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Contents

Memorial

- Derek Price--1922-1983
--Belver Griffith 2

Fact and Opinion

- In Search of Identity--Terry Shinn 5
News from "Down Under"--David P. Miller 8
Report on Conference at Griffith University--Jean Moran 8
Report on Symposium at University of Newcastle--David Oldroyd 10

Book Reviews

- Mott T. Greene's *Geology in the Nineteenth Century:
Changing Views of a Changing World*
--Reviewed by Kenneth L. Taylor 11
- Joan Lisa Bromberg's *Fusion: Science, Politics and the
Invention of a New Energy Source*
--Reviewed by Arthur L. Donovan 14
- Edward M. MacKinnon's *Scientific Explanation and Atomic
Physics*
--Reviewed by Robert Rynasiewicz 17
- Anthony Pagden's *The Fall of Natural Man: The American
Indian and the Origins of Comparative Ethnology*
--Reviewed by Paul A. Erickson 21

4S Presents Bernal Award

- Introduction of Thomas S. Kuhn--John Ziman 24
Reflections on Receiving the Award--Thomas S. Kuhn 26

Society News

- Committee Memberships and Assignments 31
4S Ninth Annual Meeting--University of Ghent 37

Eighth Annual Meeting

- Abstracts of Presentations 39

MEMORIAL

DEREK PRICE (1922-1983) AND THE SOCIAL STUDIES OF SCIENCE

Derek Price was a major contributor to our view of science as a social institution and of its role in society. He was extremely self-conscious regarding his approach to science as a subject matter. His work affirmed that "knowledge" is a special "good" and that its successful pursuit requires and takes on great independence. In historical analysis, he recognized and helped clarify that much science was done in the name of and to advance the purposes of powerful states. His work spoke constantly to the seeming inevitability of scientific discoveries following one upon another. He stood in awe of this "internal" power of science, yet worked successfully with the "external" settings of science and with science, in aggregate, as a social activity that is determined by powerful statistical laws, most of which were either developed or refined by himself. He was well aware of these political, broad contextual issues and of the fact that his own position on one or another of them offended nearly every source of power. He won honors for his work by simply being better than anyone else.

Derek Price's achievements in analysing the social dimensions of science were extremely rich and varied. In a nutshell, he created a coherent statistical model for science, constantly refined and extended it, considered it within various historical contexts, and used it to address policy questions. This program of scholarship was incredible to the rest of us in terms of requiring simultaneous concern with the historical, social and analytic features of each research problem.

He regarded Little Science, Big Science (1963) as the best statement of his modelling of science. Page for page, his Science magazine article "Networks of scientific papers" (1965) seemed more convincing. As I wrote elsewhere:

..(This paper) was the "first meaningful meshing of an explicit model for science model for science with raw data, and the results were surprisingly elegant and clever.

Modestly enough, the paper set out to describe the 'world network of scientific papers'. While the paper may not have quite managed that goal, it clearly displayed the broad pattern that scientists follow in referring to previously published documents, and by inference, something about the way new science builds on recorded knowledge...(Apart from the paper's brilliance)...the idea of science building 'at the skin or surface', new science floating on about the past thirty or forty previous papers, the laying down of an archive, and the identification of extreme degrees of elitism in science, were important.... (However, the most) important feature was the implied research program, resting mainly on the possibility of quantifying and comparing measures of elitism and a measure of a 'research front'.

Little Science, Big Science and this paper drew many younger scholars, including myself, into the statistical analysis of science; these documents remain, after twenty years, immensely influential.

Even beyond his publications, Derek was, in himself, the best and most efficient conveyance of his own model. He took delight in predicting, exactly, the outcome of other people's research. He did so conversationally; his virtuosity

drew upon his deep knowledge of the interrelationships of scientists, scientific papers, authorships and institutions and their distributions and changes over time. Perhaps needless to say, such predictions, and similarly performed detections of error in data, were taken better by doctoral students than by bureaucrats. Now, regretfully, I recall his discussing many statistical regularities which, though observed and used by him, never fully appeared in print.

In the period of his converting from physicist to historian, Derek Price interacted with J.D. Bernal and Cyril Parkinson. Partly as a result, he was, for roughly a decade, not only a public physicist, and a public historian, but also, a private social scientist. From the first years of this interval, he weighed and reconsidered non-historical, social science approaches. (And he published a few totally invisible papers.) His emergence from this closet was, of course, spectacular: Science Since Babylon, Little Science, Big Science and the "Networks" paper. Dropping physics, he continued all other lines of scholarship.

Gradually, we, the non-historians understood his own view that most of his work, however diverse, was of a piece. In contrast, his interest in and fluency with scientific gadgets seemed more a genetic happenstance. That is, if there is to be this person, why not throw in this ingredient, too? His last protracted effort in the social studies of science brought together history, social analysis and gadgetry.

He proposed a shift in the direction of the study of science to the view that instrumentation was of far greater importance, than suspected, in scientific achievement. His initial, informal pieces went to friends in 1982; his major AAAS Sarton address of May of this year was to this point. Of course, the jury is out on the issue, but already there is a debate in Scientometrics, developing about a paper, rebutting Derek, by Michael Moravezik. I hope Derek responded to the request for comments---now, I realize that Derek had started a valuable, needed, and continuing discussion toward clarifying the nature of causality in scientific achievement.

I would have liked to have had Derek remain in that debate; however, the far greater loss is the impossibility of his reviewing, with reflection and maturity, his contributions toward a statistical model of science and of his merging those with the consequent, and numerous contributions of others. To me, the "pieces of the puzzle," were nearly there. There is the further loss that some "pieces" he embodied are, how, lost from scholarship.

Overall, Derek Price's methodological elegance made all aspects of studying science much more difficult. Conceptually, he also posed difficulties because his success was achieved outside of conventional conceptual systems. Many sociologists, like Newton's contemporaries, seem to state that he has not told them what they wanted to know about science.

His contributions to science policy studies are seminal. I am particularly excited about his impact on science analysis in less developed countries. His method for obtaining the dimensions of science are especially important to such countries, and his work, in this area, may become of immediate importance in policy. Less developed countries attack the problems of maximizing their country's technological development and of gaining, especially in the case of deeply cultured countries, such as India and Brazil, recognition of their own national science's

achievements. His work contributed to a better understanding of both problems, and I hope in line with his informal suggestion, that these countries will independently devise their own citation data source, and "science indicators" research. I recognize that his suggestion has, itself, immense policy implications, entirely benevolent. I recognize, as he did, that the data from this research would be highly predictable.

There are many more detailed examples which I would like to interpose, here, but cannot because of time. In conclusion, Derek Price's understanding of the scientific literature has made major contributions to the management and evaluation of the world's major bibliographic resources and services and to our understanding of the role of recorded knowledge in science. He, with Kuhn and Merton, has "revolutionized", in a sense as severe as Kuhn's, the social studies of science; he was the principal architect of a new program for science policy research.

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[Note: This memorial was delivered 11 September 1983 at
Yale University--Ed.]

FACT AND OPINION

IN SEARCH OF IDENTITY:

THE SECOND ANNUAL FRENCH S.T.S. CONGRESS

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What are the intellectual paradigms of STS today, and what is the movement's social status with respect to the broader context of science-focused studies, namely the History of Science and Technology, Sociology of Knowledge and Philosophy? Ideally, what ought STS to be? And finally, given current constraints both cognitive and social, what is the likely future of STS in France? This series of questions provided the theme of the second annual French STS Congress held outside Paris in May 1983. Unfortunately, yet characteristically, the responses proved less replete than the questions.

The CNRS "Centre Nationale de la Recherche Scientifique" promulgated the present Programme STS "Administrative Director Dominique Wolton" in the Autumn of 1980, and endowed with relatively handsome resources, it was assigned the task to reinforce extant research in the domain and attract supplementary researches through the allocation of grants. Beyond this, the Programme was mandated to encourage collective and interdisciplinary projects and to develop an extensive STS network.

The youth of the present programme is, however somewhat misleading. The emergence of research units specializing in the STS perspective, and official support for them, dates back to the 1970's. Indeed, in 1969 the CNRS National Directory financed a study of Girard Lemaire and Pierre Bernard Lecuyer on the relationship between research strategies and laboratory organisation in the exact sciences. Again under the tutelage of the CNRS, between 1972 and 1976 sizeable awards were distributed to stimulate work in the History and Sociology of Science, and from 1976 to 1979 the analysis of research policy was financed by the Délégation Générale à la Recherche Scientifique et Technique. Yet the STS programme differs from its predecessors in at least two important respects; first, in contrast to previous initiatives, STS has extended the pale of participation beyond historians and sociologists, by soliciting the active involvement of engineers and researchers in the physical and biological sciences, political scientists, economists, linguists and perhaps most tellingly philosophers. Second, STS administrators have sought to engender a sense of communal identity as well as an appreciation for the movement's collective interests, both financial and cognitive.

The lectures and debates that analysed the programme's operation and orientation were perhaps more than customarily useful, as they revealed an internal shift within the community on the one hand, and on the other, pointed to what Professor Jacques Lautman aptly labelled a "structural STS paradox." While the first annual Congress had been overwhelmingly dominated by sociologists and by sociologically inspired reflection, the second assembly called for the revival of the philosophy of science, and demanded that henceforth STS research embrace epistemological considerations. The intent was visibly not to diminish or to marginalize the sociological and historical endeavours. The new orientation was instead submitted as an acknowledgement that advances in understanding now required the introduction of additional analytic parameters, and especially

parameters capable of better grasping the admittedly illusive, yet fundamental intellectual dimension of scientific and technological work.

In a particularly penetrating paper dealing with the cognitive and social status of STS, Lautman insisted that the Programme, present and future, could neither be regarded as a sub-discipline nor, to employ the terminology of Bourdieu, as a "champs." Fruitful STS research, declared Lautman, transgresses disciplinary and sub-disciplinary boundaries. It could, perhaps, best be described as corresponding to a "champs." The exigencies associated with professional stability and recognition, and the procural of resources however, necessitates the formulation of an administratively recognised discipline. All STS programmes are thereby impaled on the horns of a dilemma. Lautman was nevertheless not pessimistic. He claims that the essence of STS is a concern and a sensitivity for a certain category of problem, and if long-term commitment to STS may be professionally hazardous, it is likely to prove cognitively enriching - Above all, if a marriage can be achieved between epistemology and sociology.

The Congress closed with aural summaries of two research projects initiated in 1981. Michel Callon described the method that he has developed over the past 5 years which will enable scholars to trace the migration of word-sets across disciplinary barriers. During the last two decades the CNRS Documentary Centre for Science and Technology has isolated the key thematic terms of a vast corpus of literature, numbering several tens of thousands of scientific and technological articles. Callon identifies the key words in a specific discipline at a particular point in time T, compares that language with the dominant vocabulary at T', and then through the analysis of word clusters occurring at T+, he determines the cognitive source of the evolution of the target discipline. The technique is now fully operational and Callon has tested it in the areas of molecular chemistry and food technology. Audience discussion revolved around two questions:

1. In what way is this method significantly different from the Science Citation Index?
2. Given the tenets of semantic under-determination, which Callon himself espouses, how is it possible that vocabulary-based analysis alone can effectively reveal the genesis and thrust of cognitive change?

Terry Shinn presented his work on a pluri-dimensional recursive model of the knowledge production process in the physical sciences. Shinn stated that his observation of a laboratory conducting research in experimental macroscopic physics suggests that the process contains three principle components:

1. Scientists appear to employ either singly or in combination, a limited quantity of modes of reasoning. These modes may prove to be finite in number and invariant in both time and space. Growing evidence indicates that this could be a consequence of the fixed neurological architecture of the human brain. For convenience's sake, Shinn regards these reasoning modes as epistemological analogues of Chomsky's deep linguistic structures.
2. Knowledge-claims that can reasonably be advanced with respect to a given phenomenon are circumvented by antecedent understandings of that phenomenon and by the history of the instruments used to explore it. If one accepts a realist interpretation, although the phenomenon remains invariant, the understanding of it is in a continual state of disequilibrium, each phase of which possesses a history. Scientists either comprehend this, or behave as if they do.

3. Social considerations, both exogenous and endogenous to the laboratory and to the scientific profession, affect the knowledge production process since they a) influence the selection of the reasoning modes employed in problem solving and b) prejudice the historically based analytic frame. The role played by the social component is nevertheless severely restricted by limitations in epistemological avenues, by the fact that historical artefacts of cognitive states are not to be obfuscated, and perhaps most importantly by the reflexive self-regulatory properties of social agency itself. Shinn readily admitted that his model contained inadequacies, but insisted that it warranted additional development as it is more coherent and complete than the alternatives offered by the relativists and ethnomethodological school, and by scholars working in the area of artificial intelligence. He expressed the hope that other French scientists would join him in pursuing the research programme recently formulated by L. Lautman and T.S. Nickles.

Finally, what of the immediate future of STS? The CNRS has opened a new programme in the History of Technology, and another is rumoured for the Philosophy of Science. These are negative areas for the research agency seems bent on the fragmentation of manpower and resources. Be this as it may, the number of scientists involved in STS, and their enthusiasm, are such that the programme has likely achieved a "critical mass," and it is about certain to continue and to develop regardless of financial fluctuations and vacillations in science policy. In this light it will be interesting to see what the third National STS Congress brings, both cognitively and institutionally.

NEWS FROM "DOWN UNDER"

David P. Miller
University of New South Wales

"Social Studies of Science" is a flourishing area in Australia, with a firm base in 'History and Philosophy of Science' departments but also with a wide range of institutional niches and orientations as in other parts of the world. Surveys of the field in Australia have been published by Farrall, Home and Ronayne.* [*See R. W. Home, "History of Science in Australia," Isis 73 (1982):337-342; J. Ronayne, "Scientific Research, Science Policy, and Social Studies of Science and Technology in Australia," Social Studies of Science 8 (1978): 361-384; L. Farrall, "Social Studies of Science in Australia," 4S Newsletter 3 (1) (1978): 21-23.]

The major conference for the social studies of science are provided by A.N.Z.A.A.S. (the Australian and New Zealand Association for the Advancement of Science) and A.A.H.P.S.S.S. (the Australasian Association for History, Philosophy and Social Studies of Science), but numerous other gatherings enliven our calendar. Two recent examples are reported on below. Full participation in the activities of 4S is obviously difficult for Australian members, but the exploits of our yachtsmen need not prevent a continuing and expanded exchange of news and ideas through the pages of 4S Review.

REPORT ON "POLITICAL DIMENSIONS OF TECHNOLOGICAL CHANGE" CONFERENCE, 15-16 OCTOBER, 1983 GRIFFITH UNIVERSITY, QUEENSLAND, AUSTRALIA

by
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Griffith University, Queensland, Australia

This conference was unusual in that it aimed to provide a forum for those directly affected by technological change rather than merely a platform for a succession of experts. Organised from the Science Policy Research Centre at Griffith University, the conference attracted some 120 delegates from unions, industry, government organisations, white-collar professions and small businesses.

Brusque young federal Minister for Science and Technology, Barry Jones, set the tone in his opening address by converging on the problem of Australia's technological dependence, foreign ownership and control, and an ailing manufacturing sector. Claiming that attitudes to work needed drastic overhaul, Jones proceeded to outline a series of future scenarios (ranging from the prospect of a blackmarket economy operating outside the formal economy to the mushrooming of electronic "offices" to the conserver society set of options.) He was critical of many such projections of future society in that they tended to underplay the class nature of unemployment. However the key challenge Australia faced in the immediate future was to develop an indigenous high-technology industry both for wealth generation and job

creation purposes. One of Australia's greatest handicaps, he argued, was its great mineral resource base which had defined its role as an exporter of raw materials* [*Australia enjoys the dubious distinction of ranking 23rd of all OECD countries (second only to Greece) in the value of its imports of technology over the value of its exports.] and which threatened in the future to transform the "Lucky Country" into a gigantic quarry. The way out, he suggested, was through greatly increased R&D in the private sector (supported by generous tax incentives), an vastly improved skills-based economy, and measures to offset the sluggish entrepreneurship that has conventionally characterized Australian innovation, product-development and marketing.

The role of government intervention, tripartite negotiation and the trade union movement in counteracting unemployment and social dislocation was taken up by succeeding speakers. Dr. Peter Ellyard (director of Australia's only State Ministry of Technology in South Australia) advocated vastly more flexible policies for education/retraining in the workplace as well as "creative planning" to facilitate the development of brain-based industries. After all, he smiled rhetorically, Australia really would prefer to emulate Sweden rather than Zaire, wouldn't she?

Not surprisingly the trade union speakers were more concerned with issues of equity, unemployment and a social wage. Laurie Carmichael, national secretary of the Amalgamated Metals, Foundry and Shipwrights' Union, led a high-flown attack on monetarism and the regressive characteristics of a global economy dominated by the operation of MNCs. The Labor Party-Trade Union Accord, which called for substantially increased worker rights over the introduction of technology and in retrenchment agreements and the development of a social wage program, was seen as a step in the right direction. Jack Munday, environmental activist and one-time leader of the NSW Builders' Laborers Federation, questioned the role the trade union movement could be expected to play without radical transformation of its historically defensive, wage-oriented tactics. Commenting on the recent development of "tuxedo" unionism, U.S. style, Munday went on to outline a blueprint for a sustainable society in which a revamped union structure could play a vanguard role.

