

4S Society for Social Studies of Science

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4S Society for Social Studies of Science

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SCHEDULE OF SESSIONS

SCIENCE & TECHNOLOGY STUDIES--TORONTO 80

FRIDAY

<u>Time</u>	<u>Session Title</u>	<u>Chairperson</u>
9:00 a.m. to 12:00	Critique and Analysis of NSF Five-year Outlook on Science and Technology: Towards a Second Outlook	Daryl Chubin
9:00 a.m. to 12:00	Science & Technology in Developing Countries	Abdur Rahman
9:00 a.m. to 12:00	Patterns in the Social Relations of Mathematics	R. Calinger
1:15 p.m. to 3:00 p.m.	Round Table Resistance by Scientists to Scientific Discovery (Case of the Loch Ness Monster)	H. H. Bauer
1:15 p.m. to 3:00 p.m.	Appropriate Technology	J. Petersen
3:00 p.m. to 5:30 p.m.	Public Understanding of Science and Technology	J. T. Wilson
3:00 p.m. to 5:30 p.m.	Social Studies of Astronomy	David Edge
3:00 p.m. to 5:30 p.m.	Is There a Paradigm for Technology Studies	F. J. Dobney

SATURDAY

9:00 a.m. to 12:00	Quantitative Social Studies of Science	R. McGinnis
9:00 a.m. to 12:00	Comparative Analysis of Scientific and Technological Controversies	B. M. Casper
9:00 a.m. to 12:00	Social Studies of Social Science	Gabriel Haim
1:15 p.m. to 3:00 p.m.	Social Studies of Applied Research	TBA
1:15 p.m. to 3:00 p.m.	Round Table Critique & Evaluation of Social Studies of Science on the Tenth Anniversary of First Publication	David Edge
3:00 p.m. to 5:30 p.m.	Social Influences on Research Careers	Dean Harper
3:00 p.m. to 5:30 p.m.	Scientific Research in Government Settings	S. G. Solomon
3:00 p.m. to 5:30 p.m.	New Developments in Science Studies: Anthropology of Knowledge	Karin Knorr

TRAVEL NOTES

The Conference Coordinating Committee wishes to advise members of the History of Science Society, Philosophy of Science Association, Society for the History of Technology, and Society for Social Studies of Science of the following information pertaining to the October meeting.

1. Arrival in Toronto:

By automobile--for those arriving on Highway 401, exit at Avenue Road and proceed south (approximately 8 km) to the Park Plaza Hotel, 4 Avenue Road. There is an underground parking lot in the hotel. Those staying at the Plaza II should drive east along Bloor Street; the hotel is just east of Yonge Street. Those staying at the Bond Place should continue south along University Avenue to Dundas Street, then proceed east. The hotel is just east of Yonge Street. For those arriving from the Niagara frontier along the Queen Elizabeth Way, proceed along its extension (Gardiner Expressway) and exit at York Street. Proceed north on University Avenue to Dundas or Bloor, depending upon your hotel.

By rail or bus--easiest travel is by taxi from either Union Station or Elizabeth Street Terminal.

By air--There are four options from Toronto International Airport: (1) rental car (these should be booked in advance). Proceed via highways 409 and 401 as noted above; (2) taxi, which will cost approximately \$15 per carload to the hotels; (3) airline limousine, approximately \$18; (4) Two buses connect with the subway system. A bus from the terminals to Islington Station is \$1.75, the bus from terminals to Yorkdale Station is \$2.25. From Islington, take the subway (60 cents) to St. George Station (one block west of Park Plaza) or to Yonge Station (Plaza II), or change at Yonge to southbound and leave at Dundas (one block west of Bond Place). Those connecting at Yorkdale Station should proceed south to Museum Station (half-block south of Park Plaza), or around the loop to Dundas for Bond Place, and Bloor Station for Plaza II. N.B. all prices are quoted in Canadian funds.

2. Registration and Information:

The Registration/Information Centre is in the Park Plaza on the lobby level. If you have pre-registered, pick up your kit, badge, and tickets on arrival. Those not registered may do so in person. The Centre will be open from 4:00 p.m. until midnight, Thursday 16 October, from 8 to 8 on Friday and Saturday, and from 10 to 2 on Sunday. Information will be available on sessions, social events, sightseeing, etc. A message board will be located there, also. All Conference Coordinating Committee personnel will be wearing yellow badges; please stop and ask any of them for assistance.

3. Customs and Immigration:

American citizens normally only require valid identification for entry into Canada. All others will require passports and/or visas. Each person may bring into the country 40 oz. of spirits, or wine, or 24 bottles of beer plus 200 cigarets, or 20 cigars, or 24 oz. tobacco. There is no currency restriction. Americans should contact US Customs concerning limitations on items imported from Canada. US money is accepted in many shops and restaurants, but a better rate of exchange is available at exchange bureaux at the airport and at banks. Travellers' cheques and common credit cards are accepted in most shops and restaurants.

4. Activities in Toronto:

Those bringing families and those who wish to combine some sightseeing with the meetings will find a wide variety of activities in North America's fourth-largest city. Tourist information will be included in your registration kit. For children, we would particularly recommend the Metro Zoo, Island Park, CN Tower, Ontario Place, and the Ontario Science Centre.

5. Clothing:

Toronto Octobers tend to be cool. Evening temperatures may drop to 0°C. Dress as you would for a visit to New York or Chicago.

1980 4S BUDGET

<u>Anticipated Expenses</u>		<u>1979 Expenses</u>
Newsletter	\$4000	\$3370
Annual Meeting	2500	4210
Duplicating & Postage	500	240
Secretarial	500	100
Telephone	100	---
Miscellaneous	200	---
	\$7800	\$7920

<u>Anticipated Income</u>	
Membership Renewals	\$ 4650
New Members	500
Annual Meeting	2500
Balance from 1979	5800
	\$13450

THOUGHT AND OPINION

Citation Analysis and the Current Debate over Quantitative Methods in the Social Studies of Science

Edward T. Morman
University of Pennsylvania

The various techniques of citation analysis are the tools of information scientists, policy makers and sociologists more than of historians. The use of these techniques in the history of science is closely connected to pressures from practitioners in other areas and the debate over their use as sociological-historical methods can best be understood within a context at least as broad as the breadth of their applications.

