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4S Society for Social Studies of Science

Summer 1977 Volume 2 Number 3

4S Society for Social Studies of Science

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4S: PROGRESS AND PROSPECTS

Editorial.

It is now more than three years since plans were laid for a meeting of those active in social studies of science, in connection with the Montreal Congress of the International Sociological Association. Following that meeting, an informal committee was established. Its members met on several occasions in the academic year 1974-1975, and drafted the charter of an as-yet-unnamed and uncreated society. On 26 August 1975 their labors were rewarded, when over fifty scholars assembled in San Francisco to amend and ratify that charter, to elect a President and Council and to declare themselves members incorporate in 4S.

This social act of creation was followed by a pause of satisfied astonishment as we all considered what we had wrought and whether this beloved child would live and prosper. 4S existed. But would its membership grow? Would it be able to attract a truly international body of scholars? Would it become a genuinely interdisciplinary society? Would it find acceptance in the world of learned societies? Even if the answer to these four questions was yes, could 4S organize intellectually satisfying meetings? Could it find a means of publication which would unite its members in their common enterprise and usefully inform them of relevant work in unfamiliar disciplines? The answers are not yet all in. However as 4S approaches its second birthday it is possible to make a more encouraging report than anyone dreamed would be possible, during those first anxious days of prenatal discussion.

The membership of 4S stands at five hundred individuals and it continues to grow. That membership is distributed around the globe, from Australia to the U.S.S.R. and from Argentina to Japan. In all, some twenty-four countries are represented. There is thus no question that 4S is fulfilling its first aim, in bringing together in a truly international society, scholars involved with social studies of science. 4S is also steadily becoming an interdisciplinary venture. Analysis of the first one hundred responses to the Membership Survey shows the following disciplinary distribution: sociology and social psychology, 36; history, 22; natural sciences, 12; political science and public administration, 7; philosophy, 6; economics and business, 4; other disciplines, 13. No one discipline commands a majority of our members. While history and sociology together account for something over fifty percent, there are healthy numbers from areas as diverse as industrial administration and the biological sciences.

Given such variety, it is encouraging if not surprising to discover that members display a strong tendency to cluster, when asked to name their research specialties. "Science, technology and public policy" heads the list, closely followed by "social impact of science and technology," "sociology of specialties and disciplines," "sociology of science, general," "impact of society on science and technology" and "science as a cultural mode." These six specialty areas together account for forty percent of the responses -- the rest being widely distributed among the thirty other specialties listed. This evidence suggests that 4S is fulfilling its aim of helping to convert an impressive congeries of disparate programs of research into an even more impressive composite field of disciplined inquiry.

Further evidence to that end may be found in the success which attended the First Annual Meeting of the Society, in Ithaca in November 1976. The experience gained at that meeting, the proposals generated and the suggestions offered have all been of the greatest use to the Council and to the Program Committee. Arrangements for the Second Annual Meeting are detailed elsewhere in this issue. The

Editorial (Continued).

list of outstanding speakers and the subjects of the various sessions and addresses together convincingly display how 4S is learning to organize intellectually significant meetings. That emerging ability has now received its first official recognition in the vote by the American Association for the Advancement of Science that 4S be admitted as an Affiliated Society.

One final indicator of the developing state of 4S -- and one worthy of note to the student of social forms -- is the enthusiastic approval given by the membership to the various charter revisions aimed at providing wider representation in and stronger structure for the Council and Committees of the Society. No doubt still further revisions will be needed as 4S grows and matures. (The revised charter is printed elsewhere in this issue.)

As with the membership, meetings and structure of the Society, so with its publications. Volume 1 number 1 of the 4S Newsletter consisted of all of six pages. The Newsletter's growth from those small beginnings to its present state is the work of many hands. The Editorial Advisors -- representing Europe, North America and Australasia -- have been an indispensable source of support. The Council has offered steady encouragement. Colleagues, printers, secretaries and students have cooperated cheerfully in the production process. Members have volunteered their time and talents in reporting on appointments, promotions, job openings, program developments and publications. The 4S Newsletter has thus begun to serve as a useful vehicle for disseminating news of common interest to people trained and active in widely scattered disciplines and institutions.

More important, members have been generous in giving their energies to the development of those aspects of the 4S Newsletter which more directly serve our common intellectual goals. The "Review Symposium" format and the inclusion of "Thought and Opinion" have met with an encouraging response. They have begun to facilitate dialog between, and enriched understanding of, those different methodological approaches, analytical techniques and disciplinary orientations within which our common research problems are located. These aspects of the 4S Newsletter have also stirred a wider interest, as evidenced by the trickle of institutional subscriptions that is now developing.

It is therefore with a lively sense of anticipation of new developments and deep thankfulness for help received that we hand on the editorship of the 4S Newsletter to a new team. That team will consist of Jerry Gaston (Southern Illinois University, Carbondale), Henrika Kuklick (University of Pennsylvania) and Henry Small (Institute for Scientific Information, Philadelphia). Together they encompass a wide variety of disciplinary and specialty expertise ranging from citation studies to the sociology of knowledge. Nonetheless, they will need the continuing aid and counsel of the Society's members as they seek to make the 4S Newsletter an instrument of greater intellectual value. We are confident that with that aid and counsel, the publications of 4S may look forward to many more happy birthdays.

Arnold Thackray
Daryl Chubin

Election Results. Elected to two-year terms on the Council were Michael Moravcsik, Nicholas Mullins, Eugene Skolnikoff, Patricia Woolf, and Dorothy Zinberg (see profiles below). The five charter revisions were overwhelmingly approved. The revised charter is printed on pages 41-42.

Michael J. Moravcsik (Ph.D. Cornell 1956, Physics)
- a. Professor of Physics, Institute of Theoretical Science, University of Oregon
- b. Science in developing nations; measures of scientific productivity; theoretical high energy physics
- c. Science Development: The Building of Science in Less Developed Countries (1975); "The crisis in particle physics," Research Policy (1976)

Nicholas C. Mullins (Ph.D. Harvard 1967, Sociology)
- a. Associate Professor of Sociology, Indiana University
- b. Development of scientific disciplines and specialties; sociological theory
- c. "The group structures of two scientific specialties: a comparative study," (with others) forthcoming in American Sociological Review; "The National Science Foundation: an agency responding to changes in science," (with others) Social Studies of Science 5 (1975)

Eugene B. Skolnikoff (Ph.D. MIT 1965, Political Science)
- a. Professor of Political Science and Director, Center for International Studies, Massachusetts Institute of Technology
- b. Science and government; science and international relations
- c. "Science advice in the White House: a continuing debate," (with H. Brooks) Science (10 January 1975); "The relevance of Intelsat experience for the organizational structure of multinational nuclear fuel facilities," forthcoming in Sheays and Lewis, eds., International Arrangements for Nuclear Facilities

Patricia K. Woolf (Ph.D. Johns Hopkins 1974, Communications)
- a. Adjunct Assistant Professor, Department of History and Sociology of Science, University of Pennsylvania
- b. Scientific communication; diffusion of information about health care innovations
- c. "Origins of the Gordon Conferences," in press; "The second messenger: informal communication in cyclic AMP research," Minerva 13 (1975)

Dorothy S. Zinberg (Ph.D. Harvard 1966, Sociology)
- a. Director of Seminars and Special Projects, Program for Science and International Affairs, Harvard University
- b. Career development of scientists; international exchange of scientists
- c. "Education through science: the early stages of career development in chemistry," Social Studies of Science 6 (1976); "Changing trends in research travel of Western European and American scientists," National Academy of Sciences, forthcoming in 1977.

SECOND ANNUAL MEETING: PRELIMINARY PROGRAM

TIME: 2:00 p.m., Friday, 14 October to 5:00 p.m., Sunday, 16 October 1977

PLACE: Science Center, Harvard University, Cambridge, MA, and Parker House Hotel, Boston, MA

PROGRAM SCHEDULE:

Friday	Morning	Council Meeting
	10:00-Noon	Registration
	2:00-5 p.m.	SESSION I: 3 invited papers
	6:00-10 p.m.	Cocktail Party and Banquet, Faculty Club, Harvard University
Saturday	9:30-Noon	SESSION II: 3 or 4 invited and contributed papers
	2:00-5 p.m.	SESSION III: 3 or 4 invited and contributed papers
	7:30-10 p.m.	SESSION IV: Contributed papers and discussions
Sunday	10:00-Noon	SESSION V: Contributed papers and discussions
	1:30-3:30 p.m.	SESSION VI: Contributed papers and discussions
	3:30-5 p.m.	4S Business Meeting

INVITED PAPERS will include the following:

A brief address by ROBERT K. MERTON (Sociology, Columbia)
GERALD HOLTON (Physics, Harvard), "Limits of Inquiry in Science"
EVERETT MENDELSOHN (History of Science, Harvard), "At the Borders of Science"
RICHARD R. NELSON (Economics, Yale), "Modelling Technical Change as an Evolutionary Process"
CHRISTOPHER WRIGHT (Office of Technology Assessment, Washington, DC), "Policy-Oriented Science Indicators"
MICHAEL MORAVCSIK (Physics, Oregon), "Science in Developing Nations"
RUTH AND JOHN USEEM (Sociology, Michigan State), "Scientific Research in the Philippines"
DOROTHY ZINBERG (Program for Science and International Affairs, Harvard), "Circulation of the Elite: Value Elements in European-United States Scientific Interactions"

CONTRIBUTED PAPERS are still being received and refereed, but Chairman Lowell L. Hargens of the Referee Committee reports that it is very likely that papers and perhaps sessions will be devoted to the following topics, among others: Specialties in Science (some papers - but not all! - using citation analysis), International Aspects of Science, Social Stratification and Mobility of Scientists, and Cognitive Styles in Science. For further details, contact Lowell Hargens, Department of Sociology, Indiana University, Bloomington, IN 47401.

Space will be made available for informal discussions of topics of interest to participants.

