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EDITORIALS**4S ENTERS THE NEXT STAGE**

Since its beginning, the Society for Social Studies of Science has been moving toward the goal--sometimes quickly, sometimes slowly--of achieving the status of a scholarly society with all the standard features of such organizations. Every year it has held an annual meeting, complete with an exciting program and the characteristic event of most societies--the not too well attended annual business meeting.

Three years ago, **4S REVIEW** became the title of our newsletter/journal, symbolically moving us toward a more mature publication for our society. The **4S REVIEW** did not achieve fully the goals hoped for it; yet, it was another stage through which the Society passed on the way to the more mature stage we are entering now.

This is my last issue so I conclude my activity as juggler of items and deadlines, mostly the consequence of trying to accommodate last-minute submissions (promised but often not received). Putting together a newsletter is more frustrating than editing a journal (and, having done both, I am confident about that assertion). As editor may resolve to meet tight schedules and then deliver the issues to members and subscribers. Without a backlog of material at one's disposal (as with most journals), that is frequently not possible with a newsletter.

Some of the frantic activities will not be missed, but old habits die hard. Since late 1977, I have been closely associated with the production of the Society's publication, either as publisher, associate editor, or editor. For each of the 33 issues over that period of more than eight years, I have taken the material to the printer, negotiated about costs, received it from the printer, and supervised the preparation for mailing (including personally attaching labels, stuffing envelopes, and so forth). For reasons that are probably peculiar to me, I will miss some of those hassles.

Daryl Chubin and **Susan Cozzens** are excellent choices to take our publication through the next stage of its evolution. I am grateful to them, and I know the membership is too, for their willingness to take on this onerous task. I extend my best wishes to them as they develop **SCIENCE AND TECHNOLOGY STUDIES**, now the official publication of the Society for Social Studies of Science.

--Jerry Gaston

STATEMENT OF EDITORIAL INTENT:
Science and Technology Studies

The various specialisms represented in 4S--history, philosophy, and sociology of science; the economics of R&D; science policy; science indicators; bibliometrics; discourse analysis; laboratory ethnography; etc.--were fragmented when the Society was born and still are, a decade later. The Society has played a special role within this diffuse network of interests: to act as a conduit for information passing among the practitioners of the specialisms, to serve as a spawning ground for genuinely interdisciplinary lines of inquiry into science and technology, and to provide collegial encouragement for those eager to pursue them.

4S works to keep parochial perspectives from obstructing our common purpose: to increase general understanding of science as a human endeavor. For that vision to take tangible form, the Society has two tools at its disposal: its annual meeting and its journal. The annual meeting is a uniquely stimulating environment for those of us who attend, but only about a fifth of us do so. The journal must therefore serve a larger outreach function. It must reflect a broad range of work, in theory, method, interpretation, and use. It must maintain intellectual quality while reaching new constituencies who need to understand science and technology as process and product. It must convert science studies observers into science studies participants. This is the vision of Science and Technology Studies, the successor to the 4S Review.

To meet these goals, we as editors must foster change in how science and technology studies are done and reported. On one hand, we will include many types of contributions in the journal: empirical research, policy reports, theory, and reviews. We need not provide details here, since you will see these plans in action in the first few issues. On the other hand, we will ask people to collaborate in new ways in the roles of reviewers and authors. Our interdisciplinary review process will be an active one, working toward improving the communication value and synthetic perspective of all contributions to the journal. We will try to make your best better by investing, and asking reviewers to invest, in your work.

We extend the invitation to lend us your support--your intellectual wares and services. We ask you to make 4S an important part of your professional life and help make its journal, Science and Technology Studies, a distinctive contribution to an expanding constituency. Your participation is the key to its success.

Daryl E. Chubin and Susan E. Cozzens
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THOUGHT AND OPINION

[The following exchanges are reproduced as submitted--except the rejoinder by L. Gouguenheim which was retyped because the submitted version would not reproduce.]

A RE-EVALUATION OF THE CONTRIBUTIONS TO RADIO ASTRONOMY OF THE NANÇAY OBSERVATORY

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A Re-Evaluation of the Contributions to Radio Astronomy of the Nançay Observatory

Ben R. Martin, John Irvine,
Tim Peacock and John Abraham

1. Introduction

During 1978 and 1979, the first two authors carried out an evaluation of the comparative scientific performance of four European radio astronomy observatories over the period 1969-78. Three years later in 1982, a postgraduate student under their supervision conducted a subsidiary bibliometric analysis of the contributions to radio astronomy made by users of the observational facilities at the French Nançay observatory. The results of the latter study have been challenged by Stewart Gillmor on the grounds that methodological inadequacies were responsible for our arriving at the erroneous conclusion that the users of Nançay contributed less to the progress of radio astronomy during the ten years 1969-78 than those of the other four observatories (see Gillmor, 1985b, p.3). Gillmor poses the question, 'But is this so indicated by the data?', and concludes that it is not. In what follows, we examine whether Gillmor's contention is borne out by the evidence and find that, despite certain technical shortcomings in our earlier study, the main conclusions still stand.

2. Historical background to the controversy

Since the details are complicated, it is necessary to start by describing the background to the dispute. In 1978, work was begun on the first study in what was to develop into a wider programme on research evaluation at the Science Policy Research Unit (SPRU). The main purpose of that first project was to evaluate the performance of five British 'big science' centres in three fields - radio astronomy, optical astronomy and high-energy physics. A significant element in the study was the development of a suitable methodology for assessing scientific performance. We focussed initially on the two radio astronomy centres - Cambridge and Jodrell Bank and, after interviewing a number of radio astronomers, decided

that the only feasible approach was to compare their performance with that of the nearest equivalent observatories overseas. This resulted in the selection of NRAO, Westerbork, in the Netherlands (which operated radio interferometry facilities similar to those of Cambridge) and the Max-Planck-Institut für Radioastronomie (MPI) at Bonn, West Germany (which carried out most of its work on a 'big dish' comparable to that at Jodrell Bank). All four centres were primarily concerned with observational galactic and extragalactic radio astronomy.

The actual assessment of scientific performance was based on an analysis of a range of publication and citation data, coupled with the results of interviews with 70 radio astronomers, who, among other things, were invited to rank the four centres (together with five other major radio observatories) in terms of their overall contributions to radio astronomy between 1969 and 1978. By asking for judgements on contributions from the telescopes at each centre, it was made clear to interviewees that the assessment was concerned with observational radio astronomy.

In 1980, a paper describing the results was submitted to the journal, Research Policy, after several preliminary drafts had first been widely circulated for comment among radio astronomers, including the directors of the four centres studied (c.f. Gillmor, 1985b, p.9).⁽¹⁾ Although the paper (Martin and Irvine, 1983) was accepted in September 1980, publication was delayed for three years because of possible legal complications. In the meantime, we were invited to submit a shortened version to La Recherche (Irvine and Martin, 1981). Although containing peer-evaluation results for the other five observatories⁽²⁾ (one of which was Nançay), the paper concentrated on the scientific output and impact of the Cambridge, Jodrell Bank, Bonn and Westerbork research facilities.

A few months later in 1982, Lucienne Gouguenheim (Director of Nançay) and Ilya Kazes (Director of the decimetric telescope at Nançay) published a letter in La Recherche taking issue with our finding that radio astronomers ranked Nançay in last place among the nine observatories they were asked to compare (see Gouguenheim and Kazes, 1982a). (A similar piece was also published in Le Journal des Astronomes Français - see Gouguenheim and

Kazes, 1982b.) The authors advanced two main arguments: first, they pointed out that the Nançay telescope was used not only for radio astronomy (involving observations primarily of galactic and extragalactic objects) as tended to be the case at the other four observatories, but also for much geophysical work. They therefore suggested that it was unfair to include Nançay among the centres to be ranked by peers alongside the original four observatories, since this was not comparing 'like' with 'like'. Second, they argued on the basis of publication and citation data which they had compiled for Nançay that, even in terms of its radio astronomy research, their observatory had made far greater scientific contributions than suggested by our interviewees.

As far as we were concerned, the first conclusion was of less import since we had not attempted a full evaluation of all Nançay's research activities. Had we done so, observatories would have been chosen for comparison which carried out a similar range of work, especially in ionospheric physics. Rather, we were concerned about their second conclusion - if the bibliometric data on Nançay's contribution to observational radio astronomy were indeed not in accord with our peer-rankings, then our approach to research assessment was in all likelihood flawed.

At the time Gouguenheim and Kazes published their critique, we were fully engaged with other research. Consequently, it was not possible to begin repeating the bibliometric analysis of Nançay's research output until a postgraduate student offered to do this as part of an M.Sc dissertation. The data were compiled under our supervision and formed the basis of a reply to Gouguenheim and Kazes, which was submitted as a letter to La Recherche and to Le Journal des Astronomes Français in September 1983 (see Plummer et al., 1983 and 1984).

Since our original study had been concerned with contributions to observational (galactic and extragalactic) radio astronomy, the evaluation of Nançay likewise concentrated on its contributions in this field. The crucial issue for us was to test whether the bibliometric data on Nançay's work in this research field were in accord with the peer-rankings which took into account only radio astronomy. Our conclusion was that the

publication and citation data relating to this component of Nançay's work, although very different to those quoted by Gouguenheim and Kazes, were "completely consistent" with the assessment of the radio astronomers whom we interviewed that the French observational facilities had contributed rather less in this area than those at the other observatories. (The term "completely consistent" was used in the English version of the letter submitted to La Recherche - see Plummer et al., 1983. The journal translated this as "elles recourent parfaitement". We have never used the term "perfect fit" - cf. Gillmor, 1985b, p.3 and p.9).

With hindsight, it might have been better in our La Recherche letter not even to have discussed the figures on solar and ionospheric research produced by Gouguenheim and Kazes, since these were peripheral to our concern. The reason for doing so was because the disparity between their bibliometric data and ours was so large that we felt it merited some attempt at explanation. In searching for an explanation, we were perhaps influenced into looking for over-simple solutions by the discovery of an arithmetical error in their main data table. Consequently, when we noted that, if papers in solar physics, planetary physics and geophysics, together with those produced by Nançay astronomers using telescopes at other centres, were added to those reporting radio astronomy observations made at Nançay, this gave a publication total very similar to that quoted by Gouguenheim and Kazes, we were led to suggest that one possible explanation for the difference between the two sets of data was that Gouguenheim and Kazes had not excluded papers based on observations obtained elsewhere than at Nançay. However, the correct explanation seems to have been that we were working with an incomplete set of publication lists, these having earlier been sent to us by the Director of Nançay. Whether the missing list for the group working on incoherent scattering in the ionosphere was never sent, or whether it was mislaid at SPRU, we have been unable to establish. When the omission was pointed out by Gillmor (1985a) in a letter to La Recherche, we immediately withdrew our earlier suggested explanation for the difference between the two sets of data (Martin and Irvine, 1985). However, we also stressed that our study had been concerned with contributions to radio astronomy (rather than ionospheric physics) and that this was unaffected by the missing publication list.

More seriously, in his most recent paper on the subject, Gillmor (1985b) has called into question the methodology employed in our evaluation of radio astronomy, pointing to inconsistencies between the original study of the four observatories and the subsequent analysis of the performance of Nançay. In the remainder of this paper, we report the results of a new study to examine the effects of adopting a more developed methodological approach to evaluate the contributions to radio astronomy of all five observatories, and thereby resolve the dispute as to whether those inconsistencies were on such a scale as to lead us to incorrect conclusions about Nançay.