Derek Price, long an advocate, theoretician and practitioner of quantification in the history of science, is explicit about the relationship between history and policy studies. In 1965 he proposed that science and scientific manpower are uniquely appropriate for study in terms of citation patterns, since one can adopt as the operational definition of scientist "a person who publishes scientific articles", and since scientific citations tend to define research fronts consisting of important recent articles--which is the case with neither technology nor the humanities (Price, 1965a, p. 555). Price has admitted that scientists do not spend a lot of time reading the scientific literature and depend instead on informal means of communication (1965a, p. 562). Therefore, in order to justify use of the media of formal communication as the raw material with which the historian or sociologist can build a model of the structure of science, he proposes that the published literature is the codification of the results of casual conversations, letters, etc.

Eugene Garfield continues to rely on Price's formulation as the rationale for such work (Garfield, 1979, p. 81). Garfield writes as an admitted non-historian (1979, p. 93), and as an entrepreneur he wears his interests on his sleeve; but his pronouncements deserve to be taken seriously. His early interest in the citation index as an analytic tool in science policy and history has been adequately documented (Garfield, 1979, ch. 2); and the subsequent development of methods of considerable analytic power and mathematical sophistication encourage Garfield to continue to advocate citation analysis at least as strongly as Price.

Garfield is bold enough to assert that the key to understanding the structural dynamics of science lies in citation analysis, and that citation analysis can provide conclusive tests to judge among models of scientific change (1979, pp. 142-144). Garfield is not alone. Academic historians have suggested that "Kuhn's theory is particularly amenable to quantitative testing" using citations as basic data (Tobey, 1976, p. 6). Although he doesn't use the phrase "crucial experiment" in suggesting possibilities for citation analysis, one can see that Garfield's conception of the use of citation analysis is informed by a rationalistic view of science.

This rationalism is evident on two levels: science as the object of citation studies as well as the science of citation analysis itself. Garfield's implicit hypothesis on the nature of science cannot be considered further here, but his view of citation analysis as objective and thus of "preferred logical status" (Edge, 1979, p. 109, uses this phrase disparagingly but not inappropriately) is

important if only because many science policy makers tend to agree with it.

Price and Garfield are heirs to a tradition of statistical bibliography which Thackray (1978, p. 19) traces to Cole and Eales's (1917) work on the literature of comparative anatomy. Thackray (1978, p. 13) claims that the history of science generally allied itself with philosophy and the natural sciences until the 1970s; and that in doing so it rejected quantification in the study of science as a social and epistemological phenomenon. The broader disciplinary context in which the work of Price, Garfield and the subsequent elaborators of their methods can be viewed is the type of sociology of science which Thackray characterizes as the "lively North American tradition of measurement and analysis of the internal dynamics of . . . scientific communities" (1978, p. 21). Thackray notes the "unease over the ahistorical character of the recent sociology of science" by a group of European sociologists "enjoying a common paradigm." The alliance of the citation analysts with Mertonians thus adds an additional dimension to the debate: the differences between British and American sociologists (and sociological historians) of science has a particular manifestation in aspects of the critique of citation analysis methods. Interestingly, the work of the British sociologists of science is itself suggestive of a means by which one can examine the differences between these groups: differences in professional vested interests, social group interests, and disciplinary competencies, which lead social scientists to different methods of "actively seeking out information of certain kinds and at certain times" (Edge, 1979, p. 107).

One set of interests includes those associated with policy-making in science. Science Indicators 1976 (SI/76), third in the federal government's series of official attempts to measure the state of science, has been criticized for overly conservative use of bibliometric techniques, including citation analysis. In its critique of SI/76, the General Accounting Office suggests that citation techniques provide the best means to get at the "knowledge bases" of science and to evaluate its frontiers (U.S. Comptroller-General, 1979, pp. 21, 32ff). Replying to this, the National Science Board insists that through SI it "has been one of the main supporters and users" of citation research, and promises increased application of it in future editions of SI (U.S. Comptroller-General, 1979, p. 60).

It is in the thrust towards objective, quantitative studies in the service of science policy-making that citation analysis finds its social impetus. But the absence of an historiographical tradition of quantitative studies of science has resulted in a serious discontinuity between the resources available to the measurers of science and the validity of their findings.

Techniques and uses of citation analysis.

Garfield (1979) provides a reasonable summary of the pioneering studies in citation analysis proper (see chapters 6 and 7) and in co-citation analysis as a means of mapping the structural dynamics of science (see chapter 8). Cole *et al.* (1978) summarize studies done by themselves and others which use citation-based techniques toward an understanding of the nature of scientific consensus.

Citation analysis proper. The classic work applying this technique--the linking of papers and presumed identification of influence by tracing citations--is Garfield *et al.*'s (1964) analysis of the developments leading to the discovery of the DNA code. The study begins with key events identified in Isaac Asimov's

(1963) account of the discovery, and ends by contrasting the results of citation analysis with Asimov's description of the flow of influence. Garfield (1979, p. 75) later summarized four conclusions which can be drawn from this work:

First, the relationships that a citation analysis shows among the components of a given body of work correspond very well to the relationships perceived by a scientist of Asimov's rank. Second, a citation analysis can identify significant relationships and events that even a remarkable memory might forget, or that traditional techniques of historical research might miss. Third, a graphic perception of the sequence of events is superior to a narrative presentation for the purposes of historical and sociological analyses. And fourth, the manual construction of network diagrams, named "historiographs", was much too laborious for them ever to become widely used.

Co-citation analysis. In a two-part paper by Small, Griffith and others (Small and Griffith, 1974; Griffith et al., 1974) the methods of co-citation analysis are described in the course of a discussion of the structure of the entirety of science as it was captured in a temporal cross-section by the data-base assembled for one issue of Science Citation Index. Small and Griffith (1974, p. 17) explicitly link their work to that of historians and sociologists of science and to information scientists who regard sub-disciplinary specialties as the building blocks of science, and who attempt to map them.