4S Society for Social Studies of Science

SECOND ANNUAL MEETING

FACT SHEET

Dates: Friday, 14 October—Sunday, 16 October 1977

Site: Science Center, Harvard University, Cambridge, Mass. and Dunfey's Parker House, Boston, Mass.

Hosted by: Program for Science and International Affairs, Harvard University

Accommodations: Dunfey's Parker House (located one block from the Park St. subway station)
60 School Street
Boston, MA 02108
(617) 227-8600

Room Rates: Single Occupancy--$36/day plus tax
Double Occupancy--$44/day plus tax

Reservations: Room reservation cards will be sent to those who indicate an interest in attending the conference (please see "Further Communications"). The cards should be returned directly to Dunfey's Parker House by September 22.

Travel Arrangements: Conference participants should make their own travel arrangements to Boston.
Air: Travel to Logan International Airport in Boston and take a taxi or subway to the hotel.
Train: Travel to Boston's South Station and take a taxi or subway to the hotel.
Bus: Both Greyhound and Trailways have bus terminals in Boston, located just a few blocks from the hotel.
The first and last sessions will be held at Dunfey's Parker House. The meetings on Saturday will be at Harvard University's Science Center, an 8-minute subway ride from the Parker House. (If possible, local transportation between the hotel and the Science Center will be provided.) Further information will be provided to those planning to attend.

(Tear Out And Mail)

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PRE-REGISTRATION

NAME: ______

ADDRESS: ______

ACADEMIC OR PROFESSIONAL AFFILIATION: ______

PRE-REGISTRATION FEE: Professional $15.00 ____ Student $ 5.00 ____

REGISTRATION AT MEETING: Professional $20.00 ____ Student $10.00 ____

DINNER, 14 October 1977: $10.00 ______ Yes, I plan to attend; ______No

I enclose a check (payable to Cornell University) for: ______

(over)

SECOND ANNUAL MEETING

FACT SHEET (Continued)

Further Communications: You will receive (or may already have received) a letter from the Local Arrangements Committee and a postcard to return to the Committee by August 10, indicating whether or not you plan to attend the meeting. Further information, including a hotel reservation card and a pre-registration form, will be sent to those indicating an interest in the conference. The pre-registration form must be sent to the 4S Secretariat at Cornell by September 15, and the hotel reservation card mailed to the hotel by September 22. Those interested in the conference who are not on the 4S mailing list may write to Dr. Dorothy S. Zinberg, Chairperson, 4S Local Arrangements Committee, Program for Science and International Affairs, Harvard University, 9 Divinity Avenue, Cambridge, MA 02138.

For Information: Concerning program--write to Professor Warren Hagstrom, Department of Sociology, University of Wisconsin, Madison, WI 53706. (608/262-6585)
Concerning local arrangements--write to Dr. Dorothy S. Zinberg at the above address or phone 617/495-4704.

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MAIL WITH YOUR CHECK TO:

4S SECRETARIAT
323 URIS HALL
CORNELL UNIVERSITY
ITHACA, NY 14853

THOUGHT AND OPINION

On the Use of Citations In Studying Scientific Achievements and Communication

Belver C. Griffith, Drexel University
M. Carl Drott, Drexel University
Henry G. Small, Institute for Scientific Information

Rather than trying to reply in kind to some recent, slightly polemical, criticisms of the use of citations, we will discuss the assumptions underlying the use of citations in the study of science. We shall attempt to explicate the principles underlying our work, with certain technical problems, and end with a brief panegyric on research programs and approaches to study of science and scientific communication.

Any value of citations and the restrictions on such value stems from the combined operation of several assumptions, three massive qualifications, and one critically important conjecture. The operation of these principles differs widely from field to field and from application to application. In discussing these principles we would note that the assumptions are very weak ones, and that their power derives from the stochastic nature of the world, laying the groundwork for true quantification only with very large files. For example, we can see a possible restriction in mathematics, where the literature base is small and each article contains few references. In such a field citations must be far more robust than in molecular biology, where the reverse holds on both dimensions.

Let's look now at four assumptions:

I A document x cited by document y is more likely to be judged as related in content to document y than one not cited.

If we were to eponymize, this might be called the Garfield-Kessler Assumption, since it underlies both the remarkable Science Citation Index service and the benchmark research at MIT. It's a hard assumption to gainsay.

II If there are two documents x_1 and x_2, and x_1 is cited by document y and x_2 is not so cited, x_1 is more likely to have been of use in the preparation of document y than x_2.

Let's continue to honor our intellectual forebears and designate this the Gross-Price Assumption, after the first major user of, and the user who has extracted amazing intellectual power from this assumption. Note again the extreme modesty of this assumption.

III If documents cite documents in common, they are more likely to be judged as related in content than documents which do not cite any document in common.

On The Use of Citations (Continued).

Finally: IV If documents x_1 and x_2 are cited by document y, they are more likely to be judged as related to one another in content than to document x_3, which is not so co-cited with x_1 and x_2.

This is, of course, the co-citation assumption and with a friendly, generous spirit of self-eponymization--already a tradition in this field--the first author would call this the Small-Marshakova-Griffith Assumption, after the first "mappers" to use this assumption.

These modest assumptions lay the groundwork for quantification and introduce other considerations, in particular, a series of necessary qualifications. These qualifications are massive, and give the user of citations fair warning that use is fraught with danger. (The "conjecture" is, however, quite powerful and offers the researcher hope.)

I Citation measures are only the by-product of a file, and their quality is directly related to the dimensions of that file and the care taken to develop the file.

II A series of complex social, psychological and bibliographical factors intervene between any intentions of the author to acknowledge precedent work or to recognize any form of similarity.

And, less terrifying:

III Citation measures critically reflect the scale of the literature, and slightly independently, the scale and pace of research activity, as well as norms and institutions within the specialty and discipline.

These very strong restrictive qualifications regarding the use of citation measures have been repeated as admonitions, but violated again and again in the literature. Later, we shall argue, however, that only rather serious violations matter.

What specifically do these qualifications mean?

(1) A file that is developed by the investigator must be fully described bibliographically and its dimensions must be justified as part of the study.

(2) If SCI or SSCI is used, the investigator should be sensitive to the continuing improvement in coverage, particularly with regard to recent volumes of SSCI and, according to Derek Price, for the SCI prior to 1967.

(3) The difficulties encountered with developing counts for individuals have been described elsewhere, and have become in certain areas of physics extremely difficult where the authorship includes 20-70 persons. For cognoscenti, we list

On The Use of Citations (Continued).

typical considerations: homonyms, fractional authorships, self-citations, alphabetical as opposed to attributive ordering of authors, the precise relation of the citation to the content of the citing document, and multiple spellings, or arrangements, of the author(s)' name(s). (Ironically, Derek Price is a principal victim of the last.)

(4) The investigator using SCI should be warned that errors normally originate in the citing document and, therefore exhibit far more creativity than typographical errors inherent in clerical operations, many of which are automatically eliminated.

(5) The psychological and social factors have to do with habits, conventions, and perceptions of the scientist, his research group, his specialty, etc. Accordingly, they can be either an object of study or a bother, depending on the goals of the investigator. Also, different measures may be affected differentially. (The relative contributions to hepatitic research of Baruch Blumberg and Alfred Prince, as recognized by the Nobel award to Blumberg, were reflected in total number of authorships among clustered documents on Australia Antigen, but not by number of citations to each author's most-cited article.)

(6) Bibliographical factors, alone or in conjunction with social and psychological factors, perturb citation measures. The pure case is the work of Marx, where citations to a numerically small set of documents have been exploded into chaos by differently dated, different language editions (SSCI). Complex factors are introduced by the lack of any necessary correspondence between the content of discovery, the documents reporting the discovery, and a consensual perception of those documents by the research community. From the Australia Antigen example we can select three documents: one, a seminal finding that there may be a relation between the presence of Australia Antigen and hepatitis; the second, five years later, reporting a frequent association; and a third, slightly later, reporting 100% association within reasonable experimental error. The three taken as a group constituted the "crucial experiment" for that specialty. Citations to the first soared on appearance of the second and third papers--a formal finding only interpretable by recourse to the content of the papers. But in this case, community consensus, not the greatest credit, went to the second paper. One can easily see that any particular pattern of citing these papers might be intimately bound to the style of the citing author. The omission, or near omission, of any particular paper in citations can only be interpreted in terms of the pattern of citations and the content of related papers.

(7) Much of the above, as well as differences in scale and differences in custom of citations, renders the count of citations to an individual paper a fragile measure of the value of the paper. Instead, the investigator should use all means at his disposal to determine the degree of consensus within the relevant community represented by a citation count and the nature of that consensus. While the "nature" of consensus may suggest another cop-out and our rushing to content to bail out the measure, we believe it possible to turn this concept into a formally derived measure (by combining citation counts with clustering) that considers both co-citation and the total frequency of citation for each document. To give a rough idea, while papers which report standard laboratory methods in biomedicine are highly cited, they do not connect to groups of other papers and no group of new papers grows about them. On the other hand, the Nobel-award winning work of Baltimore, Blumberg, and Temin was related to existing documents and became the center of developing clusters.

(8) The scientist of science must regard his assumptions regarding the existence of a type of literature or the relevance of a particular paper to a particular literature as an hypothesis, which, we venture, is likely to be incorrect. Beginning with Parker, Paisley and Garrett, investigators have been guided by strong hypotheses, independent of the inherent structure of the communication research literature, and found instead sociology and psychology.) Even greater strangeness may arise through uncritical acceptance of document assignment to specialties by bibliographies.

In all the above, we hope we make clear that the measures which are likely to be the most fragile are counts for the individual document and for the individual scientist. These are the measures which create the greatest sensitivity within the scientific community; and in part, our intention in writing this piece is to argue for the development and substitution of more sophisticated approaches.

We now turn to a conjecture of great amiability. It appears strong but may resist convincing proof.

