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ARTICLE**Evaluation of the Nançay Decimetric Radiotelescope, Revisited****C. Stewart Gillmor****Department of History****Wesleyan University, Middletown, Connecticut**The evaluation of Nançay

In 1981 John Irvine and Ben R. Martin (=IM) published (1) in La Recherche, a popular French general science journal, an article evaluating several radioastronomy centers. Martin and Irvine (=MI) published a longer but similar article in 1983 (2). These articles contained considerable data from two British radioastronomy centers (Cambridge and Jodrell Bank), and from the Dutch center at Westerbork and the German center near Bonn. These data included publication counts and citations for the decade beginning in the late 1960's. In addition, IM provided rank-order results for nine radioastronomy centers based upon oral interviews with radioastronomers at the four centers mentioned above. Those interviewed, nearly all of whom worked in galactic and extra-galactic specialties, ranked the four centers, along with five other centers located in Australia, France, Puerto Rico and the United States. In the oral rankings, based upon answers to the question of which centers had contributed most "to scientific knowledge made over the ten-year period" 1969-1978 (IM, p. 1414; MI, p. 84), Jodrell Bank ranked seventh; Arecibo, Puerto Rico, eighth; and Nançay, France, ninth and last.

In 1982, several months after the publication of Irvine and Martin's first radioastronomy article, Lucienne Gouguenheim and Ilya Kazès, respectively Director of the Nançay radioastronomy center, and Director of the large decimetric radiotelescope at Nançay, published an article in La Recherche (=GK) (3), evaluating the work of the decimetric radiotelescope group at Nançay. The Nançay center or

observatory, has three research groups: the large decimetric radiotelescope group, the radioheliograph group, and the decametric radioastronomy group. Each group does both observational-experimental, and theoretical studies. In turn, the decimetric radiotelescope group has two teams (équipes) - the astrophysical and the geophysical. GK presented publication and citation data for articles coming from the radiotelescope group alone, thus excluding the other two groups at Nançay, and also excluding theoretical articles not linked with the large radiotelescope. As GK said, they presented bibliometric data "non pas au centre de Nançay globalement, mais aux seules équipes d'astrophysique et de géophysique du radiotélescope décimétrique, en nous limitant de plus à celles des publications de ces équipes qui sont liées à l'instrument." GK's data for numbers of papers published, mean citation rates, and numbers of highly cited papers, they argued, placed the scientific output of the large Nançay decimetric radiotelescope not last among the other four centers previously studied in detail by IM. GK stated that the "partial indicators" did not seem to have the high degree of coherence invoked by IM and they suggested reasons for this, including that Nançay, being a multidisciplinary center was perhaps not comparable, as required by IM's methodology.

In December 1984, again in La Recherche, Susan Plummer, Martin and Irvine (=PMI) (4), responded to the French astronomers. PMI recalled IM's earlier study of the "four large European radiotelescopes." They said the data they had since

evaluated for Nançay showed in fact, that "when the bibliometric values are obtained for Nançay in the same manner as for the other radiotelescopes, they fit perfectly the results of our peer evaluation" (4). "Perfect fits" occur rarely in social science studies. Irvine and Martin have recently completed a number of science policy studies and have stated "The overriding goal of the various studies of basic science specialties that we have undertaken in recent years has been to develop explicit and systematic methods for evaluating the past scientific performance of major research facilities and their associated user-groups" (5) (p. 311). John Ziman has characterized their work: "It is carried out with meticulous thoroughness and critical attention to detail- no one has seriously faulted it in factual terms." (6)

Using publication lists from Nançay provided by Dr. Gouguenheim, PMI excluded from consideration all articles not appearing in peer-reviewed journals or conference proceedings. The remaining publications, they said, they divided into 4 categories: 1) Theoretical publications; 2) Publications presenting observations made on the Nançay radiotelescope in galactic and interstellar astronomy; 3) "Publications presenting observations made on the Nançay radiotelescope in areas other than astronomy: physics of the Sun, the planets or the ionosphere and geophysics"; and 4) Publications containing observations made by Nançay astronomers on installations other than the Nançay radiotelescope.

PMI discovered an arithmetical error in GK's paper which dropped Nançay a notch on the ranking for average citations per article. More importantly, they reduced GK's figure of papers "highly cited"

(i.e., here to mean cited 12 or more times in a year) produced by the radiotelescope groups from 9 to 2, and cut the average number of publications produced by half, from 30 to 15 a year. This is, they said, comparing "the results of the Nançay radiotelescope with those of the four other" European telescopes. In sum, they say "all these indicators" would seem to show that "the Nançay radiotelescope has contributed noticeably less to the progress of radioastronomy during the ten years 1969-1978 than the instruments of the other observatories." But is this so indicated by the data?

Evaluation revisited

I will discuss here some investigations of the Plummer, Martin and Irvine evaluation of the Nançay radiotelescope. Although they talk of "radioastronomy", MI and IM never define it, noting that Cambridge and Jodrell Bank "tend to concentrate on different research areas and problems within this specialty." But "It is with radio astronomy that this study is concerned" (MI, p. 75). They state that citations are counted for "all previous research work by each centre" (MI, p. 81, and Table 8), excluding popularizations, unpublished papers, preprints and reports. Plummer, Martin and Irvine also talk of radioastronomy, though with Nançay they seem to include in the definition galactic and interstellar specialties but exclude other specialties, especially solar system and solar radio astronomy. This doesn't seem to be the equal of the other (IM, MI) studies, where they basically compared all the professional publications (excluding popularizations, etc.) theoretical and otherwise of the other observatories. For example, from Cambridge are included papers coming from all the various antennas, plus theoretical papers, plus those from

Kenneth Budden's group in wave propagation (with which I was fortunate to be associated at Cambridge in 1976). Also as example, MI (Table 12, p. 83) indicate that the Westerbork count of 7 highly cited papers includes one review and one instrumental paper.

Plummer, Martin and Irvine kindly sent me photocopies of the publication lists they said they received from the Nançay center Director in June 1982 and upon which they subsequently indicated their categorizations. The numbers of total papers and total number of highly cited papers from the four other observatories are compared in PMI (Table 1, p. 1611) against only galactic and interstellar papers from the Nançay radiotelescope and also exclude papers from Nançay which PMI classified as "Theory". I noticed that PMI in fact constructed at least 6 categories, not 4 as they stated in PMI (p. 1610), and excluded also what they considered as "Review" and "Instrumental" papers from Nançay. Yet, as we see, they included these for, e.g. Westerbork. In addition, PMI's labeling of some Nançay papers as "Theory" includes examples of what the French call "modellisation," or matching data to theory, or even data presentation. For example, one of the highly cited French papers labeled by PMI as "Theory" and omitted (7) even bears the sub-title "Optical and 21-cm Line Data," is not primarily theory and certainly contains Nançay data. It is the first in a series of three papers: paper II and paper III are subtitled, respectively, "Empirical Properties" (8), and "Theory and Applications" (9). All three were classed "Theory" by Plummer, Martin and Irvine on their lists. Another highly cited paper they list as "Theory" and exclude from counting is not primarily a theory paper, but

is a "Statistical Study of Integral Properties of Galaxies Measured in the 21-cm Line." (10) There are more examples of this, but "Theory" papers were not a category indicated in IM, or MI.

It seems appropriate to point out that IM's oral opinion poll question cited above was to evaluate "contributions to scientific knowledge" and also that PMI's Table I is titled "Indicators of Contributions of 5 Observatories to Radioastronomy Research, 1969-1978" (PMI, p. 1611). Thus they do not compare the institutions on an equal basis, and it is not clear to the reader that they use additional categories to exclude Nançay papers. These categories are not mentioned in their earlier (IM, MI) papers although PMI state (p. 1610) that they are using the "same criteria" and obtaining their values "in the same fashion" as in the earlier studies.

One of the Martin and Irvine's important partial indicators is the number of highly cited papers produced. We will use here their choice "Papers cited 12 or more times in a year" to represent highly cited (PMI). Going back and counting as did GK, and excluding purely theoretical papers, I find 10 papers issuing from the Nançay decimetric radiotelescope and cited 12 or more times in one year, in the period 1969-1978. There are five papers by the astrophysical team and five by the geophysical team, plus at least one "Theory" article by the Observatory Director which does not link to the radiotelescope work. These papers all were authored or co-authored by French radioastronomers and physicists using data gathered by the decimetric radiotelescope at Nançay. PMI (Table 1, p. 1611) give as the number of highly cited papers produced by the other four

observatories: 19 for Cambridge, 3 for Jodrell Bank, 7 for Westerbork, and 2 for Bonn. If these figures are correct, then Nançay certainly is not last; it is arguably second with 10 articles. If one limits the Nançay observatory only to one portion of the output of one of its groups, then the number is 5. But nowhere here does it seem one is comparing "like with like" (IM, p. 1410).

Solar system radioastronomy and geophysics

In their article, Gouguenheim and Kazès point out that the Nançay radiotelescope group has teams both in "astrophysics and geophysics". In her letter accompanying the publication lists she sent to Martin and Irvine in 1982, Dr. Gouguenheim (11) indicated that the big radiotelescope was used 20% of the time for geophysics. PMI treat of this use of the Nançay radiotelescope in their category "physics of the Sun, the planets or the ionosphere and geophysics." In their Table 2, titled "Comparison of Values", one obtains for this solar system astronomy and geophysics research, figures which give a total of 54 papers published in the decade 1969-1978, with a rather small number of citations obtained. I was astonished when I read this in PMI, since I had some years ago become acquainted with the excellent solar system and geophysics work of the Nançay radiotelescope. For a large international sample of geophysics and space physics literature published during the years 1967-1973 in the Journal of Atmospheric and Terrestrial Physics, I had performed a discriminant function analysis comparing expert opinion and citation data (12). Segregating these data by country of author, both as measured by expert evaluation and by citations, the highest national proportion of highly rated articles was gained by

the French. In fact, 7 of 8 of the most highly cited French articles concerned use of the Nançay radiotelescope (13). Given the sample size and the nature of the data, I take this to indicate that the French geophysics and space physics research produced by those using the Nançay decimetric radiotelescope was very good, and in some cases among the very best, but I do not claim it to have been first, or third, etc. in ranking among the other countries in the sample.

Since I am most interested in the solar system and geophysics, I set out to measure the number of French-produced articles in solar system astronomy and geophysics published 1969-1978, utilizing data from the Nançay radiotelescope. In effect, I attempted to replicate as closely as possible the evaluation of this literature made by PMI and presented in their Table 2 titled "Comparison of Values" (PMI, p. 1611). I searched the literature, examined bibliographies and obtained publication lists from the Director of Nançay. I personally examined each article. I discussed my selections with radioastronomers and with geophysicists at my own center. Following PMI's (1984) published plan of dividing the literature into four categories (at that time I didn't know that they had created at least six categories for Nançay), I excluded pure theory papers and those not utilizing Nançay radiotelescope data. A rather extensive search increased my total of articles located over that in the several bibliographies by about 10 to 15%. I located 106 articles in solar system astronomy and geophysics as against the 54 given in PMI, for the period 1969-1978. More important, I found that the number of citations received, as counted in the volumes of the Science Citation Index, was larger

by a factor of from 4 to 6 and that the mean citations per article was from 2 to 4 times larger than that given in PMI.

I should remark again that the idea of categorizing articles uniquely (as "Theory," "Observation," "Instrumentation," etc.) not only is difficult in practice but is misleading for many articles in geophysics and solar system astronomy, and indeed in astronomy in general. Most articles are multi-category. A common mode, to take as example a recent article co-authored by one of my former students, is to have 1) Introduction; 2) Basic principles; 3) Initial Experiments; 4) Discussion. This article has almost equal parts: Discussion of Theory and Method, Observations, and Instrumentation (14). In any case, I wouldn't defend my number of 106 articles as the only correct answer. If I were to exclude several articles which were mostly review, but which contained Nançay data, the mean citation rate of the remaining articles would increase. Finding the last 15% of the articles on my list lowered the overall citation average about 5%. Moving the 4-year integrated citation averages (used by PMI) through the decade produced a variance of plus or minus 15%. The papers published in French language journals, though the sample was very small, received only about half the citations per article as those articles in English language journals. This last result accords approximately with results found for biology and psychology articles for francophone Quebec given by Descarries-Berlinger (15) and in general follows the results found by Garfield (16). I think many readers are familiar with the numerous variables affecting citation evaluation and usage.

My list of 106 articles using data from the Nançay radiotelescope consists of 13 in solar system

astronomy (interplanetary scintillations, planet and comet observations, etc.) and 93 in upper atmosphere geophysics and space physics (incoherent scatter measures of electron density, electron and ion temperatures and other plasma parameters, gravity waves, etc.). These last studies have been performed by the geophysical team of the Nançay radiotelescope from 1965 to the present, mostly by personnel from a laboratory (CRPE) formerly called GRI. This laboratory's work was very favorably treated in a publication and citation study of six French fundamental research laboratories published by Larabi (17, 18). Larabi's articles are cited by Irvine and Martin (1) for the reader "to know more" and in Martin and Irvine (2) as one of "the few authors who have compared groups of scientists... ." Oddly, though their studies centered on the groups at Cambridge and Jodrell Bank, MI do not cite the book by Edge and Mulkey (19), which is the most complete study of groups in radio astronomy and which concentrates on Cambridge and Jodrell Bank.

I took PMI's list of 54 solar system and geophysics articles and read them. Eleven of the 54 indeed concern solar system data obtained by the Nançay radiotelescope. That number compares closely enough to my number of thirteen. One of the remaining 43 articles on their list utilizes the radiotelescope but is misclassified by PMI as solar ("Kinematics of Neutral Hydrogen Clouds in the Solar Vicinity from the Nançay 21-cm Absorption Study" (20) concerns galactic astronomy, not solar system.) The remaining 42 articles of their list of 54 do not in fact use the Nançay radiotelescope but use other radio and optical instruments, both at Nançay and at other observatories. For example, "Study of a Jovian Plasmasphere and the Occurrence of

Jupiter Radiobursts" (21) uses no Nançay radio or radiotelescope data but data from satellites Pioneer 6 and 7, Explorer 33 and 35, and Vela 2 and 3, plus radio observatories in Colorado and Florida.

Concerning the 93 geophysical articles in my list of 106 solar system and geophysical articles, PMI have none at all, although their Table "Comparison of Values" is meant to include all the bibliography. The reasons for this omission are not clear. In a note of 16 April 1985, to be appended to Gillmor (22), PMI maintain that these articles were not included in the bibliography sent to them by the Nançay Director, Dr. Gouguenheim. However, Dr. Gouguenheim (23) indicates that she sent the same lists to Gillmor that she sent to PMI. In any case, the geophysical bibliography was published (24) almost two years before PMI's article was published. PMI did not respond to questions asked in Gouguenheim's letter accompanying the bibliography she sent to them in June 1982 nor, according to Gouguenheim (23), did she receive any of their preliminary analyses of the Nançay bibliography nor a copy of their article, either in preprint or final form. Thus, she was not in a position to advise them whether portions of the bibliography were missing or whether articles were misclassified in their analysis.

Counting, classifying and reading articles

In their article, PMI suggest that "one of the most plausible explanations" for the divergence of results is that Gouguenheim and Kazès (3) included in their publication and citation counts, papers published by Nançay astronomers using not Nançay but other radio observatory data. They say that if PMI's categories of articles are added up in a certain

way and include articles not using the Nançay radiotelescope, then that is one way in which "such an error (sic) could have been committed." PMI are simply wrong in their implication. According to PMI (Table 2, "Comparison of Values"), for the period 1969-1978, 75 "radioastronomy" articles were published by Nançay radioastronomers using only data other than from the Nançay radiotelescope. PMI suggest that if these 75 are added to the other numbers of articles, then that could explain GK's results. From my examination of GK's data, they counted only 9 articles classified as other than Nançay by PMI. Some of these were included because GK considered them directly connected with the radio observations at Nançay, as they stated in their article. For example "A Survey of Galactic OH Clouds in the Direction of Extragalactic Sources" (25) used the Arecibo, Puerto Rico radiotelescope. This was because Kazès et al, took 62 extragalactic sources found in the Nançay H_I absorption survey and then searched for OH using Arecibo at the wavelength of 18 cm. This indeed is one article which met GK's but not PMI's definition. In other cases, PMI incorrectly classified articles as not using Nançay data. For example, "Gas Distribution, Motions and Dynamics for Some Dwarf Irregular Galaxies" (26), contrary to PMI, does include Nançay 21-cm radiotelescope data.

In a recent examination of many of their citation counts, I found generally that GK did not include review articles, nor most articles from conferences later published in book form. GK analysed the papers directly linked to the decimetric radiotelescope group only, which had produced about half the total publications of the Nançay center as a whole. I located some publications which were missed or

not included by GK which I would have included, both in the solar system and geophysics and in the galactic and extragalactic areas. These included one highly cited geophysical paper. But my article and citation counts agree much more with GK than with PMI. In responding to GK, PMI presented neither an analysis of a center, nor of a group, nor of a large instrument.

In a recent communication (27), Martin and Irvine have indicated how necessary it is to read the science articles they analyse in their studies: "Without reading the papers, it is generally impossible to establish which research facility has been used to produce the experimental results..."; "Similarly, reading is essential to distinguish between experimental and theoretical papers in a given field, something which is vital from a policy point of view..."; and "In the case of the CERN study, for example,... Here, there was no option to a manual-scanning approach because of the need to read large numbers of physics papers..." By my count, they must have read more than 12,000 articles in their various studies in the past few years. I have restricted myself in this article only to about 200 articles that I have read, from specialties in which I have some substantive knowledge, but some more general points arise concerning the PMI study of the Nancay radiotelescope:

Some general points

- The partial indicators. These do not seem to converge very well for Nancay. The number of highly cited papers is one of the key indicators for Irvine and Martin. In their radio astronomy study the total number of such articles produced by each observatory integrated over a ten-year period varied only over a range of from 2 to 7, with the

outstanding exception of Cambridge with 19. Of course, instead of 2 for Nancay as measured by PMI, GK judged 9 for the radiotelescope and I judged 10. Beyond the question of errors in bibliography lies the difficulty of deciding intellectually the provenance of such key articles. It is doubtful whether use of such small samples can be defended either theoretically or practically. Moed *et al* (28) (p. 188), note the difficulties which can occur if even one very highly cited article is omitted from the bibliography of a group. And for the articles as a whole, the range of the mean citation rates for the four centers studied originally by IM vary only by about plus or minus 25% (e.g., IM, Table 5, p. 1415), considering the centers individually or as elements within the group of four. I found that, depending upon how I chose my sample for the solar system astronomy and geophysics work from the Nancay radiotelescope, I could find an overall variance of the same amount. The oral opinion poll conducted by IM asked about contributions to scientific knowledge, but it's not clear whether those polled included a large number of radio astronomers familiar with solar and planetary radioastronomy.

