

4S Society for Social Studies of Science

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4S Society for Social Studies of Science

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1982 Presidential Address

The Politics of Knowledge Arnold Thackray*

The politics of knowledge has been a neglected area in the social studies of science. It is however an important one, and one which repays historical analysis. To set the frame for that analysis, we may suppose that seventy-five years represents the normal limit of memory, in the sense of an understanding of the world which may be transmitted between generations. And I would like you to journey with me through these limits of memory to five different historical moments, set 75 years apart from each other. I want to reflect with you on the politics of scientific knowledge today, in 1982, and also seventy-five years ago in 1907, and seventy-five years earlier still in 1832, and in 1757, and in 1682.

Being an historian, I like to travel forwards in time. So let us begin in 1682, just three hundred years ago, when a newly-elected Fellow of the Royal Society of London called William Penn was sailing up the Delaware, thinking of the City of Brotherly Love he hoped to create out of the swamps and mudflats. What of the politics of scientific knowledge in 1682? The answer can I think be simple. For all practical purposes, there were no politics of scientific knowledge in 1682. By way of illustration, the twenty-year old Royal Society of London was busy in that year electing Sir John Hoskyns as its President. Hoskyns' name, like Penn's is not one that you will find in any dictionary of science. Hoskyn's distinctions lay in his wealth, in his considerable legal abilities, and in certain traits of character. A contemporary recorded that

"there was nothing at all promising in his appearance; he was hard favoured, affecting plainness in his garb, walked the streets with a cudgel in his hand and an old hat over his eyes . . . when his spirits were elevated over a bottle, he was memorable for his presence of mind and quickness of apprehension. . ."

The world of natural knowledge was very small in 1682--perhaps a thousand individuals with even the degree of interest that Sir John Hoskyns or William Penn displayed, perhaps a hundred who might actually be able to read with a critical understanding material in the Philosophical Collections of the Royal Society (had those Collections not ceased publication that very year), or one of the two or three other journals that carried reports of original investigations.¹ What we may anachronistically label "the scientific community" had no coherent tradition of independent existence, and no identifiable concerns and resources of its own. If the politics of scientific knowledge existed, those politics were either purely personal (as in the quarrels of Isaac Newton and Robert Hooke) or best understood as a strand in the wide tapestry of the affairs of courts and monarchs (as in the quarrels

*Presidential address delivered at the eighth annual meeting of 4S at Philadelphia, Pa. on Friday, 29 October 1982.

of Newton and Leibniz, over the calculus). Perhaps in this latter sense we may say that a general politics of science was becoming apparent, in barest outline.

Move forward with me some seventy-five years, to 1757. Here in North America, the French and Indian War is in full swing. In the other Indies, Clive is winning his great victory at Plassey, and thus securing one bastion of the British Empire. In Paris, Louis XV is in the forty-third year of his reign.

Meanwhile the politics of scientific knowledge is a subject of small but enlarging meaning.

Courtly academices are popping up in every petty monarchy in Europe.² And in France especially, no provincial town seems complete without its assembly of savants. Already in Philadelphia too, in 1743, Benjamin Franklin has declared that "The first drudgery of settling new colonies, which confines the attention of people to mere necessities, is now pretty well over." Accordingly, Franklin has set to work to organize an American Philosophical Society.

By this time too, the development of Newtonianism in England, Holland and--latterly--France has shown the real power in what we have termed the general politics of scientific knowledge--that is the usage of scientific ideas and scientific claims to vindicate or criticize practices in society at large. This general politics of knowledge was an appropriate development for "Royal" societies, and for academies closely linked in style and purpose to the concerns of monarch or court. In contrast, what we may term the particular politics of scientific knowledge were far less developed.

If we may think of the general politics of scientific knowledge as the way in which scientific ideas are used in the workaday business of the ordinary world, then the particular politics of science are concerned with how apparatus and assistants, building and budgets, concepts and credits, fame and fortune, or resources and opportunities, are distributed among individuals, disciplines, and societies. This "internal" aspect of the particular politics of science has its matching externalities, in the uses that individuals, disciplines and societies make of their scientific assets and claims when negotiating for resources with other elements of the body politic.

So much for the theory. In practice, the particular politics of science had little meaning, in 1757. Individuals might intrigue, but that was part of the human lot. The community of savants itself still lacked that self-conscious sense of a separate existence, out of which a particular politics--internal or external--would be born.

Again, this is not surprising when we reflect on the scale and style of the scientific enterprise in 1757. Over the preceeding seventy-five years, the number of individuals with a passing interest in natural knowledge may have increased by an order of magnitude, to say 10,000. The core of those rarer individuals possessing a developed competence swelled to perhaps 1,000. But this increase in size of the "scientific community" is less impressive than initially appears. Remember that it developed mainly by multiplication

of societies scattered far apart (from Philadelphia to St. Petersburg), and in most imperfect communication with each other, rather than by growth and specialization within any given community. Thus Franklin's first Philosophical Society had already perished by 1757, showing that it was far easier to convene a novel gathering than to sustain a coherent interest in the content and politics of natural knowledge. Again, the Royal Society of London may have grown to over 300 members by 1757, but more than two thirds of those members were even less interested in science than Hoskyns and Penn had been. Of the around 100 Fellows with identifiable concern for the substantive content of science, over two thirds were physicians or surgeons.³ Insofar as a particular politics existed in London in 1757, it was the politics of medical rather than of scientific knowledge which was of paramount concern.

Let us move forward again, to 1832--to a world of steam engines and spreading democracy. Andrew Jackson is re-elected President following his nomination at the first-ever Democratic Convention. In Britain the Great Reform Bill becomes law. France lives in the aftermath of the July Revolution.

The character of natural knowledge is changing rapidly. If the community of those with some passing interest in science has swollen by another order of magnitude to 100,000, clearer lines of demarcation are beginning to appear between that larger community and the, say, 10,000 or so individuals who possess a definite scientific competence. This latter set is increasingly to be found in certain key cities, congregating in groupings which at once require and facilitate a differentiation of knowledge and expertise. This development was speeded by the appearance of what a speaker in a toast to the British Association for the Advancement of Science called "railway communication, and other improvements which tend to facilitate intercourse between mankind." The result is that the particular politics of scientific knowledge was taking on a vivid form and meaning.

The indicators of change are obvious. Here in Philadelphia in 1832 a flourishing Philosophical Society was but the central star in a galaxy of institutions ranging from the Academy of Natural Sciences through the Franklin Institute to the Phrenological, Geological and Horticultural Societies.⁴ If Philadelphia--or New York, or Boston--as bustling, growing cities could boast that much, London and Paris were the unchallenged centers. In London especially, there had sprung into being various powerful groups explicitly devoted to advancing one small, disciplinary segment of natural knowledge. Most obvious in their vigor, prestige and self-confidence were the Astronomical Geological, Linnean and Zoological Societies. Equally as striking as their vigor was the distress and trouble in the venerable Royal Society, as it sought to adjust to the changed politics of knowledge, and to the deliberate, reforming plots of the individuals who chose for themselves the curious label of "scientific men."

John Herschel for one was in no doubt about the changing nature of science. Contemplating the proliferating activities of the new British Association, in 1835, he wrote to William Whewell

"In what will this end! Or what will it get us if it goes on so from year to year? One evil . . . of such an enormous accumulation of reports, experiments, recommendations, discussions, etc., is the total destruction of mind and embarrassment of choice it occasions. . ."

Herschel's own answer was to "look in the index, hear one's own word, and shut one's eyes on all the rest." That, in 1835--along with the sad lament "Such is science nowadays. No man can hope to know more than one part of one science."⁵

In saying this to Whewell, Herschel had chosen the wrong audience, for William Whewell was perhaps the last of the polymaths--it was rightly said of him that "knowledge is his fort  , and omniscience his foible." Certainly Whewell published on a multitude of subjects from architecture and chemistry to mathematics, and from magnetism and tidology to theology and philosophy. He also coined the neologism scientist to describe those individuals who gathered for the meetings of the British Association, and took time out to become the English-speaking world's first serious student of the meta-sciences of science, publishing three volumes on the History of the Inductive Sciences and a further three on their philosophy.

The creation of the British Association for the Advancement of Science is the most obvious indicator of the changing position of scientific knowledge by the 1830s. Recall that in 1834 a meeting of the British Association would attract over 1200 savants to attend--a number that doubled to almost 2,500 by the 1838 Newcastle Meeting. Recall also that already in 1833 the members had the choice of six speakers at any one time; that a system of giving research grants by peer review was inaugurated that same year; and that the 1833 Meeting attracted 21 foreign visitors from cities as far apart as Paris and Philadelphia, Berlin and Baltimore (51 Americans alone visited BAAS between 1832 and 1837).

In such a context, the general politics of science took on new meanings and new energies. The very name of the British Association for the Advancement of Science implied a fresh ideology of science in relation to society. This point was understood by no one better than by John Henry Newman. In his British Critic it was correctly pointed out how, through BAAS, "physical science, the science. . . of matter and material things now arrogates in effect the same science to itself." The moral and human sciences--and above all theology, their Queen--had been dethroned by linguistic fiat.

More startling still was the development of the particular politics of science implicit in the BAAS. John Herschel rightly feared, though he scarcely foresaw, the tendencies inherent in what he called "a great, central, presiding power." One aspect of the internal politics of science was the elaboration by BAAS of a hierarchy of sciences, in which social approval was matched by financial largesse on a clearly graded scale. Section A--the approbation is in the label--was devoted to mathematics and physical science, and was the subject of 40% of the Association's official reports, and received 57% of the research money. At the far end of the spectrum Section F, Statistics, had to be content with 2% of the Reports and 5% of the funds. The particular politics of science were also well developed in their external aspects--some

19 lobbies of the British Government were mounted by BAAS in the 1830s. And one of those lobbies--a favorite project of Section A, needless to say, culminated in "the greatest scientific undertaking which the world has ever seen": the so-called "magnetic crusade." Clearly the politics of scientific knowledge had come alive, and had taken on new meanings by the 1830s.

Let us move on to 1907. The world is at peace, and prosperous. Oklahoma achieves statehood, signalling the end of the settlement of the continental United States; in Norway, women gain the vote; in Russia there are revolutionary outbreaks.

In science, Germany and Britain set the pace. Universities hold the key. The research school--unknown in 1832--has become a familiar phenomenon, and classical physicists are having their night thoughts. Names like James Clerk-Maxwell, Wilhelm Wundt, or Emil Fischer stand as symbols of knowledge as a serious, dedicated, organized affair. In 1832 science was a gentlemanly pursuit: the leaders of the British Association viewed with disdain those who depended on their science for their income. In 1907, science has become professionalized and specialized. The scientific career is no longer simply an affair of the mind; there is an instructive contrast between Charles Lyell who in 1832 was deep in his Principles of Geology, and about to marry and abandon paid employment, and Ernest Rutherford in 1907, equally immersed in his Radioactivity, but moving from a chair in Manchester in accord with a clear calculus of professional opportunity.

If science is professionalized and specialized, it is also much larger. Well in excess of 100,000 individuals now earn their livings by their science, if not by their scientific publications. The relations between this group and the wider community are also changing, for as science becomes more specialized it also becomes less accessible to the amateur (a word which itself takes on a new, pejorative meaning). The general politics of science are underdoing changes that are not well understood, while the particular politics of science take on a new intensity in a body so large and so diverse. An identifiable need arises for interpreters, popularizers and meta-scientists of science. It is these last who, in 1907, are the new amateurs of science. It would be an interesting study to identify them all--perhaps by 1907 there are already 100 individuals who give sustained attention to the meta-sciences of science. One thinks of the polymaths like Wilhelm Ostwald or Henri Poincaré. Or--in a far younger generation--of those individuals who will try to carve out specialisms in this field--Charles Singer in England, Georges Sarton in Belgium, Aldo Mieli in Italy, and Frederick Brasch in the United States.

In 1907 of course, the United States is still on the periphery of scientific affairs. But the very size of the American scientific community is becoming impressive on the world scene--the AAS had 8,000 members by 1910, and the American Chemical Society some 5000. And America is proving a congenial home for ideologies of progress. With the general politics of science thus set fair, decisions within its particular politics are being taken that will deeply mark the nature of American science through the twentieth century. The research university is consolidating its hold, and the PhD machine is being established. In 1907 some 350 PhDs were awarded in the United States--as many as 63 in a central science like chemistry. Johns Hopkins University alone granted 75 PhDs in chemistry in the 1890s.

Move on to 1982--to the world of the Falklands campaign, the Lebanese fighting, and the recurring crises in Poland.

The most striking aspects of science in 1982 are its internationalism, its size, and its close links to our economic and especially our military concerns.

The internationalism of science today is but a logical extension of that "railway communication, and other improvements" which were toasted in the 1830s. Work done in London, Ontario, is quickly known in London, England. New York to New Zealand is simply a matter of dialling correctly. Visits to, or visitors from, once exotic places have become a stable available to every leading scientist. This internationalism feeds the heightened competition and the intense specialization that are among the most obvious consequences of the huge, and largely increased, size of science. If we consider only the six scientific super-powers of France, Japan, the UK, the USA, the USSR and West Germany, then we find a labor force of well over 2 million "Scientists and engineers engaged in research and development."⁷

All available indicators today show the U.S. as the center of the world's scientific systems. For instance, the U.S. contributes around 100,000 articles a year, or perhaps one-third of the world total. The advantages and disadvantages that go with such a leadership position are obvious. On the one hand, there is the possibility of work in specialisms or on a scale not possible elsewhere--\$1/2 billion for one scientific instrument, or 125,000 members in a single-discipline scientific society (the American Chemical Society). The meta-sciences of science enjoy a correspondingly privileged position, numbering their American professionals in the thousands. On the other hand, there is the erosion of spirit that accompanies what Jerome R. Ravetz has labelled as "industrial science." Again, it is easy to admire that \$1 billion or 5% of U.S. Government funds devoted to R&D which goes directly on "the advancement of knowledge," but one is less certain how to respond to the more than 60%, or \$17 billion, which pours into "national defense and space."

The point I wish to make is that the politics of science have become far richer and more complex, and far less understood, in our day. The general politics of science has taken on new meanings and new energies in fields from sociobiology to scientific creationism. Meanwhile, the particular politics of science is notable for its variety and vigor. The internal politics of the competition among rival research schools, specialities or disciplines, is widely recognized but little-attended to except when some lucky Jim (Watson) offers a brilliant, amateur analysis. More curious still, the external politics of the accommodations between the various sciences and other groups within our culture remain unexamined.

I would like to suggest that the time is ripe for a new attention to the politics of knowledge, understood in the broad traditional sense of "who gets what, why." Such an enquiry into the politics of knowledge would obviously have to be a widely-focused interdisciplinary enquiry. It would engage--and challenge--the talents of economists, historians, philosophers, political scientists and sociologists. As such, it is a subject peculiarly fitted to be tackled by a society such as ours. There are however three additional, more compelling reasons, for attending to politics of knowledge:

1. If there is one thing that we have all come to agree on as a result of the debates and research of the last two decades, it is that knowledge is socially constructed. The politics of scientific knowledge in its general and particular forms is thus a key field of enquiry, from an intellectual or theoretical point of view. How resources are allocated, and how their allocation molds the very content of science are leading questions, on which as yet we have few answers.

2. The second reason is more pragmatic, but not less compelling. One might say that an indifference to the politics of knowledge--to this history and sociology of how resources are distributed--is a luxury of the affluent. Throughout the 1960s and 1970s, the field of social studies of science was itself the recipient of increasing, or at worst stable, resources. The crude shock of the attempts to slash National Science Foundation and other budgets in this field, with which the 1980s opened, revealed among other things our considerable ignorance of the politics of knowledge in both its historical and practical dimensions. In the decade of scarce resources which we now face, we can no longer afford the luxury of indifference.

3. My final reason relates to the whole field of the metasciences of science. If the number of scientific practitioners today runs into the millions, the metasciences of science themselves have perhaps 10,000 practitioners. And it is now possible to assemble over 1,000 of them in one learned gathering.⁸ We thus find ourselves in a position not dissimilar to that confronted by William Whewell and his scientific confreres in the 1830s.

As with the sciences themselves in Britain in the 1830s, our field today is split among proliferating and often competing societies and organizations. It was a perception of government indifference and declining support that catalyzed the creation of the British Association for the Advancement of Science. Through its creation, science took on new meanings and found fresh energies, and new political forms to match new political realities.

The metasciences of science now stand at a cross-roads themselves. If we are to find fruitful ways forward, in terms of our intellectual agendas, our orientations to government and society, and our relations among ourselves, then the time is upon us to give a sustained attention to the politics of knowledge.

Notes:

1. The quotation is reproduced from C.R. Weld, A History of the Royal Society (2 vols, London, 1848), i, 282. The estimates of the size of the "scientific community" that I give here and below are rough, order of magnitude approximations.
2. See J.E. McClellan, The International Organization of Science and Learned Societies in the Eighteenth Century (Xerox University Microfilms, Ann Arbor, MI, 1976).
3. Sir Henry Lyons, The Royal Society. 1660-1940 (Cambridge, U.P., Cambridge, 1944), appendix II.

4. Mr. Simon Baatz of the University of Pennsylvania is completing a PhD dissertation which reveals the great variety of Philadelphia science in this era.
5. The developments of the 1830s are extensively chronicled in Jack Morrell and Arnold Thackray, Gentlemen of Science. Early Years of the British Association for the Advancement of Science (Clarendon Press, Oxford, 1981).
6. See Arnold Thackray, "The Pre-History of an Academic Discipline: The Study of the History of Science in the United States, 1891-1941," Minerva 1980 18 448-473.
7. Much interesting statistical information may be found in Science Indicators 1980 (U.S. Government Printing Office, Washington, D.C., 1981), from which these figures are taken.
8. There were 1061 paid registrants at this Philadelphia joint meeting of the History of Science Society, The Philosophy of Science Association, the Society for the History of Technology, and the Society for Social Studies of Science.

