

4S Society for Social Studies of Science

Fall 1978

Volume 3 Number 4

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4S Society for Social Studies of Science

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Editors' Note

We want to call your attention to two major items:

1. The 4S Newsletter would like to publish information on course or curriculum development in the general area of the social study of science. As science studies programs proliferate, many of our members are struggling with developing new courses. We would like the Newsletter to serve as a forum for discussion of curriculum development in this area. Please share your ideas with us in the form of course descriptions, course outlines, reading lists, or even short articles on your experiences developing new courses.
2. To review books, we must receive copies from the publishers. Please include us on your list of reviewers to your publishers. Ask them to send two copies to:

Jerry Gaston
Department of Sociology
Southern Illinois University
Carbondale, Illinois 62901

Also, please continue to send us unsolicited reviews of books, and send us names of scholars who might agree to write book reviews. Finally, please volunteer yourself to review specific or general titles!

The FOURTH ANNUAL MEETING will be in Washington, D.C., November 2-4, 1979. Its general theme will be science and public policy broadly interpreted to reflect the diversity of interests of the members. We welcome proposals for panels and roundtables. Proposals for panels should contain a brief description of the topic, the names of possible speakers, and their subjects. Send these no later than 14 February 1979 which an ABSOLUTE deadline to:

Al Teich
Program in Science, Technology and
Public Policy
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THE FUTURE OF 4S
A MESSAGE FROM THE PRESIDENT

As members of an organization devoted to the social studies of science, we should take heed of Waddington's famous description of professional society activities--he calls them COWDUNG, which among other things, is an acronym for the Conventional Wisdom of the Dominating Group.

With that in mind I accept the presidency of this society with both pleasure and concern. Pleasure because I feel we are engaged in an interesting and important field. Concern because I feel that the organization has a great deal to do to develop some diversity in its membership and to increase its influence on teaching and research in the field. Let me briefly suggest some crucial problems to be addressed and some steps toward their resolution.

First it is important to ask why we exist as a professional society. Historians and sociologists, after all, have their own national and international organizations which include sections on the history and sociology of science. As I understand it, 4S was created for the interdisciplinary study of science, to combine the perspectives and expertise gained from anthropology, economics, history, philosophy, political science, psychology, research administration, science policy studies and sociology. We have done pretty well in encouraging participation among sociologists, philosophers, and historians, but relatively poorly in other fields. I feel especially concerned about our role in the fields of political science and science policy. There is excellent work going on in the politics of science that is not actively represented. Also, the science policy community badly needs a professional base for the usual reasons that justify the existence of a professional society--namely to help to develop high and consistent standards of professional work. By and large policy researchers do not see 4S as their society, and they do not relate to the work being done by members of this organization. But science and technology policy is a growing area in academic and governmental institutions often, for example, tied to interdisciplinary energy and environment projects. Encouraging this group could be an interesting way for us to expand our membership and also to increase our visibility outside academic circles. In this context, I feel the organization should develop in directions that extend far beyond the conventional pattern of inward-directed scholarship. The work we encourage should be available and readable well beyond narrow professional circles. Our *raison d'être*--the concept that distinguishes us from the more conventional disciplinary professional associations is our effort to communicate beyond disciplinary boundaries.

A second weakness in our field is an uncritical tendency to accept the conventional wisdom that science is an autonomous system, isolated from the forces of history and politics. I strongly feel that as we have an obligation to approach the study of science and technology with a more questioning and critical outlook. There are many crucial, albeit uncomfortable questions to explore. What are the issues involved in limiting scientific inquiry? What are the potentially pernicious social applications of research? Can we use social tools to look critically, for example, at the social impact of the transfer of technology to developing nations? Or indeed the applications

of scientific findings to public policies in this country? What is the relationship between knowledge and its application, between knowledge and power? What is the role of external political forces on the direction of scientific research?

- Rejecting the conventional wisdom about the autonomy of science also leads us to explore its international dimensions. What indeed is the impact of different political cultures and governmental systems on research? Only by fostering our international contacts and by trying to coordinate research with colleagues in other countries can we begin to probe this question in depth.

Given these problems what can we do over the next few years? One way to increase communication among different disciplines is to give our organization a greater educational function. We might look to SISCON for ideas about how a Professional Society can provide stimulus to teaching and to the organization of curriculum.

We must expand professional communication and information exchange if we are to reach a wider group of scholars. For example, to attract the science policy community we might devote some attention in the Newsletter to problem-oriented research. However, to develop the Newsletter contributions from the membership are crucial. Eventually we might consider merging with another established journal with a broader distribution and a format which includes review essays, research studies, and reviews of work in the field here and abroad.

Finally we can take some action to encourage international coordination in the field. Most advanced industrial societies face similar scientific and technological problems--concerns about nuclear power, DNA, and genetic manipulation are ubiquitous; everywhere scientists are disputing over questions of risk; everywhere the role of scientists seems to be changing. One of our tasks could be to encourage comparative studies and to set up a context for collaboration on common research interests.

In short, we are students of a rapidly changing and dynamic subject where conventional wisdom and dominating groups are continually challenged. Our Society must maintain the same tension, moving creatively, definitively and energetically in new directions while maintaining its present source of strength.

Dorothy Nelkin

NEW COMMITTEES IN 4S

The MEMBERSHIP COMMITTEE has been appointed: Alex Morin, NSF (Chairman); Ian Mitroff and Daryl Chubin. Please send any suggestions to Alex Morin.

A NOMINATING COMMITTEE to nominate new Council members has been selected: Dorothy Zinberg (Chairperson), Nick Mullins, Marcelle La Follette and Gene Skolnikoff.

PROGRAM COMMITTEE for the meeting is Al Teich (Chairperson), Daryl Chubin and Alex Morin.

THOUGHT AND OPINION

Third Annual Meeting:

An Impression

Steve Woolgar
Brunel University

The third annual 4S meeting at Bloomington provided a major contrast with the experience of the first meeting at Cornell in 1976. The benefits for the European visitor, of making and renewing contacts with American colleagues, were again invaluable. By comparison with the earlier meeting, however, it was possible to leave Bloomington with the feeling that at least a little of the initial enthusiasm and momentum of the society had dissipated. The Bloomington meeting seemed a less ambitious affair. The smaller number of participants entailed the notable absence of several well-known figures; an absence made all the more notable by one's recollection of the characteristically vigorous contribution of certain individuals at the Cornell meeting.

It is perhaps too easy to attribute these changes to a decline in the organisational energy invested in the preparation for this latest meeting. Clearly, efforts to launch the inaugural (Cornell) meeting had benefitted from the availability of funds for travel subsistence and the duplication of papers; such funds were clearly not in evidence at Bloomington. At the same time, the fact that the Bloomington meeting followed closely on the Wisconsin History of Science meeting in part explained the regrettable absence of historians: their 30% membership of the society was sorely underrepresented. But perhaps the most convincing explanation for the smallness of the meeting was Bloomington's location, in the words of the natives, "out in the boonies." (Natives were less clear about the etymology of this expression than about its explanatory significance.) Some of the European visitors were delighted to find that it was one of the Americans who inadvertently wound up in Bloomington, Illinois.

Apart from the Keynote and Finale sessions, the proceedings were divided such that papers were presented in one of two (and sometimes three) concurrent sessions. Undoubtedly, this eased the problem of the length of time which could be allocated to each paper presentation. However, almost nobody to whom I talked felt that 20-25 minutes was sufficient to do justice to a paper which members of the audience had previously been unable to read. This also meant that more papers could be fitted in (and that less needed to be rejected). On the other hand, this organisation heightened one's sense of fragmentation: the determined efforts of participants continually to switch lecture rooms so as to maximise their reception of the "most interesting" papers proved both disruptive and unsatisfactory. Some participants felt dissatisfied, for example, because they were unable to partake of more than one third of the total papers. Others bemoaned the dearth of lively debate, which they felt resulted from the presence of diverse opinions in one single (and relatively large) audience.

Sociologists of science will not be surprised to learn of the close connection between the organisational style of the meeting and its cognitive content. Indeed, the overall diversity of contributions reflected the growing differentiation of interest areas within the society. Well represented, for example, was the recent upsurge of interest in socio-psychological perspectives and in issues relating to science policy and the public image of science. (Nicely indicative of the work in these areas were the papers by Faust et al and by Nelkin and Rip.) By contrast, the extent of contributions which emphasised the usefulness of quantitative approaches was minor (Henry Small once again impressed with his admirable sensitivity to some insurmountable (?) problems of interpretation). Quite apart from the formal presentation of papers, it was possible to discern several "pockets of enthusiasm" (Nick Mullins' phrase) among small informal groupings of participants with specific research interests in common.

Speakers such as John Ziman made brave attempts to provide synthesising perspectives. But such was the diversity of topics subsumed in these kind of arguments that the overall effect lacked the precision characteristic of any individual topic. Moreover, one detected a distinct sense of dis-ease among parts of the audience whenever attempts were made to draw together analyses which differed substantially in terms of their theoretical orientation. On the one hand, several contributions displayed considerable sensitivity to certain key theoretical issues. Notable in this respect were the papers by Collins (who coolly stepped in to provide one of the Keynote addresses at only a few moments notice), Zenzen, Restivo, Law and Pinch. On the other hand, a number of contributions, although substantively interesting in themselves, appeared to lack any obvious theoretical-analytical framework.

Participants' awareness of the diversity of interests spilled over into a general concern about future possible direction of the society. In particular, there was a strong feeling that it might be unwise for the society to be dominated by any one particular theme or area of interest. Some expressed doubts about the extent to which the thematic organisation of future meetings would adequately accommodate the variety of members' interests. (These doubts were not quietened by the argument of one member of the NSF that the theme of science policy "might serve a useful purpose for the Foundation--I mean the society!!") Clearly, the viability of future meetings will depend critically on the ease with which contributors from a range of different backgrounds and traditions can continue usefully to interact.

Themes and Comments

Linda L. Lubrano
American University

Participant-observers of scholarly conferences sometimes wonder if they should take the pulse of a professional association by looking at the contents of panel papers or by watching the interactions of their colleagues in and outside the formal sessions. While not unmindful of the potentially more interesting aspects of the latter, I shall limit my comments here to the main themes and approaches in the papers presented at some of the panels. I would like to note, however, that it was pleasant to find some of the enthusiasm and intellectual freshness so evident at the Cornell meeting of the 4S still intact at the Third Annual Meeting in Bloomington. The atmosphere may have been less explosive this year, but sharp criticisms and polite ridicule were still there. Perhaps, given the choice of two simultaneous sessions, the audiences in each were more homogeneous in their interests; and unfortunately some of the most vociferous critics were not with us. But the different mood may also reflect the more varied and well-balanced program or the "mellowing" that can result from repetitive exposures to different techniques and modes of analysis. I hope, however, that we do not mellow too quickly, and that a larger proportion of our diversified membership will attend next year's meeting.

In addition to the high quality of many papers and the overall success of the program, there was a general sensitivity to the complex dimensions of science that come together during the process of scientific research and the application of its results. Yet, the development of new ideas and the impact of that knowledge on society were usually examined separately. Authors seemed to focus either on the psychological-cultural aspects of scientific cognition or on the social-political aspects of public issues in science. Examples of the first theme included the papers by Roger Krohn, Moriah Markus-Kaplan, and Sal Restivo; the second theme was discussed by Rae Goodell, Barry Casper, Roger Kasperperson, and others.* While many of the participants made valuable contributions on an individual basis, the long-term impact of their work might have greater significance if the two thematic orientations could be integrated. This might be accomplished at a future conference by organizing panels around topics that can be approached from different perspectives. It is also important to maintain a self-conscious examination of the normative assumptions of science and of science studies, an issue that was not given sufficient emphasis by participants in this year's meeting.

A division of the papers according to the above themes reveals some definite contrasts in modes of analysis: Those who studied the growth of scientific knowledge presented papers that were more analytical and theoretical, with references to broader conceptual frameworks that could be used to integrate their work; those who concentrated on the public applications of science

*I did not hear all the papers at all sessions. Therefore, my generalizations are an attempt to highlight the characteristics of the papers I listened to, without inferring that these characteristics describe all the papers at the conference.

usually took a case study approach, descriptive of events that had occurred, with less references to social or political theories (except in a few cases, such as in the paper by Jon Miller). Not only are theoretical frameworks helpful in our analysis of the socio-political contexts of science, but they are necessary to bridge the disciplinary gap between the two themes, i.e., to integrate our understanding of science as knowledge and science as social activity (a sometimes artificial distinction).

Another noticeable characteristic of the presentations was the relativistic treatment of data, whether the author was writing on the psychological-cultural or the social-political aspects of science. In papers on the first theme, this was a self-conscious effort; authors intentionally talked about the cultural determinants of cognitive styles and the bio-emotional aspects of scientific thinking. In papers on the second theme, relativistic assumptions were not made explicit, but they penetrated most (if not all) of the case studies. Discussions of citizen participation in science issues were limited almost exclusively to pluralistic/Western polyarchical societies, where such participation is possible. The public issues of science were seen, therefore, in relation to a particular type of political structure and in a contemporary time frame. To broaden our perspectives on both themes, it would be desirable to use more non-American and non-Western sources on science in general, and to give more attention to the development of science in other countries.