Cambridge has done outstanding work in radioastronomy ever since the ionospheric radio group at the Cavendish under J. A. Ratcliffe welcomed and encouraged Martin Ryle just after WWII. Ratcliffe's emphasis upon Fourier synthesis methods was a strong influence upon the group's move to aperture synthesis innovations and Ryle's Nobel Prize in Physics was awarded also for his instrumental and methodological contributions (19, 29).

Similarly, the brilliant work in pulsar physics of Tony Hewish

(co-winner of the Nobel Prize for Physics) and colleagues grew from his original studies of ionospheric radio scintillations, and then inter-planetary scintillations. But from the various partial indicators of PMI it seems difficult to determine which observatory was third, or fourth or last during the decade 1969-1978. Nancay is certainly not a "perfect" fit in last place.

-Comparing "like with like". As Dr. Gouguenheim indicated in her 1982 letter to Martin and Irvine (11), the Nancay and Arecibo observatories differ somewhat from other of the nine radio observatories listed in the IM oral opinion poll. Thus, part of the divergence of indicators may be because PMI are not comparing "like with like". At the Arecibo National Astronomy and Ionosphere Center, 76% of the total available observing time in 1984 was assigned to 142 scientists from 68 U.S. and foreign institutions (30), in the several specialties studied there using the large radiotelescope. As the size of a scientific institution increases, there are certainly less institutions "like" it to compare, thus the definition, measurability and reproducibility of "like" becomes critical. Sometimes IM (1) speak of "centres" (pp. 1407, 1412, 1413, 1414, 1415), sometimes of "groups" (pp. 1407, 1414), sometimes of "observatories" (pp. 1409, 1412) as if these are equivalent and interchangeable terms. Yet, PMI (4) add "radiotelescope" when speaking of what had been evaluated by IM.

-Unique classification of scientific papers. Classification of papers uniquely as being "Observation" or "Theory" or "Instrumental" may possibly work in the literature of some disciplines where the social roles of theoreticians and experimenters are quite different and clearly defined. It does not

work well in astronomy, astrophysics, geophysics and the space sciences. Simply too many papers, journals, and research efforts of individuals and teams are heterogeneous. It is also not easy to determine exactly where data or observations are mentioned in an article. Sometimes a paper will contain only a short mention of a datum previously unreported which then makes possible the reporting of columns of data, perhaps from another observatory. In addition, in these fields, reporting of ensembles of data from several observatories is common. Thus, several of the ten highly cited papers from the Nancay radiotelescope legitimately include major data contributions from other observatories in addition to Nancay. Thus, somehow the "credit" would have to be weighed if one were to apportion cited papers to various observatories. The methods would have to be clear and the results consistent for such efforts to be of real use to science policy.

-Use of bibliographical lists. The use of publication lists supplied by others is insufficient and as argued above can lead to serious mismeasurement. Some of these problems can be avoided if the investigators share their analyses of observatory data and their lists with Observatory Staff; but bibliometric researchers realize that no single bibliometric source is complete. Observatories, faculties and research departments also differ in the thoroughness and consistency of their bibliographical efforts (Moed *et al*, p. 35) (28). Obtaining a reasonably complete bibliography rests in the end the responsibility of the bibliometric investigator.

In this analysis of research done with the decimetric radiotelescope at Nancay, France and examination of

previous evaluations of Nançay I have been particularly concerned with the solar system radioastronomy and geophysical use of the radiotelescope, but I have also discussed some of the galactic and interstellar papers from Nançay, especially those which I found to be "highly cited". It seems both of these types of papers were also studied in Gouguenheim and Kazès' article (3), and also in Martin and Irvine's studies of radioastronomy (1,2,4). It is important to search for methods to be of use in science policy studies, but I believe it equally important for such methods in use to produce consistent and clear results. Just as the successful final engineered product must be better than the sealing-wax-and-string prototype, so must science policy methods in use be more consistent and reproducible in their results than the social science prototypes.

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- (28) Moed, H. F., W. J. M. Burger, J. G. Frankfort and A. F. J. van Raan, On the measurement of research performance: The use of bibliometric indicators, Research Policy Unit, Dienst OZW/PISA, State University of Leiden, the Netherlands. Second edition, 1983, 199 pp.
- (29) Sullivan, W. T., Jr., ed., The Early Years of Radio Astronomy, Cambridge University Press, Cambridge, 1984. See the articles therein by R. N. Bracewell (pp. 167-190) concerning imaging theory; F. G. Smith (pp. 237-248), P. A. G. Scheuer (pp. 249-265) and D. O. Edge (pp. 351-364) concerning Fourier synthesis,

aperture synthesis and J. A. Ratcliffe.

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CRITICAL SYNTHESIS

Studies of Scientific Writing--E Pluribus Unum?

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The rhetorical character of scientific writing currently interests many groups of scholars. These differently motivated groups, working from different conceptual models and using different critical methods, have attended to different aspects of scientific writing. Interdisciplinary awareness remains, moreover, peripheral--a curiosity for the curious and a source of random inspiration. The framing interests and disciplinary contexts of each of the types of work discourages systematic interdisciplinary communication. Nonetheless, this varied work has gathered assorted odd shaped pieces that may, with some interdisciplinary jiggling, be fit together on a new framework. This informal review starts to lay some of the pieces on a shared table, so that the work coming out of different traditions may become more visible to workers in the other traditions. The brief comments offered here are not intended to be adequate summaries or definitive judgments of the increasingly exciting work being done out of the various traditions. Nor should simple characterizations provided here be mistaken to mean that the work has settled into the orderly patterns of disciplinary middle age. The rash judgments offered here are only first approximation descriptions to create some order out of teeming chaos. The highly selected citations only indicate trends and actors and can serve as no more than an entry way into each of the fields discussed. Many noteworthy publications that I am aware of will not be mentioned, as will the even larger number I am not aware of. This essay aspires to openings and does not pretend to closures.

I first turn to the work currently most visible to readers of this journal, the studies of scientific writing coming out of the sociology of science. These studies treat scientific communication as a sub-problem of the social structure and dynamics of scientific communities. Three different approaches to scientific communities have given rise to different concerns with scientific discourse. The first, and founding, approach to the sociology of science is interested in questions of how science operates as a social system to create communal achievements out of the conflicting interests, ideas, and findings of individual scientists. The communication system is seen as crucial to the cumulative, codifying, and integrative aspects of the scientific community. Studies of the general features of the communication system, including the role of citation in the reward system and the emergence of journals and referee system, helped set the context for more detailed studies of textual features emerging from the other approaches which followed.¹

A second sociological approach, following in the wake of Kuhn's pointing to the role of smaller, local research communities in the emergence of science, is concerned with the structure and interactions within scientific specialties. Citation behavior in articles became a clue to the changing morphology of research communities. Deeper looks at the citation behavior led to examinations of the social and symbolic meanings embedded in reference to the work of others and of the process of consensus formation. This work has generally attempted to establish systematic

patterns involving larger numbers of participants in comparative studies.²

The final sociological approach focuses on the individual scientist rather than the specialty or larger social system of science. In order to account for scientific activity without considering science as any different from any of the other communal activities that sociologists study, investigators in this approach have concerned themselves with how individuals and groups advance their interests within the scientific community. Accordingly, analysis has aimed at deconstructing the naive "scientific account" of the meaning of scientific texts, and showing how scientific communications advance personal interests. Features of language indicating persuasion, indexicality, and other forms of social presentation of the self are most readily found in individual texts and case studies, for at this level of analysis we can most readily see the hand-to-hand combat of individual and group advancement.³

This latter approach has strong ties to a broader sociolinguistic movement to analyse community organization and interaction through the study of discourse, sometimes associated with ethnomethodology as well as with an interests/power model of interactions. Oral language has generally received most of this kind of attention, but written language has recently been looked at more. Service professions--such as law, medicine, clinical psychology, and social work-- which establish through interactions a power relationship between professional and client have provided the primary research sites.⁴ One motive for examining scientific discourse within this tradition is that the scientific

community is perceived as one of the major sources of power in contemporary society.

A different tradition of work on scientific writing, not so visible to readers of this journal, comes out of applied language studies. The fields of technical writing, composition, and English for specific purposes, have begun looking into the character, role, and acquisition of written language skills within scientific and technological communities in order to prepare students linguistically for such careers.

Technical writing until recently defined its task in ahistorical, asociological terms: to foster clear, precise, efficient communication in essentially fixed genres. Following this tradition and bolstered by the plain language movement, the Document Design Center in the United States and the Primary Communications Research Unit in Britain promote and disseminate studies into formats and styles most readily understood.⁵ Recently in technical writing, however, some attention has turned to the actual role played by writing in technical organizations, the writing choices made by technical writers, and the processes by which they make these choices. Questionnaire, interview, and observation studies have begun to reveal the social dynamics of technical communication--that writers are strategic social reasoners, that texts are indexical forms of social action, and that writing processes are shot through with collaborative and agonistic social processes.⁶ Recently, as well, the writing across the curriculum movement has made particular disciplinary forms of writing an issue for all teachers of writing. Studies in composition are beginning, largely following the cognitive psychology model, which

has dominated composition research in the last decade. As in the technical writing studies, investigation into the process has begun to reveal its socially imbedded quality.⁷

English for specific purposes, a specialization of English as a second language, in general follows a linguistic model. It treats scientific English as a special register of standard English, incorporating particular vocabularies and grammatic/syntactic features. Catalogues of such features in different disciplines and professions--gathered with an instructional aim--provide suggestive comparative material about the character of communication in different fields, as do a few explicitly comparative studies. Studies of the strategic rhetorical use of particular linguistic features, such as the use of verb tense in reviews of literature to indicate evaluative attitudes, also shed light on the dynamics of communication.⁸ Recent interest in the larger forms of organization and genre and in the kinds of contextual knowledge to be gained from informants, again suggests a growing interest in a historical, socially active understanding of texts.⁹

Literary studies provides a third tradition recently taking notice of scientific writing. Scientific texts, particularly the more overtly evocative ones such as The Origin of Species, are now being examined as special forms of literature, as are scientific popularizations. Studies of the cross-influences of science and literature which previously had been most concerned with the influence of science on imaginative writing have started to look at the influence of literary practice on scientific formulation. This connection between scientific and imaginative literature has seemed

particularly poignant in nineteenth century studies. The movement towards seeing scientific writing as literary creations has been aided by contemporary theories of literature which have devalued referentiality and thus returned scientific texts to the realm of human imaginative constructions, though more subtle formulations that go beyond the primitive opposition of naive positivism and naive relativism have yet to be made forcefully. Such more subtle formulations, I believe, are necessary to defined the special characteristics of scientific texts which maintain referential ambitions whether or not they achieve the epistemological magic of referential certainty.¹⁰

A limited amount of work has attempted to bridge the three approaches presented so far: social studies, applied language, and literary. The purveyors of this work have had literary training and thus are aware of the complexity of textual meaning and the variety of dimensions on which meaning is conveyed. In attempting to address more practical issues of composition, they have seen the necessity of providing a richer description of the kinds of texts the students are being taught to write and of the processes of creating these texts. To help enrich their understanding of text and process, they have turned to social studies of science and have adopted sociological models of scientific community. Their analyses reveal how the complex features of text and textual change embody and realize social dynamics.¹¹

Individuals in both the history and philosophy of science have, as well, been drawn to the reexamination of classic scientific texts, driven by issues and dynamics of their own fields. In the history of science,

the deepening understanding of the variety of intellectual projects engaged in by scientists and the changing intellectual and social contexts which individual scientists have worked in has led to a more rhetorical understanding of certain texts. That is, the texts are no longer seen as a series of propositions to be placed within the framework of emerging scientific knowledge, but are rather seen as integrated wholes, imaginative constructs portraying complex world views (which would lead to more humanistic literary readings), but even more, most recently as particular ways of addressing the world, audience and problems (which might be thought of as situated rhetorical readings)--seeing the symbolic formulations as a result of epistemology, local disputes, and social relations.¹²

The philosophy of science has also given rise to a few attempts to look at scientific texts as complex communicative documents, written documents embodying choices. These studies have been driven by the more general philosophic problem of rationality and rational procedures, which seem increasingly hard to find in the complex world of actual human relations and even harder to tie down in the world of abstractions. In the turn to history and actual practice to see what procedures scientists and other people engaged in rational enterprises engage in, the fine grain of textual structure and textual interactions gain new significance. Rather than trying to reduce scientific formulation to a limited set of abstracted "acceptable procedures" there is a new attempt to discover the full complexity and variety. Toulmin, Kuhn, Fleck, and Popper each discuss the production of knowledge within scientific communities, raising central questions about the patterns, habits, and procedures of

formulation. Perelman most explicitly reopens rhetoric as the centerpiece in the understanding of human rationality.¹³

In philosophy's rediscovery of the importance of rhetoric, contemporary rhetoricians (in American universities exiled to speech departments for the last half-century) have regained vigor and started to engage in philosophic combat on behalf of their position. In only a few instances however, has this resulted in close rhetorical analyses. Most notable has been Campbell's studies of Darwin's rhetorical situation and rhetorical response.¹⁴

Perhaps the largest potential impetus for textual studies of knowledge these days lies in practitioners of various disciplines who, by various means, have come to be interested in the discourse of their own field with the aim of somehow improving work in their field. These individuals range in their stance from the meticulous craftsman who simply wishes to understand his linguistic tools, to the sensible human who wishes to get his colleagues to understand they are only speaking in prose, to the ironic critic who would puncture the prose balloons, to the radical reformer who would create new ones. The social sciences have been more interested in this linguistic self-examination than the natural sciences in the past twenty years, largely motivated by internal debates over what the character of a science of human behavior and interaction should be.

Anthropology has undergone the most thoroughgoing self examination of its rhetoric, particularly concerning the character and authority of ethnography. Geertz most visibly reopened the question of how to write ethnography--how to

capture the lives of others on paper, and the effect of different descriptive techniques on the character of anthropological knowledge. The debate opened by Geertz has widened to consider the complex of social and political relations realized in ethnographies. Questions have proliferated. For what communities are the texts written and for what purpose? Where does authority in ethnographical reporting come from? What is the proper literary role (as both author and character) of both anthropologist and informant? What is the kind of cultural knowledge one can properly (intellectually and morally) convey? What are the power consequences of different forms of texts? What is anthropology that it produces ethnographies?¹⁵

Other social sciences have not produced such a thoroughgoing literary self-examination, but have been the subject of more scattered analyses. In sociology, Brown proposes a more ironic language for sociology; Gusfield understands the literature on drunk driving research in dramatic rhetorical terms--that is, he treats the emergence of a scholarly literature itself as a social-historical phenomenon; Bennett, in a similar vein considers the role of the genre of oral history within criminology. In a series of papers on economics, McCloskey points out, despite an overt disciplinary ideology of scientific objectivity and formal logic, economists actually argue in ways best described by the terms of classical rhetoric. Elsewhere in the social sciences, from political science to theology, the rhetorical issues in the framing of knowledge are being examined.¹⁶

All the studies I have discussed, in all their variety, see scientific writing as complex and difficult, requiring more detailed study.

Scientific writing is no longer seen as a simple, undifferentiated phenomenon that gains its character by a direct correspondence with the facts of nature. The social actions and relations embodied in scientific texts have gained increasing attention, and the knowledge symbolically captured in the text needs to be understood through the intellectual, rhetorical, linguistic, and social procedures by which that knowledge is created and framed. Local context and historical background shape the text and define available choices for symbolic representation. Individual and disciplinary control of the writing seems to require mastery of these social, historical, and symbolic issues as they manifest themselves in the smallest textual features. Scientific writing appears to be a multi-dimensional game, with the game and available moves changing form through time.

Although one can imagine, as I have in the previous paragraph, how the various pieces of work may fit together to suggest a deepened view of scientific writing, none of the separate lines of study can by itself move to such a comprehensive view. As long as scientific writing remains only a secondary problem, subordinated to other disciplinary interests, each field will maintain a narrow focus on specific features and functions of scientific writing. Scientific writing must be seen as a problem area in itself. Furthermore, none of the disciplines has the range of concepts and tools to develop the integrated picture. The study of writing, rhetoric, linguistics, sociology, history, philosophy each has different things to lend to the study, whether in detailed analytical tools, powerful concepts, depth of contextual data, or frameworks for conceiving problems and interpreting data.

The study of scientific writing is

truly an interdisciplinary problem. To claim that the study of any single dimension of scientific communication can provide an adequate account of the entire phenomenon will inevitably lead to distortions on both the practical and theoretical levels--whether by reformists developing new rhetorical prescriptions for their disciplines based on a single-dimensioned model or by philosophers who would reduce claim-making to a single straightforward set of procedures. Sociology--no matter how strong it is, and no matter what essential parts of the game it accounts for--cannot draw the complete picture, nor will its account of sociological dynamics be accurate without taking into account the changing epistemological ambitions of the community or the dynamics of literary traditions and linguistic innovations. Nor can linguists and writing teachers effectively define and teach language features without understanding how these features fit into the social and natural worlds that frame symbolic activity. Thus even the special purposes of each of the interested disciplines would gain from an interdisciplinary perspective.

Awareness of the multiplicity of traditions now finding scientific writing a significant research site constitutes the next stage of developing an interdisciplinary research program into the language of knowledge. If the conjunctions of this review excite greater interdisciplinary reading and deepening research in a range of approaches, occasions for more complete and formal sharing of interests, knowledge, and approaches should lie in the future. The shape of the joint venture will become clearer, when (and if) it unfolds.

Notes

1. Most importantly, R. K. Merton,

The Sociology of Science (Chicago University Press, 1973); also W. Hagstrom, The Scientific Community (Southern Illinois University Press, 1965), and N. Storer, The Social System of Science (Holt, 1966).

