

4S REVIEW

Fall 1983

ISSN: 0738-0526

Volume 1 Number 3

*** * * Special Program Issue * * ***

Journal of the
Society for Social Studies of Science

Society for Social Studies of Science

OFFICERS

- President:** Arnold Thackray, University of Pennsylvania
- Secretary-Treasurer:** Lowell Hargens, Indiana University
- Council:** David Edge, University of Edinburgh (UK)
Thomas Gieryn, Indiana University
Rae Goodell, Massachusetts Institute of Technology
John Holmfeld, Library of Congress
Karin Knorr, Wesleyan University
Linda Lubrano, American University
Spencer Weart, American Institute of Physics
John Ziman, Imperial College of Science and Technology (UK)
Bernard Barber, ex-officio, Columbia University

4S REVIEW

- Editor:** Jerry Gaston, Texas A&M University
- Associate Editors:** Lawrence Stern, (Book Reviews), College of the Holy Cross
Steve Woolgar, (News), Brunel University (United Kingdom)
Terry Shinn, (News), University of Paris-Sorbonne (France)
David Miller, (News), University of New South Wales (Australia)
Thomas Gieryn, (Bibliography and Literature), Indiana University
- Editorial Advisors:** Bernard Barber, Columbia University
Donald Campbell, Lehigh University
Harry Collins, Bath University (UK)
Arthur Donovan, Virginia Polytechnic Institute and State University
Karin Knorr, Wesleyan University
Sally Gregory Kohlstedt, Syracuse University
Nicholas Mullins, Indiana University
Dorothy Nelkin, Cornell University
Thomas Nickles, University of Nevada - Reno
Derek J. de Solla Price, Yale University
Brigitte Schroeder-Gudenus, University of Montreal (Canada)
Henry Small, Institute for Scientific Information
Peter Weingart, University of Bielefeld (West Germany)
John Ziman, Imperial College of Science and Technology (UK)
Harriet Zuckerman, Columbia University

4S REVIEW

Fall 1983

Volume 1 Number 3

Contents

Article

- Cognition and Social Construction in Laboratory Science
--Marc Grenier 2

Book Reviews

- William Broad and Nicholas Wade's *Betrayers of the Truth: Fraud and Deceit in the Halls of Science*
--Reviewed by Henry H. Bauer 17

- K. Hufbauer's *The Formation of the German Chemical Community (1720-1795)* and Robert E. Kohler's *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline*
--Reviewed by David Knight 24

- G. C. Smith's *The Boole-DeMorgan Correspondence 1842-1864*
--Reviewed by J. L. Richards 28

- Timothy Lenoir's *The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology*
--Reviewed by Frederick B. Churchill 29

- John Hubbel Weiss' *The Making of Technological Man: The Social Origins of French Engineering Education*
--Reviewed by Lewis Pyenson 31

Society News

- Complete Program for 8th Annual Meetings--Insert After Page---33
4S Meetings Exhibitor Requests Authors' Assistance with
Identifying Books for Exhibit--See Program Page 12

Announcements

- Federal Agencies 34
Recent Conferences/Symposia 34
Future Conferences/Symposia 43
AAAS Openness in Scientific Communication Project 44
Positions Available 45
New Appointments 48
Erratum 48
Death Notice Received 48

COGNITION AND SOCIAL CONSTRUCTION IN LABORATORY SCIENCE

Marc Grenier
McGill University

This article is an initial statement from a continuing research project (1) on a question in current ethnographies of science (2), namely, "Is there an independent causal role of cognition in scientific practice?" Although this question would not seem meaningful in terms of past approaches to the study of science (3), which saw a determining role for evidence and logic in valid scientific work, it is highly relevant to the development of social constructivist sociology of science.

The social construction movement in the sociology of science can be differentiated into at least two types, the materialist (4) and the literary. The materialist constructivist contends that scientific facts are in the last analysis reducible to economic and material interests and processes, whereas the literary constructivist asserts that scientific facts are essentially the product of discourse. Both of these constructivist approaches can be found in the recent ethnographies of science studies. Although it is agreed here that scientific facts may be to some extent the result of such processes or language, it is argued that the constructivist approach does not yet account for or try to trace the impact of perceived physical objects upon the development of scientists' cognitive activity. Scientific facts are not simply the result of cognition seen either as the product of the arrangement of words or the manipulation of economic and physical resources. The cognition of scientists is not confined by such resources nor by the vehicle of language.