Starting with the assembly of clusters of papers based on the frequency with which each cited paper was paired with every other cited paper in the citations of the source articles analyzed in their data-base, Small, Griffith and their colleagues present a complete disciplinary map of science--or at least those areas of science in which citation and co-citation rates were high enough for inclusion in their sample. It should be noted that although the map is based on the cited (older) articles, it is intended to reflect the structure of science as "recognized and maintained by current researchers" (Small and Griffith, 1974, p. 19), in their choice of which articles they cite. The changing structure of scientific specialties can be determined by preparing maps for successive years (for a discussion of later work which does this in one particular field, see Garfield, 1979, pp. 123-131). This technique is available to the historian only as citation indexes are prepared whose source articles date from the historical period of concern.

Other techniques using citations as basic data. Scholars whose background and training place them in the mainstream of North American sociology have adopted measures of citation concentration as a means to determine consensus in paradigm acceptance. Cole et al. (1978), for example, have applied sophisticated statistical techniques to citation data in order to test empirically various models of the structure of science; they report that traditional disciplines are entities of greater sociological than intellectual consequence.

Other sociologists and historians, regardless of their intellectual lineage, have often accepted citations as data which, when subjected to rather simple manipulation, can be useful in answering less global questions. Beaver and Rosen compute differential citation rates in order to determine the core journals which were used in their study of scientific collaboration (1978, p. 48); and

Latour and Woolgar measure the "symmetry of confidence" between two groups by counting citations to publications of each group in publications of the other (1979, p. 134).

The critique of citation analysis.

David Edge's recent paper (1979), an explicitly polemical review of citation techniques, can serve as a current summary of the opposition to the use of citation analysis. Edge suggests limiting the use of citations to the bibliographical, he rejects the possibility of accurate mapping using citations, and he remains an agnostic on the heuristic value of citation analyses--that is, he knows of no occasion when quantitative measures of communication "have suggested a novel idea or approach which has led an historian to adjust his research methods and strategy", but admits that it might be possible (1979, p. 122).

Edge insists that informal communication is generally more significant and that citation analysts are deceiving themselves if they believe that journal articles and books are straightforward codifications of letters, conversations and discussions at conferences:

One is tempted to say that formal communication in science is "the tip of the iceberg", were it not for two facts: (a) the "tip" is very large, extensive and important; and (b) there is every indication that the "tip" is radically different in kind from what is "below the waterline."

Since publication is only one form of communication, and one that is both less significant and radically different from the informal means, it makes no sense to base one's work on it if one is truly interested in understanding the cognitive or social structure of science.

This point is related to another of Edge's objections, a more theoretical one. Edge contends that tacit communication (in the form of learning laboratory techniques in the course of practicing them with a mentor, for example) and negotiation (both with nature and with colleagues) constitute the true stuff of communication in scientific communities. To ignore these, as citation analysts must do, means more than simply choosing an inappropriate technique; it means adopting a view of science which is profoundly suspect, in which ideas are passively adopted as a result of the flow of influence.

From these overarching methodological and theoretical arguments, Edge proceeds to more particularistic ones which imply that citation techniques might be appropriate if certain flaws were understood and corrected for. When he points out that the most important papers may frequently go uncited "because their existence, and the audience's knowledge of them are taken for granted" (1979, p. 121), he is almost suggesting that a citation analysis which considered these anomalies might be more valid. Latour and Woolgar (1979, p. 108) corroborate Edge's point in passing, by observing that a citation is an expression of modality equivalent to the admission of doubt about a statement. Once a statement becomes a fact, no citation is necessary.

Small (1978) attempts to render such criticisms irrelevant and to bolster the theoretical underpinning of co-citation analysis, by suggesting that citations

serve as "concept symbols." The symbols are created by the citing authors and the uniformity of symbology is measurable by comparing the contexts in which citations to a given publication appear (pp. 328-9). Small performed such a comparison on a sample of highly cited articles in chemistry, and found that the most highly cited of them tended to be uniform in their symbolic value (i.e. in the idea or subject represented by the citation). Direct influence or lack of citation to other key works is therefore unimportant, since the co-citation analyst only wishes to judge where the citing author locates the intellectual content of his work. Small presents the concept symbol thesis explicitly in response to demands for a theory of citing behavior (1978, p. 327). But by dismissing other possible explanations for choice of citation (e.g., the desire to persuade, to curry favor, to publicize, or to avoid offending) as inadequate (p. 337), Small avoids dealing with interactions between symbolization and other factors. He is thus left with a wide gap in his explanation of citing behavior--why do particular authors choose particular cited works as symbols for particular concepts?

An overview of the debate.

A sociological explanation of the debate over citation analysis has begun to take shape. Its elements include (1) an easily defined economic interest relating to the concerns of policy makers, (2) contrasting traditions which result in a clash of professional vested interests, and (3) concerns for the use of technical competencies by both the practitioners and opponents of citation analysis.

It is not necessary to dwell on the economic motivations of citation analysts. Garfield's first major study (Garfield et al., 1964) was funded by the Air Force Office of Scientific Research and was able to report that government agencies "provided grant or fellowship funds supporting 67% of the more recent nodal works" in DNA research (p. v). Since 1964, concern for funding "nodal" or significant research has increased with the stringency of research budgets (see Griffith, et al. 1974, p. 364); and the funds available for studies to aid in science policy decision making have grown (see Garfield, 1979, pp. 254-5).

The convergence of Mertonian sociology of science with sociometric historical studies has generated a set of professional vested interests which clash with those customarily associated with the history of science. On the one hand is the alliance of sociologist-citationists like the Coles with Derek Price--who has been an anomaly within the history of science for some time. Price's association with the citation analysts furnishes additional analytic power to his theorizing and provides him a broader base. On the other hand, Ronald Tobey (1976, p. 25) speaks for the traditions of the historical community in remarking that "it is in the interest of the historian that citational analysis not be resorted to." Not only is the technique expensive, but the historian--unlike the sociologist--should feel obliged to consult the primary literature anyway if he is to understand the cognitive structure of science. Therefore, says Tobey, "it would be efficient to skip the citational analysis altogether and simply read the literature" (p. 25).