2. S. Cozzens, "Comparing the Sciences: Citation Context Analysis of Papers from Neuropharmacology and the Sociology of Science," Social Studies of Science 15 (1985), pp. 127-153; H. Small, "A Co-Citation Model of a Scientific Specialty: A Longitudinal Study of Collagen Research," Social Studies of Science 7 (1977), pp. 139-166; H. Small, "Cited Documents as Concept Symbols," Social Studies of Science 8 (1978), pp. 327-340. S. Cozzens reviews the literature on citation studies in "Taking the Measure of Science: A Review of Citation Theories," International Society for the Sociology of Knowledge Newsletter, March 1981. The fields of Bibliometrics and Library Science have continued this investigation of large scale patterns of publication behavior, such as in D. Kronick, "Authorship and Authority in the Scientific Periodicals of the Seventeenth and Eighteenth Centuries," Library Quarterly 48 (1978), pp. 255-275 and L. Sachs et al., "Wissenschaftliche Aufsätze und Zeitschriften früher und jetzt: mit einigen Daten zur Aufsatzstruktur von Originalien der 'Klinischen Wochenschrift,'" Nachrichten fuer Dokumentation 31 (1980), pp. 107-115.
3. For example, G. N. Gilbert and M. Mulkay, Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse (Cambridge University Press, 1984); B. Latour and S. Woolgar, Laboratory Life: The Social

- Construction of Scientific Facts (Sage, 1979); K. Knorr-Cetina, The Manufacture of Knowledge (Pergamon Press, 1981). A more complete review of social studies of science relevant to scientific writing appears in C. Bazerman, "Scientific Writing as a Social Act," in New Essays in Technical and Scientific Communication, ed. Anderson, Brockmann, and Miller (Baywood, 1983).
4. For example, H. Garfinkel, Studies in Ethnomethodology (Prentice Hall, 1967); W. Labov and D. Fanshel Therapeutic Discourse: Psychotherapy as Conversation (Academic Press, 1977); R. Di Pietro, ed., Linguistics and the Professions (Ablex, 1982).
 5. Simply Stated, the newsletter of the Document Design Center (1055 Thomas Jefferson St. NW, Washington D.C. 20007) and the Journal of Research Communication Studies provide advice and studies aimed at writers and editors.
 6. For examples see the collections, New Essays in Technical and Scientific Communication, ed. Anderson, Brockmann and Miller (Baywood, 1983) and Writing in Nonacademic Settings, ed. Odell and Goswami (Guilford, forthcoming).
 7. The most complete treatments of writing in the disciplines appear in the textbooks, Bazerman The Informed Writer, second edition (Houghton Mifflin, 1985); Maimon et. al. Writing in the Arts and Sciences, second edition (Winthrop, 1985).
 8. See the collections, Common Ground: Shared Interests in ESP and Communication Studies, ed. Williams, Swales and Kirkman (Pergamon, 1984) and English for Academic and Technical Purposes, ed. Selinker, Tarone and Hanzeli (Newbury House, 1981). A recent review article is B. Coffey, "ESP--English for Specific Purposes," Language Teaching Abstracts 17 (1984), pp. 2-16. A previous generation of syntactic studies are exemplified by R. D. Huddleston, The Sentence in Written English (Cambridge, 1971); M. Gopnik, Linguistic Structures in Scientific Texts (Mouton, 1972); and K. C. Lee, Syntax of Scientific English (Singapore University Press, 1978).
 9. L. Selinker, "On the Use of Informants in Discourse Analysis," IRAL 27 (1979), pp. 189-215; J. Swales, Aspects of Article Introductions (Aston University ESP Research Report, 1981). Relevant conceptually, although not immediately on scientific texts, is V. K. Bhatia An Applied Discourse Analysis of English Legislative Writing (Aston University ESP Research Report, 1983).
 10. For example, Victorian Science and Victorian Values, ed. Paradis and Postlethwaite (New York Academy of Sciences, 1981); G. Beer, Darwin's Plots: Evolutionary Narrative in Darwin, George Eliot, and Nineteenth Century Fiction (Routledge and Kegan Paul, 1983); W. Anderson, Between the Library and the Laboratory: The Language of Chemistry in Eighteenth Century France (Johns Hopkins University Press, 1985); S. E. Hyman, The Tangled Bank (Atheneum, 1962). A seminal work in an earlier line of interest is R. F. Jones, "Science and English Prose Style, 1650-75" PMLA 45 (1935), pp. 977-1009.
 11. C. Bazerman, "Modern Evolution of the Experimental Report in Physics: Spectroscopic Articles in 'Physical Review,' 1893-1980," Social Studies of

- Science 14 (1984), pp. 163-196; C. Bazerman, "Physicists Reading Physics: Schema-Laden Purposes and Purpose-Laden Schema," Written Communication 2 (1985), pp. 3-23; C. Bazerman, "The Writing of Scientific Non-Fiction: Contexts, Choices, Constraints," Pre/Text 5 (1984) in press; G. Myers, "The Social Construction of Two Biologists' Proposals," Written Communication, forthcoming; G. Myers, "Texts as Knowledge Claims: The Social Construction of Two Biology Articles," Social Studies of Science, forthcoming.
12. For example, P. Dear, "The Constitution of Authority in the Early Royal Society," Isis, forthcoming; B. Eastwood, "Descartes on Refraction: Scientific versus Rhetorical Method," Isis 75 (1984), pp. 481-502; S. Shapin, "Pump and Circumstance: Robert Boyle's Literary Technology," Social Studies of Science 14 (1984), pp. 4881-520.
 13. J. Agassi, Faraday as a Natural Philosopher (University of Chicago Press, 1971), chapter 5; M.A. Finocchiaro, Galileo and the Art of Reasoning: Rhetorical Foundations of Logic and Scientific Method Boston Studies in the Philosophy of Science 61 (Reidel, 1980); L. Fleck, Genesis and Development of a Scientific Fact, Bradley and Trenn, trans. (University of Chicago Press, 1979); T. Kuhn, The Structure of Scientific Revolutions, second edition (University of Chicago Press, 1970); K. Popper Objective Knowledge: An Evolutionary Approach (Oxford University Press, 1979); C. Perelman and L. Olbrechts-Tyteca, The New Rhetoric: A Treatise on Argumentation (University of Notre Dame Press, 1969), S. Toulmin, Human Understanding: The Collective Use and Evolution of Concepts (Princeton University Press, 1972).
 14. For example, J. A. Campbell, "The Polemical Mr. Darwin," The Quarterly Journal of Speech 61 (1975), pp. 375-390.
 15. For example, J. Clifford, "On Ethnographic Authority" Representations 1 (1982), pp. 118-146; J. Fabian, Time and the Other: How Anthropology Makes its Object (Columbia University Press, 1983); C. Geertz, The Interpretation of Cultures (Basic Books, 1973); C. Geertz, Local Knowledge (Basic Books, 1983); G. Marcus and D. Cushman, "Ethnographies as Texts," Annual Review of Anthropology 11 (1982), pp. 25-69.
 16. R. Brown, A Poetic for Sociology (Cambridge University Press, 1977); R. Gusfield, "The Literary Rhetoric of Science: Comedy and Pathos in Drinking Driver Research," American Sociological Review 41 (1976), pp. 16-34; J. Bennett Oral History and Delinquency (Chicago University Press, 1981); D. McCloskey, "The Rhetoric of Economics," Journal of Economic Literature 21 (1983), pp. 481-517; D. McCloskey, "The Literary Character of Economics," Daedalus 113:3 (Summer, 1984), pp. 97-120; a forthcoming issue of Daedalus is devoted to a colloquium on the rhetoric of the human sciences.

SOCIETY FOR SOCIAL STUDIES OF SCIENCE: 10TH ANNUAL MEETING

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Please clip and return the following form, with a check in the appropriate amount payable to Society for Social Studies of Science, to Thomas F. Gieryn, Indiana University, Ballantine Hall, Room 744, Bloomington, IN 47405. Motel and meal reservations must be received by October 4. Room rates are \$34.00 for singles and \$40.00 for doubles. Of the three tours scheduled for Friday afternoon (see the preliminary program), we will need to arrange transportation for the tour of Troy's historic industrial and architectural sites. Please indicate if you plan to participate in this tour so that we may make appropriate arrangements.

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4S MEETINGS

TRAVEL ARRANGEMENTS TO THE 1985 MEETING

The 1985 meetings, hosted by the RPI Department of Science and Technology Studies, will be held on campus and at the Troy Holiday Inn, 6th Avenue & Fulton, (518) 274-3210. Troy is on the Hudson River in New York's Capital District, 150 miles north of New York City, 180 miles west of Boston, and 240 miles south of Montreal.

Airline connections via Albany Airport are relatively frequent, though not cheap. There are over 30 commuter flights a day to New York, and about 15 to Boston. There is direct service on major carriers from Atlanta, Chicago, Philadelphia, Pittsburgh, and Washington. Service to all these destinations on Sunday afternoon and evening, when most attendees will be departing, is good. For those flying to the New York metropolitan area, there is direct limousine service to Albany from JFK, LaGuardia, and Newark airports (\$27 one way). Advance reservations are required, and can be made by calling Hudson Valley Airporter at (518) 465-1523.

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**SOCIETY FOR SOCIAL STUDIES OF SCIENCE
10TH ANNUAL MEETING
RENSSELAER POLYTECHNIC INSTITUTE, TROY, NY
OCTOBER 24-27, 1985**

REVISED PROGRAM

THURSDAY, OCTOBER 24

Thursday, 5pm Publication Committee

Thursday, 7pm Council Meeting

FRIDAY, OCTOBER 25

Friday, 7am 1986 Program Committee

Friday, 9-11:45am

The cultural coding of scientific activity

Chair, D. E. Chubin, Technology and Science Policy, Georgia Institute of Technology

K. Amann, and K. Knorr-Cetina, University of Bielefeld, "The body of the scientist"

K. Knorr-Cetina and K. Amann, University of Bielefeld, "How scientists think"

R. Giere, Indiana University, "The scientist as satisficer"

Ullica Segerstrale, Smith College, "The sociobiology debate revisited"

E. Manier, UND, "The cultural coding of neurobiology and cognition"

Econometric, bibliometric, and sociometric analyses of science and technology

Chair: Wesley Shrum, Louisiana State University

Loet Leydesdorff, Amsterdam, "Journal-journal citations as indicators of change"

Arthur Diamond, Ohio State, "Career consequences for a scientist of a mistaken research project"

Sharon Levin & Paula E. Stephan, Georgia State, "Age, productivity and academic position: the effects of using selected samples"

Larissa A Lomnitz, M. W. Rees, Leon Cameo, University of Mexico

"Publication and referencing patterns in a Mexican research institute"

Michelle Lamont, Stanford University and University of Texas, Austin, "Changes in the human sciences and macrostructural changes in Quebec"

John A. Stewart, Univ. of Hartford, "Drifting continents and colliding interests: A quantitative application of the interests perspective"

Lowell Hargens, Univ. of Illinois, "Disciplinary differences in journal acceptance rates"

The social production of science

Chair, John Wilkes, Worcester Polytechnic Institute

John M. Wilkes, WPI, David Vachon, St. Joseph's College, and Brian Starr, WPI,

"Cognitive orientation, background and sex as determinants of career choice in the sciences"

Nina Toren, Hebrew University of Jerusalem, "Numbers, social characteristics and inequality: women in academia"

Robert McAulay, Vassar College, "Romanticism, powerlessness and deviant paradigms"

Gary Downey, VPI Science Studies, "Cultural identity and the claims of the Union of Concerned Scientists"

Gary Bowden, McMaster University, "Organizational culture, professional culture and the production of scientific knowledge: USGS crude oil estimates"

Friday, 1:30-5pm

Tour of nineteenth century industrial sites of Troy, NY

Hands-on tour: RPI microcomputer lab: capabilities, networking, applications

Tour/discussion: RPI Center for Industrial Innovation: Industry-University Relations

Friday, 1:30-3:25pm

Advisory systems and the process of policy making in controversial research areas

Chair, Pnina G. Abir-Am, Harvard

J. Genuth, MIT, "Science advice to the President and the debate over NSF"

A. Pickering, University of Illinois, "Constructing consensus: world views, institutional structures and policy making in recent high energy physics"

A. Rip, University of Amsterdam, "The mutual dependence of risk research and political context"

Paul Hoch, "Advisors of the Council for Academic assistance and policy toward refugee scientists in the 1930s"

P. G. Abir-Am, Harvard, "Theoretical controversy and decision making in trans-disciplinary areas: phasing out physico-chemical morphology in the 1930s"

Everett Mendelsohn, Harvard, Commentator

Friday, 3:30-5:30pm

Naturalistic explanations of scientific knowledge

Chair, Ronald N. Giere, Indiana University

Steve Fuller, University of Colorado, Boulder, "The demarcation of science: a problem whose demise has been exaggerated"

C. Kwa and A. Hiddinga, University of Amsterdam, "Metaphors in science"

R. P. Hagendijk and J. Cramer, University of Amsterdam, "Cognitive structure: intellectual traditions and the flexibility of scientists"

R. W. Hadden, McMaster University, "Mathematics, relativism, and David Bloor"

Kay Oehler and E. R. Fuhrman, VPI, "Discourse analysis: problems and prospects"

Friday, 7pm 4S Banquet Bernal Award Bernal Address

SATURDAY, OCTOBER 26

Saturday, 9am

China's policy for scientific research and industrialization

Chair, Dennis Simon, MIT

Anthropological studies of scientific and technological communities

Chair, Sharon Traweek, MIT

Scott Cook, Harvard, "Flutes as artifacts: the culture of flute craftsmanship"

Suzanne P. de Atley, MIT, "The context of production: traditional pottery manufacture in Karatsu, Japan"

Frank Dubinskas, MIT, "Mutable communications technology and organizational change"

Jean Jackson, MIT, "Anthropological models of fieldwork in traditional and laboratory settings"

Brigitte Jordan, Michigan State University, "Artifacts and the social allocation of knowledge in obstetric settings"

Lucy Suchman, Xerox Palo Alto Research Center, "Action and interaction in cognitive science"

Saturday, 1:30-3:25pm

The relevance of history and philosophy of science for science studies

Chair, Edward Manier, University of Notre Dame

Dudley Shapere, Wake Forest University, "External and internal factors in the development of science"

Discussants: Thomas Carroll, RPI/MIT and Steven Turner, Notre Dame

Sociology of technology

Chair, Ron Westrum, Eastern Michigan University

M. Mackenzie, NYU, P. Keating and A. Cambrosio, University of Montreal

"Science/Industry: monoclonal antibodies"

W. B. Lacy and L. Busch, Univ. Kentucky "Biotechnology and agricultural science"

Deborah Mayo, VPI "Policy values and epistemic values"

Boelie Elzen, Twente University of Technology, Netherlands, "Development of the ultracentrifuge"

Thomas J. Misa, University of Pennsylvania, "The context of technological knowledge: 'iron' and 'steel', 1873-1880"

Gil Peach, Pacific Power & Light, "Argumentation in applied research"

Policy priorities and research goals

Co-Chair: S. Zeldenrust, University of Amsterdam & Wesley Shrum, Louisiana St. U.

H. Dits, University of Amsterdam, "Political direction of scientific research: strategic retreat on objectives"

S. Zeldenrust, University of Amsterdam, "Strategic action in the laboratory: inter-organizational resources and constraints in industrial and university research"

Arie Rip and A. J. Nederhof, University of Amsterdam, "Implementation of Science Policy Priorities: The case of biotechnology in the Netherlands"

P. Vergragt, and J. Bijlsma, Univ of Groningen, "The social construction of industrial innovations"

Saturday, 3:30pm**Three mile island**

Chair, John Wilkes, Worcester Polytechnic Institute

R. C. Mitchell, Resources for the Future, "Public support for nuclear power in the post-TMI era"

A. Mazur, Syracuse University, "The media and Three Mile Island"

E. Walsh and Sherry Cable, Penn State University, "Differential routes to activism in the TMI struggle"

C. P. Wolf, Polytechnic Institute of New York, "Lessons learned: institutional response and conceptual advances growing out of the TMI incident"

J. Reed, Oak Ridge National Laboratory, and J. Wilkes, WPI, "Public knowledge and interpretation of nuclear power before and after TMI"

M. A. Dew and E. Bromet, Psychiatry, U. Pittsburgh, "A matter of perspective: community residents' and nuclear plant workers' long term reaction to the TMI accident"

Bibliometric evaluation of research

Chair, Nicholas Mullins, Virginia Polytechnic Institute and State University

A.F. J. van Raan and H. F. Moed, Leiden, "Use of bibliometric data as tools for university research policy"

Fran Narin, Computer Horizons, "Differences between centers and individual investigators in dental research"

S. Gillmor, Wesleyan University, "The radiotelescope at Nancy"

B. Martin & J. Irvine, S.P.R.U., Sussex, "Radio Astronomy Revisited"

Susan Cozzens (NSF) discussant

Science in developing countries

Antonio Botelho, MIT, "Struggling to survive: the Brazilian Association for the Advancement of Science and the authoritarian regime (1964-80)"

Calvin Morrill, Harvard, "Transfer of scientific knowledge to developing countries: a view from a Brazilian field laboratory"

Yakov M. Rabkin, University of Montreal, "Emergence of scientific cultures in developing societies"

SUNDAY, OCTOBER 27

Sunday, 7am. Breakfast A. H. Teich, AAAS, "Education in Science, Engineering, and Public Policy" (\$5 payable at registration)

Sunday, 9am

The role of pictorial representations in science

Co-Chair, Samuel Edgerton, Williams College and Roger Krohn, McGill University

Samuel Edgerton, Williams College, "Imaging astronomical phenomena in outer space: modern science's debt to modern abstract art"

R. Krohn, McGill University, "Visual displays as social cognition: evidence and argument in limnology"

David Park, Williams College, "Now you see it"

Bruno Latour, Ecoles des Mines, Paris "[to be announced]"

Frontiers in bibliometrics: today and tomorrow

Chair, Susan Cozzens, National Science Foundation

H. Roberts Coward and J. Jeffrey Franklin, Center for Research Planning, "The interaction of science and technology: exploring the relationship of a bibliometric model to patents"

Carole Ganz Brown (NSF) and Joseph Sneed (Colorado School of Mines), "Setting scientific priorities: the intellectual structure of a scientific discipline"

Charles U. Lowe, NIH, E. Nadel and E. C. Rogers, "Tracking a research front by co-term clustering in NIH grants, 1974-1984"

Susan Cozzens, NSF, "Accomplishments and challenges for bibliometrics in policy research"

Michel Callon and Jean P. Courtial, Ecoles des Mines, Paris, "The translation model and its exploration through co-word analysis: using graphs for negotiating research policies"

Henry Small, Institute for Scientific Information, "The future of bibliometrics"

Sunday, 1:30pm

Science Policy Studies: Panel

Chair, Nicholas Mullins, Virginia Polytechnic Institute and State University

C. Kruytbosch, NSF,

J. Holmfeld, Congressional Committee on Science and Technology,

P. Healy ESRC, London

Social construction of science and technology

Chair, Ruth Cowan, SUNY at Stony Brook

Susan Douglas, Hampshire College, "The role of amateurs in the formation of radio networks"