1983 48 COUNCIL

	<u>Telephone Number</u>	<u>Term expires</u>
President: Arnold Thackray Department of History of Sociology of Science University of Pennsylvania 215 South 34th Street/D6 Philadelphia, PA 19104	Univ. 215-243-5575 Home 215-688-5455	11/83
Secy-Treas: Lowell L. Hargens Department of Sociology Indiana University Bloomington, IN 47405	Univ. 812-335-3573 Home 812-336-5044	11/84
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David O. Edge Science Studies Unit University of Edinburgh 34 Buccleugh Place Edinburgh EH8 9JT SCOTLAND		11/84
Rae Goodell Department of Humanities Room 14E-310E Massachusetts Institute of Technology Cambridge, MA 02139	Univ. 617-253-4069 Home 617-926-5510	11/83
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Center for History of Physics		
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New York, NY 10017		
John M. Ziman	01-435-3023	11/84
9 Langland Gardens		
London NW3 6QD		
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Terms of office expire at the end of the Annual Meeting.

Former Presidents:

Robert K. Merton	Fairweather Hall 415, Columbia University, New York, NY 10027	212-380-3696
Warren O. Hagstrom	Department of Sociology, University of Wisconsin, Madison, WI 53706	
Dorothy Nelkin	Program on Science, Technology + Society, Cornell University, 632 Clark Hall, Ithaca, NY 14853	607-256-3810 6230
Bernard Barber	(see page 1)	

Committees:

1. Committee on Future Meetings:

Nicholas Mullins (Chair)	Department of Sociology, Indiana University, Ballantine Hall 744, Bloomington, IN 47401	(U) 812-335-7795 (H) 812-336-5465
Arie Rip	Korenbloem 6, 2661 JC Bergschenhoek NETHERLANDS	
Ronald Calinger	Department of History and Political Science Rensselaer Polytechnic Institute Troy, NY 12181	(U) 518-270-6325 (H) 518-785-8069
Daryl Chubin	Technology and Science Policy Program School of Social Sciences Georgia Institute of Technology Atlanta, GA 30332	404-894-3195
Karin Knorr	(See page 1)	

Charge: To develop plans for 4S meetings for 1984, 1985, and 1986. Those plans to include an annual meeting in Europe (1985 or 1984), a regional meeting in the west or midwest (in the same year as the European meeting) and an east coast annual meeting in whichever year of 1984 and 1985 we do not go to Europe. The committee is also to take necessary preliminary steps with regard to the 4 Society meeting scheduled for 1986.

Timetable: A document, outlining future options and making specific recommendations for 1984 and 1985, to be available by 1 April 1983.

1. a. Subcommittee on the European Meeting:

Arie Rip	(See above)
David Edge	(See page 2)
John Ziman (Chair)	(See page 2)

Charge: To develop plans for the European meeting of 4S, in liason with the Committee on Future Meetings and appropriate European agencies.

Timetable: A document, outlining the options and making specific recommendations as to time, place and funding to be available by 1 February 1983.

2. Program Committee for the 1983 Blacksburg Meeting:

Ron Westrum (Chair)	Department of Sociology Eastern Michigan University Ypsilanti, MI 48197	(U) 313-487-1073 (H) 313-769-8636
Thomas Nickles	Department of History and Philosophy of Science University of Pittsburgh Pittsburgh, PA 15260	412-624-1050
Karin Knorr	(See page 1)	
Spencer Weart	Center for History of Physics American Institute of Physics 335 E. 45th Street New York, NY 10017	212-661-9404
Thomas S. Gieryn	(See page 1)	
Linda Lubrano	(See page 2)	

Charge: To develop the program for our October 1983 Meeting at Virginia Polytechnic Institute.

Timetable: Formula of "Call for Papers" to Jerry Gaston by 30 November 1982; Proposals due by 28 February 1983; Preliminary program due by 30 May 1983, for printing in the 4S Newsletter. Final program including abstracts of all papers to be available by 30 August 1983.

3. Local Arrangements Committee for the 1983 Blacksburg Meeting:

John Smart	The Graduate School Virginia Polytechnic Institute Blacksburg, VA 24061	703-961-7120
Alan Bayer	Department of Sociology Virginia Polytechnic Institute Blacksburg, VA 24061	703-961-7120
Daryl Chubin	(See page 3)	

Charge: To make local arrangements, including special transportation arrangements, for our Blacksburg Meeting.

Timetable: Preliminary announcement of hotels, travel routes, etc. to Jerry Gaston by 28 February 1983. Full details of costs/arrangements/pre-registration to be available for publication in the 4S Newsletter, by 30 May 1983.

4. Nominations Committee:

Bernard Barber (Chair)	(See page 1)
Albert Teich	Office of Public Sector Programs, American Association for the Advancement of Science, 1776 Massachusetts Ave., N.W. Washington, D.C. 20036
Spencer Weart	(See page 2)
John Holmfeld	(See page 1)
Linda Lubrano	(See page 2)

Charge: To nominate at least 8 individuals for 4 Council vacancies, bearing in mind the need for appropriate disciplinary and

geographical representation. To nominate at least one individual for president, and at least one individual for secretary-treasurer.

Timetable: Preliminary list of candidates to be circulated by 28 February 1983; full list of candidates with curriculum vitae and statements of goals to be available by 30 May 1983, for publication for the ballot in the 4S Newsletter.

5. Prize Committee:

John Ziman (Chair)	(See page 2)
Bernard Barber	(See page 1)
Michael Mulkay	Department of Sociology, University of York, ENGLAND
Robert K. Merton	Columbia University, Department of Sociology, Fayerweather Hall New York, NY 10027 212-280-3696
Derek Price	History of Science, Yale University 203-288-7111 Box 2036, Yale Station, New Haven, CT 26520
Barbara Rosenkrantz	History of Science Department, 617-495-3741 Harvard University, Science Center 235, Cambridge, MA 02138

Charge: To nominate the 1983 winner of the John Desmond Bernal Award for distinguished contributions to social studies of science.

Timetable: Preliminary list of candidates to be circulated by 28 February 1983; awardee to be agreed on by 30 May 1983.

6. Publications Committee:

Arnold Thackray (Chair)	(See page 1)
Bernard Barber	(See page 1)
Harry Collins	School of Humanities and Social Sciences, University of Bath, Claverton Down, Bath, ENGLAND, BA2 7AY
Daryl Chubin	(See page 3)
Nicholas Mullins	(See page 3)
Rae Goodell	(See page 1)

7. AAAS Section L Liason:

Daryl Chubin	(See page 3)
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AAAS Section X Liason:

Rae Goodell	(See page 1)
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Charge: To arrange activities in AAAS, as appropriate

8. 4S News letter

Editor: Jerry Gaston

Department of Sociology, Texas
A & M University, College
Station, TX 77843

713-845-5133

Book-Review Editor:
Larry Stern

Department of Sociology, College
of the Holy Cross, Worcester,
MA 01610

9. Charter Review

Arnold Thackray (Chair) (See page 1)

Lowell Hargens (See page 1)

Harriet Zuckerman

Department of Sociology
Columbia University, Fayerweather
Hall, New York, NY 10027

212-UN5-8440

- Notes: 1. In some cases additional members may be added to committees.
2. The President is ex-officio member of all committees.

Eighth Annual Meeting - Preliminary Program Announcement

The 1983 4S Meetings Program Committee asks for papers and ideas for the meetings. Topics for the sessions could include: ethnographic studies; communication networks; psychology of science; science policy studies; philosophy of science; scientific discovery; the scientific career; controversies in science and technology; opinion change/fashion in science; public relations of science.

These topics are tentative, and we expect to add others. If you have a paper in progress, please send an outline to: Ron Westrum, 4-S, Department of Sociology, Eastern Michigan University, Ypsilanti, MI 48197 (313-487-0012).

The program committee also is soliciting ideas for sessions or papers. Should you have topics which you would like to see presented, please write to Ron. Since there is ample time at this early stage to shape the program, now is the time to express your views and ideas. Are there people you think ought to be heard from? Are there hot research projects which we ought to know about? Please communicate your thoughts to us.

Minutes of the 4S Council Meeting
Philadelphia, PA, Oct. 28, 1981

Present: Council Members - R. Goodell, W. Hirsch, L. Hargens, J. Holmfeld,
L. Lubrano, A. Thackray, S. Weart

Council Members Elect - D. Edge, T. Gieryn, K. Knorr, J. Ziman

4S Newsletter Editor - J. Gaston

Members of 4S Committees and Special Visitors - B. Barber, D. Chubin,
R. Miller, N. Mullins, T. Nickles

- I. President Thackray called the meeting to order at 7:20 p.m.
- II. The Secretary-Treasurer reported results of various 4S activities during 1982.
 - A. The 1982 election of 4S Council members resulted in the selection of David Edge, Karin Knorr, and a tie for third place between Thomas Gieryn and John Ziman. The Secretary-Treasurer proposed that both Gieryn and Ziman be seated on the Council for two-year terms, and the proposal was unanimously approved.
 - B. The current status of 4S membership and finances (see below) was discussed. It was moved and seconded that the Secretary-Treasurer explore arrangements by which 4S may obtain a return on the balance in the 4S treasury. The motion was passed unanimously.
- III. Jerry Gaston reported 4S Newsletter developments over the past year, and the progress already made in moving the Newsletter in the direction of a research journal. The next stages in this evolution, including the costs of printing and mailing, were discussed. The Council expressed its thanks to Gaston for his achievements in making the Newsletter operation efficient and punctual, and to L. Stern, for his accomplishments in developing the Newsletter's book review section. Further discussion of the evolutionary pathway to a journal or "4S Review" was deferred to the breakfast meeting of the editorial and publications committee.
- IV. Rae Goodell, the 4S representative to AAAS, reported on the current status of our affiliation. She made three recommendations for strengthening the relationship:
 1. AAAS functions should be publicized in the 4S Newsletter
 2. 4S should be affiliated with three AAAS sections
 - a. Section X (General) - 4S has been affiliated with this section for some time
 - b. Section L (History and Philosophy of Science) - this section recently accepted 4S's application for affiliation
 - c. Section K (Social, economic and political sciences) - 4S's application for affiliation with this section is under consideration.

3. 4S should participate in annual AAAS meetings by sponsoring sessions at those conventions.

Goodell asked others for suggestions about implementing point 3, and the Council agreed to solicit 4S members for participation in the next possible AAAS meetings (May, 1984).

- V. Daryl Chubin reported on the activities of the Committee on Future Meetings. The Committee recommended that the 1983 meetings be held at Virginia Polytechnic Institute during a time yet to be determined between mid-October and early November. A motion to do so was made, seconded and passed by the Council.

President Thackray reported on prospects for future joint meetings with PSA, HSS and SHOT. Because the Philosophy of Science Association meets biennially and wishes to meet alone in 1984, and because the History of Science Society and the Society for History of Technology plan to meet with other societies sharing common interests next year, the earliest feasible time for another joint meeting involving all four groups is 1986. It was moved and seconded to join the other societies for a joint meeting in 1986. The motion passed.

The Council discussed possible meeting sites for the 1984 4S convention. Council members expressed their interest in holding a meeting in Europe in the near future. It was argued that such a meeting would strengthen our existence as an international body, and would also help solidify the community of scholars with allied interests which already exists in Europe. A resolution was proposed stating that 4S desires to hold one of its annual meetings in Europe as soon as possible, that a committee should be appointed to arrange such a meeting for 1984 or 1985, and that the committee should submit its report on the meeting arrangements as soon as possible and no later than the 1983 VPI meetings. The resolution was passed. The Council also stipulated that the committee should be empowered to arrange the meeting so that it can be in conjunction with the European Association for the Study of Science and Technology (EASST). The probability of a regional meeting in the midwest, south or far west in the same year as a European meeting was also discussed.

- VI. Thomas Gieryn reported on the program arrangements for the 1982 meetings. The Council expressed its thanks for his excellent job in arranging the 4S sessions.
- VII. Nicholas Mullins reported on the activities of the Committee on Nominations for the 1982 4S election. The Council voiced its thanks to the committee for producing a strong slate of candidates and complimented it on the publication of short candidate profiles in the 4S Newsletter.
- VIII. John Ziman reported that the Bernal Prize Committee had unanimously selected Robert K. Merton for the 1982 Bernal Prize. The Council expressed its appreciation for the committee's work, and to the Institute of Scientific Information for its continued support of the award.

- XI. Roberta Miller reported on the activities of the Consortium of Social Science Associations (COSSA), which was organized to provide coordinated opposition to cuts in U.S. social science research funding. Miller asked 4S to provide a subvention so that COSSA can continue its activities. The Council discussed the issue of providing support for activities limited to the U.S., but it was noted that 4S gave small amounts of support this year to its overseas officers to enable them to attend the Philadelphia meetings.

It was moved and seconded that as a regular principle 4S should provide its overseas officers up to \$100 each to help defray their travel expenses. The motion passed.

It was moved and seconded that 4S give \$500 to COSSA for its activities. The motion passed.

- X. President Thackray announced that he would appoint a committee to take a fresh look at the 4S charter to determine if changes are in order to meet new challenges and opportunities.

The meeting was adjourned at 10:25 p.m.

Notes From The Secretary/Treasurer

We accept multiple-year membership dues payments. 4S members outside the U.S. may want to make such payments in order to reduce bank handling and currency exchange charges.

We have lost trace of the following members. If you have a current address for one of these people, please send it to Lowell Hargens, Department of Sociology, Indiana University, Bloomington, Indiana, U.S.A.: Richard A. V. Diener, Noah Friedkin, Bert L. Hardin, Otto Keck, David Koester, Helga Nowotny and James S. Perlman.

Don't forget to notify us whenever you change mailing addresses!

4S BUDGET STATUS AS OF 10/22/82

<u>Expenses</u>	<u>Budgeted</u>	<u>Spent as of 10/22</u>	<u>Balance</u>
Newsletter	4400	3393.38	1006.62
Duplicating and postage	1000	981.47	18.53
Secretarial	750	500.00	250.00
Telephone	200	217.90	-17.90
Annual meeting	2000	0.00	2000.00
Bernal Prize	1150	1044.75	105.25
Miscellaneous	300	299.67	.33
TOTAL	9800	6437.17	3362.83

Income

Membership renewals	4700	4485.00
New members	1500	1345.00
Annual meeting	2000	0.00
Bernal Prize	1000	1000.00
Mailing labels	0	175.00
Balance from 1981	10498	10498.00
TOTAL	19698	17503.00

Income to date	17503.00
Expenses to date	-6437.17
Current balance	11065.83
Anticipated expenses	-1686.62
Anticipated income	+ 300.00
Anticipated balance	9679.21

JOHN ZIMAN'S INTRODUCTION OF ROBERT K. MERTON ON PRESENTATION OF
THE JOHN DESMOND BERNAL AWARD:
"Robert K. Merton and Science Studies"

The salient issue of academic politics for the various intellectual communities concerned with the study of the diverse aspects of science and technology is how they can be brought together in a common cause. This meeting demonstrates the convergence of the main currents of academic meta-science, as the history, philosophy and sociology of science broaden and interpenetrate one another with concepts and insights. But there still seems a chasm between these more theoretical disciplines and the practical social sciences of research management, technological economics, science policy, and even science education. One of our current tasks in 4S is to convince the hard-nosed practitioners of these new crafts that they are really a bit soft in the head not to take account of advances in our understanding of how science works, at a higher level up the brow.

This policy of building bridges between the theoretical and practical disciplines of Science Studies was certainly favored by J. D. Bernal throughout his life. It can also be exemplified, at a more profound level, in the work of Robert Merton. We honor him as a famous scholar, a seminal influence within the intellectual core of the social studies of science, for nearly 50 years. At first sight, his writings seem quite disconnected from the issues that agitate the editors and readers of Nature, Science, or New Scientist — let alone the Financial Times or the Wall Street Journal. Nevertheless, nothing is easier to demonstrate than their relevance to major contemporary problems of science and of society.

The central problem of science policy could be phrased as follows: "How does a cultural emphasis upon social utility as a prime, let alone an exclusive, criterion for scientific work variously affect the rate and direction of advance in science?" That is how Merton himself, in a preface to the re-issue of Science, Technology and Society in 17th Century England, expressed the social relevance of "this youthful work," which had, of course, so much influence on the academic historiography of science. The very essence of this problem is that every "useful" scientific organization can easily become a threat to the norm of communalism, which Merton delineated so clearly in the 1940s. Even in the more open domain of basic research, the sociological eye quickly becomes aware of the hidden dangers and dysfunctions of bureaucratic rationality. His paper with Harriet Zuckerman on "Institutionalized Patterns of Evaluation in Science" focussed on the editorial and refereeing procedures of learned journals, where the norm of universalism is under continued tension. Again, it is difficult to know whether the current song and dance about fraud in science is a sign that the norm of disinterestedness is more seriously flouted nowadays than it was in the past. It is instructive to turn back to Merton's studies of priority disputes and multiple discoveries for an historical and psychological perspective on this distressing phenomenon.

In these hard times, every professional research worker is deeply concerned about prospects of personal advancement — even of future employment. One of the fundamental career problems in science is how to discover and encourage those scientists who live up to the norm of originality, without

giving them undue authority or influence. What Merton had to say about the "Matthew Effect" has its parallels in all scientific and technological careers, all through the research system. The correlation of these aptitudes and roles with age is another of the really significant career questions to which Merton has drawn particular attention.