In adapting the constructivist approach to the study of scientific inquiry, two recent participant-observer studies of science to varying degrees do underplay and, in some places, deny an independent causal role for cognition in science. The first part of this paper will attempt to provide evidence of this denial. The second part of the paper will assert a positive role for cognition in the process of scientific inquiry by providing empirical examples of two ways in which it played a crucial role in the daily activities of the scientists I studied. First, scientists used "background assumptions," presuppositions which give meaning to a focal statement that a relationship between perceived physical phenomena is true. Second, they used "hypotheses," or focal, clear statements which are translatable into physical terms. A brief discussion of the implications of our findings for the new interpretative sociologies of science is provided in the concluding section.

Part I

Based on a participant-observer study in 1976-1977 in a laboratory investigating plant proteins, Karin Knorr comes to deny that there may be a distinct or independent cognitive dimension in scientific inquiry. Instead, she claims that nowhere in the laboratory "do we seem to find the worlds of ideas we are looking for, nor the cognitive objects and interests generally identified with research" (5). Knorr's observations lead her to assert that the hypothesis-testing

Acknowledgement: I am greatly indebted to Roger Krohn for his suggestive key comments and criticisms in the preparation of this paper.

model typically attributed to scientific activity by philosophers and methods textbooks on science, which suggested an important independent role for cognition in scientific inquiry, should be replaced by a model of "successful tinkering" (6). By successful tinkering Knorr means the deliberate utilization of perceived solutions, assets, and other "opportunities for success" in a relentless effort to "make things work." These opportunities are exploited by scientists in order to acquire symbolic "capital", which refers to the expected future returns that result from having one's knowledge claims function "like a commodity of exchange which is constantly re-invested" (7).

The underlying structure of science, then, can be thought of as a material, competitive struggle to obtain scientific "capital." Although Knorr admits that opportunities for success are subject to local interpretation, that is each laboratory or work site develops its own preferred methods of "making things work," these local work preferences are dependent upon "resources and interests" (8). Interactions between scientists are deemed important in determining how resources and interests are employed, but these interactions are oriented by the practical struggle for scientific "capital" (9). Thus Knorr's model of scientific inquiry focuses on science as fundamentally a physical activity directed by the prospects for gain, not as a genuine, independent cognitive operation in problem solving.

Knorr's economic conception of science is made clear in an earlier paper (10), again using it to refute the traditional claim that science is predominantly or most significantly a cognitive activity. In that paper, scientists are agents who continually invest symbolic capital in a particular field of investment. Significantly, scientific change occurs as the "agents" of a laboratory are successful in their investments.

In a subsequent paper (11), Knorr asserts that scientists do not apply cognitive rules so to transform the research process into a written presentation. Rather, they produce inscriptions or measurement traces which they selectively utilize to construct knowledge claims. Thus it is concluded that scientific "facts" are materially bound. The writing in the scientific paper should simply be considered as providing a "con-texture" (12) for measurement traces. Throughout the scientific paper, this con-texture is claimed to be "a field of demands or a market" (13). Not surprisingly, we find that this market is that within which scientists continually struggle with and sometimes against each other to obtain symbolic capital. The struggle is over the acquisition of this capital in the market which confers value and success to the results contained in the scientific paper.

In yet another paper (14), Knorr concludes that the relationship between analogical reasoning and the extension of knowledge in science is not accounted for by conceptual interaction, again downplaying the role of independent cognition in scientific inquiry. Rather, analogical reasoning by scientists can be more appropriately understood as resulting "from a process of production and reproduction" (15). Here Knorr more explicitly makes the role of ideas dependent upon the material conditions surrounding scientific research. In fact, it is asserted that directing a discussion of analogical reasoning in science to a cognitive level of analysis is undesirable and, indeed, misleading. Conceptual interaction results from the mobilization of the "means of production" (16) of science, not from possible independent cognitive processes.