3. The revised radio astronomy study

In 1983, two of us (John Irvine and Ben Martin) were awarded a programme grant by the Leverhulme Trust, one element of which was to appraise critically the methodology we have used in various evaluations of basic science. Since the first such study in 1978, the methodology employed has gradually been developed and refined. While aware of this process, the overall effect of the accumulated changes was not always fully appreciated by us at the time. For example, as a result of certain changes, each of which individually seemed relatively minor, the methodology employed in 1979-80 to assess optical telescopes (Irvine and Martin, 1983) and electron accelerators (Martin and Irvine, 1981) represented a significant improvement on that developed for the radio astronomy study. Similarly, the methodology used to assess CERN in 1982-83 (Martin and Irvine, 1984) again represented an advance. Looking back with the benefit of hindsight, it is now clear that the methodology used in 1982 to assess Nançay was more similar to that used to evaluate optical telescopes and electron accelerators than to that adopted in the very first radio astronomy study.

The methodology in that original study was limited in three important respects. First, the unit of analysis on which the evaluation focussed (the research 'centre') was probably not the most appropriate or at least was not adequately specified. Second, no proper breakdown was made of publication output into theoretical, observational, instrumental, review articles, and so on. Last, the criteria for deciding which papers

should be included were not sufficiently clear. In view of these limitations, which were only partly overcome in the methodology used in 1982 to evaluate Nançay, it was decided to repeat the study in as systematic a way as the available time allowed, applying a more recent methodology to all five observatories in a consistent manner. This, we felt, was the only way to establish satisfactorily whether the methodological inconsistencies in our earlier work had been of such a magnitude as to cause us to arrive at erroneous conclusions.

As regards the first limitation with the original methodology, it had become clear by 1980 when the studies of optical telescopes and electron accelerators were completed that the main unit of analysis in evaluations of 'big science' should be the experimental or observational research facility, not the research 'centre' as a whole. 'Big science' facilities are highly capital-intensive and their raison d'être is the production of new experimental or observational results. In addition, the inclusion of a major theory group (as at Nançay) can seriously affect the results of a comparison between 'centres' if no attempt is made to disaggregate 'theory' from 'experiment'. (3) As a result, we shall concentrate mainly on the observational contributions to galactic and extragalactic radio astronomy from all the telescopes at the five observatories, although we also present some data on contributions to theoretical astronomy and briefly discuss work in the areas of solar and planetary physics and geophysics.

As before, the starting point for constructing the bibliometric indicators was the publication lists provided by the observatories. (4) In the case of Nançay, we have used the "liste[s] des publications du Département de Radioastronomie et des radioastronomes du Département Solaire et Planétaire" previously supplied by the centre's Director. (5) As explained later, we have not fully analyzed papers from the group engaged in work on incoherent scattering in the ionosphere (6) (even though they frequently use the main Nançay telescope) since we are concerned here with comparing contributions to galactic and extragalactic radio astronomy. (7)

The first step in the analysis was then to distinguish papers appearing in learned journals from other publications (books, conference proceedings,

articles in 'popular' science journals, in-house reports and theses), with all the latter being excluded from further analysis. This is a different approach from that adopted in our earlier study where papers in published conference proceedings were included; the justification for excluding them is that many are subsequently published as journal articles and to include them would introduce a substantial element of 'double-counting'. (8)

Next, a research assistant with a postgraduate training in astronomy scanned the articles in the learned journals and classified them into one of the following categories:

- (a) observational (galactic and extragalactic) radio astronomy - defined as any paper presenting new, i.e. previously unpublished, observational data on astronomical objects outside the solar system;
- (b) observations within the solar system (9) - any paper presenting previously unpublished observational data on objects within the solar system; since, in the peer-ranking part of the evaluation, researchers were asked to compare the telescopes at observatories in terms of their contributions to radio astronomy, it was decided to distinguish observational work in the areas of solar and planetary physics and geophysics (including ionospheric physics) from that in galactic and extragalactic radio astronomy;
- (c) theoretical astronomy - any article presenting a theoretical analysis either of the nature of astronomical objects or of previously published observational results, including their interpretation (but without itself presenting any new observational data);
- (d) instrumental - any article giving experimental, theoretical or other details relating to instruments used to make or obtain astronomical observations;
- (e) laboratory experiments - any article reporting the results of a laboratory experiment of relevance to astronomy (for example, the measurement of certain atomic and molecular spectra);

(f) review articles - a collation of previously published theoretical or observational findings;

(g) 'other' - this category consists primarily of a few articles (less than 0.5%) to which we were unable to obtain access and so could not classify into one of the above categories.

It should be emphasized that the criteria contained in these definitions are applied in the above order of priority. Thus, a paper containing a description of the instrumentation used, new observational results, and some theoretical analysis of those results, would be classified as 'observational'.

In what follows, much hinges on exactly what constitutes an 'observational' paper. Once new astronomical observations have been published in the scientific literature, they become available to the scientific community at large with the result that theoretical analysis of them (which is generally relatively cheap compared with the cost of the observational work) can be carried out anywhere and not just at the observatory where they were originally made. We therefore regard the primary output of telescopes as new observational results, and exclude papers presenting analyses of previously published results even if carried out by the researchers who made the original observations. This is reflected in our definition of an 'observational' paper. Although Gillmor does not state the definition of an 'observational' paper that he is using, his reference to data "previously unpublished" (1985a, p.9) suggests that it does not differ markedly from ours. Yet analysis of the data sheets Gillmor used in his study and which he kindly sent us⁽¹⁰⁾ shows that several of the papers he has categorized as 'observational' have been classified by us as 'theoretical' on the grounds that they consist of an analysis of previously published observational data. To take two examples quoted by Gillmor: (1) the article by Heidmann et al. (1972) (which was written at the University of Texas) brings together data from several observatories, including Nançay, but makes it apparent (on pp.95 and 102) that the observations have already been published in astronomical journals; (2) similarly, the article by Balkowski (1973) contains a statistical analysis of data from

ten observatories around the world and lists (on p.46) among the sources seven journal articles in which the Nançay observations had previously been published - as far as we can see, neither this nor the other paper reports any new observations. (We have not, however, discovered any cases where observational papers have been classified by Gillmor as 'theoretical'.) We can only assume that Gillmor has actually been using a less restrictive definition of what constitutes an 'observational' paper than we did. If so, the figures he arrives at for Nançay cannot be compared directly with any of the figures we have produced for the other observatories.

In scanning the observational papers, one important task was to establish which telescope(s) had been used to obtain the data reported. Papers based on new observations which were not made on a telescope at one of the five observatories focussed upon in this study were excluded from subsequent analysis on the grounds that they did not constitute contributions from those research facilities. Conversely, papers containing new observations made with any of the facilities at each centre were included - in other words, as in the original study, the bibliometric data do not just relate to the main telescope at each centre (cf. Gillmor, 1985b, p.9). Again, our procedure led in some cases to a different categorization than Gillmor's: for example, he claims (ibid., p.7) that we incorrectly classified an article by Tully et al. (1978) which, it is stated, "does include Nançay 21-cm radiotelescope data." In fact, the article reports new observations made with the Westerbork telescope (see the detailed description of the observational procedure on p.38). The only reference to Nançay seems to be to data published four years earlier by two of the paper's co-authors (ibid., e.g. p.37).⁽¹¹⁾

Once the articles contained on the publication lists provided by each observatory had been thus categorized, subsequent citations in the scientific literature were obtained by manually scanning the annual editions of the Science Citation Index for the years 1969-83 inclusive. This means that the citation analysis has been extended for five years further than in the original study (carried out in 1979), the reason being to assess more fully the impact of papers published in the latter part of the 1969-78 period. Similar indicators of scientific performance are used

as before, except that data on scientific productivity are not reported since we have not obtained expenditure and staffing figures for Nangay. We estimate that the citation data may underestimate the 'true' citation figures by a few percent due to difficulties with matching some citations to papers and to misspelt names for which there was insufficient time to scan comprehensively. (This would, however, affect all five observatories rather than one in particular.) Let us consider in turn the results for observational astronomy, theoretical astronomy, and observations of objects within the solar system (including geophysical and ionospheric research, and solar physics).

(a) Contributions to observational radio astronomy

Bibliometric data relating to the contributions to observational (galactic and extragalactic) radio astronomy for telescopes at the five observatories are shown in Table 1. Since the relative performance of Cambridge, Jodrell Bank, MPI and NFRA is discussed elsewhere (Irvine et al., 1986), (12) we shall concentrate here on papers produced by users of the observational facilities at Nangay. As can be seen, Nangay yielded fewer observational radio astronomy papers than the other four observatories, averaging 11 journal articles per year over the period 1969-78 compared with 14 from Jodrell Bank, 15 from MPI, 16 from Cambridge and 23 from NFRA. (13) Moreover, the Nangay papers earned less citations than those from the other observatories - papers published over the four-year periods specified were subsequently cited approximately 100 times a year on average, compared with 110 for MPI, 140 for Jodrell Bank, 210 for Cambridge and 260 for NFRA. As regards the average number of citations per paper, Nangay with a figure 2.2 was slightly more successful than MPI (2.0), (14) but some way behind the other observatories.

Figures on highly cited papers are shown in Table 2. Over the ten years up to 1978, Nangay produced three observational radio astronomy papers cited 12 or more times in a year between 1969 and 1983, significantly fewer than MPI (9), Jodrell Bank (10), and well behind Cambridge (22) and NFRA (23). A similar picture emerges for papers cited 15 or more times in a year. Only with a threshold of 20 citations does Nangay do better than Jodrell

TABLE 1
Journal articles on observational radio astronomy, 1969-78

	1970	1972	1974	1976	1978	Annual average 1969-78
Number of Journal articles published in last 2 years	33 Cambridge Jodrell Bank MPI Nangay NFRA	25 34 3 20 36	25 28 30 38 43	35 26 45 18 63	3 ⁿ 25 67 17 75	16 14 15 11 23
Number of citations to articles published in last 4 years ^a	- Cambridge Jodrell Bank MPI Nangay NFRA	250 140 0 90 120	150 150 50 90 220	220 140 140 120 330	250 140 260 100 370	210 140 110 100 260
Citations per paper for publications of last 4 years	- Cambridge Jodrell Bank MPI Nangay NFRA	4.2 2.2 - 2.4 2.5	2.9 2.4 1.4 1.6 2.8	3.6 2.7 1.9 2.1 3.1	3.4 2.8 2.3 2.9 2.7	3.5 2.5 2.0 2.2 2.8

^a All the citation figures have been rounded to the nearest 10.

TABLE 2

Highly cited papers on observational radio astronomy,
1969-78

		No. of highly cited papers	No. of times that papers were highly cited
Cited 12 or more times in a year	Cambridge Jodrell Bank MPI Nancay NFRA	22 10 9 3 23	73 16 20 5 41
Cited 15 or more times in a year	Cambridge Jodrell Bank MPI Nancay NFRA	15 2 6 1 7	46 2 13 2 14
Cited 20 or more times in a year	Cambridge Jodrell Bank MPI Nancay NFRA	8 0 1 1 3	22 0 3 1 5

a Citations were scanned in the annual editions of the Citation Index for the years 1969-83 inclusively.

Bank, but the numbers involved (one and zero respectively) are so small that the difference between them may not be very significant.