Thackray has alluded to the long association of the history of science with philosophy and natural science. Perhaps Edge, a former astronomer, brings to the debate the scientist-turned-historian's skepticism of the use of mathematics in the social sciences. Edge's colleagues at Edinburgh and their co-thinkers who represent a distinct trend in recent history and sociology of science, are

obliged to concern themselves with citation analysis because it seems to be a juggernaut and is allied to a competing school of the sociology of science. The two approaches might tend to ignore each other, effectively accepting a mutual incommensurability, but that citation analysis has proved itself capable of attracting so much attention.

Related to the question of professional vested interests is that of competency. Thackray refers to belief in quantitative methods as essential for the development of a quantitative historiography of science. Practitioners of citation analysis hold that belief and they attempt to support it with quantitative competence. Defense of this competence, as distinct from any simple financial benefit, is linked in a positive feedback loop to acceptance of the technique.

Edge, however, a practitioner of traditional small-group sociological methods in his investigations of the structure of radio astronomy, rejects the entire notion of large-scale mapping of scientific disciplines:

I find the "correct definition of a specialty" a meaningless concept, and I have no need of anyone (computer-aided or otherwise) to provide me with it. I know that this radically sociological, participant centered perspective on scientific collectives makes research more difficult than it would be under more simple-minded premises--but then I happen to believe that the problems such research addresses are difficult Citation and co-citation analysis, in striving to accumulate and average, destroys evidence of individual variations; yet it may often be because individual scientists and groups do not share the consensus view (enshrined in aggregated co-citation maps) that crucial innovative decisions are made (1979, p. 124).

Conclusion.

Citation analysis in the history of science results from the convergence of the needs of the policy makers of the post-war era, the maturation of bibliographic citation practice in the scientific literature during this century, the availability of electronic data processing, and a school of sociology concerned with the internal dynamics of the scientific community. It presupposes a model of science which is simultaneously rational and literary; the scientist is regarded above all as the producer of scientific inscriptions, and he is expected to credit the source of each idea, method or datum which he uses, in a consistent fashion. It is somewhat ahistorical because it is based on a conception of citing behavior which idealizes recent practice. And since citation techniques share no common methodological assumptions beyond acceptance of quantification and regard for the citation as a usable primary datum, it is appropriate to look to interests which reside outside the cognitive aspects of the scholarly disciplines to which the techniques are applied, for much of the reason for their application.

Its power as a bibliographic tool and its continued support by science policy makers brings citation indexing and analysis to the center of a debate within the social studies of science. Proponents of other schools of thought have been obliged to become explicit opponents of citation analysis, where they might otherwise have ignored it as a set of techniques which bear no relevance to their

work. The activities of the antagonists on both sides of the debate are equally susceptible to analysis in terms of the interests of scholars who actively negotiate with each other, with the objects of their studies, and with social and economic realities, in the creation of new knowledge.

References

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The Trouble With Scientists

John Ziman
University of Bristol

Bernard Barber's Presidential Talk at the Washington meeting of 4S (4S Newsletter, Spring 1980) properly voices concern about the "ignorance, negativism and ambivalence among natural scientists toward the social study of science." This was the theme of my public lecture to the AAAS at San Francisco last January: we scientists are appallingly ignorant about the new conceptions of science now emerging from such disciplines as sociology, philosophy, social psychology, politics and economics.¹ This ignorance is so damaging to the research process itself, as well as to the place of science and technology within the polity,² that we must certainly take up such matters in science education at all levels.³

We social scientists thus have a clear responsibility to disseminate more widely what we have found out about science. To reach uncaptive or unindoctrinated audiences, however, certain elementary strategies of communication are called for. It is best not to insist (or even infer) that sincere and competent scientific workers are simple-minded, venal or grossly deceived about the nature of their own experiences. Some degree of empathy--perhaps even sympathy--might be cultivated between fellow intellectuals struggling with similarly subtle problems. Robustly simplified versions of new conceptual schemes travel more safely across cognitive frontiers than the fragile structures that scholars love to elaborate. No experienced teacher, preacher, lecturer or writer needs to be told that.

The most serious cause of interdisciplinary dissonance, however, is the incredulity aroused by exaggerated claims to wisdom from one side or, the other. Just because we natural scientists have been arrogant about our knowledge claims, is no reason why we social scientists should assert an equal or higher epistemological status for our insights. Modern metascience has exposed the softness of the clay on which, somehow, the great temples of human knowledge must be build--including the social sciences themselves.⁴ It is noteworthy moreover, that most of the fundamental concepts bandied about amongst, say, the members of 4S--norms, facts, paradigms, disconfirmation etc.--are difficult to define, and often, apparently, logically incompatible, despite their interpretative power for the actual history of science.⁵ To deny that these useful concepts have become fighting words in some quarters would indeed demand regrettable "ambivalence." On the other hand, to close our minds (as natural scientists) to them until (as social scientists) we have resolved all the contradictions would be equally deplorable "negativism." A more constructive attitude, surely, is to cure our "ignorance" by teaching about these theoretical resources as precepts or "maxims," applicable provisionally to various aspects of the R & D system under appropriate circumstances, without inconsistency and to the benefit of science, society and the pursuit of knowledge.

1. Ziman, J.M. "Science: the New Model" to be published in the Bulletin of Science, Technology and Society.
2. Ziman, J.M. "Conceptions of Science" to be published in the Proceedings of the Conference on the Political Realization of Social Science Knowledge: Vienna, Austria, June 18-20, 1980.

3. Ziman, J.M. Teaching and Learning about Science and Society (Cambridge University Press 1980).
4. Ziman, J.M. Reliable Knowledge: an Exploration of the Grounds for Belief in Science (Cambridge University Press 1978).
5. Ziman, J.M. "Reconstructing the Reality of Scientific Growth" (Review of Black-Body Theory and the Quantum Discontinuity, 1894-1912 by T.S. Kuhn) Minerva, XVII, 321-7, 1979.

