodations/compromises and negotiations between different institutions and norms.

At the outset of what became a comprehensive study of the development of immuno-diagnostic kits incorporating monoclonal anti-bodies, we felt that a model was required that reflected the highly integrated mode in which these "conflictual" elements did, in fact, function. We have thus evolved a model—centering on the concept of a "Third Sector"—that can order and interrelate non-technology specific categories such as patents, scientific publications, "know-how," etc., in a comprehensive way and account for such phenomena as "failure" and "success." The Third Sector delineates a sector for the production of technical change, instantiated in R&D and related institutions, distinct from, but producing for, the consumer goods productive sector and the capital goods productive sector.

A Phenomenological Study of Biotechnology: Toward Development of a Theoretical Framework. Navid Mohseni and William B. Lacy, University of Kentucky

This paper develops a theoretical framework for understanding biotechnology. Drawing upon two notions from Hegel's philosophy, it takes as its fundamental premise the notion that the idea without its embodiment is an abstraction. By an abstracted presentation of reality, biotechnology represents a distorted and incomplete picture of reality. The paper develops the framework by beginning with the most immediate understanding of biotechnology, and builds to the most mediated understanding. At each stage of understanding, the internal contradictions are revealed. Seeking a more complete understanding of biotechnology, the paper attempts to transcend the contradictions by redefining it in a more complete fashion. This philosophical study of biotechnology illustrates the necessity of undertaking the study of the social-political economy of biotechnology. It proposes that biotechnology, as a social phenomenon, is the historical product of differentiated interrelated components, or moments (in Hegel's language). In addition, it shows how each component is historically developed, differentiated, and is in contradiction with the other components. Furthermore, we conclude that any isolated study of the components of biotechnology distorts the presentation of its total reality.

Session 16 Machine Intelligence

The Fifth Generation Computer: Genesis and Development of a Technological Artefact. Brian P. Bloomfield, Systems Group, The Open University, U.K.

Artificial Intelligence is one of the most hotly debated subjects of the moment. A subject once viewed with suspicion if not open derision, AI has now returned with a vengeance. In the USA, Europe, and Japan, government institutions as well as universities and private enterprise are spending huge sums of money on AI related research—the hope is that intelligent machines will provide not only new sources of wealth but also powerful reasoning tools in areas as diverse as medical diagnosis, law and corporate

planning. It is therefore timely to bring the field of AI under the spotlight.

The aim of this paper is to illuminate the culture of AI through an analysis of the discourse and style of thought of its exponents. As an illustration it draws upon the discussions surrounding the genesis and projected development of the fifth generation computer. It is argued that the idea of building an artificially intelligent machine - as embodied in the concept of fifth generation computing - cannot be divorced from the worldview of AI adherents and in particular is sustained by metaphors and ways of thinking which are rooted in technological determinism and a mechanical view of knowledge and thought.

The Turing Test - A Sociological Approach. Harry Collins, Science Studies Centre, University of Bath

The Turing Test is a test of the ability of a machine to pass as a human being; it is a test of interactive competence. The philosopher John Searle has argued that even if certain types of machine could achieve full interactive competence, we would not attribute them with intelligence if we knew how they worked. This is certainly incorrect, for there are many social circumstances in which machines that have the same *use* as intelligent beings will be attributed with the same properties - the same *meaning*. Early experiences with simple programmes such as "Eliza" bear this out. What is more, machines are likely to acquire more and more interactive competence of an Eliza-like quality.

Nevertheless, machines cannot acquire full interactive competence without being 'socialized'. Their apparent interactive competence can be explained but their lack of full competence will become less and less easy to demonstrate. The paper hypothesises the failings of all foreseeable computers when compared with socialized humans. It suggests ways in which these failings might be revealed in Turing Test-like settings.

Computer Software as Social Practice: Towards a Sociological Analysis of Copying and Compatibility. Steve Woolgar, Department of Human Sciences, Brunel, The University of West London

This paper offers a sociological analysis of the use of computer software. Users' perceptions of software capabilities are documented by means of a survey of popular and "trade" literatures and by drawing upon a small sample of interview responses. This data is used to establish some characteristics of users' knowledge about software capabilities, the ease and desirability of copying, the limitations of compatibility between different systems and so on. Particular attention is given to the generation and exchange of information about software capabilities. It is argued that users' "displays" of information about software capabilities can be usefully understood in terms of the circulation of mythemes within a particular language community. In line with recent themes in the social study of science, it is argued that the discourse of computer software can be understood as an important constituent of its technical character.

9:30 am-12:00 noon

Session 17 Sociology of the Biological Sciences

Problems in the Development of Cognitive Neuroscience: Expository Strategies in "Inter-Specialist" and "Intra-Specialist" Texts. Edward Manier, Program in the History and Philosophy of Science, University of Notre Dame

This is the second report to 4S of the results of a year's (1984- 85) observation of teaching and research in a center (a very close knit organization of ten laboratories in the psychiatric institute of a medical school) pursuing molecular and cellular explanations of the learning mechanism underlying Pavlovian conditioning of a higher invertebrate, the sea slug. In 1984, the center's director and an associate published a paper (Hawkins & Kandel, 1984) in a leading psychological journal summarizing major features of more than fifteen years of research and developing the theoretical implications of their findings for "higher order" conditioning protocols. "Higher order" conditioning has provided the focus for sophisticated computationalist/cognitive versions of the Pavlovian program in recent psychological literature (Rescorla, 1978; Wagner, 1983; Mackintosh, 1983; Dickinson, 1980; Pearce & Hall, 1980). The authors and the audience of Hawkins & Kandel, 1984, can be distinguished as biologists seeking compositionalist explanations (Shapere, 1984), on the one hand, and psychologists seeking computational/representational explanations (Gardner, 1985), on the other. This report tests the findings and further develops the views of Whitley and Shinn (1985) concerning "inter- specialist" communication through a comparative critical analysis of the *arguments, images and referents* of an inter- specialist paper in which molecular biologists address psychologists, Hawkins and Kandel, 1984 with corresponding elements in two intra-specialist papers by neo-Pavlovian psychologists, Wagner, 1983, and Pearce and Hall, 1980.

Prosopographical Study of the Nineteenth Century British Naturalists. Philip Rehbock, University of Hawaii at Manoa
This paper reports on a prosopographical study of the British naturalists of the 19th Century: an attempt to discover if there are significant patterns in the social origins, education, travel experiences, subspecialties, and publishing accomplishments of the zoologists, botanists, and geologists born between 1770 and 1860, and to correlate these patterns to their beliefs about, and conversions to evolutionary theory, geological uniformitarianism, and other key issues of Darwin's era.

The Social Construction of Quantification in American Psychology, 1880-1930. Gail A. Hornstein, Mount Holyoke College

Throughout the history of science, quantification has been taken to be a key element in characterizing a discipline as scientific. Since few phenomena are inherently quantitative, every science has had to construct a conceptual and methodological apparatus by which to render its phenomena quantifiable. However, psychological phenomena have traditionally been seen as *in principle* not capable of quantification; hence, in terms of the criteria used by other sciences, psychology was inherently unscientific.

During the period 1880-1930, American psychologists embarked upon a concerted attempt to redefine psychological phenomena (e.g., perception, intelligence, personality, learning) to make their basic properties quantifiable so that psychology could become a legitimate science. This paper will analyze the complex social and historical processes involved in these efforts, as well as the means by which the shift toward quantification was accomplished, the functions it served, and the consequences it has had for the nature of research practice in the discipline.

The Development of Systems Ecology in the United States. Chunglin Kwa, Department of Science Dynamics, University of Amsterdam

Two institutions are of primary importance for the development of a new approach in American ecology which differed markedly from previous schools also aiming at the understanding of large ecological units. At the University of Georgia and the Oak Ridge National Laboratory, the ecological programs that were being developed from 1951 and 1954 respectively, were crucially dependent on funding by the Atomic Energy Commission. The AEC provided a context in which elements of what was later to emerge as systems ecology could be combined. First, a recognition of the importance of the 'trophic-dynamic' aspect of ecosystems, as set forth by Lindeman in 1942. Its theoretical concepts, like 'primary productivity' and 'mineral cycling', were thought to be relevant to managerial purposes. Second, radiation ecology, initially developed as a goal in itself, provided important methodologies to systems ecology. Third, mathematical modeling shaped some essential presuppositions of systems ecology. The resultant systems ecology struggled for recognition during the U.S. contribution to the International Biological Program. The IBP was administered by the National Science Foundation, and this in turn provided a context in which the grand schemes of management relevance gradually faded away.

Mission-Oriented in Ecology. Jacqueline Cramer, Science Dynamics, University of Amsterdam

The central aim of this paper will be to develop an understanding of the impact of the emergence of the 'environment' as a social and political issue on the dynamics of ecology as a scientific field. In order to operationalize this general research aim, the analysis has been narrowed down to the following questions: 1. Which options have been developed for orienting ecological research toward the solution of environmental problems and what do these options mean in terms of the research to be performed? 2. What social conditions influence the institutionalization of these various options of 'mission-oriented' ecological research within particular research settings?

While the first question will be discussed in terms of a general analysis of the programmatic ideas of ecologists in combination with their actual research work, the second question will be addressed on the basis of a case study of Dutch fresh-water ecology. The results of the overall analysis will then be evaluated from the perspective of the steerability of science.

Robert Knox and the "Resurrection Men" of the Anthropological Society of London. Evelleen Richards, Department of History and Philosophy of Science, University of Wollongong. The remarkably consistent and coherent anatomical, anthropological and political views of the Edinburgh anatomist, Robert Knox, are examined and their role in late Victorian scientific racism is assessed.

It is argued that after his death in 1862, Knox's theoretical views were pressed into institutional service by James Hunt, the racist founder of the short-lived, but politically vociferous, Anthropological Society of London (1863- 1871). Their adoption of Knoxian biology and anthropology not only underpinned their racism and reactionary politics, but gave Hunt and his followers the intellectual and institutional strength to resist incorporation into the Darwinian anthropological model proffered by Wallace and Huxley, and to offer considerable professional opposition to the takeover of London Science by the Darwinian new guard. It is concluded that the struggle between these rival bodies for scientific and ideological hegemony shaped the "new" anthropology of the 1870s, and that certain of Knox's theoretical views were thus perpetuated in late Victorian scientific racism.

Session 18 Practice of Mathematics II

Ordering the Universe: The Role of Mathematics. Arthur Jaffe, Harvard University

Concrete examples will be used to discuss how excellent mathematics, however abstract, leads to practical applications in nature. Hard problems in nature stimulate the invention of new mathematics. And it is impossible to predict just where an area of mathematics will be useful. An exciting development taking place right now is the reunification of mathematics with theoretical physics.

The Empirical Object of Theoretical Physics. Eric Livingston, Los Angeles, California

What makes up the adequacy of an argument in theoretical physics? The examination of a mathematical proof as a social object illuminates this question in two ways: it clarifies what theoretical arguments in physics are not; it provides an appropriate context for examining the adequacy of such arguments as situated accomplishments. The "empirical object of theoretical physics" refers to a way of working that is essential to and identifying of theoretical physicists' work practices.

Everyday Arithmetic and Advanced Mathematics. Jean Lave, University of California, Irvine

Everyday math practice has been given definition, pejorative but only hypothetical, in opposition to an idealization of the practice of mathematics by mathematicians. The one is supposed to be concrete, informal, intuitive (and inadequate); the other abstract, formal, and logical. Anthropological research on the math-in-practice of grocery shoppers, Weight-Watchers, and school kids, shows that they strongly agreed with traditional characterizations of their math, while their everyday practice belies the description. These observations together with the descriptions of the activity of working mathematicians suggest an alternative view of the similarities and differences between different forms of math practice.

Adding Up The World: The Culture of the Calculus. Martin H. Krieger, University of Southern California

What are the features of the calculus and of the world over which it claims dominion that allow them to work together, so as to create a sense that the calculus is suitable for the world? The calculus is a way of adding things up in an invariant way, an extension of ordinary arithmetic, in which there is independence of the parts that are added, in which the order of addition does not matter, and for which there is just one answer. (The Fundamental Theorem, Riemann sums, and "individuals.") Some of these issues are highlighted when we consider the ritualized use in physics of integration-by-parts or asymptotic approximation of phase integrals.

Session 19 Sociology of Technology II

Applied Science as Technology: The Case of Health Economics. Trevor Pinch, Department of Sociology, University of York

This paper builds upon recent work which argues that ideas developed in the sociology of scientific knowledge have relevance to the study of technology (e.g. Pinch and Bijker, 1984). The paper draws upon a study of an applied social science—health economics—which we have been carrying out over the past year (for initial findings see: Ashmore, Mulkay and Pinch, 1986, and Mulkay, Pinch and Ashmore, 1986). It is argued that the issues raised by the application of a social science can be treated in equivalent terms to those raised by the application of a natural science—including the development of technology. It will be shown that discussions over whether or not a technology 'works' are paralleled by debates over the practical applicability and use made of health economics. The conclusion is that although health economics does not involve any obvious 'machines' or 'material artefacts' it raises exactly the same issues as are addressed in the new sociology of technology (for the latter, see e.g. Bijker, Hughes and Pinch, 1986).

Deciding Technology. Knut H. Sorensen, Department of Sociology, University of Trondheim

Technological problems usually have several solutions. This implies that in research and development of technology, decisions are made to follow up certain solutions and neglect others. These decisions are of considerable importance, e.g., in regard to the impact of technology in the workplace or in the households, and they should consequently be analysed in terms of how decisions are made, what kind of criterias that are affecting the decisions, and how various social interests may influence the outcome. The paper will discuss these issues, based upon data from some very recent Norwegian studies: a national survey of young engineers, and a study of a large Norwegian research institute.