The salient issue for the general public is the credibility of scientific expertise. To explain the conflicting testimony of honest scientific witnesses on many technological questions, we might do well to turn to Merton's paper on "Perspectives of Insiders and Outsiders." Or should we see this as yet another manifestation of the essential tension induced by the norm of scepticism, and of the profound epistemological paradoxes within the sociology of knowledge, to which he contributed several significant papers? In fact, the whole question of the social role of social science knowledge was behind much of his work in general sociology.

I have deliberately schematized this cursory account of the contributions of this very great scholar to our subject, in terms of the "Mertonian norms." Like all really interesting and original intellectual concepts, these have been the target of violent criticism. But the norms themselves are intended for a community where open controversy is, itself, the norm. The most important term in a mathematical series is the zeroth order term, which all the subsequent terms are merely designed to correct: the most important word on a scholarly topic is the first word, on which all the others are merely a gloss. Merton's articulation of the norms of science characterized the scientist, almost for the first time, as a relatively ordinary person in an exceptional communal role, thus bringing science down from its superhuman heights of supposed genius and virtue.

But Merton's demystification of the scientific attitude is also a defense of science as a highly valued institution. This is of the utmost significance in the practical realms of science policy. The great international scientific community, to which humanity owes so much, is under severe strain, as its norms come under attack in country after country. It is no accident that many of the fundamental Human Rights inscribed in various international Declarations and Conventions are closely correlated with the fundamental norms of science — "communalism" with freedom of communication, "universalism" with non-discrimination, and so on. As Merton himself clearly saw, during a war against obscurantism and tyranny, the functional relation between intellectual freedom and scientific progress is the axis upon which all our Science Studies must essentially turn, and the pivot of all science policy in a free society.

John Ziman
Imperial College, London

ROBERT K. MERTON'S REMARKS ON ACCEPTANCE OF
THE JOHN DESMOND BERNAL AWARD OF THE SOCIETY FOR SOCIAL STUDIES OF SCIENCE
OCTOBER 1982

I am of course delighted by John Ziman's hyperbole. It does, however, undermine my capacity for reticence. Indeed, this entire event puts me in mind of another much like it (though not as exalted) that took place a good many years ago. As I was leaving the house, my daughter asked with all the innocence of childhood: "Do you really deserve this honor?" With due humility, I assured her that I did -- and then went on to explain. It was deserved, I told her, in about the same sense that the gentleman from Texas deserved congratulations on his coup in having turned a profit of \$10,000,000 in Texas oil. When asked how he had brought this off, he gently went on to say: "It wasn't Texas oil, but Oregon lumber. And as a matter of fact, it wasn't exactly \$10,000,000 but only 2 million. And it didn't really happen to me but to my brother. And, come to think of it, it wasn't actually a profit, but a loss."

Still, whether deserved or not, a prize such as the John Desmond Bernal Award does much more than provide its recipient with pleasure and cash. Symbolizing collegial judgments, it helps chase those blue devils of acute self-doubt who attack many of us as we look back on what we have done, and on what we have failed to do, during our lifetimes of work. Finally, in strictly Durkheimian fashion, such an award symbolically reaffirms the collective values of a learned society.

In my own case, pleasure in the award is compounded several times over. First, it is being presented in the city where I was born and (more or less) grew up. Furthermore, the award was made possible by the Institute of Scientific Information, especially through the good officers of an old, admired friend, Gene Garfield. And last, I greatly appreciate its being known as the John Desmond Bernal Award. (I also enjoy the surely unplanned coincidence that the letter of notification from Arnold Thackray, as president of 4S, was dated May 10th, the precise date of publication of Gene Garfield's splendid piece on Bernal in CURRENT CONTENTS.)

Unlike Derek Price, the first recipient of the prize, I hadn't the good fortune to have studied with Bernal. My experience of him was severely limited and largely indirect, though at the outset, consequential. For in the 1930s when the Great Depression blanketed the world economy, it was above all J. D. Bernal, together with such associates as Joseph Needham, Gowland Hopkins, P. M. S. Blackett, Lancelot Hogben, J. B. S. Haldane, Hyman Levy, Julian Huxley and, at the indispensable publicist margin, J. G. Crowther, who together helped create a climate of scholarly opinion in which a sociology of science could emerge, if not thrive. In that sense, I was one of Bernal's many students-at-a-distance (as my short 1940 review of his Social Functions of Science made clear).

My direct experiences of Bernal were just two. The first was a long letter from him in 1961 commenting on a recently published article of mine dealing with the sociology of multiple independent discoveries. It was encouraging to have him say that he had himself 'long supported' the ideas expounded in the article though without the benefit of what he described as 'the detailed statistical study' reported there. But much more in point here

and now is another passage in his letter which emphasized the growing need for bibliographic efficacy in science. Here is the extract from Bernal's letter, written two years before the publication of the first annual edition of SCIENCE CITATION INDEX (though, to be sure, six years after Garfield's seminal paper of 1955, entitled "Citation Indexes for Science"):

" . . . I feel that your article refers to another thing altogether, that is, the failure of the communications system in science, a failure which is clearly growing with increased production of scientific material, for instance, as reported on by Professor Derek Price in his book SCIENCE SINCE BABYLON. [It might be said that by invoking Derek in a letter to me, Bernal virtually designates the first two recipients of the Bernal-Award-to-be.] We have long since passed the point where for most parts of science it is a lot easier to discover something new than to find out whether it has been discovered before: I mean easier in the sense of the actual amount of labour employed in carrying out an experiment against labour employed in looking up references in libraries. This is a technical fault which when things get bad enough may indeed be put right. I have made some efforts myself to see that this is done."

Bernal, it seems evident, was incorrigibly inventive in every sphere he visited or inhabited.

As it happened, the only time I got to see something of Bernal close-up was in Chile a few months later. Along with several humanists, he and I occupied the same lecture platform repeatedly during a weeklong celebration at the University of Concepción. The occasion comprised a series of public discussions of the sciences in their complex relations to culture and society rather than a focussed Great Debate. It found Bernal, then a youthful 61 (shortly before his serious illness), much as he had evidently been throughout his adult life: endlessly inquisitive, vigorous of opinion, deeply informed, open-minded and pervasively civil in response to forceful but civil criticism. His attitude of mind, consonant with the operation of organized skepticism in science, is partly caught up in the words he inscribed in the copy of his SCIENCE IN HISTORY which he made uniquely my own:

For Robert Merton,

To pick holes in -- hoping that
something will be left and that
it will be worth his time.

J. D. Bernal

Concepción

Jan. 1962"

John Desmond Bernal was an exemplary scientist and citizen of the world. I take pride in accepting the Award which bears his name.

Robert K. Merton
Columbia University

RECENT MEETINGS

First Meeting of Confrontation and Evaluation of the Science, Technology, Society Program of the Centre National de la Recherche Scientifique (CNRS), Strasbourg, March 25-27, 1982.

Reported by:
Bernard-Pierre Lécuyer
Maître de recherche
au Centre National de la Recherche Scientifique
Paris (Francis)

On March 25-27, 1982, a meeting was held in Strasbourg of all research teams or groups financed by the CNRS on the basis of its program "Science, Technology and Society" launched for the period 1980 to 1985 as a continuation and an enlargement of the previous MIT/CNRS program on "Science and Decision," called The Bernard Grégory program (Bernard-Paul Grégory (1919-1977), a prominent French physicist, trained at Polytechnique and MIT, was successively Director General of the CERN, of the CNRS, and Délégué Général à la recherche scientifique et technique).

This program aims at studying in a comparative perspective, but with an accent on the French situation what are the mutual relationships between science, technology and society. More specifically (1) these relationships should be studied in both directions without any presupposed determinism in one way or the other; (2) the technology and prospective approaches should be completed by a historical one; (3) the specific topic of each research program is in principle determined by the intersection of fields under study (e.g. for the first run of projects 1980-83: computer science, biology, chemistry) and approaches to study one given field: institutional approach (functioning and adaptation of institutional systems); social and cultural change; the impact of external logics over scientific and technical development.

The Strasbourg meeting convened by the scientific committee of the CNRS program, was initially defined by its organizers (1) as a half-way progress examination of the program; (2) as an opportunity to overcome the geographical distance and epistemological variety of the various teams involved in the program.

To the 90-odd participants (including, in addition to the members of the teams financed on the 1980-81 runs and the members of scientific committee, several foreign scientists working in similar or comparable fields) the constraints implied in the situation had obvious advantages and disadvantages.

On the one hand, the great variety of fields and approaches represented in the projects financed by the STS program, ranging from the social conditions of the introduction in small rural communities in India of solar water pumps to the study by participant observation of scientists theoretical and experimental cognitive frameworks, brought the meeting close to an arch-compact combination of PSA, SHOT, 4S, and partly HSS conventions, and for an incomparably smaller research community. Hence at times, a bewildering impression of dispersion and heterogeneity between the papers presented in terms of field, approach, disciplinary affiliation, and even level of scholarly accomplishment.

To correct the comparison attempted above with scholarly meetings, one should add that the presence of the administrative sponsors of the program as potential users (even if they are personally scientists or scholars) introduced a cause of tension and, at times, disruptions in the discussions.

On the other hand, the very causes of the frustrations and tensions experienced at Strasbourg proved by and large fruitful. The basic principle of the meeting being cross-examination (members of the team presenting a research report being in their turn discussant), a significant degree of cross-disciplinary exchanges has developed. A few examples will suffice. An economist specialized in prospective studies on European chemistry, or an American-trained Canadian historian of US chemistry, have addressed criticisms and suggestions to a project aiming at an economic, social, and technological history of the development of the plastics industry in France and Europe from 1945 to 1970. Philosophers of science have questioned the epistemological bases of computer based technics of textual data at large, and more specifically of scientific production in term of "strategic cards." In relation to the most recent advances of biotechnology, philosophers, medical researchers and sociologists have discussed the place of ethics and of ethics committees. A more general problem recurrent among several papers and discussions was that of the proper status of social experimentation and observation, of the proper distance between observers and observed in scientific and technical activities. Through all these discussions, the presence of sponsors of the program and of potential users also proved an asset in forcing scholars to come down to specifics in their statements and objections.

Further light was shed on comparable experiences in STS research programs like the Forecasting and assessment of Science and Technology (FAST) program of the Economic European Community, the activities of the "Science and Engineering Research Council" and the "Social Science Research Council" in Great Britain and the "Science Dynamics Group" at the University of Amsterdam.

A full afternoon was devoted to an examination of the prospects of this program and more generally of STS studies in the French research and teaching system. It has been admitted that by its very nature the incipient field of STS is pluriparadigmatic and will remain so in the future. Progress is not likely to arise from too global reflexions on the field (if it may be considered as such) but rather from concrete research programs based on well defined problematics. Accordingly the next meeting should retain the interdisciplinary qualities of the Strasbourg one, but should be more clearly focused on one given theoretical and general problem. More specific workshops should be organized. Problems of bibliographic inventories, publications, offering STS courses in engineering schools and training young researchers have been debated. With its evident shortcomings and potential riches, the Strasbourg meeting has given a good opportunity to witness and participate in the efforts to constitute a new area of scientific inquiry on an ill-defined pluridisciplinary basis.

EASST CONFERENCE

Problems and Perspectives of the Study of Science and Technology in Europe
held at Burg Deutschlandsberg, Austria. September 24-26 1982.

Reported by:
Trevor Pinch
University of Bath

From Double-Dutch to Dutch! - Some Personal Reflections

This was the first conference of the newly formed European Association for the Study of Science and Technology. This organization has largely replaced PAREX as the main European forum for Science Study activities. The inappropriateness of PAREX (PAREX refers to Paris and Sussex which in the early 1970s looked like being the main centres for science studies) for organizing this conference was indicated by the presence of only two participants from either Paris or Sussex (and both these came from Paris!). A brief head-count of the participants showed that over half came from the Netherlands. With the establishment of a new Chair in "science dynamics" at Amsterdam it would seem that the locus of science studies in Europe has shifted a few hundred kilometres north of the Paris-Sussex axis.

I must admit that I arrived at the conference with some pessimism. This pessimism was not helped by a misunderstanding over the local arrangements which resulted in it taking six hours to travel the 30 miles from Graz airport to my hotel. However, my humor was rekindled when I discovered that another British participant arrived even later having walked the two miles up the mountain to where the conference was being held.

My pessimism had stemmed from the economic climate in which EASST was attempting to get itself established. With funding institutions trying to save money it is not an encouraging time to launch a new organization. I knew of several colleagues in England who would have liked to attend the meeting but found it financially impossible to attend more than one "foreign" conference a year.

However, my pessimism proved to be unfounded, for this meeting clearly demonstrated that science (and technology) studies are alive and well in Europe. The thing that struck me most was the sheer professionalism of the papers presented. It could be said that European approaches, in the attempt to develop new frameworks for studying science, have sometimes not got beyond being "approaches." The complexity of much European philosophy combined with the difficulties of translation into the non-native tongue (English, of course, much to the blush of minority English participants) has in the past meant that some conference presentations have appeared to be double-Dutch, or at least not easily accessible to Anglo Saxon empiricists. However, most European work now has the character of "research programs rather than "approaches." Philosophical sophistication has been combined with the need for empiricism. The language barrier, too, seems to be less of a problem and presentations at this conference were often enjoyable as well as being concise. Of course, much of the work is uniquely European and diversity of styles remains its main feature. However, a visitor from North America would, I am sure, find the EASST conference a valuable professional forum for the presentation and discussion of work.

In assessing the success of any conference it is important to look at the wider atmosphere of the meeting. All too often it is the informal contacts which prove more valuable than the papers themselves. The setting for this conference, a castle in the mountains of Austria, was ideal. As one participant remarked: "It is a conference on the edge of the universe." Certainly it was a long way away from down-town Atlanta. The setting provided the friendly, hospitable (and alcoholic) environment in which informal contacts flourish. The relatively small size of the meeting (30 participants) may also have helped.

The program of the meeting is given below. As at 4S meetings, it was difficult to discern one overall theme. Topics that stood out for me were: scientific career strategies (Lemaine, Ziman, Toren); Piaget's theory and the development of science (de Mey, Stemerding); the interaction of science on wider social institutions and their impact back on science (Weingart, van Rossum, Leydesdorff/Dits); the place of the natural world in explaining scientific developments (Pinch) and in explaining technological developments (Bijker/Bonig/van Oost); and the place of discourse analysis in science policy (Yearley). There were also two excellent historical papers on the development of specific scientific institutions (Ludgmer/Kuppers, Schopman). In addition there were a number of papers which provided excellent accounts of recent developments in science studies (Rip, Lopponen, Cannavo, Verhoog).

Finally no account of this meeting would be complete without acknowledging the Herculean efforts of Arie Rip in organizing the program and Johann Gotschl for the handling of the local arrangements. The conference would not have been possible without the financial support of the Landserat, Dr. Christoph Klauser.

PROGRAM OF EASST CONFERENCE

SESSION 1: Analysis of Scientific Practices

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

Gerard Lemaine: "Social Differentiation in the Scientific Community."

Marc de Mey: "Piaget's Notion of 'Cognitive Structure' and Current Cognitive Analyses of the Structure of Scientific Knowledge and Puzzle-Puzzle-Solving in Science."

Steven Yearley: "Analyzing Science and Analyzing Scientific Discourse."

SESSION 2: Dynamics of Scientific Fields

Dirk Stemerding: "The Rise of Evolutionary Thinking and the Experimental Method in Biology in the Nineteenth Century."

Arie Rip: "Scientometric Studies in Biotechnology."

Wouter van Rossum: "The Development of Hybrid Fields."

Peter Weingart: "Politicization of Science and Scientification of Society."

SESSION 3: "The Occupation of Science/Scientific Institutions"

John Ziman: "Research Careers: Specialization and Change."

Nina Toren: "Employment and Occupational Integration of Soviet Scientists in Israel."

Joop Schopman: "Science in an Industrial Context."

G. Koppers/P. Lundgreen: "'Technology' as Object of Administrative Intervention and of Research within Government Laboratories: Germany — Nineteenth and Twentieth Centuries."

SESSION 4: Studies of Technology

Wiebe Bijker/Jurgen Bonig/Ellen van Oost: "The Social Construction of Technological Artifacts."

Loet Leydesdorff/Henk Dits e.a.: "Technological Development and Trade Unions."

SESSION 5: Developments and Issues in Science and Technology Studies in Europe

Paavo Lopponen: "Studies of Science and Technology in Finland — a Review."

Leonardo Cannavo: "Development or Underdevelopment of Italian Sociology of Science?"

Henk Verboog: "The Social Impact of Science Studies."

REVIEW AND COMMENT

Reviews

K. Knorr's, The Manufacture of Knowledge, Oxford: Pergamon Press, 1981.

One of the funny things about Karen Knorr's latest book is Rom Harré's foreword: "Of course," he says, "science only makes sense as a realist enterprise, an attempt, using the means at hand to truly represent physical reality as it is. Indeed, this very study is a realist enterprise, an attempt to truly represent the social order of life in laboratories and institutes of research, just as they are" (p. viii). And Harré concludes his misdirected enthusiasm: "...Knorr has developed the first really positive challenge to the philosophy of science since the days of paradigms and internal definitions of meaning" (p. viii). Harré's blindness aptly summarizes the interest and shortcomings of this book. If Knorr has provided us with a "realist" representation of science then science cannot be a realist activity (but has she done that?), and if it is the "first challenge" to philosophy of science is it a challenge for social studies of science?