The contribution of Dr. Jim Cairns, former Deputy Prime Minister and the man who once led 100,000 people into the Vietnam moratoriums of the 1960's, was decidedly eccentric. Concerned with the life-threatening crisis in "our planetary culture" epitomized by "social regression/repression," Cairns described the hegemonic influence of science and technology as a natural outgrowth of patriarchy. He remained supremely pessimistic. Ensuing discussion included a stunning moment when a crystallographer asked what patriarchy meant and what evidence there was of patriarchy in crystallography or any other field of scientific endeavour -- pure or otherwise.

The workshops (the centerpiece of the conference) were organized in three sessions: the nature of technological change considered through the lens of specific industries; its impact on the workplace; and alternative strategies. Recurrent concerns in what was characterized as an "inter-active exchange of biases" were: the need for regulation of foreign ownership; the corroded power base of unions under the weight of rationalization procedures and casualization of labor; access to and cost of information; reduced participation in decision-making processes; deskilling and "cultural

colonization"; privatization of the public sector; increased specialization and division of labor as a means of institutionalizing poverty and cementing the gap between the information-rich and information-poor; the monitoring and surveillance aspects of new technology; the need to devise industry-specific programs for the development of socially useful products.

Like all good conferences, participants emerged from the final plenary session with varying degrees of hangover from information overload, mostly compensated for by the considerable cross-fertilization between the diverse groups represented.

RESEARCH SYMPOSIUM ON CRITICAL OR RADICAL THEORIES OF SCIENCE

Sponsored by the Departments of Philosophy and Sociology
University of Newcastle (New South Wales), 10-12 November 1983

by
David Oldroyd
University of New South Wales, Australia

About 25 philosophers, sociologists, HPS and STS specialists thought the programme organized by the University of Newcastle - and the pleasures offered by the rural "retreat" of the Anglican Diocese of Newcastle at Morpeth in the Hunter Valley, NSW - more attractive than the usual end-of-the-year duties of marking and the like.

Ten papers were presented and all attracted lively, extended and well-informed discussion. However, there still appears to be within the Australian academic community in this field a significant "tyranny of distance" effect, in that the "agenda" for discussion is still set overseas. Thus the theoretical issues that attracted the most attention were those of reflexivity and relativism in relation to the sociology of knowledge. And it appears that we are still talking about possible ethno-methodological studies of science in Australia rather than producing any concrete findings. Australians seem to like the "strong programme" but are unwilling to accept a totally unqualified epistemological relativism. There is still room, down under, for some kind of epistemic input from the "real world"; but this observer was unable to ascertain exactly how much the Symposium participants deemed socially acceptable.

Though as ever the name of K.M. would probably head the list of any head count of names referred to in the deliberations, virtually no revolutionary spirit was discerned among the participants. However, there was in evidence some yearning for a new "holistic" science that would somehow bring man and nature into harmony in a "post-modern" world. How such a goal was to be achieved was not made clear to this observer. The outstanding paper was that of A/Prof. Arie Brand, speaking without wondrous lucidity on Habermas's latest work.

The meeting offered participants a most welcome opportunity to recharge their intellectual batteries, and the University of Newcastle is to be congratulated on its new venture. It is much to be hoped that this year's Symposium will serve as a model for future events of a similar kind in Australia.

BOOK REVIEWS

Mott T. Greene. *Geology in the Nineteenth Century: Changing Views of a Changing World*. Ithaca and London: Cornell University Press, 1982.

It may reasonably be expected that historical works chosen for review in this journal will normally display prominently an orientation toward social interpretation. Such is not the case here; Green's study is on the whole a straightforwardly intellectual history, a discussion of a series of controversies in geological theory. Certainly Greene is sensitive to the power of tradition and circumstance in shaping scientific ideas, and he is far from conceiving of scientists as functioning apart from their cultural contexts, but his priorities are basically "internalist." His eye is primarily on the interplay among ideas, between evidence and explanatory concepts. Accordingly he makes no claim to tell us much about the social dimensions of his subject.

Nonetheless *Geology in the Nineteenth Century* is highly deserving of attention by readers of *4S Review*, for reasons beyond even its merits as conceptual history. For one thing, Greene opens up new perspectives on some of the main outlines of geology's growth in the nineteenth century, perspectives holding fascinating possibilities for further study of pluralistic and divergent developments within a scientific community sometimes depicted as having passed beyond the usual ordeals of inception to a condition of stability and substantial unity. Furthermore, Greene writes with awareness that the story he tells is rich in potential for exploitation by those concerned with the character of geology as a particular form of knowledge and endeavor. While he wisely refrains from intruding more than a few overt metascientific observations into his historical narrative, his account is informed by the conviction that it can serve as a resource for, among others, philosophers and sociologists of science. Indeed, the book contains a clear suggestion that although he writes first for historians and geologists, Greene offers a needed therapeutic for modellers of scientific behavior.

Possibly the single greatest surprise in this book, at least for the reader acquainted with only the more conventional sketches of nineteenth-century geology, is the modest role it assigns to Charles Lyell among the dramatis personae. Lyell figures here as a character distinctly subordinate in importance to a number of other personages, and as a geologist rather outside the mainstream of geological debate and development. To what extent this gauging of Lyell results from the somewhat restricted construction Greene places on his subject, or to what degree it follows justly from a comprehensive assessment of geology's main patterns of change during the period, is a question that historians of science are likely to address with renewed vigor because of Greene's book. He presents the most vivid and forceful challenge I have seen to the widespread idea that Lyell personified and dominated over geology's maturation. As Greene makes plain, important aspects of such a challenge have been advanced before by others; it can hardly be₁ doubted that Greene's bold arguments will provoke some spirited rejoinders.

Greene's first two chapters are an attempt to extricate the period of geology's historical emergence (ca. 1775-1820) from the llegendary framework into which it was placed by a succession of writers--especially Cuvier, Lyell, Geikie, and Zittel--for whom history was, in part, a justificatory tool in service to particular scientific positions. Refreshingly and insightfully, Greene restores Abraham G. Werner to the position of esteem and influence that he held for many geologists in the nineteenth century. Werner is convincingly

portrayed as the representative of a geognostic natural history contrasting with James Hutton's teleological and deductively-inclined system of natural philosophy. Greene succeeds in explaining how Werner's methods of geognostic identification of rocks and their sequential ordering had greater impact on many practical-minded geologists than did his neptunist doctrines. We find here also the astute observation that both Huttonian and Wernerian formulations of geological theory and investigation should be seen as culminations of long-standing traditions, drawing heavily on ideas that were original with neither the Scottish nor the German figurehead. Yet for all Greene's stress on the significance of a number of Hutton's and Werner's predecessors and contemporaries, it is notable, in a study expressly discontent with many explanatory categories bequeathed by tendentious nineteenth-century histories (neptunist, vulcanist, catastrophist, etc.), that the Scottish earth-theorist and the German geognost are largely confirmed in their traditional places as the twin poles from which geological science emerged.

Following these two preliminary chapters, the heart of Greene's book examines a series of geological studies of mountain ranges and theories of crustal deformation. Where previous accounts of nineteenth-century geology have tended to focus on uniformitarianism as the doctrinal core around which geology gained permanent scientific status, Greene sees the science's main lines of development taking up problems of mountain structure and origins, problems about which Lyell had relatively little to say. Global tectonic theories, bred through efforts to comprehend the arrangement and dynamic causes of mountain ranges such as the Alps and Appalachians, took temporally progressionist and dynamically episodic forms. The tableau that Greene unfolds features three great attempts at a global tectonic theory, identified respectively with Leonce Elie de Beaumont, Edward Suess, and Thomas C. Chamberlin. Greene shows how what began as concern over the processes of mountain building early in the century developed gradually into a broader preoccupation with the dynamics of continents. The final chapter, which discusses the advent of continental drift theory (a fourth global tectonics, in Greene's scheme), is thus a lesson in historical continuity. The early-twentieth-century drift concepts of William H. Pickering, Frank B. Taylor, and Alfred Wegener are seen as sensible products of the ideas, disagreements, and innovations of nineteenth-century geotectonics.

Whether a history of nineteenth-century tectonic theories and their outcome in concepts of continental dynamics is sufficiently broad to warrant quite so general a title as this book carries may be debatable. Not to be doubted, however, is the strategic importance, in geology's development, of the themes Greene addresses. In conjunction with the strong case he makes for the key roles played by ideas of directional change and intermittent manifestations of force, Greene indicates a prolonged association of major tectonic theories with (a) one form or another of the earth's secular contraction, (b) acceptance of significant lateral (as distinct from merely vertical) crustal movements, and (c) a sense of the relevance of geometrically-expressed correlations in mountain systems. These were in the main the preoccupations of continental Europeans, and to a considerable extent of Americans as well. While the British are certainly not disregarded in Greene's treatment (the novel ideas of Charles Babbage and John Herschel on crustal deformation, for example, are given prominent coverage), the emphasis is clearly elsewhere. It is this continental European and American orientation, together with the focus on mountain structure and tectonics, that sets Greene's interpretation of nineteenth-century geology apart from most general historical accounts previously available.

Greene says he had not expected to encounter the high level of theoretical discord and diversity he finds in nineteenth-century geology, having selected his subject in anticipation of examining an instance of "normal" scientific elaboration within a basically stable field. This seems to suggest an original metageological and metahistorical intention of which some vestiges remain in what turns out otherwise as a thoroughly historical study, one in which theoretical disagreement is not unreasonably raised to a status approaching an organizing principle for the narrative. Not only is there frequently more historical value in knowing what scientists fought over than what they agreed upon; Greene also judges that an absence of theoretical consensus was often congenial to healthy and vigorous pursuit of the period's major geological problems. He ends with a suggestion that a maturely flourishing science's need for common commitment to comprehensive theories may be overrated.

Greene is surely right that prominent socio-philosophical modellers of the scientific process have tended to neglect sciences like geology, to their models' detriment. Greene's contribution toward the broadening and improvement of such models lies mainly in his skilled depiction of certain turbulent pathways of scientific change which any successful representation of science's functioning must eventually embrace. In addition he supplies some hints of his views on such matters as the relationships geological inquiry has historically maintained with adjunct disciplines. Examples may be found in the chapter titles "The Nappe Theory in the Alps: Tectonics over Physics, 1878-1903," and "The Decline of the Contraction Theory: The Challenge from Geophysics, 1870-1909." For the most part such hints remain undeveloped; the suitability of these episodes as raw material for analysis of geological discourse's structure is indicated, but not pursued. I mention this not in complaint, for a historical agenda is ample in this book. One hopes, as Greene does, to see a widening of horizons in social and philosophical studies of science so as to incorporate the geological enterprise. His book should be a sharp stimulus to that end.

Geology in the Nineteenth Century has some imperfections of the sort that disconcert the exacting scholar. There are some surprising omissions in the references, and one encounters rather more than the usual number of small errors in citations and the rendering of names. But these minor weaknesses are far outweighed by the strengths of this highly original and provocative book. It is a pleasure to read, written in energetic and often witty style. Greene has an enviable gift for clear exposition, which ought to help win his book the wide attention it deserves.

Note

1. The main revisionists cited by Greene in the matter of Lyell's historical role include Martin Rudwick, Alexander Aspövat, Roy Porter, and Gordon Davies. He might also have mentioned Philip Lawrence. Among defenders of Lyell's magisterial role from whom reactions to Greene's account may be anticipated with interest, one of the ablest is Leonard G. Wilson, whose multivolume biography of Lyell is in progress. A recent expression of Wilson's viewpoint is "Geology on the Eve of Charles Lyell's First Visit to America, 1841," *Proc. Am. Phil. Soc.*, 1980, Vol. 124, No. 3, pp. 188-202.

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Joan Lisa Bromberg. **Fusion: Science, Politics and the Invention of a New Energy Source.** Cambridge, MA: MIT Press, 1982.

Joan Bromberg's study of the U.S. fusion energy program succeeds both as history and as a study of science policy. As an historian she provides a detailed narrative of the growth of the fusion research program and makes her subject intelligible in both scientific and institutional terms. A careful observer of the ways in which policy is formulated and implemented, she also provides many informative insights into the processes by which decisions are made in big science. Funded and executed as a project history of the best sort (the author was given the time, access and freedom needed to produce a truly scholarly study), her book will naturally appeal to those who have played a part in the story she tells. It will also prove useful to readers with a more disinterested concern with science policy, especially those who bring to their study of the book a knowledge of other cases with which the one here described can be compared.

The story of fusion energy research achieves focus by virtue of its underlying physical principles. The nuclear reactions that occur during fusion, like those involved in fission, give off a tremendous amount of energy which, if somehow captured in a usable form, can be applied to the satisfaction of human needs and wants. Fusion reactions take place in special plasmas, highly attenuated gases containing charged particles, that are maintained at temperatures above 10 million degrees. The only feasible way to contain such plasmas in the state required to sustain a fusion reaction is to construct a device that uses the interactions of intense magnetic fields and the forces generated by the motion of charged particles to confine the plasma. Thus the fusion energy program is fundamentally an attempt to develop the science and technology needed to build a satisfactory magnetic containment vessel. This has proven to be a most difficult task.

The fusion energy story is diverse because of the many institutional commitments, not to mention the individual investments of effort and intelligence, that have been made since this program began. Bromberg certainly does not claim to tell the whole story. Not having been granted access to classified materials, she must pass silently by certain lines of investigation pursued during the first decade of research, just as she has been obliged to omit all consideration of laser fusion. She has also quite understandably decided to treat the scientific investigation of fusion as subordinate to her main concern, which is to describe the fusion "program strategy and its evolution." This then, is not, in the current sense of the term, a study of laboratory life, for the cognitive content of science is not the object of study. Bromberg brings to her work an essential and admirable command of the main scientific concepts involved, but her focus throughout is on institutional competition and federal policy. More particularly, she concentrates her attention on the four federal laboratories where most of the fusion research was carried on: Lawrence Livermore, Los Alamos, Oak Ridge, and the Princeton Plasma Physics Laboratory.

Fusion research began in earnest in 1950, when the U.S. decided that the proper response to Russia's first explosion of an atomic weapon was to build a hydrogen bomb. The possibilities of using fusion as an energy source received wide-spread public notice in 1951, when Juan Peron announced that Argentine, with the help of a German scientist, had successfully tested a fusion-reactor. The report was false, but the race was on. During the next three decades the research effort grew in size, complexity and sophistication. "The fifties were characterized by trial and error, the sixties by basic research, and the seventies by research more closely tied to a final, reactor product" (p. 250). But this periodization, while accurate in a rather narrow sense, gives too much weight to "technical" factors, which in Bromberg's usage are both theoretical and instrumental, as well as intentional factors. Indeed, one of the major goals of this book is to demonstrate that the fusion research strategy, throughout its 30 years of growth and change, "has always been nourished by the double root of technical and external determinants" (p. 2). It could hardly be otherwise for a program that its advocates argued should be budgeted at a billion dollars a year by 1987. (A misprint on p. 251, paragraph 3, reduces this vertiginous figure to a mere million dollars per year.)

Read as a policy study, this book exhibits both the strength and weakness of history. The weakness is that it does not present a theory of science policy that can be evaluated and refined in light of the evidence provided by this account. I therefore prefer to consider this book and others like it, a particular study in science policy rather than a case study. Although to some this may seem a trivial distinction, I believe it opens up the very serious question of how we should go about studying science policy. We can, on the one hand, see the experience of policy formation and implementation as normative and hence treat particular studies as cases in the English legal sense of establishing a precedent. Or we can, on the other hand, take a more theoretical approach and consider as cases only those studies that relate historical experience to theoretical first principles. In legal terms this latter approach would be identified with Roman law. As individuals we are, of course, free to present our work in whatever form we choose to. But if historians of science policy want to gain the attention of those political scientists and organizational theorists who study policy and are the chief advisors to policy makers, then they must in their work engage the theories that inform the thinking of those currently involved with questions of policy.

The strengths of Bromberg's study, from a policy point of view, arise from her willingness to go beyond the chronicling of events. Like all good historians she generalizes when describing the overall pattern of the story she tells and when comparing its essential features to those of similar cases. As fusion research grew in scale and complexity, there was a progressive centralization of governance, so that what began as autonomous efforts by local research teams became a Washington-based federal project with the scientists at the various laboratories acting as members of competing interest groups. The external factors that shaped the program also changed in time. In the 1950s the fusion program was part of a larger effort to sustain national prestige by maintaining U.S. hegemony over nuclear weapons and the application of nuclear power.¹ In the 1960s fusion energy research, not having led to usable results, was forced to play second fiddle to research on fission reactors, including

the breeder reactor. In the 1970s concern over environmental issues and long-term energy supply problems brought fusion research back into favor. Bromberg also generalizes very revealingly on the relations between scientific knowledge, instrumental development and research strategy in fusion research. The initial enthusiasm for fusion energy was grounded on little more than a post-war faith in the ability of physicists to solve whatever problems they address. Declassification of fusion research in 1958 and the subsequent sharing of knowledge among U.S. and foreign researchers, including the Russians, revealed how much remained to be learned. As the emphasis on basic, long-range scientific research increased, stable communities of fusion scientists formed and university degree programs in plasma physics were developed. But to secure funding for new and ever-larger machines, socially useful products were promised, even while the probable date for economically competitive power production receded ever further into the future. As Bromberg points out, the justifications offered for research support failed to keep pace with the changing political and scientific realities which the fusion energy program had to face.