The Latour and Woolgar participant-observer study of an endocrinology laboratory in part diverges from Knorr's emphasis upon the material dimension of scientific activity described above by engaging in literary analysis (17). In the first few chapters, Latour and Woolgar explored how the statements made about nature by scientists came to be transformed into independent "facts," suggesting the crucial importance of the cognitive practices of scientists in the process of inquiry. More specifically, Latour and Woolgar attempted to demonstrate how one fact - the Thyrotropin Releasing Factor (TRF) - began as a conjectural statement and then proceeded to assume a series of other statuses as scientists requested more evidence (to detach "modalities" from their statements) concerning its nature. In the end, the statement about the structure of TRF and the supposed physical entity were considered isomorphic by the scientists (18). In this manner, scientists were able to portray TRF as having been 'out there' all along, rather than the product of their artful accomplishments. On the basis of this finding, Latour and Woolgar concluded that "facts" are not the product of "hypothesis, proof, and deduction" (19), nor are they the result of economic or material interests, as for Karin Knorr, but now they are reified symbols or the product of discourse.

In Chapter Five of the Latour and Woolgar study, however, the language of symbolic or literary construction evident in the first part shifts to an economic language similar to Knorr's. The laboratory becomes the "factory", the scientists become "producers," facts become "commodities," and equipment, data, and other types of resources used by scientists become "capital." Scientists become preoccupied with investing and re-investing available resources in order to enhance their credibility for doing science, acquired when other scientists use the proffered information. From this perspective, scientists are perceived as being coy "strategists" (20) whose activity is oriented by an interest in speeding up the conversion of available resources into credibility. Although Knorr's symbolic capital differs in some respects from Latour and Woolgar's credibility, it is clear both of these ethnographic studies of science underscore the material conditions and the expected gains which are believed to motivate and orient scientific activity.

Drawing predominant attention to the material or literary dimension of scientific activity possesses two interrelated shortcomings. Firstly, denying any role to formal rationality also denies any genuinely cognitive dimension to scientific inquiry. Secondly, it neglects to address the nature of the interaction between the symbolic moves of scientists and the perceived physical objects of their investigations. In the next part of this paper, an attempt will be made empirically to demonstrate two ways in which independent cognition is crucial to an understanding of scientific inquiry.

Part II

A. Assumptions

The first way in which cognition plays an important part in scientific inquiry is in scientists' use of what I call "background assumptions," that is presuppositions that give meaning to a focal statement that a relationship between perceived physical phenomena is true. Background assumptions are mental moves as opposed to behavioral or communicative moves, and they influence scientific behavior. The assertion here will be that in order to initiate investigations of a problem, scientists are often compelled to assign certainty to perceived

unstable aspects of the problem they are investigating. For the practical purposes of beginning, maintaining, and legitimating a scientific inquiry, scientists recognize that some things just have to be taken for granted. It is this awareness of the taken-for-granted character of much scientific activity which past studies of science fail adequately to describe. Here I will illustrate the above points with a small number of representative examples from the laboratory I studied.

From my investigations, laboratory work appears to embrace and incorporate different kinds of assumptions, any one of which may or may not be questioned in later inquiries. Although my original study found at least five different types of assumptions, we can focus here on two only. The first type of assumptions are those taken for granted in limnology, a generic component of what I call "field assumptions."

One field assumption employed in the laboratory I studied was that radioactive gas exudes from the earth at a constant rate. Despite its perceived inadequacy and the paucity of the evidence upon which this assumption is based, it is nevertheless an important part of their work and findings. For example, in the analysis of the content of lake sediments one of the chemicals of interest is Polonium-210. Polonium-210 is employed as an indicator of the age of sediments, which in turn allows the demarcation of anthropogenic (man-made) from natural lead in the sediments of lakes. Polonium-210 originates from the decay of radon in the atmosphere. Radon derives from radium, the gas which the earth is believed to emit at a constant rate into the atmosphere where it is converted to Polonium-210. The Polonium-210 is rained out of the atmosphere and deposited in the sediments in lakes where it has been reasonably shown to be immobile. Thus the rate of decay of this chemical allows scientists to calculate the rate at which the sediments have accumulated and hence derive their age.

In the above description, not only is there an assumption that radium is exuded from the earth at a constant rate but also that Polonium-210 falls from the atmosphere uniformly, which in turn permits the accurate estimation of the age strata of lake sediments. From the following discussion, we see that the assumed uniform fall rate is taken to stand upon shaky ground. The talk stems from a taped formal lecture by the chief of the laboratory to a group of multidisciplinary peers about the current progress of a five-year scientific project to test for the limitation of organic growth by the lack of phosphorus in Northern Canadian lakes which he designed and initiated. After the lecture, a participant questioned whether it was plausible to assume that once in the atmosphere radon is spread uniformly so that when it decays in the form of Polonium-210 over lakes it is distributed uniformly in the sediments.