> The quality and quantity of the scientific literature "channelizes." That is, a combination of social and probabilistic mechanisms ensure that most documents of a discipline, and nearly all documents of the highest quality, appear in a limited number of sources (i.e., journals in the natural sciences). Furthermore, all such important sources may be readily recognized and ranked along this quality dimension by citation counts.

This idea certainly dates from the Garfield-Bourne controversy over the number of scientific journals which should be covered by information services, say, 3000 ± 1000 (Garfield) as opposed to 30,000 ± 2000 (Bourne); these ideas are implicit in Price's writing, too. The work of Narin on individual disciplines fully explores and supports this principle empirically. However, the power of the mechanisms involved has not been emphasized sufficiently, nor has the central importance of this conjecture to citation research been indicated.

The likely truth and power of this conjecture is essential; if all journals (60,000) voted equally, research would be impossible and of course, SCI and SSCI would lose much of their usefulness as information services. Perhaps most strangely, the power of this conjecture has permitted persons, totally ill-equipped methodologically, to do valid work simply because they cannot avoid the "centers" of literatures.

This conjecture can be tested rather simply. Results of even a two-hour study within a departmental physics library--given expertise in sampling-- would be sufficient for the highest ranked journals to be ordered highly reliably and with perhaps 70-80% accuracy, as compared to a full study of our best data, the Journal Citation Report. Our only personal misgivings about use of this conjecture focus on the difficulty of finding starting points without language or national bias.

This conjecture, which appears correct but must again be approached and used with caution, completes the set of principles which, for us, underlie citation research.

N O T E

The seminal work on statistical studies of scientific literature and its bearing on scientific achievement are Derek Price's Little Science, Big Science (New York: Columbia, 1963) and Price's article "Networks of Scientific Papers" (Science 149, 1965, 510-515). We feel the second of these more clearly indicates a research program based upon citations, and in a sense, our own work is a continuation of that program. Another broader approach is represented by Francis Narin's book, Evaluative Bibliometrics (Cherry Hill: Computer Horizons, Inc. 1976). Recent reports, as yet unpublished, critical of the use of citations have appeared, one by David Edge (Science Studies Unit, Edinburgh University), and another by Daniel Sullivan, *et al.* (Research Program on Social Analyses of Science Systems, and Carleton College). A complete bibliography is available from M.C.D. at cost. The work of B.C.G and H.G.S. on the present paper was supported by PHS Grant number 5 R01 LM00911-05.

Why I am not a Co-Citationist

David Edge

Science Studies Unit
Edinburgh University

Some of us make modest use of citation analysis in our work,[1] but remain radically skeptical of the claims of those who devote more prime time and energy to the elaboration of such methods. Why do we not accept the faith? Why can we not do the proper Kuhnian thing and let the "paradigmatic achievements" of the new quantitative methods define the field for us - posing our fundamental problems, laying down agreed techniques, prefiguring acceptable answers, and unrolling a "progressive research program"? I suggest that what is at issue here is essentially a difference of aim. My conception of "doing the sociology of science" allows citation analysis, at best, only a very peripheral role. I will try to outline my position as succinctly as possible.

Why I am not a Co-Citationist (Continued).

1) Let me first identify and reject a claim that seems to me to lurk, if only implicitly, behind these quantitative methods: essentially, the claim is that, in transcending the "limited, subjective and biased" perspective of individuals, and in giving some "public, aggregated, objective and unbiased" account, these measures have, as it were, "a preferred logical status". They are more "objective", more "reliable"; they can be used to "correct" participants' accounts; they can define "what really is (or was) the case", and can arbitrate between conflicting accounts; and so on. These quantitative procedures are often labelled "scientific", and the sociology (or history) to which they give rise is "scientific sociology" - as opposed, presumably, to qualitative, individualistic and "biased", "incomplete" sociology. Garfield, Sher and Torpie, for instance, in their pioneer 1964 paper, state:

> The writing of history is subject to much human error in spite of the dedication and relatively rigorous standards held by the professional historian... Historical description must therefore fall far short of an ideal. We can only strive to develop methods that bring us somewhat closer to the truth ... The historian, in describing the progress of science, is limited by his own experience, memory, and the adequacy of the documentation available. His subjective judgment primarily determines the historical picture of the development of events.[2]

And their paper concludes:

> It is felt that citation analysis has been demonstrated to be a valid and valuable means of creating accurate historical descriptions of scientific fields ...[3]

Small, in his most recent paper, makes a similar claim for the preferred status of co-citation analysis in preparing specialty bibliographies:

> The principal difficulty ... is that it is almost impossible to establish precise criteria as to what should or should not be included within the boundaries of the subject, and the temptation is to apply present-day criteria to earlier literature. [Co-citation analysis] uses a clustering algorithm to establish these boundaries; it involves no subjective decisions on what is to be included or excluded from the specialty literature.[4]

If this is so, then why bother to "validate" co-citation studies?[5] Differences between the co-citation results and those derived from other sources are only to be expected, and it is implicit in the method that preference, in such cases, should be given to the former over the latter. However, co-citation practitioners lose nerve at this point: not only do they undertake validations, but they allow errors. Small, for instance, says, of a paper which was missed by his computer in his study of collagen research:

> The effect of thresholding was, in this case, to exclude an important and relevant item.[6]

Was the decision that this item was "important and relevant" (and therefore, presumably, that it should be included in the specialty bibliography) a "subjective" or "objective" one?

Why I am not a Co-Citationist (Continued).

When Griffith, Drott and Small state, with emphasis, that ...

> ...the investigator should use all means at his disposal to determine the degree of consensus within the relevant community represented by a citation count and the nature of that consensus ...,

... they are abandoning the notion of a preferred logical status for citation methods. We would then be agreed that citation and co-citation figures are just part of the range of empirical data available to the historian and sociologist - no more, and no less. Where we would still differ would be over the relative weight to be assigned to citation measures, within the context of the entire range of information.

2) The advent of the co-citation technique has involved a fundamental shift of emphasis which I welcome. Previously, the citing of B by A was taken to reflect an influence of B on A. Crane, for instance, claims that:

> Within a research area, frequent citation indicates that a paper contains information that has been useful to other members.[7]

And Meadows lists "two basic assumptions" in citation analyses:

> (i) that the papers selected for citation are those which have been important in an investigation; and (ii) that citations are indicative of influence via the literature.[8]

However, in co-citation analysis, which is superficially so similar, the only assumption is that the citing of B and C together by A implies that, in A's perspective, the work of B and of C are related. In other words, co-citation "maps", strictly speaking, reflect only the perceptions of authors. But those using co-citation analysis act as if co-citations of B and C were evidence that B and C are related by communication ties, and the authors "clustered" by co-citation are interacting groups. Small and Griffith, for instance, in one of the pioneer co-citation papers, talk of "the mapping of specialties, to show their internal structures and their relationships to one another", and they continue (reiterating a version of the preferred logical status claim):

> Many of the relationships we have uncovered are, of course, known to the specialists themselves, since they were established by their own citing patterns, but the perspective this method offers is far broader than can be achieved by any individual scientist. This is the crux of the method: the observed relationships are in substance those which have been established by the collective efforts and perceptions of the community of publishing scientists. Our task is to depict these relationships in ways that shed light on the structure of science.[9]

3) I, too, wish to "shed light on the structure of science", and hope to do so via an elucidation of "the collective efforts and perceptions" of the scientific community. However, it is at this point that essential differences emerge.

One rationale often advanced for developing co-citation (and other quantitative) methods proceeds by analogy: until patient measurement had established the form of the gas laws (PV = RT), there was no "problem" for the kinetic theory of gases to "solve"; similarly, it is argued, quantitative studies of science are necessary to define the "problems" which sociological theory has then to solve . I reject this argument. To me, the behavior of scientists in the conduct of their research provides an abundance of problems of much more obvious importance than any correlations contained in a computer printout. Whenever a scientist (or a research group) decides to develop a new technique, or to pursue a fresh and unexpected phenomenon, or to adopt a perhaps unfashionable theoretical approach, there is a sociological problem: each decision brings together "cognitive" (intellectual, technical, cultural) and "social" factors, and to me, "to do the sociology of science" is to explicate such decisions, and to explore the "social grounding" of their rationality.[10]

This task starts from the "participants' perspective." Citations could be one relevant source of information of participants' perceptions. But every decision is particular: citation and co-citation analysis, in striving to accumulate and average, destroys the evidence we need of individual variations. It is often because individual scientists and groups do not share the consensus view, defined by (inter alia) co-citations maps, that crucial innovative decisions are made.

It is worth dwelling on the importance of particularity in studies in the sociology of scientific knowledge. Such is the scale of any scientific specialty, there is a limited number of researchers (and even more strikingly, of groups), who might be considered to share (roughly) similar cognitive constraints on their strategic decisions. In the early years of radio astronomy, there were only three comparable groups - Cambridge, Jodrell Bank and Sydney. In Astronomy Transformed, we present an analysis of the social structure of the Cambridge and Jodrell Bank groups, and attempt to relate these structures to technical developments at the two centers.[11] In "mapping" the social structures, we compiled a composite picture, melding interview material with quantitative measures - including mutual citation patterns. We found that the citation (and co-authorship) picture agreed closely with that derived from our other sources: in particular, the central influence of Ryle on the Cambridge group was clearly confirmed. Unfortunately, any attempt to repeat this approach on the Sydney group fails; Pawsey, the Sydney group leader, was, by unanimous agreement, an influence of comparable stature to Ryle at Cambridge; but there is no trace of this in the Sydney citation and co-authorship patterns. A citation analyst can brush this aside, as a mere individual variation which is swallowed up in the statistics. But, to sociologists of my ilk, experiences like this merely confirm the unreliability, and very subsidiary status, of citation measures.