Studies on scientific cognition have been conducted in the Soviet Union, for example, and these could be explored to raise the following kinds of questions: Do Soviet scientists perceive reality differently, enough to affect the ways they conceptualize research problems and the ways they carry out their research? To what extent do the social structures of science and cultural traditions affect scientific discovery and creativity in the Soviet Union? How do Soviet scholars in the social sciences and humanities define the study of science, i.e., do they raise different questions; do they call attention to different variables in the scientific process; do they use different modes of analysis? Similarly, we can look at the public issues of science in the Soviet Union and compare them with the treatment of the same issues in other countries. The channels and nature of citizen participation vary in capitalist and socialist democracies, despite some significant similarities. In the Soviet Union, for example, the expertise of the scientist is given greater weight than the views of "ordinary" citizens, but the scientist is accountable both to his administrative superiors (often scientists themselves) and to the general public.

More comparative research is needed, therefore, not only because the variations are inherently interesting, but also because comparison is a fundamental part of theory-building. If science is both a transnational phenomenon and the product of particular cultures, then it is only through comparative analysis that we can appreciate the complex nature of the scientific enterprise. There was some work in this direction at the conference (for example, the papers by Harrison Shull on the U.S.S.R., by Susan Cozzens on China, and by Dorothy Nelkin on the Netherlands). While

these papers were important for the generation of data and hypotheses, it would be better to work toward a systematic way of integrating that material and tying the above themes together, perhaps through the format of professional conferences. In the long run, it might be worthwhile to organize a 4S meeting as an expanded workshop that will generate cross-national hypotheses meaningful for understanding relationships between the cognitive structures of science, the public behavior of scientists, and the environments of scientific development.

ABSTRACTS OF THIRD ANNUAL MEETING TO BE PUBLISHED

It was not possible to publish in this issue the abstracts of the third annual meeting because of two reasons: some abstracts are still unavailable and we hope to include the maximum possible when they are published; and, this issue was already too large. We devoted considerable space to the discussion of science indicators and we were committed to publish book reviews that unfortunately had to be omitted from the last issue because that issue, too, had grown too big. The available abstracts will be in the first issue of 1979.

Minutes of Council Meeting, Bloomington, Indiana
November 3, 1978

Present: J. Gaston, W. Hagstrom, L. Hargens (Secretary/Treasurer-elect),
L. Lubrano (visitor), R. McGinnis, M. Moravcsik, N. Mullins,
D. Nelkin (President-elect), A. Thackray, P. Woolf

I. President Warren O. Hagstrom called the meeting to order at approximately 11:00 AM in the Indiana Memorial Union, site of the 4S annual meeting.

II. Secretary-Treasurer's Report (R. McGinnis)

A. Membership is 500 as of 10/31/78.

B. Cash balance at the Secretariat \$2,302.46.

C. The accounts have not at this time been audited by an independent accountant, as called for by Section III B of the Charter of 4S, inasmuch as that would cost the Society approximately \$500. After some discussion it was resolved that:

The requirements of Section III B of the Charter are satisfied upon transfer of all 4S funds from its account within the Cornell University Sponsored Programs Accounting Office to the new Secretary/Treasurer.

The motion was approved unanimously by those present.

III. Secretary/Treasurer introduced the matter of the Society's contractual relation with Neale Watson Academic Publications, Inc. recommending that the contract be terminated and responsibilities for maintenance of membership accounts be assumed by the Secretary/Treasurer. It was resolved that:

The Secretary/Treasurer shall assume all membership maintenance functions now held by Watson. The outgoing Secretary/Treasurer shall execute termination of the contract and the transfer of all lists to the incoming Secretary/Treasurer.

The motion passed unanimously. The Council also asked the outgoing Secretary/Treasurer to prepare a summary budget of the first three years of the Society's life.

Council Meeting
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- IV. The incoming Secretary/Treasurer was asked to draw up a 1979 proposed Budget and circulate it to the Council for approval or disapproval by mail ballot prior to December 31, 1978.
- V. The site of the 1979 annual meeting was discussed at considerable length during which the meeting was adjourned.

The meeting reconvened on Saturday, November 4, 1978.

Additional Present: I. Mitroff, H. Small (visitor), D. Zinberg

- VI. The 1979 meeting site was reconsidered.

- A. After considerable discussion it was resolved that:

The 1979 meeting shall be held at the Capital Hilton Hotel, Washington, DC on November 2-4, 1979; that three meeting rooms shall be reserved, two with capacities of 75-125 and a smaller third room; that further discussion and planning of the 1979 meeting be delegated to the Committee on Future Meetings to consist of P. Woolf, Chair, D. Nelkin, L. Hargens, R. McGinnis, L. Lubrano (Chair of Local Arrangements).

The motion was passed.

- B. The Council resolved that:

An amount equalling the penalty fee for meeting room rental be reserved in the Society budget and that the Local Arrangements Committee be authorized to arrange a banquet in the meeting hotel for the Saturday night of the 1979 meeting.

The motion passed.

- VII. The 1980 meeting site was discussed. It was resolved that:

P. Woolf shall pursue the possibilities of a 1980 meeting in Toronto in conjunction with Society for History of Technology and Philosophy of Science Association and possible other Organizations.

The motion was passed unanimously.

- VIII. The 1981 meeting was discussed. It was the sense of the Council that the Committee on Future Meetings should give first consideration to Atlanta and Philadelphia as possible locations.

Council Meeting
November 4, 1978

- IX. The state of the Publication Committee was discussed. It was resolved that:

The Publication Committee shall consist of A. Thackray (Chair), P. Woolf, J. Gaston, M. Moravcsik, D. Nelkin (ex officio); and that H. Small and J. Gaston shall continue as Editor and Publisher respectively with the deep appreciation of the Council for their services.

The motion was passed.

- X. A suggestion was made that the name of the Society and its good offices be lent to sponsorship of regional meetings. It was resolved that:

The Council shall entertain periodically applications for regional and special topic meetings, judging their merits on case-by-case basis; and that a request by D. Chubin for a Southeast regional meeting be granted.

The motion was passed.

- XI. A vote of appreciation was expressed unanimously to:

D. Chubin for his organization of a superb program; and N. Mullins, L. Hargens and other members of the Local Arrangements Committee for a flawlessly arranged meeting.

- XII. A second vote of thanks was tendered to N. Mullins on the occasion of his resignation from Council. It was resolved that:

N. Mullins shall be replaced by E. Layton as of the end of the present meeting for a term of one year.

The meeting adjourned much much later at an indeterminate time.

Respectfully submitted,



Robert McGinnis
Secretary-Treasurer

Science Indicators/1976: A Critique*

Robert McGinnis
Cornell University

I. INTRODUCTION

The appearance of Science Indicators, 1976 (National Science Board: 1977) and its two predecessor volumes (National Science Board: 1973, 1975) represents a significant addition to the social indicators' literature in both scope and quality. For those who study the institution of science,¹ the publication of these reports was the occasion for special rejoicing and some lamentation. The birth of the initial volume Science Indicators, 1972, was signalled by the most solemn of scholarly rites: the convocation of a conference and the publication of a set of essays. This book, Toward a Metric of Science (Elkana, et al., 1978), arrived on my desk almost concurrently with Science Indicators, 1976, facilitating but further complicating my evaluation of the latter. Most trivially, it complicates the references that must be made repeatedly. With apologies to the reader, I shall refer to Toward a Metric of Science as TMS and to the three versions of Science Indicators as SI/7x (x = 2, 4, 6). Most profoundly, the dozen essays contained in TMS together with the Editors' Introduction provide such a rich conceptual and contextual analysis of science indicators and raise so many critical questions about them as to defy systematic treatment in this brief evaluation, despite their clear relevance to it. I can only refer frequently to these essays and encourage the reader to do the same.

II. WHAT SCIENCE INDICATORS/1976 IS

In its own terms, SI/76 is ". . . a stage in the continuing effort to develop indicators of the status of science and technology in the various sectors of the U.S. economy" (p. vii). This dry characterization is itself, reasonably enough, a stage emerged from a more flamboyant self-description of SI/74 as a document intended "to measure and to reflect U.S. science--to demonstrate its strengths and weaknesses and to follow its changing character" (SI/74, p. vii). This is a sensible retreat since it is quite unrealistic to claim that any of this series holds a clear mirror to the institution and process of science. Rather, the NSF indicators provide an implicit model of science, an empirical characterization of that model and, from it, a prophetic exhortation, again implicit. These are the three aspects of SI/76 and its predecessors that are considered here, but only after a brief note of clarification on the series' paternity.

I shall refer repeatedly to the Science Indicators series as NSF publications, which is a bit misleading in at least one respect. While de facto it is a

*This review will appear in Social Indicators Research. It is printed here through the kind permission of the editor of that journal, Alex C. Michalos.

product of the National Science Foundation, in particular of its well-regarded Division of Science Resources Studies, it is, de jure a continuing response of the National Science Board² (NSB) to a congressional mandate requiring that it provide periodic reports on the status of science in the United States. The letter of transmittal of SI/76 from the Chairman of the NSB to the President is a near replica of the SI/74 letter, except that the phrase it contained describing the series as one of "high priority" to the Board was deleted from the more recent version. I was tempted to propose that SI/76 and its predecessors are not even publications in the traditional sense of the term, since they have no authors of credit.³ But I have little ground for such a claim in these days of rampant ghost writers in which, as S.J. Perleman observed, the individual author is becoming "as extinct as Battenberg insertions, Pluto Water and Ayvad's Water Wings" (Perleman: 1978). This is not to suggest that the Board failed to play a guiding role in the emergence of this series. In fact, we are told, it established an internal committee on science indicators as early as 1971 (G. Holton in TMS, footnote 41).

A. A Model of Science. "Mere" data become indicators, G. Holton insists repeatedly and correctly, only as a "result of a complex interaction between theory and measurement" (TMS, p. 53). While it has to be ferreted out from between the lines of text and tables, there is at least an implicit theory or model of science that directs this effort.

Among the five models discussed by M. Kochen (TMS, Chapter 5), the one that guides NSF is most closely associated with that of "science as a supporting service," as an important mechanism for the attainment of social goals. More particularly the NSF characterization of science is roughly that of a two-tiered input-output model, output of the first trickling down to become input of the second. Inputs at the first level are "resources," money and trained scientists, which are transformed into new knowledge. Scientific knowledge, in turn, is transformed into technological innovation, generating new patents, improving the Gross National Product (GNP) and our international balance of trade in products that result from intensive "research and development" (R&D). Table 1-28 of SI/76 (p. 37) provides an arresting comparative picture of the course of US trade balances from 1960 to 1976 in R&D-intensive versus other manufactured products (which could be subtitled, "Booming versus Busting"). The bottom line, of course, is our net fiscal assets, which dictate both the level and tempo at which national social goals can be achieved.

For starters, the NSF model of the scientific enterprise has a number of attractive features (I defer discussion of its shortcomings until Part III). The implicit hypotheses on which it is based can be quantified and tested. Thus, for example, I take an axiom of the model to be "money drives the system" and a derived hypothesis to be "national production of new scientific knowledge increases (decreases) monotonically in lagged time with increases (decreases) in national funds expended for scientific activities." Clearly this is a singularly important hypothesis both for a basic understanding of science and for the formulation of policies that direct it; an hypothesis of equal if not similar interest to the members of the Society for Social Studies of Science and the senior staff of the Office of Management and the Budget.

The model has additional merits. Its framework, as I have suggested, is temporal and its data base necessarily, therefore, time series. Admirable. Moreover, the model suggests, albeit loosely, that science is embedded in a societal nexus, that resources for the input-output systems of science hinge on individual career and public or corporate budgetary decisions, and that these decisions turn on the public's understanding of the willingness to support the institution of science. Finally, the model represents at least some short first steps in the direction of a kind of disaggregation that is an absolute prerequisite to any deep understanding of the social and cognitive systems of science.

D. Price argues forcefully for one sort of disaggregation: a separation of research from development (TMS, pp. 76-79). He argues that we should cut the Gordian ampersand that links R to D because they "have quite different conceptual bases and functions in different social systems" (p. 76). In fact, the NSF model and data base do provide for such disaggregation (how adequately is another matter, taken up in Part III, below). They do not altogether abandon the area of development to "traditional economists and, perhaps, also to political scientists" as Price advocates (p. 76), in part at least for reasons of which he is aware: the \$38.1 billion enterprise that R&D represented in 1976 (estimated, SI/76 Appendix Table 2-6, p. 210) packs far more political clout than the sub-enterprise of basic and applied research, which makes up a little more than 1/3 (\$13.7 billion) of this amount. The fact remains that the implicit NSF model and the accompanying data base do permit disaggregation at this and several other levels.

Inputs into the science system are not taken to be homogenous. Dollars, for example, are not all equally green. A 1970 dollar is tinted through deflation multipliers a paler shade than a 1960 dollar and considerably greener than one printed in 1976. I cannot comment knowledgeably on the accuracy of the deflation multipliers that NSF uses, nor do they provide me with much instruction in the matter (see SI/76, footnote 5, p. 46), but I appreciate the option of examining time series in constant or current dollars that most tables permit. Funds are also distinguished by broad source, federal funds occasionally being further earmarked by the executive agency from which they come. New scientists entering the system are disaggregated by level and field of certification, which is about as deep into the cognitive structure of science as NSF is willing to wade, and occasionally by gender and ethnicity, which are certainly relevant to its externalities, whether or not to the internal operation of science.