To The Editor:

I am grateful to Professor Ziman, whose writing we all much admire, for commenting on my informal remarks about the attitudes of natural scientists toward the social study of science. I wanted to stimulate discussion with my paper in the 4-S Newsletter, and I hope others will join Professor Ziman and me in expressing their views.

I am afraid, however, that I cannot go along with Professor Ziman in his recommendation that the social science of science be limited to "maxims." If all he intends by this is a cautionary reminder that all science is provisional, fine, I can accept that; indeed, we must insist on it in our work. But if he means, as I and some others, especially other natural scientists, would read him, that social science does not now have, nor ever can have, systematic theory and systematic empirical data, then I have to disagree and say that this is an erroneous view and one that interferes with the best development of the social science of science. The chief instrument of the development of the natural sciences must be our chief instrument too. We have made a lot of progress with improved theory and improved data these last forty-five years, and we should continue along this path.

Yours,

/s/ Bernard Barber
Department of Sociology
Columbia University

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These contents pages have been reprinted from Current Contents: Social and Behavioral Sciences.

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Sage Publications Ltd., London
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Social Studies of Science

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VOL. 10 NO. 2 MAY 1980

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CHRISTOPHER EVANS

We were greatly saddened to learn of the death of Dr. Christopher Evans in England on October 10, 1979. Chris Evans was an extraordinary person and will be deeply missed. In addition to his many contributions to psychology, especially dream research, he had a continuing and fair-minded approach to claims of the parnormal. He was a strong supporter of the goals of ZS (to which he was a Consulting Editor) and an astute and friendly critic of psi research, towards which he was a true skeptic, doubting but very much open to evidence and dialogue. His death leaves a vacuum in the hearts of many of us, and his kindnesses and intellectual contributions will be long remembered.

JAMES WEBB

As this issue of ZS goes to press, we have just been informed of the tragic and untimely death of James Webb. A further statement will appear in the next issue of ZS following our receipt of further information.

RECENT PUBLICATIONS

SCIENTISTS IN WHITEHALL (£12.50) by Philip Gummett, published by Manchester University Press, Oxford Road, Manchester M13 9PL England.

This is the first definitive survey of the relationship between science and the administrative machine of British central government. Essential reading for students of both science policy and administration, it is written for both specialist and non-specialist and contains much new material.

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Philip Gummett is Lecturer in Liberal Studies in Science and Co-ordinator of the Programme of Policy Research in Engineering, Science and Technology at the University of Manchester. He is co-editor of Directing Technology: Policies for Promotion and Control.

Copies may be obtained at £12.50 (MUP Giro Account no. 614 1358 for overseas payments, or by sterling cheque please) from Manchester University Press, Oxford Road, Manchester M13 9PL England (tel. 061-273 5539).

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Foreword by **Robert S. Morison**

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A META-SCIENTIFIC ANALYSIS OF RECENT LITERATURE
ABOUT THE ROLE OF NATURAL SCIENCE IN SOCIETY

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POSITION VACANCIES

MUSEUM OF SCIENCE AND INDUSTRY IN CHICAGO. Contingent upon funding, a one-year position as Researcher will be available beginning January 1 at the Museum of Science and Industry in Chicago. The Researcher will develop the content for an exhibit based on the major scientific/technological developments in the past 50 years and their social impact. Candidates should have a background in both the sciences and the humanities and hold a Ph.D. or ABD. Salary will be commensurate with experience.

Dr. David A. Ucko, Research Coordinator, will interview candidates at the Toronto meeting if the position is funded. He will be at the Park Plaza Hotel. Room number and hours will be posted. Interested candidates not attending the meeting should write to Dr. Ucko directly at the Museum of Science and Industry, 57th Street and Lake Shore Drive, Chicago, Illinois 60637 -- Equal Opportunity Employer.

AMSTERDAM UNIVERSITY. At Amsterdam University a position is available for a Visiting Professor (m/f) in the "Dynamics of Science" for one year. The task will consist of:

- teaching in the newly formed Department "Dynamics of Science," which will be formed between the faculties of chemistry, biology and philosophy
- participation in a workgroup "Dynamics of Science" that incorporates research-workers from the faculties of economics and the social sciences
- research about the social dynamics of science.

Candidates should at least conform to the following requirements:

- a thorough knowledge of "Science, Technology and Society" and/or "Science Policy Studies" as indicated by the applicant's own publications
- interest in making these fields fruitful for attempts to direct the sciences to the solution of social problems
- some experience in organizing a "science studies"-group.

The appointee is expected to express himself/herself in written and spoken English, German or Dutch.

The salary--depending on age and experience--will be between f6.141,- and f8.773,- (Dutch guilders).

Information can be obtained from the Secretary of the Committee for the Dynamics of Science, mw. drs. S. Kasanmoentalib, Biology and Society, Kruislaan 318, 1098 SM Amsterdam, The Netherlands, tel. 020-680551, to whom applications, proposals and suggestions can be sent (as soon as possible).

REPORT OF MEETINGS

Trevor Pinch and David Travis

Two recent workshops brought together those concerned with the study of controversies in science. At previous sociology of science conferences--especially that held in Bielefeld in June 1979--it emerged that those engaged in detailed studies of controversies felt the need for a forum where ideas and information could be exchanged without the need to present formal papers. The first workshop was held at the Conservatoire Nationale des Arts et Metiers, Paris March 8-9, 1980. We present here the report of one of the participants.

Workshop on the Study of Controversies in Science--John Law

This meeting of about 20 British and French sociologists of science organised around the theme of controversies in science revealed much useful diversity and difference within a more or less shared framework of assumptions.

The shared framework was that a thoroughgoing sociological study of scientific content and institutions is possible; and that science does not differ in terms of its fundamental status from other social institutions and is thus susceptible to the full range of sociological (and other social science) methods of study. (There were one or two possible dissenters here: for instance Peter Glasner (North London Polytechnic) who seemed to be influenced by Bachelard in wanting to say that scientific fields have a particular epistemological status and can, in any case, only be understood historically if this specificity is to be revealed).