4) I mentioned earlier the claim that co-citation analysis can give an "objective" decision on the composition of specialty bibliographies. I am puzzled as to why such "arbitration" is thought to be necessary. I know that the point raises considerable concern among my colleagues,[12] but I remain relatively phlegmatic. To me, the idea of a "specialty" (and of a discipline) is a social construct, a concept which allows actors to make transient sense of their experience, and to orient themselves accordingly.[13] I would expect related actors to have broadly similar, but not identical, perceptions of their collectivity. So I would expect a wide measure of agreement, but no detailed consensus, on the "boundaries" of the specialty. The "correct definition of a specialty" is, to me, 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 perspective on scientific collectivities makes research more difficult than it

would be under more simple-minded premises, but then I happen to believe that sociology is difficult. And I am comforted by the thought of the great British "middle class": here is a social construct central to any analysis of British society, which undeniably "exists" - but which stubbornly resists the attempts of empirical researchers to "define its boundaries"! I reject any technique which appears to remove from empirical sociology this "constant-triangulation-on-shifting-sands" character, and to substitute some illusory "solid foundations"

5) One final, general objection. Citation (and many other quantitative) methods draw entirely on features of formal scientific publications. Griffith, Drott and Small, in their title, refer to "studying scientific achievements and communication"; yet, in their paper, they pose problems and hypotheses in terms of the properties of literature and documents - not of the behavior of scientists. But surely the interesting questions to sociologists of scientific knowledge (and most historians of science) concern the vast "informal" area of scientific behavior (what we might call the "soft underbelly of science"), where interpersonal influences and negotiations lead to intimate choices of theory and technique, and hence determine the precise direction of the development of scientific culture itself. Studies of communication in science emphasize the importance of informal communication, and suggest that the formal and informal areas are different in kind.[14] To attempt to use clues from the formal area (eventually) to suggest explanations applicable to the informal area is, I submit, to reverse the necessary explanatory logic. Explanations of scientists' behavior in the informal domain should surely be extended to include within their scope aspects of "formal behavior" - including the relatively trivial behaviour of adding citations to papers.[15] But, quite apart from "explanatory logic", it is simply my judgment that illumination is more likely to accrue "this way round". Certainly, I cannot say that co-citation studies to date have generated any striking insights - even heuristically. But I am willing to be convinced. As Liza Doolittle (who understood epistemology profoundly) put it, in "My Fair Lady": "Show me!".

6) And one final, general point. It seems to be fashionable to say (usually with an air of rather smug satisfaction) that "the sociology of science is a self-exemplifying specialty".[16] Whether you find the insight comforting depends, of course, on the kind of sociology of science you profess: mine is reflexive, but essentially conflict-ridden, and the present debate is a "self-exemplification". To use Mannheim's terminology, the purveyors of quantitative methods seem to me to embody an "enlightenment" (or "natural law") style of thought, while the "participants' perspective" approach is in the "conservative" (or "romantic") style.[17] Placing the respective parties in their social situations, adding positions of relative power and authority (and mutual perceptions of threat), and reflecting on the form of the rhetoric in which this debate is couched,[18] I would venture that our styles are rooted in differences too deep and incommensurable to be bridged by brief academic exchanges - however clear and rational. Since the integrity of 4S is at risk, I find this outlook disturbing. But there is one crumb of comfort: the "participants' perspective" approach generates a self-awareness which allows disputants to live with their differences. What other faith do you need?

Footnotes

1. For confirmation, see D.O. Edge and M.J. Mulkay, Astronomy Transformed: The Emergence of Radio Astronomy in Britain (New York: Wiley-Interscience, 1976), Chapters 2 & 9.

Why I am not a Co-Citationist (Continued).

2. E. Garfield, I.H. Sher and R.J. Torpie, The Use of Citation Data in Writing the History of Science (Philadelphia: Institute for Scientific Information, 1964), 1.

3. Ibid., 33.

4. Henry G. Small, "A Co-Citation Model of a Scientific Specialty: A Longitudinal Study of Collagen Research", Social Studies of Science, Vol. 7 (1977), 140.

5. For details of some recent such "validations", see Social Studies of Science, Vol. 7, No. 2 (May 1977).

6. Small, op.cit. note 4, 156.

7. Diana Crane, Invisible Colleges (Chicago: The University of Chicago Press, 1972), 19.

8. A.J. Meadows, Communication in Science (London: Butterworths, 1974), 171.

9. H. Small and B.C. Griffith, "The Structure of Scientific Literatures, I: Identifying and Graphing Specialties", Science Studies, Vol. 4 (1974), 39-40.

10. For examples of such analyses, see D.A. MacKenzie and S.B. Barnes, "Biometrician versus Mendelian: A Controversy and its Explanation", Kölner Zeitschrift für Soziologie und Sozialpsychologie, Vol. 18 (1975), 165-96; Jonathan Harwood, "The Race-IQ Controversy: A Sociological Approach, I: Professional Factors", Social Studies of Science, Vol. 6 (1976), 369-04, and 'II: External' Factors", ibid., Vol. 7 (1977), 1-30. For a related approach, see H.M. Collins, "The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics", Sociology, Vol. 9 (1975), 205-24.

11. Op.cit. note 1, Chapter 9.

12. For an example of such public anguish, see S.W. Woolgar, "The Identification and Definition of Scientific Collectivities", in G. Lemaine et al. (eds), Perspectives on the Emergence of Scientific Disciplines (Paris and The Hague: Mouton, and Chicago: Aldine, 1976), 233-45.

13. On this point, Warren Hagstrom's remarks on "Sources of Resistance Among Scientists to Their Classification on the Basis of Specialty", 4S Newsletter, Vol. 2, No. 2 (Spring 1977), 15-16, are relevant and helpful.

14. See, for example, W.D. Garvey and B.C. Griffith, "Scientific Communication: Its Role in the Conduct of Research and Creation of Knowledge", American Psychologist, Vol. 26 (1971), 349-62; B.C. Griffith and A.J. Miller, "Networks of Informal Communication among Scientifically Productive Scientists", in C.E. Nelson and D.K. Pollock (eds),

Why I am not a Co-Citationist (Continued).

Communication among Scientists and Engineers (Lexinton, MA: Heath Lexington Books, 1970), 125-40; H. Menzel, "Informal Communication in Science: Its Advantages and Its Formal Analogies", in D. Bergen (ed.), The Foundations of Access to Knowledge (Syracuse, NY: Syracuse University Press, 1968), 153-67; and F.W. Wolek and B.C. Griffith, "Policy and Informal Communications in Applied Science and Technology", Science Studies, Vol. 4 (1974), 411-20. (The latter contains many useful further references.)

15. See G. Nigel Gilbert, "Referencing as Persuasion", Social Studies of Science, Vol. 7 (1977), 113-22. The need for scientists to have some framework within which to persuade each other may account for the relatively stable and consensual perceptions of specialty boundaries - and hence for the formal properties of co-citation maps.

16. The phrase reappears in the 4S Newsletter, Vol. 2, No. 1 (Winter 1977), 7.

17. For those unfamiliar with this approach, a succinct account is given by David Bloor, Knowledge and Social Imagery (London: Routledge and Kegan Paul, 1976). See also the two papers by J. Harwood cited in note 10.

18. In such disputes, claims about techniques (and, indeed, the techniques themselves take on an ideological status) i.e., they are claims, alleging to reflect "objective accounts of the world", which covertly advance and consolidate social interests.

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COMING ATTRACTIONS

In the Fall 1977 issue:

Second Annual Meeting:
Full Program and Abstracts of Papers

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Recent Dissertations

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Some Unresolved Issues in The Psychology of Science--A Research Agenda

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The growth of research bearing on the psychology of science has been such in recent years that it would be literally impossible in a brief review to even attempt to do it justice. My purpose here must therefore be a more modest one, and at the same time, a bolder one. I should like to suggest and briefly comment on seven themes or problem areas that are richly deserving of further study from a psychological perspective. The themes are: (1) cognitive styles of conducting scientific inquiry; (2) the psychological basis of the so-called "institutional" norms of science; (3) the symbolic life of science in the very special sense of the affective, socio-emotional images scientists have of themselves and the "things" they study; (4) the psychological development and internalization of the reward and "punishment" systems of science; (5) the spouses, marriages, and children of scientists; (6) the possibility of using organizational development or team-building exercises to help scientists confront and resolve deep value issues, and stylistic differences among them; and (7) the institutionalization of the psychology of science within the psychological community and establishment.

Not only do these themes represent very different kinds of issues, but they are at very different stages of development. Some have received comparatively extensive attention or are currently the subject of considerable interest; others have been virtually ignored or are in the most primitive stages of recognition, interest, and development. All, I would contend, are (or should be) of major interest and importance if the psychology of science is to achieve the status, recognition, and development which are long overdue.

1) Of the seven issues, only the first has received noticeable study from a psychological perspective. Even here, however, the number of questions raised far exceed the number of firm answers that have been produced. In the area of cognitive styles, the work of Gerald Gordon and that of his colleagues and students stand out.[1] Drawing as well as building on the earlier work of Donald Pelz at Michigan, Gordon has been able to identify four distinct cognitive styles of doing science. They are: (i) the Integrator, (ii) the Problem Identifier, (iii) the Problem Solver and (iv) the Technician. These four types not only differ systematically in their psychological make-up but also in their fundamental approach to science and to problems. The Integrator is capable of recognizing, shaping, and forming new and novel problems and possesses the necessary technical skills to solve them via conventional analytic techniques. Whereas the Problem Identifier is only good at the problem forming part of the process, the Problem Solver is only good at the solving part. The Technician, on the other hand, is good at neither of these and must literally be led or closely directed.

Some Unresolved Issues (Continued).

Gordon and his colleagues have not only been able to demonstrate that these four types, as one would suspect, have different production rates and patterns within a field, but that they are also not equally distributed across different scientific fields and specialties. Even more provocative is the just emerging (and for this reason, tentative) evidence that there is a strong interactive relationship between the cognitive style or "type" of a mentor and his pupil. In a word, if the senior professors at the so-called top-ranked and prestigious universities "tend" to be Integrators, then they may tend to choose Problem Solvers to work under them. This choice ensures further technical development of the mentor's point of view, and hence its perpetuation. Also, a Problem Solver is less likely to question the given point of view and hence to rebel against it and strike out on his or her own.