Participant: What sort of effect would you expect from the patchiness of radon itself, where you get large accumulations of other materials below the ground and yet this gets moved nowhere? It's known not to be uniform.

Chief: What I would expect from the patchiness produced on radium, I would expect the atmosphere, the residence time of radon in the atmosphere, would eliminate that patchiness considerably.

Participant: What is the weather? I do not know what it is in the daytime.

Chief: You have got that, three or four days or integrated over a couple of half lives, say, six days. The atmosphere can disperse radon a long way in that time. The mechanism, we think, is that the radium in the soil, and wherever you got porous soil that the gas can escape from, the radon diffuses up into the atmosphere. It is blown by the winds and as it is converted into lead it is rained out of the atmosphere.

Participant: By the time it is rained out, it is pretty well dispersed?

Chief: It is pretty well dispersed. But, as yet, there are no data on the geographical distribution of Lead-210 from fall-out.

Here the professor admits the lack of empirical confirmation to support the uniform fall rate assumption of Polonium-210, one critical to the legitimacy of experimental results purporting to have established the age of particular lake sediments. If radon were to accumulate for an unknown reason in one particular section of the atmosphere, decay into Polonium-210, decay into Polonium-210, and be rained out of the atmosphere, then it would not accumulate uniformly in the sediments of lakes. The lack of data about the distribution of radon, however, has not precluded the investigation of, and claim-making about, the age of lake sediments. For the practical purposes of getting on with scientific inquiry, it is apparent that some assumptions must be tentatively permitted, even without empirical support, even at the risk of the interpretability of the results.

The second type of assumption I will discuss are called "experiment assumptions," since they serve particular types of investigations rather than act as general guides to limnological work. The following fragment of a taped discussion with a laboratory member engaged in a self-designed experiment will suffice to illustrate the point.

So we only use explanations to lead us to other hypotheses and usually, as you can see, for each little event that happens...you make assumptions...Then you discover that an assumption isn't correct. Like, for example, my oxygen meter...part of the time I had it plugged in the wall, part of the time running on batteries. I assumed that it was giving the same reading. I discovered last night before I went home that, in fact, there was a different reading depending on whether it was plugged or not. So my assumption is now shot to hell. So now I've got to do little experiments on it to see what the pattern is, if it's a constant difference...

In the above quote, it is evident that the laboratory member made a number of assumptions in the course of his experiment, including the assumption of the equivalent values in different types of meter readings. Note that the initial "experiment" or working assumption - that a reading from an oxygen meter did not vary with its power source - was suddenly "discovered" to be erroneous. It is evident that the laboratory member was predisposed to overlook certain aspects of his experiment. This example shows the unique work-stopping capability inherent in assumptions. And it would seem to suggest that for the purpose of getting on with their work laboratory members have a sort of vested

interest in making assumptions, that is in cognitively skimming over certain features of their work. In the case above, consideration of an assumption interfered with the interpretation of what was to be an acceptable "reading" of oxygen. This effectively and drastically reduced the pace of the experiment in question until a measurable determination could be made of the difference of readings, the extent of the interference. Again, both this example and the prior one illustrate that in engaging in their work laboratory members tacitly made a number of assumptions concerning the nature of this work which allowed them to get on with these investigations. As the second example illustrates quite clearly, when they are forced for one reason or another to question and consider the possible effects of failed assumptions upon the work at hand, progress is stopped or slowed and the interpretation of results becomes problematic.

B. Hypotheses

A second way in which cognition is crucial to an adequate understanding of scientific inquiry is in scientists' conception and use of "hypothesis." Our discussion of the nature and role of hypotheses in scientific inquiry begins with the analysis of a get-together of laboratory members to discuss and react to the work of one of their colleagues. This colleague had selected some available data on one lake to determine the consumption of oxygen in the hypolimnion because he believed that sediment oxygen demand could account for all of this consumption. The laboratory member's investigations into the relevant data appeared to support his belief. But, as presented, the findings and conclusions appeared problematic to other laboratory members. They found his talk a bit unclear, and were confused until the senior graduate student (Bill) attempted to clarify the issue (21).