Mark Rose, Michigan Technological University, "On the Formation of electric networks in Denver and Kansas City"

Stuart Leslie, Johns Hopkins University, "Steeple building at Stanford: the boundary problems of post-war engineering and physics"

Construction of research fields

Chair, Joan Fujimura, Tremont Institute, San Francisco

Martin H. Krieger, University of Southern California, "The physicist's toolkit"

Joan Fujimura, Tremont Institute, "Construction of 'do-able' cancer research"

Iskender Gokalp, CRNS, Paris, "A study in the constitution of a frontier scientific field: the case of turbulent combustion"

Lois Peters, New York University, and Henry Eskowitz, SUNY

"Computer Science: Convergence and specialization in the growth of scientific disciplines"

Program committee, Tenth Annual Meeting, 1985

Edward Manier, History and Philosophy of Science, University of Notre Dame, IN 46556, Chair

Chris Caswill, SSRC, 1 Temple Avenue, London, EC4 Y0BD

Ruth Cowan, History, SUNY-Stonybrook, NY 11794

Susan Cozzens, Policy Research and Analysis, National Science Foundation, Washington 20550

Karin Knorr-Cetina, Sociology, University of Bielefeld, West Germany PO 8640 4800 B11d 1

Wesley Shrum, Sociology, Louisiana State University, Baton Rouge, L 70803

John Wikes, Social Psychology, Worcester Polytechnic Institute, Worcester Ma 01609

10TH ANNUAL MEETING

SOCIETY FOR SOCIAL STUDIES OF SCIENCE

ADAR 1971, 1-2-71

Advisory Systems and the Impact of Policy Processes on New Disciplinary
Outcomes: The Rise and Decline of Some Rockefeller Foundation's
Projects

The impact of research policy on scientific progress in general and on new disciplines in particular has been often viewed as a relationship between macro-indicators of input (such as the size and length of the grants) and output (such as productivity and prestige of the grantees). This relationship emerged as a direct one if not entirely linear for scholars who attributed the direction and very emergence of certain disciplines to the input of policy action. Limitations of this approach, most notably its social-theoretical deterministic bias and historical apologetic orientation, in the case of the well-documented intrusions of the Rockefeller Foundation, were discussed at a round table at the joint meeting of 4S and EASST in Gent in 1984.*

This paper will present a constructive dimension of the criticism formerly addressed at prevailing studies of policy impact on new disciplinary structures, by focusing on the often omitted processes of interaction among scientists and policy makers. In particular, the paper explores the role played by the deployment of advisory systems in decision making in controversial research areas. The goal being an understanding of the fine structure of consensus management in an enterprise like science where consensus functions as a major support system for key beliefs of the scientific universe of meaning, most notably objectivity, rationality and truth.

NOTE: The abstracts are listed alphabetically according to the last name of the first author. Most abstracts for the meetings are printed herein; but because of the mail or other contingencies, a few abstracts were not included. For affiliation(s) of the author(s), please consult the program.

*Some of these arguments appeared as 'Beyond deterministic sociology and apologetic history: reassessing the impact of policy on new disciplines', *Social Studies of Science*, 14 (1984), 252-63.

BOTELHO, Antonio Jose J.

Struggling to Survive: The Brazilian Association for the Advancement of
Science and the Authoritarian Regime (1964-1980)

The rise of an authoritarian regime in Brazil from 1964 onwards besides provoking the persecution of eminent scientists, with the consequent disruption of entire lines of research or even institutions, forced an important segment of the Brazilian scientific community, represented in the Brazilian Association for the Advancement of Science - SBPC) into a changing political relationship with the increasingly authoritarian regime. First, the Society took the defense of science its practitioners and later on opened up and preserved a much needed democratic space.

Through an analysis of the activities of the Society, particularly of its annual meetings we seek to show how the political evolution of the

country affected the development of Brazilian science and particularly how the Society's political attitudes evolved throughout the period. We identify two parts to this process. From 1964 until the late sixties the SBPC, still relatively small and unknown to the larger Brazilian public was led to take a critical stand in defense of the abuses of power against its members. In a second part, which runs from the late sixties till the early 1980s, the authoritarian regime sought to promote the scientific and technological development of the country, in the midst of a growing authoritarianism which completely choked the traditional spaces of political debate. At this point the SBPC's annual meeting gradually became an island of democracy where not only scientific or scientific-related issues, but also social and political ones were openly discussed. This inevitably led to a confrontation with the regime in 1976. The Society came out of the episode considerably strengthened and with its public prestige clearly enhanced. However, with the subsequent political liberalization of the SBPC was led to adopt a new attitude which would allow for its survival as a representative and important scientific institution.

BOWDEN, Gary

Organizational Culture, Professional Culture and the Production of Scientific Knowledge: United States Geological Survey Estimates of U.S. Crude Oil Resources, 1962-1965

In 1962 and again in 1965 the United States Geological Survey produced estimates of the amount of oil remaining to be recovered in the United States. Although these estimates differed little in magnitude, they differed significantly in the manner in which they were produced. The first estimate, based upon the assumption that the "richness factor" of future discoveries would equal that of past discoveries, was roundly criticized by estimators outside the Geological Survey. Acknowledging the validity of this critique, the 1965 estimate assumed that the richness of future discoveries would be approximately one-half that of past discoveries. Thus, if all other things had remained equal, the 1965 estimate of the magnitude of future discoveries would have been one-half that of the 1962 estimate. Interestingly, in spite of the fact that the 1962 estimate was significantly larger than any other estimate that had been made, it was not criticized on those grounds and other adjustments were made to the 1965 estimate such that its magnitude closely approximated that of the 1962 estimate.

This raises two questions. First, why was the critique of the first estimate asymmetrical, i.e. why did it center upon the technical assumption embedded within the estimate without calling into question the magnitude of the estimate? Second, why did the magnitude of the 1965 estimate approximate that of the 1962 estimate? Stated another way, why did the criticism take the specific form that it did and why did U.S.G.S. estimators respond to the criticism in the way that they did? The concepts of occupational and organizational culture are deployed in order to answer these questions. Specifically, it is argued that estimators

employed within industry objected to the assumption within the 1962 estimate because it contradicted their occupational identity. Industry employs geophysicists and spends massive amounts of money on gathering geophysical data because this information improves their chances of finding oil. To suggest, as the 1962 U.S.G.S. estimate did, that the richness of future discoveries will equal that of past discoveries implies that geophysicists and geophysical information do not influence the search for oil. In other words, the 1962 estimate treated the search for oil as a random process while industry geophysicists perceived their occupation as important precisely because it made the search non-random. Thus, the criticism of the 1962 estimate focused upon the assumption embedded within the estimation technique and not the results of that technique because it was the assumption and not the results that were seen as objectionable. This does not, however, explain why the USGS estimate was significantly higher than those produced by industry and why the 1962 estimate was repaired in a manner that avoided the criticism while retaining the result. Knowledge of the historical orientation of the USGS and the resulting organizational context, however, resolves this issue. The USGS had traditionally focused on hard rock geology rather than soft rock geology. Thus, the large estimates they produced were an outgrowth of the relative unimportance placed upon oil and gas by the organization; a large estimate suggested the existence supplies well into the future and, hence, precluded the need for any significant alteration in the organization. Significantly, it was not until the Arab Oil Embargo of 1973 and the subsequent expansion of the Oil and Gas Branch of the USGS by the Carter administration that the Geological Survey estimates fell to a level approximately the magnitude of industry estimates. In short, an organizational culture that viewed oil and gas as relatively unimportant resources produced estimates affirming that view. The estimates were significantly larger than those produced by industry because the future availability of oil was not perceived as problematic.

The theoretical implications of the above case study are examined. In specific, the similarities and differences between the above explanation and the "interest model" of explanation advocated by Barnes, Shapin, and McKenzie are examined. It will be argued that the focus upon corporate bodies, like the American Association of Petroleum Geologists or the USGS, provide a firmer basis for understanding the similarities of viewpoint among researchers than does the imputation of social class interests and avoids many of the objections that have recently plagued the usage of the concept of "interests."

CALLON, Michel
COURTIAL, J-P.

The Translation Model and its Exploration Through Co-word Analysis: Using Graphs for Negotiating Research Policies

The increasing use of science policy programmes to promote and orient research activities underlines the need for analytical tools which can

Contribute to their elaboration and evaluation.

There is a wide variety of science policy objectives and a corresponding variety of criteria that can be used to measure the impact of initiatives on further research. Two types of policy initiative are, however, of particular importance. The first aims to promote, develop and improve existing activities without necessarily changing their content. The second seeks explicitly to modify the content of a field by introducing new themes of study, creating links between previously unrelated sectors of activity or by trying to promote the emergency of a new problem area.

The paper presented will deal with the second case in which decision-makers are confronted by a series of strategic questions: What are the research problems being studied at present? What themes of common interest can be defined which will lead to closer ties between laboratories working in different areas? How can one be certain that new fields are not being completely ignored by the national research community? Usually these questions are mainly answered by scientists only. In order to contribute to the discussion which this type of question requires, decision-makers, whatever their level of responsibility, need analytical tools which make it possible to consider the content of scientific and technical research.

Based on recent post-Kuhnian social studies of science, the translation model enables scientific choices to be related with certain categories of political decision.

The co-word analysis of scientific or technical literature is a tool designed to identify the networks of translation. Its results give outsiders the possibility of negotiating research policy orientation. Empirical evidence will illustrate this point.

COOK, Scott

Flutes As Artifacts: The Culture of Flute Craftsmanship

For over half a century Verne Q. Powell Flutes, Inc. has made flutes of the finest quality and of an unmistakably Powell "style." This constancy of style and quality has been maintained through generations of craftsmen and is reflected in every flute even though each is the product of several craftsmen working within design standards that have never been made explicit.

This paper, based on participant observation, presents and interpretation of the Powell workshop as a "cultural matrix" comprising norms of know-now, values, myths, and aesthetics. I argue that these norms, as employed both in judgments of workmanship and in management decisions, have been responsible for the unvarying style and quality of the Powell flute.

COWARD, H. Roberts
FRANKLIN, J. Jeffrey

The Interaction of Science and Technology: Exploring the Relationship of a Bibliometric Model to Patents

Patterns of interaction between science and technology were explored, using a bibliometric model of 1982 epitaxy and micro lithography research activity to represent science, and data on 1982 patents in five related U.S. patent classes to represent technology. Intersects between the model and patent data were identified using model authors and patent inventors, author's institutions and patent assignees, and patent references to the scientific literature.

A substantial intersect between semiconductor research and semiconductor patents was identified, primarily on the basis of qualitatively evaluated author-inventor matches. The intersect was not evenly distributed across the model. 150 of the 196 specialties in the model showed one or more matches with the patent data, ranging from one match for an entire specialty to one match for every two specialty papers.

High match ratios appeared to be associated with specialties with an applied intellectual focus. An independent classification of model specialties as basic or applied validated this indicator of applied science statistically.

Interaction patterns between more applied specialties and basic research areas were identified, suggesting the possibility of defining technology-driving basic science. The implications of such a capability for assigning priorities in research support was explored.

Patterns of corporate participation in science and technology were examined and found to be highly variable. It was concluded that corporate activity patterns in both science and technology can provide valuable profiles, but that the reliability and comparability of such profiles is highly sensitive to corporate publication and patenting policies.

Both the model and the patent data showed a strong challenge to U.S. preeminence by the Japanese.

An exploration of the coherence of patent to patent citations among patents sharing a common science base suggested that patent citations show far less systematic patterns than the references in the scientific literature, at least in this field.

COZZENS, Susan E.

Accomplishments and Challenges for Bibliometrics in Policy Research

The purpose of this paper is selectively to review and evaluate the last

ten years of policy-oriented bibliometric research. The evaluation is based on the following question: What have we learned from this research about how science and policy interact? The review is based on the assumption that unless bibliometric research contributes to a fundamental understanding of the relationships between science and policy, it will not have fulfilled its promise. Three thrusts in policy-relevant bibliometric research will be discussed: bibliometrics for evaluation, bibliometrics for planning, and bibliometric studies of funding and knowledge growth. The review concludes that the maturity of bibliometric methods and the sophistication with which they are now being applied indicate that the next ten years will be a golden age for policy-oriented bibliometrics.

DE ATLEY, Suzanne P.

The Context of Production: Traditional Pottery Manufacture in Karatsu, Japan

Archaeological studies of craft production focus on the material products, especially ceramics. In such investigations it has become evident that archaeologists must re-evaluate their assumptions about the extent and significance of standardization in the form and composition of ceramic artifacts manufactured under different forms of specialist ceramic production. The ethnoarchaeological research discussed here employs field observation of traditional pottery manufacture at kilns in Karatsu, on the island of Kyushu, Japan, and laboratory analysis of raw materials and products from those kilns. These methods illuminate the sources of variation and the permissible parameters for it in ceramics manufactured for the Japanese tea ceremony.

DEW, Mary Amanda
BROHET, Evelyn J.

A Matter of Perspective: Community Resident's and Nuclear Plant Workers' Long-term Reactions to the Three Mile Island Accident

A number of theoretical formulations suggests that individuals' perceptions of a stressful event are major determinants of their reaction and adjustment to the event. The present investigation sought to examine this proposition as it applies to the aftermath of the Three Mile Island (TMI) nuclear accident. Specifically, we consider panel data collected from 9 to 42 months following the accident in order to a) evaluate temporal changes in respondents' perceptions and beliefs about the effects of the accident and b) examine whether those perceptions did indeed mediate the effects of the accident on psychological distress and symptomatology. Because perceptions were likely to differ as a function of knowledge and exposure to the nuclear industry, data from two samples of respondents were examined: 267 mothers of young children who lived

within a 10-mile radius of TMI, and 125 employees at the TMI plant. Results indicated that, while opinions of the dangers and risks associated with living near the plant became less extreme over time for both groups, mothers perceived substantially greater degrees of danger than did workers. This difference in perceptions was apparent even 42 months after the accident. Equally important, however, was evidence showing that perceptions appeared to exert strong effects on psychological symptomatology only for the nuclear workers: while there was very little relationship between mothers' perceptions and their symptoms, the more dangerous a worker perceived the situation to be, the more symptoms they were likely subsequently to experience.

DIAHOND, Jr., Arthur M.

The Career Consequences for a Scientist of a Mistaken Research Project

The productivity of research scientists depends in part on the fruitfulness of the research topics they choose. But the ex post fruitfulness of scientist's research provides only a signal with noise of the scientist's future productivity since the outcome of any research topic depends in part on a stochastic component. The extent to which a scientist's subsequent labor market experience is affected by an isolated ex post 'mistake' is not clear. The aim of the current research is partly to develop the economic theory of scientific behaviour, but mainly to learn what the stylized facts are upon which future theory should focus.

For the present study, limited data have been collected on over 400 scientists who either wrote on polywater or else were members of the population of scientists who could have written on polywater. 'Polywater,' condensed in very small capillaries, was thought to be a new form of water that had a lower freezing temperature and a higher boiling temperature than ordinary water. In the West the surge of articles on polywater began in 1969. By 1973 when everyone admitted that the polywater phenomena were due to impurities in normal water, over 400 academic papers had been written on the subject.

More detailed biographical and professional data have been obtained on a subsample of about 100 scientists from the large sample. As measures of current labor market success we use number of citations received and separation from university employment. We find that the choice of polywater as a research project does adversely affect the number of citations received by a scientist but does not have any measurable effect on the probability of separation from university employment. The evidence currently available does not support the hypothesis that the effect on number of citations varies with the age of the scientist.

DOUGLAS, Susan J.

Cultural and Corporate Visions of Progress and the Emergence of Radio, 1899-1919

The purpose of this paper is to explore how wireless telegraphy, as radio was initially called, was portrayed in the popular press between 1899 and 1919. According to magazine articles and newspaper accounts, radio offered tangible evidence that the technological progress and social progress were, in America, intertwined in some ineluctable upward spiral. Yet the notion of what, exactly, "progress" meant for individuals and society changed between 1899 and 1919, and this change was reflected in the press coverage of radio's role in American life. This paper will describe the visions of progress that first surrounded wireless telegraphy at the turn-of-the-century, and speculate on how and why those visions changed. The paper argues that because most people first learned about wireless telegraphy through newspapers and magazines, that the popular press played a critical role in the social construction of radio's significance. The paper also juxtaposes cultural and corporate visions of progress and argues that both had historical consequences for the development of radio.

DOWNNEY, Gary L.

Political Variation in Scientific Claims: The Union of Concerned Scientists

Over the past fifteen years, the Union of Concerned Scientists has been the leading source of scientific support for the opposition to nuclear power in the United States. The scientific claims of this organization have been highly predictable, however. As the group moved from one distinct issue to another, its technical findings were always supportive of an alignment opposed to nuclear power. The political variation in their claims raises the question of bias. Has the organization manipulated science for political ends? Have its claims been polluted by ideological commitments? Affirmative answers are difficult to reach, however, when one considers the fact that UCS has gained a reputation for making sound technical claims. The organization has carefully nurtured its scientific credibility, often basing its own interpretations upon analyses conducted by pro-nuclear scientists in government and industry. How, then can we account for this systematic correlation between their scientific arguments and their political alignment?

The paper addresses this problem in the context of the current debate between philosophers and sociologists over the nature of scientific knowledge. I argue that philosophers cannot account for the political variation in UCS claims, because they limit themselves to identifying the grounds for scientifically rational arguments. Their approach could provide a decisive account in this case only if either UCS or its opponents can be dismissed as irrational. On the other hand, current sociological approaches that view scientific knowledge as socially

constructed cannot provide a convincing account of why UCS systematically adheres to a methodological standard that is widely accepted as scientifically rational. Current studies of rhetoric and social negotiation do not account for the significant amount of agreement among scientists about what constitutes good valid scientific claims. I contend that what is needed is a reformulation of the distinction between the scientifically rational and the social that does not view them as mutually exclusive, as the philosophers do, but that also does not dissolve scientific methodology into a reasonless product of contingent negotiations, as the sociologists do.

I suggest an alternative view drawn from symbolic anthropology, which argues that USC scientists are endeavoring to maintain consistent cultural identities. An actor's cultural identity consists of a combination of his ideological commitments and social structural statuses. To maintain the social structural status of scientist, the UCS scientist must adhere to current standards of acceptable methodology. But the organization is also structurally aligned in opposition to nuclear power, and exhibits an ideological perspective consistent with that alignment. Although the organization is constrained to make claims that can be justified as scientifically rational, it selects issues and makes claims in a way that is consistent with its cultural identity. UCS scientists are thus both scientific and political animals at the same time, but this does not necessarily mean that politics cheapens their science.