This conjecture raises a host of fascinating questions. Are scientific cognitive styles "learned" or are they merely extensions of an individual's dominant personality? Or both? How rare is the Integrator-Integrator match-up? That is, is one more or less likely to become an Integrator if one studies under one? Do mentors consciously or unconsciously pick pupils to complement their own style? If so, how and by what psychological process?

2) I shall not dwell in detail on the question of the psychological internalization of the norms of science. I shall merely note that similar questions can be raised regarding how scientists come to psychologically internalize the norms of science--whatever the norms of science eventually turn out to be since, as recent debate shows, there is no longer widespread agreement concerning earlier formulations.[2]

3) The third area, the symbolic and/or affective images that scientists form of themselves, their work, their colleagues, students, objects of study, etc., is one that has long intrigued me. I am astounded that so little attention has been paid and that so little continues to be paid to this fascinating and important area. Recently, a colleague and I[3] asked a sample of social psychologists to characterize such personages as the experimental psychologist, the social psychologist (i.e., themselves), the clinical psychologist, the college sophomore, the research subject, the APA (American Psychological Association) journal referee, the funding agent, and last but not least, the rat, who along with the college sophomore ranks as one of the prime studied objects of the psychological profession as a whole. The results, needless to say, are interesting. They show an incredible psychological distance between the social psychologist and the primary objects he studies, human subjects. There is a closer affinity between the social psychologist and the rat than there is between the social psychologist and the college sophomore!

One can only wonder if a similar relationship holds for other fields and specialties. Does it make sense to talk of the psychological distance between physicists and atoms? I don't know; we do know, however, that the best scientists have always freely speculated, if not openly fantasized, about the "nature of nature" and in highly metaphorical terms.

4) A recent book by Michael Mahoney, Scientist as Subject: The Psychological Imperative,[4] is one of the few of which I know that explicitly deals with the notion of the "punishment" system in science. Mahoney suggests as did Maslow

Some Unresolved Issues (Continued).

before him, that there is much in the training of scientists that must be recognized as repressive. Mahoney develops a model of graduate education based on psychological principles designed to promote growth and creativity. One can only wonder at this time what the relationship between Mahoney's ideas and Gordon's cognitive types would be. Is it possible to train Integrators? Are they less repressive in their lifestyle, background, doing of science, etc.?

5) Another neglected area has been the question of the relationships, if any, among work, marriage, and lifestyle. Again, some colleagues and I recently undertook to investigate this.[5] It would seem that if the family of a scientist reaps some special rewards for one of its members being a scientist, then they pay some special costs as well. It tends to be left up to the female (whether she is a scientist or not) to care for the affective needs of the family. Is this but one example of the costs that science in general has had to pay for its repeated and special emphasis on emotional disinterestedness as a necessary attribute for doing science?

6) This leads to the general issue as to whether organizational development or team-building exercises can be fruitfully used to help scientists develop a better psychological appreciation for the feeling side of life? Would it help them to better appreciate the place of, and indeed even the necessity for, interpersonal conflict in science? Also, would it thereby help them to resolve such conflicts?

7) Finally, I am led to what is perhaps the most basic issue of all. Why has the field of psychology failed to institutionalize the psychology of science in the way that sociology has successfully institutionalized the sociology of science? Is it due to fundamental differences between the subject matters of the two disciplines or their methodologies? Is it due to the fact that psychologists can perform more tightly controlled and confined experiments than can sociologists? Except for psychoanalysis, psychology was not required to develop an intense interest in the psychology of knowledge in order to develop. In retrospect was this more of a liability or a benefit? One can only speculate what questions we might now be investigating were the psychology of science in a more advanced state of development and recognition. Perhaps this research agenda has been a step toward that state.

Footnotes

1. Gerald Gordon and Edward V. Morse, "Creative Potential and Organizational Structure," Academy of Management Journal, Vol. 12 (1969), 37-49; E.V. Morse and G. Gordon, "Cognitive Skills: A Determinant of Scientists' Local-Cosmopolitan Orientation," Academy of Management Journal, Vol. 17 (December, 1974), 709-723; B.T. Edison and L. Beckman, eds., Science as a Career Choice, Russell Sage, New York, 1973.

2. See Michael Mulkay, "Norms and Ideology in Science," Social Science Information, 15 (1976), 637-656.

3. Ian I. Mitroff and Thomas B. Bonona, "Sophomores, Rats, and Referrees: How Social Psychologists View Their World, " unpublished paper.

4. Ballinger, Cambridge, Massachusetts, 1976.

5. Ian I. Mitroff, Theodore Jacob, and Eileen Trauth Moore, "On the Shoulders of the Spouses of Scientists," Social Studies of Science, 1977, forthcoming.

Social Studies of Science in the Netherlands

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Social studies of science as a collective intellectual enterprise are only in a beginning stage of development in the Netherlands. In very recent years, one can discern in this country a growing interest in the sociology, psychology, economy, and more generally, the science of science. In the particular case of the history of science, one can perhaps speak of a tradition. This is indicated by the fact that already in the period directly after World War II, at several Dutch universities, chairs in the history of science were founded; at the present time there exists only in one University (the Free University of Amsterdam) a center of teaching and research which is specifically engaged with social studies of science. In this brief report on the Dutch state of affairs, I will, therefore, dwell mainly upon the history of science but also make reference to the present-day situation in other disciplines as well.

Though it perhaps goes somewhat too far to speak of a "real" tradition in the history of science in the Netherlands, there has been a continuing interest in this field of study, expressed in many books and articles (many of which have been translated into English). Of course, historians, in their more general analyses, paid attention to science as a cultural phenomenon. To give only one example, Jan and Annie Romein dealt, in passing, with the history of science in their general historical monographs. In their Erflaters van onze beschaving (Testators of our civilization) in which a history of Dutch society was written via biographies of the most important Dutch figures of the past, they also included biographies of Dutch scientists in different epochs. In another work, Jan Romein, attempting to write a "comprehensive" history of a specific cultural period (the turning point of the 19th into the 20th century), offered an analysis of the role of science in this period. (Op het breukvlak van twee eeuwen--On the edge of two centuries).

The studies by the Romeins indicate an interest in the history of science (as can be found in other historical monographs), yet one cannot, in these cases, speak of a distinct history of science. There were no specialized historians of science. The history of science established a place of its own in the Dutch university context, in the period after World War II. Approximately at the same time, in the period 1945-1950, Hooykaas at the Free University of Amsterdam (later at the State University of Utrecht), Forbes at the University of Amsterdam, and Dijksterhuis at the State University of Utrecht got chairs in the history of science. One can say that, factually, the subsequent history of Dutch history of science was, more or less, made by these three men. Let me therefore give a brief overview of the work of these historians.

Hooykaas had a very broad interest in the history of science. His publications include the history of chemistry, mineralogy and crystallography, atomic theory, evolution theory, geology, the relationship of science and religion, and science in the Renaissance and during the 17th and 18th century. Of special significance are his published Edinburgh Gunning lectures (Religion and the Rise of Modern Science, 1971). Forbes had a specific interest in the related development

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Social Studies of Science in the Netherlands (Continued).

of science and technology. He dealt, for instance, with the development of pre-classical science, analyzing science and technology in the Ancient Middle East [see his Bibliographia antiqua philosophia naturalis, 1940-1950 (6 volumes), and his inaugural lecture at the University of Amsterdam, 1947: "Ambacht en Wetenschap in het Oude Nabije Oosten"-- "Craft and Science in the Ancient Middle East" -- Attention was also paid to later developments in technology and engineering (see, for instance, his Man the Maker, 1950). Dijksterhuis published, among other things, a biography of Simon Stevin (1943). But his main contribution was his brilliant De Mechanisering van het Wereldbeeld (1950), of which an English translation appeared in 1961. In this monograph Dijksterhuis traced, most eloquently, developments from classical science until Newton's Principia, out of the perspective of the growing importance of the mechanistic view in science. Although these remarks on the works of these scholars are too brief to do justice to their contributions, they indicate the kind of work done in Dutch history of science after World War II.

I will now turn to the present-day situation in social studies of science in the Netherlands. More than before, the interest in science as an object of reflexive study is coming from different disciplinary perspectives. In October 1975, under the auspices of SISWO (Netherlands Universities' Joint Research Centre), a symposium was held to gather people working in the field of science policy and science studies. This initiative resulted in the formation of a group of approximately 30 people who have, since then, been meeting to discuss each other's work. The group still lacks a firm research program, in part because most of the members work in relative isolation in their universities, but the members of this group come from various disciplines (natural science, sociology, psychology, history).

I will only summarize some of the issues that are being studied at present: developments in specialties (with a preference for developments in social scientific fields); the historical development of science policy in the Netherlands within the European context; science policy as such; scientific criticism; the application of the cognitive paradigm in the philosophy and science of science; simulation of the granting process; bibliometric projects.

This summer, a first annual report of the group will be published, indicating the members' research topics.[1] Although more people are doing research in the field of social studies of science (this group does not encompass all Dutch "scientists of science"), the collectivity is still a rather accidental one. For this field of study to obtain a firmer base in the Netherlands, it is necessary for it to receive some institutional (university) recognition, but also--because of the small scale of the country--that international contacts (for instance through international organizations such as 4S and PAREX) are continued and extended.

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[1] For this report, contact P. Koefoed, SISWO, 128 O.A. Achterburgwal, Amsterdam, the Netherlands.

On Teaching Social Studies of Science in Papua New Guinea

Lyndsay A. Farrall*

Teaching courses in the history of science and technology, and in science and society at the University of Papua New Guinea (UPNG) was a stimulating experience. A substantial part of the stimulation came from trying to answer the ever-present question, "How is it best to teach social studies of science in Papua New Guinea?" It is comparatively easy to agree that science is an important subject for social study in contemporary societies and that science has been important in influencing the shape of the present world order. But it is more difficult to agree about which topics in social studies of science are most useful and interesting for study, especially when the students have a cultural background different from that of their teachers.