Bill: OK. So what's the question you're trying to answer about this?

Calvin: OK.

Bill: What would be the alternative, what's the hy-, put in the hypothesis and show me an alternative.

Calvin: The hypoth-, hypothesis here is that, ahm, sediment oxygen demand can account for, for all of the oxygen ah consumption in the hypolimnion.

Bill: OK.

At this point, the chief of the laboratory interjected and questioned the legitimacy of the student's apparent claim that his findings supported his "hypothesis."

Chief: Yeah? Let's say you get one. Say you got a perfect correlation. How does that bear on your hypothesis?

Calvin: Well, ah, it just means I have to keep going and try, try, try other lakes to see if, ah, in fact this is the-

Chief: No. Let's say you always get one. How does that correlation coefficient ah, what's the relation between that, these results, the correlation coefficient of one, and your hypothesis?

Calvin: Ah, getting a correlation coefficient of one...ah...OK. Still, it's, it's only one step. I mean, obviously it doesn't discount the water.

Chief: OK.

In the above conversation, it is already clear that these scientists did work with some conception of the term "hypothesis." It does not imply a formal, philosophical conception, but it does appear to have been used as an equivalent for belief, assumption, or answer to a question. In the above excerpt, the questioners used the concept of "hypothesis" to elicit clarification and specification from the speaker. "Hypothesis" was presented here by Calvin as a tentative statement asserting some kind of relationship between perceived physical phenomena. The term "hypothesis" can be defined simply as a tentative assertion to be tested. Note that the questioners did not ask the speaker for a statement derived logically from an existing body of theory, or from methodological rules. Nor do I have any examples in my body of data where this was done (22). Note here also that the laboratory member's hypothesis met with a fair amount of resistance from the chief of the laboratory. Perhaps he assumed at that time a different relationship between lake elements - a more active role for water - and was put ill at ease by the apparent boldness of the student's claim. In fact, he made it clear in the discussion that the student's findings did not actually support his hypothesis because he had not really tested it properly. The chief later told another laboratory member: "I didn't want him to convince you that he got, he tested this hypothesis when he didn't have anything." The counter-response of the chief led directly to the tempering of the "hypothesis" by the laboratory member: "The hypothesis is, the S.O.D. (sediment oxygen demand) is very important. I'd like to try to determine how important it is."

The talk from which the above fragments were taken also contained other references to "hypothesis." Calvin was engaged in another experiment, one designed to test a colleague's "explanation" for why in a chamber or core respiration decreases with oxygen concentration. In an entire lake, it takes several months for oxygen concentration to drop, whereas in a core it takes only two to six days depending on how much water is above the sediments. Bill had attempted to explain this state of affairs. In the following quotation, Calvin, who was testing the viability of this explanation, responds to the chief's request for clarity:

Calvin: With sediment oxygen demand, that is in this closed chamber as measured, the respiration drops with the oxygen concentration. Whereas in a lake it doesn't appear that the respiration drops at all with the dropping of the oxygen. So it seems unlikely that the water oxygen demand rises as the oxygen concentration becomes very low so that you get a constant rate. Ahm, so Bill had put forward the hypothesis to explain why it might be that in cores this would drop off. In other words, Bill's hypothesis was such that this was an artifact, this dropping off of apparent respiration (in the core) with oxygen concentration was an artifact, that the S.O.D. in fact was not as measured here the same as what was going on in the lab. And, at the present time, what I'm trying to do is use the same chamber with a flo-through system to test this hypothesis. The hypothesis is that in a

chamber the oxygen concentration drops so rapidly that the organisms cannot respond fast enough to continue the same rate of respiration as they would in a lake. In the lake it takes several months for the oxygen concentration to drop. In my chamber it takes two days to go down to zero, or it might take three or four days depending on how much water the sediments are using, how much water is over the sediments. So, ah, Bill's hypothesis was that if the oxygen concentration in the water was decreased at a slower rate, then you should get more of a pattern like this, respiration (loss of oxygen) should continue, should remain constant. So my experimental set-up initially was designed to give this lowering in rate, sorry, lowering rate of, ah, it was designed to gradually lower oxygen concentration or hold it at a constant level.