This approach views science as both cultural and rational. That is, it takes science to be a culture-bound activity whose culturally-defined role is to produce knowledge about a culturally-conceived nature. Nature is a conception of reality as a perfectly-ordered, physical whole, and scientific methodologies consist of the procedures for furthering knowledge about the relationships that constitute that order. Scientific action is rational because it is defined as such; rationality is a culture-specific phenomenon.

I conclude by showing how inconsistencies in an actor's identity demand positive action. If a UCS scientist lacked any valid scientific claims critical of nuclear power, he could try to convince the organization to change its alignment, or decide to quit his job. If his ideological beliefs were somehow inverted, he could try to convince his colleagues about the benefits of nuclear power, leave the organization, or even try to hide the inconsistency so he can continue to support his family.

DUBINSKAS, Frank A.

Mutable Communications Technology and Organizational Change

Electronic communications media are spreading in modern business and professional organizations. They are usually designed with particular communication **tools** in mind, but their introduction may stimulate organizational transformations which are unforeseen and unintended by

introducers. This research examines the branching model of communications built into a computer conference system, then examines its use and the re-structuring of networks of communicators who use it. Issues raised include the relationship of technological design to designers' cultural models, and the mutability of the artefact in use. This analysis challenges the dominant "social impact of technology" model, which depends upon fixed technological objects.

EDGERTON, Samuel Y. Jr.

Imaging Astronomical Phenomena in Extra-Galactic Space: "Objective" Science's Debt to "Subjective" Abstract Art

This paper will review part of a project currently before the NEH (Humanities, Science, and Technology Division), in which a sociologist of science (Michael Lynch, Whitman College, Walla Walla, Washington) and an art historian (myself) propose to examine whether or not working astrophysicists are influenced by aesthetic ideas deriving from contemporary abstract art when they program and evaluate radio-telescopic data, especially as coded on the new CCD (Charged Coupled Device) computer chip. This latter then projects images of stellar phenomena, in the form of brightly colored flat shapes, onto a TV monitor.

That matter is worth investigating stems from the fact that CCD technology, developed only in the 1980's, produces images (one even showing Einstein's gravitational light bending) which look remarkably like the modern abstract painting one sees nowadays in art museums. One need only compare any CCD picture of a star nebula with a color-field painting, say, by Wassily Kandinsky, ca. 1914, or by Kenneth Noland, ca. 1960, to sense a similarity that may be more than mere coincidence. CCD images, after all, are not impersonal, mechanical photographs. The colors are artificially selected by the data processors, as pictorial symbols for electronic evidence of differing light intensities. Nonetheless, CCD technology provides the most "realistic" images yet of spectacular extra-galactic bodies never seen before by even the most powerful optical telescopes. The irony here is that non-perspectival abstract art, once pooh-poohed by scientists as "unrealistic," now provides the model for picturing the "reality" of non-Euclidian outer space.

Furthermore, by noting similar art/science relationships in the past, as between Galileo and the renaissance artists of Florence, we are able to form three hypotheses concerning how science and art work together in any historical period. The first is that the images of new discoveries which scientists attempt to form in their own mind's eye are subject to the pictorial possibilities and limitations of prevailing artistic styles. The second is that the public at large will not understand a new scientific discovery until it can finally be imaged according to the accepted artistic style. The third is that if the new discovery can not be imaged in the prevailing style, then the style itself must eventually change and be replaced by pictorial forms which are compatible.

ELZEN, Boelie

The Ultracentrifuge: A Basic for Theoretical Explanations about the Development of Technological Artefacts

My final aim is to develop theoretical explanations of the developmental processes of technological artefacts. The kind of theory I am looking for is not a model which explains the complete developmental process from the first 'ideas' to e.g. its full scale production by many companies. My aim is to try to explain shorter term changes in the development of an artefact.

In looking for theoretical explanations I consider the development of a technological artefact to be a sequence of variation-, selection-, and stabilization-processes. A starting point for 'something' to be explained might be that at a certain point in history a 'problem' is perceived and that technology might help to solve this problem. Often then many variants of solution are possible 'in principle' but only some of them are considered to be worthwhile developing. When more than one solution is considered possible or is being worked out then mostly at certain point selections are being made where some lines of development are being pursued whereas others are not. After some time some variants of solution may be developed to the point that they 'can be considered' to provide a solution for the original problem. However, it is still an open question whether all or most of the relevant social groups or people consider this solution to be an actual solution to their problem; i.e. some solutions may stabilize whereas others may not.

The kind of theoretical explanation I am looking for is that under certain conditions some variation-, solution- and stabilization-processes are more likely to occur than others. These conditions will have to be specified in terms of a characterization of the social groups which are relevant with respect to the development of the artefact. This relevance can be any type of relation between the social group and the artefact e.g. having a problem for which the artefact might offer a solution, a wish to sell artefacts, a wish to develop artefacts, etc.

In order to obtain an empirical basis on which such a theory can be founded, I have studied the development of an artefact called 'ultracentrifuge'. This case study seems to provide a very fruitful basis for answering theoretical questions because several ultracentrifuges were developed while their developments hardly influenced each other. These developments are comparable in some respects whereas in other respects there are also important differences. Comparing these various developments then makes it possible to look for differences and similarities in their developmental processes which in their turn will give an indication of the type of variables that should play a role in a theoretical explanation.

In the paper I will first give a historical description of the development of two ultracentrifuges. These developments both have taken place around 1930, one of them in the USA, the other in Sweden. (I am currently working on a third development which has taken place in the Netherlands in the 1950s and 1960s, but I am not sure when this case

paper is extracted.

FULLER, Steve

The Demarcation of Science: A Problem whose Demise has been Greatly Exaggerated

Larry Laudan has recently argued that the demarcation of science from non-science is a pseudo-problem which should be replaced by the more modest task of determining whether and why particular beliefs are epistemically warranted or heuristically fertile. In response, Thomas Gieryn has rejected this exercise in philosophical self-restraint, arguing that the scientific community itself takes measures to distinguish itself from others who compete for cognitive authority and its attendant political and economic benefits. As a result, rhetorical strategies develop—"boundary work" Gieryn calls them—which are proper objects of sociological study. In this paper, I shall argue that Laudan is wrong to think that there is nothing at stake in demarcating science from non-science, but I shall also argue that the importance of the demarcation problem has not been fully appreciated by Gieryn. My critique will reveal a basic confusion that needs to be dispelled before the full significance of demarcating science can be seen: namely, the failure to distinguish the relatively constant social role played by what has been called "science" from the historically variable social practices that have played the role of science. In addition, I shall claim that this confusion is itself one of the key ways in which a social practice retains its status as science.

GENUTH, Joel

Science Advice to the President and the Debate Over the NSF

The debate over establishing the National Science Foundation stands out in U.S. history as the most explicit attempt to draft what Don Price has termed "America's unwritten constitution" for the relationship of organized knowledge to political power. What was at stake in the debate, I will argue, was not whether scientific research would be under peer control, but whether a program of government-funded research should form a permanent base for channeling science advice to the president. On one side, Vannevar Bush and his allies sought to make the NSF answerable to Congress and left the president with only ad hoc means of obtaining science advice. By contrast, Harley Kilgore's version of the NSF made the NSF's director virtually a member of the president's cabinet. When looked at from this perspective, the appropriate questions to ask about the NSF debate are what kinds of issues did a science advisor seem likely to place on the presidential agenda, whose prerogatives would have been affected by presidential initiatives in these areas, and why did scientists divide over this question of political administration. I will

study will be completed). I have studied and will describe these case-studies from a social-constructivist point of view. I will show that these artefacts do not exist independently from their 'social contexts'. Different people 'see' different artefacts and different aspects of the artefact are perceived to be problematic which means that the people involved will try to influence different aspects. Thus the appearance of the artefact will reflect the social context in which it was developed. I will show in the paper that the fact, that different people 'see' a different artefact has led to different developmental processes and to different appearances of the artefact.

This study has not yet been pursued to the point that I can theoretically explain 'all of the important episodes' in these developments but in the paper I will present several theoretical statements which can explain some of the important differences and similarities between the various ultracentrifuge developments.

FUJIMURA, Joan H.

The Construction of Double Research Problems in Basic Cancer Research

This paper frames the concept of "doubleability" as a problem for the social studies of science. The term is thrown out by researchers in many lines of work as a baseline condition for the successful development of a line of research. This paper begins the process of breaking open the black-box of doubleability. The case study used to examine doubleability is the new and fast growing line of cancer research on proto- and cellular oncogenes.

The analysis demonstrates that scientists construct double problems by juggling the resources available to them within the constraints on their activities. Doubleability is based within the organization of work and not in the "nature" of problems. Since work organization can change through time, double problems are processes constantly under construction. They are shaped and reshaped by multiple actors within concrete work situations throughout the tenure of the research.

I designate a problem as double if researchers can decompose it into actual work tasks -- in this case, experiments -- which can be carried out to an endpoint. At this endpoint, the change or lack of change in the phenomenon of interest is interpreted as having some meaning.

Based on the above definition, doubleability is comprised of sets of conditions which shout either yes, work can be done on a problem, or no, work cannot be done. These conditions include: modularity, accessibility, flexibility, variety, and satisfaction of evaluation criteria.

Doubleability alone is not enough to explain the growth of a new line of research. Plausibility and the establishment of research infrastructures are other conditions discussed in the larger project from which this

way at all.

A generation ago, Herbert Simon developed a theory of decision making by people in business organizations. He described it as a theory of "bounded rationality," but it may equally well be understood as simply a descriptive account of how people in fact make decisions. The fundamental principle of Simon's theory is satisficing. Satisficing is most easily understood by contrast with standard normative theories of rational decision making.

Standard decision theories assume that a rational agent can survey all possible options and all possible resulting states of the world. However, rational agents can assign a utility to each possible outcome (action-state pair) in their decision matrix -- or at least rank order all outcomes. Some theories assume that rational agents also possess degrees of belief (having the structure of a probability distribution) for all possible resulting states of the world. Finally, rational agents possess a general principle of decision making, such as maximizing expected utility, which tells them which option is the uniquely rational choice. It is clear that no real human being is a rational agent.

Simon's theory is much more realistic. His agents confine their attention to those options and possible results they deem relevant. They assess the possible outcomes of their decision by the simple test of whether they would regard that outcome as personally satisfactory or not. They then look for an option that yields a satisfactory outcome no matter which of the perceived results occurs. Failing to find such an option, they either lower their satisfaction level until some option becomes satisfactory, or they attempt to change their situation by developing new options or by blocking undesirable results. Simon has applied this theory to animals as well as to humans. I propose to apply it to scientists.

The focus of my inquiry is the work of nuclear physicists at a national cyclotron facility. Of all the many different types of decisions faced by such scientists, I shall focus on three: decisions regarding which experiments to pursue, decisions regarding experimental design, and, finally, decisions regarding which theoretical model to accept as best supported by the results of experiments. My hypothesis is that in these sorts of situations, scientists tend to be satisficers. In all three cases, this hypothesis is empirically supported by the actions of nuclear physicists and in turn helps explain why they do what they do.

Regarding the pursuit of particular experiments, I have found, for example, that scientists' decisions are strongly influenced by available instruments. They do not attempt to survey all possible experiments that have not yet been done. Rather, they focus on those that might be carried out with equipment that either is already available or might reasonably be constructed using existing facilities. Similarly, they restrict their attention to experiments for which their previous experience has prepared them. Thus they can be fairly confident that whatever data they obtain will be publishable even if it turns out not to be the most exciting possible. This is typical satisficing behavior.

seek to answer these questions by comparing the programs and politics of two Roosevelt administration organizations: the Science Advisory Board -- an ad hoc group headed by Bush's mentor Karl Compton -- and the National Resources Planning Board -- which the Executive Reorganization Act of 1937 would have elevated into the Executive Office of the President had the bill been passed. An examination of how the experiences of these boards shaped the debate over science policy following World War II may shed some light on the pros and cons of various ways of setting up advisory systems.

GIERE, Ronald N.

The Scientist as Satisficer

During the past decade, sociologists of science have undertaken to explain how scientists choose one theory over others. In giving such explanations, cognitive sociologists have invoked both general social interests (the Edinburgh School) and special professional interests as revealed in day to day social interactions among scientists (Latour and Woolgar, Knorr-Cetina). Both types of explanation are thoroughly naturalistic in that they invoke only natural causal processes to explain how scientists come to adopt one belief rather than another. This is in strong contrast to accounts given both by earlier functionalist sociologists (Merton) and by philosophers of science. The latter have traditionally appealed to methodological norms or to universal principles of rationality to which scientists are assumed to subscribe.

My view is that the naturalistic approach to understanding how scientists choose theories is fundamentally correct. The task of science studies is not to justify science, but to explain it. But the appeal to interests, whether general or more specifically professional, is incomplete. It fails to explain the connection between interests and specific cognitive choices by individual scientists. These approaches need, therefore, to be supplemented by a suitable theory of how individual scientists make cognitive choices.

It is understandable that sociologists, no matter how much they emphasize the importance of local conditions, should nevertheless resist the move to an account of cognitive choice by individuals. Such a move tends to break down the boundary between sociology and psychology, and thus threatens the existence of sociology as an independent discipline. My view is that the existence of this boundary is an impediment to developing an adequate theory of science. Here the professional interests of sociologists must give way to the interests of science studies as itself as unified field.

Perceiving a need and meeting that need are, of course, two different things. And it cannot be said that cognitive psychology has developed detailed and comprehensive theories of individual choice that are readily applicable to the cognitive choices of scientists. The theory I would recommend is, therefore, a halfway house. But half way is better than no

Similarly, in designing an experiment, these physicists typically do not devote time and resources to making the experiment as precise as possible, but only significantly more precise than similar experiments that might be done elsewhere. Again, this makes the production of publishable data fairly likely no matter what the range of values actually obtained. Obtaining publishable data is generally regarded as at least a satisfactory outcome.

Finally, in deciding to accept a theoretical model as the best available, physicists consider only alternative models actually in the literature. And the data actually obtained must, based on their knowledge of the experiment, be both fairly likely if the accepted model is correct and fairly unlikely if any one of the proposed alternatives is correct. This makes it, to the best of their knowledge, fairly unlikely that they have chosen a badly mistaken model. This way of proceeding is, therefore, unlikely to yield a clearly unsatisfactory outcome -- choosing a badly mistaken model. As a matter of fact, however, it is very difficult to design a single experiment that might so neatly sort out the available models. And even experiments designed with this intention seldom yield data that are in fact so decisive. (I have been told that Murphy's law for experimental physics is that an experiment always yields the most uninformative of all possible data.) Thus decisions to accept a model tend to be complex affairs based on a number of different experiments. But it is still fairly clear that these physicists are seeking not the ideally optimal choice, but only one that is satisfactory for the time being.

In conclusion, it should be pointed out that what counts as a satisfactory outcome in various contexts is not merely a subjective matter for individual physicists. Individuals differ, of course, but in some contexts there is also wide agreement on what is satisfactory and what is not. In one experiment I observed, the leader of the project made a mistake and ruined several specially designed detectors. His associates finished the run with substitute detectors, but there was universal agreement during later discussion of the experiment that the data obtained was not publishable. Although our understanding of scientific activities is not complete without an account of individual cognitive choices, neither can it be complete without an account of social interactions and how they influence individual choices.

GILLMOR, C. Stewart

Replication of An Evaluation of Research Results From the Nancay Radiotelescope

The literature concerning data from the large radiotelescope at Nancay, France is evaluated for the disciplines of geophysics and solar system radioastronomy for the period 1969-1978, using publication and citation data. The results are shown to be in strong contrast with those from a recent study of the Nancay radiotelescope by S. Plummer, B. R. Martin and J. Irvine (La Recherche, 15, December 1984, pp. 1610-1611).

Martin and Irvine's notions of "converging partial indicators", "comparing like with like", and their methods of selecting and classifying the Nancay literature are discussed. In addition, other results on the evaluation of the Nancay radiotelescope with be presented.

GÖKALP, İskender

A Study in the Constitution of a Frontier Scientific Field: The Case of Turbulent Combustion

Turbulent Combustion (TC) is a relatively new scientific research area (its origin may be dated in the years 1930-40) constituted at the interface of the Mechanics of Turbulence and the Chemistry of Combustion. These two domains have shared very little in common before the emergence of TC: they were autonomous scientific domains in the sense analysed by D. Shapere (in F. Suppe, The Structure of Scientific Theories, 1974, pp. 518-565). The process of the constitution of the TC, its further development and current autonomization constitute an interesting case study pertaining to discussions concerning topics such as relations between scientific fields or theories, unification of science, reductionism, interfield theories, etc. Moreover, the investigation of the patterns of the autonomization of this frontier field informs us about the basic mechanisms of interdisciplinarity. Indeed, the intervention of only two autonomous domains (and both from the same side of the exact sciences/social science barrier) without any "background knowledge" between them, designates the TC as a simple model for interdisciplinarity studies.

In this communication, the constitution of TC as a frontier scientific domain is investigated from several angles. As a first point, different stages of its growth are sketched and compared with conventional growth patterns (three or four staged patterns as identified by the sociology of science) of non-frontier domains. The peculiarities of the development stages of TC are then emphasized. In the same framework, the constitution of the TC scientific community is also investigated. Namely, the time evolution of the respective shares of the scientific origins (working initially in turbulence, combustion or other fields) of the members of this community is investigated.

Besides these sociological informations on TC, some questions concerning the topic of the constitution of a frontier paradigm are also addressed. Within this framework, the following points are raised.

- *The interaction mechanisms of the intervening fields (combustion and turbulence) are investigated. The role of a shared vocabulary is emphasized. The concept of the "flame generated turbulence", proposed in the early stages of the TC is treated with detail in order to exemplify a given state of these interaction mechanisms.
- *The influence of the intervening fields on their reciprocal conceptualization is dealt with, by using data from the exemplary situations given by the necessity of inclusion of a chemical source term into the Navier-Stokes equations or by the necessity of taking into

HAGENDIJK, R.P.
GRAMER, J.