Granted access to a government-financed research center in Berkeley, California for a one-year period, the materials presented here focus on one research area, plant protein research. The method of inquiry advocated by Knorr is hard to dispute, at least if one stays at the level of principles. Everyone prefers a "sensitive" methodology to a "frigid" one (p. 17) and it is now well accepted that scientists should be studied while working at their bench. In practice, however, Knorr's claim for an "ethnography of knowledge" (p. 25) is not fully supported. Ethnographers, if they had written a book on this Berkeley lab culture, would have told us things about the setting, about the group (i.e., its past, its folklore), about the material life of the people, about their gestures, instrumentations and craftsmanship. Although the author might have collected all these elementary data, she does not present it to the reader. Manufacture of Knowledge is in no way a case study of a particular setting. All the data come from conversations and interviews and from one article written by the privileged informant. The genre is not ethnography or anthropology. It pertains more to a philosophy of science based on verbal and written material of abstracted "reasoners." There is nothing wrong with that genre, except that it does not fit the claim of doing "ethnography" and that it is especially ill-suited for a demonstration that local idiosyncrasies play a major role in manufacturing science. Paradoxically, Karen Knorr gives us an abstract demonstration of the concrete nature of science.

Taken as a general summary of philosophical arguments about science, backed up by a wealth of references, the first chapter is a solid plea for "constructivism." In her words, "What we have is not a process of opinion formation, but one in which certain results are solidified through continuous incorporation into ongoing research" (p. 8). Realism is a superfetatory hypothesis to account for the solidity of science. With the word "constructivism" Knorr has provided all of us with a convenient label. If other writers, after reading her first chapter, still want to confuse a castle in the clouds and

a castle in stones, it is their problem. Her work is concerned with the process of fabricating solid castles which are built. Period.

Now the question is, of course, with what are the sciences built. That they are made of heterogeneous things coming from all over the place is not any more in question. That the materials brought to build them cannot be put in any clearcut categories (internal/external; fundamental/applied; social/cognitive), has also been made so plain by so many studies that the principle is now beyond question. The question is now to compare local settings to study how solid structures of different forms and shapes are made with all these bits and pieces. This is the aim of the second chapter. But it is especially in this chapter that the discrepancy between the stated research project (ethnography) and the final text (philosophy) is the most disturbing. Knorr states that science is indexical and local. She presents the scientist as an "indexical reasoner." The word "reasoner" is puzzling. It is like a fossil of a bygone period. But she does not describe the setting that makes, she says, the stuff of science. In a subchapter, called "the neglected research site," Knorr diagnoses her own problem. The research site of the Berkeley laboratory is so much neglected that we know next to nothing about it. It is of course possible to address the philosophical argument of indexicality versus universality, but I am not sure it is the work of a purported "ethnographer." Anyway, as Harre's foreword shows, it is a hopeless task. To take a metaphor, it is not the same research project to say that science is a macrame, and to study in detail how different macramés are made from very many threads of various shapes, color, resistance and fibre.

Everyone now agrees that sciences are made from bits and pieces, and the next question concerns how they are transferred from a specific context to local idiosyncracies where they are used as building blocks to make more solid a given scientific fact. Knorr tackles the question of the displacement of elements in her chapter 3. But for a reason that I still do not understand, instead of extending all of the arguments made on this process of translation that composes laboratories, she enters into a long argument against the metaphor theory. Again she chooses a dead philosophical target instead of the living issues of a new field. For instance, she takes time to criticize the "conceptual interaction" (what on earth could this monster be anyway?) and does not study the material transfer of elements. She writes that "...conceptual interaction is not limited to the use of metaphor, but seems to be a routine feature of 'displacements of concepts' in general" (p. 52). But "metaphor," "displacement," "transfer," "translation," and "deformation" all say the same thing in English, Latin or Greek. Concepts here have no role to play. It is much more interesting to follow the real moves that transform elements in the tinkering process of fact fabrication. She admits that physical proximity plays a part (p. 66), but she cannot study these moves realistically because she sticks to "reason," "idea," "concept," and "thinking process," and is too far away from the ethnographic level.

My comments concerning her fourth chapter will certainly be biased to a large extent since I strongly disagree with Knorr's assessment of the credibility model. Analysts of science in the making always have to rebuild or reconstruct science from the bits and pieces it is made of. So it is quite normal; after having presented constructivism, then idiosyncrasies, then the transfer process, that Knorr had to tackle the question of the building process itself. What does bind all these heterogeneous elements together? If no cognitive level, no theory, no paradigm is strong enough to hold the tinkering process of science, how are we going to build it? Credibility is one of these useful concepts that help bind together elements of different origins and nature. But it also has another advantage. It shows a movement of capital that can account for the strength of science with the help of no other assumption. It can be criticized, but I am not quite sure that Knorr finds fair arguments to do so.

She argues, for instance, that credibility is not capital since there is no surplus value and no exploitation. But this is not so. The obsession for inscriptions and the infernal cycle for producing new information, shows quite well the existence of a surplus value, which is the only justification of a cycle that would be, without it, absolutely sterile. Since the mere replication of a whole experiment is worth nothing, even if it spends in the process millions of dollars, a capital cycle is clearly at work there. The gentle precapitalist economics described by Hagstrom does not fit at all with this cycle, nor does the only "symbolic" capital proposed by Bourdieu. The confusion of scientists as to where they are in the exploitation cycle, is obvious, but is not a proof that the cycle does not exist. It only proves (as I have shown at length elsewhere) that they can be simultaneously, in the same career, employees, employers, petty capitalists, large corporations or craftsmen. Knorr's critique is limited to the "symbolic" capital. But science is not "symbolic." For instance, she criticizes the notion of capital by showing that scientists themselves are at stake on the scientific market (p. 77). But this is precisely what is accounted for by the credibility cycle. Scientists may be considered in some circumstances as small firms reinvesting all new information in the extension and acceleration of their cycle of credibility. The obsessive concern of scientists for "value" strikes Knorr (p. 76) as it did every observer. But this is what has to be explained. The value of the inscriptions, of the data, of the scientists, of the instruments is constantly conflated, and it is to account for this striking phenomenon that the notion of credibility had to be built. She also criticizes the model because it is a rehashing of the "home economicus" argument. But she does not realize that the main interest of a cycle is that it pushes out of the picture the problem of motivations. Scientists can be maximizers, minimizers, or "satisfiers" (p. 73), they can be greedy or fascinated by nature's secrets. It does not make a bit of difference.

Knorr's unbalanced criticism of the credibility model is unsatisfying, even to herself. She is bound to find an Ariane's thread to hold together the tinkering process of science. If she does not, then, her constructivism falls apart. Her solution is called "variable transscientific fields and resources" (p. 81). The notion, although difficult to grasp, rightly points at an important aspect of science: the link to foreign networks from which the elements to build facts are borrowed and from which they are transferred. But the notion does not address any of the issues that she claims the credibility model does not resolve. That you need to get out of the laboratory to

understand it, is important to state. But the necessity of accounting for the value of scientists and information is still there. When she gets out, she does so with the same abstract and intemporal prose with which she got in. I have the feeling that this move back to the former problem of transfer and translation, is more an escape than a new argument able to counter the advantage of the credibility model. But again I had problems reading this chapter and am obviously biased on that question.

The fifth chapter is quite disconnected from the others and tackles a different but important question: how to study a scientific text. She describes the scientists as a literary "reasoner" (again, why reasoner?) and describes of course a "writer." This is the best chapter of the book because it is at long last an empirical one. She follows in detail the fate of the successive drafts of an article. Avoiding the traps of the old rhetoric and of the new semiotics, she follows the simple notion of "transformation." She shows how the form of a statement is modified from text to text. Transformation is a useful notion since it helps us to get rid of the notion of "representation" and of the notion of fiction. It is a pity that this chapter should be so disconnected from the rest. But the reason for this is interesting. She treats lightly at the beginning the inscriptions and the instruments that make them. But by doing this, she misses the only way to link the handwork of working in laboratories with the writing process of drafts and papers. In the continuum of transformations she makes artificial cuts so as to save this "reasoner." But scientists are not reasoners much of the time. They inscribe and write obsessively and the writing process cannot be separated from the tinkering she shows in chapter 2.

Knowing Knorr's passion and intelligence, knowing her erudition in three languages, it is a feeling of disappointment that comes from reading Manufacture of Knowledge. My opinion is that she made a mistake in writing down the wealth of materials she gathered in her Berkeley lab. She tried to convince philosophers of science that her constructivism was a full-fledged philosophy. Like many sociologists of science, with no formal training in philosophy, she let herself be impressed by epistemologists' arrogance. Having studied philosophy for ten years I have none of this shyness. I know that philosophers have lived much too far from the practice of science to be, in any way, convinced by empirical evidence. The incredible blindness of her own "foreworder," should have brought the point home. Sociologists of science should do their job, and enrich their own new field. Answering philosophical questions is not their job. This is not to say that no theory should be built. But general and abstract arguments are not theories. I propose that we do either precise empirical work or construct short and coherent theories, but I feel that it is useless to talk in general terms of idiosyncrasies and tinkering. This general and empty style is favored by what Anglo-Saxons call "philosophy," but this is not the only tradition in philosophy. Instead of giving us a new case study in the ethnography of knowledge, Knorr gives us another of these general arguments on science. While her book is certain an advance in the field of philosophy of science, I do not feel that it represents an advance in the sociology of science. All the major problems we have to tackle are left pending, and for some (like the metaphor or the credit problem) we have moved one step backward.

I want to finish this review with a plea. Since lab studies are beginning to be a craze, would it be possible for the observers to put in their reports at least the minimum of background information on the groups and settings they study? All lab studies should at least contain information on the setting, the budget, the publication lists, the citations, the career patterns, the instruments, the fields from which the instruments come, and so on. This would allow at least some point of comparison between observation sites. This seems to me more important for the future than trying to address philosophical points that philosophers do not understand anyway.

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Karin D. Knorr-Cetina, The Manufacture of Knowledge; Oxford, Pergamon, 1981.

While the previous review, by Bruno Latour, of Karin Knorr's book was written by a fellow "social anthropologist" of science, the reader is warned that the present reviewer is not a sociologist at all but a philosopher--albeit one unusually sympathetic to current sociology of science. Indeed, Knorr's book is one basis for my optimism that sociologists have something important to say to philosophers as well as to each other. Of course, the fact that Knorr's book is so philosophical will be seen as a defect by sociologists who feel that philosophy is better eliminated than assimilated and by those who hold that space devoted to (broadly) philosophical matters would be better allocated to additional ethnographic descriptions.

Philosophers as different as Dewey, Wittgenstein, Heidegger, Quine, and Sellars have, for some decades now, rejected "absolutistic" or Cartesian foundationist epistemologies in favor of more or less thoroughgoing forms of fallibilism, with their attendant social conceptions of justification. However, by and large, the younger generation of philosophers have been content to consolidate this attack on tradition and, it seems to me, have done rather little to furnish details of the new conceptions of knowledge and inquiry. Perhaps what Knorr says about relativist programs in sociology¹ applies also to fallibilistic philosophical programs: their strength lies in "overcoming the tradition" and not in the deliberate construction of a new, positive viewpoint. At any rate, I am hopeful that the microsociological and microhistorical studies now coming on the scene can begin to provide us with the sort of detail that can only come from empirical case studies.

"Theory of justification" affords the most important illustration of my point. At least since Dewey and Wittgenstein, right-minded thinkers have recognized that there is no natural stopping place to the regress of justification. (As Steve Woolgar has put it, there is a potential regress of how- and why-questions lurking everywhere.) Where justification stops in any given instance is not a matter of "nature" causally forcing all attendant minds into the same cognitive state; rather, it is a matter of "convention," of social control, including the unavailability of resources for pressing further one's opponent's claims--the inability to generate additional, specific objections without appearing overly picky, stubborn, or just plain silly to one's colleagues or fellow citizens. Now this is all quite interesting as an alternative to the foundationist, "bedrock" picture of justification; but it remains very general, abstract, and programmatic as it stands in philosophical statements. Nothing is said about what the mechanisms of social control actually look like in particular cases. Philosophers have done little to solve Ravetz's paradoxes concerning "how recognizable, objective scientific knowledge can (but need not) eventually emerge from the very personal and fallible activity of research."²

It is sociologists more than philosophers who are stepping in with data and accounts of how labelling, criticism, heuristic appraisal and other cognitive activities are actually managed in the doings of scientists. It is micro-sociologists and historians who attempt to describe in detail what it is which brings controversy to a close, which lets one view stand amid specific objections while undercutting another; how scientists decide when a problem exists and is interesting and tractable enough to merit attention; how they

decide when a problem is solved; and so on. This is all broadly compatible with what some philosophers have been saying, but it promises to go well beyond what they have said and perhaps what they can say as armchair philosophers. Whether or not sociologists see themselves as doing epistemology, I think that philosophers can learn much about epistemology from them.

This point about the social nature of justification is now widely recognized to constitute a strong reason for abandoning the social/cognitive distinction as demarcating sociology from philosophy. This may be added to the four reasons Knorr gives for questioning the distinction (pp. 22/3) and opens the way to an internal sociology of science--or rather a sociology of science in which the internal/external distinction as an absolute, discipline-demarcating distinction withers away. Not that Knorr neglects the point that I am making here. On the contrary, restated in her own terms it is the central theme of her book. Her discussion of scientific decisions (selections and translations), their contextual contingency and indeterminacy (Chapters 1 and 2), builds the framework within which she can deal with justification matters such as the role of negotiation.

To be still more specific, I found especially stimulating Knorr's discussion of the appraisal of scientific ideas--in particular, their "interest" in terms of what "is going to be most productive as quickly as possible within the frame of facilities at hand" (p. 59). Her treatment of the "promise" of an idea or potential solution is, in my judgment, a definite improvement over the standard, hypothetico-deductive account of the generation and the testing and acceptance or rejection of problem solutions. Her suggestion that research is solution-driven rather than problem-driven, that problems typically are constructed around ideas already in hand, surely contains a large grain of truth; however, I think she here undervalues the importance of asking good questions and of choosing good research problems. Problems, too (as everyone knows), can be evaluated as interesting, promising of success in the short run, etc.

Knorr's claim that the immediate aim of scientific research is to achieve a success, to construct a product and "make it work," is correct, I think; however, I do not see that this resolves the philosophical problem of whether scientists aim at or achieve truth. Methodologists who deny that truth has any useful role to play in methodology may find Knorr's view congenial; but even most of those who think it necessary to ascribe epistemic goals to scientific activity will agree that truth or knowledge is not an additional goal, independent of "success" or "making it work." ,

Much the same can be said about the realism issue. Although the dominant theme of Knorr's book is that scientific claims are humanly constructed products, she is neither a flaming idealist nor even a relativist. I think it would not miss the mark too far to liken her position to that of Nelson Goodman³: there is no one way the world is; indeed, it makes no sense to talk about the way the world is independently of all conceptualization. Nor is there any reason to think that there exists a unique "best" total or partial description of the world, in some uncontroversial sense of 'best'. There is an antecedent reality, to be sure, but there is no antecedent description of the world (a self-description, as it were) which we are trying to approximate. Thus we can agree that our scientific problem solutions and larger world views are constructions without saying, in the idealist manner, that the world is simply our own construction. If this is Knorr's view con-

cerning realism, I think that it is as defensible as any.

Early in the book Knorr does for a moment sound like relativists such as Harry Collins, when she says, "It would seem, then, that nature is not to be found in the laboratory (p. 4)." But she immediately adds, "unless it is defined from the beginning as being the product of scientific work." Even with the qualification, I think this is needlessly paradoxical. Since no one any longer believes that scientists or anybody else have direct, unmediated access to "the world," why should we be surprised that what protein biologists study are objects which they themselves have manufactured, processed, purified, etc., or that microphysicists study objects or events which they have produced in large accelerators. Usually, it is only by means of such constructions that we can confront scientific ideas with nature in any sense. This is what it is to study nature. How else could scientists reasonably proceed? Contrary to Rom Harré's preface, the book does not, I think, demonstrate that the experimental control of thought is remote from laboratory life. Isn't it a little ironic that the very writers who emphasize the causal contingencies, the unexpected-but-somehow-expected bumps and kicks of laboratory life, should wonder where the world enters into scientific investigation?

Similarly, it borders on a false dilemma to contend that scientific research is constructive rather than descriptive (p. 5). Better to say that the constructive/descriptive distinction, like the internal/external, the cognitive/social, and the discovery/justification distinctions, pretty much collapses under micro-observation of scientific practice. (It also collapses on the Goodmanian view hesitantly ascribed to Knorr above. All descriptions, good or bad, are social constructions.) And once the reader makes it past the opening pages of the book, this is what Knorr does say, and say well. I was pleased to see that the rough edges (such as the false dilemmas) present in her earlier papers are largely missing from the book. As one should expect from so talented an investigator and so articulate a writer, Knorr's thought is steadily maturing.

It should be clear that I find the microstudies of Knorr, Latour and Woolgar, and the Edinburgh and Bath groups quite valuable. I shall leave it to professional sociologists and anthropologists to appraise the ethnomethodological approaches (of which Knorr's is one) among these microstudies. Nevertheless, one word of caution is in order, concerning the danger of rapid overgeneralization. While philosophers (until recently at least) can be faulted for generalizing from zero case studies, it is also dangerous to generalize rapidly from one, two, or a small, biased sample of studies. Thus one finds quite general claims (and general conceptions) of scientific activity emerging from the studies of Knorr and of Latour and Woolgar, yet both parties focussed on laboratory life, and laboratories of particular sorts at that. Only this narrow breadth and depth of field can explain the undervaluation of theory to be found in Knorr's book, I believe (although I applaud her brief treatment of theories as policies and as resources). At least the status and role of theory should be left an open question, to await microstudies of think tanks, university departments, other scientific specialties, etc. It is true that the traditional emphasis on theoretical physics needs correcting, but it remains a mistake to swing at once to the opposite extreme.