A number of features about Papua New Guinea (PNG) influenced the nature of the courses which were developed. First, literacy is a phenomenon of the last hundred years, the period of colonial rule. Hence the historian's usual reliance on documentary evidence is not possible in studying the history of PNG. Second, PNG is still predominantly an agricultural society. Thus the use of social and historical generalisations developed in relation to industrialised societies is often inappropriate. Third, PNG is committed to policies which use modern science and technology as part of the underpinning for developments needed in the new nation. This seems to make questions of the political economy of science and of its relation to its cultural setting even more important than in industrialised countries.

A course in the history of Papua New Guinean agriculture has provided a means of presenting a straightforward chronological history which at the same time takes a number of these considerations into account. Recent archaeological work has made it evident that the cultivation of root crops in the Western Highlands of PNG is of the same order of antiquity as cereal cultivation in the Middle East. By focussing on the questions of when and how agriculture began in PNG, the course helps students become familiar with the way in which archaeology can be used in studying the history of PNG. In addition, students are introduced to one kind of concept often used in historical and social studies by means of the example of the "agricultural" or "neolithic revolution" as it has been developed and debated by prehistorians.

The course also shows that the techniques of oral history can be used to investigate the village agriculture of PNG. Agriculture involves the use of both practical and theoretical knowledge which provides an appropriate way of introducing discussion about the nature of science and technology. In PNG, the beginning of colonial administration was virtually simultaneous with the

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introduction of cash-cropping. Agriculture in the form of coconut products, cocoa, and coffee has always provided a major part of export income. The history of cash-cropping and agricultural exports in PNG can be shown to be closely connected with the course of industrialisation in other parts of the world and with the establishment of a "world economy". The concept of the "Industrial Revolution," so important for the understanding of much social theory as well as historical study, is dealt with. Since agriculture is still a key sector in the developing national economy, important questions of science and technology policy-making can be raised in the final parts of the course.

The course on the history of agriculture in PNG was designed as a course in the history of science and technology. Such topics as the nature of science and the relationship of science to its social and cultural contexts were dealt with briefly or not at all. In other courses it did seem appropriate to emphasize these topics. However they were taught in the context of the interaction between cultures in a post-colonial situation where science was often seen as an essential part of the culture of the colonial power, Australia, but not of the cultures of the colonized peoples of PNG.

In the past, knowledge in PNG was cast in an oral mode. It was formulated, transmitted, and received via the spoken word. Although there doubtless are differences between oral and literate cultures, Robin Horton has argued that traditional African religious thought, a product of oral culture, is similar in many ways to modern scientific thought. An oral knowledge system closer to PNG is the system of navigation used by the people of Puluwat Atoll in the Caroline Islands. This system was used for comparative purposes in discussing the nature of science.

Puluwat navigators undergo a very long apprenticeship. This includes formal schooling on land and practical experience at sea. The ultimate object of the apprenticeship is to be able to navigate small ocean-going craft between small islands which in some cases are more than a hundred miles apart. Each student is taught a set of subjects by his instructor who has memorized all his navigational knowledge. Knowledge taught includes the movements of stars, star courses between islands, a system of dead reckoning used to keep track of the boat's position, regularly occurring ocean swells and wave patterns, ocean currents, prevailing winds, means of finding land, geographical features, etc. Puluwat navigation involves the bringing together of large bodies of observations into an organized system of knowledge. It involves activities similar to the observing, generalizing, theorizing and model-building of modern science. It seems not unreasonable to call Puluwat navigation scientific.

Historical and social studies of science would be enriched by further comparative studies of scientific knowledge systems in oral and literate cultures. The mode of scientific thought is not the most interesting feature about science from the viewpoint of a third world country. Its association with affluence and power is more important. If, as is generally believed, science is a source of economic progress no third world country can afford to be without it. And if, as Kenneth Boulding has argued, it is the nineteenth century "social invention" of "organized research and development" which has sustained economic growth, then third world research and development institutions must be established.

For these reasons it seemed appropriate to use as the focus of a course on science and society in PNG the question of how PNG could establish a scientific community which would meet national needs and aspirations. This question leads naturally to a discussion of science policy-making and the restraints placed upon policy-making by the post-colonial situation. But it also leads to consideration of the restraints placed upon policy-making by the nature of the modern scientific community. The financial and human resources necessary for carrying out much scientific research in the contemporary world are overwhelmingly in the hands of the superpowers and the affluent nations. Is it possible for the relationship of a PNG scientific community to the international scientific community to be other than a neocolonial one? From the point of view of the third world further investigations of the political economy of modern science are very important.

We are all familiar with the importance of the social organization of the scientific community in affecting the production, publication and eventual acceptance or rejection of the results of research. To what extent can that social organization be extended into the third world without being an agent of Western cultural imperialism?

Review Symposium

Harriet Zuckerman, Scientific Elite: Nobel Laureates in the United States. New York: The Free Press, 1977, 335 pp, $14.95.

Zuckerman's principal subject is the evaluation and reward system in American science as it is illuminated by the careers of an "ultra-elite"--the 92 Nobel laureates, recipients of the ultimate reward, who did their prize-winning research in the United States. Her book is based on an enormous amount of research, including interviews with 41 of the laureates and material from biographical and autobiographical treatments. She has also gathered a prodigious array of statistical data for the purpose of comparing the laureates' career patterns with those of matched samples of run-of-the-mill scientists as well as members of the mere "elite" elected to the National Academy of Sciences. Zuckerman has analyzed the social and religious backgrounds of her subjects. She has also examined in institutional terms their education, training, employment, and research, their degree of collaborative efforts and rates of ascent in the scientific hierarchy. These data are presented in convenient tabular form, discussed in lucid, jargon-free prose, and often interpreted with happily illuminating quotations or anecdotes.

Zuckerman supplies an absorbing account of the evolution of the Nobel prize as an institution. She also offers useful commentary upon the inequities of the prize: They arise because of its limitation to only a few fields of science and because, while only one prize is awarded annually in each field, the size of the group occupying the 41st chair, so named by analogy with those meritorious researchers excluded from the 40-member French Academy, has been steadily increasing. Yet the most stimulating aspects of Zuckerman's study lie in the statistical data. Among the more interesting results, Zuckerman destroys the myth that scientists are washed up who fail to make their marks by age 30. Nobel laureates in the United States and elsewhere did their prize-winning research at

Review Symposium (Continued).

an average age of over 38 (36 for physicists, 39 for chemists, 41 for laureates in physiology and medicine). Then, too, some 19% of all laureates and about 27% of American prize-winners are Jewish; only about 1% are Catholic; and the Protestant remainder includes very few Baptists and fundamentalists. More generally, American laureates have tended to come from "socially advantaged" homes; to attend the leading scientific schools as undergraduates and, still more, as graduate students and postdoctoral fellows; to study with scientists who also had or would win the Nobel prize; to obtain their first jobs at the leading institutions and to be promoted at a rapid rate.

On the basis of these results, Zuckerman argues that the evaluation and reward system in American science is characterized by a consistent "accumulation of advantage." By this she means that the "exercise of the merit principle in science contributes to the marked inequalities in scientific performance and research. . . ." Scientists who "do well when they are young have a better chance to do well as they get older." More important, after examining the processes involved in the accumulation of advantage, Zuckerman arrives at "considerable doubt . . . that marked differences in performance between the ultra-elite and other scientists reflect equally marked differences in their capacity to do scientific work." Yet the degree of doubt thus cast is rendered questionable by certain troublesome features of this book.

While attentive to historical change, Zuckerman's study lumps together the laureates who entered science before and after 1940. Zuckerman justifies this ahistorical design on grounds that the numbers she had to deal with were too small to permit division into two groups. Fair enough, but that explanation does not increase one's confidence in her conclusions. Of the 92 laureates studied, 71 were reared in the United States; 60 of them were educated before the 1940s. Thus, as she is aware, Zuckerman's results about the educational characteristics of the laureates apply mainly to the period between the two world wars. Furthermore, by my count only 16 of the 92 laureates received their prizes before 1945. Presumably, much of the prize-winning research was done in a post-1940 environment. Surely the Depression, World War II, and the massive federal patronage of academic science since have exercised significant impact upon the opportunity structure of science, including recruitment, training, and research, not to mention the evaluation and reward system.

Zuckerman seems ambivalent--and her evidence is often ambiguous--as to whether advantage in the evaluation and reward system accumulates unfairly. The system has indeed been unkind to women and non-Jewish minorities, but one cannot blame the scientific system for the norms and practices of an entire society. For those who do enter science, Zuckerman stresses that, compared to Ph.D. scientists generally, laureates have tended to come twice as frequently from the homes of professionals. Of course, the category "professional" lumps together doctors and lawyers with school teachers and clerics, who are ordinarily less prosperous. If the advantage here is thus at least as much educational as material, it may be an advantage nonetheless. On the side of fairness in the merit system, Zuckerman points out that the rather large proportion of Jews among the laureates came from materially disadvantaged backgrounds and made their way up through the scientific system despite, certainly before World War II, considerable anti-semitism. Then, too, Zuckerman observes that, for all laureates, the disadvantages of social background were virtually eliminated by the time of graduate school.

Review Symposium (Continued).

Zuckerman claims that the effectiveness assumed for the evaluation and reward process by its managers constitutes a self-fulfilling prophecy: Young scientists singled out as promising do better because they are granted objectively superior conditions for work. Perhaps this is true, but the subject would seem to warrant considerably more scrutiny than Zuckerman has given it. The future laureates, occupants of the 41st chair, and members of the National Academy aside, many other young scientists received their training at the elite institutions, studied under outstanding scientists, and found employment in stimulating research environments. Why did they not make it into the ultra-elite, or even the elite? The laureates recall that they learned from their distinguished mentors a way of doing research, a taste for the significant problem. Why did their less accomplished peers not assimilate the same scientific style? Could it be that they were for the most part endowed with less drive, imagination, and scientific talent than their Nobel brethren? It is difficult to see how these issues can be explored without assessing the actual merits of the scientific work done at various stages of their careers by the laureates and some suitable control group. Perhaps such a venture would constitute an unmanageable task even for a small sample of laureates and other scientists. But perhaps the sociology of science simply cannot be done without attention to the content of science, not if it seeks to draw believable conclusions about some of the most important features of the scientific enterprise.