Cognitive Structure: Intellectual Traditions and the Flexibility of Scientists

In recent years there has been a trend in the sociology of science to stress the 'negotiated' and 'locally contingent' character of scientific knowledge. Ethnographical studies have been carried out to highlight the local social processes of 'negotiation' and 'construction'. It has been suggested by a number of authors that 'specialists' communities' and 'disciplines' are not of much relevance to the understanding of scientific practice. Nevertheless, even if one accepts this position, the question remains: how are the activities of scientists related to one another and to the general state of affairs in their field? To say that research practice is contingent on the local laboratory situation, does not imply that research activities take place in a microsociological vacuum. Scientist's perceptions of the cognitive structure of their field, together with the research priorities implicit in these perceptions, play an essential role in designing research projects and writing grant proposals as well as in the final reporting. This suggests that such perceptions of the cognitive structure are a major constraining factor in the entire research process.

This paper investigates how a group of Dutch fresh-water ecologists perceive the cognitive structure of their field, the sources of these perceptions and the extent to which these perceptions reveal elements of the cognitive structure that constrain the cognitive flexibility of the sciences.

The perceptions of the scientists have been analysed using a technique of triadic comparisons. Kelly's (1955) work on the psychology of personal constructs and Coxon and Jones' (1978) work on the cognitive aspects of social stratification were sources of inspiration. The data of the triadic comparisons were analysed using multidimensional scaling analysis. The paper compares the results from this analysis of the perceptions of the cognitive structure with the citation structure of the field and with historical studies about its cognitive development. The constraining character of the major elements in the perception of the cognitive structure will be analysed using interview material on the perception of cognitive flexibility by the scientists themselves, as well as data on the stability of their research career.

From the analysis it appears that the scientists perceive the cognitive structure of their field in terms of the multiple intellectual traditions which form the cultural heritage of the field. The paper will discuss the extent to which the basic distinctions in their perception of the field's cognitive structure can also be found at the level of the citation structure. Given a certain position via the cognitive structure of the field it appears that the options a scientist has to do research are constrained. This constraining character of the cognitive structure is related to the educational background of the scientists and to intellectual traditions in the institutional history and the communication structures of the field. The paper will discuss the

account the temperature fluctuations in the Arthenius chemical production term. The problems raised in both autonomous fields by the incursion of these new items are stressed.
*The answer of each autonomous community to new experimental exigencies raised by their unification are emphasized on the example of the advent of non intrusive optical turbulence measurement techniques.

HADDEN, Richard W.

Mathematics, Relativism and David Bloor

David Bloor is the major proponent of the strong program in the Sociology of knowledge and science. Bloor's contention is that relating science to society is an intellectual practice in strict conformity with the major tenets of science itself; thus, the relativism involved should produce reactions neither of fear nor disdain in scientists.

Most criticisms of Bloor have come from the rationalist side of the rationality/relativism controversy and have expressed the fear that the acceptance of Bloor's relativism leaves us with no standards for judging validity, reliability, knowledge or science. Even though scientific rationality may not be absolute, it is the supreme accomplishment of our culture; it is all we've got. My criticism of Bloor, however, is that he is not really a relativist at all. Bloor claims to hold to four principles: relativism, impartiality, symmetry, and reflexivity; in fact, he violates all of them. He espouses a scientific position, for which he apologizes at the end of his Knowledge and Social Imagery.

In looking specifically at mathematics I take issue with both the rationalists version of mathematical knowledge as special and with Bloor's strong-program interpretation of the development of modern mathematics. Both Bloor and his rationalist critics view the rise of science and mathematics through positivist lenses; Greek science symbolized order and hierarchy, whereas modern mathematics is founded on the technical requirements of the age. For Bloor's numbers came to perform a new technological function. I argue, however, that a new concept of number had to be constituted first, which could then prove to have a technical applicability. This new number concept was socially constituted in the practice of expanding European commerce from Fibonacci to Stevin.

In the case of Stevin, for example, an early life in commerce and commercial arithmetic, as well as his position as commercial and administrative advisor to Prince Maurice, lead to his self-conscious attack on the Greek theory of number in De Thiende and L'Arithmétique. Thus Bloor's notion of a mechanistic connection between science and society presupposes the validity of the very mechanistic thought which I take as my task to interpret in social, relativistic terms.

implications of these results for the debate on the 'constructed' character of science.

HARGENS, Lowell L.

Disciplinary Differences in Journal Acceptance Rates

We have known for over a decade that journals' acceptance rates of papers submitted for possible publication vary widely across disciplines. In the natural sciences these rates typically range from 60 to 85 percent, while in the social sciences and humanities they typically range from 10 to 30 percent. Two possible explanations of this variation have been advanced: first, that it is a consequence of variation in codification or consensus across disciplines, and second, that it is a consequence of disciplinary variation in journal space shortages. These explanations have recently become the center of attention because of their relevance to the more general issue of whether disciplines do indeed exhibit different levels of consensus.

In this paper I present evidence on both of these explanations. Using time series data on acceptance rates for journals in various disciplines and published commentaries by journal editors, I assess the space shortage explanation. These two types of data both suggest that this explanation cannot account for the observed variation in acceptance rates. Next, I develop a probability model of the journal review process to determine whether factors within that process can account for the observed disciplinary variation in acceptance rates. The factors included in the model are (1) the average probability that a reviewer will judge a paper to merit publication, (2) the degree of agreement among reviewers of a given paper, (3) the number of reviewers whose opinions are originally solicited by a journal editor, and (4) editors' responses to disparities in reviewers' assessments. The model demonstrates that the average probability that a reviewer will judge a paper to merit publication is much more important than the other three factors. Furthermore, variation in this probability alone is sufficient to produce the observed differences in acceptance rates. The probability model also suggests that consequences of variation in the probability can account for variation in the third and fourth factors. Finally, the importance of disciplinary variation in the first factor suggests that disciplinary variation in codification or consensus produces the observed variation in acceptance rates.

HOCH, P.K.

The Academic Assistance Council and its Policies Toward Refugee Academics in the 1930s

This paper examines the emergence of policies for the major refugee-aid

organization in Britain for displaced academics after the rise of a Nazi government in Germany in 1933 and its spread to other countries thereafter. Inasmuch as scholarship in Germany was predominant in most scientific disciplines before 1933, the migration of refugee scientists in the following years--and the policies developed to deal with it--was to have a major impact on the structure of world science, including the place of British (and American) science within it. It is therefore important to examine the institutional pressures on the AAC--especially from the British government and universities--and their policy outcomes and results from the refugee scientists and science generally. This was indirectly to have a considerable impact on the subsequent development of such specialties as theoretical physics, biochemistry and physical chemistry, among many others.

JACKSON, Jean

Anthropological Models of Fieldwork in Traditional and Laboratory Settings

This paper analyzes how ethnographic research in a clinical-research setting in a rehabilitation hospital differs from traditional anthropological fieldwork projects carried out in the Northwest Amazon, Mexico and Guatemala. Topics covered include: boundaries (entering and leaving the field), multiple roles (of the anthropologist, of the "natives"), power and authority, obtaining permission, culture and subculture, translating native languages, unobtrusive participant observation, gaining confidence, objectivity and bias, and ethics.

JORDAN, Brigitte

Artifacts and the Social Allocation of Knowledge in Obstetric Settings

This paper reports a comparative study of obstetric settings across cultures, focusing on the relative sophistication of accompanying technology, and the ramifications of the technology for participants and for the structure of the event. A central observation is that technology in obstetric settings organizes the social allocation of knowledge and the distribution of relevant information. Changes in the conception of relevant information and rights to its ownership in obstetric settings, in turn, have profound implications for the organization and experience of the event of childbirth itself.

KROHN, Roger

Visual Displays as Social Cognition: Evidence and Argument in Limnology

This paper will attempt to ground a study of the practical, every day reasoning of research scientists in an interaction theory of perception (Gibson) and in the process of the construction of a "sense of order" (Gombrich). Psychological research on the "cognitive limits" of scientists and others (Wason, Iversky, Kahnman, Hynatt, Doherty, Mahoney) will be re-interpreted as pattern seeking and order construction rather than methodological or logical fallures. The question arises, why do scientists persist in "confirmation bias", "ignoring base rate data", "judgemental heuristics", "pseudo-diagnosticity", and "anchoring and adjustment" and how do they eventually produce consensual knowledge?

The first thematic answer is that some reasoning patterns still remaining implicit are prior in time and in confidence to others that have been articulated as rules of scientific method and employed for their persuasive force. Participant-observation of research and research seminars in limnology that visual displays are much more prominent both in the process of analysis and in persuasive contexts than the literature on science would lead us to expect. Why are practitioners so focused on these visual displays? The hypothesis is that they represent attempts to translate a large variety of research materials gathered at different times by different people, and under different circumstances into a "simultaneous visual field" where confident discriminations can be made.

The second interpretive theme is that research reveals a cycle of tolerance of improvisation followed by selective rigor and scrutiny as visual patterns are sought and then selectively tested, e.g. mathematically. In seminars there is notably little talk among research practitioners about visual pattern search, in relation to its large practical role, while there is a very great deal of talk about statistical-mathematical measures and tests so important in the persuasive and critical process. During field research, visual patterns are much more and statistical analysis much less in evidence. Even in the latter case there is more articulation of problems and exceptions ("scatter" and "outliers") than of the types of patterns sought and found.

Finally the paper will attempt to trace the series of translations that leads from field materials to visualization and observational traces to numbers to words and sentences and eventually to publications. We can see scientists' discourse as a running commentary parallel to the history of the environmentally derived traces. Because of the long series of translations, the final, formal logical statements can not be brought back to directly confront environmental materials. Still, the statements can be seen to have been ultimately environmentally derived and relevant to future actions toward it.

KWA, Chunglin
HIDDINGA, Anja

Metaphors in Science: A Sociological Perspective

While there is a continuing interest for metaphors in scientific discourse among philosophers of science (Hesse 1966, Boyd 1979, Darden 1982, Martin & Harter), among sociologists of science this does not seem to be the case. Some important explorations were made by Barnes (1974) and Edge (1974-75), but in later writings of the Edinburgh school the concept of interest and the related problem of causal explanation have almost entirely superseded the concept of metaphor (Barnes 1977, see also Woolgar 1981, and rejoinders of Barnes 1981 and McKenzie 1981). In our opinion however, the possibilities for sociological analysis of metaphors in science have not at all been exhausted.

As Boyd (1979) has pointed out, many metaphors in science may be considered theory constitutive. They are not used for ornamental or didactical purposes, but phrase what cannot be said otherwise, and bring with them a set of heuristic implications, in a word: a research programme. By their nature metaphors impute a set of meanings drawn from one particular domain to another. Accompanying values, promises, etc. are transferred too. Metaphors may be derived from other sciences. However, they often do not rest on esoteric forms of scientific knowledge, but, instead, on more or less widely diffused concepts and models from the social world. Very often, these metaphors are technological in nature: the electric network in 19th century American neurology (Rosenberg 1966), the steam engine in Freud's neurosis doctrine (Russekman 1981), the computer in modern cognitive psychology (Boyd 1979, Lakoff & Johnson 1980), cybernetic control theory in modern American ecology (Kwa 1985). Other fields of human experience also provide metaphors: war in medical bacteriology, in cancer research (Sontag 1978), and in entomology (Perkins 1982). Metaphors of war imply the possibility of victory, technological metaphors the possibility of control. From these broader analogies that fit the metaphorical meanings, concepts can be drawn to typify newly perceived phenomena or new ways of understanding. The analogy of electric network gives rise f.e. to the concept of circuit, the steam engine to the concept of pressure, the computer to that of information processing and cybernetic control to that of feed back.

It seems to us that the social origins of metaphors used in science have importance consequences. Metaphors serve as a means of communication with the public, through which metaphor basic science appears as both legitimate and worth public support. This means that metaphors, analogies and models do not only constitute cognitive resources for scientists, but social resources as well. Analogies which are widely diffused in society might be considered (under the appropriate circumstances) to constitute mentalities or dominant ideologies (non pejoratively understood). They attribute plausibility to metaphors that draw from them.

On the basis of two cases, post war American ecology and medicine (obstetrics), we want to examine how metaphors work in the communication between scientists and the public, how metaphors can be said to organize coalitions between scientists and non-scientists, how they mobilize support and legitimation for science.

LANONT, Michele

Changes in the Human Sciences Field and Macro Structural Transformation in Quebec

The spectacular development of economics, psychology and sociology since World War II, and the simultaneous decline of the humanities raise two interesting sociological problems: how are these changes related to macro-structural transformations in post-industrial societies? What are the macro-structural effects of the developmental of the social sciences? This paper describes changes in the "human sciences" fields in Quebec in the seventies, and relates them to macro-structural changes in that society. It compares the social characteristics of students and professors of nine social sciences and humanities disciplines (1972-1978) in order to describe changes in the human sciences field. It argues that professional social sciences disciplines contributed to the exceptionally rapid growth of the state and of the upper-middle class in Quebec since the mid-sixties. It also argues that higher education policies of the Quebec Government has systematically favored the development of professional disciplines and a reduction of the autonomy of Quebec universities, which in turn accelerated the decline of the humanities.

LESLIE, Stuart W.

Steeple Building at Stanford: The Boundary Problems of Post-War Engineering and Physics

Stanford University earned a national reputation in electrical engineering and physics in the post-war years in large measure by creating flourishing institutional hybrids in the spaces between traditional disciplines—e.g. the Stanford Electronics Laboratories, the Microwave Laboratory, the Stanford Linear Accelerator Center, and the Division of Applied Physics. However these "steeples of excellence" involved more than an intellectual symbiosis of two disciplines. Understanding the content and direction of their research programs requires an appreciation of a new kind of political economy of science and technology and of the ways in which it shaped, and was shaped by, the entrepreneurial strategies of Stanford's institution builders.

LEYDESCHORFF, Loet

Journal-Journal Citations as Indicators of Change

Measurement of the effectiveness of science policies is analyzed as a multi-level problem. Journal-journal citations are discussed as a potential candidate for a domain beyond the control of policy-makers and authors or research groups and therefore may function as a relatively stable and easily accessible baseline for the 'calibration' of outputs

and outcomes of science policy. A method is developed, using SCI's JCRs which is then applied to the two cases of water pollution and humanisation of labor.

This method can also be used as a simple indicator for the development of journal-journal citation patterns over time.

LOWE, Charles U.
NADEL, Edward
ROGERS, Everett C.

Tracking a Research Front by Co-term Clustering of NIH Grants: 1974-1984: A Case Study in Immunology Research

Results of co-term clustering of funded NIH research grants covering the period 1974-1984 will be presented. Single-link clustering was applied to a data base of the NIH grants indexed by key words from the CRISP thesaurus (an average of 15 terms per grant). Topics will include: (1) a description of characteristics of the cluster file; (2) the contents of a representative cluster, immunology, using multidimensional scaled maps; (3) a method for choosing optimal normalization levels in order to link clusters longitudinally. Immunology will again provide the example.

MACKENZIE, M.
KEATING, P
CAMBROSIO, A

A Third Sector Approach to the Science/Industry Interface: The Case of Diagnostic Kits Using Monoclonal Antibodies

This paper will be a report of, and conclusions from, a study of the emergence and development of hybridoma and monoclonal antibody technology in the biomedical field and the transportation of these techniques to the commercial sector.

The purpose of the study is twofold: first to offer a description and analysis of the development and dissemination of a scientific technique with specific reference to its absorption and utilization by the commercial/industrial sector; second, to test and develop a model and methodology that is appropriate to the analysis of the processes constitutive of the science/industry interface, and comprehensive enough to accommodate the broad spectrum of phenomena involved.

Others have already noted that the biotechnology field may be characterized by a noticeable reduction of the possibility of distinguishing between pure and applied science. Consequently to this reduction there has been the emergence of a number of hybrid organizations which function both as academic and commercial institutions and the

Ever since Darwin the possibilities for the cultural coding of biological information have been set by two self-images of science: reductionist (radical materialist) and wholist (romantic materialist). The first tradition is exemplified by the German biophysicists (Helmholtz, du Bois-Reymond, and Ludwig); the second by the logic and language of the early drafts of Charles Darwin's theory of the transmutation of species. These alternatives can be distinguished by their opposed views concerning the relation of nature and culture.

The radical materialist manifesto of the biophysical school divided nature and culture by describing and explaining the organs of thought (the brain and the nervous system) strictly in terms of their basic physical and chemical properties. This enthusiasm for the reduction of biology to the unity of physics and chemistry was matched, within the mandarin ethos of the German professoriate, by an equally strong insistence upon the limits of science as but one among several quite distinct cultural voices. Science, according to du Bois-Reymond, is incapable of dealing with such basic philosophical puzzles as human freedom, moral responsibility, or the nature of consciousness. This school obliterated cultural concerns in favor of a radically reductionist, materialist ontology of science only to resolutely isolate them through reverence for a philosophical and artistic tradition ranging from Kant and Goethe to Nietzsche.

The wholist (romantic) approach to science united natural and cultural categories. This was the rhetorical strategy of Darwin's Origin and its effective development was crucial for his effort to cause the old cultural fabric to totter and fall by demonstrating the evolution, from much simpler forms of life, of the capacities underlying the highest features of human culture. The romantic strategy is unmistakable in his insistent expression of the basic concepts of this theory in metaphorical (equivocal) terms, e.g., 'struggle for existence', 'natural selection', 'island economy'. Unlike the German biophysicists, Darwin sought to provide biological explanations or explanations of human freedom, the moral sense, the religious sense of awe, and the nature of human intelligence. So pervasive and ineliminable is his tendency to personify nature (and attribute awareness to animals) that heirs of the opponent tradition often doubt Darwin's understanding and practice of "the scientific method".

An analysis of nature inevitably reflects some view concerning the relation of nature and culture and such views are usually embedded in (1) general methodological prescriptions putatively distinguishing established scientific procedures from non-science, or immature or incomplete science, and criteria for utilizing information from other scientific disciplines, and (2) metaphysical beliefs concerning, e.g., the adequacy of a severely reduced physicalist ontology. Scientific information may be culturally loaded to facilitate its communication to particular audiences. Such efforts typically include forms and figures of speech reflecting the culture of the intended audience. Consequently, (3) face-to-face, power-knowledge relationships, (4) the relative effectiveness of the means by which different scientific disciplines and institutions compete for satisfactory levels of social and economic support, and (5) moral, economic and political beliefs embedded in the

construction of complex legal, financial and technical arrangements between otherwise separate academic and commercial establishments. The hybridoma/monoclonal antibody area is virtually paradigmatic of the kind of advance that has led to these - institutional changes - it is a significant scientific advance (Milstein and Köhler, Nobel Prize, 1984), and its application to diagnostic technology constitutes the first major market sector in biotechnology. Our study then examines the development of in vitro diagnostic kits under different institutional circumstances including, 1) small companies with special relations to university departments, and 2) a para-university research center founded for the purpose of integrating academic functions and scientific research with commercial goals.