In the same vein, I find it interesting (and mildly amusing) that an account which stresses the idiosyncratic nature of laboratory life at different research sites within the same subspecialty should lead to the tentative conclusion that there is no interesting difference between the natural and the social sciences (a sweeping, general conclusion), or, indeed, between the sciences and everyday life! To be fair, I think Knorr does succeed (and importantly) in showing how much the professional life of the scientist resembles that of lay persons. But once the scientist-as-practical-reasoner point is made and appreciated, one expects to be shown some notable differences among scientific forms of life, just as among lay forms. Would it not be surprising if so fine-grained an analysis, which can discriminate different sites within the same subfield, should not turn up significant differences between distinct scientific fields, not to mention lay endeavors? And it seems to me that while Knorr's book usefully raises the question about how the sociology of science is related to the sociology of scientists, it does not answer it.

Finally, Knorr's overemphasis on the local nature of scientific practice early in the book creates a tension with her interesting later critique of the standard use of the "scientific community" concept (Chapter 4). Her own investigation leads her to the conclusion that "The scientists' laboratory selections constantly refer us to a contextuality beyond the immediate site of the action (p. 81)." These contextualities are "variable transscientific fields," and they not only transcend the local research site but also go "beyond the boundaries of a scientific community or scientific field, however broadly defined (p. 82)."

To conclude: Karin Knorr has written an exciting and important book, one which should help to bridge the traditional divide between sociology and philosophy. As I said, some sociologists will prefer to burn their bridges before them; and most of my colleagues in philosophy will consider incredibly naive my optimism concerning epistemologically relevant, internal sociology of science.⁴ It will be easy (but inadvisable) for them to dismiss different parts of Knorr's book on various grounds: false dilemmas, a bias against theory, a localistic bias against universally adopted standards, the "that's what any fallibilist would expect" or "so what?" reaction; and the "construction in contrast to what?" reaction--to name the most likely. I dare not speak for the sociologists. But I am afraid that my colleagues in philosophy have not yet realized that they are about to be hit by a sociological wave comparable in effect to the historical wave which revolutionized our field twenty years ago. Indeed, in its introduction of "space ensembles" of methodologically distinguishable laboratories, schools, and other research sites, in addition to historical, "time ensembles," microsociology gives us a vivid sense of the uncertainty, the underdetermination, the openness of the future of science at any given time that history of science never quite achieved.

Notes:

1. Karin Knorr, "Relativism-What Now?", Social Studies of Science 12 (1982):133-136.
2. Jerome R. Ravetz, Scientific Knowledge and Its Social Problems, Oxford: University Press, 1971, p. 2.
3. Ways of Worldmaking, Indianapolis: Hackett Pub. Co., 1978.
4. The term 'epistemologically relevant, internal sociology of science' (ERISS) is Donald Campbell's.

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Reply to My Critics

Reply to Bruno Latour

It is rare for a writer to learn something from a review, and what is more to find some agreement with a critic. To be sure, my agreement with Bruno Latour (henceforth BL) is of the peculiar kind which rests on a more profound difference of opinion, and what I have learned I have learned not so much about my book than about my critic's scientific interests. However, I am grateful to BL for not producing one of those superficial and pointless misreadings by which authors feel regularly haunted, and for offering me an opportunity to point out our differences.

Bruno Latour castigates my book for not belonging to the genre of ethnography or anthropology, and for having more in common with what he perceives to be philosophy of science. I shall happily concede that I was not doing traditional anthropology in The Manufacture of Knowledge and hope to convince the reader that I had good reasons for not embarking on such an enterprise. Second, I shall gladly let myself be associated 'more' with philosophers' concerns than with those of anthropologists proper, and I shall explicate why I hold this preference. The profound difference of opinion upon which this agreement rests is that I wish to contribute to the sociology of scientific knowledge whereas my critic, from all I gather from this review, desires something else. Since the time of both of our fieldwork in California in 1976-77, I have been puzzled by an occasional difficulty in understanding BL's arguments, which become quite clear if one assumes that BL is not, like some of us, pursuing the questions of the social conditioning of scientific knowledge. This, and only this, makes comprehensible to me BL's continued insistence on the credibility model, a model which (alas, once again!) dissociates the content of scientific knowledge from any substantive non-technical connections. I consider BL's discussion of the genre of my book, his rejection of my "philosophical concerns," and his defense of the credibility-model the major points of his criticism. In the following, I shall take up these points in detail.

1. I begin with BL's claim that the "genre (of the book) is not ethnography or anthropology," that it does not tell us enough about "the group, its past, its folklore, about the material life of the people, about their gestures, instrumentation and craftsmanship" (p. 30). Traditionally, ethnographers of native cultures have indeed told such stories. They went out to collect a sort of inventory or natural history of tribal life, that is of a group's subsistence pattern (BL's "material life"), its technology ("instrumentation and craftsmanship") and its symbolic culture ("gestures", "folklore"). I have been trained as a traditional anthropologist, and I may remind BL that the above approach to tribal culture has been criticised heavily from within anthropology, for example by ethnoscience or by anthropologists like Clifford Geertz. I fully agree with this criticism, and though I cannot expound it here in detail, I may mention that it cast heavy doubt on the value of most received ethnographies of native tribes (e.g. Sturtevant, 1964).

But, of course, BL may use the notions ethnography and anthropology more loosely and less technically and simply mean that he would have liked to see considerably more of my "observations" and "field notes" incorporated into the text. If this is the gist of his complaint, however, I'm quite perplexed. Indeed, I would argue that my chapters 2, 3 and 5, together with sections in chapters 4 and 6, include more direct micro- ethnographic data (quotes from my observation protocol or from interviews with scientists and from scientists' writings) than any other laboratory study available in print. In particular, I thought that my claims about local idiosyncracies, indexicality and the social situatedness of scientists with which BL takes issue were illustrated rather heavily, even tediously, by such quotes. Perhaps BL does not simply want more empirical data but rather more specific information on "the group and its past" (see above), on budget, publications, citations, career patterns and interrelations (p. 32). My answer is simple but consequential: First, I did not go into the laboratory to research the history of a specialty group or to do citation analysis, which, I think, one can do better from the outside, as a vast number of competent non- ethnographic case studies on these topics indicate. Second, I have not been interested in the group of scientists per se, nor even in 'science' and its social context (e.g. budgets and publication lists), but instead in the production of knowledge. This is where my interests differ profoundly from those of my critic, who, from what I gather, would have me do received sociology of science (of specialty groups, specialty communication, career patterns and specialty histories) applied to the laboratory site.

Now I do not wish to suggest that the sociology of scientists' specialist groups is without merit, nor that scientists and knowledge are somehow independent. Quite to the contrary. But, as received sociology of science has demonstrated for several decades, one can study scientists' actions and communications (including their writing, as citation analysis shows) without ever approaching the question of how knowledge claims are built in and through scientific practice, of how they are accepted, overthrown, attached to and detached from 'nature'. BL knows as well as anybody that it is the singular characteristic of some recent developments in social studies of science to include in the investigation the content of scientific objects in a way that is pertinent to our understanding of knowledge (not science). For some of us at least this focus on the content of scientific knowledge does not just mean that we should study scientific work from a sociological perspective. Rather, it means a return to the hoary old puzzle of how knowledge is socially conditioned, the questions which Mannheim defined as constitutive for the sociology of knowledge. When I went to the laboratory, I was not interested in doing anthropology, and even less interested in doing traditional sociology of science applied to laboratory work. Instead, I wanted to use the ethnographic method to do traditional sociology of knowledge applied to scientific research. That is why, on p. 25, I refer to this work in terms of an "ethnography of knowledge," not science, or elsewhere in terms of an "empirical theory of knowledge" and an "empirical epistemology" (p. 3, 28).

(2) Of course, BL may reject the fruitfulness or feasibility of the sociology of scientific knowledge. If so he ought to have given us an argument to that effect. Instead he extends his critique of The Manufacture of Knowledge to what he considers to be its philosophical concerns. These ought to be avoided, he thinks, by the true sociologist of science. I fear that BL here simply continues to urge me to do something I had at the time deliberately chosen to forego: received sociology of science. But there is more to be said here.

While BL may well have studied philosophy for ten years, his comments about philosophers admit to at least two different conclusions. The first is that BL surveyed philosophy of science and found nothing worthwhile whatsoever in the trade: he found, astonishing even for the hard nosed sociologist, that "philosophers do not understand" even their own philosophical points (p.32). A second conclusion would be that BL has perhaps studied the wrong kind of philosophy? After all, his self-proclaimed expertise in the area lead to assertions which strangely contradict the writings of many recent social students of science. Most recent programs in empirical social studies of science acknowledge that they are indebted to, perhaps even heavily indebted to a historian-philosopher, Thomas Kuhn and such philosophers as Bohm, Toulmin, Feyerabend, even Richard Rorty and Nelson Goodman, not to mention Wittgenstein (on whom Bloor writes a book) or Mary Hesse, Jurgen Habermas or Karl Marx! Virtually all prominent British sociologists of science draw upon one or more of these philosophers, as do those Americans (e.g. Chubin, Restivo, Zenzen), Germans (e.g. Bohme, Daele, Krohn, Weingart) and perhaps even French sociologists who attend to the content of technical knowledge. One might also mention expertise controversies between some sociologists and philosophers (e.g. Bloor and Laudan), which surely testify, by the very possibility of their existence, to some mutual interest and some overlapping concerns (scientific knowledge?!) between philosophy of science and recent social studies of science. By the same token, one finds today's Presidents of the Philosophy of Science Association make positive reference to sociologists' work on science, as witnessed at the Philadelphia meetings, and one appreciates the opportunities provided at the same occasion for sociologists and philosophers to address common topics in the same session (e.g. on the context of discovery). Finally, does one really need to remind BL of the pragmatists (Dewey, Peirce) who, now nearly a century ago, insisted and demonstrated that philosophy can and must attend to scientific practice, a direction with which neo-pragmatist problem-solving models (Nickles, Laudan) and some recent "friends of discovery" among philosophers of science have much in common in their outlook on science as a practical activity? Not all philosophers live "much too far from the practice of science"! Most I know, including Lakatos (viz. his work on mathematics), attend at least to the history of science (though I do of course agree that certain kinds of history may not have done a service to philosophers).

My answer to BL's second major criticism then is twofold. First, I maintain, and think I have clarified above, that what BL baptizes "philosophical concerns" in my book are simply concerns of the sociology of scientific knowledge, unrecognized by my critic; concerns, that is, with the question of the social conditioning of scientific knowledge. Second, I wish to point out, should this be needed, that when one is interested in understanding scientific knowledge it is hard to avoid contacts with philosophers of science. Given that sociologists have until very recently avoided the question, philosophers and some philosopher-historians are the only ones who have devoted systematic attention to the topic. In fact, much of recent sociology of scientific knowledge can be seen, as Mary Hesse suggested, as providing the empirical basis for the thesis of underdetermination, proposed by the historian-philosopher Pierre Duhem at the turn of the century and subsequently promoted by philosophers such as W.v.o. Quine. It is after all not impossible to see certain theoretical insights by philosophers I have named (including many like Michel Serres whom I have not mentioned and to whom I feel increasingly indebted) as pre-empting most of sociologists' recent claims about knowledge. This, of course, need not disturb us; we may give empirical meaning to an

abstract thesis, and we are undoubtedly acquiring new insights and having an influence upon the very philosophy we started out with. For these reasons I would argue that I see no reasons whatsoever to try and avoid such contacts and that I find BL's sweeping statements offensive not only to philosophers, but also to sociologists. After all, do sociologists not laugh about the occasional philosopher who, without knowing enough about sociology, sweepingly dismisses the field or one of its pursuits (like sociology of knowledge or science)?

Now I think that BL's mischievous dismissal of philosophy of science has a simple explanation, as indicated before. I contend that he is not, like some of us, given to what Campbell and Restivo have called an "epistemologically relevant" sociology of science, and hence sees no reason to deal with or listen to philosophers. This is not easily apparent from BL's own laboratory study (Latour and Woolgar, 1979), though it is supported by BL's recent work on Pasteur (e.g. Latour, 1983). In Laboratory Life, laboratory inscriptions appear to be identified with scientific knowledge, and there is the notion that science centers around the transformation of inscriptions that are qualified as preliminary into statements equated with natural facts. While I thought at one time that this was to propose a conception of scientific knowledge, I now believe that I may have been mistaken. The idea of the transformation of statements, I contend, is no account at all, but merely a 'baptism' of what others have called the progress in science from hypotheses to scientific facts. Similarly, BL and SW's portrayal of the political strategies scientists pursue in and through their work also misses the mark. Scientists political stratagemes do not enter and shape knowledge in quasi-automatic ways; hence one cannot assume that the portrayal of such stratagemes is in itself an account of how knowledge is socially conditioned. Take an example. Previously we thought that scientific truth may best be served by scientists' virtuous adherence to the norms of universalism, communism, disinterestedness and organized skepticism. Now we may think that perhaps the progress of knowledge is served better when scientists act like scientific entrepreneurs, ruthless maximizers and investors of "scientific credit." In other words, it is perfectly conceivable that scientists may pursue entrepreneurial strategies without in the least affecting the content of their knowledge claims. Nothing in Laboratory Life substantiates how scientists' political strategies may enter the content of knowledge and account, say, for how and why TRF is TRF. Rather, Latour and Woolgar's third chapter takes the construction of a fact to refer to "how, where and why it was created" (1979:127), which turns out to mean how, where and why Guillemin arrived at THE structure of TRF. The reference to political strategies in Laboratory Life remains within the framework set by Bourdieu (1975): By intention or consequence, "scientists' scientific pursuits are at the same time" political strategies. Not: technical results are socially conditioned, but technical operations are, intentionally or not, vehicles or political operations. If successful, they lead to the accumulation of scientific credit.

One further point needs mentioning. While I fully share BL's fascination with literary inscriptions I do not share his apparent belief that these can be equated with scientific knowledge without the added component of the variable interpretation of these inscriptions. But it is precisely this interpretative flexibility which BL seems to think inscription devices eliminate when he portrays them as unproblematic instruments of persuasion, of complexity reduction or of consensus formation (Latour, 1982).

The identification of literary inscriptions with knowledge per se in Laboratory Life make sense if we see inscription devices as tools in eliminating indexicality and variable interpretations. Admittedly, these are obnoxious features of social life; so much so that they are not likely to be scared away by appeal to inscription devices. BL appears to think that in scientific laboratories, human beings somehow manage without reasoning, and resort instead to the writings of inscriptions that by some marvel turn out to be interpretation-independent (p. 31). I think that most of our laboratory data consist of scientists' reasoning, since their literary inscriptions are literally meaningless, to them and to us, without continuous interpretations. BL considers the word scientific "reasoner" "like a fossil of a bygone period" (p. 31). Now I don't know in which time dimension BL dwells with his inscription devices, but I have a hunch that it might be a fictional world inhabited by few serious scholars concerned with the social studies of science. To think that one can catapult scientists' reasoning (interpretation) out of scientific practice by means of inscription devices strikes me as a fossil idea. Have we not tried, for decades, in philosophy and sociology of science, to claim that scientific theory choice, scientific controversies, etc. can be settled by means of scientific experiments, that is, by a process of trying nature with the help of technical instrumentation? And is not the very notion of a new sociology of scientific knowledge spurred by the idea of underdetermination based on the insight that we had failed? The output of an inscription device, say a peak in a curve, can be unambiguously persuasive only on the basis of stable practices of recognizing and interpreting the peak as "a peak." I believe that it is such practice which sustain the persuasive efficacy of inscriptions, not the other way round. Hence my emphasis on reasoning practices in The Manufacture of Knowledge and hence the emphasis on irremediable indexicality. It is, of course, possible that I have failed to grasp the radical implications BL appears to attribute to inscription devices with regard to interpretation. But until BL has shown us how his magic of eliminating indexicality and interpretative flexibility with the help of inscription devices works, I shall expect that we will have to live with these inflictions for quite another while.

(3) BL's third major critique centers on his defense of his credibility model against my concept of transscientific (or transepistemic) fields. I submit that the credibility model equally does not transcend the Bourdieu's framework mentioned earlier (though I do not doubt that it may transcend particular claims of Bourdieu's, 1975). And, as I have argued before, I submit that BL and his co-author may not intend such transgressions. If one makes this assumption, BL's continued defenses of the credibility model becomes understandable, as well as his failure to see that my notion of a transepistemic (or transscientific) field intends to cope with questions very different from his. This being said, it is easy enough to counter BL's attack on my attack of the credibility model in The Manufacture of Knowledge.

The crux of my criticism (which BL chooses to ignore in his presentation) is that I consider the credibility model a rehash (BL's terminology) of the idea, as old as sociology of science, that we must conceive of the relation between science and society in terms of a (quasi-economic) exchange model: Money is converted into equipment, equipment into data, data into arguments, arguments into articles, articles into recognition and recognition into (grant) money. BL synthesizes the steps in this process of exchange by proposing the mnemonically useful devise of the picture of a cycle. But is a step back to use a laboratory study to come up with a reconstruction of internal-external relations in terms

of exchange for the following reason: like all previous exchange models of science, it insulates the content of scientific knowledge from the influence of society (other than mediated, as previously, by money transfer, career chances etc.). It hardly needed saying that such transfers happen, but it is neat to picture some of the conversions involved (the model leaves out 'problems' for example) in cyclical form. Other than that I fail to see what substantial advances the credibility model adds to previous exchange ideas (functionalists, too, were concerned about system properties and not about motivations). In particular, the question today and one uniquely suited to though surely not the sole purpose of laboratory research is how knowledge is socially constituted, not how scientific organizations (labs) or professorships, or equipment, are socially supported.