The interior terrain of the system is partitioned along three main dimensions: sector of performance, field of science or technology and type of activity.⁴ The last of these, the ubiquitous trichotomy of Basic Research, Applied Research and Development, raises the hackles of several contributors to TMS other than Price. As I shall suggest, it raises mine also because it introduces a quagmire of shifting definitions and of consequently altering interpretations of some basic aspects of the science system as it operates in contemporary America.

The implicit NSF model then is really quite sophisticated at this relatively early stage of its development. It is dynamic, quantitative, societally embedded and somewhat disaggregated. That it appears not to work very well empirically and that it is far from satisfactory as a mechanism for advancing our understanding of science and the policies that affect it are matters that I shall take up in Part III.

B. Data for the Model. The three versions of Science Indicators are an evolving data base for at least informal evaluations of the model that I have sketched. The second edition, SI/74, contained some major departures from the original. The voice of the Delphic oracle that occupied a chapter of SI/72, ("A Delphic Experiment," SI/72, pp. 83-93) has since been silenced, mercifully some would contend. "Sophisticated opinionating," opinionated Price (TMS, p. 72). Even with this deletion, SI/74 was two-thirds longer than the original and substantially improved in several respects. The chapter dealing with "Science and Engineering Personnel," for example, was expanded from 14 pages of text in 1972 to 29 in 1974. The accompanying tables grew from 17 to 24.

A first glance at SI/76 suggests that it is a clone of its predecessor except for the addition of one or two data points to most tables. Such is not the case. About a quarter longer than SI/74, the current version contains both further improvements and other changes that are disturbing. Although it was patterned closely after SI/74, three differences are noted: the addition of some new indicators (and the deletion of others), some changes of data for common time points "because of changes in some classifications and the acquisition of more accurate information" (SI/76, p. vii), and better documentation of data sources (together with a partial patchy bibliography). The first and third changes are minor but to the good. The second may drive proponents of policy-oriented social indicators up the wall. Clearly one cannot quarrel with the correction of factual errors, but major reclassifications of data or changes in the classification system itself tend to dampen one's faith in the data and to make life miserable for longitudinal analysts who wish to use them.

The 182 pages of text and 110 pages of appendix tables of SI/76 are broken into six chapters: International Indicators of Science and Technology; Resources for Research and Development; Resources for Basic Research; Industrial R&D and Innovation; Science and Engineering Personnel; Public Attitudes Toward Science and Technology. Each chapter is introduced with a set of "highlights," each bullseyed in the Washington fashion (actually, NSF appears to shoot square bullets). For example (p. 2):

The United States contributes significantly to the world's scientific and technical knowledge base. Non-U.S. authors cited U.S. scientific publications 15 percent more in 1975 than could be expected from the U.S. share of the world's scientific literature--citations to U.S. chemistry, physics, and biomedical research publications were respectively 42 percent, 30 percent, and 26 percent more than could be expected.

I have chosen this particular highlight as an illustration for several reasons. It shows clearly the view of science as a national activity that is inherent in the "support service" model of science. It is also illuminating because it is atypical in several respects. It is one of the relatively few conclusions not derived from NSF or other federal data, but taken instead from a commissioned study. It is almost the only instance of the explicit use of even a rudimentary probability model, and its supporting data are contradictory.⁵ W. Kruskal ("Taking Data Seriously," TMS, pp. 139-169) complains of the "considerable endogamy" (p. 142) in SI/72 because of its heavy use of NSF-generated documents. While his complaints about inadequate documentation, treatment of error structures and statistical conceptualization are well taken, I do not agree that the S.I. series are overly reliant on NSF data. The economic and demographic data on science generated by the Foundation are simply the best and most thorough available.

In order to give a sense of what SI/76 does and does not contain, I shall consider Chapter 3, "Resources for Basic Research" in some detail. The chapter's 25 pages contain 21 tables or figures, of which 18 have to do with expenditures or obligations (two numbers that can be quite different: see SI/76, footnote 7, p. 71), two with publications and one with scientists. These figures and summary tables are backed up by 22 appendix tables which provide detail about sources of funds and performers of basic research.

What emerges is a picture of growth in current dollars (estimated 1976 expenditures for basic research were up 35 percent to \$4.75 billion from 1970, Appendix Table 3-1) but decline in constant dollars (down about 8 percent in the same period), with about two-thirds of the tab being picked up by the federal government; of increasing concentration of basic research in academia (51 percent of expenditures in 1970, 55 percent in 1976); and of some spectacular shifts in the way the pie was divided among fields (for example, in constant dollars between 1970 and 1976, the biological sciences grew by 15.7 percent while astronomy declined by more than a third, Appendix Table 3-6). In 1975, academia consumed 54 percent of the funds for basic research (Appendix Table 3-2) and produced 73 percent of the articles that appeared in that year (p. 66).

The text of this chapter, as with SI/76 generally, is a substantial improvement over the earlier versions. Its liberal sprinkling of careful caveats should dispel many of the misgivings voiced by Holton, Kruskal and others in TMS.⁶ Thus, on p. 67 we are provided a thoughtful list of deficiencies in the chapter's indicators, which includes their lack of treatment of any substantive aspects of basic research, of research productivity or effectiveness, and of information about applications. Concern also is voiced, altogether appropriately, about the imprecision of the classification of resources and activities into basic versus applied research versus development, a concern that is repeated frequently in TMS. About the trickle down between the two tiers of the model as I characterized it earlier, NSF has this to say (p. 67, see also p. viii):

Even with . . . estimates of the contribution to growth made by new knowledge in general, there is no method for relating the cost of basic research in

particular with its total returns--intellectual, social and economic. However, the many and varied uses of basic research suggest that the benefits are substantial, particularly in comparison with the relatively small investment involved.

The chapter moves quickly from a dozen highlights through a brief examination of national funding patterns for basic research by broad sources and into a more detailed study of federal funds by specific agency source. Displayed data usually are time series for the period 1960-1976. The chapter proceeds to a consideration of expenditures (or obligations) for basic research in each of the major performance sectors: universities and colleges, FFRDC's, federal laboratories, industry and nonprofit institutions. It concludes with a very brief stab at saying something about payoffs for the investment in basic research that has cost the nation more than 4 billion (current) dollars annually since 1974. Data are derived from a specially commissioned study of publications in "a set of U.S. scientific and engineering journals which were intended to be representative of the total literature in each field" (SI/76, p. 88). Articles were further identified by U.S. sector of the first author's attributed location "on a sampling basis" otherwise undescribed. As their design, collection and analysis are described, I do not find these data to be convincing. Nor is any attempt made to connect them to earlier parts of the chapter. Thus, empirical investigations of the implicit model themselves remain implicit. And so it goes pretty much throughout SI/76.

C. Implicit Prophecy. Kruskal muses about the reasons for NSF's heavy utilization of time series and concludes ". . . a fair guess is that predictive extrapolation is the major motive. One uses the series, apparently, to make an informed guess about their future course" (TMS, p. 162). No such guesses are made in the SI series, but the portents, as Ziman refers to them (TMS, pp. 281-283), are there. Reading the entrails of SI/72 and SI/74, Holton sees "a sense of the ending of the 'endless frontier' in the United States, the decrease in momentum and advantage with respect to other countries and the glum future of contracting prospects for political and financial support for science and technology" (TMS, p. 54). Such gloomy portents emerge most clearly in SI/74, beginning with its first sentence of text (p. 2): "The proportion of the Gross National Product (GNP) spent for R&D has declined steadily over the last decade in the United States, while growing substantially in the U.S.S.R., West Germany and Japan. . . ." The Soviet data, we learn in a footnote, should be treated as "gross estimates." On the same page of SI/74, we find that, although U.S. scientists have won the lion's share of Nobel prizes in science, the share "declined after the 1951-1960 decade, primarily as the result of fewer prizes for research in physics."

This sense of impending doom is rather more muted in SI/76, where we read that the decline in R&D expenditures as a fraction of GNP occurred "during the middle and late 1960's" and continued "into the early 1970's," (SI/76, p. 2) and that the other major countries except for Japan weren't doing all that well either. In fact, the R&D/GNP fraction in the U.S. was a relatively steady 2.3 percent between 1973 and 1976 (Appendix Table 1-1).

The Nobel prize picture also is a bit altered: "While U.S. scientists received all the Nobel prizes awarded in 1976, the U.S. share of total prizes has remained at about 50 percent since 1946." "Hoorah for our physicists," one wishes to shout patriotically. What a difference a couple of additional data points (together, possibly, with some intra- and inter-office politics) can make.

What should become clear is that the Science Indicators series is an evolving improving work that models science, that documents the model empirically and that should provide useful guidance for science policies. One of several innovations in SI/76 seems to anticipate a complaint by Holton about the emphasis in SI/72 and SI/74 on the international "competitive, not the cooperative, element." Appendix Table 1-10 of SI/76, titled "International Cooperative Authorships of U.S. Publications. . ." should please him, as much as this rapidly improving series should please all who are concerned with patterns of the institution of science. This said, I shall consider briefly:

III. WHAT SCIENCE INDICATORS/1976 IS NOT

SI/76 is not a good model of science. More properly, the model that it provides is inadequate in respects that are crucial both to a scholarly understanding of science and to its wise management. I have only two reservations about the characterization of inputs to the system. Data on fiscal resources, which must be the best most thorough of their kind available anywhere, are well disaggregated, with two important exceptions: the failure to make distinctions between grants and contracts and between direct and indirect costs. Particularly for funds that flow into basic research, the distinction between grants and contracts could conceivably have a substantial impact on the traditional relationship between sponsor and scientist and on the nature and quality of the scientific work performed. The distinction between direct and indirect costs may be far more important to an assessment of the actual cost of doing science than the most accurate deflators that money can buy.

I recognize that the two suggested forms of disaggregation would involve definitional nightmares and thus may be impractical as additions to the SI series, but they do merit systematic research. While contracts in some agencies may differ little from grants in others, conventional scientific wisdom seems to hold that the form of support has strong implications for researchers, such as their freedom to follow new lines of investigation suggested by the early results of a project. Similarly, while indirect cost policies may vary wildly among both supporting and performing agencies, it is nonetheless important and immediately relevant to know how such variations affect the amount of research that is done per dollar in different fields and sectors.

Important as are such missing features of the inputs, they pale in contrast with those that are omitted from the interior of the model. What is missing there is virtually the entire cognitive and social structure of science.

The authors of the Science Indicators series are clearly aware that any attempt to assess the health of science without closely considering the state of scientific knowledge would be meaningless and ludicrous. The series provide at least a modest range of data on scientific publications by country, field and employment sector and on citations to them, taken as indicators of their "influence." Yet Holton, after vividly describing the "turbulent worldwide activity" that characterized the year 1972, can say "there is no indication of any of this in SI/72" (TMS, p. 58). In an important respect, this is wrong. Some big events do show up as publications and are duly entered into the publication and citation counts. The problem is that anything resembling a major breakthrough simply gets buried within the fat-layers of aggregation. To study the growth of papers in and citations to physics, for example, ignores everything that we are learning about the fact that networks of literature growth and utilization are clearer and more meaningful in the smaller specialties of a discipline (See, for example, TMS Chapters 8 and 9). But Holton is getting at more than the need for better disaggregation in counts of publications and citations. I agree with his contention that such indicators cannot reflect singular historical events that may have powerful influences on the conduct of science. Such events may be of a scientific nature, such as the completion of the world's largest particle accelerator, or political, such as Nixon's sacking of his Science Adviser. A brief qualitative chapter on such contemporary events would be a welcome addition to the series.

Nor are the authors unaware of the intricate social fabric within which scientific knowledge is produced. Horizontal differentiation abounds throughout the tables of SI/76 and there is even some recognition of vertical stratification (see SI/76 Appendix Table 3-11, for example). Yet these are pale reflections of the social structures, such as authority and reward, and processes, such as mobility and aging, at a level that sociologists of science insist are fundamental to any real understanding of how the system of science operates.⁷ Granted the body counts of Nobel laureates, there is still virtually no sense of science as vocation. I say this despite my conviction that Chapter 5, Science and Engineering Personnel, is among the best and clearest of the lot in SI/76. What is missing in this chapter, in my view, is a fundamentally important conceptualization of science as cohorts of scientists pursuing more or less successful productive careers in science, cohorts that are produced by academic departments of varying quality, whose parentage is presumably detectable in the career paths of their products.

As to the output sectors of the model, one can only shrug one's shoulders and agree with the authors who say:

While some difficulties remain in the defining and obtaining of input indicators, indicators of output present still greater challenges and consequently are less developed. Many of the "outputs" or results of science are the product of other social entities as well, so that in measuring them one is measuring more than the effects of science. In addition, many of the

results that science is thought to have are not definite enough to be measured directly (SI/76, p. viii).

SI/76 certainly examines an array of logically convincing outputs: publications and citations to them, patents, and innovations. It also considers a plausible set of economic return indicators, such as royalties and fees, relative unit labor costs in high technology industries, and foreign trade balances. The trouble of course is the absence of critical evidence that links investment in science to economic return. And it may be that such evidence is, for the present at least, beyond our methodological ken (on this, see the Chapters by Kochen, Grilliches and Ezrahi and also Ziman, pp. 265-68 in TMS).