In the above talk, the term "hypothesis" is being used in a variety of ways. But it appears that what ties them all together is the scientists' conception of hypothesis as a temporary statement which attempts to explain relationships between perceived physical phenomena. It was, in other words, an assertion, belief, or guess, which was assumed to be testable. The result of the "hypothesis" in question was to view the drastic drop in respiration with decreased oxygen concentration in cores as an "artifact" and explain it away by asserting a lack of correspondence to actual lake processes. For this correspondence to be accurate, the oxygen concentration in the core had to be artificially decreased in order to see if respiration continued at a constant rate, that is, the way it would be expected to occur in lakes. Notice that in the talk there is no discussion of where the "hypothesis" came from, whether the literature, work in the laboratory, or otherwise. Nor was that issue discussed in the entire conversation. We see that for these scientists how hypotheses are used in practice and from where they are believed to be derived are two quite different questions. In this instance, however, the "hypothesis" was not clear to all members present, including the senior graduate student to whom the "hypothesis" had been credited. Hence, the insistence by this student that the question to be answered by the experiment be explicit.

I want the hypothesis on the wall, like, put it down. I, you put it down, the hypothesis, then, I guess the question is does respiration rate decrease as a function of time in your chamber or is it a function of concentration?

The work of "hypothesis" here is to force the clarification of argument. This continual insistence upon having a clear idea or hypothesis prior to engaging in an experiment is more problematic than first appears. The first glimpse of an idea is often amorphous, undefined. Then discussion among colleagues brings clarity. The original formulation was problematic not only for the audience, to whom it was being explained, but also for the laboratory member himself, who must properly design a physical research situation to test for the viability of relationships formulated at the cognitive level. To put it plainly, to prove a relationship between perceived physical phenomena is the problem for everyone concerned. In the case above, Calvin later encountered so many problems in constructing an acceptable experiment to replicate lake processes that he began to doubt his original idea or hypothesis, which in turn led him to consider dropping the experiment altogether. This worried the chief of the laboratory.

Chief: We had a clean hypothesis when we worked out this experiment. Well, what happened to it? Where did it go?

Calvin: Well, I went back to, from my hypothesis to sort of seeing why I was doing it, and sort of went around in circles.

Again, a laboratory member insisted upon a clear statement of the problem, its tentative explanation, and the physical translation of this explanation into an experiment. In discussing their work with each other, we find that scientists were putting great emphasis on the language of "hypothesis," and that it does major work of clarification in the course of argument and in the physical expression of conceptual problems.

The above assertions are also supported by the way laboratory members used the language of hypothesis when explaining their work during interviews. In the following excerpt, for instance, Calvin explains the nature of his work.

I'm working, basically, what my hypothesis has been previously, is that sediment oxygen demand, uptake of oxygen in the sediments (as opposed to in the water), is important in lakes. Otherwise it would be hard to sort of justify doing it. But we don't know. Some people have said it could be responsible for one hundred percent of the oxygen uptake in the bottoms of lakes. And other people say it's, you know, might only be five to ten percent. And if we don't know, ah, it's very hard to know where to look for the explanations for why the oxygen in some lakes drops faster than others. Cause we all like to look for explanations, not just to say OK this happened and because of this therefore my results are valid... We use information to lead us to new hypotheses to test. In other words, the explanation is the fun part.

This bit of talk again shows how laboratory members consciously viewed "hypotheses" as a tentative statement or idea which attempts to explain relationships between physical entities. An hypothesis is both an ideational and a physical entity. Wrapped up in the very meaning and use of "hypothesis" to laboratory members is the belief in an external physical world which can be adequately described for the purposes at hand. Thus, in view of these observations, it would not be accurate to assert that the statement about a physical phenomenon suddenly becomes the phenomenon itself at the completion of scientific inquiry, as Latour and Woolgar do (23). Rather, such an inversion occurs prior to it at the point of physical translation of the "hypothesis." Given the assumption that an external physical world of objects exists, parallel statements, symbolic and material, become possible, and it is justifiable from the scientists' point of view to "test" for their equivalence.