There are few previous studies in this field, but research into the engineering profession has drawn attention to the contradiction, observed by many engineers, between economic and professional criterias. This paper will delve further into this discussion, relating the findings in regard to criterias to the situation of researchers involved in technological R&D, their professional preferences, the degree of their freedom of research, how projects are funded and managed, etc.

Tube Contra Bulb: The Social Construction of Fluorescent Lighting and the Role of Economic Power. Wiebe E. Bijker, Twente University of Technology, The Netherlands

Electric fluorescent lighting is almost as old as incandescent lighting. But it took some 40 years before the search for a lamp which was 'not too small, too hot and too red' (like the incandescent lamp) did result in the stabilization of an artefact 'fluorescent lamp'. To understand this delay, it is important to take into account the oligopolistic structure of the electric lamp industry—for example as manifested in the existence of several cartels and patent licensing agreements. Thus, the case-study of fluorescence lighting offers an opportunity to explore the usefulness of concepts like 'economic power' in describing and analyzing the development of technological artefacts. I will argue that these concepts do not have much explanatory power. They beg the question: the economic strength of, for example, the General Electric Company should not be used as *explanans*, but has to be explained instead, if our objective is to understand the social processes of technological development and if we want to preserve the 'seamless character' of the web of history.

The Failure of a Successful Artefact: How the Svedberg Ultracentrifuge Conquered the Scientific World and Also Did Not. Boelie Elzen, Twente University of Technology, The Netherlands

In 1923, the Swedish chemist Theodore Svedberg designed an artefact, which he called 'ultracentrifuge' (UC), for his study of colloidal substances. In the early 1940s, after many years of development work, an UC of the Svedberg type was put on the market. Although Svedberg had obtained widely acclaimed scientific results with his UC and although he was a very well known and respected scientist (Nobel Prize winner in 1926), this ultracentrifuge became a commercial failure. However, in the late 1940s, in the USA, a completely different type of UC was marketed which rapidly found its way to the colleagues of Svedberg.

Using the case study of the ultracentrifuge, it is argued that it is not very fruitful to speak of success or failure of technological artefacts. If our objective is to understand the social processes of the development of artefacts then it seems to be more appropriate to talk about increasing and diminishing degrees of stabilization.

2:00-4:30 pm

Session 20 Pittsburgh and the Business of Industrial Science, 1886-1896: Studies in the History of Industrial Research

Engineering R&D at Westinghouse 1886-1896. Ronald R. Kline, IEEE Center for History of Engineering

The early history of the Westinghouse Electric Company presents an opportunity to examine the process of engineering research and development at a major electrical manufacturer before the advent of industrial research laboratories. Three examples of engineering R&D at Westinghouse will be discussed: the invention and development of a commercial alternating current system by William Stanley in 1886; the failed effort of Nikola Tesla and Charles Scott to develop a practical induction motor between 1888 and 1890; and the formulation by Benjamin Lamme of a successful technique of designing induction motors by the mid-1890s. Attention will be paid to similar work at Thomson-Houston and General Electric, the influence of European engineering, and the change in electrical engineering from cut-and-try methods to techniques based on engineering theory.

Alcoa Laboratories: Impetus and Direction in Incremental Innovation. Bettye H. Pruitt, The Winthrop Group

Since its organization in 1919, Alcoa Laboratories has made a number of well known innovations in both process and product technology, including the Combination Process for refining low-grade bauxite, continuous direct-chill ingot casting, and a host of high-strength alloys for aerospace applications. At the same time, however, Alcoa research has produced innumerable incremental innovations that, taken together, have played a less visible but no less critical role in the company's technical progress. This paper will analyze various clusters of these smaller scale innovations—the improvement of aluminum paint and of oxide finishes for aluminum; improvements in lubricants and lubrication technology in the plants; and the application of new research technology, such as lasers, radioactive tracers, the quantometer, and the analog computer, to

fabricating and quality control. In comparing them to the big-hit projects, aimed at achieving a major breakthrough, the paper will address the question of what motivates and guides the application of science to industrial concerns.

The Origins of Industrial Research in the American Steel Industry: The Development of U.S. Steel Corporation's Research and Technology Department, 1928. Paul Tiffany, The Wharton School, University of Pennsylvania

This paper will detail the development by the United States Steel Corporation of a central research department. While such an organization was not formally created by the firm until 1927 (and not implemented until the following year), the roots of this outcome stem from an earlier period. Utilizing hitherto unopened historical documents in the archives of U.S. Steel's Technical Center in Monroesville, PA, the paper chronicles the events, discussions, and problems that led to the new research facility's creation. Moreover, it will also address why U.S. Steel—and by extension the entire domestic industry—was so late in entering into such an activity on a formalized basis. This latter topic will attempt to place the industry in perspective of other industries (and firms) that were leading the way into corporate research. Finally, a brief conclusion will outline the progress of this research function at U.S. Steel over subsequent years.

3:00-5:00 pm

Session 21 Science and Public Policy

Scientific Evidence and Public Policy Decisions. Jane C. Kronick (Sociology), and Robert J. Dostal (Philosophy), Bryn Mawr College

Elise Boulding has characterized American society as sharing a widespread belief in the ability of science to "reduce uncertainty and provide control." In public policy for environmental hazards, science and scientific evidence is a primary source of appeal both to reduce uncertainty regarding what the public should do and to provide a means of control over the extent of public responsibility. Yet the use of scientific evidence in public policy decisions has been a growing source of public controversy, particularly in areas involving nuclear power or hazardous wastes.

Few studies have examined the interaction among policy issues, values and scientific evidence in policy disputes. This study, funded by the EVIST program of the National Science Foundation, adds to the work of Mazur and Shrader-Frechette, among others, by documenting the interaction among lack of clarity on governmental responsibility, reliance on legal metaphor to understand policy issues and placement of an irrational burden upon scientific evidence to resolve problems of policy preference. This study focuses upon the role of scientific evidence in Congressional hearings on hazardous and radioactive wastes. Using the techniques of close textual analysis and the skills of an interdisciplinary team crossing the social sciences and humanities (law, sociology, philosophy and comparative literature), we have examined and identified the critical issues under debate, the metaphors dominating the understanding of policy issues in this area and the role played by scientific evidence.

Lack of consensus on the allocation of responsibility for public costs is linked to a search for means to limit public responsibility. Legal metaphor, particularly "burden of

proof," provides an understanding of the problem allowing such limitation. Science is viewed as providing the evidence to establish such "proof" despite the lack of either an appropriate ecological perspective or time orientation in the public perspective. Expectation that traditional scientific evidence can answer unambiguously complex issues of policy preferences places an irrational demand on science. Such demand assumes a uniform scientific method, single interpretation of data and capacity to generalize from limited data to unspecified future events. This contrasts with the differing perspectives of scientists trained in different methodologies and occupying social roles differing in responsibilities and resources. Attention is drawn to the unwillingness, in the absence of criteria of governmental responsibility, to utilize simple and unequivocal sources of information, such as the toxicity of chemicals, as a basis for policy decisions. Rather certainty is sought from complex data gathered in less than ideal circumstances, often using techniques designed for other purposes, a search which is resolved without clarity either with respect to "evidence" or policy preferences and sets the stage for later public controversy.

Risk Analysis as Science and Policy. Lee Clarke, Sociology, UCLA

Risk analysis is one of the fastest growing fields in science. Chemists, psychologists, engineers, even geographers are all contributing to this burgeoning area of inquiry. As important, formal risk analysis is increasingly utilized in making policy, both public and private. Yet there is not that much scholarship on the social aspects of risk analysis.

My paper will develop two main themes. First, what were the major social conditions that facilitated the rise and proliferation of formal risk analysis? Preliminary analysis suggests that the science of risk originated as an organizational response to changing norms concerning corporate responsibility, and to various social movements that began with Rachel Carson's *Silent Spring*.

Starting in the early 1960s laypeople started to understand highly specialized languages, technical details, tort law, resource mobilization, and bureaucratic organization. In addition, Ralph Nader's publication of *Unsafe at Any Speed*, and the widespread acceptance of it, signalled important changes in conceptions of corporate responsibility. The first million dollar award for product liability in 1962 symbolized a shift toward using the legal system to hold corporations responsible for hazards they create. (In 1962 there was a single million dollar award, eight in 1970, 26 in 1975, 134 in 1980, and 360 in 1983.)

The corporate response to this change was the institutionalization of the explicit, formal risk assessment as a method for allocating benefits and costs. Used in this form, the risk assessment is an addition to the array of tools bureaucracies have to resolve competing demands. One of the more celebrated examples was the Ford Motor Company's use of formal risk analyses in deciding to produce the burning Pintos. The second theme I will develop is the ways in which the science of risk analysis is used in public policy. On the one hand is the argument that risk analyses can provide value-neutral evaluations of policy options. If all alternatives, hazards, and benefits can be quantified into a formal model, this reasoning holds, then optimal choices are more likely to result. On the other hand is the argument—based on social theory in general and organization theory in particular—that even if optimal choices exist, they are not likely to benefit all of society. Rather, the choices that flow from a formal risk analysis are likely to be biased by a variety of social forces. Moreover, although there is certainly merit in clearly defining alternatives, hazards, and

benefits, social theory suggests that risk analyses are as often used to justify risks that an elite or an organization has already defined as acceptable. This view does not argue that risk analysis is not a science, but sensitizes us to the fact that science can be a tool in the service of many masters.

Ideals of Science in the Humanities and Their Ethical and Policy Implications. Aant Elzinga and Sven Andersson, Department of Theory and Science, University of Gothenburg
The point of departure in the paper is some empirical work in Sweden based on interviews and an analysis of several debates on the role of researchers in the arts and humanities. This is a question posed in the light of the challenge presented by rapid developments in the natural and engineering sciences as well as technological impact in society. What will be the prospects and critical tasks of researchers in the arts and humanities in the future high tech society? Sweden is an interesting case because of its clear-cut interventionist policies and articulate opposition on the part of researchers in the humanities. We will on the basis of a model regarding a number of different ideals of science discuss our findings in the project thus far and present for discussion alternative views and standpoints regarding the issues underscored in the title of the paper. The title of our project is "Knowledge and Visions: Contemporary Ideas on the Humanities in Science and Politics."

Why Science is No Use to Policy. David Collingridge, Technology Policy Unit, Aston University
A sceptical view about the role of scientific research in policymaking is defended from current aspects of the sociology and the philosophy of science, together with a number of case studies, including lead in the environment and educational testing in the USA and UK. Consensus building within science can only occur under conditions of a low degree of criticism, autonomy of researchers and well maintained disciplinary boundaries, all of which are immediately broken when scientific research attempts to be relevant to policy. Policy issues throw up research problems which are not only multidisciplinary, but also extraordinarily difficult to solve. In addition, actors whose interests are threatened by such results become, quite properly, very much more critical of them than they would ever be to theories in pure science. Instead of limiting the political debate, scientific research degenerates into endless technical squabbles under these conditions. Research cannot be used to legitimate policy decisions; scientists have no privileged role in decision making.

Session 22 Medicine and Medical Policy

Clinical Practice and Preclinical Research: The Influence of Diagnostic Laboratories on Research in Bacterial Variation, 1918- 1939. Olga Amsterdamska, Department of Science Dynamics, University of Amsterdam
Diagnostic bacteriological laboratories in hospitals are various public health agencies constituted a primary audience for much of the bacteriological research conducted during the first decades of the twentieth century. These laboratories offered bacteriologists not only employment and some research opportunities, but also gave them access to the clinical material necessary for their work and constituted a testing ground for research results. Consequently, scientific reputations in bacteriology were de-

pendent not only on the evaluations of the academic research community, but also on the judgments of practitioners—physicians and pathologists—working in clinical bacteriological laboratories.

This paper will investigate the cognitive effects of such a "multiple reward structure" on the development of research in bacterial variation during the 1920s and 30s. It will be argued that the needs and demands of clinical laboratory practitioners affected not only the process of evaluating bacteriological research (by introducing to it criteria of utility, efficiency, simplicity and diagnostic relevance), but that they also constrained the selection of research problems, and the development of theoretical frameworks and research techniques in bacteriological research conducted at the universities and in research institutes. The different theoretical frameworks used to explain and study bacterial variation will be compared in terms of their responsiveness to the not always consonant medical interests of the various types of bacteriological laboratories.

The "Ethical Compact" Governing the Investigator-Subject Relationship in Human Experimentation. Kathryn S. Arnow, Washington, D.C.

The classical ethical dilemmas of physician/investigators in clinical trials stem from their societal obligation to shift to a strictly regulated relationship with the human subjects, in return for the opportunity to scientifically test their particular hypotheses.

Broadening the usual two-party perspective, this paper views the investigator-subject relationship in the context of the many-sided *ad hoc* ethical compact that governs a drug trial. Formed by the respective requirements of the Biomedical Research Agency, the Drug Regulatory Agency, the investigators' Peers, the host Medical Center, the Human Experimentation Monitor, and relevant others, the compact sets the ethical constraints of the experiment and channels needed resources to investigators, in exchange for their compliance.

Critical issues explored include:

- the process by which the compact shapes the risks assumed by the informed consenting subject;
- the effect of the trial's dispersion of the investigator's ethical responsibilities on the underlying patient-physician relationship.

The Context of Cognitive Change in a Research Programme in Obstetrics. Anja Hiddinga, Department of Science Dynamics, University of Amsterdam

Recent developments in the sociology of science have increasingly made problematic the actual content of scientific knowledge: some claiming that changes taking place at the cognitive level should be understood in relation to their social context. Scientific knowledge has more and more come to be seen as 'socially constructed'. In the many studies, from many perspectives in contemporary sociology of science (ethnomethodological, strong programme) attention has largely been directed at the formal and physical sciences: seemingly the strongest test of constructivist claims. Social and life sciences have received little attention.