Since Gillmor rightly raises the question of the reliability of research evaluation results for policy purposes, it is important to note the source of the difference between these figures for highly cited papers and those in our earlier papers. The overall numbers are generally larger in this study, partly because some papers produced between 1969 and 1978 have subsequently become highly cited in the additional period scanned of 1979-83, and partly because a more rigorous procedure for identifying highly cited papers has been adopted than previously.⁽¹⁵⁾ These factors do not, however, explain the divergence between our figures and Gillmor's. Taking his main indicator, we find just three observational radio astronomy papers produced at Nancay between 1969 and 1978 which were cited 12 or more times in a year. Gillmor reports a figure of five such papers, but includes the articles by Heidmann et al. (1972) and Balkowski (1973), which, as explained earlier, should according to our definitions be classified as 'theoretical' rather than 'observational'. (Alternatively, if Gillmor's apparently less restrictive definition of an 'observational' paper were applied to the other four observatories, it would have the effect of raising some of their figures for this indicator.)

The relative positions of the five observatories ranked in terms of the various indicators are summarized in Table 3. As can be seen, Nancay is ranked last for all the indicators apart from average citations per paper. These bibliometric results would thus seem to be consistent (within the intrinsic uncertainties of the whole procedure) with the results of the peer-ranking exercise in our original study where Nancay was ranked behind the other four observatories in terms of its contributions to observational radio astronomy.⁽¹⁶⁾ In short, our admittedly unrefined first attempt at research evaluation still generated reasonably robust conclusions on the relative performance of the observatories. While never claiming that those earlier results were 100% accurate⁽¹⁷⁾, we would maintain that they were sufficiently well founded to be potentially useful to policy-makers - this was, after all, the main aim of our study (rather than attempting to compile a definitive historical account regardless of the effort involved).

TABLE 3
Relative positions of the five observatories in terms of
their contributions to observational radio astronomy.

	1969-78					Cambridge Jodrell MPI Nançay NFRA				
	Bank					Bank				
Annual average publication rate ^a	2	4	3	5	1					
Average number of citations to work of last 4 years ^a	2	3	4	5	1					
Average citations per paper	1	3	5	4	2					
Numbers of highly cited papers	1	4	3	5	2					
Rankings by radio astronomers	1	4	3	5	2					

^a Based on the figures in the last column of Table 1.

^b Based on the figures in Table 2.

^c Based on the figures in Table 13 of Martin and Irvine (1983).

The British are sometimes criticized for harbouring an unfortunate penchant for the 'sealing wax and string' approach in their research while more affluent colleagues overseas can afford the luxury of 'gold-plated', high-technology alternatives. Given Gillmor's (1985b) comments on the subject, it is of interest to note that perhaps the most important contribution to observational radio astronomy during the period we studied was the discovery of pulsars made on an extremely cheap and comparatively primitive British instrument. The observations thus obtained were perfectly valid even though they were subsequently subject to considerable improvement using more sophisticated equipment. We would contend that establishing the right balance between cost and precision is as important in research evaluation as it is in science.

(b) Contributions to theoretical astronomy

Although we believe that the focus of assessments of big science facilities should be on their observational or experimental output, it is worth looking at the theoretical contributions made by staff at the five observatories since here a somewhat different picture emerges. However, before doing so, one important caveat should be noted. In attempting to compare contributions to theoretical astronomy, we are to some extent failing to compare 'like' with 'like'. Although not having the relevant input figures to hand, we suspect that the number of 'theoretical astronomers' at the two British observatories is smaller than that at the others. The reason for this is the institutional separation between observational astronomers (in the Mullard Radio Astronomy Observatory at Cambridge and the Muffield Radio Astronomy Laboratories at Manchester) and several of their theoretical counterparts (located in the Institute of Astronomy at Cambridge and the Department of Astronomy at Manchester University).⁽¹⁸⁾

The relevant bibliometric data for theoretical astronomy are given in Tables 4 and 5. In the first, one can see that Nançay researchers produced an average of 9 theoretical astronomy journal articles per annum between 1969 and 1978, more than NFRA (6) and Jodrell Bank (4), and only slightly less than Cambridge (11) and MPI (11). Moreover, Nançay papers published

over preceding four-year periods were subsequently cited an average of 110 times a year, the same rate as Cambridge and well ahead of MPI, the next most successful observatory with 80 citations a year. Nancay publications earned an average of 2.8 citations per paper, ahead of Cambridge (2.5) and only marginally behind NFRA (2.9).

Nancay was also comparatively successful in terms of producing highly cited papers, as can be seen from Table 5. A total of 10 Nancay papers published between 1969 and 1978 were cited 12 or more times in a year in the period up to 1983, the same number as Cambridge and considerably more than MPI and NFRA, which each recorded 5. With a threshold of 15, Nancay was slightly more successful than Cambridge (with 8 such highly cited papers compared to 6 for Cambridge), while for papers cited 20 or more times in a year, the positions were reversed (Nancay producing three and Cambridge four). Overall, the record of Nancay and Cambridge for highly cited theoretical astronomy papers is very similar, both having been significantly more successful in terms of this indicator than the other three observatories.

The relative positions of the five observatories according to the various indicators are summarized in Table 6. The results suggest that Nancay's contributions to theoretical astronomy over the period 1969-78 were appreciably greater than those of Jodrell Bank, MPI and NFRA, and not greatly different from those of the Cambridge group. Just how significant this finding is remains to be demonstrated since differences in size of research communities and funding may be an important factor.

(c) Contributions to solar/planetary physics and to geophysics

As noted previously, our original study was concerned with four centres whose observational work is concerned primarily with galactic and extragalactic astronomical objects. Researchers at Nancay, in contrast, divide their activities much more evenly between galactic and extragalactic astronomy, on the one hand, and observations of objects within the solar system (including the earth) on the other. (19) In the present study, only a limited analysis has been carried out of publications in the area. (20) From this, we have found, like Gillmor, that it is much more difficult to

TABLE 4
Journal articles on theoretical astronomy, 1969-78

	1970	1972	1974	1976	1978	1969-78 Annual average
Number of Journal articles published in last 2 years	18	22	28	17	27	11
Cambridge	8	8	10	6	7	4
Jodrell Bank	7	25	20	20	34	11
MPI	9	18	27	24	15	9
Nancay	8	15	13	10	15	6
NFRA						
Number of citations to articles published in last 4 years ^a	-	80	100	150	130	110
Cambridge	-	40	40	30	30	40
Jodrell Bank	-	50	100	80	90	80
MPI	-	30	200	130	100	110
Nancay	-	90	80	70	50	70
NFRA						
Citations per paper for publications of last 4 years	-	2.0	1.9	3.2	3.0	2.5
Cambridge	-	2.6	2.3	1.8	2.3	2.3
Jodrell Bank	-	1.5	2.1	1.9	1.7	1.8
MPI	-	1.2	4.4	2.6	2.5	2.8
Nancay	-	3.8	2.9	3.1	2.0	2.9
NFRA						

^a All the citation figures have been rounded to the nearest 10.

TABLE 5

Highly cited papers on theoretical astronomy, 1969-78

Cited 12 or more times in a year	No. of highly cited papers				No. of times that papers were highly cited ^a			
	Cambridge	Jodrell Bank	MPJ	Nancay NFRA	Cambridge	Jodrell Bank	MPJ	Nancay NFRA
	10	1	5	5	29	2	16	31
								13
	6	0	3	2	12	0	9	18
								8
	4	0	1	3	6	0	4	6
								1

^a Citations were scanned in the annual editions of the Science Citation Index for the years 1969-83 inclusively.

TABLE 6

Relative positions of the five observatories in terms of their contributions to theoretical astronomy, 1969-78

	Cambridge	Jodrell Bank	MPJ	Nancay	NFRA
Annual average publication rate ^a	1.5	5	1.5	3	4
Average number of citations to work of the last 4 years ^a	1.5	5	3	1.5	4
Average citations per paper	3	4	5	2	1
Numbers of highly cited papers ^b	1.5	5	3	1.5	4

^a Based on the figures in the last column of Table 4.

^b Based on the figures in Table 5.

distinguish between 'observational' and 'theoretical' papers in this particular area. (It should be noted in passing that we would again probably classify some papers differently from Gillmor. For example, he categorizes the paper by Hedin et al. (1977) as a Nangay 'observational' paper, even though it describes the construction of a theoretical model and apparently reports no previously unpublished Nangay observational data.)⁽²¹⁾ However, we do not believe this task of classification to be insuperable given adequate resources and time, which were unfortunately not available for the present study. This is one reason why we shall not present any bibliometric results for ionospheric research here.

However, a second and more important reason is that it is peripheral to both this study and our earlier work. In the original evaluation we were concerned with contributions to radio astronomy, and in our subsequent Nangay study with establishing whether or not the views of radio astronomers were consistent with Nangay's performance in observational radio astronomy as measured by bibliometric data. In that so much of his discussion focuses on ionospheric physics, many of Gillmor's criticisms are, in our view, misplaced. Furthermore, in order to evaluate research performance in ionospheric physics, one would need to compare Nangay with other centres carrying out work in the same area. As we have frequently emphasized, because publication and citation practises can vary considerably between scientific fields, bibliometric indicators should only be used to compare 'like' with 'like'. Consequently, we are sceptical about the attempt (Gillmor, 1985b, pp.4-5) to compare the numbers of highly cited papers produced by Nangay in the fields of solar/planetary physics and geophysics with those produced by other observatories in the field of galactic and extragalactic astronomy.

4. Conclusions

There are a number of conclusions which can be drawn from the results of the revised study of radio astronomy reported here. First, the methodology employed in our original study was inevitably less satisfactory than that adopted in later studies. The field of institutional research assessment was not at the time well developed, yet a start nevertheless had to be made with the development of suitable policy techniques. We would not, therefore, entirely disagree with Gillmor's (1985b, p.10) characterization

of the methodology as a "sealing-wax-and-string prototype". However, it was at least applied consistently to the four observatories in the original study, and, as shown elsewhere (Irvine et al., 1986), the result of adopting a more developed procedure leads to little change in the resulting conclusions about the relative contributions of the four observatories to radio astronomy.

Second, the approach pursued in 1982 to assess Nangay was somewhat different from that in the earlier study and the results were therefore not entirely consistent with those for the original four observatories.⁽²²⁾ By the same token, the bibliometric figures produced by Gillmor for Nangay are also inconsistent with those from our original study, having been derived by applying somewhat different definitions, criteria and methods for bibliometric scanning to one observatory alone rather than to all five. What we have described in this paper is how the application of a more rigorous methodology in a consistent manner to Nangay and the other four observatories still leads to the conclusion that the users of the French facilities contributed less to observational (galactic and extragalactic) radio astronomy than those at the other four centres over the period 1969-78.