Joseph Henry once remarked that contributions to science must be weighed, not counted. Zuckerman has made some attempt, through the use of science citations, to weigh the significance of the laureates' work. She found that it was significant indeed, more significant by this measure than the work assessed at comparable times in their careers of nonlaureates elected to the National Academy. Ample funding, assistance, laboratory facilities, and colleagues may be necessary for productivity in research; they are, one would think, not entirely sufficient for contributions of outstanding scientific merit. Zuckerman's conclusions about the accumulation of advantage appear to rest on a tacit theory of quality in science. To put it more technically, her analysis proceeds from a multivariate equation that relates scientific accomplishment to some combination of native talent, home environment, training, opportunities, research situation, and possibly, state of the scientific field. Despite all the work that has gone into this study and all the valuable data it reports, Scientific Elite raises many more questions than it answers because both the theory and the equation are unstated, unspecified, and worse, unexamined.

Daniel J. Kevles
California Institute of Technology

Scientific Elite: Nobel Laureates in the United States by Harriet Zuckerman is a collective biography of the American Nobelists. It will be a standard reference resource for information on those Americans who won the Nobel Prize between 1901 and 1972. The group of American Nobelists is examined prosopographically for their social origins, their master-apprentice relations, their organizational ties, and their experience after the Prize. The Prize itself is examined in terms of its position as a reward and the mechanism of choice. The age of the participants in the Prize winning research, their collaborative styles and explicit recognition of that collaboration in name ordering, and the citations to Prize research, are also considered.

Review Symposium (Continued).

The Nobel Prize is seen in the context of a career framework. Yet it is a capricious award both in terms of the fields in which it is awarded and the quality of the research for which it is and is not awarded. The connection between career character and the award is not simple. The scientific career, its rewards and reward systems, are the foci of the book. Zuckerman follows "a style closely associated with the sociological research of the late Professor Paul Lazarsfeld and of Professor Robert K. Merton" (p. xiii). The information on "face sheet" variables (e.g., age, father's occupation, college of baccalaureate origin) are related in cross classification tables to winning the award, and to the appropriate comparative populations. Verbal argument rather than statistical models is used to connect the tables. Tables are used even when, as in the table on doctoral origins by institution, a graph and a Gini index computation might summarize the information more adequately.

The model of the social character of careers of successful Nobelists does not show much effect of the reported variables. There is some "cumulative advantage"; however, Nobelists are the members of a very small group. Like the very rich, they are not like you and me and may not be subject to the same influences that shape more prosaic careers. Unfortunately there is not much variance in the levels of excellence to provide leverage for an analysis of these problems. The comparison with the holders of the forty-first chair (Nobel quality but without the Prize) might provide that leverage. However, it is not carried out systematically as only those who have been designated as holders of that chair in official or semiofficial documents are discussed. Some combination of objective criteria might have been used to create a pool of comparable persons for a more complete study.

The study does not do as well as a general study of the scientific elite. The search for social structural effects on the selection process and the use of the selected Nobelists as a population to describe the structure of the elite seem problem-filled. Age, cohort and period effects would seem to bear on Nobel Prize winning. Only age was investigated. However, cohort effects, such as what area was active when winners were in graduate study or postgraduate work, were not. Period effects, such as the strength of competition at the time one was considered for the award, are also unconsidered. The invariance of social structural effects across the period is explored for only a few effects.

However, the use of direct quotation and insightful discussion puts this study into its own special class. Interview materials present the Nobelists in their own words. The unattributed quotations fill out a picture of the texture of the experience. I can only regret that more use was not made of the interviews and the historical cases, such as the insightful discussion of the social structure of the Drosophila work. The high quality of this material is a direct result of the use of the interview guide in the Appendix. A careful look at this guide is recommended to all those who interview scientists.

Nicholas C. Mullins
Indiana University

PROGRAMS AND MEETINGS

Forthcoming Meetings.

ICHS. A session on "Social Revolutions, Scientific Development and Science Policy" has been organized for the XVth International Congress of the History of Science, Edinburgh, Scotland, 17 August 1977. The chairmen will be Jean-Jacques Salomon (France) and Gunter Krober (Germany-DDR). The following presentations will be made: Peter Buck (USA), "Science, the People, and the State in Mao's China"; Yehuda Elkana (Israel), "The Image of Science and Its Influence on Science Policy in the Industrial Revolution"; Loren Graham (USA), "Science Policy and the Soviet Experience"; Jean-Claude Guedon (France and Canada), "France, the First Republic and the Emergence of a 'Science Policy'"; H.M. Karanda (Tanzania), "Policies for Science and Technology in Tanzania"; Wolfgang Krohn (Federal Republic Germany), "'The New Sciences' and the Urban Revolution of the Renaissance"; Everett Mendelsohn (USA), "Social Revolution and Science Policy: Themes and Orientations"; S. Mikulinsky (USSR), "Social Revolution, Science & Scientific Revolution"; Shigeru Nakayama (Japan), "Japan's Confrontation with European Science, Technology, and Medicine in the Late 19th Century"; Dorothy Nelkin (USA), "Social Conflict and Public Control of Science & Technology"; E. Olszewski (Poland), "An Example of Non-governmental Scientific Policy in a Revolutionary Situation - Warsaw, 1900-1914"; A. Rahman (India), "The End of Colonialism and the Development of Science Policies for New Nations"; Barbara Gutmann Rosenkrantz (USA), "State Medicine in the United States, 1870-1970"; Brigitte Schroeder-Gudehus (Canada), "Revolution and Higher Education: The Prussian Ministry of Education and the Universities in Early Weimar Germany"; Tilman Spengler (Federal Republic Germany), "Devolution in Research, Revolution in Development: China's Science Policy in the Period of the Cultural Revolution"; Gunter Wendel (German Democratic Republic), "Science Policy Changes in the German Democratic Republic after 1945".

NLCB. The National Legal Center for Bioethics is presenting a conference in Washington, DC, on 20-22 October 1977, on "Policy Making and Health Resource Allocation." The conference will be held at the Twin Bridges Marriott Conference Center. Experts in law, medicine, ethics and economics will examine public policy and life-prolonging technology, concepts of distributive justice, relationships between definitions of health and societal responsibility, and the constitutional parameters of the policy-making process. Special attention will be given to the underlying rationale of policy-making methodologies. The conference is designed to accommodate 500 persons interested in health resource allocation. The National Legal Center for Bioethics is a non-profit, tax-exempt corporation established in 1975 to affect social policy in the emerging field of bioethics through diverse programs in law, science and ethics. The conference is made possible by Information Planning Associates, Inc., Gaithersburg, MD, publisher of Bioethics Digest and the conference proceedings. For further information, contact Joseph F. Sedlak, Conference Coordinator, P.O. Box 24021, Washington, DC 20024.

PSA. The Philosophy of Science Association will hold its Sixth Biennial Meeting at the Jack Tar Hotel, San Francisco, on 28-30 October 1978. The program will include symposia and invited papers as well as sessions devoted to the presentation of contributed papers. Contributed papers will be pre-printed as the first volume of PSA 1978. The symposia and invited papers will be printed later as the second volume. The submission of contributed papers is invited. Contributed papers on any topic in the philosophy of science are welcome. Maximum length is 3500 words and the closing date for submission is 1 March 1978. Two copies, each

PSA (Continued).

including a 100 word abstract, should be prepared in double space typescript. Blind refereeing will be used so that the author's name and institution must be on a separate cover page. Send papers to Ian Hacking, Program Committee Chairman, Department of Philosophy, Stanford, CA 94305, USA. PSA is primarily interested in papers that the authors believe are ready for publication. Others, however, will be considered (the author should indicate that the paper is not ready for publication and is being submitted for possible presentation only).

SHOT. The Society for the History of Technology will hold its Twentieth Annual Meeting in Washington, DC, on 20-22 October 1977. Address inquiries to Robert C. Post, Program Chairperson, Smithsonian Institution, Washington, DC 20560.

NSF Announcement.

Effective 1 October 1977, the Science Policy Research Program in NSF's Division of Social Sciences will be abolished. Responsibility for review and evaluation of research proposals on topics previously assigned to this program will be assumed by the Sociology Program and the History and Philosophy of Science Program in the Division of Social Sciences or, where appropriate, by the policy research units of other offices such as the Directorate for Scientific, Technological, and International Affairs, or the Office of Planning and Resources Management. For further information, contact Dr. Herbert Costner, Division of Social Sciences (202/632-4286).

IN THE LITERATURE

Recent Publications.

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2. American Association for the Advancement of Science, Science for Society: A Bibliography, 6th Edition, (1977, $3.00, remit to AAAS, Department OSE, 1776 Massachusetts Ave, NW, Washington, DC 20036).
3. Ben-David, Joseph and Terry Nichols Clark (Eds.), Culture and Its Creators. Essays in Honor of Edward Shils. Chicago: University of Chicago Press (1977, 326 pp, $15).
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Recent Publications (Continued).

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18. Goodell, Rae, The Visible Scientists. Boston: Little, Brown, and Co. (1977, 242 pp, $9.95).
19. Gorz, A., "On the class character of science," New Scientist 72 (18 November 1976), 390-391.
20. Greenberg, Daniel, "Advice for a new president," Science and Government Report, 6 (December 1976), 1-3.
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24. McCormmach, Russell (ed.), Historical Studies in the Physical Sciences, Vol. 7. Princeton, NJ: Princeton University Press (1976, 490 pp, $28.50).
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Annuals, Journals and Newsletters.