Bromberg also offers several instructive comparisons between the fusion program and other big science programs. She points out that while in the 1960s the space program provided an arena for competition with the Soviet Union, the fusion program was one of the foremost areas of scientific cooperation. She also provides an interesting comparison between the various patrons and participants in leading areas of modern physics.

...Whereas solid state electronics research has been carried out under the commercial pressures of a highly competitive industry, space research has been influenced by a large array of government and private interests, and high-energy physics has reflected the needs of university-based scientists, magnetic fusion research bears above all the imprint of the government laboratories (pp. 252-3).

This is overall a most valuable book. Clearly written, rich in historical detail and studded with suggestive generalizations, it reminds us how many approaches can be fruitfully brought to bear on science and technology studies. But it also indicates that it will take a great deal of collaborative and interdisciplinary work to move beyond particular studies, of which Bromberg's is such an excellent example, so as to make this vast subject widely intelligible.

Note

1. Those interested in related developments in other areas of nuclear research will want to consult two essays in the recent collection *Energy in American History*, edited by Arthur L. Donovan, published as a special double issue of the journal *Materials and Society*, Vol. 7, 1983: George T. Mazuzan and J. Samuel Walker, "Developing Nuclear Power in an Age of Energy Abundance, 1946-1962," pp. 307-319; and, Jack M. Holl, "Eisenhower's Peaceful Atomic Diplomacy: Atoms for Peace and the Western Alliance," pp. 365-378.

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Edward M. MacKinnon. **Scientific Explanation and Atomic Physics.**
Chicago: The University of Chicago Press, 1982. x + 450 pp.

Edward MacKinnon has for a number of years been engaged in research on the history of quantum mechanics and has published several articles in this area. These articles form the heart of this current work. A philosopher by profession, MacKinnon nonetheless has established himself as an able historian familiar with the treatment of sources and technically competent in his chosen subject matter. This current expansion continues to display high standards. At minimum, the larger portion of this book serves as a fine, albeit selective, introduction to the history of quantum mechanics and its early interpretation. Chapters 4-9 treat respectively: the origins of quanta with Planck and Einstein, the emergence of the Bohr atom and the problems of the old quantum theory, Heisenberg's invention of matrix mechanics, De Broglie's and Schrödinger's contributions to wave mechanics, the forging of the Copenhagen interpretation, and finally, Einstein's critical reaction. There are some interesting departures from orthodoxy. MacKinnon claims, for example, that De Broglie's rationale for identifying particle velocity with group velocity rested on a gross conceptual confusion. Also, Schrödinger's attempts at a classical interpretation of wave mechanics is argued to have been precipitated by his realization of the formal equivalence of wave with matrix mechanics, which compelled him to seek something superior in his own approach over Heisenberg's. Bohr and his school, however, constitute the main focus, and MacKinnon does an excellent job of conveying the interaction of personalities and ideas--Bohr's grappling with the diminishing prospects for a coherent, yet complete, descriptive account of the atom; Heisenberg's cavalier and almost opportunistic instrumentalism; and Pauli's more cautious concern with first principles and foundational consistency, which, in MacKinnon's estimation, tended to keep him in the role of constructive critic rather than of groundbreaker.

But this is also history with a philosophical message. What does the history of atomic physics reveal about the nature of scientific explanation? Here we should understand what MacKinnon means by "scientific explanation." It is not the "-ology" of why questions that occupies the current literature. Rather, the intended sense derives, I believe, from Duhem, who posed the question, What is the aim of physical theory? Duhem considered two responses: (1) the summarization and logical classification of a class of experimental laws, and (2) the explanation of a class of experimentally established laws through the postulation and characterization of an underlying reality. But the second answer, Duhem argued, subordinates science to metaphysics. The postulated underlying reality, ex hypothesis, is empirically inaccessible, and hence its nature is a metaphysical problem. Duhem thus opted for the first response. Duhem's question, of course, was a normative one--what ought to be the aim of physical theory. Historically, as Duhem himself documented, the aim of science by and large was explanatory, although current scholarship tends to be more charitable in regarding the relation not as that of the subordination of science to metaphysics, but instead as a complex interplay of empirical and conceptual elements. Quantum mechanics, however, presents a notable prima facie exception to the explanatory rule insofar

as its experimental laws have proven resistant to interpretation in terms of any pat picture of reality.

There is a strong resonance between Duhem's distinction in the aims of science and Bohr's differentiation between the "descriptive" and the "formal" aspects of scientific explanation. Ultimately, though, Bohr produced an alternative to what Duhem took to be an exhaustive dichotomy. The Copenhagen interpretation is not just logical classification, nor is it traditional scientific realism. It embraces the idea that physics should give us explanations, but denies that physics can provide a representation of things as they exist objectively. In this regard (and others) there is a strong affinity with Kant's critical philosophy. Part of MacKinnon's project is to make these connections and to establish the outlines of an explanatory genus distinct from the one envisioned by Duhem.

More than that, Bohr and Kant become unabashed heroes. Although the reader is told that he or she can skip without loss of historical continuity chapter 2 on Kant's philosophy of science, directed ultimately at the concept of matter presented in Die metaphysischen Anfangsgründe der Naturwissenschaft, the grand analogy would be missed: Kant was to the interpretation of classical physics as Bohr was to the interpretation of quantum physics (or would have been had he written as a professional philosopher). MacKinnon's thesis is not just that they should be lauded for producing an alternative, but that this alternative is the preferred one. Explanations postulating an objectively existing reality stumble, not because they subordinate science to metaphysics, but because they have proven to be ultimately incoherent.

In order to accommodate Kant as well as Bohr, the historical scope extends to include the entire history of "atomism." The first chapter sketches the subject from antiquity through Euler, and an intermediate chapter surveying the nineteenth century bridges Kant and the twentieth century. The quality of the added history, however, does not come up to that of later chapters. Chapter 1 is particularly uneven and strained. MacKinnon introduces the subordinate theme of the "mathematization of nature," but discusses it entirely in terms of the medieval doctrine of the intension and remission of qualities to the exclusion of other influences, such as the Archimedian tradition. Later chapters presuppose a familiarity with "Leibnizian intelligibility," but Leibniz himself is not discussed and all we are told about "Leibnizian intelligibility" is that it is "intelligibility manifested through conceptual consistency" (p.39). MacKinnon asserts that Newton thought "atomic assumptions" were necessary for classical mechanics (p. 12) and finds it "paradoxical" that "Newton accorded atomic assumptions an apparently peripheral role in the first edition of the Principia" (p. 27). The plausible explanation is that just as Newton wanted his doctrine of white light and color to be demonstrated without assuming the corpuscular nature of light, so he intended his mechanics (to him literally "the mathematical principles of natural philosophy") to stand independently of any thesis concerning the nature of matter, although he hoped it might ultimately provide him with the basis for an argument for the existence of atoms, just as he thought it provided him what he needed to argue for universal gravitation. If this is so, there is no irony, as MacKinnon thinks there is, in Euler's

success in "reinterpreting mechanics as essentially a science of macroscopic matter" (p. 5).

The expansion in historical scope, besides yielding some inadequate history, also threatens the overall thematic coherence. "Atomic physics" is now a pun. Classically, the issue is the nature and existence of a species of ultimate simples. With the twentieth century the problem is the behavior of a category of composite systems already recognized to exist and already partially understood. This equivocation could be tolerated if the philosophical quandaries emerged in parallel fashion. But they do not.

MacKinnon explains that he intends "to use both historical reflection and philosophical analysis to bring out the interpretive problems generated by the use of atomic assumptions in scientific explanation" (p. 11). The intimation is that these problems arise because they are intrinsic to the subject matter and not because they are instances of the general problems of explanation, e.g., the problems raised by the role of unobservables in reductive explanations. One problem for classical physics is that the assumption of genuine atoms of finite size produces a dilemma. These atoms cannot be elastic, or else they have parts and are not genuine atoms. On the other hand, if they are hard rigid bodies, then interatomic collisions are completely inelastic, and hence, discontinuous. With quantum mechanics, the internally generated problem (although I don't see that it is due to atomic assumptions) is that the empirical correlations governing atomic behavior are not explicable in terms of classical models. Unless hidden variables are presumed to exist, straightforward realism is difficult, if not impossible. This forces a re-evaluation of standards of scientific explanation, suggesting, e.g., that a description of reality without inclusion of the conditions of observation is impossible.

MacKinnon's thesis, now, is that in each case (viz. the classical and the quantum) the interpretive problems of physics are insoluble unless the type of perspective advocated by Kant and Bohr is adopted. In the quantum case, even if we don't agree that Bohr's position is the inevitable, at least we suspect that something about the nature of scientific explanation needs drastic re-evaluation (perhaps the assumption that all variables are single valued). Classically, however, the problems arise from the specific choice of models and not from the use of realistic models per se. If Kant succeeded in giving a coherent account of matter in Die metaphysischen Anfangsgründe der Naturwissenschaft, it was not because the model was presented as part of the critical philosophy. (Without the critical philosophy the claims can no longer be taken as a priori, but that's a separate issue.) Nothing intrinsic to Kant's account of matter prevents it from being reinterpreted realistically. In fact, Kant subscribed to essentially the same model in his precritical period.

The critical aspect of the doctrine becomes important only in the face of externally motivated questions. As MacKinnon states,

One explains macroscopic properties of bodies in terms of the properties attributed to the atoms constituting these bodies. Yet one can only speak of the properties of atoms in terms derived from macroscopic bodies. How can the order

of explanation reverse the order of meaning on which it depends? (p. 13).

This is a problem whenever reductive explanations employ unobservables in the explanans. But if the real issues are of this general character, they should be discussed as such, and not just brought to bear when it is convenient for MacKinnon's purposes.

The philosophical issues never really are untangled or given definite shape. MacKinnon claims that two issues raised by his study that "slip through the nets of logically oriented philosophers" (p. 2) are those of "foundational concepts" and "coherence." In the text, however, "foundational" is just a bloated catch-all used frequently when near synonyms (primitive, fundamental, unanalyzed) would have been more appropriate. "Coherence" becomes no more than an epithet for what Bohr and Kant achieved but others failed to. Bohr and Kant also exemplify "the primacy of pragmatics"--"one accepts the science that works on the grounds of its success" (p. 2). But the content of this principle is never sufficiently articulated. At minimum it entails that a successful theory should not be rejected just because it conflicts with previously accepted norms of explanation, but the norms should be revised to accommodate the theory. Thus Einstein is accused of violating the principle in his rejection of the Copenhagen interpretation. But what should a theory be accepted as? Certainly we should not build a philosophy of nature on a theory just because it is instrumentally successful. Exactly when should a theory be taken as foundational and when should a reduction to a deeper theory be sought? This sort of question slips by.

As history, this book is by and large successful. As it aspires to greater ambitions, the wings begin to melt.

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Anthony Pagden. **The Fall of Natural Man: The American Indian and the Origins of Comparative Ethnology.** Cambridge Iberian and Latin American Studies. Cambridge: Cambridge University Press, 1982.

Race is in the eyes of the beholder. How people are classified into races depends on which traits the classifier uses and how important he perceives them to be. Throughout history there have been two patterns of racial classification, those in which races are perceived to be alike and those in which races are perceived to be different. These two patterns prevail even in scientific anthropology. As the distinguished evolutionist George Gaylord Simpson said, taxonomy is an art as well as a science.

The history of racial classifying in anthropology can be traced back to Greek philosopher-scientists, early Church fathers and Medieval scholastics. Christianity supported the classificatory pattern of relative racial similarity. There was need to reconcile racial taxonomy with the Biblical story of Adam and Eve. More important, unless all people were at least similar enough to be convertible to Christianity, religious proselytizing was pointless. In the history of anthropology the doctrine that races are relatively similar due to a common ancestry is called monogenesis.

When Saint Augustine codified the monogenist doctrine in Roman times, the New World was unknown. In fact, Saint Augustine pronounced that it could not exist; there simply were no lands on the opposite side of the earth--no antipodes. In the initial centuries following the decline of Rome, Europe became parochial, and this idea was not seriously challenged. Columbus' discovery of the New World, however, changed that. This new land with its strange Amerindian inhabitants was without recognizable precedent in Europeans' experience. To some, the Amerindian was not fully human and was difficult to reconcile with monogenist orthodoxy. The New World gave impetus to an alternative view, polygenesis, the doctrine that races are essentially different due to separate origins. Monogenesis and polygenesis became opposing patterns of classifying races in anthropology.

From the beginning, polygenesis was considered unorthodox because it conflicted with the Bible and the Christian ethic of brotherhood. But in the centuries following the Scientific Revolution, as science gained ground on religion, polygenesis gained ground on monogenesis and by the mid-nineteenth century, when anthropology became a profession, polygenesis was widely accepted. During the decades preceding professionalization the monogenesis-polygenesis controversy was the main theoretical issue in physical anthropology. Anthony Pagden has made an important contribution to the historiography of that dispute.

The conventional wisdom among historians is that polygenesis supplanted monogenesis because the Amerindian "fact" of racial difference could not be accommodated by the monogenist "theory" of racial similarity. Pagden takes exception to this inductivist epistemology. Rejecting what he calls a naive view that facts are "out there" and, once discovered, will speak for themselves, he argues that facts cannot even be described much

conforming facts. The Fall of Natural Man is about how orthodox racial theory was modified from the early sixteenth through the early eighteenth centuries. This period saw in Europe a veritable flood of nonconforming facts in the form of travel literature and missionary accounts. Pagden shows how, independently, Christian theologians were tinkering with orthodox to shore up inconsistencies that were inherent in theory.

In the early 1500s, the anthropology of Amerindians was Aristotelian and Thomist. The Amerindian was perceived to be imperfect by nature and therefore a natural slave to Europeans. Pagden begins by exposing the philosophical basis of the natural slavery ideas as it related to the idea of barbarism. The fall of natural man is how natural slavery came to be replaced by the modern idea that culture rather than biology or psychology is held accountable for Amerindian-European differences. The motive to move in this direction was there from the start: If natural slaves were imperfect by nature they could not achieve perfection and could never attain salvation by conversion to Christianity. To become Christian, the Amerindian needed at least some endowment of free agency, and free agency was at odds with the natural slavery idea.

The core of Pagden's book is a detailed examination of the scholarship of three Spaniards who tackled this paradox and ended up granting the Amerindian more and more free agency, thus undermining the natural slavery idea and paving the way for the modern concept of culture. He begins with the sixteenth century Dominican Francisco de Vitoria of the Salamanca School of theology. Vitoria wrote at a time when the number of Amerindians that had come under Spanish control was so huge that the public had become concerned about the possible illegality and immorality of the Spanish conquest. By reinterpreting Classical and Christian texts (more than by reading accounts of Amerindian life) Vitoria decided that Amerindian barbarism was not the product of an inherently inferior mind, rather it was the product of cultural influences. Amerindians were likened to European peasants. Both were not nature's slaves, but nature's children.

Two of Vitoria's followers helped fashion this idea into the conceptual framework of modern cultural anthropology, or comparative ethnology. Bartolome de Las Casas argued that pre-conquest Amerindian society fulfilled all of Aristotle's requirements for a truly civil, i.e. non-barbarian, society and differed from European society for cultural rather than biopsychological reasons. For Las Casas, culture was the means by which men learn to exploit God's gift of human potential in nature. Once tapped, this gift saw a race grow in complexity until sooner or later it attained civilization and, ultimately, Christianity. Although it was teleological, this crude cultural evolutionism allowed for analysis of particular cultural evolutionism allowed for analysis of particular cultural and historical circumstances that might have hindered the Amerindian's progress toward perfection. According to Pagden, Las Casas' Apologetica historia (1551-) was the "first detailed comparative analysis of Amerindian culture."

Las Casas' work remained largely unknown during his lifetime. Not so for the second member of the Salamanca School, Jesuit historian José de Acosta, whose Historia natural y moral de las Indias (1590) was influential throughout the 1600s. Acosta challenged Saint Augustine's pronouncement that antipodes did not exist and postulated a geographical connection

between the New World and the Old World, explaining that differences between Amerindians and other barbarians were due to different amounts of time these groups had spent being nomadic preceding civilization. Acosta insisted that there were several kinds of barbarism that could be graded by examination of culture traits. It was but one more logical step to realize that if Amerindians were frozen in time compared to Europeans, Amerindian culture might be used to represent the culture of prehistoric Europeans, linking ethnology with archaeology in what is now called the comparative method of anthropology. This step was taken one century later by Joseph-François Lafitau, whose Moeurs des sauvages américains comparées aux mœurs des premiers temps (1724) is sometimes identified by historians as the first early modern ethnology. Pagden shows how the comparative approach--perhaps the culture concept itself--antedates Lafitau by more than one hundred years. This places the dawn of early modern anthropology in the sixteenth century rather than in the eighteenth century Enlightenment, where tradition now has it.