The complexity, and publicly contested nature of the interactions between medicine and its social context make this an interesting and accessible field of investigation for the sociology of science. In addition, medical research is tightly linked with its context through medical practice. Clearly this is true for the clinical disciplines, where both the practice and the research are institutionalized in the

hospital. In this sense the clinical researcher - as simultaneously oriented to treating diseases of the individual patient and attempting to contribute to the advance of medical knowledge, embodies the dual nature of clinical science.

This intimate connection leads to the consideration that, as a working hypothesis, medical practice provides the relevant context for the pursuit of clinical research. While at the same time the organization of such research - in the clinic - provides a structuring of that context. Structured medical practice is therefore of central explanatory significance in relation to research programmes in clinical science. This is not to say that factors in the broader society are not of significance for clinical research, but that for the most part their significance will be exercised through the mediation of medical practice. To accept this, it is not necessary to deny the vital importance for clinical research of developments in the basic biomedical sciences (in developmental physiology for example, or biochemistry, or immunology). The conceptual and theoretical resources upon which the clinical scientist may in principal draw are constantly expanded through work done in these other fields. So too are the technologies available to him.

The paper focusses on a research programme in obstetrics, carried out in the Yale Medical School in the 1920s and 30s. This programme entails research on X-ray pelvimetry, the roentgenological measurement of pelvis of pregnant women in order to predict complications at the time of delivery. Its development will be described, in the context of changes in the medical practice paralleling this programme.

The Emergence of Reproductive Science in the U.S., 1910-1940: Conditions and Consequences. Adele E. Clarke, Tremont Research Institute

Not a single English-language book was devoted to reproductive science until Marshall's *Physiology of Reproduction* was published in Britain in 1910. Yet by 1940, reproductive research had both emerged and coalesced as a recognized and established line of scientific work in biology, medicine and agriculture and preeminence in the field had shifted to the United States. This paper examines the social and scientific preconditions of the emergence of reproductive science and their consequences for its organizational and problem structures c. 1910-1940.

Social preconditions included the birth control, eugenics and neo-malthusian movements which developed a legitimating public forum on human reproductive issues between c. 1900-1920. Economic conditions promoted industrialized approaches to animal agricultural production, requiring enhanced control over animal reproduction.

Biomedical scientific conditions also altered in directions supportive of reproductive science including the development of the "new biology" with its physiological and biochemical approaches and the discovery of hormones as "chemical messengers" regulating bodily processes. Second, the resolution of the enigma of heredity led to what I term the trilateral segmentation of subdisciplines within the biological sciences and the emergence of newly-bounded lines of work of genetics, developmental embryology and reproductive physiology. In each of the three professional fields—biology, medicine and agriculture—the reproductive system was a new territory, a frontier for research. Alliances were developed across the three professions building the reproductive research enterprise as an intersection of efforts and grounding the work and scientific problem structures in a nexus of mutual and compatible concerns and research markets.

Sunday, October 26

9:30-11:30

Session 23

Work Organization in Science and Technology

Linking Concepts with Work Organization: An Analysis of a Natural History Expedition and Its Ensuing Publications. Susan Leigh Star, Tremont Research Institute, and James R. Griesemer, Department of Philosophy, University of California, Davis

The relationship between work organization and published theories has large, unknown components. Among these, the problem of demonstrating relationships (if any) between the course of institutional change and the history of ideas for a particular scientific domain is central.

In this paper we will trace the form and content of research reports on the problem of speciation within a single research program to institutional arrangements which govern the conduct of research. The program of ecological and biogeographical research established at the Museum of Vertebrate Zoology, University of California, in the early twentieth century is important both for its contribution to the developing evolutionary synthesis and for its central role in more regional problems of biology. We discuss the roles that the local configuration of audiences and patterns of work organization play in guiding the formulation and solution of research problems.

We develop these themes in an analysis of a single expedition mounted in 1911 by the Museum of Vertebrate Zoology and of the ensuing research reports to illustrate the nature of the data needed for a comparative institutional history of a single scientific field. Data is drawn from archival material - notebooks, correspondence and institutional records - and published secondary sources.

Work Organization and the Research Process. Sierp Zeldenz, Department of Science Dynamics, University of Amsterdam

A central theme of most modern social studies of science is the determination of the influence of 'external factors' on the development of science and research. In this paper it is contended that it is fruitful to do such studies on the level of research groups (rather than individual scientists), and over longer periods of time (5-15 years) thus implying a methodology of reconstruction rather than of participant observation. Research is reported on a comparative study of 6 research groups in macromolecular chemistry, based on detailed oral and written material. The focus is on the relation of research goals, programs, strategies and processes with the 'organizational environment' of the groups. One aspect concerns differences in the relation of 'core research' (either theoretical or technological) with more short term and applied research.

An extended version of the "garbage can" model of organizational choice (March and others), in which concepts concerning environment and strategic action are included, is used as conceptual framework. Building on that special attention is paid to the relation of the groups with different fields of practice (industrial, agricultural, medical) and the implications of this for the research process.

Scientific Labor and Monopoly Capital: Managing R&D in the Corporate Laboratory. Daniel Lee Kleinman, Jack Kloppenburg, Jr., Leslie Oakes, Suzanne Ogilby, University of Wisconsin- Madison

In the modern era science has become a prominent productive force, and the scientist has emerged as a producer of pivotal economic importance. Marxists have addressed the problem of the class position of scientists but, despite a rich corpus of labor process studies, have largely neglected the analysis of scientific work regimes. In this paper we explore theoretically and empirically the nature and constitution of the labor process in corporate research laboratories. There are certain similarities between the labor processes of industrial workers and scientists employed in the private sector. Capital must secure adequate labor performance from the scientist and simultaneously obscure who it is that benefits from that labor. However, the unique characteristics of "knowledge production" present a particular set of obstacles to the extension of systematic control over scientific labor processes. We analyze three principal strategies which corporations have used in an attempt to overcome these obstacles. Further, we explore the patterns of conflict between scientific labor and management now being manifested in companies engaged in "biotechnology" research.

1:00-3:00 pm

Session 25 Social Influences on the Development of Scientific Disciplines

The Interdisciplinary Origins of Scientific Disciplines. Henry Etzkowitz, SUNY-Purchase and Lois Peters, New York University

The multi-disciplinary sources of new scientific disciplines that have emerged in the twentieth century such as molecular biology, polymer science and computer science have been noted. The creation of a new discipline through convergence and synthesis of elements of existing disciplines will be examined through a study of the disciplinary background of a sample of computer scientists. Hypotheses will be offered to explain the differences and similarities between 19th and 20th century modes of discipline formation.

Emigres and New Disciplines: The Case of the 1930s Refugee Scholars. Paul K. Hoch, Science Policy & Innovation Research Unit, University of Warwick, England

This paper will attempt to shed light on the special role of refugee migrations, "brain drains" and other international movements of scientific and scholarly personnel in scientific growth, particularly as regards the development of new scientific specialties and disciplines such as theoretical physics, applied mathematics, bio-physics and molecular biology in Britain and America. Attention will focus on the set of scholarly migrations following the rise of Nazism in Germany and other continental European countries after 1933.

To the extent that a scientific specialty or discipline is bound together by the rules of an existing paradigm defining the rules for its "normal science," it has been argued by Mulkay, Chubin and others that major scientific change often comes from outside the existing specialist group—not untypically through the "migration" into the group of innovative outsiders. In the special case where the innovative outsiders are also internationally renowned practitioners in their own right, perhaps displaced by political upheavals, it may be that they are particularly well qualified to synthesize the approaches of their native and

host cultures, and overcome resistance to the new scientific innovation. At any rate, that is a thesis which will be considered here. Examples will be chosen from both the physical and social sciences, as well as various specialties within history and philosophy, including the history, philosophy, and sociology of science.

Problem Paths: An Analytical Tool for the Social Construction of Scientific Problems. Joan H. Fujimura, Tremont Research Institute

This paper proposes paths as a concept for analyzing the processes by which scientists go about constructing and solving problems, and it provides a scheme for mapping problem paths. A problem path is the course taken by scientists as they work around contingencies, take detours which become long-term or permanent changes in direction, decompose problems, or abandon them and switch to new problems. Several types of problem paths and their properties are discussed. The problem path concept goes beyond scientists' reconstructions of their research processes, which include theoretical or technological barriers as common roadblocks in problem construction. It incorporates all of the activities and conditions involved in constructing doable problems, such as lack of markets, other resource constraints, mistakes, and unattended articulation work. Thus, the concept also elucidates discontinuities in problem construction and the strategies scientists employ in transmuting from one problem to another. The data are drawn from a study of molecular biological research on cancer. The approach is drawn from the sociology of work.

The Sociology of Philosophy: Toward a Triple Level Theory. Randall Collins, Department of Sociology, University of California, Riverside

The methods of the sociology of science are applied to explaining the historical appearance and intellectual contents of movements in philosophy. A triple-level theory is proposed, which attempts to build on the strong points, while avoiding the weaknesses, of internalist and externalist approaches to the history of ideas. Intellectuals work out of problematics, and create new polarities, in the realm of ideas themselves; this is especially so in a field like philosophy, which explicitly separates itself from the empirical contents of the sciences. But these ideas themselves are social, insofar as they constitute a medium of discourse, meaningful to a particular social network of intellectuals. These intellectual networks, acting over time, shape and transmit cultural capital to particular persons, as well as shaping the emotional energies (ambitions, enthusiasms) which issue in creativity. These networks constitute the innermost of three social levels. These networks arise only at particular historical times and places, as allowed by the second level, the organizations within which intellectuals make their careers and obtain material resources (especially educational and religious institutions). Historical movements of philosophical creativity thus take place especially when there are major periods of organizational struggle and change in education and/or religion. Finally, there is an outermost level, the political and economic context of the larger society, which shapes what sort of educational and religious organizations exist. It is a fallacy to reduce the content of ideas to a reflection of the economic structure of the entire society; this is only one indirect level of social influence, in addition to the mediating structures of organizations, and the intimate social structuring and restructuring of the intellectual field. The argument is developed with reference to the movement of idealist philosophy in Germany, 1780-1830.

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Coordination of United States Research Programs: Executive and Congressional Roles

Albert H. Teich

American Association for the Advancement of Science

Editor's Note: In 1984, the Science Policy Task Force of the Science and Technology Committee of the United States House of Representatives raised a broad set of questions it wanted to explore over the next two years. Among them were the following: "How is the Federal Government organized for the conduct and support of research? Is the pluralism of many support agencies working effectively in practice? What are the roles of the OSTP and the OMB in coordinating Federal science funding and avoiding wasteful duplication?" A background paper on these topics was subsequently commissioned by the Division of Policy Research and Analysis of the National Science Foundation. This article consists of excerpts from that paper.

In the full version, a first section described coordination and management of research in the top six United States research support agencies. The second section, presented here, moves into an area of federal decisionmaking which is less familiar to readers of S&TS, budgetmaking in the White House and Congress. The description provides an antidote to any simplistic notion that scientific communities in the United States control their base of federal support.

The federal budget process is the central arena in which priority decisions are made in basic research, as the programs of the research support agencies are proposed, reviewed, and acted upon in the context of—and in competition with—all other federal programs. Two agencies in the Executive Office of the President play pivotal roles in this process—the Office of Management and Budget (OMB) and the Office of Science and Technology Policy (OSTP). The operations of these two agencies with regard to federal basic research are discussed in the first section. The role of Congress is discussed in the next section.

Before discussing OMB and OSTP, however, it should be noted that program planning and decision-making often begin outside of the budget process in reviews of progress and research needs in various disciplines, generally conducted by the National Academy of Sciences (NAS). While the recommendations of these groups are not in any sense binding on the agencies to whose programs they relate,

the agencies frequently follow their guidance in framing program plans and budget proposals. These disciplinary studies are usually known by the names of the individuals who chair them. Examples include the Bromley Report in physics, the Westheimer and Pimental Reports in chemistry, and the Field Report in astronomy.¹ Each study is intended to reflect a consensus view of the leading figures in the field of its research needs and priorities. The studies examine such factors as the "ripeness" of different subdisciplines and problem-areas for breakthroughs or rapid progress, the potential applications of progress in these subdisciplines to practical problems, and the capabilities of relevant portions of the research community in terms of facilities, instrumentation, and human resources.

Other organizations, including the American Association for the Advancement of Science (AAAS), disciplinary societies (such as the American Chemical Society, the Institute of Electrical and Electronics Engineers, and the American Psychological Association), and university associations, as well as prestigious individual scientists and engineers are also influential in shaping budget and program outcomes, prior to and during the Executive Branch budget formulation process, as well as during Congressional consideration of the budget. Competing

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priorities and external budget constraints, however, generally yield programs and priorities somewhat different from those originally envisioned by any of the actors involved.

Executive Office of the President

The Office of Management and Budget. The primary function of the Office of Management and Budget is to prepare a consolidated budget for the federal government on behalf of the President. Every January the President submits this budget, which is actually a proposal covering the federal fiscal year that begins on the following October 1, to the Congress, and the Congress uses it as the basis for making appropriations (which may, of course, depart considerably from the President's proposed budget). The President's budget serves many functions in the United States governmental system. Principally, however, it is a plan for the operations of the government, reflecting the relative priorities of different activities and programs as seen by the President and his administration.²

In preparing the budget, OMB makes estimates of economic conditions and government revenues, allocates spending limits to the various agencies and reviews agency proposals for consistency with the President's priorities. In this process, research and development (R&D) programs are considered among the great variety of other programs that the government conducts. Thus, responsibility for basic research programs of different agencies is divided among various units of OMB.

The basic research programs of the six major research support agencies are parcelled out as follows: the programs of the Department of Defense (DOD) fall under the National Security Division, which has overall responsibility for the DOD budget, and which is organized into branches for each of the services. The basic research programs of the National Institutes of Health (NIH) are reviewed by the Health Branch of the Health and Income Maintenance Division. Basic research in the Department of Agriculture (USDA) comes under the Agriculture Branch of the Natural Resources Division.