ISR. Interdisciplinary Science Reviews is a quarterly journal devoted to critical interdisciplinary reviews of the natural, technological and social sciences, by recognized authorities (an editorial board of over 60 scientists from 15 countries). ISR is essential reading for all who plan and direct science policy, or finance and carry out scientific research. To order at the special rate of $22 (a 50% reduction), send check to Heyden & Son, Inc., Kor-Center East, Bellmawr, NJ 08030, USA.

JISTA. The International Society for Technology Assessment (ISTA) is a new world membership organization concerned with the ways man can assess the social, economic and environmental consequences of existing and potential technology initiatives. It is established as an international society with offices in Washington, DC. The membership of ISTA is drawn from a variety of institutions, communities and persons all over the world interested in the exploration and resolution of problems facing local, regional, national and international communities. Individual membership is $28.00 (US) per year and includes a subscription to the Journal of the International Society for Technology Assessment: A Forum for Social Impact Evaluation. The Society also sponsors a bi-annual International Congress and interim regional or topical meetings for which special rates are available to members. Reduced membership rates also apply to ISTA DOCUMENTATION, a paperback series of hard-to-get, new and timely TA literature. A newsletter

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PAREX Newsletter. The second PAREX number (February 1977) is now available. It includes notes on meetings, publications, theses, and research opportunities. All correspondence regarding suggestions and inquiries should be addressed to Secretariat PAREX, Maison des Sciences de l'Homme, 54 boulevard Raspail, 75270 Paris Cedex 06, France.

RCHS Newsletter. The Winter 1977 issue of the Newsletter of the International Sociological Association's Research Committee on the History of Sociology has appeared. It features news of forthcoming meetings, work-in-progress and information on archives. Membership dues of $10 entitles one to the Committee's quarterly Newsletter and other communications concerning meetings and activities. Contact Robert Alun Jones, Secretary, RCHS-ISA, University of Illinois, Department of Sociology, Urbana-Champaign, IL 61801.

Research in Philosophy and Technology. This Annual Compilation of Research, edited by Paul T. Durbin, aims to fulfill three functions by serving as an annual bibliographical update, as the outlet for the proceedings of a series of philosophy of technology conferences and symposia, and as a substitute for a journal in the field, with submitted papers not intended for one of the conferences or symposia welcome at any time. Volume 1 has just appeared (350 pp, $17.50 cloth, JAI Press) in three parts: Part I-Method, Descriptive Frameworks, and a Practical Program for Philosophy of Technology (3 papers), Part II-The University of Delaware Conference, 1975 (9 papers), and Part III-Bibliographical Update: Philosophy of Technology, 1972-1974, compiled by Carl Mitcham.

Research in Sociology of Knowledge, Sciences and Art. The essays in this Annual Compilation of Research, edited by Robert Alun Jones, consist of original research done in the fields of the sociology of knowledge, science, and art. As the contents of the first volume suggest, the focus of the series will be explicitly interdisciplinary, with a number of theoretical and methodological perspectives, as well as nationalities, represented. The series will also serve as the vehicle for essays of a length or content inappropriate to more conventional scientific journals. Volume 1 will appear in September 1977 (350 pp, $17.50 cloth, JAI Press). It will contain twelve essays by Judith Blau, Stephen Cole, Joseph Gusfield, Henrika Kuklick, Don Martindale, Ian Mitroff and Ralph Kilmann, and others.

SGRIA. Science & Government Report International Almanac--1977 is a one-volume work containing: (1) highly authoritative, specially written individual reports on science-policy developments in the leading industrial nations; (2) a directory of national and international research-related organizations, plus a listing of foreign science attaches in major capitals of the world; and (3) texts of important documents concerning research and development in 1976. The Almanac is published under the auspices of Science & Government Report, the independent, authoritative newsletter edited by Daniel S. Greenberg that has been serving the international scientific community since 1971. To order, send $54 (price reduced from $75) to P.O. Box 6226, Northwest Station, Washington, DC 20015.

APPOINTMENTS AND VACANCY

Harvey A. Averch	Assistant Director for Science Education National Science Foundation
Edward Barboni	Assistant Professor, Department of Sociology, Grinnell College
L. Vaughn Blankenship	Director, Division of Advanced Productivity Research and Technology, National Science Foundation
Daryl Chubin	Assistant Professor, Department of Social Sciences, Georgia Institute of Technology
Lyndsay Farrall	Senior Research Fellow, University of Melbourne
Pamela Hecht	Assistant Professor, Department of Sociology, University of South Carolina
Richard R. Ries	Acting Director of Operations, Office of Assistant Director for Scientific, Technological and International Affairs, National Science Foundation
Aaron L. Segal	Program Manager, International Science Studies Program, National Science Foundation
Kenneth Studer	Assistant Professor, Department of Sociology, Virginia Commonwealth University
Thomas Ubois	Acting Deputy Assistant Director of the Directorate for Scientific, Technological and International Affairs, and concurrently serving as Acting Division Director, Division of Policy Research and Analysis, National Science Foundation
Alexander Vucinich	Professor, Department of History & Sociology of Science, University of Pennsylvania

University of Wisconsin-Madison. Applications for a tenured or tenure-track position in the History of Science Department beginning Autumn 1978, are invited. Primary interest in history of modern (18th-20th c.) biological sciences is required. The position carries responsibility for both graduate and undergraduate teaching. The deadline for applications is 15 November 1977. Direct inquiries to Robert Siegfried, Chairman of Search Committee, History of Science Department, South Hall, University of Wisconsin, Madison, WI 53706.

MEMBERSHIP SURVEY

Full results of the 4S Membership Survey will be communicated in a subsequent issue of the Newsletter. If you have not already returned your copy of the questionnaire (4S Newsletter, Volume 2 number 2, pages 21-22), PLEASE DO SO NOW to Warren Hagstrom, 4S Membership Survey, Department of Sociology, University of Wisconsin, Madison, WI 53706.

ADMINISTRATIVE DOCUMENTS

Revised Charter.

I. Preamble

This organization is called the Society for Social Studies of Science. Its purpose is to promote research, learning and understanding in the systematic social analysis of science. The Society is an independent organization which cooperates with other organizations having similar interests.

II. Organization

A. Membership.

Membership in the Society includes all scholars interested in the social and policy aspects of science who have paid their current dues.

B. Offices and Election.

1. The Society has the following elective officers: a president, a secretary-treasurer, and seven other council members. The terms of office are: president--two years; secretary-treasurer--three years, council members--two years.
2. Four voting members of the council constitutes a quorum. Voting members of the council consist of the elected officers. *Ex-officio* members of the council, as specified below, are non-voting members.
3. There shall be a standing committee on annual meetings.
4. There shall be a standing committee on publications.
5. A nominating committee consisting of the president as chair and four other members of the Society designated by the president, no more than two of whom are officers, shall present to the Society at least six months before the annual meeting at least one nominee for each office to be filled.
6. Further nominations by petition signed by five members shall be presented to the president at least four months before the annual meeting.
7. Election shall be by mail ballot, the results to be announced at the annual meeting.
8. The retiring president shall serve for two years as an *ex-officio* council member, but shall be otherwise ineligible for election in that period.
9. Four members of the council shall be elected in odd-numbered years, three in even-numbered years.
10. All officers must be members of the Society.
11. If an elective office becomes vacant, the council shall appoint a replacement who will serve until the next annual election, at which time a person will be elected to serve out the remainder of the term.

III. Duties of Officers

A. The President.

The president of the Society or a designated member of the council shall preside at all meetings of the Society and the council. The president also is responsible for the program at the annual meeting.

Revised Charter (Continued).

B. The Secretary-Treasurer.

The secretary-treasurer shall keep the records of the Society and shall receive and have custody of funds of the Society. The accounts shall be subject each year to an audit by members chosen by the president of the Society or by a certified public accountant. At the end of each three-year term the accounts must be audited by a certified public accountant. The secretary-treasurer shall be responsible for communicating the agenda of coming council meetings to members through the newsletter and for presenting any responses or suggestions received from members at such council meetings. The secretary-treasurer shall perform such other duties as the council shall assign.

C. The Council.

The council shall set membership dues and have control and management of the funds of the Society. It shall act as a committee on time and place of meetings, and perform such other duties as the Society may delegate to it. The council may adopt any rules and regulations for the conduct of its business not inconsistent with the Charter of the Society. The council will regularly communicate its actions to the membership.

IV. Committees

The council shall recommend standing committees to the Society membership for its approval or rejection in annual meetings. Proposals for changes in the standing committee structure may also be moved by any member of the Society in annual meetings. Officers of the Society may be appointed by the president to sit as ex-officio members of any committee.

The president may appoint ad hoc committees as circumstances dictate or the council recommends. With the approval of the president, committees may be established that include non-members.

V. Activities

A. Meetings.

The annual business and other meetings of the Society shall be held at such time and place as are determined by the council. Adequate notice of such time and place shall be given to members of the Society. Special meetings of the Society may be called by action of the council. Meetings of the council shall be called by the president or at the request of a quorum of the council.

B. Publications.

The Society, acting through the council, will seek to publish appropriate literature (including a newsletter) for distribution to the membership and for sale.

C. Facilitation of the Society's Activities.

The Society, acting through the council, may receive and disperse funds in order to carry out its activities.

VI. Amendments

Proposed amendments to this Charter may be submitted at any time by any member to the council of the Society for consideration at its next meeting. Recommendations of the council shall be reported at the annual business meeting of the Society and such recommendations shall be approved, revised or rejected by majority vote at that meeting. Recommendations, if approved, shall be put to a mail ballot of the Society for ratification by a majority of those voting.

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CALL FOR NEWS

Material for inclusion in the Fall issue of the 4S Newsletter should be sent by 1 September, to Henry Small, Institute for Scientific Information, 325 Chestnut St., Philadelphia, PA 19106.

4S

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