Our next example of the use of "hypothesis" in the laboratory I studied concerns a laboratory member's explanation of why the phosphorus concentration in one of the lakes included in the five-year project "crashed down to zero" by the second summer, to everyone's total bewilderment. This bewilderment was caused by the assumption dominant in limnological theory at the time which claimed that biological productivity in lakes was dependent upon the amount of phosphorus they contained. The conversation below begins with a laboratory member explaining the accepted theory.

- John: The theory is that, well, in lakes anyway, the vast majority of lakes are phosphorus limited. In other words, it doesn't matter how much you add of any other nutrient. If you don't add more phosphorus, you won't get more biological productivity.
- Researcher: OK.
- John: That's, that's the theory. But it seems that these lakes in Schefferville don't conform to that theory which is, scientifically speaking, fascinating.
- Researcher: OK.
- John: And this phosphorus, I'm going out a little bit on a tangent, but it explains why he's (the chief) preoccupied with this benthic stuff. Because in Lejeune Lake, where the phosphorus concentration crashed the second year, his hypothesis is that although the plankton in the water might not be phosphorus limited, the benthos or the fauna in the bottom of the lake in the sediments is phosphorus limited.
- Researcher: So, he's in the process right now of trying to figure out what's happening in Schefferville?
- John: Yeah. Yeah. Well, scientifically speaking, it's very fascinating and everything. There's no doubt about it. It's ah, it sort of brings into question some of our, some of our theories, some of the assumptions of all these phosphorus models.

The laboratory member above is presenting the chief's explanation or "hypothesis" for why the phosphorus decreased to zero in one of the project's lakes, in terms of the difference of water-born plankton from lake-bottom fauna. This drop in phosphorus was taken as evidence that the claim of established theory that all biological life in lakes is limited by the amount of phosphorus was erroneous. The plankton in a lake are not supposed to be phosphorus limited while the benthos or fauna in the bottom of lakes are still supposed to be, which has become the basis of a new "hypothesis" to test at some future point. At that point, another chemical, nitrogen, will be tested to see if it is the limiting nutrient of benthos. The "hypothesis" has lead to a careful review and evaluation of the dried sediment samples from the lake in question, since it was unexpectedly noticed that many of them contained "benthic stuff," fragments of shells and moss from organisms living in the bottom of lakes. This "benthic stuff" might have a far different phosphorus content than the sediment. Thus, if the sediment sample is not ground up as fine as possible and if through mishandling a piece of shell or moss falls out, "you may not be getting a representative sample of everything that's in there." It then becomes entirely possible that the measurement of phosphorus content in the sample becomes untrustworthy. So the chief of the laboratory instructed those responsible for the handling of the sediments to grind everything in the sample to a fine powder form with a coffee grinder. The grinding of the benthos in the dried sediment samples is an attempt to check the validity of the chief's "hypothesis" that the growth of fauna at the bottom of lakes is phosphorus limited. If they are, then the

additions of phosphorus in lakes could be totally absorbed by the benthos, which should then show an increase the next year in the amount of detectable phosphorus. This would, in turn, increase the total amount of phosphorus in the second dried sediment samples. The grinding is also a way to check if there is any benthic response at all to phosphorus enrichment of lakes. So it becomes important from the scientists' point of view to know which cores have benthos in them and where they were taken from in the lake.

We see from this example that a tentative explanation for a relationship between perceived physical phenomena, that is to say an "hypothesis," has led directly to a number of physical actions by scientists. The casting of doubt upon the existing limnological theory of uniform biological limitation of lakes has allowed the formulation and design of a new experiment to test for what chemical does limit the productivity of benthos in lakes. Second, it has led to a back-tracking and re-evaluation of methods used to obtain a representative dried sample. From this back-tracking, a new procedure for grinding the sediment was instituted. Third, it created a need for the mapping of the location of the "benthic stuff" in the lake in question. All these actions occurred as the result of the chief's "hypothesis," denoting the crucial importance of ideas in scientific inquiry. The chief reacted to what he perceived to be a relationship between physical phenomena via the formulation of an "hypothesis" which sought to express this relationship.

The chief himself also in practice utilized the language of "hypothesis." In order to show how this was done, we will take a closer look at the beginning of the lecture which he was invited to give his peers about his work on the five-year project. He began the lecture matter-of-factly by making explicit the reasoning behind the project:

The objective, first of all, of this experiment was to test a very simple hypothesis: the retention of phosphorus in lakes is of phosphorus loading. This is a very simple hypothesis. It is implicit in every model I know of that has been produced to predict the concentration of phosphorus and the concentration of phytoplankton in lakes. This assumption is indeed an assumption in all these models... and thus it is a reasonable and predictable test. That is the advantage of the program, to test this assumption.