One unit of OMB—the Energy and Science Division—has special responsibilities for R&D and basic research. This division reviews the budgets of the National Science Foundation (NSF), the National Aeronautics and Space Administration (NASA), and the Department of Energy (DOE). In addition, it also prepares, as an addendum to each year's budget, a special cross-cutting analysis of R&D and basic research in the entire federal government. Although this cross-cutting analysis is not prepared until after most decisions on agency R&D programs have already been made, it does sometimes allow the central

management of OMB (in conjunction with OSTP, as discussed below) to make marginal adjustments in agency R&D programs in order to balance support of research among fields or to deal with special situations that cut across agency lines and interests (such as the need for instrumentation for academic laboratories). This analysis, known in recent years as "Special Analysis K" of the federal budget, is a basic source of statistics on federal R&D funding and an important policymaking tool.

One other unit of OMB plays an important role in basic research funding, particularly as it relates to academic institutions. That unit is the Financial Management Division, which is under the management side of OMB—i.e., the associate director for management (in contrast to the units just described, all of which are on the budget side). The influence of the Financial Management Division derives from its responsibility for setting OMB's policy for reimbursement of indirect costs under research grants to colleges and universities. This policy is a source of recurrent controversy between OMB and the scientific community. The controversy has surfaced again during the past several months with an OMB proposal to limit these reimbursements.

OMB's budget review procedures are, on the whole, informal. They are the product of a tradition that has evolved over many years and is continually modified in response to changing conditions and the interests and policy directions of different administrations. In addition, the level of detail at which programs are scrutinized differs substantially among the various agencies and their respective overseers in OMB. For example, the National Security Division, responsible for the entire \$285 billion (Fiscal Year (FY) 1985) DOD budget (including \$32 billion for R&D and \$851 million for basic research), has a professional staff of only 28 people while the Energy and Science Division, whose purview—NSF, DOE and NASA—is less than one-tenth the size of DOD's budget, has a professional staff of 19.³

The overall size of an agency's budget, the size of its basic research programs, and the relation between the two are important factors in determining the form and extent of review that the basic research programs receive, both in OMB and in the Congress. The details of NSF's basic research programs receive more careful scrutiny in the lower levels of OMB and in Congress than many other agencies, because almost all of NSF's budget is devoted to basic research and thus its details are exposed to view rather than buried in a larger package. As a \$1.5 billion independent agency in a trillion dollar budget, however, NSF as a whole does not command much high-level attention. In fact, under normal circumstances, NSF's budget does not even rate a formal review by the director of

OMB. Its budget is generally fixed by a memo at the level of the cognizant OMB associate director.

NASA, DOE, HHS, USDA, and of course, DOD are much bigger agencies and do receive a director's review in OMB. However, in most of these cases, the basic research programs are relatively small elements of large budgets and thus are not examined at the same level of detail as NSF. A partial exception to this is NIH, whose crucial role in the highly-visible and highly-political biomedical area makes it the subject of considerable attention. In recent years this attention has focused not so much on the scientific nature of NIH's research programs but on the agency's overall level of funding and the number of individual investigator-initiated grants (particularly new and competing grants) that that level of funding would permit.

The scenario that has been played out for each of the past several years is that NIH has requested a substantial increase. Its request has been trimmed back first by the Department, then by OMB prior to submission to the Congress. Congress has appropriated considerably more than the administration requested for NIH. And OMB has subsequently sought to rescind some of the NIH appropriation, or otherwise reduce its spending level, while at the same time OSTP has pointed with pride to the growth in federal funding for basic research, part of which is attributable to the Congressional increase in NIH.

Consideration of NIH's budget by OMB and Congress is further complicated by the strength of NIH's several external constituencies, and by the fact that—although it is small relative to the total HHS budget, it is one of the few sizable discretionary elements within that budget. Interestingly, it appears that because of these factors and the way OMB is organized, there is little coordination between the NIH budget process and the other major elements of the federal basic research structure.

Both OMB and Congress treat the DOD budget quite differently than they treat the civilian agencies. Not only is much of DOD's budget classified, but there are also practical limits on the number of items that outsiders or even the expert staff members in Congressional committees have the time and patience to scrutinize. The budgets for DOD R&D are presented in more than 850 line items (averaging more than \$25 million each), many of which are classified. All DOD basic research (the contract portion of which is rarely classified, as a matter of policy) is divided into only eight line items—one for extramural and one for intramural research in each of the three military departments plus an extramural program in DARPA (the Defense Advanced Research Projects Agency) and a small intramural allocation to the Uniformed Services University for the

Health Sciences. Priorities among specific basic research programs and disciplinary areas are generally decided within the context of the military department budgets, except where they are affected by Congressional earmarkings.⁴

Some internal coordination of basic research is facilitated by OMB's structure. As noted above, the NASA, DOE and NSF budgets are all handled by the Energy and Science Division, and the USDA budget is handled by the Natural Resources Division. The heads of these two divisions report to the same associate director. The proximity of individuals within these groups permits a good deal of informal coordination, particularly in areas where agency programs overlap or relate strongly to one another—e.g., high energy and nuclear physics (NSF and DOE), astronomy (NASA and NSF), and biotechnology (NSF and USDA).

Despite the fact that its domain includes less than half of the basic research budget, the Energy and Science Division is still looked upon by most observers as the critical element of OMB for basic research. The fact that it is the only unit of OMB to include significant numbers of scientists among its staff no doubt contributes to this impression. In addition, the division's responsibility for preparing "Special Analysis K" gives it the opportunity to articulate, on an annual basis, the nearest equivalent that exists in our system to an administration science policy. Regardless of the fact that, as noted above, the "Special Analysis" is prepared on the basis of reports submitted by agency officials after their budgets have been put in final form, the exercise still serves to focus attention on the totality of federal R&D and basic research. The figures provide a context for Congressional consideration of each agency's research activities and provide all of the actors involved in the budget process with a common baseline against which to assess budget trends and policy shifts as they affect R&D and basic research.

Office of Science and Technology Policy. The White House Office of Science and Technology Policy, headed by a director who is also the Science Advisor to the President, is the highest level science policy body in the Executive Branch. OSTP exists to provide the President and his closest staff advisors with reliable scientific advice on the many issues that arise involving science and technology. At the same time, the presence of the Office at the White House level helps assure that the special needs of science and the R&D community receive consideration in a manner that transcends the narrow interests of individual bureaucratic agencies.

Although the legislation that created OSTP envisioned an important role for it in long-range planning for United States science and technology, the nature of Presidential decision making and White

House operations has led the Office to devote more of its efforts to urgent short-term problems and to specific interests of the Science Advisor. In the current administration, OSTP has played important roles in promoting the Strategic Defense Initiative and the permanent manned Space Station (following Presidential decisions to pursue these efforts), but the Office has been less involved in collaborating with OMB in the review of agency R&D budgets, a function it often performed in the past.

OSTP has no formal role in the budget process. It can, however, exert influence in a number of ways: for example, as a technical consultant to OMB in the review of agency budgets; as an advocate or collaborator with various agencies in the development of proposals which are subsequently submitted to OMB in the budget process; or as a separate source of advice or advocacy to the President or the White House staff on specific programs. While some OSTP-OMB interactions are documented in the form of memoranda, many important ones are informal—telephone calls, personal visits and corridor conversations within the Executive Office Building, or OSTP-arranged introductions of outsiders to OMB staff.

OMB staff can call upon OSTP for technical advice in the review of agency budgets. The notation that OSTP supports an OMB staff position can lend weight to that position in, for example, briefing papers prepared for a director's review of an agency budget. OSTP can also call issues to the attention of OMB without being asked, if its officials feel strongly enough, or OSTP can stimulate agencies to develop proposals for new programs and incorporate these proposals into their budget requests.

Much of OSTP's work in the current administration has involved stimulating and helping to promote ideas that its director has regarded as particularly important. Its activities in support of the Strategic Defense Initiative have been widely reported. The office was also deeply involved in the development of NSF's program of multidisciplinary engineering research centers. In still another recent instance, OSTP officials helped to generate a multi-agency initiative related to supercomputers by encouraging the agencies to develop proposals, then working with OMB staff (for example, by arranging a briefing on the subject) to ensure that the proposals were reviewed by OMB in a coordinated manner.

OSTP played a somewhat different kind of role in regard to NASA's aeronautical research program, which OMB proposed for elimination in its first round of budget cuts in early 1981. By conducting a review of the program and producing a report that documented the program's achievements and virtues, OSTP gained a reversal of the OMB position and preserved the program.⁵ More recently,

however, OSTP has not been involved in OMB's annual budget reviews of the aeronautics program, and it has continued to thrive.

It is much easier for OSTP to take advocacy positions on specific programs than it is for OMB. Like OMB, OSTP's work must generally support the President's goals, but, unlike OMB, the science office does not need to be directly constrained by budget targets. Where OSTP's achievements are generally gauged—at least by outsiders—according to the initiatives for which it is responsible, OMB's performance is measured by its overall success in controlling federal expenditures. This divergence in roles inevitably creates a certain tension between the two offices. When times are good and budgets are growing, this tension is relatively easy for both offices to manage because there are fewer hard choices. When budgets are tight, the tension is more troublesome and tends to reduce the extent of collaboration.

The Role of the Congress

Overview. Congress exerts influence over federal basic research in several major ways: approval of overall budget levels for the federal government; review and approval of specific agency budget requests for basic research; setting policy on matters related directly or indirectly to basic research; conducting oversight investigations on research performance, policy issues and other matters related to basic research; and providing an overall "protective umbrella" for science over the years.

The "protective umbrella" role is perhaps the most difficult to define with precision, but it may be the most important. Since World War II, there has always been a relatively small number of Senators and Representatives who have maintained a keen interest in science and, in varying ways, exercised leadership roles. These individuals have never formally organized in a "science caucus," but through their respective positions they have acted as senior statesmen and legislative spokesmen for science. From both parties and all parts of the political spectrum, they have provided the Congressional footing for the enduring government-science partnership.

In setting overall budget levels for the federal government, the Senate and House Budget Committees take the lead, based on the President's budget submission to the Congress. In this part of the process, as discussed below, basic research is seldom considered explicitly. The programs and agencies containing basic research compete for resources with a wide range of governmental activities, and issues other than R&D (e.g., deficits, taxes, and military versus civilian spending) are usually at the center of the debates that take place.

The review and approval of specific agency budget requests, including those parts for basic research, are handled in a variety of ways depending on the agency concerned. Sometimes the process involves both legislative (authorizing) committees and the appropriations committees; sometimes only the appropriations committees are involved. Certain agencies, such as NSF and NASA, receive annual authorizing legislation for their programs, after which, in principle, the appropriations committees consider and pass their appropriations. Ostensibly, the authorization sets a ceiling and includes various policy provisions. In practice, the appropriations bill provides the "real money" and often includes such policy matters as the committee wishes. Other agencies, such as Interior and NIH, operate under permanent authorizations, and their budgets—as well as many related policy matters—are handled by the appropriations committees with little or no involvement by the cognizant legislative committees.

The Congressional budget process has evolved over many years. Appropriations came first and are still fundamental. An appropriations bill is essential to the continued operation of an agency. The notion of annual authorizations came later in Congressional history, as members outside of the appropriations committees sought greater influence over agency budgets and policies—sometimes resulting in tension and discord between legislative and appropriations committees. Authorizations may be important, but they are not essential, and agencies such as NSF have sometimes gone for several years without them.

The budget committees and their role of setting overall budget levels are a more recent innovation in the Congressional budget process. They have been superimposed on top of existing authorizing and appropriations processes. Congress is still in the midst of working out how to mesh the various elements of its budget and policy setting systems and procedures, and there is often confusion and tension surrounding the efforts to coordinate the activities of various committees and processes.

Policy is set at all levels—budget, appropriations and authorization—but at different levels of detail and with different breadth of coverage. The authorizing committees generally develop the more detailed expertise in the substance of agency activities—including basic research. However, when an appropriations subcommittee has an interest in a topic, there is nothing to prevent it from conducting its own legislative inquiry.

Overall, Congress must be understood as a complex set of interacting power centers. The rules are seldom clearcut or immutable, and they leave a great deal of room for discretionary actions on the part of committees and individuals. The informal influences

are as often as not more important than the formal procedures. It is in this context that decisions on the fate of basic research programs—like all other federal programs—are made.

The Budget Committees. The budget committees work on figures at a very high level of aggregation, the House rounding budgets to the nearest \$50 million, the Senate to a tenth of a billion dollars. They look at expenditures in terms of 18 "budget functions"—for example, National Defense, Health, Community and Regional Development, and Energy—which cut across agency lines so that policymakers can allocate resources according to the purposes that government activities serve. The functions are designated by code numbers. Function 250, "General Science, Space, and Technology," includes NSF, the space programs of NASA and the general science (i.e., high energy and nuclear physics) programs of DOE. While this function is often regarded as the key indicator of the status of basic research in the federal budget, it actually covers only about a third of federal basic research, mainly in physical and engineering sciences. Other budget functions containing significant amounts of basic research include National Defense (050), Energy (270), Agriculture (350), and Health (550).

Under the Congressional budget process, the budget committees of each house propose resolutions which set overall targets for revenues and expenditures and which allocate totals to each function. Each house approves a resolution and a final version is worked out in conference. Basic research programs—like most other programs—receive consideration only at a very gross level at this stage in the process. The share allocated to each function is weighed against other functions in building a total package that attempts to meet the nation's needs without exceeding deficit targets. The limits established for each agency (or in some cases program) within the function are arrived at with only a very broad picture of the agency's (or program's) substance and its priority relative to others. While R&D overall has received generally sympathetic and favorable attention in the budget committees during the past several years, no consistent attempts have been made at this stage to examine or consider the various elements of the basic research budget in relation to one another.⁶

The Appropriations Committees. Although the budget committees provide a framework within which Congress is able to see "the big picture" on revenues and expenditures, it is the appropriations committees that actually craft the legislation that gives the agencies money to spend. For historical reasons, the entire federal budget, as proposed by the President, is divided up into thirteen appropriations bills combining some of the smaller

agencies and dividing up the largest ones. These bills, which have little relation to the budget functions discussed above, include such amalgams as "Treasury, Postal Service and General Government," "Commerce, Justice, State, the Judiciary and Related Agencies," and "Labor, Health and Human Services, and Education."