Whatever their weaknesses, the output indicators do suggest that some real and possibly disturbing changes are taking place, particularly in the industrial component of the system. U.S. patents, for example, do appear to be declining, but the levels of aggregation are such as to make the appearance less than certainty. More work with patents, "innovations" and "breakthroughs" probably would be useful if pursued at a much more disaggregated level, possibly that of individual product lines that are of comparable R&D intensity.⁸

SI/76 is not consistent empirical history. Revising history is good clean intellectual fun except, perhaps, when the event revised is something so concrete as the number of current dollars expended in a particular year for a given scientific enterprise. I understand and respect NSF's reasons for such revisionism. Recall we were forewarned that "some numerical data" were altered "because of changes in some classifications and the acquisition of more accurate information" (SI/76, p. vii). It is only when changes in classifications are such as to alter our perception of, say, the relative deprivation of an entire scientific field that such alterations become disturbing. Some illustrative results of such alterations can be seen in Table I. Somewhere between the publications of SI/74 and SI/76, the 1969 current dollar totals for basic research somehow were shaved to the tune of \$298 million. This kind of surgery

[Table I on next page]

lopped \$353 million from the two versions of 1971 current dollars, wiping out the 2.6 percent gain between 1969 and 1971 that was reported in SI/74. Astronomy suffered egregiously, having lost more than 40 percent of its reported 1971 basic research obligations, escalating the earlier reported 23 percent decline to about 30 percent over this brief interval. Of course, astronomers could claim that their productivity per dollar necessarily increased by a corresponding factor over this period.

The reason for at least some of this is to be discovered on p. 73 of SI/76: most of the NASA funds that were classified as going to basic research in SI/74 had been reclassified by the publication of SI/76 as going to development. All of which suggests to me that the ubiquitous trichotomy might well be junked. Would not expenditures/obligations reported jointly by field and sector, both presumably less arbitrary classifications, tell us most of what we want to know in this regard?

TABLE I
Comparison of Federal Obligations for Basic Research
By Selected Fields of Science, 1969 and 1971, as Reported in SI/74 and SI/76

Current Dollars (millions) in Fiscal Year and Field of Science					
Source *	1969		1971		
	Current \$	% of total	Current \$	% of total	% of change
	<u>All Fields</u>				
SI/74	2077	100	2132	100	+2.6%
SI/76	1779	100	1779	100	0.0
Diff. as % of SI/74	-14.3	---	-16.6	---	---
	<u>Life Sciences</u>				
SI/74	569	27.4	596	28.0	+4.7
SI/76	539	30.3	570	32.0	+5.8
Diff. as % of SI/74	-5.3	----	-4.4	----	
	<u>Physical Sciences</u>				
SI/74	819	39.4	743	34.8	-9.3
SI/76	662	37.2	582	32.7	-12.1
Diff. as % of SI/74	-19.2	----	-21.7	----	-----
	<u>Astronomy</u>				
SI/74	282	13.6	217	10.2	-23.0
SI/76	174	9.8	122	6.9	-29.9
Diff. as % of SI/74	-38.3	----	-43.8		

* Source: SI/74 and SI/76 data are both taken from their respective Appendix Tables 3-6.

Finally, SI/76 is not a forceful policy document. I say this in part for reasons that I have enumerated above: the model is too highly aggregated to reveal the fine structure of science and is misspecified by the exclusion of basic cognitive and social characteristics. Some of its data are inconsistent (see my footnote 5). Its interpretation "is left mainly to the reader" (SI/76, p. vii).

Some of these shortcomings, as I judge them to be, may be ironic results of the fact that SI/76 and its predecessors are political documents. The NSB was clearly and properly motivated by political considerations in creating these reports, as was the congress in mandating them. Other agencies, particularly the Office of Management and the Budget, have a keen interest in the fiscal and other political implications of science indicators, and of social indicators generally, one would imagine. These "political contexts of science indicators," as Y. Ezrahi refers to them in an excellent analysis (TMS, Chapter 12, pp. 285-327), may also have a bearing on two additional shortcomings of SI/76 as a policy document: the facts that it is largely non-analytical and that it deals only briefly with or ignores altogether some of the most basic problems of contemporary science.

The time series that SI/76 provides are clearly valuable and interesting indicators. But unless one is willing to take the position that time itself is a causal variable, one must recognize that such series are only descriptive of recent history while the implicit model on which they bear is analytical, involving logical as well as temporal priorities, concerned with cause and effect. Evaluation of the model calls for extensive statistical manipulation and, eventually perhaps, a fully mathematical formulation. For the present, one would be happy to see a few year-specific multiple regression coefficients, for example. The intelligent reader does not have to be journeyman statistician in order to be given an intuitive appreciation of what such statistics do and do not have to say.

As to the failure of SI/76 to confront basic issues, I refer both to societal problems whose solutions may require major scientific efforts and to problems that reside within science itself. To harp on topics not included is always to risk at least the appearance of taking cheap shots. Even so comprehensive a document as SI/76 cannot possibly ride everyone's favorite hobby horse. But still, there are at least one or two societal issues and another one or two within science itself about the immediacy and importance of which there must be something approaching consensus among knowledgeable people.

One such issue is energy to which five paragraphs of SI/76 are devoted, two of which discuss pollution abatement. The Delphic Oracle gave this problem a 97 percent rating on the extent to which it could benefit from science and technology and an 86 percent rating on its need for major increases in R&D (SI/72, Table A, p. 85). Another is world food supply and distribution, which the Oracle overlooked altogether (but see the entire issues of Science, 9 May 1975 and Scientific American, September 1976). Energy research is a relatively new component of big science. Agriculture is the oldest and in some respects still the biggest of the scientific fields of study, and one which has come under heavy fire in recent years (NRC 1972; Mayer and Mayer,

1974). Both energy and agricultural research would be difficult areas to cover in the Science Indicator series since they may differ from traditionally studied disciplines in both their organization and output; they may be more Baconian than Cartesian, Haberer (1969) seems to suggest. Nonetheless, the magnitude of the human and scientific problems which these research areas face surely merit systematic consideration.

Within science itself, the series has dealt thoroughly, if not altogether satisfactorily, with some of the Big Issues, such as where the money comes from, in what amounts and what is done with it. There are others which the three versions of the series have dodged, some wisely. However, to avoid certain major contemporary issues is a disservice to science. Two such issues are the supply of highly trained scientists relative to the foreseeable demand for them⁹ and the quality of their scientific training. The former is so obviously relevant a matter as to require little further comment, except possibly to say that a spotty table of unemployment rates (SI/76, Appendix Table 5-19) is less than a frontal attack on the supply/demand problem. Although the growth rate of new Ph.D.'s has gone to zero or slightly less, the population of U.S. doctorate scientists continues to grow exponentially.¹⁰ Surely the Science Indicators Series is an appropriate forum for the discussion of this issue, yet the implications of even those relevant data that were contained in SI/76 (Appendix Tables 5-20 - 5-26, pp. 288-293) were left untouched.

Related questions that concern the quality of graduate education in science and engineering were ignored altogether. I believe that it is important to know whether the burden of Ph.D. production is continuing to shift from the most highly regarded graduate departments to those of lesser reputation, as evidently was happening a few years ago.¹¹ If so, what implications does this have for the careers of cohorts of doctoral scientists that are being produced? In this regard, is the increasingly used route of post-doctoral study a wise investment of time and money, or is it only a "holding pattern" for otherwise unemployed Ph.D.'s?¹²

One final reason why SI/76 is not an altogether compelling policy document: the model gives the appearance of not working particularly well. Constant dollar inputs have been sliding downhill for the past decade, yet the science/engineering labor force continues to grow as do our scientific publications and world citations to them. The unemployment rate remains trivial for scientists and engineers. The U.S. share of Nobel prizes is holding steady. One can imagine heads nodding in agreement within the offices of OMB: evidently American science can survive and even prosper with far less than the funding levels to which it became accustomed in the post-sputnik years. Such a conclusion cannot be justified by the data in SI/76 or its predecessors. Nor could it be justified, I am convinced, by any set of relevant valid Indicators. Unemployment is not a key issue. Irrelevant employment is. To evaluate the state of a nation's scientific effort, the number of its research reports is far less important than is their quality. The number of Nobel prizes may reflect something about the state of science, but only with a substantial time-lag.

Some of the relevant data are to be found in SI/76 and its predecessors, but one must search through layers of aggregation to find them. Between

1973 and 1975, the number of scientists and engineers in our 100 leading research universities increased by about 3,300 (SI/76, Appendix Table 3-12) and the number in any kind of R&D activity by 15,000 (NSF, 1977, p. 6) while the number of new doctoral scientists and engineers produced between 1972 and 1974 was more than 56 thousand (SI/76, Appendix Table 5-26). If young scientists represent new blood infused into the research enterprise, then science may soon need geritol (see also SI/76, Appendix Tables 5-5 and 5-8). Although the indicators tell us little about the impacts of these trends on the quality of scientific research, it is difficult to imagine that they are enhancing it. I believe that the model is essentially correct, but I suggest that additional indicators, further disaggregation and analysis are necessary if we are all to be convinced.

IV. WHAT SCIENCE INDICATORS 1978 CAN BE

Reality probably dictates that the next edition of the Science Indicators series will resemble closely the current one. However, the history of the series suggests that it will contain useful improvements. Even so modest an innovation as the inclusion in SI/76 of an index to text and appendix tables is greatly appreciated by the reader. A similarly useful addition would be a methodological appendix, that could range from a brief description of at least the commissioned studies, which are not readily available, to a comprehensive discussion of all sources of data that are included. The latter extreme would require so much additional material as to make its publication as a separate volume a possibility.

Substantively, one would hope to see at least some first steps toward innovation in two major respects: disaggregation and analysis. An initial example of the former would be a modification of funding data by suitably adjusting the activity classification (about which the Director of NSF's Division of Science Resources Studies has some cogent thoughts. See C. Falk, 1973) and then breaking funds into direct versus indirect costs, at least in the academic sector. An example of the second might consist of a table of regression coefficients for selected years and fields of science, the dependent variables consisting of publications from academia (which produces nearly three-fourths of the total. See SI/76, p. 66) and citations per publication at the level of either academic departments or preferably of individual authors. Independent variables could include appropriate measures of the prestige of the employing department (R.T. Harnett *et al.*, 1978), federal and other research funds to the institution for that field of science (this would provide at least an approximation of a critical test of the major hypothesis), age of the author (and its square: is there an "over-the-hill" effect?) and quality measures of authors' graduate and postdoctoral (if any) training (are there measurable effects of the type and quality of graduate education of science? If so, how far into the career do they last?). Even though the data for such an analysis exist, the sheer labor involved in matching, merging and cleaning them for analysis would be substantial and might involve some political difficulties, as might the analyses and publication of results. For all their problems, such results could yield such large increments to what we know about how science works as to make the effort well worth its costs.

Even if SI/78 and its successors depart from the current version in only the most modest of ways, the NSF and its governing Board should continue to receive the sincere appreciation of the entire scientific community. The inevitable accompanying brackbats simply signify our recognition of the importance of this work, which is, as Derek Price aptly noted, a noble effort.

FOOTNOTES

1. A not inconsiderable number. Students of the scientific institution established in 1975 a Society for Social Studies of Science. The organization today has more than 500 members drawn from the spectrum of social sciences. Its membership also includes physical and biological scientists and several who are directly involved with science policy.
2. The National Science Board was established by the U.S. National Science Foundation Act to provide policy direction for NSF.
3. If one looks carefully over the inside of the front cover, one will discover the names of four persons who make up the Science Indicators Unit: R.R. Wright, D.E. Buzelli, G.R. Glaser and J.S. Bond. These are the defacto producers of SI/76.
4. Performance sectors are partitioned into academia, with universities sometimes broken out from two- and four-year colleges, federal laboratories, federally funded R&D centers (FFRDC's), other non-profit organizations, and business and industry and the ubiquitous "other." Fields correspond reasonably well to those of academic science (such as physical science, broadly, or astronomy more narrowly). Technological fields correspond more closely to conventional partitions of industrial products, such as chemicals or aircraft.
5. Text Table 1-6 contradicts Text Table 1-7. In the case of chemistry, for example, Table 1-6 claims that 65 percent "of all citations (sic) found in publications of other countries" go to U.S. publications. If this were the case, the foreign citation ratio for chemistry would have to be 2.96 instead of the 1.42 reported in Table 1-7. The correct figure, as I have reconstructed it, is 31.2 percent of non-U.S. references going to U.S. publications. The 65 percent evidently includes those from the U.S.
6. It is unfortunate that TMS, published in 1978 with a Preface dated September 1977, contains only one essay (Holton's) that makes even minimal use of SI/74 and none that alludes to SI/76, although I am told that the authors were given copies of the latest version well in advance of its formal release.
7. Granted, the NSF Science Indicators Unit probably cannot justify the doing of basic research because of its scholarly interest. But even academic sociologists of science raise questions that are of substantial relevance for policy. One such is whether awards, such as federally supported postdoctoral fellowships or research grants, are allocated on the basis of an applicants'

achievements or on what could be more particularistic grounds, such as the prestige of the Ph.D.-granting department.

8. Holton (TMS, footnote 65, p. 67) will be disappointed to see that the tabular form showing the distribution of major U.S. innovations by size of company (SI/76, Figure 4-30, p. 118) is unchanged from the format of SI/74, about which he correctly complained.

9. NSF has in fact given supply/demand balances thoughtful study (NSF 1971). The failure to deal with this problem in SI/76 thus is all the less understandable.