I want to add, as I did in The Manufacture of Knowledge, that the model may be dangerous (another of my criticisms BL chooses to disregard). The simple and unequivocal conversion the model implies of research products into money and career may lead us back directly to an outdated interpretation of social stratification. Applied to the credibility model, this suggests that those who produce the most credible information in greatest quantity will be those who earn the better 'positions' in science (more recognition, more research money, more equipment etc.). Twenty years of social research have shown that achievement (here: the production of credible information) may well play a minor role in explaining social stratification. BL cannot possibly wish to legitimize prevailing forms of stratification in science by a demonstrably misleading model of social stratification.

Thus, I repeat, that I consider the credibility model beside the point of recent sociology of scientific knowledge, though it may continue to make sense to talk in terms of the language of exchange if one is not interested in explaining the social constitution of knowledge itself. I readily admit that the notion of transscientific fields proposed in The Manufacture of Knowledge does not offer a cycle of transformation to hang on to. But once we leave the cozy and familiar fields of quasi-economic exchange to talk about the content of science, model-elegance is more difficult to achieve. Moreover, it may be premature at best to synthesize what little we know about the (social) construction of knowledge into fully fledged models. But I may assure BL that it is the goal of my present research to add more flesh to the sketch of transscientific fields. After all, it is no small matter to replace the cherished heart of much sociology of science, the idea of scientific communities, by what Edge has called a "radically participant-centered conception" (1979).

To conclude the first part of my reply let me note that I have inadvertently written a critique of BL's work in the process of defending myself against BL's criticism. It occurs to me that The Manufacture of Knowledge may be read as an implicit and sometimes explicit critique of Laboratory Life, even though it was not intended to be: the making of knowledge versus scientists' lab life, reasoning and interpretation versus inscription, transscientific or transeπισtemic fields versus credibility cycles. Our differences make sense: I have offered as their explanation that BL is not really interested in the question of the social conditioning of knowledge objects in themselves, whereas I have been. I should like to emphasize, at the end of this rather harsh rebuttal that there exist a number of goals in social studies of science which I do not wish to pursue at this point in time, but which I nevertheless hold in high esteem. Bruno's goal, if I read it correctly this time, is one

of them. For example, I think that his work on Pasteur may be the first real challenge of prevailing conceptions of the impact of science on society, or of the role science plays in society. I do not think that this work is the first real challenge of the sociology of scientific knowledge.

Reply to Thomas Nickles

Thomas Nickles' review of The Manufacture of Knowledge raises a series of questions more interesting than those posed by BL in the sense that they cannot easily be answered. The empirical question of knowledge justification in science, the question of local contingencies in scientific inquiry, or the conception of science as a problem-solving activity do not lend themselves lightly to coarse discussions in little space. However, I shall try to offer some remarks which point out the direction in which a more thorough analysis might go.

First, the constructive-descriptive distinction, and my possible accord with positions like that of Nelson Goodman. I agree with Nickles that the constructive-descriptive distinction may be obsolete in principle, though I think that in practice it continues to be useful to drive home certain points. I also concede that Goodman's notion of Ways of Worldmaking engagingly depicts a version of constructivism, and that it is tempting to follow him on his trail. If I hesitate it is because I am not quite sure where he wants to lead us to, despite reading and rereading Goodman a couple of times. I shun the consequences and the restraints of representational vernaculars. To be sure, Goodman treats physical theory and commonsense depictions of the world as different world versions which share no neutral description. Yet in my reading of Goodman these versions retain the character of representations, even though there is no one world for them to be representations of. Reference dissolves, or so it seems, in differences between world conceptions.

With my version of constructivism, I wanted to raise the possibility of dissolving reference into practices, the practices of fabricating bits and pieces of the world-to-be from, among other things, brute pre-interpreted matter. Antecedent 'reality' continuously enters the picture, for example as the source material from which worlds-to-be are wrested. But these antecedent realities do not provide for the referent; rather they provide for material (matter-based) practices of achieving certain technical effects. Reality is an upshot of successful practices of encountering these effects. Strictly speaking, one can explain successful predictions in science in at least two different ways: one can take them as an indication that one has captured something real, or one can take them as an indication that one has successfully instituted practices of (re) generating a certain technical effect (an effect that can be reproduced as 'identical' for the purpose at hand). It is plain that the second interpretation is likely to lead to a breakdown of the distinction between science and technology. And my adopting this interpretation explains to some extent why I choose the title, The Manufacture of Knowledge.

Now it is clear that I do not, in the book, elaborate a constructivist theory of reference on a philosophical level. This is why The Manufacture of Knowledge is not an attempt to contribute to the philosophy of science. It is equally clear that sociologists and some philosophers today have doubts about the feasibility and desirability of basing theories of knowledge on a philosophical foundation. But in one way or another, the question of reference Nickles raises will have to be addressed by future laboratory studies in some depth, if only to avoid subjectivist or naively relativist misinterpretations by critics less conscientious and informed than Nickles.

A second issue discussed by Nickles on which I want to add some brief remarks is my conception of scientific work as solution-driven, which contrasts with his and others' conception of science as a problem-solving activity. I think that our differences on this point may be partly but not wholly linguistic. Depending on where we begin to look at a chain of scientific actions, we will find scientists who have "run into troubles" or have "come up with an excellent idea." Furthermore, solutions pose problems of "making them work," and problems can, as Callon has argued (1975), be translated into solutions. What Nickles calls a good research problem may in my terminology be called a solution, since it may be a good question precisely because it promises an interesting "solution." As Nickles has argued elsewhere, in erotetic logic a question is defined in terms of a set of possible answers, a definition not unlike his own (1980:36).

Now it is plain that once the relation between problems and solutions becomes tautological, it makes no sense to distinguish between solution-driven versus problem-driven logics of research. I want to point out that in scientific inquiry, scientists do distinguish between troubles and opportunities, between problems and problem-solutions. But they do not distinguish between these according to one invariable criterion. One wants to urge the philosopher interested in scientific practice to not confound distinctions which scientists make routinely, and to confront the problem of locally contingent variable interpretations of these distinctions. While acknowledging the questions possibly raised by a solution-driven account of inquiry for the problem-solving model, Nickles dismisses too lightly, I think, the questions 'localistic' accounts of scientific work have in store for philosophers.

Briefly put, my and others' insistence on the local character of scientific inquiry implies that the outcome of scientific action is shaped by interpretations of rules and decision criteria that vary over different situations. I submit that in addition to the potential breakdown of standards and decision criteria during periods of controversy, there exist a continuous though mostly invisible breakdown and renegotiation of the relevance and meaning of these criteria in everyday laboratory life. I hold this phenomenon to be crucially relevant to an understanding of knowledge production, because, I repeat, it is these local renegotiations of the meaning and relevance of general rules which determines the content and structure of resulting knowledge claims, and not the general rules or criteria in themselves. It is this interpretative variability of natural science inquiry which has led me to point out certain similarities to social science inquiry. Once one accepts the interpretative flexibility of natural science inquiry, much, perhaps all, of the alleged distinctiveness of social science inquiry disappears.

I want to conclude my reply to T. Nickles by fully accepting his cautioning us against the danger of rapid overgeneralization from one, or a few laboratory ethnographies and other microstudies of science. I think he is right, and many more such studies are needed. I would also think that the micro-sociology of scientific knowledge can profit from communication, with perhaps even collaboration with those philosophers of science who are interested in scientific practice in areas such as the problem of indexical contingency posed above. To some of my sociologist-friends such suggestions will appear heretical. I trust in their reflecting that they may after all have been exorcising strawmen, and that the time may have come for a more mature attitude toward interdisciplinary cooperation.

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BOOK REVIEWS

Barry Barnes, T. S. Kuhn and Social Science. New York: Columbia University Press, 1982. 135 pp. \$20.00 cloth, \$10.00 paper.

Extending his past work, Barry Barnes has written an excellent book, lucid, exceptionally well written, and a powerful statement of his own "strong program" for a constructivist and instrumentalist sociology of science. Although, as the title, T.S. Kuhn and Social Science, indicates, he takes off from Kuhn's work, he goes beyond that work in a number of ways. For one thing, though he provides a fine account of Kuhn's sources, discoveries, and consequences, he makes Kuhn's constructivist theory more explicit and better grounded than Kuhn himself did. He has both a generous appreciation of Kuhn's work and a useful awareness of its limitations. For another thing, he points out the lack of any basic theory or explanation in Kuhn of why normal science and scientific revolutions occur as they do. His own explanatory theory is what he calls an "instrumentalist" one, a social determinism through the influence of "goals and interests."

Barnes is nicely clear about the significance of his book for general sociological theory. Although he is mostly concerned with scientific ideas, his constructivist and instrumentalist theories are meant to apply as well to all "knowledge," to use that vague term we inherited in the sociology of knowledge from the German wissenssoziologie. Indeed, these theories are applicable to all of "culture" in the way that Parsons defined the term, that is, all types of idea- or symbol-systems. Thus, as with all the best works that start by discussing the thought of some important social theorist, such as Kuhn, Barnes uses his subject as a vehicle to make his own theoretical contribution. That, of course, is what Parsons did in The Structure of Social Action with Weber, Durkheim, Marshall, and Pareto. Like Parsons, Barnes has produced a book that will be of interest to all social scientists who take their basic theory seriously.

One of the virtues of Barnes' work is his philosophical sophistication and his intimate knowledge of the substance of scientific ideas. This sophistication and knowledge allow him to use or criticize, as necessary, the valuable new empirical work in the sociology of scientific ideas that his colleagues, especially in Britain, but also in Europe and in the United States, have recently produced. It also allows him to be critical, as he predominantly is, of non-empirical, "moralizing" philosophers of science like Popper, Lakatos, and Scheffler. He knows how to use well such other philosophers of science as Wittgenstein, Hesse, and Quine. This philosophical sophistication and intimate knowledge of the substance of science is a happy recent development in the sociology of science. Indeed, such sophistication and substantive knowledge would be a happy development in any sociological specialty studying any of the different cultural spheres such as art, music, religion, or language. In the early development of the sociology of science, pioneers like me tended to restrict ourselves primarily to concern with the institutional structures of science and their dynamics because of our large ignorance of the substance of science. When we did broach substantive science topics, as I did, for example, in papers like "The Floppy-Eared Rabbits" or "Resistance by Scientists to Scientific Discovery," we had to proceed carefully and clumsily. Such an approach was not due, as has some-

times been alleged, to any principled lack of interest in culture of idea-systems, but only to awareness of the limitations of our expertise. In fact, in principle, anyone who accepts, as I do, the Parsonian theory that culture is as much an independent variable in social systems as social structure or personality, has to find the study of scientific idea-systems as important as the study of the social structural or institutional aspects of science. On this theoretical view, the emergence of sociologists of science who are expert in the substance of scientific ideas has been very welcome.

Like all important books in sociological theory, Barnes' book has its obscurities and limitations. For example, it is not clear to me precisely how he deals with the relativism problem. He seems to be saying that scientific ideas are co-determined by previous scientific ideas, by other cultural systems, by various social structural components of the social system, and by "nature." This co-determination theory would be satisfactory: it avoids the radical relativism that leaves out, as some of his colleagues have, either ideas or "nature." But Barnes is obscure on this fundamental point and I hope he will make his views sharper next time around.

Another limitation occurs in Barnes' discussion of his "instrumental" determinants, his "goals and interests." It is not specified what these are in any general theoretical terms, nor is any comprehensive set or classification of them described. Among the "goals and interests" that Barnes mentions are: "prediction and control" (a la Habermas, with qualifications), "professional vested interests," and "socio-political interests." We are not told whether this list is exhaustive, nor how the items are related to each other, nor how they are related to other possible interests, nor just what each of them means. Are "goals and interests" derived from both culture and social structure? That would be acceptable, but since Barnes does not give us a systematic statement about this important theoretical category, we cannot answer our question. His list is undefined, ad hoc, and therefore unsatisfactory.

Finally, in relating his constructivist and instrumentalist theory to other current sociological theories, Barnes has brief but useful appreciations and critiques of symbolic interactionism and ethnomethodology. I cannot say the same for his discussion of "functionalism." Here he repeats the hoary error that functionalism is a theory that asserts the exclusivity and predominance of values as determinants of all social structure and the other parts of culture. It is significant that he cites no examples of work done under the influence of this alleged functional view. He probably could find a few mistaken folk, but mostly, functionalists have tried to show the inter-workings of social structure and culture in behavior and they have treated values as only one of the many components of culture, along with science, religion, philosophy, ideology, language, and the arts. There is a good deal in so-called "functionalist" sociology which is in agreement with Barnes, and much else also from which he could learn. Constructivism is certainly acceptable to functionalism; it was used by functionalists long before the term "constructivism" became popular. We were brought up on W. I. Thomas' concept of "the definition of the situation," on Merton's concepts of "self-fulfilling" and "suicidal prophecies," and especially on Parsons' concept of "culture." And Barnes' search for an explanatory

theory for the development of normal and revolutionary science and indeed of all culture is exactly the search the functionalists are engaged in. My Science and the Social Order (1952) has two chapters on the social influences on the development of science, one historical, the other treating the contemporary scene. It is time, all around, to give up prejudice, stereotypes, hostility, and defensiveness and get on with the job of creating a better sociology.

I strongly recommend Barnes' book to all social scientists and historians, not just sociologists of science. It is an important attempt to construct basic social theory. It asks the right questions and gives interesting answers. If these are not always correct, they provide a valuable challenge for us to do better.

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Catherine Ailes and Francis W. Rushing, The Science Race (New York: Crane, Russak and Company, 1982), 254 pp.*

Between 1972 and 1974, the United States and the Soviet Union entered into a series of eleven bilateral agreements in areas of public policy that were affected directly by advances in science and technology. These included agreements for cooperation between the two countries in fields of health, transportation, energy, and environmental protection. The bilaterals were administered on each side by the respective government ministries, state committees, and federal agencies, with the consultation and active participation of specialists from universities, from industry, and from other professional communities.

Catherine Ailes and Francis Rushing have provided us with a valuable summary of the data gathered under one of the bilaterals, namely the US-USSR Agreement on Cooperation in the Fields of Science and Technology. Usually referred to as the S&T Agreement, this was the bilateral that was broadest in scope. It was divided into thirteen working groups, with projects in microbiology, in physics, in electrometallurgy, and in the application of computers to management. One of the working groups was on science policy, and it was divided further into four subgroups: planning and management of research and development, training and utilization of scientific and technical personnel, systems for stimulating the development of fundamental research, and financing research and development. The data described by Ailes and Rushing came from the training and utilization subgroup -- clearly a very small portion of the overall quantity of material exchanged between the USA and the USSR under all of the bilaterals.

The quality of data received on both sides was affected directly by the political and bureaucratic contexts in which the exchange of data took place. This was particularly true for the information that Americans received in the science policy working group. The US team provided information on the American research network, and the Soviets did the same for their country. This meant that neither team had direct access to the raw data on the other side. The result was that categories of information on which both teams had agreed were often interpreted differently, and the resulting statistics and reports were not exactly parallel. Both sides suffered from bureaucratic problems as well, but members of the Soviet working group were more limited than were the Americans in the kind of detailed documentation that they could make available. It is telling, for example, that some of the reports provided by the Soviet subgroup on the training and utilization of scientific manpower were from articles already published in professional journals.

An appraisal of the Ailes - Rushing book would have to take all of this into account. Their data base consisted primarily of the reports and other materials gathered under one subdivision of the S&T Agreement. Fortunately, they supplemented these materials with research from the publications of American specialists in the field in order to provide a more general over-

*This review was written while the author was a National Fellow at the Hoover Institution, Stanford University.

view of the setting for education and employment of scientists and engineers in each country. The result is a book that is both a summary of the data obtained under the bilateral and an introduction to the expansion and distribution of human resources in the USA and the USSR.

The opening chapter sets up the basic contrasts between a planned economy and a market economy, and the impact of each on the supply and utilization of scientific and technical personnel. In a later chapter, the authors elaborate on the different methods by which specialists in each country try to forecast future manpower needs. They conclude that both societies face difficulties in balancing the demand for scientists and engineers with the number of young people entering these professions. The authors caution that, although the United States may appear to have fewer demographic constraints than does the USSR, it still suffers from the lack of a national policy for the development and support of scientific and engineering manpower in the years ahead.

A major portion of the book is devoted to Soviet and American educational policies and the role they play in the training and recruitment of scientists and engineers. After a very elementary introduction to the institutional differences in the education systems, the authors present a potentially interesting comparison of the processes by which technicians are trained. However, the brief chapter on technical training in specialized secondary schools and in industry lacks a conclusion, so that the point of the comparison is lost. The authors do a better job in their discussions of higher education and graduate training, although here too the coverage is very general. They tie the main differences in educational achievements to an underlying difference in philosophy when they note, for example, that even though the general student population in the USSR receives better preparation in science and mathematics, the more diversified curricula in the USA have other advantages for students who will be working in a complex and interdependent world.

The second major emphasis in the book is on the employment of Soviet and American scientists and engineers. It is extremely difficult to measure the exact size of the scientific and engineering populations, because each government uses different definitions and statistical categories. The authors turn, instead, to trends and growth rates within each country. The tables and charts are useful for seeing the general dimensions of the research and development communities. More interesting, however, are the two chapters on the distribution and mobility of scientists and engineers among different employment sectors.

On the whole, Ailes and Rushing have done a good job of summarizing and presenting the data from the bilateral exchange. They have organized the material very clearly and have given adequate background on the basic institutional dynamics of each society. The treatment is sober and well balanced, also, with regard to the differences and similarities between the Soviet and American R&D systems. An elaboration of these observations would have enriched the study considerably. It is, however, an easily readable and informative book for a general audience.