Pagden's major primary source of information is the written record of university lectures, which he points out is a yet-little-understood social and intellectual milieu for the recording of history. For this reason his book might seem esoteric, even be difficult reading, for the historian used to early ethnological descriptions rather than early philosophical disquisitions in anthropology. But that is Pagden's point--there was this very kind of classically scholastic discussion (taking place independent of the facts. Pagden interprets the fall of natural man as a liberalizing development that resulted in movement away from a biopsychological toward a culture-historical theory of race, that is from a hereditarian toward an environmentalist philosophy. In the history of cultural anthropology this interpretation is correct. But at the same time, tinkering with the classical racial orthodoxy had an opposite effect in the history of physical anthropology. There the decline of monogenesis led to the rise of polygenesis, and polygenesis was heavily hereditarian. Although they are attenuated, these two opposing tendencies are still present in general anthropology. Pagden's contribution is to show that understanding how we got that way can begin two hundred years earlier than previously thought.

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4S PRESENTS BERNAL AWARDINTRODUCTION OF THE 1983 RECIPIENT OF THE
JOHN DESMOND BERNAL AWARD--THOMAS S. KUHN

John Ziman

We are all Kuhnians nowadays. That is the simplest measure of the influence of the work of Thomas Kuhn on the various disciplines that are brought together under the umbrella of "Science Studies." When the great continent of Science was discovered to be part of the academic world, landings were made at various points, flags were flown, and the whole territory formally claimed by each of several nations. The philosophers came as pilgrims, and grimly annexed the cold northern coasts; the historians arrived with cargoes of slaves, and settled in the sunny South; a little band of sociologists established themselves precariously in the Far West; there even came some political scientists and economists, to trade with the natives. The natives, incidentally, regarded all these intruders with disdain, and went happily on, hunting their quarks, quasars, and chromatids: they have still not been tamed.

At first, each colony was able to remain loyal to its own disciplinary flag and its own academic doctrines. But as the little territories expanded, and the colonists pushed inland, they began to come into contact and conflict. A great landmark of Science, such as Newton's theory of Universal Gravitation, could rightly be claimed by the philosophers as a logical necessity, by the historians as a contingent event, by the sociologists as a communal institution and even by the political economists as a technological innovation. Enter Thomas Kuhn - not Kuhn the revolutionary, but Kuhn the unifier. The deep message of *THE STRUCTURE OF SCIENTIFIC REVOLUTIONS* was that these jurisdictional disputes were futile. A scientific theory can only be grasped metascientifically as an entity with intertwined philosophical, historical, and sociological characteristics. Kuhn's imaginative notion of a scientific paradigm was the new concept needed to bring all these disciplines together, and to unite them into a single nation. That is why we are all Kuhnians nowadays, and why it is appropriate that this distinguished scholar should receive this notable award.

What more need be said? We have all of us, here, had to read Kuhn's book - which is a pleasure - and books about Kuhn's book (which is not always quite such a pleasurer) - and even articles about books about Kuhn's book. We have all had to discover and express our own understanding of his ideas, in our reading and writing, our research and our teaching. For some this experience has been liberating: for others, I daresay, that paradigm shift into another dimension of thought has been painful and is still incomplete. This influence has not been restricted to science studies in the narrow sense: Barry Barnes argues convincingly that it is subverting, if not revolutionizing, whole empires of the social sciences and humanities, who must somehow come to terms with the intrinsic social relativity of all forms of human thought, I don't think Tom himself entirely approves of all the atrocities being committed in his name, but that is the price to be paid for being a prophet, in scholarship as in life.

Let me end on a more personal note. I first met Tom Kuhn at Berkeley, in 1964, when we had some lovely talks and arguments strolling round that sunny campus. But his name was not unfamiliar, for I had come across Kuhn, T. S. and van Vleck, J. H. *Phys. Rev.*, vol. 79, pages 382 and 515 (1950). I will not try to explain the beauty of the quantum defect

method, for that was in another country (and besides, the wench is dead), but it was an elegant bit of theoretical physics which clearly indicated that the junior author would have made quite a name in that nation if he had not abandoned it so young. Ah well, as one renegade physicist to another - you were right Tom, and I should have followed you sooner myself. Nevertheless, that was an essential preparation for just such a scholar - one who has given us recently an account of the origins of the quantum theory that seems to put every equation of Planck and Einstein precisely into his historical place. And perhaps it was necessary that the unifier of the continent of science studies should have been, so to speak, a native-born scientist, able to communicate in that tongue also to those whom he has brought together.

REFLECTIONS
ON RECEIVING THE
JOHN DESMOND BERNAL AWARD*

Thomas S. Kuhn

When Arnold Thackray wrote, six months ago, to tell me I had been nominated for the John Desmond Bernal Award of the 4S Society, I was a good deal surprised but also much moved and greatly pleased. Neither of those reactions requires a speech, but I shall for a moment expand upon them.

As John Ziman has just said, my professional training was in physics. History of science is a field I first encountered late in graduate school, as assistant in a science course for non-scientists. That course was the invention of James Bryant Conant, then president of Harvard. It was built largely on case histories, and most of the reading consisted of excerpts from original scientific texts: Copernicus, Galileo, Boyle, Newton, Lavoisier, and others. That much will be familiar to anyone who has encountered one or another of the Harvard Case Histories in Experimental Science, but neither they nor Conant's public descriptions of the course reflect another of its aspects. Staff and students also read in such works as Hessen's Social and Economic Roots of Newton's Principia, Merton's Science, Technology and Society in Seventeenth-Century England, and G.N. Clark's Science and Society in the Age of Newton.

Those two sorts of material sat side-by-side in the course. Both seemed essential to the aims we hoped to achieve, but there was little apparent relationship between them. Both were simply there, and bringing them together seemed a most significant challenge. Though I did not take that problem as my own, my consciousness of its central importance goes back to the very start of my career, and it is gratifying to be honored now for contributions to its solution. That is the background for my pleasure.

My surprise has a different source. Doubtless, as John Ziman's presentation suggests, the work that brings me to your attention is The Structure of Scientific Revolutions. But when I wrote that book I thought of it as exclusively internalist, a limitation by which I was actually a bit embarrassed. In the introduction, I took pains to document my awareness that conditions in the larger society did in fact impinge on scientific advance. I had elsewhere, I pointed out, written on Copernicus and the calendar as well as on Carnot and the steam engine. But considerations of the social setting of science had, I insisted, no place in the present book. It was based entirely on the technical writings of scientists, the sorts of sources to which the Harvard Case Histories had introduced me.

*The initial remarks that follow are a lightly polished version of those made when accepting the Bernal Award on the evening of 5 November 1983. The Addendum which follows them is drawn in part from my contributions to a symposium the following morning. In putting the Addendum into final form I have benefitted greatly from conversations with Sharon Treweek and Steve Woolgar, but they bear no responsibility for the views expressed.

Let me put that point another way. Probably the thing I do best and certainly the one to which I have devoted most time is climbing from the writings into the minds of dead scientists, figuring out how they thought, why they believed what they did, and how they came to change their minds. My principal efforts have, that is, been directed to what I have sometimes called "the dynamic interrelationships of pure ideas," and I could scarcely be more aware that to many members of this audience any phrase of that sort is hair raising. How could someone who uses it to describe his central concerns be a suitable recipient for the Bernal Award?

Clearly I had overlooked something significant about my own work. There are reasons for the invitation you have extended to me, and my attention was first called to them in a letter from Alexandre Koyré. Those of you who know his work will be surprised to find him in that role. For those who do not, let me say a few words about him. More than any other single figure, Koyré provided my model of the way to deal with old ideas and their development. He was not literally an internalist, for he emphasized the role of non-scientific ideas, especially philosophical and religious ideas, in the development of science. But he was a passionate opponent of the thesis that socio-economic factors might also play a role. Bacon was for him a fraud, and he denied to the Baconian program any role at all in scientific development. Confronted with an account of the contribution to Galileo's work of Renaissance engineering, and especially of the Venetian Arsenal, Koyré responded with a remark that still raises my hair: "Galileo went to the Arsenal to teach, not to learn."

Nevertheless, nine months after Structure appeared, Koyré--then suffering from what shortly proved a terminal illness--wrote me a letter from which I shall quote a single paragraph here.

I am reading now. I have received it from Blackwell's just before we left--your Structure of Scientific Revolutions. I hardly need to tell you how interesting and exciting I find it. Your concept of "paradigm" and your socio-psychological considerations about the behavior of the "profession" seem to me to be really illuminating and to fill the gap between the history of science as such and the social history that till now were miles apart.

The idea that I was a gap filler startled and initially puzzled me, but I shortly realized what Koyré had in mind. By emphasizing that scientific knowledge was the product of scientific communities, themselves set within the larger society, I had uncovered an interface through which the socio-economic influences he had elsewhere rejected could play a role in the development of science. It pleases me greatly that so many of you join him in valuing a contribution to which I had myself been blind.

Addendum

After the conclusion of the preceding remarks, my two fellow panelists, Bernard Barber and John Ziman, independently accused me of disingenuousness, and my remarks had, in fact, been simplistic. The preface to Structure acknowledges that a reading of Ludwik Fleck together with a remark by Francis Sutton had made me realize my ideas "might require to be set in the sociology of the scientific community." I could not, when

I wrote those words, have been altogether innocent of sociological consciousness.

Nevertheless, when accepting the Bernal Award, there was a reason for speaking as I had: I did not then want to be heard complaining that, twenty years after Structure, the gap between the social study of science and the development of scientific ideas was in most places very nearly as wide as it had been when I first encountered it. With that occasion now behind me, I can provide the beginnings of a more balanced account of the relations, actual and potential, between the concerns of Structure and those of the sociology of science.

Structure is sociological in that it emphasizes the existence of scientific communities, insists that they be viewed as the producers of a special product, scientific knowledge, and suggests that the nature of that product can be understood in terms of what is special in the training and values of those groups. Having insisted upon those points, however, I proceeded to make up the sociology of such communities as I went along, or rather to draw it from my experience with the interpretation of scientific texts supplemented by my experience as a student of physics. That is an abominable way to do sociology, and it did not occur to me that its outcome would, qua sociology, have a claim on the attention of members of that profession. They might, I suppose, learn something useful from my book, but that something could not be sociology.

That is a better account of my sense of irony in receiving the Bernal Award, and the complaint I had previously veiled emerges from it. If Structure is a contribution to the sociology of science, then the core of that contribution has been missed or else denied by many of those who trace their own work to it. My concerns, both in Structure and elsewhere, have indeed been sociological, but they have also and inseparably been cognitive or epistemic. The question that more than any other has guided and motivated me is not simply why scientists choose the problems that they do, but also, far more urgently, why the special nature of group practice in the sciences has been so strikingly successful in resolving the problems scientists choose. What is it about what scientists do, I have been asking, that makes their output knowledge? Those concerns prove extraordinarily hard to find in the literature of science studies.

Surely, that need not be the case. If I could draw sociological conclusions from the developing cognitive practices and products of scientists, then it should be possible to traverse that path in the reverse direction. One would then begin with sociological inquiry and relate its results to the contents of the research reports scientists produce, to what occurs when those reports are applied, and, above all, to the way those contents and consequences change with time. One would, that is, use sociological investigation in explaining the relation between the cognitive content of an older science and the content of its successor. At that point sociology of science would truly be sociology of knowledge, and that is the ambition of Structure that remains to be fulfilled. In most circles it is not, I fear, even recognized. In a few the very existence of such a topic is denied.

Why should this be? Why should the gap between the concerns of Structure and those of the social study of science continue so vast? To that question I do not know the answers, but I shall suggest three worth considering. First, though I think it unlikely, the gap may be in principle unbridgable, in which case complaint serves no purpose. Second and more likely, the cause of the gap may be the background of the people being recruited into the social study of science. If the gap is to be bridged, that will have to be done by people with sufficient specialized training to read the technical papers of the scientists they study. There are other significant sorts of work to do in the social study of science--work that does not demand significant background in science--but they should not occupy the entire field simply by virtue of its currently standard patterns of recruitment.

Right or wrong, these two explanations of the gap are obvious. I think of another, however, which is less so. For many years science was taken to be the discipline which achieved objective knowledge by the application (often seen as quasi-mechanical) of proper method. So conceived, science was autonomous. Internalist studies, which were philosophical and historical, then examined the nature of scientific method and chronicled its development and application. Externalist studies, which were sociological and historical, examined the ways in which social conditions, primarily institutional and ideological, enhanced or impeded the growth of scientific knowledge. Both the content and the cognitive status of scientific knowledge were the exclusive business of the internalist.

The first great break in that conception of an autonomous science occurred in the early 1930's, primarily under the influence of Marxist studies. The shape of scientific institutions, the selection of the problems their members pursued, and the general direction of scientific advance were all seen to be affected (some people saw them as entirely determined) by the socio-economic interests of the society within which scientific activity occurred. Science was no longer autonomous: factors external to the enterprise affected its content as well as its rate of development. Externalists, while increasingly restricting their attention to the socio-economic--to what were increasingly referred to simply as "interests"--simultaneously expanded their scope to include the content of the sciences and the direction of scientific development. What was left for the internalists--still centrally concerned to trace and to explain the cognitive achievements of sciences--was the analytic and historical study of methods and ideas, the range of the latter now often extended to include such areas as philosophy and religion. It was only as this shift took place that my mentor, Koyré, could come to seem the archetypical internalist.

The developments just sketched were, I think, unequivocally fruitful. The scientific enterprise is not autonomous. Though socio-economic interests are not the only means through which society impinges on science, they are surely the main ones, and they had previously been too little studied. Sociology of science made a major contribution by focussing upon them, and that focus remained appropriate while sociology of science remained externalist, concerned primarily with the effects of the larger society on science and eschewing the internalist's concern with science as knowledge. But the belated emergence during the past two decades of an internal sociology of science, one that takes scientific communities and their products as the objects of sociological scrutiny, has shifted sociological

concerns without altering the model used to pursue them. Interests remain the dominant factor that practitioners of the new field employ in explanation, and the interests they deploy remain predominantly socio-economic. To me the result often seems disaster.

Almost by definition, resort to the term "interest" has excluded the special cognitive interests inculcated by scientific training and required, as an ingredient in explanations, by any sociology that claims to absorb or displace the traditional concerns of internalist studies. We shall not, in my view, account at all for scientific knowledge without recognizing the determining role in scientific practice of just such special interests: love of truth (fear of the unknown, if one prefers); fascination with puzzle solving (compulsion to exhibit special skills); repudiation of the past (obsession with the law of the excluded middle); and others of the sort. While internal sociology of science continues to ape external in excluding considerations like these, the gap between the concerns of Structure and those of the social study of science will continue vast, and my enterprise will have failed in its central purpose. Structure, after all, was intended to suggest that the status of knowledge is in no way reduced when knowledge is seen as social.

Members of the panel on "Issues in the Comparative Analysis of Science-Related Controversies" at the 1983 4S meeting are interested in continuing the discussions that were started in Blacksburg. Gary Downey, John Wilkes, Gerald Markle, and Arie Rip have prepared updated versions of their remarks and will provide copies to all those willing to provide comments and/or critiques. A copy of the statements of the panelists may be obtained from the panel organizer.

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SOCIETY NEWS

**PRESIDENT MULLINS ANNOUNCES
COUNCIL AND COMMITTEE MEMBERSHIPS AND ASSIGNMENTS**

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CHARGE: Council is in overall policy charge of the society. At least one member of Council is appointed to each committee, but not as Chair. The Chair of each committee is responsible for the activity of the committees. All committees report to Council for action and approval.

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CHARGE: The immediate past President (Thackray) is a voting member of the Council under the Charter. The previous Presidents are all nonvoting members of Council (assuming the Charter revision).

Program Committee

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Virginia Polytechnical Institute and
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Sal Restivo (Council)

John Wilkes
Social Science
Worcester Polytechnical Institute
Worcester, MA 01609
(617)

Home:
4 Longfellow Rd.
Worcester, MA 01602

CHARGE: To develop the program for the November, 1984 program in Ghent, in coordination with Council.

TIMETABLE: Call for papers in current issue of 4S Review. Preliminary program due by 30 May 1984 for printing in 4S Review. Final program including abstracts of all papers to be available by 1 August 1984.

Publications Committee

James McCartney (Chair)
Department of Sociology
University of Missouri
Columbia, MO 65211
(314) 882-3542

David Edge (Council)

Harry Collins (Council)

Daryl Chubin (Council)

Jerry Gaston (ex officio, Editor 4S Review)
Department of Sociology
Texas A&M University
College Station, Texas 77843
(409) 845-5133

Sal Restivo (Council)

Arnold Thackray (Council)

CHARGE: To set general policy for publications of the Society. To review and oversee the publication of the 4S Review. To supervise other possible publications such as a membership directory.