10. Although the number of new Ph.D. degrees awarded in science or engineering was 38 less in FY 1975 than in 1974, this gives no sense of the birth rate. About 11 percent of the 19,048 persons who received Ph.D. degrees in science or engineering in FY 1975 planned to seek employment outside the U.S. (NRC, 1976 Table 2, p. 14). This left about 16,950 eligible additions to the doctoral labor force, which in the same year was reported to be about 277,500 (SI/76, Appendix Table 5-9, p. 281). Thus, while about 6 percent of the domestic doctoral labor force was "reproduced" in 1975, less than two percent was needed for replacement of those who left the labor force in the same year. Nor does this take into account immigrating doctoral scientists, about whose numbers little is known (NSF, 1973 B). Granted that about a quarter of the new doctorate recipients indicated plans for postdoctoral study, but a roughly comparable number was coming out the other end of the postdoctoral pipe line.

11. Analyzing NRC data from their Doctorate Records File, J. Niland (1972) found that Ph.D. production in departments rated as "good" "strong" or "distinguished" in 23 fields fell from 74.1 percent of total production in 1960 to 55.5 percent in 1970. In the same period the contribution of unrated departments rose from 2.6 to 17.4 percent of total production.

12. From the NSB's perspective, a systematic appraisal of graduate education in the sciences would not be a new departure, but a return to the theme of its first annual report (1969-A, 1969-B).

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Science policy in the 1960's was often dismissed as another "literature in search of a field." No doubt the demands of policy-relevance were too often at odds with the more subtle but insistent constraints of an emergent social science. All the more surprising, therefore, to find among its offshoots a literature on science indicators: acquiring not only wide-spread policy influence, but something like tolerance and even sub-field status in the social studies of science.

If this is so, it is chiefly the work of the Science Indicators Unit, and their reports for the National Science Board of NSF, entitled Science Indicators, or SI/72, SI/74 and SI/76. These are government documents, a fact of some significance which many critics have failed to grasp.¹ From that point of view, the undertaking is successful to the extent that it is useful and influential in various policy arenas. Its relevance to academic discussion about the sociology of science is obviously important. Among other effects, it can lead to improvements in the series. But references and other signs of usefulness in the literature would be secondary to its utility and relevance in policy processes.

There are, of course, defects and ways in which the series can be improved. I shall suggest some of these later on. But the Indicators reports have achieved really remarkable influence in the science policy arena in Washington, and in several other countries and organizations. This is in addition to its academic status, and contributions of the project to state-of-the-art in policy intelligence. I shall explore some of the dimensions of this influence, a view from the outside, from a sympathetic and not disinterested outsider.

The NSF Connection. The U.S. involvement of government and science seems almost indecently intimate, more alarming than say, President Eisenhower's parting discovery of a military industrial connection. We find government agencies not just funding research initiatives, but openly soliciting them at professional meetings; we find complete non-governmental research installations set up and supplied with research support services, their results often marketed or monopolized by the funding agency; and we find science curriculum packages designed and actively marketed, budgetary lobbying, extensive personnel exchanges, and so forth. In all of this, NSF has become the key linking mechanism. It is impossible to imagine U.S. science without this organization, which certainly warrants more critical attention from scholars than it has received.

Two aspects of the NSF connection should be singled out with respect to indicators: their pioneering work in science statistics, and their annual problems in fighting for what amounts to a very large science budget.

Part of the foundation's original mandate of 1950 was to keep a register of U.S. scientists. Indeed much of "Science, The Endless Frontier" was taken up with the attempt to transform the early surveys of research into acceptable time series. By 1954, the data-gathering function was made explicit, including responsibility for evaluating the impact of research activities. The now-familiar

sectoral surveys were devised at that time, and have since been collected in continuous series.² The definitions and survey designs have been copied extensively around the world. Ten years later, when Christopher Freeman drafted the "Frascati Manual" and attempted to standardize and rationalize national surveys for OECD and UNESCO, he was essentially following a guide already devised by NSF.³

The usefulness to policy makers of these massive annual increments of survey data must have been questioned when, in the late sixties, hard and specific decisions had to be taken about priorities. A number of field retrospectives and goal discussions were commissioned at that time, often from NAS, the traditional fountainhead of whitewash. But the moment for conversion of science statistics into indicators for policy use had arrived. Dr. Heyns, a senior academician and Board member associated with the project in its early stages, later reported to a House Subcommittee:

By late 1971, we had identified six major purposes or functions of the science indicators reports, most of which in retrospect still are appropriate:

- (1) To detect and monitor significant developments and trends in the scientific enterprise, including international comparisons.
- (2) To evaluate their implications for the present and future status and health of science.
- (3) To provide the continuing and comprehensive appraisal of U.S. science.
- (4) To establish a new mechanism for guiding the nation's science policy.
- (5) To encourage quantification of the common dimensions of science policy, leading to improvements in R & D policy-setting within federal agencies and other organizations, and stimulating social scientists' interest in this important area of public policy.
- (6) To provide a regular focus for the Board's annual reports.⁴

The Indicators project suffers somewhat, while it immensely benefits from the NSF connection. On the benefit side, the connection meant that the ambitious undertaking described by Dr. Heyns was at least in some measure realizable. The Foundation was already beginning to fund and contract for research in most of these areas quite extensively,⁵ and of course the facility was in place for generating and analyzing survey data. These resources, together with a small staff and contracting budget, and a measure of independence derived from its connection with the Board of NSF, placed the Indicators Unit in an admirable position to create something new and useful to the formation of science policy.

One measure of their success in this is the fact that at least three countries now have science indicators projects underway, (Brazil, Mexico and Canada).⁶ Apparently several other countries have investigated the project, along with NATO and OECD. The latter has had an active committee on science indicators for some time.

On the other side, the NSF connection has meant involvement in a very large bureaucratized organization, with elaborate review procedures and committees, and with more or less continuous political activity on the outside as well, particularly with OMB and Congress. A casual romp through the back issues of "Science and Government Report" will reveal the last point in graphic detail.

To illustrate, all of the reports have graphed the expenditures series in both current and constant dollars, using a deflator derived from the implicit price index of GNP. This was instrumental in showing the erosion of real support levels for research after 1968 (Chart 2-7, SI/76). (A more dramatic, and probably more accurate indicator might have gone further and normalized this series by the numbers of researchers, to take account of growth.) But the message was not lost on OMB, and SI/74 was credited with restoring much of the lost ground (SGR, June 15, 1976). However, Congress traditionally considers only current dollars in budgetary debates. Apparently one of the first tasks of the new Office of Technology Assessment was to attack the use of this deflator.

NSF must be ever vigilant in these matters. The Bauman amendment of 1975 would have given Congress an item veto over any NSF grant proposal. The present effect of Proposition 13 and Senator Proxmire and colleagues on the NSF appropriation for fiscal 1979 could be altogether devastating.⁷

The polity runs on information. As David Easton noted, it particularly favours supportive information. In this situation, the Indicators reports are used, along with the results of policy research and contracted evaluations generally, despite the disinterestedness and rigor of the work itself, to provide necessary support for given amounts and implicit priorities. Policy information of this sort, particularly the evaluative material, invariably comes to perform a legitimizing as well as an informative function.

State of the Art Improvements. The early imagery of the indicators literature has evolved somewhat over the years. Covering notes and Congressional statements in the early years were much into the "health of science." This betrays perhaps an organismic view of science as an ailing patient, safely etherised, ready for diagnosis and possibly expensive treatment. Continued interest in such items as "capacity to perform," "aging faculty" and the like introduce a definite sexual concern in the imagery. Though I don't agree with Professor McGinnis* that a full-blown system model is behind SI/76, the surface language has clearly shifted in that direction.

In the first stage of the introduction of a system model of science, one would expect to see it used as a way of just condensing a vast amount of data, and re-ordering the indicators. But up to the present, they have been refining indicators based on the pattern and categories of the NSF surveys and other source data: manpower surveys, sectoral divisions, basic vs applied, intramural vs extramural, public attitude surveys and so forth.

The public opinion of science section is an interesting holdover from SI/72. It was a small section overshadowed at that time by the professional opinion

*Robert McGinnis, "Science Indicators 1976: A Critique," to be published in Social Indicators Research.

of a Delphi panel, covering some interesting points. After 1972, the Delphi material was dropped, and the public opinion survey has expanded considerably, which I take as a signal that the legitimizing function is not dead.

The "output" indicators, particularly those based on two commissioned studies of citations and innovations,⁸ are no doubt used in this way as well, but they are more important for their influence on state-of-the-art science indicators, and state of research in sociology of science. I shall only mention in passing the role of such federal agencies as NSF and DoD in virtually contracting out the creation of the information service industry. But it is quite to the point that the sociology of science would be very different if research were not piggy-backed on large-scale bread-and-butter contracts to such firms as Computer Horizons, Gellman Assoc., Institute for Scientific Information, etc. One problem is that the field has difficulty in checking the imposition of its methodological standards on large-scale research of this kind, if it has such standards, and must often rely on the contracting agency to carry out this function.

SI/74 carried the dissenting opinion of a Board member on the crude use of national publication counts from SCI for example. SI/76 is indeed more sophisticated in this respect. It documents the sources more adequately; it also makes use of the abstracting services for publication counts by field.⁹ In citation indices, though not elsewhere, it often normalizes the data series as well, so as to overcome limitations in the source data. For example, instead of just taking the linguistic bias in ISI journal coverage, citation to publication ratios by field by country are used. In this way a kind of highly aggregated index of cosmopolitanism is also generated, measuring the extent of foreign use of U.S. publications.

These are important improvements, and more are promised for SI/78. Nevertheless, quotation of the source does not absolve the authors from a duty to provide technical and procedural notes on the sources. (Of course, the academic literature, particularly in citation analysis, has all these shortcomings as well.)

If the advice of several of the academics in Metric is followed, future reports will contain the results of specific analyses of research activities at a "middle range" level of aggregation. This would impose an even greater necessity for full technical notes, and this applies to the NSF surveys as well.

Policy makers and others would have a better grasp of things if they understood the severe limitations of many government surveys. For example, a critical analysis of 36 surveys was recently carried out for the American Statistical Association, with quite significant and devastating results.¹⁰ Statisticians are not the value-free creatures of legend: recall only Petty and Pearson, the morbid and self-serving political arithmetician, and the chief number cruncher of the eugenics movement.¹¹

SI/76 provides reasonable and reasonably complete information on such important policy issues as the extent of the erosion of the support base; it provides interesting but incomplete information on several effects of this in

areas such as aging, employment and support facilities; it provides a number of good proxy indicators, and fairly complete references to the literature, on the issue of U.S. technological edge and the debate over technological protectionism.

It has ignored much of the vital area of the support infrastructure of research, unfortunately leaving indicators of the communication systems and the costs of journals to another project.¹² Of course, it is not many things. It is not for example, informative with respect to dynamics of particular research areas, or the critical ways by which the dispersion of resources is changing. The level of aggregation, as others have pointed out, is often too large to really inform debate. (It can be noted that the exact opposite is the case for the related academic literature.) What we must have one day, the more so in countries with strategies of specialization, is objective and evaluative information about the relative dynamics of research fronts. I believe indicators of this sort are inevitable: much better therefore to work on them critically and in the open, and to prepare to use them in the same way.

SI/76 is not yet a system of indicators nor indicators of a system, (if it were, it would not respect national boundaries). The project has not yet given us the policy intelligence tool which it has always promised. But it is an achievement, and it will encourage and influence science policy debate in many quarters.

Footnotes

1. The critical response is represented in Toward a Metric of Science, Y. Elkana et al (eds.), Wiley (N.Y., 1978). The problem of standards for the scientific evaluation of work in policy analysis seems to be unresolved. One presumes therefore socio-economic implications of this for scientists such as physicists and systems engineers who undertake policy research. A paper by Christopher Wright, "Policy-oriented Science Indicators," 4S Meeting (Boston, 1977) may have adopted an approach similar to this critique, but the proceedings have not been published as yet.
2. The situation was extensively documented at the 1958 Meeting of the American Statistical Association. See especially the papers by Jacob Perlman, Willis Shapley and Kenneth Sanow, published in Methodological Aspects of Statistics on Research and Development Costs and Manpower, NSF (Washington, 1959).
3. Frascati Manual, OECD, 1963, revised edition (Paris, 1976); and Christopher Freeman, The Measurement of Scientific and Technological Activities, UNESCO, ST/S/15 (Paris, 1969).
4. Roger Heyns, Subcommittee on Domestic and International Scientific Planning and Analysis, Committee on Science and Technology, U.S. House of Representatives, Washington (May 19, 1976).
5. Their current annotated list of 251 projects is issued by the Policy Research and Analysis Division, and it is a treasure of science policy research: "Summary of Active Awards and Completed Projects," NSF (Jan. 1978).

6. Unfortunately, the special resources of NSF are not as portable as the Indicators project. Only Japan has better survey data (in the industrial sector), although data in most OECD countries are now comparable in many respects. Inadequate and discontinuous surveys, different attitudes to public information, and instability in political development, all tend to constrain the chances for the successful development of science indicators abroad.

7. R.J. Smith, "Budget-Cutting Mood Begins to Hit Science" Science, 201: July 28, 1978, p. 330; A Roark, "NSF's Budget: The Snipers Take Their Toll," SGR, 8:13 (Aug. 1, 1978).

8. Computer Horizons, Evaluative Bibliometrics, NSF:NTIS:PB-252339, (Washington, 1976); Gellman Research, Indicators of International Trends in Technological Innovation, NSF-C889 (Washington, 1976).

9. NFAIS, Science Literature Indicators Study, 9 (Philadelphia, 1975). These time series are by field with the authors broken out by sector.