The differences and diversities were numerous:

(1) The importance of controversies: several speakers organised their remarks around the formal subject of the meeting. Harry Collins (University of Bath) argued that the best research strategy is to study controversies where normally tacit social assumptions are made explicit, whereas the best rhetorical strategy (for persuading non-sociologists that science is a social construction) is to study high status and non-controversial areas of science. Other speakers, notably Benjamin Matalon (Universite de Vincennes) who spoke about the normative status of statistics in psychology, also organised their remarks around the theme of controversies. Many other contributors, however, paid scant attention to the subject of controversy, and Bruno Latour (Centre Nationale des Arts et Metiers) argued that to talk about "controversies" and in particular to distinguish between them as "major" and "minor" was to carry actors' categories over into sociology in a counterproductive manner. His view, expressed with clarity and force, was that we should rather study the way in which debates are opened and closed using general sociological, economic, or semiotic methods.

(2) The role of the sociologist: if most contributors agreed that science including knowledge is a social construction, then there was disagreement about how we should next proceed. As I mentioned above, Collins seemed to take the view that it is important to persuade outsiders of this fact, presumably because science is generally an institution with very high credibility. Other participants (Nigel Gilbert, University of Surrey, John Law, University of Keele) were less concerned with outside audiences, taking a rather more "professional" view of their role. For them, professional sociological analysis of the social construction of knowledge and social structure sufficed. They held that it was neither possible to persuade outsiders, nor profitable to try. Bruno Latour took a third view: that the sociologist, at present lodged outside science as

an alienated observer, should collaborate in its production, thereby changing it and at the same time affording himself an advantageous vantage point.

(3) Methods: Despite the generally shared view that science is not "special" several participants were concerned that this might nevertheless appear to be the case if we were to allow "science studies" to be isolated from other branches of social science. Law argued that general methods of sociological analysis should be employed. It was, however, Latour who was most insistent on this point. He thus invited Francoise Bastide (College de France; Ecole des Hautes Etudes) to present an example of the production of meaning in a scientific paper. Outlining a selection of semiotic tools, she argued that these offered the possibility of a formal analysis of how any text offered the information necessary for a reading. In discussion it became clear that discourse analysis was being offered as something like a formal shorthand for understanding the structure of textual persuasion. This met with a fairly (though by no means universal) sceptical reception from participants, both French and English. Gerard Lemaine (Ecole des Hautes Etudes en Science Sociale) argued that, lacking a metatheoretical basis, such analyses might ramify indefinitely. Several of the British participants seemed unhappy with the approach because it appeared to offend the tenets of interpretive sociology: that the agent be seen as active and that meaning be seen as a transaction between agent and text, rather than as residing in the text.

Inevitably a short report such as this cannot do justice to all the themes that arose. For instance Peter Halfpenny (University of Manchester) offered an outstandingly clear philosopher's analysis of the mode of explanation of a variety of different sociologies of science, and Gerard Lemaine outlined his conception of "hypernormal"--trivially normal--science. Overall, however, this observer came away with three overall sentiments. First, the meeting was important in revealing the ground shared by many British and French sociologists of science. Secondly, I had the feeling that it might be difficult to capitalize as fully as possible on these shared assumptions and interests because of language problems. And thirdly, that profitable though the meeting was as a way of improving informal contacts and exploring shared ground, it would have been better if somewhat more formal papers had been precirculated. Deeper exploration of important themes would then have been possible.

The meeting was ably organised by David Travis (North London Polytechnic) and Bruno Latour. It was funded by Parex, and hospitality was generously extended to the British participants by Bruno Latour, his friends and relatives.

A second workshop was held at the University of Bath, June 24-25, and was organised by Harry Collins. It was attended by those who had recently completed or were carrying out field studies of controversies in non-applied science. The purpose of the meeting was to see what practical steps could be taken towards encouraging collaboration between different researchers. A proposal by Collins to instigate a data bank on scientific controversies was extensively discussed. It was agreed that steps toward setting up a data base on field studies should be taken. The data base would initially consist of copies of the relevant literature-sets, catalogued for computer storage and retrieval. It was also agreed that a continuing series of workshops should be organised around themes of common interest. It is hoped that a draft proposal for funding of these collaborative ventures will emerge in the near future.

The workshop was followed by a talk from Daryl Chubin on current American sociological interest in the study of controversies, particularly those with a public dimension.

IIT ETHICS CENTER PUBLISHES ENGINEERING ETHICS BIBLIOGRAPHY

IIT's Center for the Study of Ethics in the Professions has published a 158-page annotated bibliography on ethical and social issues in the engineering profession.

The paperback publication is titled A Selected Annotated Bibliography of Professional Ethics and Social Responsibility in Engineering. "It is the first publication of this scope and is intended as a guide to the rapidly developing body of writings about the subject," says Dr. Robert F. Ladenson, Project Director for the preparation of the bibliography and Associate Professor of Philosophy at Illinois Institute of Technology.

Major categories covered include professional ethics in engineering; social responsibility in engineering; and general materials from history, philosophy, and sociology that are relevant to engineering practice.

Topics include codes of ethics, bribes and kickbacks, consequences of "whistle blowing," and the sometimes conflicting roles of business, government, and professional associations.

The bibliography was compiled by Ethics Center faculty and staff and includes an appendix, an author and title index, and a subject index for easy reference.

The bibliography is available on request without charge to educators, engineers, professional engineering societies, and others interested in ethics references.

Requests for copies should be sent to the IIT Center for the Study of Ethics in the Professions, Illinois Institute of Technology, IIT Center, Chicago, Illinois 60616, phone (312) 567-3017.

INTERNATIONAL ENGINEERING ETHICS PROJECT

A research project--to identify and examine the ethical conflicts and dilemmas facing engineers practicing in a culture other than their own--is currently being undertaken by an engineer/philosopher team. Although international business ethics and public policy concerning technology transfer have received some discussion, this project will concentrate on the ethical problems confronting the individual engineer engaged in some particular engineering project in another culture.