Bernal Prize

John Ziman (Chair/Council)

Thomas Kuhn (1983 Prize Winner)
STS Program
Massachusetts Institute of Technology
Cambridge, MA 02139

Robert Merton (1982 Prize Winner)

Arnold Thackray (Council)

CHARGE: To nominate the 1984 winner of the John Desmond Bernal Award for distinguished contributions to the social studies of science. To provide Council with a document suggesting procedures for selecting future Bernal Prize committees.

TIMETABLE: Preliminary list of candidates to be circulated by 28 February 1984; awaree to be agreed on by 30 May 1984.

Nominations Committee

Ron Giere (Chair)
130 Goodbody Hall
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Bloomington, IN 47405
(812) 335-8916
(812) 335-3622 (messages)

Bernard Barber (Former President)

Stew Gillmor
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(203) 387-9411 x348

Dorothy Nelkin (Former President)

Henry Small
Institute for Scientific Information
3501 Market St.
Philadelphia, PA 19106
(215) 386-0100

Ron Westrum (Council)

CHARGE: To nominate 10 individuals for 4 Council vacancies (assuming the Charter revision), with appropriate disciplinary and geographic spread.

TIMETABLE: Preliminary list of candidates to be circulated by 28 February 1984. Full list of candidates with c.v. and statement of goals to be available by 30 May 1984, for publication in the 4S Review.

Membership Committee

J. Scott Long (Chair, Secretary)

Lowell Hargens (Treasurer)

Edward Woodhouse
Department of Science and Technical Studies
Rensselaer Polytechnical Institute
Troy, N.Y. 12110

CHARGE: To advise Council on membership retention and expansion.

TIMETABLE: Proposal together for circulation by August 1, 1984.

AAAS Committee

Rae Goodell (Chair)
Department of Humanities
Room 14E-310E
Massachusetts Institute of Technology
Cambridge, MA 02139
(617) 253-4069

Daryl Chubin (Council)

David L. Hull
859 W. Wellington Ave.
Chicago, IL 60652

Albert Teich
Office of Public Sector Programs
American Association for the
Advancement of Science
1776 Massachusetts Ave., N.W.
Washington, D.C. 20036
(202) 467-4310

CHARGE: To coordinate representation from the 4S to the AAAS sections and the entire association. To develop a procedure for setting up 4S sponsored sessions at future AAAS meetings, starting with Los Angeles May, 1985.

TIMETABLE: A document suggesting representation strategy to be prepared for Council by 1 September 1984. A procedure for 4S sessions to be set up by 1 March 1984.

Future Meetings Committee

A. Bayer (Chair)
Department of Sociology
VPISU
Blacksburg, VA 24061
(703) 961-6121

Ronald Calinger
Department of History and
Political Science
Rensselaer Polytechnical Institute
Troy, N.Y. 12181
(518) 270-6325

T. Gieryn (Council)

T. Nickles
Philosophy Department
University of Nevada
Reno, NV 89557

CHARGE: To develop plans for 4S meetings for 1985, 1986 and 1987. Those plans will include the 4-society meeting in 1986.

TIMETABLE: A document outlining options for 1985 and any proposals for 1987 to be available by 1 May 1984. A recommendation for 1985 to be made by 1 September 1984.

Local Arrangements - Ghent

Marc De Mey
University of Ghent
Blandinnberg 2
9000 Ghent
BELGIUM

David Edge (Council)

CHARGE: To coordinate with local arrangements being made in Ghent by local committee.

TIMETABLE: To present local plans for publication in 4S Review by 1 March 1984.

Local Arrangements - AAAS

Charles Bazerman
Department of English
Baruch College
The City University of New York
17 Lexington Ave.
New York, N.Y. 10010

Lois Peters
Center for Science and Technology
Policy
New York University
114 Liberty Street Rm 600
New York, N.Y. 10006
(212) 285-8946

CHARGE: In coordination with AAAS committee, to arrange 4S activities in New York for May, 1984.

TIMETABLE: To have plans available for publication by 1 March 1984 in 4S Review.

**4S NINTH ANNUAL MEETING
UNIVERSITY OF GHENT
BELGIUM**

15-18 November 1984

The 1984 annual meeting of the Society for the Social Studies of Science will be held jointly with two other scholarly associations: the European Association for the Study of Science and Technology (EASST) and Communication and Cognition (C&C), a Belgian group for interdisciplinary studies of cognition and science located at the University of Ghent.

The meetings have been designated as a centennial for George Sarton. Sarton was a Belgian born scientist who founded ISIS, the highly reputed international journal devoted to the history of science and its cultural influences. To bring together representatives of current science studies from both America and Europe is considered the most appropriate commemoration of a scientist and scholar who valued so highly an integrated approach to science.

CALL FOR PAPERS

The 4S Program Committee welcomes members to submit papers to be presented at the Sarton Centennial Meetings, whether or not they are sure to travel funds to attend. Through the joint efforts of the three co-sponsoring societies, we are working toward a program of particularly high quality. Three-hour sessions will allow for longer presentations than in the past, and the diversity of perspectives of the sponsoring groups promises a particularly stimulating gathering.

Deadlines for program materials take on more than their usual importance this year. They are:

1 March 1984	Papers or abstracts due to program coordinators (names and addresses below)
1 May 1984	Preliminary program announced
1 September 1984	Extended abstracts due for accepted papers

Extended abstracts (four pages) will be reproduced and distributed to each registrant.

MEMBERS PLANNING TO ORGANIZE SESSIONS ARE URGED TO CONTACT THE PROGRAM COMMITTEE WELL AHEAD OF THE MARCH 1 DEADLINE FOR PAPERS.

Papers or abstracts may be submitted to any of the three program coordinators:

4S: Susan E. Cozzens Room 1229 National Science Foundation Washington, D.C. 20550 (202) 357-7826	EASST: Arie Rip Chemistry and Society Program University of Leiden 2300 RA Leiden Netherlands	C&C: Werner Callebaut Communication and Cognition Blandijnberg 2 9000 Ghent Belgium
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[Examples of topics for papers are on p. 38.]

TOPICS

Papers may be on any subject of intellectual interest to the Society. Past programs have included work on the social and psychological processes of scientific investigation, the development of technologies and sciences, science and the public, and science policymaking. In addition to work within this general range, papers on the following topics, many of which have not been covered in the past, are welcome:

Medical science and medical practice
 Economic perspectives on the development of technologies, specialties, and disciplines
 Science education: historical and comparative perspectives
 Women and science
 Research evaluation: its conceptual basis
 Sociology of scientific societies and interest groups
 Science and War
 Sociology and social history of mathematics
 Autonomous technology: where does the discussion stand?
 Science in developing countries
 The concept of scientific progress used in science policymaking
 Science in industry
 Instrumentation and scientific advance
 The scientific labor market
 Effects of organizational context on scientific research
 Quantitative approaches to the study of knowledge growth
 The reception and disposition of innovation
 Linguistic practices and social structure in science
 Social and political influences on problem formulation and choice
 The political sociology of Really Big Science

EIGHTH ANNUAL MEETING

ABSTRACTS OF PRESENTATIONS
LISTED ALPHABETICALLY

BAZERMAN, Charles
English Department
Baruch College

"The Physicists Reading Physics: Purposes and Schema in Selection, Understanding and Evaluation"

Reading, as well as writing, is a constructive activity. In interviews and observations of research physicists, concerning how they select, understand and evaluate the research literature in their field, I have found their reading processes permeated with their individual purposes of furthering their own research and with their individual schema of their fields. These schema, or personal maps of the field, include not only consensual knowledge about the phenomena being discussed, but also perceptions as to the most promising lines of current work, methods that are most likely to produce good results, and personal knowledge about the other workers in the field. Texts rather than being treated as static arguments are read as part of a dynamic process of research activity.

BIJKER, Wiebe
University of Technology Twente

"A Unified Approach to the Sociology of Science and Technology - The Sociology of Artefacts"

It is argued that a unified approach to the sociology of science and the sociology of technology will bring benefit to each field. Such an approach is developed in this paper and the accompanying one (by Trevor Pinch). The basic principles of the unified approach are outlined and in this paper illustrated by reference to examples drawn from technology. In particular, the concepts of "interpretative flexibility," "rhetorical closure," "closure by redefinition of problem," and "relevant social group" are exemplified by drawing upon a case study of the development of the bicycle.

A discussion of possible differences between this example and those encountered in science (see accompanying paper by Pinch) is included.

CAPSHAW, James H.
University of Pennsylvania

"Muzak for the Masses: Psychology in Industrial Culture"

"Functional music" is ubiquitous in modern society. Beginning with its introduction into industry during World War II, its use has spread to offices, commercial businesses, banks, department stores and shopping malls, hospitals, and many other locations. This study traces the development of functional music, focusing upon the role of psychological research in legitimating its claims of improving worker morale and increasing productivity. The Muzak Corporation figures prominently in this analysis because

of its virtual monopoly on the production, sale and distribution of functional music since the 1940s. Aside from questions about the "real" effects of functional music, the paper concentrates on revealing the assumptions and values of management concerning the nature of human behavior and how it can be shaped to fit the economic priorities of the workplace and the marketplace.

COHN, Steven
University of Maine

"The Effects of Funding Changes Upon the Rate of Knowledge Growth in Algebraic and Differential Topology"

It is frequently asserted that changes in levels of basic research funding have major impacts upon rates of scientific knowledge growth. However, aside from anecdotal evidence there is no data on this point. Determination of the effects of funding changes is crucial to the formulation of science policy. This study represents an initial and exploratory attempt to measure the effects of funding level changes. The study proposes a model, based upon research in the history and sociology of science, on the effects of changes in funding, manpower and facilities upon the rate of scientific knowledge growth. The model implies that, within wide limits, funding changes may not affect knowledge growth rates in the short term but will in the long term. The extent of medium term effects is hypothesized to be dependent upon the structure of funding and the utility of the research done by less eminent scientists. The rate of knowledge growth was measured by the judgments of a scientific panel of the rate at which important articles have appeared. Correlation and multiple regression analysis yielded results that were broadly in accord with the model, though a number of uncertainties remain.

COLLINS, H. M.
University of Bath

"Scientific Knowledge and Science Policy: Some Foreseeable Implications"

One current model of pure science policy is examined. This model is based on measuring the contributions of individual laboratories by their output in terms of a variety of indicators. It is suggested that all these indicators reflect the "reward system of science" and its atomistic bias.

The sociology of scientific knowledge looks at science as a knowledge producing system. From this perspective policy decisions based on measures founded in the atomistic reward system can lead to results which could be counter-productive in the long run. Science needs to be looked upon as comprising an infra-structure of institutions and skills which, while they contribute little which is directly measurable, support the small body of outstanding work. The appropriate questions are to do with the size and

organization of the minimal infra-structure. Other questions concern scientific activity at the other extreme-risky innovative research which is more likely than not to lead nowhere. The peer review system supported by bureaucratic measures is likely to squeeze such research disproportionately in time of financial stringency. It may need to be safeguarded through a modified system of review.

CONSTANT, Edward
Department of History
Carnegie-Mellon University

"Kuhnian Communities and External Social Structure the Dissemination of Petroleum Engineering Knowledge"

In this paper, I want to assert that theories of science, whether orthodox Kuhnian models, emphasizing community structure and communication, ethnographic approaches, focusing on laboratory or institution level direction of scientific research, or socially deterministic models, emphasizing the "social necessity" of science, indeed are relevant to technology. More critically, I should like to offer the converse and somewhat bolder argument: that the process of technological change, especially changing technological knowledge, is relevant to science and throws some light on the above noted controversies.

Examination of the evolution of one engineering science, petroleum engineering, and its dissemination to and interaction with its host society, reveals both extraordinarily complex patterns of growth and, of course, major social consequences--the radical redefinition of oil production policy. The development of petroleum engineering is characterized by the following features: (1) knowledge production in a complex, stratified, and differentiated social system; (2) knowledge dissemination shaped by external social structure, especially the oil and related industries and political arrangements; (3) knowledge credibility and acceptance strongly influenced by social values and subculture membership. These rococo patterns in the development of an engineering science imply that controversies over the social nature of scientific research may in fact represent different perceptions of a complex social reality rather than differing interpretations of a simpler and more tractable world.

COZZENS, Susan E.
National Science Foundation

"The Internal Structure of Problem Sets: A Closer Look"

Gieryn has proposed that it is important for sociologists of science to study the determinants and consequences of individual problem choices. To assist in this effort, he has introduced two concepts: the problem set, that is, the configuration of problems being pursued by a researcher at a given point in time; and the research career, the series of problems studied by an individual.

In relation to the structure of problem sets, Gieryn has studied primarily one dimension of variation: size. Two "styles of problem choice" have been distinguished, foxes and hedgehogs, so labelled from Berlin's aphorism: "The fox knows many things, but the hedgehog one thing of much importance." Operationally in Gieryn's work, foxes have large problem sets and hedgehogs have small ones. Gieryn has explored the consequences of these styles for recognition of the scientists's work and found that hedgehogs are on average better recognized than foxes.

In my paper, I explore the concept of problem choice style further, using as materials the curricula vitae of three well-established scientists. I identify two of these as hedgehogs and one as a fox, but I do so on the basis of the structure, not just the size, of their problem sets. Structure, in these cases, is defined sociologically: the hedgehogs are studying multiple problems, but within a limited area which allows them to interact with a single "Invisible College." The fox, in contrast, has a large problem set which brings him into contact (and therefore competition) with more than one established research network.

While the purpose of the paper is primarily to explicate new concepts which usefully describe the internal structure of problem sets and to trace the relationships between structure, formal position, and access to collaborators in the three careers described, some speculation will also be provided, based on the interaction among these three researchers, on the general social consequences of being a fox.

CROSS, Stephen J.
Johns Hopkins University

"Discipline and Disciplines: Social Interests and Professional Strategies in American Social Science, 1918-1940"

This paper examines issues in the development of the social sciences as instruments of social engineering in America between the world wars. Rather than investigating this problem from the perspective of techniques and their applications, however, the paper argues that the social sciences were fashioned--in the very course of their establishment as academic research disciplines--as embodiments of social and professional interests in social control. The paper investigates the embodiment of logics of managerial practice both at the level of the research enterprise and at the level of social and behavioral theory. The interaction of class ideologies and professional strategies is examined at each level. The paper focuses upon patronage by Rockefeller philanthropies and the emergence of a professional establishment in the social sciences after World War I.

DOHERTY, Michael
Department of Psychology
Bowling Green State University

"Toward a Psychological Demarcation Rule"

Philosophers have shed much light on the distinction between science and non-science. Current conceptions of the nature of science have been deeply influenced by Popper's demarcation rule; for a hypothesis to be scientific it must be in principle falsifiable by empirical observation.

Considering demarcation from a psychological standpoint, we see the falsifiability criterion as one important aspect of the scientific approach to knowledge—scientists are much more likely to be aware of the tentativity of knowledge than are non-scientists. We argue that a parallel awareness, that of fallibility in the observations, is at least as important a characteristic of the scientific attitude. The argument leads naturally to a programmatic conception of research in the psychology of science.

DOUGLASS, Carl D., John C. James, Donald T. Disque, and Janet J. Bartch
Division of Research Grants
National Institutes of Health

"NIH Support of Research Investigations by the 'Phage Group', Forerunner of Molecular Biology"

This study was initiated at the suggestion of the Associate Director of Program Planning and Evaluation, Office of the Director, National Institutes of Health. The purpose is to delineate and analyze the support, provided by NIH, of a defined group of scientific investigators, the "Phage Group," who recognized the value of bacteriophage as an experimental material for elucidating the physical basis of biological reproduction, the mechanism for transmission of genetic information. Phage work and the resultant findings were a major source for the development of molecular biology as a scientific discipline. The present report shows that NIH support of Phage Group investigators contributed significantly to the overall research effort to understand the mechanism of heredity.

DOWLING, John
Mansfield State College

"Influence or Impotence? Scientists and Nuclear Weapons Policy"

Scientists were in on the creation of nuclear weapons and still play a major role in weapons research and development. Today they have an important role in nuclear weapons policies. While scientists are on all sides of the fence on nuclear weapons issues, the fence is definitely slanted toward the current Reagan administration's policies. In this paper I examine the influence of individual scientists both inside and outside the defense establishment, as well as the effects that such groups as the Federation of American Scientists, the Union of Concerned Scientists, Physicians for Social Responsibility, and the Livermore and Los Alamos Weapons Laboratories have on nuclear weapons policy. This examination centers on the nuclear freeze, but also involves the comprehensive test ban treaty and President Reagan's latest star wars scenario. Finally, I explore the role of individual scientists, typified by Edward Teller and Hans Bethe, on nuclear weapons policy.

EINHORN, Hillel J., and Robin M. Hogarth
 Graduate School of Business
 University of Chicago

"A Theory of Diagnostic Inference: Judging Causality"

Diagnostic inference is concerned with determining the causal process that produced a set of outcomes/results/symptoms. A model of causal reasoning within diagnosis is presented that attempts to answer the following questions: (1) How, and how much, do alternative explanations reduce the causal strength of a hypothesis?; (2) What factors determine the plausibility of a causal explanation?; (3) How does context affect the causal "relevance" of an explanation? We first propose that people use a sequential anchor-and-adjust strategy in discounting an explanation by alternatives. The amount of discounting depends on three factors: the plausibility of alternatives, the initial strength of the hypothesis, and a parameter reflecting the weight given to disconfirmatory evidence. It is then shown that the strength of a causal explanation is highly dependent on an implicit causal background (as in figure/ground relations), and on probabilistic factors called cues-to-causality. The cues considered are temporal order, contiguity, covariation, and similarity of cause and effect. A model for weighting and combining the cues is shown to account for much research in a wide range of fields. The three components of the theory are then tested in a series of experiments and the results discussed.