10. ASA, Development of Survey Methods to Assess Survey Practices, NSF (Washington, 1978).

11. Skeptics should refer to William Petty's, An Essay in Political Arithmetick concerning Ireland, which the Irish have perversely kept in print, and Bernard Norton "Karl Pearson and Statistics," Social Studies of Science, 8:1 (Feb., 1978).

12. King Research, Statistical Indicators of Scientific and Technical Communication (1960-1980), NTIS:PB-254 060, NSF (Washington, 1976). Because of its heavy reliance on secondary sources and questionable surveys, attributing most charts to "Market Facts, Inc." the resulting report which came out in SI/74 format has become known in the trade as the "Lampoon Edition" of Science Indicators.

Science Indicators and Other Indicators: Some User Observations*

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The reports on Science Indicators which began in 1972 have been a useful addition to the analysis of science and technology policy issues. These reports have served to bring together in one place many of the statistical series which serve in various ways as indicators of the health and progress of the nation's science and technology enterprise.

There is, however, general agreement that these SI reports are still in their infancy. While the promise that science indicators will make a significant contribution to the understanding of past developments, current trends, and future policy making, and while the 3 SI reports issued to date** have made a modest contribution, much remains to be done.

Comments from the perspective of a user can take several forms. Some users might focus on the need for the addition of specific indicators which are not currently included but which they believe would be particularly helpful to the users. Other comments might focus on one or more of the methodological aspects affecting the compilation and use of the indicators. Yet others might pay attention to the effects which the SI reports have had on the nation's science policy.

In this paper the focus will be on the lessons and insights that have been learned from the development and use of indicators in a number of other fields and their applicability to SI. That experience appears to have made almost no contribution to the evolution of the SI reports and might well, if selectively applied, serve to further improve both the content and use of the SI reports.

An immediately obvious characteristic of most other indicators is the nature of their frequency and timing. For most, whether they are economic, environmental or some other type, timing and frequency is closely tied to their use. The frequency and timing of the SI reports is largely a matter of historical accident. The biennial frequency is the result of the statutory mandate placed on the National Science Board to produce an annual report, and the decision of the Board to devote this report to SI every other year.

Given the fact that these reports have generated the level of interest they have, and given the potential for their increased use, the frequency and timing of the SI reports probably should be reexamined. From the users' point of view it would no doubt be best to have science indicators reported in a manner that would make them available at a time when major decisions about science are about to be made. In the case of the Congress this is, of course, the annual authorization and appropriations activities.

But an even higher frequency should not be excluded. The frequency of other kinds of indicators is, as noted, closely tied to their use and range, from

*A version of this paper was presented at the Third Annual Meeting of the Society for Social Studies of Science, November, 1978, Bloomington, Indiana.

**"Science Indicators" for 1972, 1974, and 1976.

the hourly Dow-Jones reports to the ten-year census cycle. In the business world indicators on such factors as retail sales, automobile production, and electric power use appear weekly. For government use monthly reports on unemployment, consumer and wholesale prices, machine tool orders, and new housing starts are made available.

In determining the frequency of these indicator reports, utility is the dominant factor. The state of developments in science and technology affect budget allocations, program decisions, and legislative developments occurring throughout the year, and quarterly SI reports might well make a real contribution to national science policy making.

The timing and frequency of the SI reports are the more important as it is now being proposed that the timing of other major science and technology reports, such as the Five Year Outlook,¹ should be synchronized with the present timing of the SI reports, possibly leading to a reduction rather than an increase in user impact.

A further observation which quickly emerges when comparisons are made between science indicators and other kinds of indicators is the aggregation and disaggregation practices found in many other fields. For economic indicators especially, a strong trend has been the increased use of composite indicators. A single indicator, such as, for example, the Composite Index of Leading Indicators or the Consumer Price Index, is made up of a number of other, more specialized but related indicators to provide, in a single series, an over-all measure.

At the other end of this spectrum is the use of a single, often quite specialized indicator which is taken to represent broader developments. One of the best known examples is that which reports orders for industrial machine tools, and which serves to show and predict expected economic upturns or downturns in the economy as a whole.

The SI reports have not made use of either of these approaches. Instead they have chosen to present a group of varied indicators on the apparent assumption that the health of science "as a whole can be measured by examining the health of its parts."² Such an approach entails the danger of a proliferation of indicators, conflicting interpretations, and, especially for the indicator users, a loss of focus.

In the science field composite indexes might well be feasible, if not for the entire enterprise, then for the major sectors. For example, given the concern about the state of research in the universities, a composite index might well be developed based on indicators of manpower turnover, new research project starts, equipment obsolescence and others. Similar composite indexes might be developed for industrial research, industrial innovation, etc.

Individual indicators to represent broader aspects of the scientific enterprise should also be tried. Given, for example, the concern about the tenure crunch in the universities and its impact on university research, would a series devoted simply to science faculty retirements be indicative of other factors such as younger faculty hiring, increased Ph.D. and

postdoctoral hiring opportunities, and renewed intellectual vigor in university research? Given the concern about industrial innovation, would a series devoted simply to the formation of new, small R&D-based businesses serve to indicate the health of the high technology sector of industry? Given the need to decide funding levels in individual areas of science, would a series devoted to the number of unsolicited proposals, a series once termed "proposal pressure," serve to indicate the potential and vigor among the fields of science?

A different question arising from the experiences in other indicator fields is what variously has been called the question of "standards" or the issue of "normative theory." This is the question of whether there can be established for each indicator a level or a range which will enable the user to know if the indicator shows "strength" or "weakness" or, in more general terms, whether the level shown by the indicator is desirable or undesirable.

The SI reports make only the most general reference to this question. They speak of the capacity of science and technology "to meet the needs of the nation."³ Beyond this there are no attempts to suggest what the needed level of funding, number of patents, number of Ph.D.s, etc. should be.

In some other indicator fields this question has received a good deal of attention and has led to quite sophisticated considerations of the interaction between indicators, standards, and policy-making. For example, water quality indicators initially provided a single number as an indication of "general water quality."⁴ In reviewing the ways in which this index was used, it soon was realized that a single value as a standard for when water was of acceptable quality would not be sufficient. Instead, different threshold values were introduced to be used depending on whether the water was for use in irrigation, drinking, recreation, etc. Furthermore, this realization led to a system of different weightings for the individual variables used in composing the index.

Further refinements in water quality indexes specifically take into account other variables which affect water use. Thus, for example, for the purpose of allocating funds for water pollution abatement, some indexes include external factors such as the number of people served and the cost of water treatment.

A different approach to the search for standards of water quality is the use of self-integrating phenomena, chiefly the so-called biological indicators. By observing the behavior of biological organisms an integrating effect is achieved because fish, for example, simultaneously respond to a number of chemical, physical, and biological water quality variables. Changes in fish behavior, including as an extreme fish kills, and the analysis of fish population distributions can serve to demonstrate whether many variables are within the tolerance levels of fish life, thus by-passing direct measurements.

In the field of housing indicators the chief concern has been the development of indicators of housing quality.⁵ Especially interesting, and perhaps

relevant for science indicators, has been the response to the need for making adjustments in housing quality standards to match variations in the external environment, including such very practical variations as the availability of Federal housing funds.

Housing indicators take into account, for example, the level of crowding as measured by the number of persons per room. In 1950 the commonly accepted standard for overcrowding was 1.5 persons per room. Ten years later, due to improvements in housing conditions and the resulting changes in social attitudes, the overcrowding standard was widely considered to be 1 person per room. Today, with housing costs accelerating and housing expectations as a result being lowered, the standard for crowding is again being raised.

Housing indicator analysts have gone further. Some have explicitly tied the standards to the availability of resources. One such analyst noted that if the standard setting procedure "reveals that a large number of substandard dwellings exists, it is said to prove the need for greater public subsidies to assure 'decent' housing for everyone. It might, however, just as logically be said to prove the need for reformulating the criteria to assure that certain standards could be achieved within cost limits deemed to be feasible." In effect this approach begins with the level of available resources, and establishes the standards only after the budget allocation of those resources has been made. This assures that, pragmatically, i.e. within the constraints of available funds, the standard can be enforced and a degree of uniformity in achieving national housing standards can be sought.

In the field of science and technology the whole question of standard setting associated with individual science indicators remains for all practical purposes untouched. The reasons for this are many and varied and include a tradition of judgmental rather analytical decision-making, an extensive period of funding growth which made such decision making both acceptable and successful, and a concern about the soundness of such use of indicators and standards.

The emerging environment of severe fiscal constraints on Federal expenditures may well force a reconsideration of these reasons. Students, makers, and users of science indicators might well begin to consider how indicator-associated standards can be developed and used in the interest of the nation's science and technology. In such an endeavor the lessons of variations in standards depending on use, of self-integrating mechanisms for standard setting, and of the incorporation of changing external factors into standard values might well find application.

As an example, a question which is the subject of much current science policy debate is whether the scientific instrumentation and research facilities available to university researchers is sufficiently up-to-date. A numerical indicator to show the degree of instrumentation and facility obsolescence might well be developed. The standard against which to measure the level of this indicator could be established in absolute terms based on what the optimum types of instrumentation and facilities should be. But it might be better to recognize that Federal funds for this purpose necessarily are limited, and to establish instead a standard which would insure an attainable

and fairly distributed level of instrumentation and facility modernization. Furthermore, it might be well to recognize that conditions in each of the sciences vary and may change over time, thus confirming that standards once set are not cast in concrete.

Finally, it is worth making some observations about the extent to which the selection and presentation of indicators in the SI reports reflect particular assumptions and objectives. The user of indicators must always bear this in mind lest he be misled by the apparent objectivity of the numbers, tables, and trends. In the case of the SI reports, this is particularly relevant at this point in time because many of the underlying assumptions which together have formed the rationale for Federal science policy are currently undergoing serious reexamination.⁶

Students of other indicators such as crime indexes and, most notably, social indicators have noted that the selection of indicators in any field is guided by the concepts and motives of the index originators. One such student notes that "Indicator series are organized on the basis of their designers' motivations in doing the studies and their usually implicit, sometimes unconscious, assumptions and hypothesis regarding what is important to know about the phenomenon in question."⁷ An academic analyst of SI has noted in this connection: "At an early stage, the plausibility of some data for use in indicators may be a good guide. Eventually, however, there must be some explicit theoretical base for choosing some data, discarding others, and noting the absence of needed fine structure. No such explicit theoretical base appears in SI-72 or SI-74. Instead one can occasionally glimpse an implicit theory of science."⁸

The selection of indicators, another observer notes, frequently is guided by the desire to advance a particular point of view: "Typically, the party that introduces a given indicator into the discussion of a specific policy is not primarily concerned about learning the actual state of affairs. The fact that he may have complete prior conviction regarding what that state is places him in no less of a need for an indicator. His primary need is to get some other party to act on the premise that a certain state of affairs exists."⁹

These observations about other indicator systems suggest that it may be possible to examine SI for the purpose of establishing the assumptions and objectives of those who formulated and developed these reports. There have been in past discussions of SI scattered hints and suggestions about what these assumptions are.¹⁰ Based on those and a further review of the SI reports it appears that the following set of assumptions and objectives constitute a good portion of those which guided the SI designers:

1. Science and technology are invariably closely related in a cause and effect relationship.
2. We must be concerned about the comparative international performance of the U.S. in science and technology.
3. The level of Federal science funding is the overriding indicator of the health of American science.
4. The continued health of U.S. science is dependent on the continuing growth in the real level of Federal science funding.
5. University performed basic research is the mainstay of U.S. science.

In a very real sense the SI reports thus tell the user who is even moderately sensitive to this aspect of the reports a good deal about the beliefs and policy preferences of the authors.

This group of assumptions and objectives, and perhaps others not detected, are of course not new. They form part of the rationale which since 1945 have served to justify Federal funding of science. That rationale has been accepted by the scientific community, by the scientific agencies, and by the scientific committees of the Congress for many years, and it is therefore perhaps not unexpected to find it serving as the basis for the selection of the indicator series in the SI reports.

Nor is it perhaps unexpected to find that these assumptions and objectives are not explicitly stated. But that will probably not continue to be a satisfactory state of affairs from the point of view of SI users. Because the consensus about the old rationale for Federal science policy is beginning to fracture and undoubtedly will be reexamined and recast, there will be demands for both an explicit statement of the underlying assumptions and, as the new assumptions forming the recast rationale emerges, a demand for new and quite different indicator series in SI.

But equally important, the absence of an explicit recognition of this in the SI reports exposes them to what two careful reviewers of one chapter of the SI reports has called the "temptation of self-validation."¹¹ These reviewers focused specifically on that part of the SI reports dealing with "Public Attitudes Toward Science and Technology." They concluded that the design of the survey forming the basis for the analysis was shaped to find out if the public shared the scientific community's own perceptions of science and technology, the worth of its achievements, etc. They stated: "Implicit in the survey questions is a bias which appears to exclude items that would aid in interpreting the data other than the charting of quite positive attitudes toward science and technology."

In the case of the remaining chapters of the SI reports it would not appear unreasonable to note that the series selected to serve as indicators similarly may tend to serve as self-validating for those assumptions and policy preferences which led to their selection in the first place.

Not only are many of these assumptions being questioned, but in the case of a number of the indicators which have been included questions must be raised about whether, as a result of changes over time, their validity as indicators of what they originally showed have changed. For example, the continued use of funding as an indicator of the health of science raises the question of whether this indicator still measures the same thing as it did, say, 20 years ago.