The people who will be least satisfied are those who are specialists in political science, comparative sociology, education, and science policy. They will find the book lacking in depth and avoiding some of the fundamental issues of political and social analysis. There is no discussion, for example, of how the policymakers utilize these data, if at all, in the making of science and technology policy. What is the political significance of knowing that there is a different distribution of engineering personnel in each country? Do these data tell us something we did already know about Soviet and American capabilities and achievements? If there is a "science race," who is running and what is the prize? It is doubtful that a quantitative increase in the number of scientists and engineers is as meaningful a measure of scientific capabilities as is a comparison of their scientific and technical performance. And, in the end, it is the performance that is most critical in assessing which nation is ahead in any particular field.

Scholars in the fields of education and comparative sociology would expect, among other things, to find a discussion of the substantive differences and similarities in the content of what is being taught in the classrooms, the attitudes of students toward learning about science and technology, and the problems young scientists and engineers encounter when they try to apply what they learned in school to their jobs. The authors do not include enough data on which social groups are being recruited into scientific and technical fields. Nor do they explain the differences in social status and political influence that scientists and engineers have in each country. The data would be far more meaningful if it were analyzed in terms of the changes that normally occur in highly industrialized societies. Patterns of social stratification and career mobility, for example, are usually set within traditions that help explain the differences in the distribution of professional personnel, despite some of the underlying similarities that arise from industrial development. However, the statistics in this volume are not related to any theoretical or conceptual framework, which is why we are left without a clear perspective regarding the data's significance.

Finally, students of Soviet science will find little that is new or challenging. We can learn more about Soviet scientific and technical personnel by reading Soviet publications on the attitudes of young scientists and engineers, on their recruitment into the workforce, on their career mobility, and on their job satisfaction. Surveys, such as those conducted by Shcherbakov, Nikandrov and Tarasov,¹ reveal that students who specialize in science are not always motivated by an intellectual commitment to their profession, and they do not always receive the quality of training necessary for their subsequent research. There is a high turnover of young scientists and engineers in the employment sector, caused partly by their dissatisfaction with work or with their job positions and partly by the lack of symmetry between training and employment skills. Additional problems exist in personnel management and in the formal channels of communication between scientists and engineers. Overriding these institutional and traditional constraints, however, are informal associations among scientists that keep major portions of the research sector alive and well. The fact that Ailes and Rushing do not explore these issues in any depth is not so much a failure on their part as it is a reflection of the limitations in the data that has been gathered through inter-governmental sources.

The implementation of the S&T Agreement was a lesson in international politics, as the spirit of detente in the early 1970s gave way to greater tensions between the USA and the USSR by the end of the decade. The experiences of implementing the bilaterals were lessons also in the domestic politics of US foreign policy. Representatives of different government agencies had to justify their programs of US-USSR cooperation at congressional hearings. Private citizens lent their support and advice to the different stages of program development. There was a decentralized process of decisionmaking for the selection of projects, a division of responsibilities for carrying them out, and very little coordination across the board. And, the emotional issues of human rights, which were seen initially as an obstacle to continuation of the agreements, eventually became central to the protection of the American scientific community on the domestic front.²

The governmental process in the Soviet Union also played a significant role in the way the agreements were structured. Similar assessments were being made in the Soviet Union regarding the relative value of different programs. Soviet specialists on US science and technology were represented in the Soviet working groups, but not as much as would have been desirable. It was revealing to see that Soviet officials were asking the American team questions that could easily have been answered by scholars in the USSR, and, initially at least, a similar situation existed in the United States. People whose career experiences were in the government seemed to place more credibility on information coming from their counterparts in the other country, than they did on information gathered by scholars at home. This situation seems to be changing in the United States as greater confidence and better working relationships have developed between the academic and governmental communities, largely as a result of this exercise.

Notes:

1. A. I. Shcherbakov, Sotsial'no-ekonomicheskie problemy effektivnosti nauchnogo truda (Novosibirsk: Nauka, 1975); O.M. Nikandrov and V.F. Tarasov, "Nekotorye voprosy effektivnosti podgotovki i truda molodykh nauchnykh kollektivov," in Problemy deiatel'nosti uchenogo i nauchnykh kollektivov, VI (Moskva: Akademiia nauk SSSR, 1977). See Linda Lubrano, "Survey Research As A Source Of Information For Soviet Science Policy," Soviet Union/Union Soviétique (forthcoming).
2. See Linda Lubrano, "National and International Politics in US-USSR Scientific Cooperation," Social Studies of Science, Vol. 11, No. 4 (November, 1981), pp. 451-80.

Linda L. Lubrano
School of International
Service
The American University

Russell McCormmach, Night Thoughts of a Classical Physicist.

Russell McCormmach's Night Thoughts of a Classical Physicist is an exciting and remarkable book. Yet the very things which make it so remarkable also make it a very difficult book to review. The difficulty stems from the fact that McCormmach has attempted to interweave a variety of formats long available to historians but seldom if ever used together in the same document. The reviewer's dilemma is twofold: first where to begin and then how to reflect, in a limited space, the richness not only of content, but of form as well.

One is tempted to compare Night Thoughts to historical novels. And the one that immediately comes to mind is Max Brod's The Redemption of Tycho Brahe in which Brod used the fictional form to construct a context for considering what the encounter between Johannes Kepler and Tycho Brahe might have been like given the character of the two men, their respective social backgrounds and positions, and the nature of the times in which they lived.

But McCormmach's central character, Professor Jacob, while historical, is not like any person who has ever lived. Rather than representing a particular person or being representative of a kind of person, he is representative of a class--the class commonly referred to as the German mandarins. His thoughts are not the thoughts of one person, but, as McCormmach documents in a set of end notes, they are the thoughts of many late nineteenth and early twentieth century German physicists. The range is broad and includes Max Planck and Wilhelm Wien, Theodor Des Coudres and Otto Wiener, Woldemar Voigt and Gustav Mie, Felix Auerbach and Heinrich Hertz, to mention only a few. The upshot is that the only fiction in this narrative is the central character. His history, his thoughts, and most of his actions in the narrative are a pastiche of the thoughts and actions of the genuine articles--German mandarin physicists. As is so often the case with such pasteups, the character lacks depth. There is a two dimensional quality about him which even results in a lack of focus. We are presented with a character whose thoughts are representative of a class of people. They might come from sources as different (yet as similar) as Richard Nixon and Ronald Reagan.

In spite of this difficulty, McCormmach's skill is such that from the very first words of the novel a kafka-like shroud of doom surrounds the central character:

Would it ever get light, the old professor wondered.
He felt the cold rise up his arms.

It is not, however, the doom of a person we are witnessing. It is the demise of a social class and attendant social institutions. And it is because of this that as a novel, McCormmach's efforts must be judged a failure. Virtually nothing happens. The central character hardly does anything but ruminate about the past in light of the shortcomings of the present. Though he meets people, and though he converses with his wife and acquaintances, they have little more dimensionality than Jacob himself. It amounts to a literary version of the Bergmann film Wild Strawberries and the transfer of the device to another medium is not very successful. By the middle of the novel, the narrative runs out of gas and the reader who had no other perspective is likely

to give up. Fortunately, there are other perspectives. In fact, it is a mistake to judge Night Thoughts as a novel.

In presenting us with the ruminations of his central character, McCormmach has relied on personal letters and official documents contained in many university archives. I have visited most of those archives and examined many of the documents that McCormmach has used. In truth, the ghosts represented by those documents never took on the human dimensions for men (and I suspect I speak for most historians) that they took on for McCormmach. His sensitive use of that material, the artful way he has woven it together for the purposes of the narrative has peeled the veil from my eyes with regard to the human meaning of archival documents. It has been a transforming experience. In fact, I would make Night Thoughts required reading for all beginning graduate students in history.

Night Thoughts is also an important document in the examination of the social aspects of the history of ideas. The character of the German academic community and its social niche always has had qualities which make it appear strange to outsiders--difficult for an American to comprehend, for example. It is very easy to get lost in the seemingly unending and mystifying labyrinth of relationships among research, work, Kultur, Wissenschaft, and sense of duty. McCormmach's narrative is aimed at making sense of that labyrinth and to my mind it admirably accomplishes that task. What McCormmach has provided for his readers is the equivalent of a language translation for the relationship of work, play and values among German academics. The book is a kind of German-American dictionary.

Of course, other scholars have written on the social character of academic communities. One thinks for example of Fritz Ringer's excellent book, The Decline of the Mandarins. It is interesting to compare the qualities of the two books. There is a warmth to McCormmach's work which is lacking in the Ringer book. It is not that one is better than the other. They are different and complementary.

McCormmach was editor of the journal Historical Studies in Physical Science. He had the reputation of being a wonderfully stern taskmaster. When it came to attempts on the part of authors to characterize the relationship between personal aspects of dramatis personae and their professional work, McCormmach was particularly sensitive to standards of scholarship and (if my experience was at all typical) discouraged such discussion as inappropriate. McCormmach's vision for the format under which such discussions are appropriate now becomes clear. He has given us another important object lesson in historiography.

The central incident in McCormmach's narrative is an excellent example of the power of this approach. The incident actually took place some ten years prior to the time in which the book is set. That event, which more and more dominates the thoughts of McCormmach's central character, Professor Jakob, was the suicide of the German physicist, Paul Drude. Drude had been a rising star among the mandarins. He was the author of several important monographs and textbooks. In 1905, he had accepted "the call" to Berlin where he assumed the directorship of the physics institute at the university. He had done this with some reluctance since Berlin, being the mecca of world physics represented the center of an incredibly intense maelstrom. At the same time

Drude was editor of the world's most important and prestigious physics journal Annalen der Physik (Annals of Physics). A year later, he was dead.

McCormmach seems to have been riveted by Drude's untimely death. Drude might have become the Hermann von Helmholtz of his generation. What was it that drove him to such a desperate, irreversibly final, act? One gets the impression that it is a puzzle which has, at some time, dominated McCormmach's own thoughts. And now that he had finally understood it, he looked around for an appropriate medium with which to communicate his insights. We can only thank him for being so clever and brave in showing what might be a proper vehicle.

Stanley Goldberg
Hampshire College

"Bridge to the Future"

A Symposium Commemorating the Centennial of the Brooklyn Bridge

May 18-20, 1983

The Barbizon-Plaza Hotel, New York City

Sponsored by

Brooklyn Rediscovery,
a program of the Brooklyn Educational & Cultural Alliance, and

The New York Academy of Sciences

Using the occasion of the centennial of the Brooklyn Bridge, this three-day symposium will address the subject of human creativity and the ways that individuals and groups influence and are influenced by technological development. Special events to be held in conjunction with this symposium include a spectacular Brooklyn Bridge walk at sunset, and a visit to the Brooklyn Museum to view a major bridge exhibition followed by a reception at the world-renowned Brooklyn Botanic Garden.

The conference chairpersons are: Melvin Kranzberg, Georgia Institute of Technology, Atlanta, Georgia; and Margaret Latimer, Director, Brooklyn Rediscovery, Brooklyn Educational & Cultural Alliance, Brooklyn, New York.

The symposium has been made possible through the generous support of the National Endowment for the Humanities.

FOR FURTHER INFORMATION, CONTACT: Conference Department, The New York Academy of Sciences, 2 East 63rd Street, New York, NY 10021, 212-838-0230.

THOUGHT AND OPINION

The Significance of "Science Studies"

Science Studies is an interdisciplinary field concerned with the ever-changing ideas and institutions of science, technology, and medicine, and their interactions with other aspects of economic and cultural life. It draws on historical, philosophical, political, economic and sociological scholarship, and on the experience of practitioners of science and technology. It extends from the historical and philosophical analysis of the origins of the experimental method, to the economics and management of industrial research laboratories; from the past and present interactions of biological and social thought to the politics of advanced medical technologies; from the source of scientific ideas to the consequences of their applications.

Advanced study and research in this field is currently one of the most stimulating and demanding areas of academic endeavour. A number of established subdisciplines are united around a single major theme calling on an unusual diversity of scholarly skills. To contribute to this field an intimate acquaintance with particular areas of science, technology or medicine must be combined with other skills, such as the ability to sift data and make imaginative use of recalcitrant and diverse sources of information. Because of the intellectual rigours of the subject, and the central role of science and technology in our culture, these studies also contribute more to the theories and methods of their parent disciplines than their relative size would suggest. It is important that academic opportunities to enter this rapidly developing field and to advance it intellectually should not be curtailed.

The educational significance of Science Studies is as wide and deep as its scholarly concerns. As an element in the training of scientists, engineers, and doctors, it ensures that they appreciate the conceptual foundations of their disciplines and of the ways in which their work interacts with the interests of the wider society. It can be the integrating theme in courses combining a number of specialist subjects. It provides humanities, social science and education students with an informed view of the nature of science, technology and medicine and of their cultural and social significance. It can be the means of reintroducing the culture of science to students of limited scientific or technological background.

With proper support, Science Studies can make an important contribution to the creation of a citizenry capable of making the kind of informed judgments on scientific matters that are essential in a modern industrial democracy. There is also clear evidence that such studies are of immediate practical benefit to students in their subsequent careers. Unlike students whose training has been narrowly confined to a single traditional academic discipline, students with some experience of Science Studies show a greater willingness to come to grips with the complexities of industrial society, and their career patterns indicate that they are prepared for situations that demand new approaches. Courses in this interdisciplinary field should be available to students in all institutions of higher education as major or minor components of their degrees.

Science Studies Committee
Council for Science and Society
United Kingdom

On the Support of Research on Ethical Problems
in Biomedical Research and Clinical Medicine

In July, 1982, a small group of people familiar with the history and work of the program on Ethical and Value Issues in Science and Technology (EVIST) of the National Science Foundation (NSF), were asked to review the work of that program and to provide consultation on possible future directions for it. During the course of a consultation meeting, held on July 12-13, 1982 it became evident to the ad hoc advisory group that broader federal support was needed for research programs on ethical issues in science and technology, and in particular for research programs in biomedical research and clinical medicine. Accordingly, the ad hoc group took the initiative of preparing a statement and resolution on the subject. The statement and resolution below represent the views of the individual signers and not necessarily those of the National Science Foundation or the institutions of which the signers are a part.

The statement and resolution follow:

A Resolution
July 13, 1982

For well over a decade, significant moral and value problems have been raised by advances in science and engineering, and in biomedical research and its clinical applications. As the power and depth of scientific knowledge have increased, so too have the ethical dilemmas. The response of the federal government to them has been important and timely. The National Science Foundation, responsible for basic research in science and engineering, has supported research on the ethical and value issues in those fields since 1975, through its EVIST program. Beginning in 1978, as a result of congressional mandate, NSF has also supported research on ethical and value issues in biomedical research and technology. For its part, the National Endowment for the Humanities, in cooperation with the NSF-EVIST program, has also provided support for research on ethical and moral values of science and technology, including the biomedical sciences.

Despite those programs, there is increasing evidence that the federal response is lagging behind both the needs and the opportunities. This is especially true for the biomedical sciences, where the need for careful examination of the moral problems of clinical medicine has outstripped available resources for research support; and where genuine opportunities for greater public understanding and wiser participation in medical decisionmaking are being neglected for lack of adequate resources. This is all the more true during a period of budgetary austerity, which adds to the inherent difficulty of many decisions the added burden of cost-constraint.

At present, the only available regular federal source of funds for research on ethical questions are the NSF and NEH programs. Yet it has been noted frequently that the major biomedical research and health care delivery agencies do not support research on the ethical implications of the very research and delivery they support. That task has been left to agencies--NSF and NEH--already addressing a wide range of other ethical and value problems of science and technology. They cannot do adequate justice to the latter range of issues while also trying to be responsible to the widespread and growing needs of the biomedical community. It would be both more reasonable and more efficient if research on the moral problems of biomedicine were directly, or cooperatively, shared by federal health agencies.

Therefore, this ad hoc advisory committee after due deliberation has passed the following resolution:

that support for research on the ethical problems of biomedical research, clinical medicine, and health care delivery should be accepted as a major responsibility by such agencies as the National Institutes of Health, the Alcohol, Drug Abuse and Mental Health Administration, and other health agencies; and that this be accomplished by means of separate study sections, by the awarding of separate research grants, and by sharing with NSF and NEH appropriate joint research support.

Bernard Barber
Professor of Sociology
Barnard College
New York, NY

Taft Broome
Professor of Civil Engineering
Howard University
Washington, DC

Daniel Callahan
Director
The Hastings Center
Hastings-on-Hudson, NY

Rosemary Chalk
Staff Officer
Committee on Scientific Freedom
and Responsibility
American Association for the
Advancement of Science
Washington, DC

Albert Fritsch
Executive Director
Appalachia-Science in the
Public Interest
Lexington, KY

Clifford Grobstein
Professor of Biological Science
and Public Policy
University of California, San Diego
La Jolla, CA

Gerald Holton
Professor of Physics and the
History of Science
Harvard University
Cambridge, MA

Eileen Serene
NSF Advisory Council
Yale Law School
New Haven, CT

ANNOUNCEMENTS

Call for Papers

The AMERICAN SOCIETY FOR ENVIRONMENTAL HISTORY will hold its 1983 meeting at the Marcum Conference Center at Miami University, Oxford, Ohio on April 8-10 in conjunction with the Great Lakes Chapter of the American Studies Association. The program committee seeks papers dealing with a broad range of research methods, perspectives and subject matter in environmental history including but not limited to geography, quantitative methods, literary analysis, the use of archeological, biological, or other scientific information, and American Studies. Within this general framework the committee wishes to emphasize environmental change in the Midwest, international and cross-cultural studies, and environmental change outside the United States. The program will, we hope, provide a good cross-section of the many approaches and interests which have contributed to the development of environmental history. For further information, contact: Professor Thomas R. Dunlap, Department of History, Virginia Polytechnic Institute and State University, Blacksburg, VA 24061.