Last, in the area of theoretical astronomy, Nangay appears to have been much more successful. The bibliometric data presented suggest that, together with Cambridge, it contributed more in this area than the three other observatories. We have not seen it as relevant to our task to assess Nangay's contributions in the areas of solar, planetary and ionospheric research. Nangay may, as Gillmor suggests, have been relatively successful in these areas of research. However, to establish such a conclusion convincingly, one would need to compare Nangay's performance with that of similar centres working in these fields.⁽²³⁾

Postscript on the political context of policy research

Although we have endeavoured to keep the above paper as factual and impersonal as possible while at the same time replying to all the serious criticisms made by Gillmor, we feel that it is nevertheless vital to bring

one final point to the attention of the reader. This is that Gillmor fails to give his readers an adequate understanding of the wider context in which the preceding debate took place, thereby portraying certain events as purely technical or professional errors on our part. In this way, for example, what turns out to be a relatively inconsequential issue of a missing publication list is elevated to something implying unprofessional conduct on our part. Such a charge we reject. The problem, rather, stemmed from the fact that extension of the study to include Mangay was beset by political difficulties, and, given the circumstances in which our reply was produced, we do not see that our behaviour was in any way improper. Gillmor is correct that we did not first send our La Recherche letter to Gouguenheim and Kazes for comment. This was partly because they had not circulated copies of their two articles to us for comment, and partly because of a fear on our part that pressures would be brought to bear to delay or halt publication. (24) At the time, the three-year wait for the Research Policy paper to be published had only just ended. Furthermore, shortly before then a 'Committee of Inquiry' had been set up to review the future of Mangay, apparently influenced in part by our first La Recherche article but also by wider criticism from outside the astronomy community. The Committee concluded that the Mangay telescope was worth supporting for a further ten years, and the government largely accepted this conclusion, though it did not provide all the funds requested. This was the wider context at the time we submitted replies to both La Recherche and Le Journal des Astronomes Français (JAF). Given the reluctance in some quarters (see note 24) to re-open the debate, it was perhaps not surprising that we encountered significant delays even in receiving a response. After some correspondence with the editor of JAF, who was also the Chairman of the Committee of Inquiry on Mangay, we were finally given the following, very honest reason for not publishing it:

"Given that your article effectively constitutes a response to the piece by Gouguenheim and Kazes in JAF, your paper ought normally to be published. I do not in fact have any fundamental criticism to make on its content. But, for diplomatic reasons internal to the French astronomical community, I think it would be preferable that this text should not appear in JAF." (private correspondence, 12 January 1984, our translation)

(It is somewhat ironic that a year later JAF agreed to publish an article by Gillmor (1985c) containing criticism of the very article by us it had refused to publish.) Moreover, it was only after some pressure that La Recherche finally agreed to publish our response as a letter in December 1984, well over a year after submission. We find it difficult to imagine that the Director of Mangay had not in the intervening months been shown copies of our responses by the editors of JAF and La Recherche.

This highly charged political context in which the exchange of letters in La Recherche took place is unfortunately ignored by Gillmor. Yet writing his critique as a visiting scientist in a research institute closely associated with Mangay, he was himself to become part of this wider political process and must have understood the issues involved. Perhaps with hindsight, it would have been better if we had first sent our La Recherche letter to Gouguenheim and Kazes. But if so, Gillmor, having berated us for this, should surely reflect on his own failure to send us for comment papers disputing our findings prior to submission for publication (25), thereby denying himself the opportunity to correct various erroneous criticisms. (26)

Footnotes

1. We have on file letters from the four directors, three of whom were complimentary about the study and expressed their agreement with its findings. None disagreed with the ranking of Nangay.
2. With hindsight, it would probably have been better to have left out the names of these five observatories, thereby avoiding the political problems which led to the subsequent dispute.
3. This had not been seen as a major problem in the first study since all four of the original observatories devoted most of their efforts to observational work.
4. These may, as Gillmor points out, be partially incomplete. A more rigorous approach would involve scanning all the relevant journals. Although this was the approach adopted in our assessment of CERN (see Martin and Irvine, 1984), limitations of time and resources prevented it being used here. It should be noted, however, that the directors of the four observatories covered in our original study (one of whom checked our analysis thoroughly) did not take issue with our publication counts. Our view is that the addition of missing publications would result only in marginal changes since accountability requirements to funding agencies mean that it is in the interests of large research facilities to keep reasonably comprehensive records of scientific output. Furthermore, in policy research, the costs of evaluation need to be counterbalanced against the benefits of being 100% accurate. We do not agree with the suggestion (Gillmor, 1985a, p.8) that missing even one highly cited paper can be crucial for an evaluation of a costly research facility with large numbers of users; as can be seen from Table 2, it would require another six papers cited 12 or more times to alter Nangay's relative position for this particular indicator, and even then this would assume that we had not missed any such papers for MPI. In our view, the desire to apply the painstaking and time-consuming approach of the historian of science to research evaluation is not normally feasible. In any case, as we see later, conceptual as well as practical problems often limit the extent to which it is worth pursuing 'perfection' in measurement.
5. In the period under study, there existed the so-called 'Radio Astronomy Group' at Meudon and Nangay with its own International Visiting Committee. Reports of this Committee for the years 1973, 1975, 1977 and 1979 make it clear that incoherent scattering researchers were at that time not regarded as part of the Radio Astronomy Group, while solar research was only included some time after 1977 following a recommendation to that effect by the Visiting Committee. In 1979, the Visiting Committee in their report (p.4) were critical of the organizational structure of the Nangay Observatory, and in particular the fact that "the Observatory is a part of the Department of Radio Astronomy (DERAD)" which was quite separate from

- "the Department of Solar and Planetary Astronomy (DASOP)". We do not, therefore, agree that our decision to focus on the 'radio astronomy' component of Nangay's research activities, and to exclude solar, planetary and ionospheric research, is as arbitrary as Gillmor suggests. His description (1985b, p.2) of the organizational structure at Nangay appears to relate to the situation post-1979.
6. This publication list was kindly supplied by Gillmor.
7. Mor have we included in this or our earlier study publications from Budden's group at Cambridge on ionospheric physics - Gillmor's (1985b, p.4) claim to the contrary is incorrect.
8. For an explanation of the reasons why books, popular articles and internal reports were excluded, see Irvine et al. (1986).
9. It should be stressed that such work, while often involving telescopes and satellites, nowadays tends not to be regarded as part of astronomy. For example, the US National Science Foundation, besides including ionospheric research as part of 'atmospheric sciences' (which is in turn classified under 'environmental sciences'), also includes 'solar' research and 'extraterrestrial atmospheres' in the same category (see the illustrative disciplines listed for atmospheric sciences in NSF, 1985, p.142). 'Radio astronomy', in contrast, is listed as part of 'astronomy', which is in turn grouped under 'physical sciences'.
10. We gave Gillmor an opportunity to respond to an earlier draft of this paper before submitting it for publication.
11. Furthermore, the final acknowledgement (ibid., p.47) leaves little doubt where the new observations reported were made: "The Westerbork Radio Observatory is operated by the Netherlands Foundation for Radio Astronomy, with financial support from the ... ZWO. We wish to express our thanks to the host people who, through their great care and competence, provided us with observations of high quality."
12. This paper extends the evaluation to the end of 1983, showing some interesting trends compared with those identified in the original radio astronomy paper.
13. These figures are lower than those in the original study, especially for MPI, since a much more restrictive definition of papers has been used, with conference papers in particular being excluded. It should also be noted that the figures somewhat misrepresent the performance of MPI, which really only began effective operation in the middle of the period under study.
14. Again, it should be stressed that this figure may understate the influence of MPI (see note 13).
15. The old procedure missed some papers which, after initially receiving few or no citations, suddenly became highly cited some years after publication. For details of the old and new procedures, see Irvine et al. (1986).

16. As we note elsewhere (Irvine et al., 1986), the methodological approach employed here, although not significantly altering the relative positions of the observatories, does result in a narrowing of the gap between the most and least successful.
17. Gilmor (1985b, pp.6-7) correctly points out that the papers by Crovisier (1978) and Conseil et al. (1972) were incorrectly classified in our 1982 study. Although we checked a sample of the papers categorized by the postgraduate student in order to assure ourselves about the level of accuracy, a few mistakes inevitably remained.
18. It might be argued, therefore, that a comparison of the theoretical contributions from Cambridge and Manchester should include the efforts of those researchers in these other departments who are concerned with the interpretation of radio astronomy observations.
19. This was explicitly recognised in the letter we submitted to La Recherche, which stated that "a significant fraction of the centre's research effort has been devoted to solar, planetary and ionospheric physics, and to geophysics" (Plummer et al., 1983, p.6).
20. This was necessary, for example, to establish that the missing publication list on incoherent scattering did not contain any papers on observational radio astronomy or theoretical astronomy.
21. In the acknowledgements at the end of the paper, the authors thank Arecibo Observatory but not Nangay or St Sautin de Mours (see Medin et al, 1977, p.2146).
22. For example, we did not (with the exception of highly cited papers) distinguish between types of paper in the first study and separate those involving observations of objects within the solar system from radio astronomy papers reporting studies of galactic and extragalactic objects. This was because the number of papers of the former type was so small for the four observatories that it would have made little difference if they had been excluded, so this was not seen as an important issue at the time of the first study.
23. This would in our view require rather more than merely analyzing the output from Nangay and elsewhere on the basis of papers published in a single journal (see Gilmor, 1985c).
24. We had heard from French researchers with whom we discussed our preliminary findings that any further developments in the controversy over Nangay would not be universally welcome. Our fears subsequently turned out to be well-founded, at least to judge from the response we received from Le Journal des Astronomes Français.
25. In April 1985, we received a copy of the critique in La Recherche together with an accompanying note (dated 3 April) from Gilmor informing us that it had not only been submitted but had also been accepted for publication. In June, Gilmor sent a copy of the JAF article, on which was written a note (dated 17 June) stating that this too "was recently accepted for publication". (We had by then already

been shown a copy by an American colleague.) Finally, in July, we received the 4S paper which, according to the front cover, had been "submitted to the 4S Review".

26. See, for example, footnote 7.

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In my recent article in the 4S Review¹ I examined Plummer, Martin and Irvine's (hereafter=PMI)² article evaluating the "Nançay radiotelescope" and earlier work by them, especially that published by Irvine and Martin in 1981 (hereafter=IM)³ and the response to this by Gouguenheim and Kazès⁴ of the Nançay Observatory in 1982. Now in the current issue of 4S Review (Vol. 2, No. 4), Martin, Irvine and colleagues Peacock and Abraham (hereafter MIPA)⁵ have "re-evaluated" their earlier efforts. I am pleased to see that Irvine and Martin are continually striving to validate their earlier studies but my basic points remain as I stated them¹: Neither PMI, nor IM, nor Martin and Irvine (hereafter=MI)⁶ uniquely defined how they would use the term "radioastronomy". They seemed to use different definitions without indicating those to the reader and they do not use the definition as does the International Astronomical Union. In their paper, PMI say that they used bibliometric values for Nançay obtained "in the same manner" as for the other four radiotelescopes. Now MIPA admit that PMI did not do so. In an earlier paper, IM say that they used "all" the professional publications of the four radio observatories first studied; now MIPA say that they did not. PMI say that they classified the Nançay literature in four categories, but in fact they used at least six. Now, MIPA have yet another scheme for categorization and inclusion or exclusion.

I believe that their first journal article on evaluating radio

observatories (IM) was published in the French language as was their more recent one (PMI). They use terms such as "radiotélescope" and "parfaitement" in these articles, yet seem to maintain that they are misunderstood because of the translator and they refer to an unpublished English language version of PMI which certainly few readers can have seen (I have not). Nevertheless, as I indicated in (1), Irvine and Martin speak variously of their study as being of "centres", "groups", "observatories" and "radiotelescopes" as if these were simply interchangeable terms.

In their remarks about British use of "cheap and comparatively primitive" equipment versus "affluent colleagues overseas" using "gold-plated", high-technology alternatives, MIPA seem unduly agitated, but more importantly they have misunderstood the purpose of my remark about "sealing-wax-and-string" prototypes. The major point is that I believe modern quantitative applied policy studies, such as those in which Irvine and Martin engage under contract to clients, must be held to high standards of rigor and be consistent and reproducible in their results. The point is that operational products differ from prototypes, usually, in their degree of finish and quality control. A completely different matter developed elsewhere in my article concerned Hewish's brilliant pulsar work which evolved from earlier near-earth, ionospheric scintillation studies which successively probed further out into the universe.

Overall, Martin and Irvine say that they are especially concerned with the high costs of modern big science facilities and the need to assess the output of big science, yet they seem not to have evaluated the output of the Nançay decimetric radiotelescope in all areas.

I have discussed ^{7, 8} some of the conditions faced in comparing citations received in one field of science with another. In mentioning one of my recent articles ⁸, MIPA say they are sceptical of my efforts in this, yet Martin and Irvine have combined citation results from different fields in their papers IM, PMI, and in other studies.