It is believed that this study is the first of its kind. The co-directors, Dr. E.C. Jones of Iowa State University, an engineer with international experience, and Dr. C.A. Smith of the University of Missouri-Rolla, a philosopher working in the area of engineering ethics, say that, to their knowledge, "almost no attention has been paid to the ethical problems confronting individual engineers who find themselves at the interface with another culture and who must cope."

Conflicts can arise for anyone working in another culture with alternative customs and with alternative conceptions of justice, welfare, public interest, the good life, treatment of minorities, etc. But in addition to such general concerns, this project will address specifically the ethical dilemmas that arise within all areas of engineering work: design, implementation, management, worker relations and interaction with local institutions such as political, legal, regulatory, economic, religious or educational.

The design and implementation of a technological system for another culture may reinforce or conflict with the values and way of life of that culture, the project culture. Design of the workplace provides a situation illustrating such effects; the workplace can be designed to accommodate or violate the social relations, roles, status and stratification in the project culture. The engineer's conviction that

social stratification of workers is inefficient and/or reprehensible may have to be balanced against worker dissatisfaction with any arrangements that violate such stratification. In like manner, a project culture that has a different valuation of safety and health can present design conflicts, as can alternative attitudes toward the environment, resources, and getting one's hands dirty in "hands-on" work.

A second area of conflict concerns management styles--the management expectations of the project culture may hold many surprises for the engineer. Leadership styles necessary for acceptance can be personally or morally repugnant to the engineer. Alternative conceptions and values of the project culture can present ethical dilemmas in labor relations; when equal treatment of all violates the social roles of the project culture: when petty thievery is regarded as a fringe benefit or is the result of alternative views of property; when hiring and firing practices conflict with cultural practices. Alternative conceptions of "who is a worker" can also present problems; for example, local exclusion of certain individuals or groups because of taboo or prejudice, or a high valuation of total group participation including those who are inefficient or present safety problems.

Engineers must interact professionally with local institutions. Alternative laws, legal institutions and enforcement mechanisms can obviously raise ethical questions for the engineer. But conflicts may also arise where there are alternative economic assumptions and practices: haggling, "just price" assumptions, bribery, or the belief that buying and selling are personal relations involving introductions and paid go-betweens. Similarly, alternative political practices can present problems; for example, in relations with local political leaders, in gaining approval for plans and procedures, in handling political pressures, etc. (Areas of political instability obviously create another set of special problems.) Dealing with regulatory agencies having unfamiliar procedures and/or alternative sets of regulations is another area of potential conflict for the engineer, particularly if the regulations are weaker than those the engineer is used to, or if the rationale for the regulations is not understood or is judged to be reprehensible, or if the regulations are commonly ignored in the project culture.

Engineers will experience culture shock involving alternative life-styles and customs in the project culture. Some of these cultural differences may be judged personally or morally repugnant. In similar fashion, the engineer's actions may raise questions in the project culture. The social interactions of the engineer with local groups can also raise questions concerning the apparent (or real) identification with a particular social class or group. A different set of questions may be raised if the engineer remains aloof in an enclave of his or her own nationality. Experience of cultural differences can effect the engineer's perceptions of the project culture and the culture's perceptions of him or her, and lead to "professional culture shock" that can create ethical conflicts on the job or exacerbate the conflicts already noted.

The co-directors of this project are currently engaged in collecting more detailed information and establishing a network of contacts. It is expected that an analysis of these situations will aid in the identification and anticipation of such difficulties, will aid engineers in handling such situations, and will help to minimize stress for both the engineer and the project culture. Individuals who can contribute to this study, particularly those with first-hand experiences, are asked to contact either Professor E.C. Jones, Department of Electrical Engineering, Iowa State University, Ames, Iowa 50011, or Professor C.A. Smith, Department of Philosophy, University of Missouri-Rolla, Rolla, Missouri 65401. All information received will be treated confidentially.

CHARLES BABBAGE INSTITUTE SELECTS PERMANENT HOME

The Board of Trustees of the Charles Babbage Institute for the History of Information Processing (CBI) announced this week that it has selected the University of Minnesota as its permanent home. CBI is a nonprofit foundation which promotes research on the history of computers and computation. CBI is currently located in Palo Alto, California.

CBI was founded in late 1977 by Erwin Tomash, founder of Dataproducts, a large manufacturer of printers headquartered in California. Tomash recently retired as Chairman of the Board of Dataproducts to devote more of his time to CBI's program.

In its two years of existence, CBI has begun a number of activities, including: an annual fellowship to graduate students working in the history of data processing, an oral history program in which scholars interview computer pioneers, publication of a Newsletter and other materials disseminating information about the history of information processing, and an educational campaign aimed at the preservation of historical materials. When settled at the University of Minnesota, CBI will expand these activities and initiate others. In particular, it will establish a scholarly historical research program and an extensive archival program. In making their selection, CBI's Board of Trustees noted that the University of Minnesota offers to the Institute the facilities to implement a vigorous program in the history of information processing. The university has strong programs in the history of science and technology, in management information systems, and in computer science and engineering. In addition, a new Midwest Regional Institute for Computer Science and Technology is being launched in cooperation with local industry. The strength of the computer industry in Minnesota was one of the factors which led to the selection of the University of Minnesota as CBI's host institution.

As a part of the arrangement with the university, Roger H. Stuewer, Professor of History of Science and Technology at the University of Minnesota, will serve as Acting Director until a permanent director is appointed.

Stuewer holds a joint appointment in the School of Physics and Astronomy and the Minnesota Center for Philosophy of Science and teaches courses in the history of nineteenth and twentieth century physics. He is also on the graduate faculty of the American Studies Program. He is the author of The Compton Effect: Turning Point in Physics (Science History Publications, 1975) and editor of two other books, Historical and Philosophical Perspectives of Science (University of Minnesota Press, 1978) and Nuclear Physics in Retrospect (University of Minnesota Press, 1979).