The House and the Senate each have a Committee on Appropriations, and each is divided into thirteen subcommittees corresponding to the thirteen appropriations bills. The treatment basic research receives in this critical part of the budget process is far from coherent. The six key agencies fall under five separate appropriations bills and subcommittees: NSF and NASA are contained in the "Department of Housing and Urban Development—Independent Agencies" bill; NIH is in "Labor, Health and Human Services, and Education;" DOD is, of course, in "Defense;" USDA is in "Agriculture, Rural Development and Related Agencies;" and DOE's basic research programs are in "Energy and Water Development" (DOE's fossil fuel R&D programs, which do not contain basic research, are included in the "Interior" appropriations bill).

Little coordination takes place between the appropriations subcommittees responsible for the various bills, and that which does is concerned with issues of greater moment than basic research. The process is complicated by the fact that final appropriations reflect the results of compromises between Senate and House versions of a bill. Any coherence that a single house might attempt to give a set of appropriations measures would likely be lost as each is taken to conference separately and subjected to separately to negotiation and compromise with the other house.

In some years, when Congress has been unable to pass all thirteen appropriations bills by the end of its session, those on which action is incomplete have been bundled together in an "omnibus continuing resolution." Since it is a single piece of legislation, such a continuing resolution might seem to offer the possibility of coordinated treatment of functional areas such as basic research. Whatever prospects there might be, however, are more than overwhelmed by the chaotic atmosphere that generally surrounds Congressional consideration of the measure.

As they do in the deliberations of the budget committees, basic research programs must compete with essentially unrelated functions in the context of their appropriations bills. DOE's high energy and nuclear physics programs, for example, are considered by the same subcommittee that handles water development projects (rivers, dams and the like—often termed "pork barrel" projects). To the extent that there are overall practical limits on the size of that appropriations bill, trade-offs must be made between these

quite different types of activities. The appropriations for NSF and NASA are contained in the same bill that covers the Department of Housing and Urban Development, the Veterans Administration, the Environmental Protection Agency, and many other independent agencies.

Although in most cases they are relatively small elements in large appropriations bills, basic research programs often receive a significant amount of attention at this level. For example, as noted above, the House and Senate Appropriations Subcommittees on Labor-HHS have been for a long time the dominant Congressional forces on the NIH budget, exerting strong influence over the level of NIH funding and the shape of its programs. Only in recent years have the legislative committees, particularly in the House, succeeded in increasing their influence over basic biomedical research—an instance of the type of tension between appropriations and legislative committees mentioned earlier. Similarly, the ideas and views of the chairman of the House Appropriations Subcommittee on Agriculture, Rural Development and Related Agencies have been a major factor in determining the course of USDA's basic research programs for many years.

Of particular note is the role of the House Appropriations Subcommittee on HUD—Independent Agencies, whose jurisdiction includes NSF and NASA. This body has often given informed, sympathetic attention to basic research in its agencies. It has maintained close contact with the agencies as well as with the members and staff of the House Science and Technology Committee. Although the frequency of interactions has varied over the years, in many instances it has effectively coordinated its actions with those of the authorization process.

In some cases, the attention basic research has received in the appropriations process has been greeted with less than universal acclaim. In recent years, subcommittee or (more frequently) floor action on appropriations bills containing basic research has become an arena for adding-on or earmaking funds for university buildings and facilities not requested by the agencies and not subjected to peer review. This type of attention is more the exception than the rule, however. Although the appropriations committees are able to consider their agencies' budgets at whatever level of detail they wish, they generally do not possess either the expertise or the staff time to involve themselves in very deeply in a wide range of program specifics.

Legislative (Authorizing) Committees. Legislative committees are generally the most deeply involved in the substance of their agencies' programs. They often develop special expertise in these areas and become advocates for the agencies they oversee. Several of the key basic research agencies are subject to annual

authorizations for their programs, giving Congress an opportunity to review their activities in detail. The House and the Senate differ markedly in the way they are organized to deal with authorizations and legislative oversight of federal agencies, including those involved in basic research.

In the House of Representatives, the committee with broadest responsibility in R&D is the Committee on Science and Technology. This committee, which is organized into seven subcommittees, has legislative responsibility for a wide range of federal basic research programs, including those of NSF, NASA, and DOE. It also has special oversight responsibility for the overall federal civilian R&D enterprise. Responsibility for NIH is held by the Subcommittee on Health and the Environment of the Committee on Energy and Commerce, while jurisdiction over the R&D programs of the Department of Defense (including basic research) belongs to the Subcommittee on Research and Development of the Armed Services Committee. The House Agriculture Committee has jurisdiction over USDA's agricultural research programs, through its Subcommittee on Department Operations, Research and Foreign Agriculture.

On the Senate side, the committee with broadest jurisdiction for science and technology is the Committee on Commerce, Science and Transportation. This Committee is charged with responsibility to "study and review, on a comprehensive basis, all matters relating to science and technology..." Through its Subcommittee on Science, Technology and Space, the committee maintains legislative responsibility for NASA and the R&D programs of the Commerce Department. It also shares jurisdiction over NSF with the Committee on Labor and Human Resources. Labor and Human Resources, in addition, is responsible for NIH. Other committees with important responsibilities in basic research include Armed Services (DOD); Energy and Natural Resources (DOE); and Agriculture, Nutrition and Forestry (USDA).

The form and level of involvement of these Congressional committees and subcommittees in basic research differ substantially from one case to the next. As noted earlier, the primary Congressional power over NIH has resided for years in the House and Senate appropriations subcommittees. Only during the last year has the Subcommittee on Health (located in the Energy and Commerce Committee) been able to gain final passage of an authorization bill enhancing its influence over NIH programs at the expense of the Appropriations Committee.

Despite its special oversight responsibility for all federal basic research, the House Science and Technology Committee has exerted relatively little influence on NIH over the years. (A partial exception is the Committee's recent involvement in the

dispute with OMB over federal reimbursements to universities for indirect costs, a dispute that had its origins in HHS and subsequently grew to encompass all federally-funded academic research.) Its influence over basic research in DOD has likewise been minimal. On the other hand, the Science and Technology Committee has played a major role with regard to NSF, NASA, and DOE basic research and in other Congressional deliberations relating to issues of science and technology policy that cut across agency lines, including such areas as supercomputers, biotechnology regulation and development, technology transfer, national laboratories, university instrumentation, and science and engineering manpower.

In the Senate, the Committee on Commerce, Science and Transportation has, for the past several years, sought to wrest control over NSF from the Committee on Labor and Human Resources in order to broaden its influence over national science affairs. This jurisdictional dispute prevented passage of an NSF authorization bill for four years, but had relatively little practical effect on the Foundation's programs. The dispute was resolved last year with the two committees agreeing to share jurisdiction and authorization bill was passed in the fall, but the arrangement still seems rather awkward and the committees have not developed a substantial informal level of coordination and communication.

Overall, relatively little coordination takes place among legislative committees with responsibility for basic research in either the House or the Senate. In both houses, the biomedical and defense fields occupy large and powerful domains more or less distinct from other areas of basic research and have little incentive to work with the science committees. The tight schedules and high-pressure atmosphere with which Congress operates leave little time for staff to work with counterparts on other committees unless there is an immediate and pressing need. Coordination between House and Senate is sometimes better, despite differing styles and organizational structures, and (at the moment) different political parties in charge, because the counterparts face the eventual need to go to conference together. To the extent that issues bridging the interests of more than one agency in basic research are treated in the Congress, they are examined by science committees (most often in the House) under their general responsibility for the welfare of United States science and technology.

Concluding Observations

The portrait of the federal basic research system painted here is one of a diverse enterprise with many elements working largely independently of one another. The bulk of federal funding is disbursed by

six agencies with very different missions, organizational structures, and traditions. OMB management is geared to the structure of the agencies in which the research is embedded, rather than to the logic of the research itself. OSTP, in theory capable of taking an overview position relative to the research enterprise, has, in practice, only limited influence over budget outcomes. Congress, with two chambers and three levels of budget and policymaking in each, is fragmented by nature—and by design—in the way it treats issues, including basic research funding.

Despite this relative lack of central planning and coordination within the federal government's basic research funding apparatus—or perhaps, as some would argue, by virtue of it—United States basic science is generally healthy, indeed in many areas, robust. To some extent this is due to internal working-level coordination among program managers in the research supporting agencies. To an even greater extent, however, it is due to coordinative mechanisms that operate outside the government, in the scientific community. The research institutions themselves—universities, national laboratories, and others—and the mechanisms of communication that serve the research community—journal publication, scientific meetings, committee activities in professional associations and academies, research collaboration, and informal communication—all provide means of interrelating the activities of federal research funding agencies. This field-level coordination is in essence the glue that holds the federal government's basic research programs together—within scientific fields, and to some extent across fields.

This paper has sought to portray the federal structure for funding and coordination of basic research. Of necessity, it has paid relatively little attention to the influences of the larger scientific community on the nature of government research. A fuller understanding of the United States research system requires that federal role be examined in this broader context.

NOTES

1. National Academy of Sciences, Committee on Science and Public Policy, *Physics in Perspective* (Washington, DC: NAS, 1972 [Bromley Report]); National Academy of Sciences, Committee on Science and Public Policy, *Chemistry: Opportunities and Needs* (Washington, DC: NAS, 1965 [Westheimer Report]); National

Research Council, Commission on Physical Sciences, Mathematics, and Resources, *Opportunities in Chemistry* (Washington, DC: National Academy Press, 1985 [Pimental Report]); National Research Council, Assembly of Mathematical and Physical Sciences, *Astronomy and Astrophysics for the 1980s* (Washington, DC: National Academy Press, 1982 [Field Report]).

2. For a discussion of the federal budget process as it relates to R&D see Willis H. Shapley, *Research and Development in the Federal Budget: FY 1977* (Washington, DC: American Association for the Advancement of Science, 1976) and Willis H. Shapley, Albert H. Teich, and Jill P. Weinberg, *Research and Development: AAAS Report VII* (Washington, DC: American Association for the Advancement of Science, 1982), chapter 6. See also, Philip M. Smith, *The National Science Board and the Formulation of National Science Policy* (Washington, D.C.: 1981), especially chapter VI, "The Funding of R&D: Budget as Policy."

3. Charles B. Brownson, ed., *1985 Federal Staff Directory* (Mount Vernon, VA: Congressional Staff Directory, Ltd., 1985).

4. For a discussion of the difficulties of analyzing the DOD budget for R&D, see chapter 6 in Willis H. Shapley, Albert H. Teich, and Gail J. Breslow, *Research and Development: AAAS Report VI* (Washington, DC: American Association for the Advancement of Science, 1981). It should be noted that in 1986 five new extramural line items were added to the DOD 6.1. budget, one each for the three services, one for DARPA and one of the Office of the Secretary of Defense. These are known collectively as the "DOD University Research Initiative."

5. Office of Science and Technology Policy, *Aeronautical Research and Technology Policy* (Washington, DC: OSTP, 1982).

6. One exception to this occurred in 1984, when the House Budget Committee did its own R&D cross-cut as part of its effort to formulate an alternative budget plan.

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See You All in Pittsburgh?

I was so underwhelmed by your responses to my first "President's Page" that I am tempted just to give you the reference and tell you to read it again. Come to think of it, that's not so bad an idea! See *Science and Technology Studies*, Vol.4, No.1 (Spring 1986), 33. But seriously . . .

Our Program Committee, with its *troika* of co-Chairs (Ruth Schwartz Cowan, Steve Cole, and Elizabeth Garber, of SUNY-Stony Brook), seems to have done us proud for the Pittsburgh meeting. The fare in prospect is most tempting: it looks like being a memorable occasion. Congratulations all round. But there is also business to discuss at Pittsburgh. A General Meeting is billed, and I am anxious that this will offer you all a chance to express your views fully and freely. There are important decisions to be taken, and Council cannot take them effectively unless you turn up and speak (or drop me a line . . . see below).

What will be on the agenda at Pittsburgh? I mentioned many of the issues in my last Page, but here is my present checklist, with comments:

1. We have to review the effects of the increase in membership dues, and decide on the dues for 1986/87. Have we overpriced ourselves?
2. We will seek opinions on the success of *Science & Technology Studies*, and on its future development. I hope that my admiration for Susan and Daryl's work to date is widely shared: but there will be no resting on laurels.
3. We have to agree details of the 1987 Meeting, in Worcester, Massachusetts. Is 13-15 November a suitable date? Will our Australasian members be able to attend? Is \$70 too high a charge for accommodation?
4. We have to agree on preliminary arrangements for our next "away fixture" Meeting, in 1988. Some of you have reservations about meeting in Israel, but we hope to be able to put a case (and costings) at Pittsburgh that will enable a decision to be taken. Has anyone an alternative?
5. And there there is the 1989 Meeting. Chicago has been proposed as the venue. Any comments? Dates?
6. The 4S Secretariat is now located at Bloomington, where Tom Gieryn is installed in the new position of Secretary/Treasurer: we will need to confirm that this arrangement is to our liking.

But discussion at Pittsburgh will obviously range more widely than this. We will be announcing there the results of our balloting for next year's influx of office holders: there will be scope for discussing the election process, the conduct of the Society's business by Council and the Committees, and the health of 4S democracy in general. I expect more talk about preserving the status of the Bernal Prize. I will be nagging away about fundraising. I also intend to keep my ears open for innovative ideas about 4S activities (see item 7 of my last Page). And I expect Al Teich to be around with *his* ears open for news of the effects of financial cutbacks on 4S/SSS/STS/HPS groups on US campuses. (Sounds of battles elsewhere will also be heard.)