For me, questions remain about four general areas concerning the work of Martin and Irvine. Converging Partial Indicators: My results and those of Gouguenheim and Kazès differ considerably from the various results concerning Nançay obtained by Irvine and Martin. If one alters a definition and drops some categories and adds others, according to MIPA, Nançay's articles deemed by them to be "theoretical" (even though this includes papers that the authors have titled as data presentations) gain sufficient citations to place Nançay in second place out of five, "appreciably greater than those of Jodrell Bank, MPI and NFPA, and not greatly different from those of the Cambridge group." Yet Martin and Irvine (PMI) maintain that Nançay is ranked last of the five for what they call "observational radio astronomy" contributions. But the oral opinion poll question administered by Irvine and Martin (IM) asked their subjects to rank radio observatories according to their "contributions to scientific knowledge." Which knowledge? The results here seem neither clear nor convergent. In their latest paper (MIPA) the data concerning the other four observatories seem also to change in curious ways.

Like with Like: Martin and Irvine (MIPA) now admit that Nançay may not be sufficiently "like" the other observatories, since they were not attempting "a full evaluation of all Nançay's research activities" even though they did include solar system studies done at the other observatories. The question of "like with like" remains a major one for Irvine and Martin's

methodology.

Classification of papers: Here I believe Irvine and Martin are all at sea. Each of their studies produces virtually a new methodology: new rules are invoked, new categories are added, old ones are dropped, other categories are melded, scanning methods (they say "reading") change. I have not been sent nor have I seen their newest manuscript describing the latest methodology.

Use of bibliographical lists: This still seems to be a weak point in Irvine and Martin's work. Even though Dr. Gouguenheim, the Nançay Observatory Director, requested a response from Martin and Irvine when she sent them her observatory's data on 2 June 1982, they never sent her their PMI paper, either before, during or after its submission to La Recherche, nor did they send her any items at all during this period. The proper use of bibliographical lists is very complicated. PMI would have been wise to have allowed the Nançay Observatory Director to have assisted them in checking and assessing their final bibliography.

I do not wish to comment at length on Martin and Irvine's (MIPA) discussion of the "highly charged political context" of their applied policy studies. MIPA state that as a "visiting scientist in a research institute closely associated with Nançay" I became part of the wider political process." I am sure they have ample opportunity to discuss their ideas about the social construction of scientific knowledge. I will admit, however, that since 1958, at one time or another for extended periods I have been a research worker at several institutions or laboratories where solar system and/or galactic radio astronomy research was in progress. This includes a period as Fulbright Research Fellow at the Radioastronomy portion of the Physics Department at Cambridge University during 1976 during the

time when the work assessed by Irvine and Martin was being done. So I do accept their claim that I understand the issues.

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- 8) Gillmor, C. S., "L'utilisation en géophysique du radiotélescope décimétrique de Nançay," le Journal des Astronomes Français, sous presse (1985).

Comments on the Paper, "A Re-Evaluation of the Contributions to Radio-Astronomy of the Nançay Observatory"

L. Gouguenheim, Former Director (1980-1985),
Nançay Observatory

It is not my purpose here to give detailed comments on all the points in the above paper which appear to me not entirely correct.

I would like to concentrate on the main point raised by the work of B. Irvine and J. Martin and their collaborators, i.e. the meaning and the accuracy of their method of evaluation.

Scientific research relies on a number of rules and methods such as the use of a common vocabulary and a set of definitions of which the whole community is well aware, and the usual rule of assessing a given accuracy to any numerical result.

From this point of view, many astronomers would be surprised by the definition of radio astronomy given by the authors, which is unduly restricted to galactic and extragalactic field. The rules in the Astronomical fields are generally defined by the International Astronomical Union (IAU). Every astronomer knows that IAU commission 40, entitled "radio astronomy" is concerned with radio astronomical studies of all astronomical objects, including the Sun and the Solar System. So, from the astronomical point of view, there is no reason to reject these last studies in an "Evaluation of the Contribution to Radio Astronomy of the Nançay Observatory", and this will not be understood by the common reader who will only retain that Nançay observatory has published 112 papers on "observational radio astronomy" in the 10 years period considered by Martin et al., while the total number of publications of the observatory was equal to 500.

Another example is the definition here adopted - and to my knowledge clearly defined for the first time - of the class of so-called "theoretical

Table 1

	1984	Cambridge	JB	NRPA	MPI	Nancay
Annual Average (1969-1978) of Publications	1984	40	20	54	55	15
	1986	16	14	23	15	11
S	S	2.5	1.4	2.3	3.7	1.4
Total Number of citations in 1974 to articles published in last 4 years	1984	330	220	480	340	100
	1986	150	150	220	50	90
S	S	2.2	1.5	2.2	6.8	1.1
Id. in 1978	1984	550	190	780	610	100
	1986	250	140	370	260	100
S	S	2.2	1.4	2.1	2.3	1.0

The following comments are rather obvious:

- 1) The definition of "observational radio astronomy" in these two papers are almost the same. At least could it be understood that some papers classified in 1984 as "observational" belong to the 1986 wider class of "theoretical papers".
- 2) The numerical values quoted are subject to very large fluctuations; this can be seen from the ratio "S" of the 1984 to the present data number given in Table 1 which ranges from 1.0 to 6.8... And the authors seem to be a little optimistic when they assert that "the result of adopting a more developed procedure leads to little change in the resulting conclusion" (p. 21) (on p. 14 of this issue of 4S Review).
- 3) These fluctuations do not seem to be at random. The S values are small for Nancay (1.0 to 1.4) and Jodrell Bank (1.4 to 1.5) and clearly larger for the 3 other centers (from 2.1 to 6.8).

papers". Such a wide definition, including in particular the interpretation of observational data, has little chance to be adopted by the astronomical community.

It is my feeling that people involved in this work of scientific evaluation cannot make their own definitions and classifications by themselves, without close interaction with the scientific community involved. It is quite remarkable that this behaviour leads Martin et al. to classify as "theoretical" a paper subtitled by the authors themselves as "data"...

My next comment concerns the accuracy of the method of evaluation used by Irvine and Martin and their collaborators. In their paper published in La Recherche (1984), Plummer et al. give the following definition¹ of the papers retained by them, which they define as "category A": "Publications presenting observations made with the Nancay radio telescope in the field of extragalactic astronomy and interstellar medium" and they precise also that "in the method used to calculate the number of publications in our first study (i.e. concerning the 4 other observatories), we retained only the papers reporting on astronomical observations made in the center studied, i.e. type A communications".

From this definition, they retain the numbers that are reproduced in Table 1 as "1984 data" and which are directly taken from the paper by Plummer et al. (1984). These data concern:

- (i) the annual average (1969-1978) of publications
- (ii) the total number of citations in 1974 to articles published in the last 4 years
- (iii) same figure in 1978.

The present paper by Martin et al. defines the category of "observational radio astronomy" as "any paper presenting new, i.e. previously unpublished, observational data on astronomical objects outside the solar system". The numerical data corresponding to the 3 items described above are given in their Table 1 and reproduced in my Table 1 as "1986 data".

In order to test whether these large differences can be explained by the inclusion of papers now classified as theoretical ones, I give in Table 2 the

Table 2--Annual Average (1969-1978) of Publications

	Cambridge	JB	NRPA	MPI	Nancay
"observations"	1984	40	20	54	55
					15
"observations"	1986	27	18	29	26
+ "theory"					20
S	1.5	1.1	1.9	2.1	0.8

total annual average of both observational and theoretical publications together taken from Tables 1 and 4 of Martin et al.'s paper. Clearly, the discrepancy noted above cannot be explained in this way. We have no indication on the class to which belong, for example, the 300 papers published at MPI in the 1969-1978 period which were classified as "observational" nor astronomy" by Plummer et al. and which are neither "observational" nor "theoretical" in the present paper.

I have no real interest in classifying articles and counting citations. I just wanted to stress how tricky and subject to large errors is this kind of game and how risky it would be to use such inaccurate results to decide, for example, the future of a whole observatory.

I certainly do not wish to debate with Martin et al. on the "highly charged political context in which the exchange of letters in La Recherche

took place" (p. 23 (on p. 16 of this issue of 4S Review)) and I leave them the entire responsibility of their various assertions. I want only to precise that I have no responsibility at all in the one year delay between their submission to La Recherche and the publication of the Plummer et al. paper and that the editors of La Recherche never gave me a copy of this paper before publication. These two points are confirmed in a letter from M. Barrère to B. Martin and J. Irvine which I place at the disposal of anyone interested.

Note:

1. These definitions are my own English translation for the French version of the Plummer et al. paper, which was signed by the authors, with no indication that the translation was made by La Recherche and not submitted to the agreement of the authors: I have no way to consult the English, unpublished, reference quoted by the authors as Plummer et al. (1983).

References

Martin, B.K., Irvine, J., Peacock, T., Abraham, J., 1985 4S Review, this issue, pp. 4-18.
 Plummer, S., Martin, B.R., Irvine, J., 1983 quoted by Martin et al., unpublished.
 Plummer S., Martin, B.K., Irvine, J., 1984 La Recherche 15, p. 1610.

Rachel Laudan. *The Nature of Technological Knowledge. Are Models of Scientific Change Relevant?* Dordrecht/Boston/Lancaster: Reidel, 1984.

One of the puzzling features of the rapid expansion of science studies which has been mirrored in the growth of organizations such as 4S, has been the lack of a sociology of technology. While sociology of science has burgeoned, the study of technology has become the province of the historian, economist, and philosopher. Happily this situation is starting to change. The present collection of work by historians, philosophers and sociologists is another step on the way towards the development of a sociology of technology. The six papers in this volume were first presented at a workshop on 'Models of Scientific and Technological Change' sponsored by the Center for Philosophy of Science at the University of Pittsburgh and held in April 1981. Unfortunately Derek Price's untimely death meant that he was unable to see his paper published. Price's eclecticism meant that he, at least, saw the need to place fresh emphasis upon the study of technology.

Ironically the needs for a sociology of technology have been most convincingly demonstrated by historians such as Edward Constant and Thomas Park Hughes who, in their marvellous studies of the development of the turbojet and the electrical power industry, have found it necessary to draw upon sociological notions. The lack of interest by sociologists is reflected in the paucity of sociologically illuminating detailed case studies of the development of technology. Regrettably this is true also of the present volume - the case studies are conspicuous by their absence. A case study or two would surely sharpen the issues and

give the debates in this volume greater salience. There is one other lacuna - there is no contribution from the sociologists of science working within the school of 'micro studies'. Since our understanding of science over the last decade has been greatly aided as a result of that perspective, a paper from that tradition would have broadened the scope of the book.

The highlight of the collection is Rachel Laudan's introductory essay. She sets out in a clear and insightful way some of the problems facing the development of a sociology of technology and how history, sociology and philosophy of science might contribute. She also presents a most useful review of the relevant literature. As Laudan emphasizes, all the contributors are concerned to treat technology in its own right as an autonomous body of knowledge rather than as the appendage to science suggested by the label 'applied science'. The importance of technology as knowledge is now recognized by many scholars. The 'black box' of technology must be opened if we want to understand how, in particular historical circumstances, a variety of social, economic and technical processes get brought together to shape technology. If science can be understood in this way, why not technology too? Also noticeable throughout the collection is a welcome shift in the object of study. Although individual inventors may be important in certain cases and in certain epochs, the stress has now moved on to looking at how communities or social groups shape technology.