A University of Minnesota/CBI consultative committee will be established to maintain close liaison between CBI and the university's departmental and collegiate units. A permanent director will be sought through a search conducted by a committee jointly appointed by the CBI Board of Trustees and an administrative committee of the University of Minnesota, chaired by the Dean of the Institute of Technology.

CBI's site selection process began in the spring of 1979 with the appointment of a Site Selection Committee under the chairmanship of Walter F. Bauer, President, Informatics, Inc. The Committee sent letters to the presidents of forty-one major universities having programs in computer science and technology across the country asking for indications of interest. About half of these responded positively, and several other academic institutions, having read about CBI's site search in the CBI Newsletter and other publications, asked to be considered as well. The Committee developed a "Proposal Invitation" which was sent to all interested organizations.

Thirteen universities submitted proposals and, of these, the University of Michigan and the University of Minnesota were chosen as "finalists." A team of members of the Site Selection Committee visited these universities in mid-April and the Site Selection Committee chose Minnesota by written ballot soon thereafter. The Board of Trustees endorsed the selection.

Tomash remarked that he was "most gratified by the high level of interest in the Institute indicated by the many fine proposals that were received. Selection of the finalists and the final selection itself was most difficult."

Members of CBI's final Site Selection Committee were: Walter F. Bauer, (Chairman), Informatics, Inc.; Paul W. Berthiaume, New York Times Information Bank; James W. Birkenstock, IBM (retired); Walter Carlson IBM; Albert S. Hoagland, AFIPS; Robert P. Multhauf, Smithsonian Institution; E.R. Piore, IBM (retired); Joan N. Warnow, Center for History of Physics; and Spencer Weart, Center for History of Physics.

For further information, contact Mr. Paul Armer at (415) 328-0984.

ERRATUM

MEETING OF THE SECTION "SCIENCE STUDIES" OF THE GERMAN SOCIOLOGICAL ASSOCIATION, BERLIN, 18-20 APRIL 1979. The report on this conference in 4S Newsletter 5, 2 (Spring 1980): 34-35 unfortunately failed to mention that this was a joint conference with the Science Center Berlin and that it was organized by Doris Janshen (Berlin), Otto Keck (Ulm) and Wolff-Dietrich Webler (Bielefeld). The conference papers are now made available in a book Technischer und sozialer Wandel - eine Herausforderung an die Sozialwissenschaften, edited by the conference organizers (forthcoming from Anton Hain, Königstein, Ts., West Germany).

O. Keck
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Universität Ulm

THE ANNALS OF THE HISTORY OF COMPUTING

The Annals of the History of Computing, which began publication in July 1979, focuses on the history of computing by including contributions from individuals who participated in, or witnessed, the events and decisions that have shaped our present computing environment.

The Annals is the outgrowth of a study by the AFIPS Publications Committee, chaired by Aaron Finerman, on creating a journal to record vital contributions that make up the history of computing. The Annals is published under the direction of an Editorial Board composed of distinguished computer professionals and historians drawn from the United States and other nations. Heading the Board as Editor-in-Chief is Bernard A. Galler of the University of Michigan. Valuable liaison is also furnished by the AFIPS History of Computing Committee.

Recent issues of the Annals have contained these articles, in addition to departmental contributions:

Vol. 1, No. 1 (July, 1979)

The BINAC Nancy Stern

The History of FORTRAN John Backus

Early Work on Computers at Bletchley I. J. Good

The History of the JOHNNIAC Fred Gruenberger

Vol. 1, No. 2 (October, 1979)

An Annotated Bibliography on the Origins of Computers B. Randell

The Birth of an ERA: Engineering Research Associates, Inc. 1946-1955 E. Tomash
and A.A. Cohen

Vol. 2, No. 1 (January, 1980)

Programming the EDSAC Martin Campbell-Kelly

Early Polish Digital Computers R. W. Marczyński

Errors in the History of Computing N. Metropolis and J. Worlton

Computing by Cryptologic Organizations Samuel S. Synder

"WHAT IS A SURVEY?"

A nontechnical pamphlet about surveys has been issued by the American Statistical Association. Entitled "What Is A Survey?", this publication is designed to help people understand why surveys of human populations are made, how they are made and the uses to which they are put. The purpose of this pamphlet is to fill a gap in the information available to the average person about surveys.

The 24-page publication contains separate sections on the characteristics of surveys, how a survey is carried out, how the results of a survey are used, and what enters into survey budgets. A closing section tells the reader where to get more information about surveys and survey research.

The authors of this pamphlet are Robert Ferber, Director, Survey Research Laboratory, University of Illinois; Paul Sheatsley, Survey Director, National Opinion Research Center, University of Chicago; Anthony Turner, Chief, International Mathematical-Statistical Staff, U.S. Bureau of the Census; and Joseph Waksberg, Vice-President of Westat, Inc.

Copies (up to 50) are available without charge from the American Statistical Association, 806 15th St., N.W., Washington, D.C., 20005. Bulk copies (beyond 50) are twenty-five cents each.

APPOINTMENT

P. THOMAS CARROLL has accepted an appointment as Assistant Professor of History in the Department of History and Political Science, Rensselaer Polytechnic Institute, Troy, New York 12181; effective September 1, 1980.

FORTHCOMING MEETING

There will be a session on bibliometrics and citation analysis at the annual meeting of the American Society for Information Science on October 9, 1980 in Anaheim, California. For further information contact: ASIS, 1010 Sixteenth Street, N.W., Washington, D.C. 20036; 202-659-3644. The session is organized by Pauline Atherton, School of Information Studies, Syracuse University, Syracuse, New York.

4S Society for Social Studies of Science

The 4S Newsletter is published four times each year at the Department of Sociology, Southern Illinois University, Carbondale, Illinois 62901, and sent to all members of the Society for Social Studies of Science. Membership is on a calendar year basis. Membership dues (\$10 for professionals, \$5 for students) and institutional subscriptions (\$20) should be sent to: The Secretary/Treasurer, 4S, Department of Sociology, Indiana University, Bloomington, Indiana 47401.

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