The common thread in current analyses of university/industry relations in biotechnology emphasises the conflicts between different types of activity regulated by anti-theoretical norms, epitomised by the opposing strategies of scientific publication and patenting. The "Third Sector" model which we employ ("a macro-economic productive sector additional to, and analogous to, the sectors for production of capital goods and consumer goods but organized for the production of technical and technological change goods") offers a conceptualisation which straddles the previously sacrosanct division between university and industry. Thus a methodology is generated which emphasises accommodation rather than conflict and which, we argue, is closer to the practical and economic realities of biotechnology than previous approaches.

MANIER, Ed

It's Time to Open the Black Box: The Cultural Coding of the Neurobiology of Cognition

Contemporary neurobiology is dominated by the sense that "it's time to open the black box" (the brain and central ganglia) and reduce the cognitive phenomena of learning, emotion, and communication to the "more basic and fundamental" principles of cell biology (biophysics, biochemistry, molecular biology, developmental biology, and electron microscopic anatomy). Although "it's still early days" in the development of an adequate reductive picture of the simplest of the candidate phenomena, neurobiologists are now confident that they "see single (ionic membrane) channels open and close" by means of the technique known as patch clamping. Since the control of the flow of ions through neuronal membranes modulates the action potential (transmitting and analyzing information in the nervous system), "seeing" a channel open and close creates an optimistic sense of kinship with the physicists and their bubble chambers. However, preliminary results from a case of the effectiveness of cross-field communication patterns connecting the diverse but putatively "converging" research areas of neurobiology, contemporary animal learning theory and the ethology of animal communication suggest that the construction of coherent fields of research from such disparate sources is more problematic than slogans of reductionism imply.

dominant institutions of a society all affect the cultural coding of science.

This level of social and cultural complexity naturally results in situations in which converging research areas exemplify different scientific traditions, with distinctive vocabularies, criteria of adequacy, experimental paradigms, and explanatory goals. Migration, translation and communication across such cultural boundaries face special difficulties manifest in current efforts to integrate neurobiology, learning theory, and ethology.

Actual scientific practice at the borders of such disparate fields can be nearly warlike. Interpretation and appraisal of such practice and the resultant knowledge claims require a concept of effective scientific communication understood as (1) accurate, complete, and reciprocal inter-field exchange of information (methods, results, explanatory models, theoretical goals, and ambiguous and tacit aspects of scientific practice) and (2) reciprocity in the exchange of criticism and appraisal of reported results, and in requests for reformulation of investigative priorities, research designs, and evaluative criteria. The notion of 'effective communication' is invoked only as a critical regulative ideal and does not imply the existence of examples of effective communicative links between neurobiology and psychological or ethological studies of animal cognition.

Nevertheless, critical analysis of interfield communication of scientific information is a good place to begin the discussion of the cultural coding of science. Interfield communication provides a compelling context within which to examine the negotiation of such key epistemic properties of a scientific theory as its empirical scope, semantic precision, heuristic power, or explanatory adequacy, as well as a relatively tractable model of the processes leading to the assimilation of scientific results into the general culture.

MARTIN, Ben R.
IRVINE, John

Radio Astronomy Revisited

In 1978-79 we carried out an evaluation of the scientific performance of four radio astronomy observatories in Western Europe. The method involved applying a range of bibliometric indicators, each reflecting a different facet of research performance, to the four observatories, and comparing the results with 'peer-rankings' obtained by asking a large sample of radio astronomers to rank the observatories in order according to their relative contributions to radio astronomy over the period 1969-78. Two years later in 1981, (together with a postgraduate student) we expanded the study to include the French radio telescope at Nançay.

Earlier this year, in part as a result of comments and criticisms by Stewart Gillmor, we became aware of certain limitations and defects in

the methodology employed in that earlier work. The radio astronomy study was the first research evaluation that we carried out, and the methodology was very much of a 'first-generation' nature. In particular, the unit of analysis on which the study focused (the research 'centre') was probably not the most appropriate or at least was not adequately specified; we had not used the breakdown of publication output (between theory, observation, instrumentation, review articles, and so on) employed in later studies; and the criteria for deciding what papers should be included or excluded were not sufficiently clear. Moreover, the process of evolution in the methodology that took place between 1979 and 1981 (a process the magnitude of which we did not fully appreciate at the time) meant that the results of the Nançay study were not completely consistent with those for the original four observatories.

In the light of this, we decided to repeat the radio astronomy evaluation examining the effect of applying a more rigorous assessment methodology of the type used in later generation studies. In our paper we shall present the results of this reappraisal and, if time permits, also look at what changes have taken place in the relative performance of the radio observatories since 1979.

MAYO, Deborah G.

Policy Values and Epistemic Values

The recognition that values enter at all stages of hazard analysis, both in statistical assessment of hazards and in subsequent policy decisions, is typically thought to cast doubt on the use of statistical-scientific considerations as an objective tool for adjudicating controversies over hazardous technologies. I claim that the resulting tendency to downplay the importance of scientific-statistical considerations has exacerbated the problem of holding risk assessors accountable to the policy values of society. The purpose of this paper is to explain why. I argue that in order to ensure the public's policy values are adequately reflected in hazard regulations it is crucial that the interested public or its representatives (as well as judges, lawyers, scientists, and policy-makers) be given tools for understanding and criticizing the statistical hazard assessments underlying hazard regulations. This requires, not formal rules or "recipes" found in statistics' texts, but rules for critically interpreting statistical hazard assessments. I show that the ability to formulate such rules is based on the possibility of distinguishing scientific and epistemological values from policy values. Failure to make that distinction results in forfeiting a vital tool for understanding and resolving controversial hazard assessments.

MCAULAY, Robert

Romanticism, Powerlessness, and Deviant Paradigms

The cases of Velikovsky, Creation Science, and Laetrile constitute more than simply (rejected) alternative paradigms on the margins of science. Each, in varying ways, is illustrative of the role which deviant theoretical perspectives may play as expressions of protest against aspects of contemporary science and society. There are, in each case, elements of a Conservative Romantic response to the limits of scientific rationality, and underlying images of Nature which intimate a sense of powerlessness or social decline. More generally, however, these theories express opposition to the hegemony of certain values associated with modern Science.

MISA, Thomas J.

The Context of Technological Knowledge: "Iron" and "Steel," 1873-1880

Historians of technology have embraced knowledge as a branch of inquiry but typically treat knowledge as an objective commodity. To illustrate a subjective use, this paper analyzes a controversy in the American metallurgical community. The economic crisis following the panic of 1873 precipitated open conflict between advocates of the two leading methods for distinguishing "iron" from the higher-priced "steel." Both methods had technical merit but different commercial implications. Leading metallurgists and steel manufacturers employing low-temperature processes (billet, shear) supported the "chemical" classification, which used percentage of carbon to delimit steel. Other prominent metallurgists and steel manufacturers employing high-temperature processes (Bessemer, crucible) supported the "fusion" classification, which ignored carbon content and classified as steel all samples that were completely fused. By analyzing debates at professional meetings and matching this data with biographies of the principal disputants, I show how this controversy fronted for commercial and professional interests. This paper combined the sociology of science and the history of technology and suggests a common ground between the two disciplines.

MORRILL, Calvin

The Transfer of Scientific Knowledge to Developing Countries: A View from the Laboratory

Concern over the transfer of scientific knowledge and the development of indigenous scientific research in developing countries has spawned an enormous amount of literature from a variety of perspectives (see e.g. Gruber and Marquis, 1969; Segastá, 1973; Stepan, 1976; Erber, 1979; Clark, 1980; Velho and Krieg, 1985). Many of these approaches focus on "human factors" and acknowledge the importance the bearers of ideas have in the transfer of scientific knowledge. This notion moves away from conceptions of knowledge transfer as the dissemination of disembodied ideas or information which can be most effectively distributed through

widely available written sources. Yet, despite the wealth of information generated over the past two decades, we still know relatively little about the transfer process itself, beyond the general idea the dissemination of scientific ideas, among working scientists, is most effectively done via face to face interaction (see Collins, 1975; Ziman, 1975).

This paper uses ethnographic data collected from a laboratory study to suggest the relevance of such investigations for the development of science in underdeveloped countries and the transfer of knowledge between developed and underdeveloped countries.

OEHLER, Kay

FUHRMAN, Ellsworth R.

Discourse Analysis: Problems and Prospects

Discourse analysis is one of the contemporary developments in science studies that seeks to provide an alternative to the Hertonian and related approaches. In this paper, we examine the aims of discourse analysis as proposed by Michael Mulkay and its shortcomings. We suggest that discourse analysts do not accomplish what they claim to achieve and certain obstacles they face may be insuperable. Even with the use of claims about reflexivity, discourse analysts have not developed an argument for the importance of a reflexive sociology for the sociology of science.

PARK, David

Now You See It

Many illustrated books seem to be illustrated mainly in order to give readers a rest from reading. A book about the westward movement in this country, for example, might devote a page to a picture of Indians killing a buffalo, and another to a picture of a train. By contrast, one usually thinks of science as being presented to the public without unnecessary frills and expects not to encounter very much of the kind of illustrative material that conveys no information. In this paper I will examine four topics bearing on scientific illustration.

1. Illustrations that portray famous scientists. Clearly their purpose is not expository. I shall trace the history of this kind of embellishment and venture some guesses as to what it really conveys.
2. The old alchemical works were freely illustrated with images which are at first sight difficult to interpret at all in chemical terms, and yet they seem to have been considered essential parts of the text. I shall investigate non-trivial reasons why so much of the exposition of

these books resides in the illustrations.

3. The first treatise setting forth Newtonian dynamical theory in a way that was (for its day) mathematically rigorous was J.L. Lagrange's Mechanique analytique (1788). In the Preface of this work occurs the proud assertion "On ne trouvera point de figures dans cet ouvrage," and in fact, inspection reveals that there are none. Comparing this work with others on the same subject, I shall explain what Lagrange meant by calling attention to this fact.

4. In the latter part of the nineteenth century physicists were trying to understand the theory of the electromagnetic field. Maxwell's equations had been published in 1865, and we now know they are correct, but for a long time they did not command wide assent and even among their adherents there was some doubt as to what they meant. The French, who in the tradition of Lagrange preferred to work out physical theory as the mathematical consequences of clearly stated axioms, found themselves methodologically and temperamentally at odds with their English counterparts who were glad to cobble together a theory out of assumptions made ad hoc, as long as the phenomena were saved. I shall discuss some of the documents relating to this contrast in styles and make some comments on who seems to have won.

PEACH, H. Gil

Argumentation in Applied Research: Organizational Dances around a Circle of Freedom (A Follow-Up on the Hood River Experiment)

The Hood River Project, a major U.S. residential energy conservation experiment, is a model of "maximum feasible" energy conservation within existing technology as an alternative to the construction of new power plants. It is also an experiment that tests a model of consensus building in applied research.

In this consensus model, a "circle of freedom" was created by an interorganizational consensus process which gave force to observance of science norms, adjusting the special perspectives and "frames of reference" of organizations as an expression of common interest in research credibility. Continuing credibility and the technical quality of a research process subject to continuing oversight from multiple perspectives produced continuing renewal of "rationally motivated consensus" as suggested in Dunn's "jurisprudential" extension of the theory of quasi-experimental design. The model supports Campbell's "critical realist" approach to applied science oriented toward the goal of truth.

The paper begins with implementation of the consensus mechanism as an organizational tool. Restivo's "weak program" in the sociology of science provides the orienting perspective. Within this perspective, Dunn's "jurisprudential" approach is operationalized by means of the "argument maps" of Tolmin's transactional model. The paper then

explores the resilience of the consensus model as results came in. Finally, the promise and reality of the Hood River model of "doing applied science" are considered from the practical perspective of producing "used and useful" results and from the deeper perspectives of Restivo's "weak program" and Campbell's evolutionary epistemology.

PETERS, Lois

Computer Science Conversions and Specializations in the Formation and Growth of Disciplines

Convergence of subject disciplines and of private and public sector organizations have been important to the emergence and growth of computer science. This is in contrast to previous studies which have shown that specialization in a discipline and subsequent differentiation has been most important in the growth of academic disciplines (e.g. experimental psychology arising from psychology). Specialization has also been a factor in the growth of computer science. For example, much computer science research has its base in electrical engineering. Preliminary analysis of the growth of computer science indicates that organizational linkages and multiple sources of information and research were an integral part of its birth and continue to be a part of the development of this field. This indicates that discipline formation is more complex than previous models suggest.

It is the purpose of this paper to describe the continuing processes of convergence and specialization in computer science through analyzing co-authored papers in the current literature. We will examine the degree to which cross-disciplinary and cross-organizational collaboration has contributed to the various subject areas of computer science. It is our hypothesis that collaboration is occurring to a greater extent in some subject areas and not others. A study of this phenomena will provide a greater understanding of the dynamics of conversion and specialization in the growth of a discipline.

Towards this end we have constructed a data base on collaboration in authorship in computer science fields that suggests that both conversion and specialization continues to be important in the growth of this field. For example, there has been increased collaboration in computer science journals publications. In a sample of 341 articles found in three computer journals, singled authored articles represented only 36% of the articles written in 1984 compared to the 1979 level of 50%. This decrease was due to more university authors collaborating in 1984. Co-authored articles increased from the 1979 level of 50% to 64% in 1984. Interdepartment research within universities and inter-university research both contributed to this increase. The most significant increase in joint research was in international inter-university research from a level of 9% in 1979 to 14% in 1984. University-industry cooperative research increased only 1%.

The Library of Congress classification system in a preliminary analysis

RABKIN, Yakov M.

Emergence of Scientific Cultures in Developing Societies

Scholarly literature on science outside the older industrialized nations has steadily expanded during the recent two or three decades. This paper is an attempt to survey this literature as it relates to questions of transfer of Western scientific cultures to, and their local transformation by, developing societies. Particular emphasis is put on potential uses of these secondary sources for comparative analyses of the emergence of scientific cultures in non-Western and peripheral Western societies. Belonging to the genre of the state of the field review and to that of methodological critique, this paper is conceived as an introduction to comparative studies of peripheral scientific systems. Problems of cultural acceptability of science, issues relating to explicit and implicit roles of science in developing societies constitute the main foci of this survey which draws on literature published in the field of science studies in major European and several other languages.

REED, John H.
WILKES, John M.

Public Knowledge and Interpretation of Nuclear Power Before and After TMI

The implications of Three Mile Island for public knowledge and opinion are best understood in light of the pre-existing situation. In a sense TMI represents a perturbation in an established trend of changing attitudes about nuclear power. It occurred just prior to the passage of support for the technology from majority to minority status in the general public, but it is not clear that the event is therefore the cause of the realignment in opinion. It is equally possible that the eroding support for nuclear power in the face of an emerging mass movement set the stage for a routine event with the right symbolic potential to become a cause celebre and provoke an official response that ratified the opposition's interpretation of the issues raised by nuclear power.

In this paper the crucial events of 1975-76, several years before the TMI incident in 1979 are reviewed to set the stage for the incident in terms of the unfolding social processes that produced 1) technically trained dissidents in the nuclear establishment, 2) legal challenges, 3) electoral challenges, 4) a significant defection of potential public opinion leaders and 5) a vulnerable industry-regulatory environment prone to accidents.

The interpretations of nuclear power and ultimately the TMI incident from the pro- and anti- nuclear perspectives are also discussed as is the interesting timing of the key drops in public support. All this is placed in the context of what the public seems to have known about the technology in question at various crucial stages such as the time of the referenda, the eve of TMI and the one year anniversary of the well-known accident.

also indicates both convergence and specialization. We tested this hypothesis by examining edited volumes for the degree of cross disciplinary and cross organizational collaboration represented in multiple co-authored articles and the growth of subject specialization as shown by the proliferation of such headings and titles falling in the category of computer science.

We believe our data on research collaboration illustrates the importance of cross disciplinary and cross organizational interactions in the evolution of this discipline.

PICKERING, Andy

Constructing Consensus: World Views, Institutional Structures and Policy-Making in Recent High-Energy Physics

This paper explores the interplay between conceptual, social, institutional and technological developments in high-energy physics (HEP).

The latter half of the 1970s saw the establishment within HEP of the 'new orthodoxy': the communal acceptance of the so-called 'new physics' world view. Within the new physics, the map of interesting and significant natural phenomena was redrawn: certain very rare phenomena came to be seen as uniquely informative, while more common phenomena which had been at the centre of the 'old physics' were seen as complex and uninformative. Thus an integral part of the establishment of the new orthodoxy was a transformation of experimental practice in HEP, a transformation involving the deployment of novel and very sophisticated techniques to explore the rare phenomena of interest, and involving also an acceleration of the existing trend to larger and larger collaborations of experiments. In parallel with this transformation of contemporary practice in HEP, the late 1970s saw a transformation of the future of the field. The new experimental facilities conceived for the 1970s and 1980s were all directed towards the exploration of the rare phenomena central to the new physics world view.

It can be argued that the establishment of the new orthodoxy, with its concomitant transformation of experimental practice and technological embodiment in the future of HEP, cannot be understood fully in terms of the dynamics of research practice at the individual level. Instead, the new orthodoxy was enforced and reproduced via the institutional structures of HEP - the review and policy-making bodies which control access to experimental facilities and which determine what new facilities should be constructed.

This paper examines the multidimensional nexus of developments surrounding the new orthodoxy in HEP, with particular emphasis upon the role of institutional structures in the establishment of the orthodoxy and in its propagation into the future.

excellent data on the trend of opinion is available from the Harris Associates, but the crucial data on the knowledge issue and impact of the THI event are drawn from representative studies of 1,000 Massachusetts residents and selected student populations that bracket the event. These data added to the Harris time series and other selected surveys make it clear that the significance of THI was not the event itself or its contribution to public knowledge about the technology and the debate that surrounded it. The significant impact was that it provoked an official review that shook public confidence in those perceived to be responsible for the development of the technology and the public safety. There are several other ways that this sort of crisis could have been precipitated, but THI was a particularly good vehicle from the standpoint of popularizing the opposition ideology and its implicit interpretation of nuclear power and the issues it raises.

RIP, Arie

The Mutual Dependence of Risk Research and Political Context

Building on rather diverse roots, the field of risk assessment, or risk research more generally, has grown rapidly (in size and in content) since the late 1960s, in response to the increasing importance of public decision-making about risks/risky activities/siting of plants, etc. Decision-making requires anticipation and assessment of potential risks, and risk assessment is designed to answer this need. Risk analysis, in particular, offers the hope of comprehensive and quantitative information on risk that will rationalize decision making.