The theme of communities is taken up by Edward Constant in his essay, 'Communities and Hierarchies: Structure in the Practice of Science and Technology'. Constant makes a distinction between

communities in science and those to be found in technology. It is clear that successful technological innovation requires integration of a number of different communities; in science, Constant suggests, this seems to be a less pressing problem. Constant calls upon scholars of science to examine it for similar passages of development where a variety of communities act together. Indeed a recent study of the high energy physics community has indicated the key part played by the integration of different communities of specialists (Andrew Pickering, Constructing the Quark, Edinburgh University Press, 1984).

One of the most familiar community-type terms to be found in the study of science is of course, Kuhn's 'paradigm'. Garry Gutting's essay 'Paradigms, Revolutions, and Technology' is a salutary reminder of all the difficulties of such terminology. If paradigms cannot unambiguously be identified in science imagine how much harder it is to apply such notions in technology where, by common consent, a far greater number of diverse groups are involved. Indeed, the problem of delineating and identifying groups in technology in order to permit social science methodology to be successfully applied is one of the most pressing problems facing the new sociology of technology. Another Kuhnian inspired term which has created all sorts of difficulties in the sociology of science is that of 'anomaly'. Edward Constant's attempt to adapt this term to technology by referring to 'presumptive anomalies' seems to have produced similar difficulties. 'Presumptive anomalies' arise when the science of the day predicts that a particular technological system will fail. This term, as pointed out by Laudan in her essay 'Cognitive Change in Technology and

Science', needs more careful analysis and definitions if it is to be useful. For instance, in some cases science will not only predict a failure but will also offer a remedy for a successful new technology to overcome the failure. Such cases, according to Laudan, must be distinguished from those which only embody a prediction of failure.

Norman Hummon's essay 'Organizational Aspects of Technological Change', provides some welcome empirical examples, although I was left wanting to know more details. Also, he offers a distinction between the art of technology and the science of technology and how different balances are maintained in different organizational forms. The science of technology is defined as rational problem solving, drawing upon the formal structured body of scientific knowledge. Unfortunately the problem which philosophers, sociologists and historians of science have been wrestling with over the last decade is how such a definition might work for science, never mind technology. If, as many would argue, science is more of a tacit, craft based activity than has previously been allowed, then the type of distinction Hummon suggests seems to beg the question.

Much of the book addresses the theme of the similarities and differences between science and technology. The last two contributors, Derek Price and Peter Weingart, provide contrasting answers. Price's brief essay, 'Notes Towards a Philosophy of the Science/Technology Interaction', can be seen as a powerful statement of the argument that science and technology are rather similar, and that it is only our image of science (as presented by philosophers) which is wrong. Price argues that instrumental

technologies played an extremely important part in shaping developments in science. Our view of theory-dominated science is all askew. According to Price it was new developments in instruments which led the way to most upheavals in science. Weingart, on the other hand, eschewing most of the new developments in sociology of science which lend support to Price's view, argues that different 'orienting principles' embedded in (and institutionalized in) the systems of knowledge which comprise science and technology means that scientific development and technological development are not strictly analogous. Now, I doubt that few scholars would disagree with Weingart that technology does present a more diffuse and amorphous appearance than science; the underlying question, however, is whether or not this represents any fundamental distinction. If one takes seriously the recent findings of sociologists of science which portray science as an amorphous body of practices, which may be presented in terms of a variety of rationalizations, then the most promising path forward is to assume that the two bodies of knowledge are constituted by fundamentally similar social processes, and to go on to demonstrate empirically how and under which circumstances the differences between science and technology are constituted.

This slim collection raises more problems than it solves, but it is a most welcome and timely volume and does provide a necessary stepping stone on the way towards a sociology of technology. We must still await, however, the completion of Laudan's enterprise - empirical studies of technology informed by work upon science.

Trevor Pinch
University of York

REPORT OF RECENT WORKSHOP**Workshop on the Analysis of Scientists' Discourse****University of St. Andrews****20-22 September 1985**J. Potter, University of St. Andrews

This workshop was the fifth in series concerned with discourse analysis and the issues it raises for science studies. This was the first of the series to be directly sponsored by the Economic and Social Research Council. The topic for this meeting was discourse analysis and reflexivity: how should discourse analysis be conducted, and its findings presented, when it is exactly these things which are the topic of study?

The following researchers from the disciplines of sociology, psychology, philosophy and literary studies attended:

M. Ashmore (U. York)
 G. Myers (U. Lancaster)
 T. Pinch (U. York)
 P. Stringer (U. Nijmegen)
 M. Wetherell (U. St. Andrews)
 S. Yearley (U. Belfast)
 A. Mckinlay (U. St. Andrews)
 S. Parkinson (ESRC)
 J. Potter (U. St. Andrews)
 T. Walker (U. Brunel)
 S. Woolgar (U. Brunel)

The papers presented breakdown as follows:

(1) Reading Readings (Potter). This paper examined the relation between participants' and analysts' readings of discourse, and thereby explored the issue of whether members' readings can be used to 'ground' analysts' readings. Problems raised with such a procedure led Potter to argue for a more pluralist relationship between 'traditional' social studies of science, on the one hand, and the 'new literary forms movement' (which advocates forms of writing which reflexively display their own constructed status), on the other.

(2) The Critical Problems of Writing the Problem: A Double Text (Ashmore). This looked at the way the problem of reflexivity is dealt with in the work of Steve Woolgar and, in particular, his shifting use of an evaluative contrast between 'in practice' and 'in principle', each pole of the dichotomy being treated as fundamental on certain occasions. The paper was written as two intersecting discourses; one the 'straight' analysis of Woolgar's work; the other concerned with the way Woolgar's work is formulated in the first discourse. It is this kind of text, which attempts reflexively display its own textuality, which is referred to as a 'new literary form' (NLF).

(3) The Social Construction of Popular Science: The Narrative of Science and the Narrative of Nature (Myers). This was concerned with the way narratives are constructed in popular journals (Scientific American) as opposed to specialist academic outlets (Science). Myers argued that in academic journals the narrative is organised around the actions of the scientists, while in pop journals it is based on natural phenomena. In this way, the pop views promote the cultural authority of science by obscuring the processes which led to particular views of nature.

(4) Dictates of Method and Policy: Vocabularies of Choice in Scientific Work (Yearley). Here the concern was with the way research is recurrently described in terms of the scientist's long term goals, with little reference to contingent influences such as funding, promotion, equipment and so on. Making a similar point to Myers, the paper suggests that such narratives enable the effective depiction of research as socially beneficial (fundable).

(5) Fragmentation (Stringer). This paper is built up in three segments, each providing a different version of the author's own research career and its significance. The first is a 'standard' academic biography, describing events and influences; the second is a (parody of a) traditional social studies of science reconstruction; the third is a 'personal' account stressing the authors beliefs and values. The paper is similar to Myers' and Yearley's in its stress on the different narratives available for reconstructing the research process, but differs in adopting a reflexive strategy to make this point.

(6) Reconstructing Man and Machine: A Note on Sociological Critiques of Cognitivism (Woolgar). This examined the controversy in the field of artificial intelligence between sociologists (Coulter) and cognitive scientists (Fodor) over the role of brain states in explaining behaviour. Woolgar points out that despite their contrasting conclusions both 'sides' utilize similar argumentative strategies and make similar assumptions.

At the start of the workshop a straw poll was carried out to assess the participants' feelings about the importance of the issue of reflexivity. The group split approximately three ways, some suggesting a reflexive dimension is essential for doing discourse analysis, some opposing this view, and some undecided.

The papers reflected this split. While Potter's and Woolgar's were about reflexivity, Ashmore's and Stringer's adopted textual forms which made the reflexive import of their arguments explicit. For example, instead of Stringer producing a standard discourse analysis of the way different kinds of scientific life history are used for different purposes he constructed three such accounts of his own life history. The aim was to make the different conventions underpinning the writing of such histories explicit by this juxtaposition. In contrast, Myers' and Yearley's papers opted for the use of 'straight' discourse analysis with no (overt) reflexive dimension.

Perhaps the best formulation of the workshop theme was given by Ashmore in the course of the discussion: 'If... what this article is attempting to say is that realist talk is not adequate, and it does it in a way which instantiates realist talk again, then you have got a problem. The problem is trying to find ways to say that, to do that trick'.

During the course of the weekend a number of critical issues were raised concerning the need for reflexivity.

(A) Some thought the sheer difficulty of reading NLF's would put off readers and prevent their claimed effects. However, others disputed whether NLF's are in fact difficult to read, or suggested that their novelty might attract readers.

(B) It was suggested that NLF's could easily become another textual orthodoxy, which would no longer encourage the reader to question his or her own reading practices. In response, the point was made that the reader would have had to have learned a good deal about the construction of texts to find them orthodox or clearly unproblematic.

(C) Some participants worried that an over-involvement with reflexive aspects of discourse might lead researchers to downplay the effects of large-scale social influences on science. However, the alternative danger was also pointed out: it is essential to understand how much large-scale influences are formulated in discourse.

(D) The point was made that researchers might have exaggerated the need for NLF's because of an inappropriate view of discourse analysis as an empiricist enterprise. In contrast, others argued that NLF's are important not just to combat the excesses of traditional empiricist discourse but because of the persuasive qualities of realist talk of all kinds.

Despite these areas of controversy, there was a broad agreement that the exploration of ways to draw attention to the literary practices of social studies of science researchers, as well as those of scientists themselves, is useful and important. The discussion was able to helpfully elucidate some of the advantages as well as shortcomings of reflexive strategies.

Although the workshop was focussed on reflexivity, it was inevitable, given the membership, that issues of broader relevance to discourse analysis would arise. Indeed, on both nights participants were up until the small hours hotly debating a whole variety of current problems in the social study of science, both as an institution and an academic enterprise. The questions discussed included the following: Is there any sense in which more fine-grain analysis of texts can be said to have got closer to the truth? What amount of discourse need be reproduced in papers to give the reader an adequate understanding of the data? Do standard analyses of discourse embody a degree of reflexivity to the extent that they display one way of reading a set of texts? How far is the analyst drawing upon prior knowledge in the course research?

One point which came through particularly strongly was that there were very different opinions of the nature of the disagreements which had taken place in print between discourse analysts and radical relativists. This was identified as an issue in need of further examination in future.

Two further workshops were scheduled. The first, to be convened by M. Ashmore at York, will take up issues concerning reflexivity raised at the St. Andrews meeting. The second, to be convened by S. Yearley and P. Stringer at Belfast, will focus on discourse analysis and the study of science utilization. An application will be made to the ESRC for funding for the latter meeting. Any researchers with a particular interest in these topics should contact Ashmore or Yearley respectively.

SOCIETY NEWS

MINUTES OF THE 4S COUNCIL MEETING

Troy, New York

24 October 85

Present: Council Members--LaFollette, Long (Secretary), Mullins (President),
Restivo, Westrum

Local Arrangements Committee--Caporaal, Hackett, Richter, WoodHouse,
Worthington

Program Committee--Manier

Others--Edge (President Elect), Cozzens (Council Elect), Giere
(Council Elect)

1. President Mullins called the meeting to order at 9:30 p.m.
2. The Treasurer's Report was presented by Secretary Long and approved by Council. A current balance of \$9812.62 was reported. Additional revenues were expected from the Troy Meetings. It was noted that due to the major financial contribution of RPI to the Troy Meetings, the Society's income would probably exceed expenses for the first time in four years.
3. Secretary Long announced the results of the election: David Edge was elected to a two-year term as President; Ron Giere, Rachel Laudan and Susan Cozzens were elected to three-year terms on Council.
4. President Mullins announced that Joseph Ben-David had been selected for the John Desmond Bernal Award for 1985. ISI provided \$500 for the award, plus \$500 for expenses.
5. President Mullins raised the question whether a \$500 yearly contribution to COSSA should be continued. A motion to terminate funding was passed. President Mullins agreed to write to COSSA terminating our membership.
6. Manier reported on efforts to organize the 11th Annual Meeting to be held in Pittsburgh.
7. It was agreed to meet on October 26th at a time to be arranged by President Mullins. The meeting adjourned at 11:00.