FISHER, Donald
 Department of Social and Educational Studies
 University of British Columbia

"The Development of the Social Science Research Council: Integration as Science"

The paper describes and analyzes the origins and development of the United States Social Science Research Council (SSRC) during the 1920s and 1930s. This organization was at the centre of Rockefeller philanthropy's plan to transform the group of separate disciplines characterized as social into an integrated social science. The analysis of the relation between Rockefeller philanthropy and the SSRC shows clearly that substantial control was exercised at all levels of policy, organization and program. Yet an assessment of the SSRC's impact on the social sciences in North America shows that while the movement toward "science" did occur, especially in statistics and economics, there was no comparable move toward integration. It is concluded that the structures of institution, discipline and career all pressed toward specialization. These structures were firmly based in the long term social forces of an increasing division of labour and professionalization. The attempt at the level of ideology to combat these forces by breaking down the barriers between disciplines was overwhelmed. It is argued that Rockefeller philanthropy also moved away from the objective partly because of the implicit danger to the social system of policies that were based upon integrated research. The paper concludes by describing how the SSRC helped fix a new definition of the social scientist as technician who could provide the expertise and the knowledge needed to make the changed relation between the public and private sectors work. Rockefeller philanthropy used the SSRC to push social scientists irrevocably away from fundamental development of social science toward applied research.

GIERYN, Thomas F.
 Department of Sociology
 Indiana University

"Political Ecology of the National Science Foundation: Do the Social Sciences Belong?"

This paper illustrates a distinctive orientation in the sociology of science: political ecology. It is "political" in its focus on conflicts over culture authority, political power, material resources and professional prerequisites; it is "ecological" in assuming that processes of "boundary-work" (i.e. mapping, niche-formation, turf-defending) are fundamental to these conflicts. How do scientists decide who is doing "science" and who is not? These questions were much debated in the nascent National Science Foundation, where one hot issue was the desirability of including social science within NSF funding programs. The debate hinged on how the boundaries of "science" were drawn: depending upon one's point of view, "science" was defined in ways that obviously included or excluded social science. This paper reports on preliminary analyses of congressional hearings and NSF archives, and identifies patterns in boundary-work by natural scientists, social scientists, politicians and bureaucrats.

GIERYN, Thomas F.
 Indiana University
 TATAREWICZ, Joseph N.
 National Air and Space Museum

"Federal Funding and Planetary Astronomy, 1950-1975"

Planetary astronomy offers an excellent opportunity to examine the effects of Federal funding on scientific careers and specialties. Study of the moon and planets, once a major specialty in astronomy, became a minority specialty early in this century, having been eclipsed by stellar and galactic research programs. The space program contributed to the renewal of planetary studies as plans were being made for exploratory voyages. NASA, NSF, and DOD to varying degrees made available relatively large sums of money for new instruments, facilities, observations, theoretical work, and predoctoral training. Evidence that links the investment in these programs with renewed interest in the planets by astronomers and also by geophysicists has been largely anecdotal. We attempt to measure in a more objective way this renewed activity in studies of the moon and planets, and to relate this to the infusion of money. A data-set already existed containing biographical data (education, source and field of degree, etc.) and bibliographic data (complete subject-coded lists of publications) for 2308 American astronomers and astrophysicists professionally active between 1950 and 1975. We augmented this data with selected information on funding received from NASA and NSF for planetary research. The analysis reported here concentrates on the changing composition of the community of scientists who published on planetary topics before and during the NASA program, with special attention to career patterns in funding data. Future analysis will make use of the funding data collected on the level of institutions, problem areas, and individuals.

GILLESPIE, Richard
University of Pennsylvania

"The Psychopathology of Industrial Life: Elton Mayo and the Prehistory of the Hawthorne Experiments"

This paper traces the evolution of Elton Mayo's theories of industrial relations and worker productivity prior to his most famous work as interpreter of the Hawthorne Experiments and founder of the human relations school of industrial management in the 1930s and 1940s. A study of his research in Philadelphia industry in the mid-1920s shows that Mayo was attempting to apply psychopathological theories to worker behavior. Workers who held radical political views or were militant unionists were held to be mentally disturbed and their political views were dismissed as irrational. His blending of psychiatry, social psychology, and physiology into a theory of industrial relations echoed and underscored the ideology of the more liberal leaders of industrial corporations, and his research attracted the support of the Rockefeller philanthropies and Harvard Business School. This analysis of Mayo's early industrial experiments demonstrates the necessity for a reinterpretation of the history of the Hawthorne Experiments. For while Mayo was concerned to reform industry by persuading managers to take into account the human needs of their workers, his science of human behavior provided a technology of control, with power resting firmly in the hands of the employer.

GILMORE, C. Steward
Department of History
Wesleyan University

"Federal Funding and Knowledge Growth in Ionospheric Physics, 1945-1981"

Ionospheric physics studies the properties of certain parts of the earth's atmosphere. The field formed an identifiable focus within geophysics for about 30-35 years, up until about 1960, after which it "metamorphosed" into a much larger and more complex area, in the midst of which classical ionospheric studies still persist. The paper focuses on the period from the end of the second World War until the metamorphosis occurred, tracing the growth of the field and the shift in the pattern of funding for it, describing the origins of the developments which resulted in the transformation, and summarizing the views of major figures in the area on their interactions with the funding system. The paper uses a wide range of methods to study the field: personal participation, an extensive knowledge of the literature, interviews, surveys, and bibliometrics.

GILMORE, C. Steward
Department of History
Wesleyan University

"Geographic, Intellectual, and Social Mobility in the History of a Scientific Community"

A sample of 361 individuals working in 25 countries in the period 1932-72 each did research in Ionospheric Physics for periods lasting at least one year. A time history of each individual's ionospheric work history, in

terms of problem area, and institutional address will be utilized to present features of career change due to intra-, inter-, and extra-disciplinary factors.

HULL, David L.
Department of Philosophy
University of Wisconsin

"Thirty-One Years of Systematic Zoology"

The number and percentage of pages devoted to various topics in Systematic Zoology are traced from the inception of the journal in 1952 to the present. The categories used include papers that (a) are largely descriptive, (b) argue for or against a particular taxonomic philosophy, (c) apply the methods of a particular taxonomic philosophy, (d) are primarily quantitative, (e) discusses nomenclatural problems, and are either theoretical or applied papers in (f) evolutionary theory, (g) phylogeny reconstruction, or (h) biogeography. These figures are then mapped onto changes in editorship of the journal to see what effects such changes have on the sorts of papers published. The changes in editor of primary concern are the move from the American Museum of Natural History to the University of Kansas, the home of phenetic taxonomy, in 1963 and the return to the American Museum ten years later in 1973 when it became the center for cladistic analysis in the United States. Can claims of bias be substantiated just by looking at the effects of the refereeing process?

KERN, Leslie
Department of Psychiatry
The Ohio State University

"The Role of Data Error in Inducing Confirmatory Inference Strategies in Scientific Hypothesis Testing"

Recent studies of scientific inference (e.g., Mynatt et al., 1977) suggest that individuals display a pervasive cognitive tendency to seek confirmation, or verification, of any hypothesis they are asked to test. The present investigation suggests that certain parameters of the research problem situation can serve to enhance, or to mitigate, the likelihood that a confirmatory inference strategy will be used. Doctoral candidates in physics, chemistry, and biology were asked to test hypotheses about events occurring within a dynamic, computer controlled environment using equipment which introduced one of two types of error into the data returned to subjects. Subjects whose data contained "stochastic" error received information that was totally random, totally unrelated to the true condition of the phenomenon under investigation, on a certain proportion of their experimental trials. Subjects whose data contained "measurement" error received information that reflected a true measure of the phenomenon, plus some amount of random variance, on each of their experimental trials. While the introduction of stochastic error induced a strong confirmatory bias, the introduction of measurement error had no such effect. A group of control subjects, whose data contained neither form of error did not manifest any tendency toward confirmatory bias. Data on a fourth group of subjects, whose equipment introduced both forms of error simultaneously, suggested that the effects of measurement and stochastic error on scientific inference are non-interactive.

KLAYMAN, Joshua
 Graduate School of Business
 University of Chicago

"Processes of Discovery and Learning in a Probabilistic Environment"

There is some controversy these days over the extent to which the laws of nature are fundamentally deterministic. For human beings, though, there is no question that the world is full of unknown and uncontrolled variables, and thus nearly all learning and discovery takes place in a probabilistic environment. What are the cognitive processes through which people learn probabilistic relationships in the environment? Many psychological studies have found this type of learning to be extraordinarily difficult. However, most of the studies have characteristics that lead subjects to search for a single, deterministically "correct" solution. In contrast, natural learning situations tend to be oriented toward gradual improvement in understanding and predictive validity. It is hypothesized that in this latter case, people improve their prediction models chiefly by discovering and incorporating new valid cues, a process that has not received much study. A new study is presented in which subjects attempted to predict behaviors in a simulated environment which included a large variety of potential predictive cues, varying in salience and validity.

During "learning" periods, twelve subjects were given the opportunity to freely design and test environments. Predictive accuracy was assessed and interspersed test periods. Subjects showed significant improvement through seven to eight hours of test experience. Half of the subjects were also asked to provide verbal and written accounts of their thinking during the procedure. These data support the hypothesis that addition of new cues is the primary mechanism of learning. An additional group of six subjects completed the same procedures, except that they were not permitted to design freely their experiences. These "observer" subjects did not do as well as their experimenter counterparts, but did also show improvement. The results of this study raise many interesting questions about how learners go about the process of discovery and testing, and how they can be helped to do so.

LAUDAN, Rachel
 Science Studies Center
 Virginia Tech

"Cognitive Change and Social Structure in Science and Technology"

Somewhat surprisingly, recent models of scientific change, which have been applied to almost every kind of human activity, even those that would scarcely seem amenable to such treatment, have rarely been extended to technology with its prima facie close connections to science. In part this is due to the struggles of students of technology to show its independence from science, but another important reason is the seeming complexity and diffuseness of the technological community when compared to the scientific community. Distinctions within technological community are in order, together with an analysis of the contribution of each sub-group to the cognitive structure of technology.

LOWE, Charles Upton
National Institutes of Health

"Measuring the Growth of Knowledge in the Biomedical Sciences"

This research develops indicators of scientific activity by tracking the appearance, growth, and eventual senescence of the invisible colleges of biomedical research. The invisible college is the unit of study for it identifies independent scientific domains while concomitantly recognizing the social element in research activity. The approach is built upon the use of co-citation analysis for an empirical identification of the domains of science (co-citation clusters) and comparison of these clusters with the domains obtained by evaluation of NIH research management tapes. The experience of NIH in manipulating discrete elements of biomedical research for purposes of evaluation and management is a system built upon logic, and we expect to compare the results from analyses of these data with findings using co-citation analysis which organizes biomedical science using the empiricism of published documents. The product of the research will be graphic methodology to monitor the life cycle of the invisible colleges in biomedical research, thereby formulating a valuable tool for science decisionmakers, those responsible for resource allocations, and for those having responsibility for documenting science activity through use of indicators or other modalities of measurement.

LUBEK, Ian
Department of Psychology
University of Guelph

"Social Psychology Applied to the History of Science: Publish or Perish in 'Fin de Siècle' France--the case of Augustin Hamon and George Bernard Shaw"

The "social psychology of science" approach is useful in examining the evolution and dissemination of ideas both in social studies of contemporary science and in studies of the history of science. The social psychologist (microsociologist, ethogenist, etc.) may focus on a variety of social processes and relationships that affect scientific behaviour (in laboratories) or outputs (in journals). The present paper focuses on asymmetric power relations between authors and editors as a determinant of scientific outcome, and stress the negative aspects of gate-keeping and marginalization of an innovative set of ideas. As a case history, let us focus on the work of Augustin Hamon, one of a series of "lost social psychologists" whose ideas, formulated during the period 1890-1905, were relatively unknown then, as now. Because he scrupulously kept all his correspondence during this period, it has been possible to document the increasing difficulty he had in finding outlets for his "scientific" approach to social psychology in the professional journals and with the publishing houses of his day. After a series of encounters with mainstream "gate-keepers," Hamon temporarily turned the asymmetric power tables by founding his own journal (*Humanité Nouvelle*, 1897-1903) and editing his own sociology book series. But his own openness to marginal and innovative viewpoints in the "sociological sciences" embroiled him in a series of law-suits with his publishers over such issues as the censorship of content of articles, etc. These legal proceedings

spelled the end of his career as an anarchist "social psychologist," despite help from other social thinkers of the day, including playwright George Bernard Shaw. Shaw offered the indebted Hamon a career-change opportunity and, beginning in 1904, Hamon and his wife became the translator into French, of the entire literary work of Shaw, and helped pave the way, through their writings, for the entry of Shavian ideas into France. Hamon's career and social psychological ideas, blocked by strong gate-keeping from the social science community, did not have great longevity; his second career as a literary figure (1904-1945) was actively aided by the success and power of Shaw in literary circles. Hamon had thus abandoned the "margins of science" for an entrance into the "wings of the theatre."

LUGG, Andrew
Department of Philology
University of Ottawa

"Well-Reasoned Belief and Its Social Explanation"

In a discussion covering Marx, Mannheim, Forman, and Brown, the major argument is that the kind of explanations given by Marx and Mannheim were compatible with rational belief, and that the cases of Forman and Brown can be viewed in the manner of Marx and Mannheim. Hence the accounts of the latter fall in the tradition of Marx and Mannheim, but philosophers should not be disturbed by this. Additional arguments previously made by the author in other contexts are also brought to bear on the problem.

MYERS, Greg
Department of English
University of Texas at Austin

"The Uses and Limits of Rhetoric: Two Case Studies of the Composing Processes of Biologists"

Studies of successive drafts of articles and research proposals by a zoologist and a botanist show the researchers' sense of their relation to other members of the research community, especially: (1) a distinction the writers draw between rhetoric (which can be adapted to the audience) and content (which cannot), (2) an awareness of the different contexts in writing an article and writing a proposal, (3) a strategy for incorporating criticisms and comments from inside and outside the research group, and (4) a variation in freedom between fields, between the biologists' relative freedom to adapt their presentation to their audience and the relatively restricted choices allowed for physicists.

In my studies I consider the writing of two professors heading large research groups at the University of Texas. I focus especially on a theoretical article and a major proposal by each researcher, comparing the successive drafts of each piece. The revisions include changes of titles, organization, sentence sequence, words, added and deleted information, references, and illustrations, and changes in the appearance of the text. The changes affect the tone, the scope of claims made, the line of argument, and the relation to previous work, as well as improving clarity and readability. Some changes seem to respond to specific written comments by readers; others are explained by the writers themselves in interviews. Besides showing what parts may change, I show what parts may not change: the core of contents and findings that the writers see as the content of the article or the proposed research.

MCCAIN, Katherine W.
School of Library and Information Science
Drexel University

"Mapping the Dynamic Structure of Macroeconomics"

Author cocitation analysis is a relatively new approach to the study of the intellectual structure of subject specialties via their associated literatures (see e.g. H.D. White and B.C. Griffith, 1981, *JASIS* 32(3):163-171; 1982 *J. Documentation* 38(4):255-272). Authors, rather than individual documents, are the units of analysis. "Distances" between authors are derived from counts of the cocitations of author pairs. Factor analysis, multidimensional scaling and clustering programs are used to (1) identify important structural components and organizational dimensions and (2) create maps of authors in "intellectual space."

A previous paper (K.W. McCain, 1983, *Scientometrics* 5(5):277-289) reported the analysis, using author cocitations, of the literature of Macroeconomics over the period 1972-1981. Macroeconomics was shown to be organized primarily into "schools of thought" focussing on basic questions of theory formation and interpretation. Current work-in-progress involves the use of author cocitation analysis to study the dynamic structure of the literature of Macroeconomics and two other subject specialties. This paper describes changes in the intellectual structure of Macroeconomics over the periods 1972-1977 and 1978-1982/3. These changes include (1) the emergence of two new theory groups--Rational Expectations Theory and Implicit Contracts Theory--in the late 1970's; (2) the decline or increasing isolation of existing groups; (3) the migration of prominent scholars from older to newer theory groups.

PINCH, T. J.
Department of Sociology
University of York

"A Unified Approach Towards the Sociology of Science and Technology-The Sociology and Facts"

It is argued that a unified approach to the sociology of science and the sociology of technology will bring benefits to each field. Such an approach is developed in this paper and the accompanying one (by Wiebe Bijker). The basic principles of the unified approach are outlined and in this paper illustrated by reference to examples drawn from science. In particular the concepts of "interpretative flexibility" and "rhetorical closure mechanism" are exemplified by drawing upon a case study of a debate over the shape of the Sun (solar oblateness) and a debate over the existence of solar oscillations. A discussion of possible differences between these examples and those encountered in technology (see accompanying paper by Bijker) is included.