In short, in the odd moments when our academic program fails to hold your attention, there will be plenty to talk about—and, what is more to the point, a *forum* in which such talk can be translated into action. I hope you will all take advantage of this opportunity. See you there. Meantime, I am happy to read letters (at 25 Gilmour Road, Edinburgh EH16 5NS, Scotland, UK) and receive phone calls (tel. 031-667.3497).

David Edge

Joseph Ben-David, 1920-1986

Joseph Ben-David died January 12, 1986, in Jerusalem after a long illness of cancer. He was one of the pioneers of the modern sociology of science. His passing has saddened all those who knew him, as well as those who knew him from his works.

Ben-David was born in Gyor, Hungary, on August 19, 1920, and migrated to Israel in the late 1950s. According to remarks which he made to me some years ago, he was employed for a while as a printer. He studied at the Hebrew University in Jerusalem, at a time when there was no sociologist on the faculty. He recalled that his sociology teacher was the existentialist theologian Martin Buber, who taught the courses labelled "sociology" at the time.

Ben-David eventually became the George S. Wise Professor of Sociology at Hebrew University, jointly holding appointment as the Stella M. Rowley Professor of Education and Sociology at the University of Chicago. He travelled and lectured extensively in the United States, Britain, and Europe. Among his visiting positions was one at the University of California, Berkeley in 1964-65, during which time I was his research assistant.

Professions, Education, Politics

Ben-David's work covered a number of areas. His first book was *Agricultural Planning and Village Community in Israel* (UNESCO, 1964). This might be considered an instance of bread-and-butter sociology. More central to his life's work were his extensive researches in the sociology of professions, education, and politics (and in the nexus among these topics). Especially notable here was his lengthy article in *Current Sociology* (1963) on professions in the class systems of modern societies, in which he compared the structure of professions arising from elite vs. mass educational systems, drawing attention to their consequences for the occupational and cultural stratification of the society, and for the degree of political radicalism or conservatism of the educated stratum. The political theme was developed more extensively in his paper "A comparative study of academic freedom and student politics," *Comparative Education Review* (1966) (a paper for which I was research assistant, and was generously listed as co-author).

Ben-David wrote extensively on higher education. His leading contribution being to stress the structural differences among the major types of modern university systems, and to trace them back to their historical roots. This resulted in several works commissioned by the Carnegie Commission on Higher Education: *Amer-*

ican Higher Education: Directions Old and New (McGraw-Hill, 1972; re-issued by the University of Chicago Press in 1981 as *Trends in American Higher Education*); and *Centers of Learning: Britain, France, Germany, U.S.* (McGraw-Hill, 1977).

Ben-David's distinctive contribution to the sociology of science can be seen against this background. His approach was to ground science in its social context, not so much in a free-floating climate of ideas, nor yet an economic or class system (though these might have their influences), but above all in the actual organizations within which scientists worked. His leading perception was that the "scientific revolution," or more precisely the series of scientific revolutions since the 17th century, were the result of institutional changes by which autonomous organizations arose. These institutions insulated intellectuals (at least some of them, and in some respects) from the ordinary political, religious, or other concerns of the surrounding society. The archetype of such an insulated structure was the modern research-oriented university, which emerged with the German university reforms of the early 19th century.

I suspect this was Ben-David's initial insight which led him to the sociology of science. Hence his influential papers of the early 1960s revolved around the theme that changes within the sciences, and especially the rise of new sciences, or periods of creative outpouring within a particular science, can be explained by organizational shifts in university systems. Thus his "Scientific productivity and academic organization in 19th century medicine" (*American Sociological Review*, 1960) attempted to show the relationship between the opening up and closing down of career opportunities for medical scientists in university positions, and the surges of innovation in biomedical science. The argument was extensively worked out in a master's thesis by Awraham Zloczower (Hebrew University, 1960). (A follow-up analysis by Turner, Kerwin, and Woolwine in *Isis*, September, 1984, largely bears out and extends the argument.) The related question, what determines these patterns of change in university organizations, was answered by international comparisons in Ben-David and Zloczower's paper, "Universities and academic systems in modern societies," *European Journal of Sociology* (1962).

Ben-David's vision was that institutionalized roles in organizations are the fundamental underpinning of scientific activity, and that whatever changed these roles, qualitatively or quantitatively, would affect the kinds and amounts of science that was done. In another paper of the same period, "Roles and innovations in medicine," *American Journal of Sociology*,

1960), he attempted to show that major innovations, such as those of Pasteur in microbiology and of Freud in psychoanalysis, were the results of "role hybridization." The combination of an orientation towards abstract knowledge coming from an academic role, with new materials and problems found in practical, non-academic roles, resulted in the creation of new scientific fields. Again, the pressures of academic careers are in the background; a crucial part of Ben-David's argument is that the shutting-down of career opportunities forced young academics into practical activities, while the prestige structure of academic superiority motivated them to maintain their intellectual orientation towards a generalizing science and thus reframe practical material as subjects of new sciences.

Later, Ben-David and I combined these several arguments to explain the origins and expansion of experimental psychology ("Social factors in the origins of a new science," *American Sociological Review*, 1966; a fuller version is my M.A. thesis, UC Berkeley, 1965). In this case, the role-hybridization is between high-status positions in physiology and declining prestige positions in philosophy within the mid-19th century German university system, career movement being induced by the relative over- and under-crowding between these fields. Using master-pupil chains as well as quantitative measures of career pressures and productivity, we attempted to show why social conditions for the emergence of experimental psychology existed in the German academic system earlier than in other European/American systems. In a volume produced by OECD in 1968, *Fundamental Research and the Universities: Some Comments on International Differences*, Ben-David went on to apply this structural reasoning to understand mid-20th century policy issues of fostering scientific productivity.

Scientist's Role in Society

Ben-David's most ambitious work is *The Scientist's Role in Society* (Prentice-Hall, 1971). Though this is a relatively small book, there is a tremendous amount of material compacted into its 200 pages. Ben-David had already been working on it for some time when I became his research assistant in 1964, and it was not finished until 7 years later. Initially it was to be merely a little introductory volume in the "Foundations of Modern Sociology" series; but in those days scholars were used to bootlegging their serious work into textbooks or other opportunities as they came along, and Ben-David made the most of it. (Recall, before the great expansion of university enrollments, and subsequently graduate enrollments, in the 1960s, the publishing business was a good deal more restrictive

than it is now.) Appropriately, the University of Chicago Press reprinted the volume as a scholarly work in its own right in 1984.

I believe that Ben-David's strategy was to work backwards from his home base, the 19th century world of German universities, in which the scientific role had achieved its most powerful organizational insulation and hence its strongest orientation towards "pure science." In previous periods, he searched for those at least partially equivalent structures that gave the scientific role its current degree of autonomy—which is to say, which constituted the scientific role *per se*. Ben-David found these bases in the earlier history of universities and schools; in the academies of the Renaissance (the topic on which he set me to do archival research in 1964); and ranging further afield from these organizational structures in which intellectuals found support for their careers, in the flux of religious and political movements that at various times (as in ancient Greece or in 17th century England) picked up science for use in ideological prestige and controversy.

Though *The Scientist's Role in Society* is not systematically theorized (something which I attempted to do in Chapter 9 of my *Conflict Sociology*, 1975), there are several leading themes. Science is not immediately affected by society; it is precisely its structural autonomy from everyday practical and ideological concerns that is crucial to the intellectual orientation of the scientific role. This autonomy, though, is based on organizations which provide the material means (and the cultural transmission) for intellectual careers; hence the ups and downs of scientific productivity, and the shaping of new fields, are determined by organizational dynamics and funding of the universities, academies, laboratories, and so on, which are the basis of science in that historical period.

Ben-David did not rest the productivity of science merely on the maximal insulation of science from the outside world, however; not only did he point to the spur to innovation provided by role-hybrids (Pasteur; Freud; earlier instances in 17th and 18th century science), but he argued that it was the structural openness of the American university system in the 20th century to practical/academic hybrids that accounted for much of its ascendancy to scientific leadership. He was more suspicious of political and religious intrusions into science (one might say, political/religious-intellectual hybrids), though on the historical record he was willing to admit that ideological movements sometimes fostered science for their own polemical purposes (as in 17th century England). But political/religious ideology is too fickle, he felt, to provide a strong and permanent basis for the scientific role. In Ben-David's view, science needs its own autonomous organizations if it is to have the internal dynamics, and the orientation towards knowledge of

some degree of "purity," which makes for scientific advance. Like a practical realist (or a practical politician!), Ben-David could see that this autonomy was always relative, and that too much of it was not a good thing. Judicious mixtures of practical interests, and even sometimes of ideological fervor, could help the scientific enterprise, as long as it remained a *mixture* and the institutional basis for scientific autonomy continued to be the ruling partner.

The Middle Ground

One may see that Ben-David occupied a middle ground on the issue of the social determination of science. On the one hand, he did as much as anyone to demonstrate, with systematic data and analytical comparisons, that scientific productivity and innovation are determined by social factors. Yet in his new Introduction to the 1984 edition of *The Scientist's Role in Society*, he argued against the "strong programme" and the constructionist themes of the last decade of science studies. Scientists discovered objective truths, they did not create them; the place of the sociology of science is to show the external conditions which made possible more discovery or less, in one direction or another, at a given time and place.

It is ironic, perhaps, that Ben-David gave an initial impetus towards constructionism by the success of his sociological theories. Furthermore, if one proceeds from the social determinism of progress in existing fields, to the creation of new fields, the path leads naturally to explaining the rise of subfields, and on inevitably to the level of particular theories, particular visions of the world—in short, to the social shaping of the content of science itself. Ben-David did not wish to go this far. For one reason, the key to the scientific role is precisely its autonomy, its socially-based ability to detach itself from external social pressures and hence to concentrate on the work of discovery. Without this, science is just another ideology; but historically, science emerged precisely as the autonomy from ideological structures. I suspect that Ben-David is at least partly right; though science is itself socially constructed (and socially constructing of its own contents), it remains a distinctive human activity because of its success in discovery. Ben-David's concept of

autonomy, and the concentric circles of social bases which it invokes, may well prove the key to further progress in the sociology of science.

In his later years, Ben-David did less work in the sociology of science. He was part of the generation working in the 1960s during which (to use a term Ben-David himself adopted for such phenomena) the field experienced its "takeoff into sustained growth." The organizational approach which he stressed has been upstaged by other styles of doing sociology of science, though a recent work like Richard Whitley's *The Intellectual and Social Organization of the Sciences* (Clarendon Press, 1984) is an impressive extension of the Ben-David tradition. A few years ago, Ben-David remarked to me that he thought the Society for Social Studies of Science indicated the success of the field, although he complained that it had settled down to a rather routine level of "normal science," turning out case studies which added detail but relatively little intellectual news. In the sociology of science itself, Ben-David was always looking for the excitement of discovery.

He gave the predominant impression of being averse to the intrusions of politics into the lives of intellectuals. During the student movement of the 1960s, his attitude was aloof. There are some times when you have to go to the barricades, he said to me, but those times don't come along very often. For the rest, he preferred that we get back to work. Yet there was an underlying streak of militancy in the man. He disparaged the trendy constructionist interpretations of recent Marxism, but declared that what there was of value in Marx was in the economics of *Capital*. He led an academic strike in Israel, when labor union issues escalated into what he declared was "a matter of principle." Apparently, there were times when he found it necessary to go to the barricades. There is a vivid portrait in Saul Bellow's *To Jerusalem and Back*: Bellow is being conducted around the city by his friend Joseph Ben-David, who is making an impassioned argument for why Americans must support Israel in its fight for survival. Right or wrong, Joseph Ben-David was a man of commitment. We will miss him.

Randall Collins
University of California, Riverside

Who Needs Needham?

From 16-21 May the University of Sydney hosted the Fourth International Conference on the History of Chinese Science. The event attracted delegates from the United States, Europe, Japan, and South East Asia as well as a strong contingent from China. However, the grand old man in this field, Joseph Needham, was absent. Needham's East Asian History of Science Library at the University of Cambridge has, over the past few decades, been the center of monumental documentation of the history of Chinese science and technology, published in many volumes as *Science and Civilization in China*. The assessments made by Needham and his collaborator Lui Gweijen of the significance of China in the building of the world's scientific and technological heritage provided the backdrop to much of the conference, with one session devoted entirely to a discussion of the "Needham puzzle." Even those at the conference who disagree with Needham's interpretation confess to having been profoundly influenced by his work.

The conference was arranged by Professor Ho Peng Yoke of Hong Kong University (who collaborated with Needham on the alchemical volume of *Science and Civilization*) and Professor Wang Gangwu of the Australian National University. The presence of Dr. Wang Ling, also of the Australian National University, meant that two of Needham's collaborators were present.

The delegation from China was headed by Professor Ko Tsun, a noted authority on China's ancient metallurgical technology from the Beijing University of Iron and Steel Technology, and Professor Li Peishan, deputy director of the Institute for the History of Natural Science in Beijing. Ko and Li had a singular advantage over their ten other colleagues in that they spoke fluent English. Communication was something of a problem, and so in the major seminars there was little contribution from the mainland Chinese delegates. There were nonetheless some interesting papers given by the Chinese delegation.

An historical overview of traditional Chinese medicine provided by Professor Cai Jingfeng from the Academy of Traditional Chinese Medicine in Beijing raised some interesting issues about the way China's indigenous medical practices had been encouraged by government policy and legitimized by scientific research since 1949. An issue which was not resolved was whether the traditional cosmology and physiological model of the body, which is characterized by notions such as the balance of Yin/Yang and the circulation of the Chi, will still remain

after acupuncture and herbal practice have been absorbed into China's dominant system of modern scientific medicine.