26 October 1985

Present: Council Members--LaFollette, Long (Secretary), Mullins (President),
Restivo

Publications Committee--McCartney (Chair)

Others--Edge (President Elect), Cozzens (Council Elect),
Giere (Council Elect)

8. President Mullins called the meeting to order at 7:15 a.m.

9. A proposal from John Wilkes for holding the 12th Annual Meeting at WPI was discussed. It was agreed that a special section on Education in Science and Technology Studies should be included at the meetings.

Concern was expressed that high registration and room costs would be required by the proposal. The proposal was accepted with the understanding that President-Elect Edge would pursue options for minimizing the cost of the conference.

10. A motion was passed that 4S's obligations to finance future meetings should be limited to the income received from Registration Fees.

11. David Edge reported that Harry Collins has proposed the University of Bath for the 1988 Meetings. (This proposal has since been withdrawn.) John Ziman was recommended as Chair of the 4S Future Meetings Committee.

12. In order to allow a smoother transition when a new President takes office and to increase the efficiency of the operations of the Society, a proposal was passed to amend the constitution as follows:

Paragraph II.B.1. shall read:

The Society shall have a council composed of the following elective officers: a president, a president-elect, and nine ordinary council members. The term of office for ordinary council members shall be three years. The president shall serve for a one year term as president-elect followed by a two year term as president.

Paragraph II.B.2. shall read:

Three ordinary members of council shall be elected each year. If an elective office becomes vacant, the council shall appoint a replacement who will serve until the next annual election, at which time a person will be elected to serve out the remainder of the term.

Paragraph to be inserted between II.B.2 and II.B.3. shall read:

The Secretary and Treasurer (who may be the same person) shall be appointed by the President, with the approval of the Council.

Paragraph III.B. shall read:

The President-Elect shall assist the president of the Society in management of the Society.

This change will require the approval of the membership.

13. In order to justify a private non-profit status for the Society:

Paragraph VII. shall read:

In the event that the Society dissolves, assets remaining after payments of all outstanding debts will be contributed in equal shares to the History of Science Society, Philosophy of Science Association, and the Society for the History of Technology.

This change will require the approval of the membership.

14. A proposal was passed to list Council Meetings in the Annual Meeting program and to schedule these meetings for times other than mealtime.

15. President-Elect Edge agreed to set up procedures for dealing with vendors who wish to set up Exhibits at the Annual Meetings.

16. A proposal was passed to sell back issues of the 4S Newsletter and the 4S Review for \$20 per year.

17. McCartney, Chair of the Publications Committee, presented recommendations concerning the 4S Review. On the basis of these recommendations and after discussion of the Cozzens/Chubin Proposal, the following items were approved:

a. Susan Cozzens and Daryl Chubin should be appointed as Co-editors for the 4S Review for a term of three years, with the possibility of a three-year extension. They should take responsibility for the Spring, 1986 issue.

b. A maximum budget of \$11,000 was approved for the first year of operation. This figure was approved with the understanding that the Co-editors and President-elect Edge would explore sources of additional funding and seek ways to decrease the cost of producing the journal.

c. The journal's name should be changed to Science and Technology Studies.

18. To cover the increased costs of operating the Society and to allow the gradual expansion of Science and Technology Studies, dues should be increased to \$15 for Student Memberships; \$30 for Professional Memberships; and \$40 for Institutional Memberships. Requests for additional contributions to the Society should be included with dues renewals. President-elect Edge agreed to draft a letter to accompany membership renewals.

19. Votes of thanks were made to:

The Local Arrangements Committee for the outstanding efforts in organizing the 10th Annual Meeting.

Members of the administration at RPI for their generous financial support.

Jerry Gaston, who will be ending his term as Editor, for his many years of service as Editor and as a member of the Editorial Board of 4S Review.

The Publications Committee, chaired by James McCartney, for their efforts.

The Program Committee, chaired by Edward Manier, for providing an exciting program for the 10th Annual Meeting.

20. There being no further business, the Council adjourned.

SOCIETY FOR SOCIAL STUDIES OF SCIENCE AWARDS

1985 John Desmond Bernal Award to

Joseph Ben-David

[In announcing Joseph Ben-David as the 1985 winner of the Bernal Award, the Society made this announcement, and was deeply saddened to learn of his death only a few weeks following receipt of the award. A future issue of Science and Technology Studies will include an appropriate tribute to Joseph Ben-David.]

It is now twenty-five years since the American Sociological Review published a path-breaking article on "Scientific Productivity and Academic Organization in Nineteenth-Century Medicine." The article served as an inspiration to scholars everywhere, as the field of social studies of science began to develop. Appropriately enough, the author of that article, Joseph Ben-David, was a vigorous participant in the first meeting of the Society for Social Studies of Science at Cornell University in 1975.

Through his presence and above all through his writings Professor Ben-David has been a central voice in our young field. His work has always displayed an extraordinary erudition and scope, moving easily from Enlightenment Paris to the modern American research system, from seventeenth-century England to the German universities two hundred years later, and from historical specifics to sociological generalizations. The bulk of Joseph Ben-David's work in the social studies of science--itself but a fraction of his larger corpus--is scattered in numerous articles. However his central insights have been brilliantly brought together in his major book, The Scientist's Role in Society. That seminar study--first published in 1971 and reprinted in 1984--has inspired, encouraged, stimulated, and engaged each fresh wave of recruits to our burgeoning field.

For his many distinguished contributions to the social studies of science, 4S is proud to present the John Desmond Bernal Award for 1985 to Dr. Joseph Ben-David, the Stella M. Rowley Professor of Education and Sociology at the University of Chicago and the George S. Wise Professor of Sociology at the Hebrew University of Jerusalem.

4S FINANCIAL REPORT FOR 1985

<u>OLD BALANCE</u>	December 31, 1984	7387.55
<u>INCOME</u>		
	Membership Dues	7992.53
	Conference Fees from Troy Meetings	6279.00
	Sale of Mailing Labels	78.00
	Bernal Prize - Expenses for 1985 Recipient	500.00
	Interest	130.29
	TOTAL INCOME	14979.82
<u>EXPENSES</u>		
	4S Review - Four Issues	5606.94
	Academic Services (Subscriptions Services)	1664.58
	Consortium of Social Sciences Associations	500.00
	Expenses from Ghent Meeting	172.00
	Duplication and Postage	82.90
	Bernal Plaques for 1984 and 1985	87.50
	Telephone	99.26
	Troy Meeting (Banquet, Meals, etc.)	3785.18
	President's Expenses	202.00
	Miscellaneous	5.00
	TOTAL EXPENSES	12205.36
<u>SUMMARY</u>		
	OLD BALANCE 1984	7387.55
	TOTAL INCOME 1985	14979.82
	TOTAL EXPENSES 1985	-12205.36
	NEW BALANCE 1985	10162.01

4S MEMBERSHIP REPORT FOR 1985

Professional Members	422
Student Members	67
Institutional Members	60

TOTAL MEMBERS	549

Society for Social Studies of Science Sponsors AAAS Symposium

Friday, 30 May 1986

Qualitative and Quantitative Data Sources in the
Sociology of Science

Arranged by CHARLES U. LOWE (Special Assistant to the
Director, National Institute of Child Health and
Human Development)

Fri., 30 May / Bellevue-Stratford / Skyline

8:30 a.m. Presiding: HELEN H. GEE (Chief, Program
Evaluation Branch, Office of the Director,
National Institutes of Health)

University Collaboration in Micro-Electronic
Research: Survey Research with Private Funds -
A University Based R&D Center and Government Policy
Makers

Judith G. Larsen and Rolf Wigind (Cognos
Associates, Los Altos, CA and Center for Public
Affairs, Arizona State University, Tempe, AZ)

Methods for Investigating Agricultural Biochemical
Research

Lawrence Busch and Kenneth Pigg (Professors,
Department of Rural Sociology, University
of Kentucky, Lexington, KY)

Hierarchy of Structure in the Sociology of Science:
A Study of Research Supported by NIH

Charles U. Lowe and Edward Nadel (Special
Assistant to the Director, National Institute
of Child Health and Human Development and
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Sociometric Aspects in the Hierarchy of Research
Supported by NIH

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California, Los Angeles, CA)

Quantitative Methods for Depicting the Large Scale
Structure of Science

Henry Small (Director of Research, Institute
for Scientific Information, Philadelphia, PA)

Problems and Moves in Natural Science

Nicholas J. Mullins and Judith L. Steed
(Professor, Department of Sociology, Virginia
Polytechnic Institute, Blacksburg, VA)

The symposium will encompass a number of new research activities directed at studying the sociology of science. The studies represent analyses of data from a variety of quantitative and qualitative sources. Some data sources are traditional, for example scientific publications or interviews. Others will be novel, such as the portfolio of funded research grants awarded by NIH over the past 13 years. The speakers will report on a wide range of basic and applied sciences and some will include several comparisons between different areas of science; others will focus on a single type of data and methodology. Several papers will examine strategies to integrate multiple methods and data sources. It should be possible to explore the following issues: interaction between research strategy, data sources, and results of analyses; how data sources and research strategies effect results; which strategies prove most effective in studying different areas of science; the interaction between research strategy and the kinds of questions studied in the sociology of science.

INTERDISCIPLINARY ANALYSIS AND RESEARCH

• Theory and Practice of Problem-Focused
Research and Development

Daryl E. Chubin
Alan L. Porter
Frederick A. Rossini
Terry Connolly
editors

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Sociology of the Sciences Yearbook Series

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Expository Science: Forms and Functions of Popularization

edited by Terry Shinn, *Maison des
Sciences de l'Homme, France* and Richard
Whitley, *University of Manchester,
England*

SOCIOLOGY OF THE SCIENCES
YEARBOOK 1985

1985, 304 pp.
paper \$24.00 ISBN 90-277-1832-6
cloth \$39.00 ISBN 90-277-1831-8
A Reidel Publication

Exposition refers to the entire gamut of
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formal exchanges that occur within the
sphere of science where the latter is very
broadly defined.

This volume questions the reality of a
radical demarcation between the goals
and instruments of scientific populariza-
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ticulate research results, ideas, and ob-
jectives within purely professional texts.
In the included essays the traditional notion
of scientific popularization has, to vary-
ing degrees, given way to an alternative
concept, scientific exposition.

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Popularization as a Relation Between Sci-
entific Fields and Their Publics. Part I: Ex-
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pacts of Present Day Popularization: Index.

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Raymond Bentley, *University of
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Focusing on East Germany's capacity to
innovate and diffuse technology, this book
sheds light on the technological gap that
has developed between the two Germanies.
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dustrial branches, evaluates the strengths and
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development system, compares the R&D
effort of the two Germanies, and discusses
the government policies that affected tech-
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at the formation of interest groups. He also
compares evidence from the GDR with data
from other countries, including the USSR.
Based on a wealth of GDR documents and
material, this book is the first comprehen-
sive treatment of the subject.

Dr. Bentley wrote this study as a guest
researcher of the Abteilung fuer
Wissenschaftsforschung at the University of
Ulm, Federal Republic of Germany.