Risk assessment is not without its controversies: its pretensions have been criticized by opponents of nuclear energy, its lack of reliable statistics to build upon, as well as other sources of inaccuracies have been deplored by professional risk analysts (who, at the same time, argue that risk analysis is still the best we have got to bring more rationality into decision making), while more recently psychologists, sociologists and political scientists have added further dimensions to risk research (risk perception, judgement under uncertainty, decision theory, institutional processing of uncertainty, rituals and cultural bias, etc.). A less visible controversy is the one between professional risk analysts and the earlier safety and reliability engineers, a controversy that is related to the criticism of industry (who want safety) about government decision making (where government officials as well as risk analysts want rationalization of decision making).

Recent developments are the requirement to do some risk assessment before embarking on regulation or other measures (but sometimes complemented by cost benefit analysis) - as in US Congress -, and government initiatives to base their standards and regulations on risk analysis (partly computerized) - as in the Netherlands. An emerging socio-political response is the curious alliance of industry and of spokesmen for the public, who join in criticizing risk analysis for its limitations and inaccuracies. This prods risk analysts into a defensive stance, and

into improving risk analysis.

The interrelations between research and political context are very visible in this case. They can be traced in other cases (e.g. biotechnology, medical research), and are, in fact, a general feature of scientific development.

ROSE, Mark H.

Technology in its Urban Setting: Electric and Gas Systems in Kansas City and Denver, 1880-1940

Gas and electric service—as systems chiefly—are of course embedded in social contexts. Up to 1940, those contexts were principally local and urban, and revolved around processes such as outmigration, the formation of neighborhoods, and even the relationship between wives and husbands. The net result of this interaction was a clustering on the urban landscape of technological knowledge, technological systems, and a receptive culture, an ecology of technology.

SEGERSTRALE, Ullica

The Sociobiology Debate Revisited

In this paper I argue that the sociobiology debate is not simply a politically motivated "nature-nurture" controversy between conservative hereditarians and progressive environmentalists, nor is the behavior of the participants radically different from "normal" scientific behavior. While it is in the interests of the debaters to sustain such an impression, a closer examination shows that the issues under dispute are primarily of a broader meta-scientific nature, dealing with both epistemological and ethical problems of contemporary science. I also show that "nature-nurture" controversies of the sociobiology and IQ kind far from being abnormalities in reality illustrate various aspects of standard practice in science. What is happening in these controversies is that inherent problems of science are expressed in a transformed way as scientific and moral "errors" of individual scientists. This, in turn, is due to an interaction between the cognitive and strategic concerns of the actors involved. Finally, I argue that "nature-nurture" controversies play an important role in science as tacit forums for meta-scientific discussion.

My focus in this paper is especially on the sociobiology controversy, starting a decade ago around the publication of Wilson's *Sociobiology*, but my analysis is relevant also to the earlier IQ controversy, from which sociobiology inherited much of its criticism (and many of its critics). I base my conclusions on an examination of written material as well as extensive interviews with participants, observers and "referees"

in this debate (see Segerstråle, 1983, 1985).

From a cognitive point of view, one can explain recent "nature-nurture" debates as follows: The subject matter of sociobiology and IQ research is of such a nature that it brings scientists from different scientific and moral/political traditions together in the same controversy. This means that these scientists come to interpret the same text in different ways, because of their different conceptions of what constitutes "good science" as well as their different views of the proper social role for a contemporary scientist. In this respect one can describe the sociobiology debate as a clash between "critical universalists" and "communicative naturalists", where the former more reductionistically oriented ones consider the scientific interests, plausibility arguments, and various standard practices of evolutionary biologists as scientifically suspect (cf. Bazerman, 1981). In turn, beliefs and practices at variance with a scientist's own conception of "good science" are explained as "extra-scientific" motivated "errors" (cf. Gilbert and Mulkay, 1982). At the same time, these two camps tend to have different conceptions of their own duty as scientists: they are either "weeder" or "planters" in regard to tentative knowledge in fields with potential sociopolitical implications.

Here a basic opposition in views concerning the connection between scientific research and its social consequences is of central importance. While for traditional scientists like Wilson the content of scientific knowledge is decoupled from its possible social abuse, the radical critics of sociobiology see scientific error as both ideologically motivated and as producing knowledge that inevitably will be abused. This leads the critics to hold scientists like Wilson morally responsible for scientific "error", which in practice often boils down to voicing "incorrect" scientific belief.

In the sociobiology and IQ controversies, these basic cognitive differences are now exploited for strategical reasons. In these debates, the scientists' routine quest for recognition characterized by Bourdieu (1973) as a competitive struggle for "symbolic capital" has now been expanded to include the moral realm as well. In this realm, the critics have a certain advantage because of the current sociohistorical feasibility in the U.S. to obtain moral recognition for unmasking "racist" or "biological determinist" scientists. In fact, the critics often undertake a "moral reading" of the target of their criticism, the purpose of which is to demonstrate how scientific error leads to conclusions with maximally undesirable social implications, thus providing maximal moral recognition for the revealer of this fact. In this endeavor, the critics can be shown to pursue "optimization strategies", by which both scientific and moral recognition can simultaneously be obtained from scientific criticism of carefully selected targets. One of the major strategies for criticism used by the opponents of sociobiology is the radicalization of plausibility arguments into strong, easily dismissible claims. (This does not prevent the critics themselves from employing plausibility arguments when it suits them).

Meanwhile, the defenders of sociobiology and IQ research can reap moral

benefits among those who have a traditional view of science, through upholding an image of unproblematic objective science against the threat by left-wing ideologues. In this way they can dismiss also serious scientific criticism by the critics as ideologically motivated and sweep also clear errors under the rug.

The continuous possibility of accumulating new moral capital can now explain why it is in both parties' interest to keep the controversies going. The limiting factor for the critics is the supply of new "biological determinists" who can be subjected to their critical routine, while the potential targets will be able to draw on support from traditional elements within the scientific community and the larger society. Because of the public nature of these recent "nature-nurture" debates and the conduct of much of the controversy in popular books and book reviews, the general public in this way gets involved in granting moral recognition to different co-existing scientific claims.

What is taking place, then, is an indirect negotiation of what is to be counted as "good science", involving both the scientific and the lay community. It is ironical that while the critics of sociobiology and IQ research are in principle concerned with such problems as bias in science and carelessness in scientific research, in practice their strong socialization to the competitive struggle and to profit maximization in the realm of symbolic capital induces them to attack individual scientists, thus morally short-circuiting their meta-scientific ambitions.

But "nature-nurture" controversies, despite the fact that meta-scientific issues are not overtly debated, may be crucial to the change of the nature of science over time. These debates are largely conducted through the selective use of plausibility arguments. Such arguments are an important part of scientific discourse in general, but especially central to fields where the data are mainly tentative - often coinciding with research dealing with humans. At the same time, the "reasonableness" of the claims involved necessarily refer to the prevailing (divergent) scientific and social beliefs at any particular time, as well as to views about the proper relationship of science to moral and social concerns. Plausibility arguments in "soft" scientific fields may thus be an important means through which the nature of science is being renegotiated over time, i.e. a mechanism for scientific boundary change.

By bringing various scientific practices to the surface, the sociobiology and IQ controversies demonstrate how scientists for both cognitive and strategical reasons tend to routinely accept or reject plausibility arguments as if these were based on "hard" evidence. This "precipitation" of plausibility arguments makes the actual boundary negotiation invisible. In addition, it obscures the fact that much of scientific knowledge in general is of a tentative nature. In this way, while they are indirectly negotiating the nature of science, both sides in these recent controversies are reinforcing the popular view that ("good") science provides firm knowledge of vital importance to social and ethical decisions, i.e. they are together upholding the social authority of science.

STEWART, John A.

Drifting Continents and Colliding Interests: A Quantitative Application of the Interests Perspective

A quantitative analysis of geoscientists' published opinions on continental drift theory between 1910 and 1950 indicates that more prominent scientists resisted this revolutionary theory. Was this resistance based on the greater knowledge of the productive geoscientists or were they protecting their reputations? The latter interpretation gains plausibility because the current acceptance of plate tectonics implies that the previous evidence given for drift should have established it as a plausible theory. The analyses and discussion in this paper illustrate (a) how quantitative evidence can be related to the 'interests' perspective, (b) the importance of assumptions in distinguishing 'social' and 'scientific' interests, and (c) some of the elements in the 'strong programme' in the sociology of scientific knowledge.

SUCHMAN, Lucy

Action and Interaction in Cognitive Science

Researchers in Cognitive Science view the organization and significance of action as derived from plans, which are prerequisite to and prescribe action at whatever level of detail one might imagine. Mutual intelligibility on this view is a matter of the recognizability of plans, due to common conventions for the expression of intent, and common knowledge about typical situations and appropriate actions. As common sense formulations designed to accommodate the unforeseeable contingencies of situated action, however, plans are inherently vague. Researchers interested in machine intelligence attempt to remedy the vagueness of plans, to make them the basis for artifacts intended to embody intelligent behavior, including the ability to interact with their human users. This paper examines the assumptions about action and interaction that underlie Cognitive Science, based on a case study of a machine designed on the planning model, and intended to be intelligent and interactive. A conversation analysis of "interactions" between users and the machine reveals that the machine's insensitivity to particular circumstances is both a central design resource, and a fundamental limitation.

TEICH, Albert H.

Education in Science, Engineering, and Public Policy

Under the auspices of the AAAS Committee on Science, Engineering and Public Policy, my office has been conducting a study of education in the

field of science, engineering and public policy.

The study is focused on policy-oriented academic programs (primarily at the graduate level) that seek to prepare students for careers in this area, rather than those that simply provide enrichment for students in other fields. We have identified more than twenty such programs, and collected data on them through questionnaires sent to the program heads. In addition, we are completing a questionnaire survey of the graduates and former students of these programs -- some 1400 in all -- and will soon be interviewing employers of science policy professionals and individuals who have entered the field without formal training in it.

The first product of this study is a GUIDE TO EDUCATION IN SCIENCE, ENGINEERING AND PUBLIC POLICY published recently by AAAS. The ultimate product will be an analytic report discussing the state of the field and suggesting means by which the academic programs might better serve the needs of professional practice.

TOREN, Nina

Numbers, Social Characteristics, and Inequality: Women in Academia

This study examines the effects of minority size on the academic position of women in higher education in Israel. Findings from faculty women show that their proportional presentation is negatively related to their achievement in terms of academic rank; the smaller their proportion in a scientific discipline the more does their hierarchical distribution resemble that of their male colleagues. It is also found that women, as a rule, participate in larger proportions in the humanities than in the natural sciences. It is suggested that sex ratios affect women's position in combination with the stereotypes attributed to the feminine diffuse status-characteristic in different contexts. In scientific fields in which women's sex status is more salient they fare less well than in disciplines in which it is neutralized.

VERCRAGT, Philip
BIJLSMA, Jan

The Social Construction of Industrial Innovations

In the last few years considerable attention has been paid to the social construction of scientific knowledge. The authors have shown that scientific knowledge not only reflects 'laws of nature', but that the content of scientific knowledge is to a high degree contingent, dependant on social processes of validation, competition etc. within and without scientific laboratories.

It is quite interesting to apply these findings to the innovation process

in industrial laboratories. Industrial laboratories can be considered as examples of technical systems, as they are characterized by 'cognitive complexity', formal organisation, sectorial diversity and occupational pluralism'. The difference between the production of 'academic' scientific knowledge and the innovation process in industrial laboratories is, of course, that the former consists of immaterial knowledge claims, to be expressed by written accounts, while the products of innovation processes mostly consist of material artefacts, which are sold on the market. Is it possible to analyse a material artefact as a social construction?

Outside the industrial lab, Bijker and Pinch have shown that the social construction of an artefact can be analysed as a process in which social groups, with their problems, give rise to solutions that are in turn stabilized to become new artefacts. But how the transformation of some public demand into the solution of a research problem takes place within the context of the highly complex highly organised modern corporate research laboratory? Dosi also has tackled this problem. In his analysis he considers the innovation process as a part of a 'technological trajectories' on which the 'selection environment' exerts influence. Technological trajectories are constructed within 'technological paradigms' which can somehow be compared to Kuhn's scientific paradigms. However, the exact nature of the interactions between the R&D-lab and the (selection) environment remains unclear.

It is important, however, to look more closely at the social process of innovation. The social studies of science cannot stop at the door of the industrial laboratory, where science intermingles with production and economics. Also, the understanding of industrial innovation as a social process has to be studied for social reasons. In society, a growing demand comes into existence to discuss and to influence innovative processes into directions wanted by social groups and by governments. Now, a new product or process can mostly only be judged as it leaves the doors of the production plant: as a consequence, controversies have arisen over many issues, like environmental problems, recombinant DNA and nuclear energy. In early stages of the innovative process, judgement by potential users and others groups of consequences of technological innovations has been sparse, and has shown to be nearly impossible. The only fields where the government has put influence on innovative processes are product safety issues (for instance in the pharmaceutical industry) and worker's health and safety.

In the Netherlands, the government also has developed a policy to influence technological developments by a new type of technology assessment. In this TA, wishes and problems expressed by social groups have to be transformed into research intentionally designed to meet their requests. Because of the low budget as compared to the high one of stimulation of new technologies (biotechnology, micro-electronics etc.) the effect of the new policy is open to doubt.

Also, trade union organisations show a growing interest in the innovative processes of large corporations. Several attempts have been made, either to influence corporate policy with respect to research and development decisions, or to formulate own plans and priorities. These activities

suffer from lack of expertise and problems of support from the trade-union members, because these R&D-activities are far away from normal bargaining activities.

Thus, a growing interest and concern exists about industrial innovation processes and the (im)possibilities to influence those by the aims and interests of social groups and governments. But the nature of those innovative processes remains largely unknown. Of course, much is known about the management of industrial R&D, in terms of organisational structures, money allocation factors that determine success and failure of projects. But most of these studies have been written more or less from the viewpoint of the corporation and its management: the question for them is how to maximize the output of R&D in terms of patents, applicable knowledge and marketable products.

The approach we shall adopt here is the result of studying some industrial innovative processes, the aims of the participants, the decisions that have been taken. Our aim was to construct a framework which enables us to study these processes and especially the factors determining the outcome of decision making processes about direction and aim of industrial innovation processes.

SOCIETY NEWS

Secretary Scott Long Announces Council Members' Terms

The following communication has been received from Secretary Scott Long. It is printed here to maximize this information to the membership.

The membership of the 4S Council has been adjusted to make it conform with the guidelines of the Society. Members of the Council are:

With terms expiring in 1985:

**Harry Collins
Sal Restivo
Ron Westrum**

With terms expiring in 1986:

**Marcel La Follette
David L. Hull
Marc DeMay**

With terms expiring in 1987:

**Bruno Latour
Ruth Cowan
Steven Shapin**

Secretary Long also noted that he, as Secretary, and Tom Gieryn, as Treasurer, have finally been able to replace Lowell Hargens who held the position of Secretary/Treasurer for many years. "It is a clear indication of his contribution to the Society that it took two of us to replace him."

ANNOUNCEMENTS

American Institute of Physics

"MOMENTS OF DISCOVERY" AUDIOVISUAL TEACHING PACKAGE AVAILABLE

Teachers of physics or astronomy and of history or sociology of science will welcome a new audio-visual package designed to show the human dimensions of scientific discovery. Issued by the Center for History of Physics of the American Institute of Physics, the package contains two units: "The Discovery of Nuclear Fission" and "An Optical Pulsar Discovery." The first unit includes audio recordings of the actual voices of Bohr, Rutherford, J.J. Thomson, Fermi, Einstein, Szilard, and others, woven into a 36-minute narrated account of the discovery of nuclear fission. The second unit includes what may be the only live recording of a moment of discovery as it was taking place; two young scientists inadvertently tape-recorded their voices on the night they found the first optical pulsar. Fascinating excerpts are included in a 44-minute cassette along with interviews of the scientists and narration by Philip Morrison.

Both units are accompanied by extensive Teachers' Guides with reprinted readings and documents, suggestions for use inside and outside the classroom, and student exercises. There are also scripts with the complete text of the cassettes, illustrated with photographs and diagrams, and a set of 20 photographs with pictures of the physicists. Aimed at students first encountering a serious course dealing with science, whether in high school or college, the units will be found useful for bringing alive a variety of concepts both scientific and humanistic.

"Moments of Discovery" was developed with funding from the National Science Foundation, the Heineman Foundation, and the Friends of the AIP Center for History of Physics, and is therefore available at a subsidized cost. It is sold as a package (binder containing two cassettes, two Teachers' Guides, two illustrated scripts, and slides) for \$85.00 including postage and handling; add \$2.50 billing charge if the order is not prepaid. Address orders or inquiries to Audiovisual 5, Center for History of Physics, American Institute of Physics, 335 East 45th St., New York, NY 10017.

THE CHARLES BABBAGE INSTITUTE

The Center for the History of Information Processing

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

PREDOCTORAL FELLOWSHIP 1986-87

The Charles Babbage Institute is accepting applications for a Graduate Fellowship to be awarded for the 1986-87 academic year to a graduate student whose dissertation will address some aspect of the history of computers and information processing. Thesis topics may be chosen from, but are not limited to, the infrastructure of the information processing industry, and specific technological developments in the information sciences, including both hardware and software. Proposals which deal with the economic and organizational milieu of these developments, or with the economic, legal or social history of computing are especially encouraged.

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

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

Applicants for the Predoctoral Fellowship and the Professional Internship should send their materials to The Charles Babbage Institute, University of Minnesota, 104 Walter Library, 117 Pleasant Street S.E., Minneapolis, MN 55455, U.S.A. by January 15, 1986. No special application forms are required. Dependent upon funding one or both of these awards may be made.

PROFESSIONAL INTERNSHIP 1986-87

The Charles Babbage Institute is accepting applications for a Professional Internship to be awarded for a period of three to nine months between June 1, 1986 and May 31, 1987. The Internship is available to professional staff interested in an introduction to the history of information processing. Appropriate applicants might include, but are not limited to, historians and social scientists interested in the history of information processing and its infrastructure, academics interested in preparing new courses in this history, or records managers and archivists interested in related archival problems.

Residence is required at the Babbage Institute, on the University of Minnesota campus. Interns are required to conduct a research project under the direction of the Institute staff. Routine office and clerical support services are provided.

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

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

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