Such a shift in meaning, arising from a shift in the composition of the phenomena which produce the numbers used in the indicator, has occurred in the case of unemployment statistics. The collection and use of unemployment statistics was advanced substantially during the depression years. At that time the indicator was generally taken to provide a measure of how widespread economic hardship and suffering was among the American people. Today, this interpretation is still attached to the gross unemployment

figures by newspapers and by many in the political arena. At the same time, careful students of the data on which the unemployment indicator is based note that in today's growth economy, and with substantial numbers of women and students in the work force, the indicator, and especially short-term fluctuations in it, reflects things other than economic hardship.

In the case of science funding it is likely that as an indicator it has undergone a change. It originally may have served well to show the health of science. But the growth, for example, of overhead rates on research grants from the old statutory limits of 15 and 20 percent to rates that frequently exceed 100 percent, means that a good deal of research funding goes for many purposes other than research. Similarly the portion of research funds now going to provide ships, airplanes, and other logistics and operating expenses is notably higher today than it was 20 years ago.

In the particular case of the emphasis on funding in the SI reports, it can furthermore be argued that from the point of the users perhaps this is the aspect of the policy content which policy makers such as agency heads, budget staffers, and congressmen know the most about. What they need more of, and what they themselves as non-specialists are less able to judge, are qualitative indicators of what is happening within science and technology.

All this is not intended to suggest that science indicators and SI reports are, from the point of view of a user, going down the wrong track. Rather, what is being suggested is, as noted at the outset, that a review of the experiences with the development and use of other indicators may serve to further advance science indicators so they can serve their intended purpose of assisting those who must struggle with the specifics of the nations science and technology policies in doing a better job.

FOOTNOTES

1. Public Law 94-282, The National Science and Technology Policy, Organization and Priorities Act of 1976, Section 206.
2. Harvey Brooks, "Science Indicators and Science Policy," paper presented at Review Symposium on Science Indicators, Social Science Research Council, Washington, D.C., May 12, 1978.
3. National Science Board, Science Indicators 1974, U.S. Government Printing Office, 1975, p. vii.
4. A comprehensive review of the different approaches to water quality indicators may be found in: Wayne R. Ott, Water Quality Indices: A Survey of Indices Used in the United States, Environmental Protection Agency Report EPA-600/4-78-005, January 1978.
5. See the discussion by: William C. Baer, "Housing Indicators and Standards in the USA," Ekistics, vol. 44 (August 1977), pp. 71-77.
6. I have discussed some aspects of this in; "Science - Dilemmas Down the Road," The Wilson Quarterly, vol. 2 (Summer 1978), pp. 74-81.

7. Todd R. LaPorte and Donald Chisholm, "Indicators of the Public's Attitudes Toward Science and Technology: Science Indicators 1972, 1974, and 1976 - A Review and Prospective Reflections," paper presented at Review Symposium on Science Indicators, Social Science Research Council, Washington, D.C., May 12, 1978, p. 3.
8. Gerald Holton, "Can Science Be Measured?" in: Yehuda Elkana and others (eds.), Toward a Metric of Science: The Advent of Science Indicators, Wiley, 1978, p. 53.
9. Albert D. Biderman, "Social Indicators and Goals," in: Raymond A. Bauer (ed.), Social Indicators, The M.I.T. Press, 1966, p. 103.
10. Some suggestions may be found in Brooks, op cit and in Holton, op cit.
11. LaPorte and Chisholm, op cit, p. 3.

BOOK REVIEWS

Robert K. Merton and Jerry Gaston (eds.), The Sociology of Science in Europe. Carbondale, Illinois: Southern Illinois University Press, 1977, pp. xi +383, \$19.85.

This volume provides an exception to one of my cherished insights. This insight is that the titles of sociological books always imply that the contents cover a wider range of cases than one actually finds. In the field of the sociology of science, for example, the well known publication Social Stratification in Science by J. and S. Cole is almost exclusively devoted to an examination of the American academic physics community. My insight led me to predict that in an edited collection with the title The Sociology of Science in Europe I would find overviews of the field in a few European (mainly Western) countries. The Western European countries are well represented in this collection. There are individual essays on Germany and Austria, Britain, France, Italy, and Scandinavia. Eastern European countries are not as well represented. There is one essay on Poland, one on the USSR and an Appendix, "References to Hungarian Research in the Sociology of Science." It is my impression that the sociology of science is quite highly developed in a number of Eastern European countries. This impression has been reinforced by the recent arrival of the first issue of Scientometrics. This is an international journal devoted to quantitative analysis of science. While that journal is written in English, its editorial offices are located in Hungary.

The essays on the various European national traditions form Part Two of this volume. It is the one essay that constitutes Part One of the volume that makes this a deviant case, that is, that makes this book an exception to my insight. This one hundred and forty page essay was contributed by one of the editors--Robert K. Merton. This is not Professor Merton's grand or middle range synthesis of the current status of world or European sociology of science, but rather "An Episodic Memoir." The rationale for the inclusion of this memoir is "A bit of oral history about my own experience in the United States at the time may complement the accounts in this volume of what was going on contemporaneously in the various European countries" (p. 18). Thus while the reader may approach this volume with the expectation of encountering a number of essays on European sociology of science, the first and by far the longest essay in the volume is concerned mainly with the development of the field in the United States and with the indirect and direct role of Robert K. Merton on that development. The modest nature of the title of this book is puzzling. Why not The Sociology of Science in Europe and America? I believe that the inclusion of Merton's essay, and in particular its location as the first contribution, will deflect attention from the European scene to the United States and to Merton. I, for instance, will devote more space in this review to Merton's essay than to any other contribution. There are, however, a number of comments that I should like to make about the contributions to Part Two before I concern myself with Merton's essay.

Contributors to the collection "were asked to provide accounts of the beginnings of the sociology of science in their respective countries and of research developed there to the relatively recent past" (p. x). This request has been interpreted in various ways. Klima and Viehoff suggest that in Germany, Austria and the United States a change has occurred in the focus of research.

This is that while pre-World War II work was concerned with comparative historical analysis, most recent work has had a narrower and more specialized focus. They also point to a trans-historical convergence in conceptualization and method through the influence of the work of T.S. Kuhn and that of a number of British sociologists of science. The most informative historical account is provided by Krauze, Kowalewski and Podgorecki. They discuss, at some length, a 1925 essay of Znaniecki's "The subject matter and tasks of the science of knowledge" that has not been translated into English. This essay is relevant for it focuses on "cognitive structures" rather than on the sociology of scientists. In its focus the essay not only differs from Znaniecki's well known classic The Social Role of the Man of Knowledge but also concerns itself with issues that are at the center of the interest of many contemporary researchers. In addition to this essay, important works of the late Stanislaw Ossowski remain untranslated. While the translation of contributions that are even as potentially significant as those of Znaniecki and Ossowski is not likely to bring the rewards that original research brings, Krauze et al. are in a unique position to make a contribution to the English speaking community of scholars in the sociology of science by translating this body of work. Mulkay's essay on Britain focuses on work that has been influenced by Merton's writings, as well as the more recent research that finds "Kuhn's work as a more suitable point of departure" (p. 240). Mulkay devotes quite a bit of space to a discussion of Jerry Gaston's examination of the high energy physics community in Britain. Gaston is an American sociologist. While his work tells us something about the British physics community, it does not tell us anything about the interests of sociologists of science in Britain. A most curious essay is the one on the sociology of science in France. My first reaction to this essay was that I was reading more about the development of French scientific, educational and sociological communities as researched by non-French sociologists than by natives. The historical analysis relies heavily on the work of Ben-David and T. Clark, while the analysis of the contemporary scene explores the contributions of Ronald Brickman and Elizabeth Crawford among others. I soon realized that I was not familiar with the name of the author of this chapter, Paul Frank. There are no references to his own work in his discussion. An examination of the list of contributors revealed that Paul Frank is "the pseudonym of an American scholar and observer of the French scene for several years." Are there no sociologists of science in France who could have written this chapter? Does the author fear that his entree and/or funds would have been in jeopardy had he identified himself? The question of authorship makes this a curious contribution. At the same time this author, in contrast to many other contributors, examines the social sciences. The interesting work of a number of French scholars on the diffusion and popularization of scientific knowledge is also presented. Barbano (Italy) and Jamison (Scandinavia) also explore the social sciences. The latter briefly examines trans-national developments. In this respect he argues that while there has been little "American style" sociology of science in Scandinavia and that the influence of Kuhn has been "rather small," he does suggest that "A new school of critical, Marxist-influenced social scientists has developed in each of the Scandinavian countries" (p. 336). Here two Western Europeans, Habermas and Althusser, are singled out. Finally, the essay on the USSR by M. Dobrov does not discuss the "sociology of science" or a critical Marxist approach, but is rather concerned with the structure and management of Soviet science.

Almost all of the European contributors appear to be in agreement about two statements concerning the future of the sociology of science. These are: (1) the sociology of science is a small component of what might be more appropriately called in English "the social studies of science," "science studies," or "the science of science," and (2) a major area of focus for the immediate future revolves around science policy issues. The latter transcend the methodological and theoretical baggage of the sociologist and require the hand of information scientists, political scientists, economists, and management and organizational analysts etc. The centrality of science in both socialist and non-socialist industrialized societies is apparently playing a major role in the emergence of science policy studies. My impression is that sociologists will play an increasingly smaller role in the development of these studies. In this respect the career of the sociology of science does not differ from that of other sociological specialties.

If one forgets about the propriety of the inclusion of Merton's essay in this volume, then I suggest that all who are interested in the career of Merton, the development of the sociology of science in the United States, the career of Thomas Kuhn, or the functioning of the American academic elite will be rewarded by a reading of this essay. An "oral history" from any insider is likely to be a fascinating document. This is particularly true when the insider has the writing ability and theoretical sophistication of Robert K. Merton. Both of these are amply illustrated in his essay.

In the first half of the essay Merton goes over familiar ground. He touches on the contributions of his mentor George Sarton, those of Eugene Garfield (citation analysis), those of Derek Price (parameters of science) and the role of science indicators. He also mentions his early use of what developed into content analysis and prosopography "collective biography" in the analysis of scientists and their work.

The second half of Merton's essay "focuses on some not widely known interpersonal and sociocultural contexts of Kuhn's immensely consequential Structure of Scientific Revolutions. . ." (p. 76). Merton uses Kuhn to demonstrate his position that the "self-selection of intellectual micro-environments" by scholars influences cognitive development and that "such self-selection is in turn socially patterned." Merton has always been concerned with the ways in which social structures influence the lives of individuals. He is here concerned with the life of one individual, Thomas S. Kuhn. Merton starts his analysis in 1943 when Kuhn received his B.S. *summa cum laude* in physics from Harvard. Kuhn's career is traced through The Society of Fellows, a Harvard Ph.D., a Guggenheim Fellowship, faculty positions at Harvard, Berkeley, and Princeton, a fellowship at The Center for Advanced Study in the Behavioral Sciences, and finally, membership at The Institute for Advanced Study. Merton simultaneously presents a chronological listing of Kuhn's work. At each stage in his career Kuhn was in a position to interact with what has been referred to in another context as "the best and the brightest." These environments, particularly The Society of Fellows and The Center for Advanced Study in Behavioral Sciences, "encourage the sort of wide-ranging exploration of developing interests and interaction with scholars and scientists drawn from other disciplines that make for serendipity in the domain of learning" (p. 82). It was at such settings that Kuhn was able to turn from his work in physics and to educate himself

in the history of science. Kuhn's career is a paradigm case in the significance of cumulative advantage.

Merton's comments concerning the role of cumulative advantage in American academic and scientific communities will not come as a revelation. The major point appears to be that one should be bright enough to select to attend an elite university. In a footnote, Merton states that he has not analyzed choices made by Kuhn such as "the decision to go to Harvard rather than, say Temple University or Clemson College . . ." (p. 123). Merton, of course, is a graduate of Temple University. He did, however, have enough foresight to select Harvard as the site of his graduate education. It is at the elite institutions that one is likely to come to the attention of a circle of senior scholars which is likely to "include a good share of the gatekeepers engaged in allocating rewards and resources" (p. 91). When, in their role as gatekeepers, these seniors make decisions concerning the allocation of rewards and opportunities on a national basis, they do not rely primarily on the more objective measures such as publications but rather upon first hand observation. When a student or a young colleague is defined as "absolutely first class" there is no need to have this definition confirmed by further evidence of publications. Individuals so defined are therefore spared the pressure of "frequent (and sometimes premature) publication." Thus, in the case of Kuhn "Had the criteria for a Guggenheim award been wholly confined to the record of publication, his prospects would probably have been dim" (p. 91). Merton goes on to argue that had Kuhn "been a bust" during the early years he would not have been given the subsequent opportunities that permitted him to develop his work.

While case studies are fascinating, thus the popularity of biography, they often deflect our attention from broader and more fundamental issues. The broader issue here is that students and younger colleagues who are not in a position to be evaluated on a personal basis by members of the elite appear to be at a significant disadvantage in competition for the rewards and opportunities available in academic and scientific communities. At the bottom of this entire system, as well as Professor Merton's essay, there seems to be an assumption that all of the talent necessary for the development of any particular field is possessed by the students and faculty of America's elite institutions.

The issues that Merton's essay raised in my mind may have been unanticipated by the author. If, however, we do focus on the role of elites within scientific and academic communities, as well as their relationship to the political and economic elites in the society we will be on the road to the development of a major contribution to the sociology of science in Europe and America.