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298 pages w/tables, appendix, biblio, indexes
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PHILOSOPHY, TECHNOLOGY AND HUMAN AFFAIRS

An Anthology in the Philosophy of Technology
 Edited by Larry Hickman

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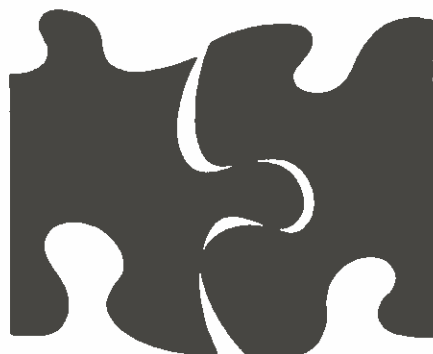


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Organized Technology

Networks and Innovation in Technical Systems

by Wesley Shrum



Science and Society
 A Purdue University Press Series in Science,
 Technology, and Human Values

About the Author

Wesley Shrum graduated with honors in philosophy, sociology, and English at the University of Kansas in 1977. He earned a Ph.D. in sociology from Princeton University in 1982, where he served as project director for two studies of interorganizational networks. Professor Shrum has been assistant professor of sociology at Louisiana State University since 1982. He has contributed articles to such journals as *Social Studies of Science*, *Social Forces*, *Sociological Inquiry*, *Scientometrics*, and *Technology in Society*. He has taught courses on science and society, complex organizations, western civilization, and the occult in literature.

About the Book

From the Manhattan Project to the space shuttle, government-organized technological enterprises have proven to be vital national resources. Their very size and complexity makes unprecedented demands on the organizational abilities of managers. Understanding them is a major challenge for social scientists.

Organized Technology is a first step in meeting this challenge. The book is based on an intensive study of radioactive waste and solar cell research, two large-scale technical systems important to U.S. energy policy. Historical and organizational analyses are combined with results from interviews with a national sample of scientists, engineers, managers, policymakers, and public-interest advocates in a new approach to technology analysis.

Social network methods are used to illuminate the differences between technologies such as nuclear waste disposal, dominated by the federal government, and potentially profitable technologies such as photovoltaics, where the private sector plays a larger role. *Organized Technology* examines the interaction of government agencies, national laboratories, private firms, universities, regulatory agencies, Congress, and public-interest groups in the technology development process.

The implications of this study are of interest to administrators, researchers, policymakers, and others with a desire to understand the organization of contemporary science and technology.

300 pages, paperbound, figures, notes, bibliography, index, \$4.50
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Social Studies of Science

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Volume 15 Number 1
February 1985

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SOCIAL CONTROLS AND THE MEDICAL PROFESSION

Edited by Judith P. Swazey, *The Acadia Institute,
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280 pages 0-89946-186-7 \$27.50

SOCIAL CONTROLS AND THE MEDICAL PROFESSION explores the complex issues concerning the roles and governance of physicians and other professionals in our society. Using an interdisciplinary approach, the book outlines the complex role of the professional in our society, analyzes questions about the nature of professions, and offers recommendations for the conduct and governance of professions. Essays on the nature and effects of self-regulation, including professional autonomy, the selection process for entry into medicine (and other professions), professional training and socialization, and professional competence — how professions define, inculcate, and maintain standards of professional competence and how they define and deal with incompetent professionals. Part I provides comparative perspectives, looking at various aspects of the training, work, and self-regulatory norms and practices of physicians, lawyers, ministers, and scientists. Drawing on themes in Part I, Part II examines the interrelated subjects of competence and impairment in the medical profession and how these are affected by knowledge, skills, performance, normative expectations, and behavior. In addition, Part II explores the central question of what constitutes a profession, its social roles, and how its members govern themselves and their colleagues.

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14. Formal Controls on Impaired Physicians, *Charlotte B. Cloutier*
15. Components of Physician Competence, *Renee C. Fox*

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The challenges facing agriculture today have never been so difficult. Major technological, economic, and political changes have not only transformed the basic operating context of American agriculture, but have raised many new ethical and distributional issues concerning society's often unclear values and goals.

This collection of papers by an interdisciplinary team of scientists, social scientists and philosophers is a major contribution to the creation of a framework broad enough to address these new challenges. Taking into account a wide range of social, health, resource, and environmental factors, the contributors develop new concepts and means of data evaluation in order to deal with agricultural externalities, provide a broad review of ethical, social and political values and goals, and examine the broader setting of U.S. agriculture.

Anyone who cares about the future of U.S. agriculture will appreciate this book. Its focus on various neglected dimensions in research as well as the contributions of both humanists and social scientists make it a uniquely valuable contribution to the literature on the subject.

Nov. 1985 (Paper March 1986) ca. 220 pp.
Tables \$31.50 (cl); \$14 (pb)

Rowman & Allanheld Pub.
Totowa, NJ 07512

The Preservation of Species The Value of Biological Diversity Edited by Bryan G. Norton

Gathered at the Center for Philosophy and Public Policy at the University of Maryland, members of a multidisciplinary working group held extensive discussions on the preservation of biological diversity on earth. From these sessions emerged a book that addresses two related questions: first, why preserve species, and, second, what priorities should govern decisions when there are insufficient funds to save all species? This interdisciplinary volume is the first to occupy the middle ground between abstract discussions of species' value and field managers' guides to saving particular species in particular habitats. It illuminates the context in which managers must make policy decisions and provides insights into factors important to all who value biological diversity.

Contributors to the book are J. Baird Callicott, Robert L. Carlton, Jr., Stephen R. Kellert, Terry L. Leitzell, Thomas E. Lovejoy, Bryan G. Norton, Alan Randall, Donald H. Regan, Lawrence B. Slobodkin, Elliott Sober, and Geerat J. Vermeij.

Bryan G. Norton is Associate Professor of Philosophy at the New College of the University of South Florida in Sarasota.

Written under the auspices of the Center for Philosophy and Public Policy, University of Maryland
About 272 pages, January, 1986. \$29.50

Princeton University Press
41 William Street
Princeton, NJ 08540

ANNOUNCEMENTS**New Journal Announced: Mathesis--Revista de divulgación e información en la Historia y Filosofía de las Matemáticas**

The main goal of the journal is to provide the necessary material to develop, in a satisfactory way, undergraduate and graduate courses in the fields of history and philosophy of mathematics in Latin American countries, and to facilitate the sources--both primary and secondary--required by scholars to carry out original research in these subjects. For this reason, Spanish is the official language of the journal. Most of the sources are essays, articles, books, reviews, and summaries already published; but because of their limited, null or costly distribution and acquisition, they are unavailable for the students and researchers. There are others, published in different languages, that are also difficult to obtain and understand for many of us. But this journal is not only proposing, as it was indirectly mentioned above, to bring us near to these sources of the past, but at the same time, to give us the opportunity to know what is done today by professionals on these subjects. Therefore, the editorial board is also committed to present to their readers some of the most recent works written by these experts.

The journal is open to all points of view, and to all approaches to the history and philosophy of mathematics. It treats all aspects of these fields (including biographies, education, applications, organizations, institutions, and so on). It also discusses the history and philosophy of other disciplines--Physics, Biology, Astronomy, among others--when these essays analyze related aspects to our area of study.

Mathesis is published four times a year. Each volume is made up of four issues. Each volume contains approximately 500 pages.

The journal contains the following sections, which do not necessarily appear in all its issues:

1. **Articles.** - This section includes primary and secondary sources--both on the history and philosophy of mathematics. The section contemplates the periodic publication of complete books, which are already out of print. For example, the first volume attempts to publish the book by Federico Enriques: Para la Historia de la Lógica. The second volume will contain the work of Herman Weyl entitled: La Filosofía de las Matemáticas y de la Ciencia Natural. In future volumes, the works of other great mathematicians (for example, Newton, Euler, and Hilbert, among others), and contemporary historians and philosophers could also be incorporated.

2. **Educational Notes.** - This section publishes brief articles, reports, and news on the diverse programs and courses in our two areas of interest. These "Educational Notes" include: reports on the uses of historical and philosophical approaches in the mathematical education, pedagogic activities

of historians and philosophers, and other facts associated with the role of these disciplines in the teaching of other academic disciplines.

3. Projects and reports of works in progress. - This section contains reports on academic projects in progress, including undergraduate and graduate theses, queries, conjectures, challenges and answers.

4. News and notices. - This section is dedicated to inform the readers of congresses, meetings, promotions, positions, necrological news, and other events of interest to the international community of historians and philosophers.

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6. Reviews. - This section aims to present critical reviews, not only taking into consideration recently published books or articles, but also many works of the past--now considered "classics."

7. Sources. - The task of this section is to inform the reader on the contents of different Latin American libraries in order to locate specific books, journals and documents. In the long run, this section should inform us on the location of materials related to our areas of study in archives, universities and other institutions of all Latin America.

8. Bibliographic Information. - As its name indicates, the function of this section is to offer the reader the bibliographic information that will allow the readers to update their knowledge on the most recent publications.

Send Correspondence to this address:

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Virginia Tech Workshop, 9-10 May 1986
Re-examining the Scientific Revolution

A workshop will be held at Virginia Tech May 9-10, 1986, on "Re-Examining the Scientific Revolution." Papers will be given by Roger Ariew, Peter Barker, Frederic Baumgartner, Ezra Brown, David Lux, and Joseph Pitt, all of Virginia Tech and by Mordecai Feingold (Boston University), Daniel Garber (Institute for Advanced Study, Princeton), and Robert Westman (U.C.L.A.). The workshop is sponsored by the Departments of History and Philosophy, the Center for Programs in the Humanities, and the Center for the Study of Science in Society, Virginia Tech. For more information and reservations, please contact Peter Barker, Center for the Study of Science in Society, Price House, Virginia Polytechnic Institute and State University, Blacksburg, VA 24061. (Telephone: 703-961-7687)

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 Ethics, Evidence, and the Management of Technological Hazards
 Tuesday, 27 May 1986**

The Annual Meeting of the American Association for the Advancement of Science will feature a symposium on "Ethics, Evidence, and the Management of Technological Hazards." The symposium is scheduled for Tuesday, May 27, 1986 in the Commonwealth Room of the Hershey Philadelphia Hotel. It will begin at 2:30 p.m. and end at 5:30 p.m. Symposium speakers will examine scientific, philosophical, and value issues that arise as scientists and engineers participate in risk management processes.

Participants include William Colglazier, Director, Energy, Environment and Resources Center, Univ. of Tenn., Knoxville; Roger Kasperson, Director, Center for Technology, Environment and Development, Clark Univ., Worcester, Mass.; Valerie Mike, Clinical Prof. of Biostatistics, Cornell Univ. Medical College, New York City; Deborah Mayo, Assistant Prof. of Philosophy, Virginia Polytechnic Institute, Blacksburg; Jack Campbell, Deputy Assistant Administrator, Office of Policy, Planning and Evaluation, U.S.E.P.A.; June Fessenden-Raden, Prof. of Biochemistry, Biology, and Society, Cornell Univ., Ithaca, NY; Jerold Mande, Legislative Assistant for Health, Environment and Science Policy to Senator Albert Gore, Jr.; Robert Moolenaar, Project Director, Health and Environmental Sciences, Dow Chemical Co., Midland, MI; Sheldon Samuels, Director, Health, Safety and Environment Dept., AFL-CIO; and Ellen Silbergeld, Senior Scientist, Toxic Chemicals Program, Environmental Defense Fund, Inc.

The symposium is being organized by Deborah Mayo and Rachelle Hollander, Director, Ethics and Values in Science and Technology Program, National Science Foundation. For further information, write the EVIST Program, Room 310D, National Science Foundation, Washington, DC 20550 or telephone EVIST at 202/357-7567.

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