One of the useful features of the conference was the inclusion of public lectures. These tended to address the broader issues of Chinese science and its links to the West. Ho Peng Yoke gave an illuminating and entertaining paper on the links between China's traditional astronomy and astrology. The Chinese tradition of keeping records of major astronomical events is the longest and most extensive in human history and still provides fascinating and useful information for contemporary astronomers studying supernovas and the past behavior of comets. The reason for this long tradition stems from Chinese cosmology, according to which any unusual event in the heavens was considered as either legitimating or casting doubt upon the Emperor's "mandate of heaven" by which he held office. For this reason the department of astronomy in the Imperial capital was one of the most politically sensitive. The leaking of inauspicious observations was equivalent to a breach of national security.

One of the most interesting of the general papers was given by the Japanese historian of science Professor Shigeru Nakayama from Tokyo University comparing the emergence of scientific institutions in China, Europe, and Japan. A student and collaborator of Thomas Kuhn, Nakayama uses the notion of paradigms to explain the different style of learning and the reasons why modern scientific institutions and methodology developed in Europe rather than in China. An extensive examination of this issue can be found in his book *Academic and Scientific Traditions in China, Japan and the West* (University of Tokyo Press, 1984).

Possibly the most contentious issue raised at the conference came out of the paper given by Qian Wen Yuan, a physicist by training, who, after some unhappy experiences during the cultural revolution, left China to study in the United States. His first book (*The Great Inertia—Scientific Stagnation in Traditional China* [Croom Helm, 1985]) challenges Joseph Needham's assumptions that Chinese traditional scholars and technicians could have arrived independently at modern science via an alternative route to mechanistic reductionism. Qian, a confessed reductionist and positivist, regards the overvaluing of the achievements of China's traditional sciences as foolish and even dangerous.

Needham on several occasions states that he believes that China's traditional mathematicians and physicists, though lacking in Newtonian mechanics, could nonetheless have skipped this phase and

ended up with something approximating to quantum mechanics and relativity theory—that Newtonian mechanics was not an absolutely necessary phase for the development of modern science.

Qian questions whether the term "science can be used to include almost anything." Although he does not devalue the achievements of China's traditional culture, he argues that to describe its medicine, astronomy, or cosmology as scientific devalues the currency and meaning of the word "science." In this argument he did not fare well and was accused of trying to use physics as the measure of all science. His ultimate confidence in reductionism as the single most important aspect of modern science was greeted with incredulity. Yet his iconoclasm certainly stimulated the most intense debate of the con-

ference. It is a pity that Needham could not have been there to answer Qian. Even though Qian is considered something of a renegade as a historian, many of the Chinese delegates admitted to me that they also thought that Needham had, on the whole, rather overestimated the achievements of traditional Chinese science—modesty or realism?

No doubt the issue will continue to be thrashed out at the Fifth International Conference on the History of Chinese Science to be held in the United States. Any inquiries about the publication of the proceedings of the Sydney conference should be directed to Professor Ho Peng Yoke, Department of Chinese, University of Hong Kong.

John Merson
University of New South Wales

Our Thanks. . .

. . . to authors who contributed to our last issue for their willingness to put up with the inevitable problems which arise in a change of production process. We apologize for the many inconveniences they suffered, and for the introduction of mistakes into their texts. The Editors

Errata

In "Incorrectness and Specific Doubts: Comment on Shapere," by P. Thomas Carroll, the third paragraph on page 10 should have been indented, indicating a direct quotation.

In "The Sociology of Science in Its Place: Comment on Shapere," by Stephen Turner, on page 17, first paragraph, third sentence should read: "Even if it is conceded that theories which are *de facto* taken to be the best theories, or have become part of the given, are the best, on purely internal grounds, there remains a large range of variation in *prospective* judgments and expectations." On page 18, the last sentence of note 2 should read, "That redescrptions originating in these traditions should reproduce the conflicts between these traditions and positivism should surprise no one." In note 4, quotation marks should enclose the passage from Shapere: "extrascientific considerations . . . operative." In note 5, the first sentence should read ". . . there are consequences for what gets internalized. . ." rather than ". . . there are consequences stemming from what gets internalized. . ."

Awards

Congratulations to Adele Clark, Tremont Institute, who has been awarded the "Outstanding Dissertation of the Year Award" by the Medical Sociology Section of the American Sociological Association. The thesis, titled: "Emergence of the Reproductive Research Enterprise: A Sociology of Biological, Medical and Agricultural Science in the United States, 1910-1940," was written at the University of California, San Francisco.

The Third World Academy of Sciences is instituting a prize of \$10,000 to be awarded to the best research essay which brings to light the scientific achievements of a Third World scientist prior to the 20th century, whose work has not been hitherto clearly recognized. The research essay should summarize the major achievements of the scientist. It should indicate to what extent the contributions of the scientist were recognized within his or her community and should establish in what way the achievements are linked to the sources of modern scientific thought. Arrangements will be made for the essay to be published in the form of a book by the Third World Academy. The closing date for announcement of intention to present an essay is 30 October 1987. The Prize will be awarded in 1988. For further information contact the Executive Secretary of the TWAS at: TWAS History of Science Prize, International Centre for Theoretical Physics, P.O. Box 586, 34100 Trieste, Italy.

A New Unit

The Economic and Social Research Council of the United Kingdom (ESRC) is establishing a *Science Policy Support Group* which will provide policy makers with information and advice, and encourage research relevant to science and technology policy in the research council system and more generally within government.

The core staff of the Group, which will be housed from September 1986 at the Technical Change Centre in London, includes two 4S members: physicist-turned-sociologist John Ziman and Peter Healey, who had responsibility for science policy within the ESRC. The initial tasks of the Group will be to organize seminars which will bring together policy makers and researchers to discuss current issues in light of research findings, to set a research agenda, and to provide ESRC finance as the basis for persuading other funding bodies to help fund work on

that agenda. The Technical Change Centre is at 114 Cromwell Road, London SW7 4ES, UK.

Job Opening

The Science Indicators Unit at the National Science Foundation has an opening for an analyst. They expect to be able to recruit this fall for the position. For more information, contact Carlos Kruytbosch, Room L-611, National Science Foundation, Washington, DC 20550, USA, (202) 634-4682.

Newsletters

The *Pacific Circle Newsletter* has been started to link historians and other scholars whose interests are drawn to questions of the development of science in the Pacific, through "the Pacific Circle," formed in 1985. The Pacific region, for these purposes, includes nations which border on the east, west, and south of the Ocean. "Pacific science" is also taken in its broadest sense, encompassing both imperial and colonial scientific efforts and indigenous systems of belief about nature.

The newsletter will be circulated semi-annually to all interested scholars. Arrangements are now being considered for a two-day Pacific Circle meeting to be held at the East-West Center in 1987, and for a gathering at the 16th Congress of the Pacific Science Association in Seoul, Korea, 10-20 August 1987. For further information on the newsletter or other Circle activities, contact the editor, Professor Philip F. Rehbock, History Department, University of Hawaii, 2530 Dole Street, Honolulu, Hawaii 96822.

The *Social Psychology of Science Newsletter* (edited by Ron Westrum, Department of Interdisciplinary Technology, Eastern Michigan University, Ypsilanti, MI 48197 USA) is a publication of the Psychology of Science Subsection of 4S. To subscribe, send \$5.00 to Ron Westrum. He also welcomes contributions in the form of preprints, reprints, references, queries, etc.

Books

The Technology Studies Resource Center announces Volume 5 of its Working Paper Series, *Technology and Literature* (March 1986). Edited by Stephen H. Cutcliffe, the 96-page volume contains papers by Mark L. Greenberg, Lance Schachterle, and Joseph W. Slade, and commentary by Edward J. Gallagher.

Copies are available for \$6.00 each from the Technology Studies Resource Center, Lehigh University, Maginnes Hall #9, Bethlehem, PA 18015 USA, or call 215/861-3351.

Confronting Nature: the Sociology of Solar-Neutrino Detection is about an experiment that did not yield expected results. Drawing on interviews and correspondence, author Trevor Pinch builds up a detailed picture of how scientists cope with the unexpected. The study is unique in that it attempts to write the history of science before the scientific facts are available. 268 pages, Dfl. 110,000/L34.50. Order from Kluwer Academic Publishers Group, P.O. Box 322, 3300 AH Dordrecht, The Netherlands, or from your local Kluwer distributor.

The Development of Personal Construct Psychology by Robert A. Neimeyer applies Nicholas Mullins' model for the development of scientific disciplines. Neimeyer's tools for analysis include an evaluation of the style and volume of works published in the discipline over its first quarter century, the compilation of professional biographies of theory group members to examine their collegial and apprenticeship ties, and the construction of coauthorship maps that suggest the cohesiveness of the group's communication structure and level of disciplinary maturity. 176 pages, \$18.95. Available from The University of Nebraska Press, 901 North 17th Street, Lincoln, Nebraska 68588-0520, USA.

Other Publications

Michael J. Moravcsik, Coordinator of the International Workshop to Assess the Coverage of the Scientific Output of the Third World, has issued the final report of the Ad Hoc Task Force, *Strengthening the Coverage of Third World Science—The Bibliographic Indicators of the Third World's Contribution to Science* (July 1985). The project was supported by the U.S. National Science Foundation and the Rockefeller Foundation. For copies of the 16-page report, contact Michael J. Moravcsik, Institute of Theoretical Science, University of Oregon, Eugene, OR 97403, USA, or call 503/686-5207 or 5204.

Publishing Opportunities

Knowledge and Society: Studies in the Sociology of Science, Past and Present, a research annual published by JAI Press, invites manuscripts on topics in the general area of science and technology studies. Starting with the 1987 issue, the series will come under the editorship of Andrew Pickering, Robert Alun Jones, and Lowell Hargens. Longer manuscripts as

well as articles and research notes will be considered. Manuscripts undergo peer review. Four copies of each manuscript should be sent to: *Knowledge and Society*, Department of Sociology, University of Illinois, Urbana, IL 61801, USA.

A new series, "*Worcester Polytechnic Institute Studies in Science, Technology, and Culture*", aims to publish monographs, tightly-edited collections of essays, and research tools on interdisciplinary topics in the relationships of science and technology to social and cultural issues and impacts. The series is edited by Lance Schachterle and Francis C. Lutz, both of WPI, who invite proposals in English from scholars throughout the world. The series will be published by Peter Lang Publishing, Inc., NY. Please send a letter describing your proposed submission, plus a brief curriculum vitae, to: *WPI Studies in Science, Technology, and Culture*, The Projects Center, Worcester Polytechnic Institute, 100 Institute Road, Worcester, MA 01609, USA.

The Iowa State University Press *Series in the History of Science and Technology* invites manuscript submissions. The series is intended to enlarge publishing opportunities in the history of science and technology and increase public understanding of the nature of technological and scientific creativity and their relationship to one another and to the social, cultural, political, and economic context within which they arise. Contact Prof. Robert E. Scholfield, Department of History, Ross Hall, Iowa State University, Ames, IA 50011, USA.

Social Epistemology, A Journal of Knowledge, Culture and Policy is a new quarterly envisaged primarily as a reference tool featuring commissioned surveys (critical synthetic essays) and open peer commentary. The journal will provide a forum for philosophical and sociological inquiry. To contribute, contact the editor, Steve Fuller, at the Department of Philosophy, University of Colorado, Boulder, CO 80309-0232 USA. For a free sample copy, in North America write to Taylor & Francis Inc., 242 Cherry Street, Philadelphia, PA 19106-1906. Elsewhere write to Taylor & Francis, Ltd., Rankine Road, Basingstoke, Hants RG24 0PR, UK.

Sociological Forum is the new quarterly journal of the Eastern Sociological Society. The first issue appeared in Winter 1986. Edited by Robin M. Williams, Jr., the journal features innovative articles, review essays, and "notes and insights" that stretch the boundaries of social science. Occasional special issues are planned, including themes of interest to 4S members: "international conflict management in a nuclear world," "unanticipated consequences revisited," "change and controversy in sociology of

science." For information about manuscript submission and review, write to *Sociological Forum*, Department of Sociology, Cornell University, Uris Hall, Ithaca, NY 14853-7601, USA.

Archeomaterials is dedicated to the publication of studies of products and processes which influenced historical and social trends before the modern era. Contributions should place the technology within cultural perspective, rather than be purely descriptive. The first two issues will appear in September 1986. The editor is Dr. Tamara Stech, to whom manuscripts may be sent at: Department of Materials Science and Engineering, University of Pennsylvania, Philadelphia, Pennsylvania 19104.

Funding Opportunities

The *Guide to Federal Funding for Social Scientists*, sponsored by the Consortium for Social Science Associations (COSSA) and edited by Susan D. Quarles, describes over 300 U.S. programs of interest to researchers in the social and behavioral sciences and related areas of the humanities. The *Guide* includes program descriptions, funding priorities, and application guidelines, and features three contextual essays. The 512-page volume was published by the Russell Sage Foundation in July 1986 and distributed by Basic Books. The cost is \$19.95 for individuals (\$14.95 for members of affiliate organizations such as SHOT), \$24.95 for libraries and institutions. Orders, including payment or purchase order, should be sent to: Consortium of Social Science Associations, Department G-580, 1200 17th Street, NW, Suite 520, Washington, DC 20036 USA.

The *National Humanities Center* supports advanced study in history, philosophy, literature, and other fields of the humanities. Both senior scholars and those several years beyond the doctorate may apply for fellowships to pursue research and writing in residence at the Center. In addition, scholars from the natural and social sciences, the arts, the professions, and public life are welcome to apply. Applications must be postmarked by 15 October. For application materials write Kent Mullikin, Assistant Director, National Humanities Center, 7 Alexander Drive, Research Triangle Park, NC 27709, USA.