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Jerry Gaston, The Reward System in British and American Science.
New York: Wiley-Interscience, 1978, 204 pp., \$18.95 cloth.

This is an important, albeit flawed book. Gaston sets out to examine the reward system in three fields in each of two countries. His purpose is to determine if the system operates fairly--do scientists get what they deserve from the scientific communities to which they belong? While concern with the operation of the reward system in science has motivated much research in recent years, Gaston's book is unique in its attempt to uncover how the system's operation is affected by cognitive differences among disciplines and by the social organization of science in a country. As he aptly notes, the failure of past research to consider the effects of cognitive and social factors has been a major limitation; differences among fields and/or countries in independent studies has made the synthesizing of results at best a risky business. In this respect Gaston has made an important contribution to the social study of science, a contribution that is certain to generate increased interest in cross-disciplinary and cross-national studies of the scientific career. Unfortunately, a number of statistical flaws and debatable conceptual assumptions leave his empirical findings ambiguous.

After presenting a general overview of the Mertonian model of science in Chapter 1, five previous studies of the reward system are reviewed in Chapter 2. Noting the disagreement among past studies on the degree of universalism in the reward system, the author suggests that this may be accounted for by differences in the designs of these studies or alternatively by the different fields and/or countries studied. This leads to the central thesis of Gaston's book: the reward system is more universalistic in more codified sciences where greater consensus exists on the importance of a given piece of research, and in countries with a more centralized science policy since in a decentralized system unequal competitive opportunities can more readily affect the allocation of rewards and resources. Chapter 3 provides a fuller description of the fields and countries considered in the study: biology, chemistry and physics, initially considered to follow that order of development in codification; the United States with a pluralistic and decentralized science policy, and Britain with a centralized science policy. Chapter 4 describes the data base. A random sample of 600 scientists (3.7% females), 100 from each of the six field/country combinations, is drawn from American Men of Science and Who's Who in British Science. These sources were used for biographic information, various abstracting services were examined for article counts, and Science Citation Index, 1972 was used to code citations. Chapters 5 through 8 consists of quantitative analyses of various aspects of the reward system.

In Chapters 5 and 6 Gaston examines the allocation of recognition and concludes that each of the communities is highly universalistic, with the following ranking of increasing levels of universalism: American chemistry, British chemistry, American biology, American physics, British biology, and British physics. The ranking of biology above chemistry, contrary to the initial hypothesis, causes Gaston to suggest that biology may be more universalistic than chemistry. However, a more convincing explanation may be that the extreme variation in degrees of codification found among subfields of biology (for

example, compare zoology with biochemistry) produces this result. But without additional information on the specialties of his biologists, very little can be determined. Given his purpose, the choice of the heterogeneous field of biology is unfortunate. Statistical and conceptual problems may also explain his findings.

Gaston's approach is to compare the relative magnitudes of particularistic and universalistic factors in regressions for recognition, as indicated by the number of awards and the number of citations received. The problem is distinguishing between universalistic and particularistic effects, particularly in regressions on citations. While in later chapters Gaston notes that citations may reflect the quality of a scientist's work as well as the recognition he receives, in Chapters 5 and 6 he considers them solely as a form of recognition. A scientist's cumulative publications is considered to reflect productivity, and the effect of publication counts on citation counts, controlling for other factors, is considered to show the universalistic operation of the reward system. Yet if citations reflect both productivity and recognition, a common assumption in the sociology of science, then the effect of publications on citations is not a clear indicator of universalism--it may not be interpreted as the effect of productivity on recognition. The fact that citations are more highly correlated with publications (his indicator of productivity) than to awards (his second indicator of recognition) suggests that citations more closely reflect achievement than recognition. Throughout these chapters, Gaston's results are contingent upon the assignment of variables to the classes of ascriptive variables and performance variables. While Gaston clearly sets out his assumptions in this regard, it is important for the reader to determine if she accepts them or not, realizing that these assumptions may have a major impact on the conclusions reached.

A less debatable concern is the statistical approach employed. While cross-tabulation, partial correlation and regression are all used, the major conclusions are based on the regressions, and so we will deal solely with this approach. In interpreting the results, Gaston focuses on the changes in the total variation explained that occur when additional variables are entered into a regression. These changes are extremely sensitive to the order in which variables are entered (due to the intercorrelations among independent variables), and in the regressions presented there is no obvious ordering of the variables. Gaston is aware of this problem and in fact presents the results of several orderings. Still the reader is left without the necessary information for determining the relative strengths of the independent variables. The presentation of standardized regression coefficients, an established procedure for dealing with this problem, would have eliminated much of the ambiguity. Second, his concern is too focused on determining which variables are important in explaining variation in the dependent variables, rather than in the way in which the variables operate. For example, he never points out the surprising fact that professional age negatively affects recognition as indicated by citations (even though age is positively correlated with citations), while it positively affects the number of awards received. In general there is little comparison of specific coefficients within equations or among equations. Finally, while the primary concern of these chapters is whether differences exist among scientific communities in the allocation of recognition, no statistical test of the hypothesis that the differences among the regressions

are due to sampling variation is performed (an analysis of covariance would have been appropriate). The few statistically significant coefficients in the field- and country-specific regressions are so similar that at least this reader is left with the feeling that the extensive efforts to rank the six scientific communities are based on regressions that are not statistically different.

Gaston's major contribution in Chapter 7 is conceptual. After reviewing the well-known Matthew effect, he suggests what he calls the Podunk effect whereby scientists at low-prestige institutions receive less recognition than they deserve due to their location in the prestige hierarchy. He also points out the possibility of the Wehltam effect (Matthew backwards) whereby scientists in prestigious universities do not receive the recognition they deserve, and the Knudop effect whereby scientists in low-prestige departments get more recognition than they deserve. This is a sound point, but empirical tests of such an effect are quite problematical. How does one determine how much recognition a scientist deserves? Gaston regresses citations on publications and assumes that scientists deserve the expected value of the regression given their number of publications. Winners are positive residuals; losers are negative residuals. The problem is that winners (with many citations and few publications) may be what Cole and Cole have labeled perfectionists--the producers of few but exceptional articles; losers may be mass producers who produce many poor quality articles. Gaston's analysis is based on the assumption that citations reflect recognition and not productivity. To the extent that citations reflect quality instead of recognition, alternative conclusions must be drawn.

Chapter 8 examines the processes of reinforcement and cumulative advantage. Gaston usefully distinguishes these two related processes. "[R]einforcement deals with why scientists continue in research activities and successfully produce scientific publications, but cumulative advantage deals with how some scientists are able to obtain resources for research that in turn leads to successful research and publication." Noting the results of past studies he indicates that the hypothesis of cumulative advantage implies increasing inequality in productivity. He proposes to test this with the coefficient of variability, an established measure of inequality defined as the standard deviation divided by the mean. Unfortunately, the measure presented is the variance divided by the mean, which will not necessarily provide the same rankings as the coefficient of variability. As a result, one cannot determine whether inequality is increasing or decreasing in his data. Gaston indicates that the reinforcement hypothesis requires controls for contextual and other effects, which are beyond the scope of his analysis, and simply presents correlations which suggest that early citations are not nearly as influential on later publications as are early publications. He suggests that publications themselves, as opposed to citations, may be a significant form of reinforcement.

In this book Gaston has summarized several important problems in the sociology of science and has suggested new ones. He has presented a framework, isolated important variables and suggested important problems. On the basis of his analysis, he concludes that scientists basically get what they deserve--universalism is the dominant norm in the scientific community. Science in Great Britain is more universalistic than in the U.S.; physics is more

universalistic than biology and biology is more universalistic than chemistry. But these conclusions must be qualified by the assumptions and methods employed. The effects of the conceptual decisions the author has made requires further study, and each reader must decide if he or she finds the assumptions acceptable. The statistical analyses are often limiting, and sometimes unintentionally misleading, telling the reader far less than would have been possible.

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ANNOUNCEMENTS

ISIS

The Editor of Isis, Arnold Thackray, writes to express his interest in publishing research on social aspects of science that conforms to the highest standards of historical scholarship.

Isis is an international review devoted to the history of science and its cultural influences. The journal was founded in 1912 by George Sarton, the pioneer Belgian historian of science. Since 1924 Isis has been the official journal of the (American) History of Science Society. Isis now enjoys a readership of between five and ten thousand scholars, in forty countries on all five continents. Each annual volume is published in five parts, with a total of between 800 and 1,000 pages. Approximately 200 books are reviewed each year and over 3,000 articles noticed in the "Isis Critical Bibliography."

Books for review and articles submitted for publication should be addressed to the Isis Editorial Office, University of Pennsylvania, 215 South 34th Street, Philadelphia, PA 19104. Subscriptions (\$26 for institutions, \$22 for individuals and \$13 for graduate students) should be sent to the Isis Business Office, Science History Publications, 156 Fifth Avenue, New York, New York 10010.

CONFERENCE ON PHILOSOPHY AND ECONOMICS: CALL FOR PAPERS

The Department of Philosophy at Michigan State University announces a conference on "Philosophy and Economics" to be held May 18-20, 1979, in East Lansing.

In a variety of ways, problems of economic activity and thought have asserted themselves with a renewed intensity in the 1970's. This has occasioned a wide-spread rethinking of the foundations of economic theory, of the place of economic considerations in formulating social policy, and of the various ways economic concerns bear on social well-being. These are philosophical issues so far as they involve an examination of basic theoretical and practical commitments, yet they touch on the activity and problems of a number of disciplines beside philosophy and economics. The aim of this conference is to promote discussion of some of these issues under four main headings:

1. ECONOMIC JUSTICE
(e.g., problems of distributive justice, justice and the market economy, the reposing of economic questions in the light of recent debates on justice)
2. METHOD AND CATEGORIES OF ECONOMIC STUDIES
(e.g., assumptions of neo-classical economics, political economy, theory of value, decision making and utility theory)
3. WORK IN AN INDUSTRIAL ECONOMY
(e.g., themes of labor and human nature, alienation, industrial democracy, technology and the quality of work)
4. ECONOMICS AND ISSUES OF SOCIAL PRACTICE
(e.g., problems in the application of economic concepts in regional planning, energy policy, educational policy)

Since these are issues of general social concern and bear on the work of a number of disciplines, we seek to avoid a narrow focus on specialized technical matters. Though our emphasis is philosophical, we believe that a fruitful exploration of these issues requires the interaction of philosophers with scholars in all the disciplines bearing on material life. Thus we hope to establish a level of discourse that is appropriate for contributions from representatives of a number of fields and points of view.

We hope to publish representative papers from this conference in book or journal form. Though we have limited funds, we hope to give at least partial support for the expenses of participants.

Papers should be submitted by February 1, 1979, to Conference on Philosophy and Economics, Department of Philosophy, Michigan State University.

For further information, contact Professor Bruce Miller or Professor Richard Peterson, Department of Philosophy, Morrill Hall, Michigan State University, East Lansing, Michigan 48824.

POSITIONS AVAILABLE

DIRECTOR, SOCIAL SCIENCES DIVISION: The current Director of the Division of Social Sciences at the National Science Foundation, Dr. Herbert L. Costner, has announced that he will be leaving that position in the fall of 1979. The Directorate for Biological, Behavioral and Social Sciences is now seeking candidates for his replacement, to serve a two-year term. The Division includes programs in Economics, Geography and Regional Science, Political Science, Sociology, Law and Social Science, Measurement Methods and Data Resources, and History and Philosophy of Science.

The Division Director is responsible for administering programs of basic research grant support, planning and budgeting, representing the social sciences in the Foundation, and providing leadership for the NSF social science effort. Appropriate candidates would have a broad knowledge of the social sciences, experience in basic research, and administrative skill. The position is excepted from competitive civil service and equivalent to GS-16 or -17, with a salary ranging from \$42,423 to \$47,500. Nominations and applications may be sent to the Assistant Director, Biological, Behavioral and Social Sciences, NSF, 1800 G Street N.W., Washington, D.C. 20550.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY, Science, Technology and Politics/Public Policy: One or two three-year appointments at the Assistant or Associate Professor level, beginning September 1979, in the Political Science Department and/or the new interdisciplinary program in Science, Technology and Society. Candidates should be prepared to teach broadly oriented as well as more specialized courses dealing with the relationships among scientific, technological, political, social, and substantive policy factors, especially in the United States. Duties include both undergraduate and graduate teaching, advising, and thesis supervision. Address applications by February 1, 1979, including vita and references to: Professor Allan Altshuler, Political Science Department, E53-473, Massachusetts Institute of Technology, Cambridge, Massachusetts 02139. Equal Opportunity/Affirmative Action Employer.

APPOINTMENTS ANNOUNCED

Dr. ROSEMARY STEVENS has been appointed to a professorship in the Department of History and Sociology of Science at the University of Pennsylvania, from 1 January 1979. Dr. Stevens (formerly of Yale University and currently chairman of the Department of Health Systems Management at Tulane University) is the author of The Alien Doctors: Foreign Medical Graduates in the United States and Medical Practice in Modern England: The Impact of Specialization and State Medicine.

ROBERT S. BUD (Department of History and Sociology of Science, University of Pennsylvania) has been appointed Assistant Keeper in the chemistry section of the Science Museum, London. Mr. Bud is currently completing his Ph.D. dissertation on "The Chemical Society of London."

4S *Society for Social Studies of Science*

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

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