PRESSMAN, Jack D.
University of Pennsylvania

"The Salvaged Soul: The Use of Psychosurgery to Return the Nation's Mentally Ill to Active Employment, 1942-1955"

Psychosurgical operations promised to reclaim productive citizens from the nation's institutionalized mentally ill. Under the conditions of severe overcrowding in American mental hospitals and the resulting strain on government budgets in this period, this promise inspired the enthusiasm for lobotomy operations. Contemporaries did realize that lobotomy operations irrevocably deprived patients of some fundamental aspects of their personalities. Advocates of these operations, however, argued that because many patients were able to return to some form of active employment the social and economic benefits outweighed the personal risks. The potential conflict between personal liberties and social needs was side-stepped through the assumption that the best criterion available for judging mental well-being was the ability to maintain a paying job.

RESTIVO, Sal
Science and Technology Studies Division
Rensselaer Polytechnic Institute

"The End of Epistemology"

In this paper, I explore encounters between sociology and epistemology, and discuss the relevance of these encounters for a sociological theory of knowledge, cognition, and representation. Sociology is involved in (a) the adjectival strategy used to transform traditional or philosophical epistemology into comparative (Fleck), evolutionary (Campbell), and emancipatory (Restivo) epistemologies; (b) the strategy of "critical liquidation" (used by Sohn-Rethel to get from Kant to Marx); and (c) the "philosophy without mirrors" strategy used by Richard Rorty to arrange the death of epistemology). In considering the relationships between the results of these encounters and the development of theory in the sociology of knowledge, I draw illustrative materials from recent studies in the sociology of scientific practice, and the comparative sociology of mathematics.

ROUSE, Joe
Department of Philosophy
Wesleyan University

"Science as Social Practice: A Philosophical Interpretation"

Philosophies of science have traditionally focused their attention upon scientific discourse, and have taken for granted the claim that this discourse can be decontextualized and studied apart from the research practices from which it arose. Even epistemological holists usually claim only that the interpretation and justification of sentences must take place against a background of other sentences. In my paper, I will argue that such views do not take scientific

practice sufficiently seriously. I will attempt to provide a general account of practices and their role in the interpretation, justification, and partial decontextualization of scientific discourse, and to show that this challenges some traditional conceptions of the relation between philosophy and sociology of science.

SCHOPMAN, Joop
Department of Philosophy of Science

"Semiconductor Development in the Netherlands - 1955; A Study of Its Determinants"

In the period till 1955 development of semiconductors took only place within the N.V. Philips' Gloeilampenfabrieken, i.e. in an industrial context. Their work in this sector of solid state physics can be divided into three relatively independent contributions: the pre-germanium barrier layer semiconductors, the semiconducting oxides and finally the germanium-silicon work.

The scientific/technological characteristics of these three are very different. In the first case, it is a technological trial and error, which nevertheless resulted in several industrially useful applications. The second one was a beautiful specimen of "molecular engineering," which could be realized thanks to theoretical insight. It initiated a new field of research and still is industrially important. The third contribution was initially a trying to cope with the technical performances realized by others; later original work could be done. Several factors which influenced these development could be identified. A stringent boundary condition was the industrial context. The scientific efforts were not only determined by the character of Philips' industrial products, but also by the possibilities created by accidental and structural market constraints and by the patent position of the competitors. Moreover, in addition to the surplus value of the already mentioned theoretical insight, "sociological" idiosyncracies of the disciplines involved and the management were important for the realized contributions. The same must be said of so called "scientific intuition," of the internal dynamics of research and production, and of purely accidental facts.

SMALL, Henry and GREENLEE, Edwin
Institute for Scientific Information

"Collagen Research in the 1970's"

Collagen is the most abundant protein in the human body and is a major constituent of connective tissues, giving both form and strength to organs. This paper examines the development of collagen research between 1970 and 1980 using co-citation clustering. Presenting the changing intellectual foci of the field as a series of "maps," the paper traces the institutional locations and sources of support for the authors of the papers which formed the key intellectual resources moving the field forward. The maps show several developments, including a shift in institutions from a government laboratory to university and industrial labs, and a major refocusing of intellectual interests. Preliminary comparison with NIH funding data for collagen research as a whole suggests that the series of maps captures only the core of an expanding area, much of which may involve applications of the developments represented in the clusters.

SHRUM, Wesley
 Department of Sociology
 Louisiana State University

"Participation, Elite Status, and the Evaluation of Technical Subfields"

Sociologists of science and technology have long been concerned with determinants of the innovativeness of individual researchers, while the innovativeness of entire fields has been relatively neglected. Yet evaluations of the relative contributions of technical fields play an important role in many accounts of technical development. First, they are held to be important influences on problem choice and hence the rate and direction of scientific development. Second, the differential evaluation of fields affects the funding and resources granted by organizational and program administrators. Finally, participation in marginal research areas requires high levels of commitment which may be enhanced through positive evaluations.

This study investigates the correlates of evaluations of technical subfields using a national survey of 297 individuals in nuclear waste and photovoltaic research. Nineteen distinct subfields are examined with respect to the following issues: (1) Is there a positive bias associated with working in a particular subfield? (2) Do elite researchers tend to be more "objective" in their evaluations of subfields? (3) Are biases more pronounced for those working in "marginal" subfields? (4) Does centrality in the social structure of a wider field reduce bias in the evaluation of subfields?

The findings are important for theories which imply the scientific elite must be more judicious in their evaluation of fields for the optimal development of the institution and for policy makers who utilize assessments made by researchers in directing resource flows.

TRAWEEK, Sharon
 Program in Anthropology and Archeology and Program in Science, Technology and Society
 Massachusetts Institute of Technology

"Laboratory Practice in High Energy Physics"

During five years of anthropological fieldwork at high energy physics laboratories in the United States, Europe, and Japan I have studied the place of research equipment in laboratory practice. The planning, selection, development, construction, operation, maintenance, and modification of particle accelerators and their diverse attendant detectors is central to the practice of experimental high energy physics. Participation in this process is participation in the scientific debates of the field. In this talk I report on the place of accelerators and detectors in the construction of physicists, data, and scientific debates.

TURNER, Stephen
 Department of Sociology
 University of South Florida

"Patronage and Trust in Science"

This paper considers the development of patron-science relations in the United States in four contexts: 19th century science, especially in the federal government and the observatory building movement; agricultural research and the development of the "iron triangle"; the National Science Foundation; and the National Institutes of Health. Each of these patronage relations has been governed by distinctive sets of mutual expectations and norms, or more generally, forms of trust. These forms of trust have organizational implications, and of course bear on the content and quantities of science funding. The forms of trust that are central to the patronage of NSF and NIH are perhaps prone to breakdown in certain ways, and this suggests that alternative structures might be desirable.

VOSS, James F.
Learning Research and Development Center
University of Pittsburgh

"Social Science Problem Solving: Similarities and Differences to Problem Solving in Other Disciplines"

We recently have been investigating the problem solving of experts, novices, and intermediates in the social sciences or, more specifically, in political science. The work has led to postulating a general problem solving control structure as well as an argument structure, the latter being especially employed by the more expert solvers. This research is discussed in relation to the question of how problem solving varies with the nature and development of different academic disciplines.

WEINGART, Peter
Department of Sociology
University of Bielefeld,

"The Structure of Technological Change: Reflections on a Sociological Analysis of Technology"

It is obvious that a shift in analysis which conceives technology as knowledge and focusses on invention as an activity of knowledge production can unify the hitherto diverse studies of technology. The integrating analytical framework is that of a systems theory of social processes as communication in which the production of technical knowledge is related to different types of institutionalized orientations. Parallels between scientific and technological knowledge are far from exact, because the latter is a much less narrowly delimited enterprise, and because technology does not get into crises by the elaboration of concepts.

WESTRUM, Ron
Department of Sociology
Eastern Michigan University

"What Happened to the Old Sociology of Technology?"

By the 1980's the sociology of technology, except for a few highly specialized fields (such as studies of technology in organizations) seems largely non-existent. Yet this area was once a thriving field of sociological endeavor, whose best known theorist was William F. Ogburn. There were, however, many others who worked with and contemporaneously with Ogburn and their work is important

and interesting. This paper considers what they tried to establish and why the field, as an active area of inquiry, largely disappeared.

WILKES, John
Social Science and Policy Studies
Worcester Polytechnic Institute

"NASA's Great Computer Debate or Technology and the Politics of Space Science"

Debate over a new computer system at NASA is shown to reflect deeper divisions within the agency as to its ultimate mission. Should it be primarily a data-gathering entity for the scientific community, and emphasis its operational reliability or instead specialize in the research and development of new hardware, leaving the routine functions to others. Yet the major issues are not directly confronted, but rather the conflicting interests are projected into a debate over the kind of computer system necessary for NASA.

WOODHOUSE, Edward J.
Rensselaer Polytechnic Institute

"The Speed of Technological Change: A Policy Analysis"

Except for periodic concern about a U.S. "lag" in industrial innovation, both public and professional discourse on technology has focused primarily on its direction rather than its rate. This certainly is true when particular impacts are at issue, such as the effect of microelectronics on employment. But even discussions of decision-making procedures (science court, technology assessment) typically concern what uses of technology are desirable. This paper explores the possibility that the speed of technological change may be a separate and important topic, one that deserves more systematic attention than is accorded by brief exclamations of awe or despair about how fast (or slow) things are changing.

Section I examines whether there is compelling reason to be interested in the pace of technological development, entirely apart from considerations about direction. Section II analyzes the economic, political, psychological, and cultural forces that jointly determine the rate of technological change, delineating a half dozen specific obstacles to intelligent modulation of pace. Section III considers policy options that may be available for slowing down (or accelerating) the speed of change, including tax policies, premarket approval mechanisms, auction mechanisms, and changes in corporate liability. With significant exceptions in particular areas of applied technology, overall the analysis suggests more reason to be concerned that the present pace of technological change is excessive than that it is inadequate.

ZIMAN, John
Department of Social and Economic Studies
Imperial College of Science and Technology

"Problem Choice in Quasi-Academic Science"

Success or failure as a "quasi-academic scientists" depends as much on the choice of questions for investigation as on skillful performance in the research

itself. "Problem choice" is thus a major determinant in the careers of individual scientists and scientific collectivities. Through the grant proposal mechanism, it has also become the occasion for the injection of public funds and policies into quasi-academic research.

The term itself needs explication, since it suggests more rationality in the research process than is usual or desirable. Does it allow sufficiently for exploratory or serendipitous investigations? Does the formulation and assessment of grant proposals exaggerate the "scientific" rationality of research plans, and thus overemphasize "normal", "puzzle-solving" science? In any case, data concerning actual research projects that have been successfully undertaken gives inadequate information on those options which were not chosen, or which proved abortive.

Numerous diverse considerations enter into individual research plans and policies. Notional estimates of the "importance" of a scientific question have to be balanced against the supposed "difficulty" of getting an answer to it - including the risk that nothing useful will be gained at all by a particular investigation. The safest policy often seems to be to "persist" within a narrow specialty, but this has the hidden danger that the specialty itself may become obsolete as a result of scientific progress. Individuals not only vary greatly in their temperamental willingness to take such risks they may be strongly influenced by their immediate career situation, by employment prospects, or other characteristics of the administrative context of their work.

ANNOUNCEMENTS: FEDERAL AGENCIES**DEPARTMENT OF HEALTH & HUMAN SERVICES**

The Office of the Assistant Secretary for Planning and Evaluation (ASPE) is accepting applications for its 1984 Policy Research Associates Program. This program recruits five professional staff members annually via a competitive nomination and selection process for a two-year temporary appointment.

The ASPE serves as the principal advisory unit to the Secretary of the Department and has ongoing responsibility for developing the Department's legislative program, participating in the annual budget process, and overseeing Department-wide research and evaluation activities. There are five separate offices within ASPE: Program Systems; Health Policy; Social Services Policy; Income Security Policy; and Evaluation and Technical Analysis.

Policy Research Associates (PRAs) will work on projects which provide exposure to all facets of the policy process. For example, they will: research and analyze aspects of policy questions; prepare and assist in the preparation of background and decision memoranda; review and analyze legislation, budget, evaluation and research proposals; meet with senior policymakers in the Department and represent the Assistant Secretary in discussions; and answer Department correspondence and inquiries about various HHS programs.

The Policy Research Program has two complementary goals:

- To offer professionals early in their career an opportunity for broad experience in an executive agency of the Federal government; and
- To secure quality professional staff who will bring fresh perspectives and new ideas to the policy development process.

Candidates must have a minimum of either a graduate degree or two years of work experience in a relevant field. The salary for the PRA Program is competitive with other professional entry-level Federal positions and will fall within the GS-9 or GS-11 range (\$20,000 - \$25,000). Health benefits will be available.

We are interested in candidates who are at the beginning of their careers and who believe Federal government experience will be of value in fulfilling their career objectives. Candidates should be able to write cogently, handle multiple projects on short timetables, and analyze, frame and articulate public policy issues. A basic understanding of the public policy process, particularly in the area of health and human services is desirable.

Complete application packages must be postmarked by March 1, 1984. Application materials may be obtained by writing to:

Policy Research Associates Program
Room 457F - H.H. Humphrey Building
200 Independence Avenue, S. W.
Washington, D.C. 20201

ANNOUNCEMENTS: RECENT CONFERENCES**SOCIETY FOR PHILOSOPHY AND TECHNOLOGY**

The second biennial meeting of the Society took place at Polytechnic Institute of New York in Brooklyn in September 1983. Over thirty speakers addressed the group on the general theme of information technology and computers. With about half of the participants from Germany, France and the Netherlands, and the other half from the United States and Canada, the conference had a distinctively international character. The next meeting is being planned for somewhere in Europe in the fall of 1985. Anyone interested in obtaining more information or becoming a member of the Society should contact its Secretary, Stanley Carpenter, Social Science, Georgia Institute of Technology, Atlanta, Georgia, 30332.

Elected officers of the Society include Alex C. Michalos, President; Kristin Shrader-Frechette, Vice President; Edmund Byrne, Treasurer; Albert Borgman, Don Ihde, Joseph Pitt, Friedrich Rapp and Langdon Winner, Members of the Board of Directors; Paul Durbin, Editor of Research in Philosophy and Technology; and Carl Mitcham, Bibliography Editor.

ANNOUNCEMENTS: FUTURE COLLOQUIA, SYMPOSIA, CONFERENCES**NINTH ANNUAL AAAS COLLOQUIUM ON R&D**

Washington, D.C.: "R&D in FY 1985: Budgets, Policies, Outlooks" will be the theme for the Ninth Annual Colloquium on R&D and Public Policy of the American Association for the Advancement of Science (AAAS). The Colloquium will be held 29-30 March 1984 at the Shoreham Hotel in Washington, D.C.

The Colloquium will provide a timely view of research and development in the FY 1985 budget, and a look at trends in federal and industrial R&D funding. Speakers will discuss the roles of both the public and the private sectors as they relate to R&D, and will examine various aspects of the university-industry-government research partnership. Special sessions will focus on R&D budgets of major agencies, including the Defense Department, National Science Foundation, National Aeronautics and Space Administration, National Institutes of Health, and Department of Energy. Questions of implementation -- what happens to R&D dollars after they are allocated -- also will be addressed.

Leaders from the White House, key federal agencies, Congress, industry, and the scientific and engineering communities will participate in the Colloquium.

AAAS Report IX: Research and Development, FY 1985, prepared by AAAS and 15 other scientific, engineering, and educational organizations will be distributed to Colloquium registrants and will provide background material for the meeting. Registrants also will receive a summary of the Colloquium proceedings after the meeting, and a report on Congressional action on the FY 1985 budget in the fall.

In keeping with AAAS policy, the meeting will be accessible to disabled participants.

For further information on the Ninth Annual Colloquium on R&D and Public Policy, write to the AAAS Office of Public Sector Programs, 1776 Massachusetts Avenue, N.W., Washington, D.C. 20036.

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PLANNING HISTORY GROUP

The Third Annual Luncheon of the Planning History Group will be held on Friday, April 6, 1984, at Noon in the Mediterranean Room of The Biltmore Hotel in Los Angeles. The luncheon is being held in conjunction with the meeting of the Organization of American Historians. Gerald D. Nash, University of New Mexico, will present a paper entitled "Planning for the Postwar City: The Urban West During World War II." Roger W. Lotchin, University of North Carolina-Chapel Hill, will chair the session. Tickets will be available as part of the pre-registration package for the OAH meeting or at the OAH Registration. As the number of tickets is limited, purchase through pre-registration is encouraged. For additional information, contact: Blaine A. Brownell, The Graduate School, University of Alabama at Birmingham, Birmingham, AL 35294, 205-934-7110, or Mark H. Rose, The Program in Science, Technology, and Society, Michigan Technological University, Houghton, MI 49931, 906-487-211.