The *Columbia Society of Fellows in the Humanities* will appoint a number of post-doctoral fellows for the academic year 1987-1988. Fellows newly appointed must have received the PhD between 1 January 1985 and 1 July 1987. Deadline for receipt of applications is 15 October 1986. Application forms can be obtained by writing to the Director, Society of Fellows in the Humanities, Heyman Center for the Humanities, Box

100, Central Mail Room, Columbia University, New York, New York 10027, USA.

The National Endowment for the Humanities has a program of grants for important, original research in *Humanities, Science, and Technology*. The application deadline is 1 October 1986 for projects beginning after 1 July 1987. For more information, contact Division of Research Programs, Room 318, National Endowment for the Humanities, 1100 Pennsylvania Avenue NW, Washington DC 20506, USA. Telephone (202) 786-0210.

The *Woodrow Wilson International Center for Scholars* seeks proposals from individuals throughout the world for outstanding projects representing diverse scholarly interests in the humanities and social sciences. For academic participants, eligibility is limited to the postdoctoral level; normally, successful applicants have published one book beyond the Ph.D. dissertation. Fellows devote their full time to research and writing on one major project while in residence at the Center. The length of a fellowship can vary from four months to a year. Deadline for receipt of applications is 1 October. For information contact: The Wilson Center, Smithsonian Institution building, Room 331, Washington DC, 20560, USA.

Information on funding available from the *American Council of Learned Societies* may be obtained by writing to the Office of Fellowships and Grants, ACLS, 228 East 45th Street, New York, New York 10017.

Meetings Past

The Economic and Social Research Council (U.K.) *Newsletter* 57 (May 1986) features a section on the *ESRC Science Policy Conference* held at Wiston House, Sussex, in February 1986. This section, "A Mirror for Science," contains reports and commentaries from several participants. For single copies, contact the ESRC Information Office, 160 Great Portland Street, London W1N 6BA, UK.

Second in a series of seminars on the history of popularization and diffusion of sciences and technologies, a conference on *Amateurs in Science* was held at Cité des Sciences et de l'Industrie, 211 avenue Jean Jaurès, 75019 Paris, France. Session chairs included: Robert Fox (Lancaster University), Michel Callon (Center for the Sociology of Innovation, Paris School of Mines), and Yves Laissus (Library of the Museum of Natural History, Paris).

A session on *Science in Asia* was part of the First International Symposium on Cultural Interactions be-

tween Japan and other East Asian Countries, held 19-21 October 1985, in Sendai, Japan. A second symposium is scheduled for 29 August to 3 September 1986 in Sendai. For information on its results, write: Dr. Tadashi Yoshida, Associate Professor of the History of Science, Research Institute for Japanese Culture, Faculty of Arts and Letters, Tohoku University, Kawauchi, Sendai 980, Japan.

Memphis State University hosted a *Psychology of Science* conference held 17-18 April 1986. Topics included creativity and cognition as well as overviews of the field. A book is planned. For details, contact the conference organizers—Will Shadish, Art Houts, Bob Niemeyer, and Barry Gholson—at the Department of Psychology, Memphis State, Memphis, TN 38152, USA.

The *First International Mining History Conference* was held 27-30 August 1985 in Melbourne, Australia. This international and multidisciplinary meeting was conceived and organized by Peter Richardson (University of Melbourne) and Gill Burke (Polytechnic Central of London). The 85 participants discussed topics ranging from the economics and politics of the industry, to technical aspects of safety, to descriptive history, to migration patterns that mining produced. To obtain conference proceedings or receive the newsletter which was established at the conference, contact Kathleen Ochs, Humanities and Social Science Department, Colorado School of Mines, Golden, CO 80401, USA, or Peter Richardson, Economic History Department, University of Melbourne, Parkville, Victoria 3052, Australia.

AVISTA, the *Association Villard de Honnecourt for the Interdisciplinary Study of Medieval Technology, Science, and Art*, sponsored its first sessions and held its annual business meeting at the 21st International Congress of Medieval Studies at Kalamazoo, MI, 8-11 May 1986. The group is actively seeking to contact potentially interested new members. Please contact: Charles Stegeman, 2 College Circle, Haverford, PA 19041, USA.

The Achievement of Lewis Mumford, a four-day celebration of Mr. Mumford's 90th birthday, attracted over 1300 persons to the campus of the New Jersey Institute of Technology last October. The lectures, films, and art exhibit were capped by the showing of a videotaped interview with Mr. and Mrs. Mumford at their Dutchess County home.

Meetings Future

The Charles Babbage Institute is organizing, under the sponsorship of the Sperry Corporation, *Com-*

puting in the 21st Century: A Symposium on Computing and Society, Past and Future, to be held 9-10 September 1986 in Minneapolis, MN, in celebration of the 40th anniversary of the founding of Engineering Research Associates, Inc. For additional information contact the Charles Babbage Institute, 103 Walter Library, 117 Pleasant Street SE, Minneapolis, MN 55455, USA.

EASST Conference, Strasbourg, 29 September-1 October 1986. The fourth annual meeting of the European Association for the Study of Science and Technology will take place in Strasbourg under the auspices of the Council of Europe. Marcello Cini (Rome) will present the introductory lecture. The thematic structure for the conference includes:

Sensory Dimensions in Science (visualization, auditory dimensions, the body of the scientist, corporal senses in scientific practice, etc.); Consensual Levels of Scientific Practice (the laboratory unit; technological networks, interdisciplinary challenges, styles of research); and Making Sense of Science in Historical and Socio-Political Contexts (science policies, collectivization in science, popular presentation of science, ethical problems in science).

Special sessions include: Mustering Resources (the procedures and criteria for formulating and funding research proposals, in various countries, now and in the past, and their influence on the research process); High Energy Physics Since the War; *Cybernétique et sens de la science*; and Money, Writing, and Consciousness: seeking the specificity of science in its sensory origins.

For information on local arrangements contact: Josiane Olf-Nathan GERSULP, Université Louis Pasteur, 4 rue Blaise Pascal, 67070 Strasbourg Cedex, France.

The *British Society for the Philosophy of Science* will hold its annual conference at Newham College, Cambridge, 26-28 September 1986. Topics include creativity in science (speakers: Bohm and Briskman), reliability and method (Jardine and Papineau), the modularity of mind (Fodor and Lohrson-Laird), and prospects and promises of human sociobiology (Kitcher and Ruse). For information contact: Dr. D. H. Mellor, Cambridge University, Philosophy Faculty, Sidgwick Avenue, Cambridge, CB3 9DA, UK.

An interdisciplinary research conference on *computing and organizational issues relating to office information systems* will be held in Providence, RI, 6-8 October 1986. Sessions and panels include topics

such as model of the distributed office, organizational analysis, interfaces, and cooperation vs. automation. J. C. R. Licklider will give the keynote address. The program chair is Stanley Zdonik, Brown University. For information contact: Carl Hewitt, Laboratory for Computer Science, MIT, Cambridge, MA 02139, USA.

A conference on *Testing Theories of Scientific Change* will be held in Blacksburg, Virginia, 19-22 October 1986. For information on the program, registration and accommodation, and related transportation from Blacksburg to the four-society meetings in Pittsburgh, contact Rachel Laudan or Peggy Stewart, Science Studies Center, Virginia Tech, Blacksburg, VA 24061, USA. Telephone: (703) 961-7687.

The *Institute for Industrial Archaeology*, run jointly by the University of Birmingham and the Ironbridge Gorge Museum Trust, sponsors a series of short courses on topics related to museums and the history of technology. The courses, ranging from one to four days, extend through February 1986. For a complete program write Dr. Michael Stratton, Ironbridge Gorge Museums, Ironbridge, Telford, Shropshire TF8 7AW, UK.

The Inter-University Center of Postgraduate Studies in Dubrovnik will host a course/workshop in *Ethics and Sociology of Science*, on 13-24 April 1987. Course directors Aant Elzinga (Gothenburg), Thomas Brante (Gothenburg), and Henk Verhoog (Leiden) describe the course theme as posing the possibility and desirability of reconciling the normative-ethical and the sociological analysis of science. This theoretical interest extends to the social roles and responsibilities of sociologists of science themselves. Related themes to be covered include popularization of science, technology assessment, technocratic vs. democratic approaches, public fear, and whistleblowing. For further information contact Henk Verhoog, Institute of Theoretical Biology, University of Leiden, Groenhovenstraat 5, 2311 BT Leiden, The Netherlands.

The 1987 Summer Conference on *History, Philosophy, and Social Studies of Biology* will take place on the

campus of Virginia Polytechnic Institute and State University in Blacksburg, Virginia, 16-20 June 1987. These meetings are highly informal and seek to establish fruitful intellectual contact among biologists and individuals from any field of science studies working on biological topics. Suggestions for subjects and speakers for plenary sessions and workshops are welcome, preferably before 1 October 1986 and no later than 1 January 1987. Abstracts for work-in-progress papers should be submitted no later than 1 April 1987. Blacksburg is located between the Blue Ridge and Allegheny Mountains with a comfortable summer climate. It is accessible from Roanoke Airport or Interstate 51. Accommodations will be \$20 per day for room, board, and recreational facilities; the conference fee is \$6.50. Suggestions and inquiries should be directed to: Richard M. Burian, 1987 Summer Conference, Department of Philosophy, Virginia Polytechnic Institute and State University, Blacksburg, Virginia 24061, USA.

The organizing committee of an international conference on *bibliometrics and theoretical aspects of information retrieval* is calling for papers. The conference will take place 24-28 August 1987 in Diepenbeek, Belgium. A nonexhaustive list of topics includes: statistical methods for scientometrics, classical bibliometric laws, applications of citation analysis and network theory in libraries and university science policy, obsolescence, fuzzy and probabilistic information retrieval, and indexing theory. For further information contact Limburgs Universitair Centrum, Algemeen Secretariat, International Conference, Universitaire Campus, B-3610 Diepenbeek, Belgium.

Please note new addresses for both editors of *Science & Technology Studies*:

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MEETINGS CALENDAR

9-10 September 1986. Minneapolis. Charles Babbage Institute. "Computing in the 21st Century: A Symposium on Computing and Society, Past and Future." (See announcement this issue.)

17-19 September 1986. London. The Royal Institution. A RICHST Symposium, "Laboratories: The Place of Experiment." Contact Dr. Frank James, Laboratories Symposium, RICHST, The Royal Institution, 21 Albemarle Street, London W1X 4BS, UK.

26-28 September 1986. Cambridge, England. British Society for the Philosophy of Science Annual Conference. Contact Dr. D. H. Mellor, Cambridge University, Philosophy Faculty, Sidgwick Avenue, Cambridge, CB3 9DA, UK.

29 September - 1 October 1986. Strasbourg. EASST and GERSULP General Conference. (See information this issue.)

29 September - 4 October 1986. Florence. IUHPS Scientific Instrument Commission. VIth International Symposium on Scientific Instruments. Contact Paolo Brenni, Museum of the History of Science, Piazza dei Giudici, 50122 Florence, Italy. (055) 293493.

6-8 October 1986. Providence, RI. Conference on Office Information Systems. (See announcement this issue.)

19-22 October 1986. Blacksburg, Virginia. Conference on "Testing Theories of Scientific Change." (See announcement this issue.)

23-26 October 1986. Pittsburgh, PA. Society for Social Studies of Science (in conjunction with the History of Science Society, the Philosophy of Science Association, and the Society for the History of Technology). Annual Meeting. (See registration materials this issue.)

1 November 1986. London. British Psychological Society and British Society for the History of Science. Conference on "The Psychology of Science." Contact Graham Richards, The Psychology Department, North East London Polytechnic, The Green, Romford Road, London E15 4LZ, UK.

2-4 April 1987. Cincinnati Ohio. North Central Sociological Association 1987 Annual Meeting. The meeting will include a session on the sociology of science and technology. Persons interested in presenting a paper at this session should communicate with Richard Senter, Department of Sociology, Central Michigan University, Mt. Pleasant, MI 48849, USA.

13-24 April 1987. Dubrovnik. Course/workshop on "Ethics and Sociology of Science." Contact Henk Verhoog, Institute of Theoretical Biology, University of Leiden, Groenhovenstraat 5, 2311 BT Leiden, The Netherlands. (See announcement this issue.)

16-20 June 1987. Blacksburg, Virginia. Summer Conference on History, Philosophy, and Social Studies of Biology. Contact Richard M. Burian, 1987 Summer Conference, Department of Philosophy, VPISU, Blacksburg, VA 24061, USA. (See announcement this issue.)

24-28 August 1987. Diepenbeek, Belgium. Conference on bibliometrics and theoretical aspects of information retrieval. Contact Limburgs Universitair Centrum, Algemeen Secretariat, International Conference, Universitaire Campus, B-3610 Diepenbeek, Belgium. (See announcement this issue.)

Science & Technology Studies welcomes information about forthcoming events of interest to its readership. Deadline for submission of news for the fall issue is October 15, 1986.

Information for Contributors

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

The editors urge contributors not to be constrained by the format of traditional scholarly research articles in their submissions. Contributions may also take one of the following forms: letter to the editors, commentary on a published article, opinion column, or synthetic review essay.

Contributors should submit five copies of their manuscripts, three for reviewers and two for the editors. Two review processes are available: blind and open. If a blind review is desired, all indications of the author(s) should be removed on three of the copies. If open review is desired, the author's identity will be disclosed to the reviewers.

Contributions will be reviewed for interest to a multidisciplinary audience concerned with science and technology, for communication with that audience, and for quality of data and dependability of information. Verbatim comments of all reviewers will be forwarded to authors. If publication is approved, the names of the recommending reviewers may be appended to the article when it appears.

In deference to the many disciplinary writing styles represented in the Society, our referencing policy is pluralistic. References alone, references plus end notes, or end notes alone may be used. In format, the journal follows the *The Chicago Manual of Style*. In end note style, examples are:

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

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

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

In reference style, examples are:

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

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

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

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

Manuscripts may be submitted to either co-editor:

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