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The CANADIAN SOCIOLOGY AND ANTHROPOLOGY ASSOCIATION will be holding their annual meeting June 1-4, 1983 in Vancouver, British Columbia. At that time sessions in the Sociology of Science, held jointly with the Canadian Society for History and Philosophy of Science, will be held. Special effort is being made to draw international contributions that will heighten the profile of the sociology of science in the context of Canadian sociology, and that reflects both the developments and the diversity that have characterized the field recently.

The CSAA provides travel subsidies for students and certain others. Thus, individuals who find transportation costs prohibitive may be able to find at least partial relief. In order to qualify, however, individuals must be a member of the CSAA. Applications can be obtained from the Secretary-Treasurer, Canadian Sociology and Anthropology Association, Concordia University, 1455 de Maisonneuve ouest, Montreal, Quebec, H3F 1M8. For further information, contact: Gary Bowden, Department of Sociology, University of Calgary, 2500 University Drive, Calgary, Alberta, Canada T2N-1N4.

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You are cordially invited to submit papers for the 1983 Annual Meeting of the AMERICAN SOCIOLOGICAL ASSOCIATION. The deadline for submitting papers is December 31, 1982. Copies of abstracts and final papers are due May 1, 1983. Please write to: J. Scott Long, Department of Sociology, Washington State University, Pullman, WA 99164-4020. Telephone: (509)335-3249; (509)335-4595.

* * * * *

The 15th Annual Meeting of CHEIRON will be held from 15-18 June 1983 at Glendon College, York University, Lawrence Avenue, Toronto, Ontario, M4N 3M6, Canada. Papers are solicited which deal with topics in the history of any of the behavioral or social sciences or with related historical and social sciences methodology. Deadline for receipt of submitted papers is 15 February 1983. Length of papers should be approximately seven double-spaced, typewritten pages. Three copies should be submitted. Papers will be subject to a process of blind refereeing. Submitted papers and queries relating to the program should be directed to: Dr. H.B.M. Hurwitz, Department of Psychology, University of Guelph, Guelph, Ontario N1F 2W1 Canada. Queries relating to local arrangements should be directed to: Dr. Kurt Danziger, Department of Psychology, York University, Downsview, Ontario M3J 1P3 Canada. Queries relating to membership in CHEIRON should be directed to: Dr. John Popplesstone, Executive Officer, CHEIRON, Archives of the History of Psychology, University of Akron, Akron, Ohio 44325 U.S.A.

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Call for News

CALL FOR SCIENCE AND TECHNOLOGY NEWS. If you have any newsworthy, "airworthy" science and technology pieces, to a 1-3 minute format for a new public educational TV program, "Science Watch," please contact: Dr. Eugene Griessman, Georgia Tech News Bureau, Faculty/Alumni House, Atlanta, GA 30332. Telephone: (404)894-3195.

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PROJECT ON THE HISTORY OF LASERS. Four professional societies - The American Physical Society, The Laser Institute of America, The Optical Society of America, and the Quantum Electronics and Applications Society - have joined with the American Institute of Physics' Center for the History of Physics and the Institute of Electrical and Electronics Engineers' Center for the History of Electrical Engineering, to initiate a project on the history of lasers.

The project's central activities will be the taking of oral histories, and the locating of papers, photographs, tapes, and equipment of historical significance.

We welcome news of scholarly studies, completed or in progress, on the history, sociology, or philosophy of laser science and engineering and the laser and electro-optical industry. We also solicit information on oral histories already conducted and on archival or personal collections of papers and equipment.

Send all information to: Joan Lisa Bromberg, Director, Laser History Project, 25 Stoddard Street, Woburn, MA 01801. Telephone: (617)938-8389.

* * * * *

The NSF-NEH announces that applications for INTERDISCIPLINARY AWARDS are due at NSF on February 1, 1983. The awards enable individuals trained in either the sciences or the humanities to enhance their ability to address contemporary ethical issues in science and technology. Applicants must have five years professional experience and a record of accomplishment in their special field. Applications must identify host specialists with whom applicants will work over the course of the award. Recipients receive a stipend of up to \$20,000 yearly for a maximum period of two years, and a modest allowance for travel and activities support. Detailed application forms may be obtained from the EVIST program, NSF, Washington, DC. 20550, or from the HST program, NEH, Washington, DC 20506.

* * * * *

MELLON FOUNDATION GRANT TO SUPPORT ARCHIVAL APPRAISAL STUDY AT M.I.T. The Massachusetts Institute of Technology has received a \$100,000 grant for the Andrew W. Mellon Foundation of New York City for a pioneering project to develop appraisal guidelines for archival records in science and technology. (For archivists, appraisal is the process of identifying records for long-term preservation.)

The scientific growth that began with World War II brought on a corresponding growth in the creation of records that has continued ever since. Little has been done to see if there are procedures that could be adopted to manage these records and assure the preservation of historically important material.

Under the Mellon grant, the Institute Archives and Special Collections of the M.I.T. Libraries, which has expertise in gathering and preserving archives in science and technology, will draw up appraisal guidelines and test their use in several research settings. The principal investigator, Helen Slotkin, Institute archivist and head of Special Collections, is presently developing a theoretical background for appraisal under a smaller, parallel grant from the National Science Foundation.

The project will pool the interests of records creators, records managers, archivists and historians. A unique feature of the M.I.T. project is that it will not be confined to M.I.T., but will draw upon archival and technical advice from other universities, science and technology based corporations and two state archives. This will enable the investigators to test the general applicability of the guidelines in various settings and gather advice from different perspectives.

The project will examine records not only of permanent historical value but also the working records--grants and contracts, periodic reports, accounting statements, for example--of ongoing research efforts. An archivist, who specializes in the permanent records, and a records manager, who maintains the large mass of disposable records, will work together.

"Record management and archival programs work most successfully when they are linked together," Ms. Slotkin said, "because the skills and goals of the records manager and the archivist are integrally related.

"We hope to demonstrate that by intervening as early as possible in the life cycle of records it will be possible to maintain an orderly flow of information about ongoing research and at the same time identify those documents--usually not more than five to 10 percent of the total--that are of enduring historical value," Ms. Slotkin said.

The procedures developed will be tested and adjusted on research projects at M.I.T. Later they will be tried at some of the participating organizations to see if they have general applicability.

Other universities participating include Case Western Reserve University, the University of Illinois at Urbana-Champaign, the University of Wisconsin-Madison and Yale University. Bell Telephone Laboratories, Inc., Digital Equipment Corporation and THE MITRE Corporation are among the cooperating companies. The Ohio Historical Society and the State Historical Society of Wisconsin are also participating.

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Positions Available

MICHIGAN TECHNOLOGICAL UNIVERSITY. The Department of Social Sciences, one of five academic departments in MTU's College of Sciences and Arts, is seeking a Head to begin Fall, 1983. The Head will direct a faculty of nineteen representing anthropology, archeology, geography, history, political science, psychology, and sociology. The department, which has a vital role in the education of MTU's 7,500 undergraduates, most of whom major in engineering or the physical sciences, also houses an interdisciplinary program in Science, Technology and Society (STS). Candidates should have a broad understanding of the social sciences and a strong record of research, publication and professional involvement in one of the disciplines. Particular consideration will be given to those candidates with an interest in STS, strong managerial skills, and a demonstrated capacity for leadership.

Send resume, brief description of research interests, and names of three references by January 1, 1983. Write: Dr. Larry D. Lankton, Chair, Search Committee, Office of the Dean of Sciences and Arts, Michigan Technological University, Houghton, Michigan 49931.

* * * * *

ISIS: EDITORIAL ASSISTANTSHIP FOR 1983-84.

Duties -- The Editorial Assistant takes responsibility for the day by day administration of the Book Review section of ISIS. The Editorial Assistant also helps the Editor and his associates keep abreast of developments in the history of science and participates in every aspect of the work of the journal.

Qualifications -- The assistant must be an accepted candidate in the Ph.D. program in the History and Sociology of Science at the University of Pennsylvania. Previous experience in the history of science or in editorial work is preferred though not required.

Stipend -- Currently \$6,400 plus tuition. The assistantship is a one year, half-time position that begins on 1 July 1983.

Future Role -- It is hoped that in the second year the Editorial Assistant will succeed to the position of Editorial Coordinator (also half-time), for which the stipend is currently \$6,800. The Editorial Coordinator plays a major role in the production of Isis (proofreading, layout, maintaining time-tables) and in promotion, distribution and back issue sales.

Applications -- Candidates should first submit a letter outlining their academic experience and indicating the relevance of ISIS and the assistantship to their graduate work and future plans. Candidates will be sent the usual University of Pennsylvania application forms by which to seek admission to the Department of History and Sociology of Science. The candidate's initial letter (which should be addressed to the Editor) weighs heavily in the selection process. Applicants from all countries are welcome. The closing date is 1 February 1983.

* * * * *

VPI and SU Science and Technology Studies--The Virginia Tech Center for the Study of Science in Society seeks to appoint two or more scholars who will engage in research, teaching, and program development in science and technology studies. The Center, a department-level unit in the College of Arts and Sciences, supports interdisciplinary research and graduate degree programs focusing on the interactions between science and technology and their social and cultural contexts. Studies of the role played by cognitive, social and political factors in the development of science and technology are emphasized. Preferred fields of specialization: sociology, philosophy, political science. Visiting appointments, with possibility of renewal, or continuing appointments. Rank and salary commensurate with experience. Application deadline, 7 January 1983. Send applications to Arthur Donovan, Director, Center for the Study of Science in Society, Virginia Polytechnic Institute and State University, Blacksburg, VA 24061.

* * * * *

VPI and SU Technology Studies--The Virginia Tech Center for the Study of Science in Society seeks to appoint a scholar interested in developing an interdisciplinary program of research and teaching in technology studies. The Center, a recently founded department-level unit in the College of Arts and Science, supports research and graduate degree programs in science and technology studies. It focuses particularly, but not exclusively, on studies of the role played by cognitive, social and political factors in the development of science and technology. Primary consideration in making this appointment will be given to the following criteria: (1) ability to initiate and carry out a coherent and scholarly research program in technology studies, (2) ability to work with members of the College of Engineering in developing forums, including regularly scheduled classes, in which the insights of the humanities and social sciences are made relevant to the concerns of professional engineers, (3) potential for contributing to the development of interdisciplinary science and technology studies in the Center. Rank and salary commensurate with experience. Application deadline, 7 January 1983. Send applications to Arthur Donovan, Director, Center for the Study of Science in Society, Virginia Polytechnic Institute and State University, Blacksburg, VA 24061.

* * * * *

RENSSELAER POLYTECHNIC. Tenure track assistant professor position available for outstanding Ph.D. in any social science discipline. First preference is an economist with a humanistic orientation and experience in policy research. Others with exceptional qualifications in economics will be considered. Candidates must show outstanding promise for funded research, and must be willing and able to work effectively with others on interdisciplinary projects. This position is funded under a grant from the Andrew W. Mellon Foundation for strengthening tenure track academic positions for humanistic scholars in applied interdisciplinary research. It offers a stimulating research environment, a minimum teaching load, ample travel funds, superb computer facilities, and fully competitive starting salary and fringe benefits. To apply, send a curriculum vita that includes the names of four references to: Professor Abbe Mowshowitz, Research Director, Science and Technology Studies Division, Rensselaer Polytechnic Institute, Troy, NY 12181. Deadline for receipt of applications is January 15, 1983.

News from AAAS

(1) Publication announcements:

- Impact and Challenge, proceedings of the seventh annual AAAS Colloquium on R&D Policy, Washington, 23-24 June 1982, edited by Albert H. Teich and Jill P. Weinberg, September 1982. (Available from AAAS Sales Office, 1515 Massachusetts Ave., N.W., Washington, D.C. 20005, \$8.00 prepaid.)
- Congressional Action on R&D in the FY1983, by Willis H. Shapley, Albert H. Teich and Jill P. Weinberg, forthcoming late 1982. Contact Office of Public Sector Programs, AAAS, 1776 Massachusetts Ave., N.W., Washington, D.C. 20036 for further information.
- 1982-83 Directory of the AAAS Consortium of Affiliates for International Programs. Prepared by the Office of International Science, AAAS, 1776 Massachusetts Ave., N.W., Washington, D.C. 20036.
- Education in the Sciences: A Developing Crisis. Available from the Office of Science and Technology Education, AAAS, 1776 Massachusetts Ave., N.W., Washington, D.C. 20036.

(2) Application deadline is February 1, 1983 for the following programs:

- 1983-1984 Congressional Science and Engineering Fellowships, for postdoctoral to midcareer scientists, engineers and other appropriate professionals. Fellows spend one year working as special legislative assistants on staffs of members of Congress or Congressional committees.
- 1983-84 Science, Engineering, and Diplomacy Fellowships. Fellows spend one year working as staff officers in appropriate bureaus of the Department of State or the Agency for International Development.
- 1983 Mass Media Science and Engineering Fellowships, for advanced students in natural and social sciences and engineering to work for ten weeks during the summer as reporters, researchers, and production assistants at mass media sites (newspapers, magazines, radio and television stations). Priority will be given to graduate students, but applications will also be considered from outstanding undergraduates and postdoctoral students.

Information and application materials for all of these programs are available from the Office of Public Sector Programs, AAAS, 1776 Massachusetts Ave., N.W., Washington, D.C. 20036. Minorities and handicapped persons are especially encouraged to apply.

(3) Meeting reminders:

- AAAS Colloquium on R&D Policy, Washington, D.C., 24-25 March 1983.
- AAAS Annual Meeting, Detroit, Michigan, 26-31 May 1983.

Information from Meetings Office, AAAS, 1101 Vermont Ave., N.W., Washington, D.C. 20005.

NEH National Endowment for the Humanities

WASHINGTON, D. C. 20506

SUPPORT FOR THE STUDY OF HUMANITIES, SCIENCE, AND TECHNOLOGY

The National Endowment for the Humanities has a longstanding interest in research which brings humanities scholarship to bear on science and technology. This interest is supported by the Humanities, Science, and Technology program within the NEH Division of Research Programs.

In 1983, the Endowment plans to offer limited support through this program for two specific areas of science studies:

1. The NEH Humanities, Science and Technology program will continue to collaborate with the Ethics and Values in Science and Technology program of the National Science Foundation to support interdisciplinary studies of science-related ethical issues and the value presuppositions in science and technology they may involve. This joint activity has categories for the support of individual projects as well as larger scale collaborative projects:

- Deadlines: Individual Awards: February 1, August 1
Collaborative Projects: February 1, August 1
- Procedures: Further information and application guidelines may be obtained from either:

Humanities, Science, and Technology Program
National Endowment for the Humanities
806 15th Street, N.W.
Washington, D.C. 20506
(202)724-0276

or

Ethics and Values in Science and Technology
National Science Foundation
1800 G Street, N.W.
Washington, D.C. 20550
(202)357-7552

2. The NEH Humanities, Science, and Technology program will support a limited number of projects which undertake basic research in the methodological and conceptual foundations of humanities approaches to the study of science and technology. It is expected that most such projects will be collaborative efforts of two or more scholars. Individual applicants should inquire as to the eligibility of their project in the NEH Division of Fellowships and Seminars.

- Deadline: February 1, annually

- Procedures: Guidelines for this category are currently being developed. To receive further information when it becomes available, contact:

Humanities, Science, and Technology Program
National Endowment for the Humanities
Washington, D.C. 20506
(202)724-0276

In addition to the categories of support described above, humanities projects relating to science and technology may be eligible for support in a number of other NEH programs. The programs listed below may be contacted directly. If you have questions about which program might be most appropriate to your interests, contact the HST program.

- Research Conferences, regional studies and other collaborative research efforts (General Research Program: 202-724-0276);
- Scholarly editions, translations, reference works and publications (Research Materials Programs: 202-724-1672);
- Individual fellowships, summer stipends and summer seminars (Division of Fellowships and Seminars: 202-724-0333);
- Curriculum development and implementation (Division of Education Programs: 202-724-0351);
- Projects in museums, libraries, or media (Division of General Programs: 202-724-0231)

Mail to:

National Endowment for the Humanities
Mail Stop 350-G
Washington, D.C. 20506

Please send further information and application guidelines for the NEH Humanities, Science, and Technology Program to me at:

Name _____

Address _____

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The assessment and implementation of health care priorities in developing countries
The family as a resource unit in health care: changing patterns
The nature and influence of popular movements in health policy and modern health practice
The relevance of traditional medical cultures to modern primary health care
Values in health care choices
Working conditions and health—social epidemiology, patterns of stress, coping and change
The Role of Spatial Analysis & Geographic Data in the Detection
of Disease Causation.

For further details and application forms write to:

Dr P. J. M. McEwan
Glengarden
Ballater
Aberdeenshire AB3 5UB
Scotland

Errata--We regret the errors in Volume 7, Number 3
in which Donald A. MacKenzie's book was shown
with the incorrect subtitle, both on the cover
contents page and on the page where the review
began. The correct title of his book is
STATISTICS IN BRITAIN, 1865-1930: THE SOCIAL
CONSTRUCTION OF SCIENTIFIC KNOWLEDGE.

DEADLINES FOR NEWSLETTER MATERIALS

Vol. 8:1--Spring 1 March 1983

Vol. 8:2--Summer 1 June 1983

Vol: 8:3--Fall 1 Sept. 1983

Vol: 8:4--Winter 1 Dec. 1983

4S *Society for Social
Studies of Science*

The 4S Newsletter is published four times each year, beginning with V.6:3 at the Department of Sociology, Texas A&M University, College Station, Texas 77843 and sent to all members of the Society for Social Studies of Science. Membership is on a calendar year basis. Membership dues (\$15 for professionals, \$5 for students) and institutional subscriptions (\$25) should be sent to: The Secretary/Treasurer, 4S, Department of Sociology, Indiana University, Bloomington, Indiana 47405.



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