ENGINEERING ETHICS SYMPOSIUM AT AAAS MEETINGS

A symposium on "Engineering Ethics: In Classroom, Job, and Profession" has been scheduled at the annual meeting of the American Association for the Advancement of Science in New York City. The session will take place on 27 May 1984, from 2:30 - 5:30 P.M. in the New York Hilton. The symposium will examine the responses of engineers, their associations, and the institutions that employ and educate them to newly visible social and personal demands for professional accountability and responsibility.

Taft Broome, Chairman of the Civil Engineering Department at Howard University, Director of the Large Space Structures Institute, and Vice Chairman of the Ethics Committee of the Association of American Engineering Societies, will speak about the societies' search for an elusive uniform code of ethics. Michael Baker, Executive Vice President and Research Director of the Education Fund for Individual Rights, will report on results from an investigation into ethical problems and mechanisms for resolving disputes for engineers in regulated industries; and Vivian Weil, Senior Research Associate, Center for the Study of Ethics in the Professions, Illinois Institute of Technology, will talk on the latest developments in engineering ethics curricula.

The panel of discussants includes Robert Anderson, PE, Manager of Corporate Technical Education at General Electric; Dennis Chamot, Assistant Director for the Department of Professional Employees, AFL-CIO; and Rachelle Hollander, Program Director, Ethics and Values in Science and Technology, U.S. National Science Foundation.

SOCIETY FOR THE HISTORY OF TECHNOLOGY
1984 ANNUAL MEETING

Call for Papers

The SHOT Program Committee calls for session and paper proposals for the 1984 Annual Meeting, which will take place at the Massachusetts Institute of Technology in Cambridge, Massachusetts, on 18-21 October.

We seek a diverse and well-balanced program, including sessions on technology in medieval as well as modern periods. We would like to encourage historians of technology to address topics which have received relatively little attention at recent meetings. Accordingly, we will particularly welcome contributions on non-Western technology, technology and the state, literary images of technology (e.g., Orwell's 1984), technologies of mass media (e.g., film, radio, and television), the history of technical education, and especially interdisciplinary proposals relating the history of technology to other specialties such as women's history, labor history, urban history, or new methodological approaches. In keeping with tradition, we plan to schedule both works-in-progress sessions and sessions sponsored by SHOT's special interest groups (Jovians, Pelicans, TEMSIG, TS & E, and WITH). We encourage these groups to submit proposals.

The deadline for proposals is 1 May 1984. Individual proposals must include a 150-word abstract of the paper and a one-page curriculum vita. Session proposals should include a statement of the general theme as well as an abstract and curriculum vita for each participant. Please address initial inquiries and proposals to: Jeffrey L. Sturchio, Department of Humanities, New Jersey Institute of Technology, Newark, NJ 07102. Telephone: (201) 645-5224.

Because the Committee receives more proposals than can possibly be accommodated in the limited program space available, we will only consider those proposals which are complete by the 1 May deadline. To broaden participation in SHOT meetings, we will follow the usual practice of giving preference, other things being equal, to individuals who have not made presentations in recent years.

1984 SHOT Program Committee

Susan Douglas (Hampshire College)

Stuart W. Leslie (Johns Hopkins University)

Jeffrey L. Sturchio (New Jersey Institute of Technology)

ANNOUNCEMENTS: LECTURE SERIES

THE ISRAEL COLLOQUIUM FOR THE HISTORY, PHILOSOPHY AND SOCIOLOGY OF SCIENCE: 1983-1984

- | | | |
|------------------------------------|---|--|
| *1. Tuesday, Nov. 8
Tel-Aviv | From Empiricism to
Judgment and Argument | MAURICE A. FINOCCHIARO, University of
Nevada, Las Vegas
Commentator: Asa Kasher, Tel-Aviv University |
| *2. Thursday, Nov. 24
Jerusalem | The Nature and Status of
Rational-Choice Explanations | JON ELSTER, Oslo University
Commentator: Marcelo Dascal
Tel-Aviv University |
| *3. Thursday, Dec. 1
Jerusalem | Social-Scientific Models of
World-Wide Processes | KARL DEUTSCH, International Institute for
Comparative Social Research in Berlin, and
Harvard University
Commentator: Yehoshafat Harkabi, The Hebrew
University |
| *4. Tuesday, Jan. 1
Tel-Aviv | De-Industrialisation and
Re-Industrialisation | PETER MATHIAS, All Souls College, Oxford
Commentator: Nahum Gross, The Hebrew University |
| 5. Tuesday, Feb. 14 | 1. Cognitive Limits to Science

2. Cognitive Science and
the Brain | GUNTHER S. STENT, University of California
Berkeley
SHIMON ULLMAN, The Weizmann Institute of
Science |
| 6. Thursday, March 1
Jerusalem | Limits of a Deductive
Construal of the Function of
Scientific Theories | C. G. HEMPEL, University of Pittsburgh
Commentator: Yemima Ben-Menahem
The Hebrew University |
| 7. Tuesday, March 13 | 1. Philosophy and Medicine
in 16th Century Italy
2. Medicine and Magic in
16-17th Centuries North Africa | CHARLES SCHMITT, The Warburg Institute,
University of London
RON BARKAI, Tel-Aviv University |
| 8. Thursday, April 5
Jerusalem | Realism Versus Anti-Realism:
What Is the Issue? | L. JONATHAN COHEN, The Queen's College,
Oxford
Commentator: Eddy Zemach, The Hebrew
University |

- | | | | |
|-----|-------------------------------|---|--|
| 9. | Tuesday, April 24
Tel-Aviv | The Hermeneutical Status of
the History of Science: The
The Views of Hélène Metzger | GAD FREUDENTHAL, CNRS, Paris
Commentator: Pierre Kerszberg,
Tel-Aviv University |
| 10. | Thursday, May 3
Jerusalem | Mathematics and Logic in
Late Medieval Science | JOHN MURDOCH, Harvard University
Commentator: Sabetai Unguru, Tel-Aviv
University |
| 11. | Tuesday, May 22
Tel-Aviv | On Cause, Principle and Unity | DAVID FINKELSTEIN, Georgia Institute of
Technology
Commentator: Yakir Aharonov, Tel-Aviv
University |
| 12. | Thursday, May 31
Jerusalem | Relativism | RICHARD RORTY, University of Virginia
Commentator: Menachem Brinker, The
Hebrew University |
| 13. | Thursday, June 7
Jerusalem | Patent Statistics as Indicators
of Innovative Activity | ZVI GRILLICHES, Harvard University
Commentator: Yoram Weiss, Tel-Aviv University |

*[These, of course, have occurred prior to publication of this issue - Ed.]

In Collaboration with Robert S. Cohen and Marx Wartofsky of the Bost Colloquium for the
Philosophy of Science. Selected papers will be published by Humanities Press.

Lectures begin at 6:30 p.m. (Gathering for coffee and light refreshment at 6:00 p.m.). Most Lectures are in English.
In Tel-Aviv: Gilman Building, Hall 449, Tel-Aviv University.
In Jerusalem: The Van Leer Jerusalem Foundation, Einstein Square.
For further information: Edna Margalit, the Colloquium Coordinator, P.O.B. 4070, Jerusalem.

VIRGINIA TECH'S CENTER FOR THE STUDY OF SCIENCE IN SOCIETY
LECTURE SERIES IN SCIENCE AND TECHNOLOGY STUDIES
1983-84

*5 October 1983

"Past and Present: Internal Difficulties of Evolutionary Theories"

Jacques Roger, History of Science, University of Paris

Department of History, University of Virginia (Visiting)

*2 November 1983

"Puritanism and Modern Science: Do We Know about the Subject more than in 1938?"

Joseph Ben-David, Sociology, University of Chicago and Hebrew University of Jerusalem

10 February 1984

"The Problem of Indiscernible Particles"

Bas van Fraassen, Philosophy, Princeton

4:00 p.m., campus location TBA

28 March 1984

Topic To Be Announced

Arthur Fine, Philosophy, Northwestern University

4:00 p.m., campus location TBA

9 May 1984

"Marriage, Family, and Published Productivity In Science: Some Myths and Some Evidence"

Harriet Zuckerman, Sociology, Columbia

4:00 p.m., campus location TBA

23 May 1984

"Red Star: An Early Bolshevik Science Utopia"

Loren Graham, History of Science, Program in Science, Technology and Society, MIT

4:00 p.m., campus location TBA

The public is cordially invited to attend.

For further information call or write:

Center for the Study of Science in Society
Virginia Tech
Blacksburg, VA 24061
703/961-7687

*[These, of course, have been presented prior to publication of this issue - Ed.]

THE CHARLES BABBAGE INSTITUTE

for the History of Information Processing

The Charles Babbage Institute for the History of Information Processing was founded in 1978 with the goal of conducting and promoting historical research in the technical and socio-economic aspects of information processing. Located on the University of Minnesota campus, with the support of the information processing industry, the Institute maintains an archival center, serves as a clearinghouse for historical information, and sponsors other scholarly activities. In an effort to increase the number of qualified scholars in the field, the Institute is sponsoring a Predoctoral Fellowship and a Professional Internship.

PREDOCTORAL FELLOWSHIP 1984-85

The Charles Babbage Institute for the History of Information Processing is accepting applications for a Graduate Fellowship to be awarded for the 1984-85 academic year to a graduate student whose dissertation will address some aspect of the history of computers and information processing. Thesis topics may be chosen from, but are not limited to, the infrastructure of the information processing industry, and specific technological developments in the information sciences, including both hardware and software. Proposals which deal with the economic and organizational milieu of the developments, or with the economic, legal or social history of computing are especially encouraged.

Residence can be at the home academic institute, other research facility where there are archival materials, the Babbage Institute, or some combination of these. There are no restrictions on the location of the academic institution which will be the venue for the Fellowship. The stipend will be \$5,000 plus an amount up to \$2,500 for tuition, fees, travel, and other research expenses. Priority will be given to students who have completed all course work and have completed all requirements for the doctoral degree except the research and writing of the dissertation. However, even incoming graduate students will be considered. Fellow's may reapply for up to two one year continuations of the Fellowship.

Applications should include biographical data and a research plan or design. Applicants should arrange for three letters of reference, certified transcripts of college credits and GRE scores (or their equivalents abroad) to be sent directly to the Institute.

Applications for both the Predoctoral Fellowship and the Professional Internship should be sent to The Charles Babbage Institute, University of Minnesota, 104 Walter Library, 117 Pleasant Street S.E., Minneapolis, MN 55455, U.S.A. by January 15, 1984. Dependent upon funding one or both of these awards may be made.

PROFESSIONAL INTERNSHIP 1984-85

The Charles Babbage Institute for the History of Information Processing is accepting applications for a Professional Internship to be awarded for a period of three to nine months between June 1, 1984 and May 31, 1985. The Internship is available to professional staff interested in an introduction to the history of information processing. Appropriate applicants might include, but are not limited to, historians and social scientists interested in the history of information processing and its infrastructure, academics interested in preparing new courses in this history, or librarians and archivists interested in related archival problems.

Residence is required at the Babbage Institute, on the University of Minnesota campus. Interns are required to conduct a research project under the direction of the Institute staff. Support services include desk, secretarial and photocopy service, and telephone budget for Institute-related activities.

The stipend for the Intern Fellowship is \$1,000 per month. Interns may receive additional outside support from government, industry, or academic institutions, but must devote their full time to the history of information processing while the Internship is in effect.

Applications should include biographical data, a statement of interests, a proposal of dates during which the Internship would be held, and the names, together with telephone numbers and addresses, of three references.

ANNOUNCEMENTS: POSITIONS AVAILABLE**RENSSELAER POLYTECHNIC INSTITUTE**

New Department of Science and Technology Studies seeks a sociologist with specialization in science and technology studies to teach upper division courses in own areas of sociology specialization and to contribute new perspectives to the ongoing research activities of a large multi-disciplinary faculty. The position offers an opportunity to interact with scholars from a variety of social science and humanities disciplines, to teach outstanding undergraduates as well as graduates in the M.S. program in Science, Technology and Values and to utilize the unique resources of a technological setting, including superb computer facilities. This is a one-year visiting assistant or associate professor appointment. Send curriculum vitae and the names of three references to: Professor Shirley Gorenstein, Chair, Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, NY 12181. Deadline for application: February 10, 1984. RPI is an equal opportunity employer with an Affirmative Action program.

COLORADO SCHOOL OF MINES

Located in the foothills of the Rocky Mountains just west of Denver, C.S.M., one of the world's leading schools of mineral engineering, is seeking a dynamic scholar and educational leader to head its Department of Humanities and Social Sciences. A non-degree-granting department of some twenty faculty members, Humanities and Social Sciences is charged with providing the high-caliber students at Mines with the essentials of a liberal education, including breadth of knowledge, sensitivity to important social issues, and enhanced communication skills. While candidates may demonstrate strength in humanities, social sciences, or communications, experience in organizing and leading interdisciplinary programs and in generating funds from outside sources are considered important qualifications. Additional requirements are a Ph.D., a record of demonstrated scholarly accomplishment, and at least five years' experience at the rank of Associate Professor or Professor.

A letter of application, a resume, and three letters of recommendation should be submitted no later than 29 February 1984.

Dr. Donald W. Gentry
Dean of Undergraduate Studies
Colorado School of Mines
Golden, CO 80401

The Colorado School of Mines is an Equal-Opportunity, Affirmative-Action Employer.

OXFORD SCIENCE STUDIES SUMMER SEMINAR

Oxford Science Studies Summer Seminar



at
The Queen's College, Oxford
July 14-28, 1984

The phrase "science studies" embraces a wide range of inquiries into the nature, origins and social, economic and political relations of science, medicine and technology. The Oxford Science Studies Summer Seminar is intended to act as an international forum for discussions between scientists, scholars, policy-makers, teachers, and others with professional or lay interest in these fields. Drawing on some of the best current research, it will explore topical issues by means of keynote lectures, short presentations and seminar sessions. The summer seminar will take the form of two linked weeks which may be attended either separately or in combination. The academic programme will be complemented by social events and opportunities for visits in and around Oxford.

The Oxford Science Studies Summer Seminar is arranged by the Department for External Studies of the University of Oxford. For further information please contact:

Dr. J.R. Durant,
Department for External Studies,
University of Oxford,
3-7 Wellington Square,
Oxford OX1 2JA.
U.K.
Telephone Oxford 52901.

**SCIENCE WRITING EDUCATORS' GROUP
OFFERS A FREE SAMPLE OF ITS JOURNAL--
SCIPHERS: A SCIENCE JOURNALISM REVIEW**

Members of 4S with an interest in the public communication of science may want to examine a sample copy of Sciphers: A Science Journalism Review.

Sciphers is published by the Science Writing Educators' Group, an organization of university teachers of science writing. A typical issue is likely to contain:

- a general article about science journalism (the last issue examined science and humor in the mass media);
- news items about meetings, training programs, and innovations in science journalism;
- a review of a book about science or science journalism;
- an interview with a well-known science journalist, and
- an annotated bibliography of popular and scholarly articles about science journalism.

The publication is issued quarterly in newsletter format. Editors hope to enlarge the subscription base this year, which would permit expansion of the format. Subscriptions cost \$8 for one year or \$15 for two years.

A free sample copy of Sciphers can be obtained by writing Sharon Dunwoody, Journalism and Mass Communications, Vilas Communication Hall, University of Wisconsin, Madison, WI 53706.

**DEADLINE FOR MATERIALS TO BE PUBLISHED IN
4S REVIEW**

Vol. 2:#1-----1 March 1984
Vol. 2:#2-----1 June 1984
Vol. 2:#3-----1 September 1984
Vol. 2:#4-----1 December 1984

4S REVIEW

ISSN: 0738-0526

Journal of the Society for Social Studies of Science

The **4S REVIEW** is published four times each year, beginning in the spring of 1983 with Volume 1, Number 1. The **4S REVIEW** succeeds the **4S Newsletter** which concluded with Volume 7, Number 4.

4S REVIEW is sent to all members of the **Society for Social Studies of Science**; membership is on a calendar year basis. There are three categories of membership: **Professional**, \$15; **Students**, \$5; **Institutional** (including libraries), \$25.

Correspondence concerning membership and subscriptions should be sent to:

J. Scott Long
4S Secretary
Department of Sociology
Washington State University
Pullman, Washington 99164

Correspondence concerning manuscripts for publication, reviews, opinions, and news should be sent to the appropriate editor:

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

Lawrence Stern, (Book Reviews), Department of Sociology, Texas A&M University,
College Station, Texas 77843

Steve Woolgar, (News), Department of Sociology, Brunel University,
Uxbridge Middlesex, UB8 3PH, United Kingdom

Terry Shinn, (News), Groupe d'Étude des Méthodes de L'Analyse,
Maison des Sciences de L'Homme, 54 Boulevard Raspail,
75270 Paris, France

David Miller, (News), School of History and Philosophy of Science, University of
New South Wales, Kensington, N.S.W., 2033 Australia

Richard Gillespie, (News), Department of History and Sociology of Science,
University of Pennsylvania, Philadelphia, Pennsylvania 19104

Thomas Gieryn, (Bibliography and Literature), Department of Sociology,
Indiana University, Bloomington, Indiana 47405

4S REVIEW

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

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