This passage states that a relationship empirically supported in previous literature is now being treated again as an "hypothesis," or an "assumption," in order that it may be tested in a long-term limnological experiment. Here a scientist views accepted claims, or even prior "facts", as tentative assertions or simply untested statements. At one time scientists appear to treat statements as equivalent to an objective situation, while at other times those statements are considered to be simply ideas or assertions which need not have correlates in the physical world.

The above discussion of scientists' conception and use of "hypothesis" may be summarized as follows. They actually used the notion of "hypothesis," and in a variety of ways. Operationally, hypotheses were used as tentative focal statements about relationships between perceived physical phenomena. They were used to facilitate the specification and clarification of an argument. This was

believed to be necessary and prior to any attempt to legitimate and translate conceptual problems into physical research situations. It is probable that "hypothesis" performs other as yet unknown functions in scientific work as well. All this would seem to contradict assigning ideas or hypotheses or cognition in general a minimal or subservient role in scientific work.

Discussion

Constructivist orientations to an understanding of science have been helpful in loosening the adherence of investigators of science to philosophers' idealized descriptions of the nature of the process of scientific inquiry. Although it has encouraged the careful observation of actual scientific practice, it also appears to represent an overcompensation for the earlier philosophical emphasis upon the primacy of impersonal rules of logic, of rationality in the production of scientific knowledge. To autonomous reason it opposes the economic, material, or literary dimensions of scientific inquiry. This has resulted in the severe downplaying of any independent role for cognition in science and consequently in the neglect of the interaction between the cognitive moves of scientists and their own physical manifestations of the perceived physical objects of their investigations.

In contrast, the present paper sought to recover the role of active, improvising thought in the social production of scientific knowledge and to reconstitute the phenomenal world as one determinant of what is said about it. It suggests that the notion of "rationality" in science must be expanded to a more informal, active concept of scientific work in practice. This expanded conception of scientific thinking, which we called "cognition," suggests that the scientific laboratory is not just where scientists reason, nor is it just where they physically work; many things in addition happen there and wherever scientists meet and discuss their work, among them the cognitive moves and physical/symbolic translations described above.

To be more specific, this paper contests two major themes in the new ethnographies of science. The argument is for an independent causal role for cognition in scientific practice, although simple substitution of idealism for materialism is not intended. It should be possible to determine the causal role of other factors upon the process of scientific inquiry, such as the constant discussion and criticism among scientists of each other's work. Here, the ways in which cognition plays a crucial role in that process was demonstrated empirically.

The second major issue in the new ethnographies of science addressed here is whether science is just like (no more than) practical reasoning. The question is, "Is scientific reasoning different?" The first answer can be 'no', and the similarities between science and commonsense pointed out, such as problem-solving, improvisation, and analogical reasoning. But then, the second answer has to be 'yes', science is different. In science, assumptions are explicitly recallable and can be legitimately brought to focal attention to be questioned. Under such circumstances the holder of the assumption is obligated to sit still and listen, review and possibly modify its understanding and use. If this occurred in everyday life to the same extent as in science, people might well cease to speak to each other. Thus, for scientists an assumption can become an hypothesis, a focal, clear statement ("I want the hypothesis on the wall") and, finally, translatable into physical and observable terms.

If commonsense is taken as the reference point, it can be argued that science is the same as other types of reasoning. On the other hand, scientists' reasoning differs with regards to how statements should be treated. One way in which they differ is in the types and use of assumptions and hypotheses. Scientists are practical reasoners, but at the same time special in ethic and technique. People in everyday interactions are not obligated by an ethic continually to review and criticize the assumptions contained in their statements or the statements of others. Thus the simple question as put above appears unanswerable. Science is both like and unlike commonsense. Specific practical successes are important in science, as evidence for the cognitive framework being developed, but not in their own terms. They can be taken as tokens for future practical successes, but they are not sufficient in their own right.

The findings here contrast with the findings of two major recent ethnographies of science, who found they could describe laboratory life without reference to ideas or hypotheses and thus assigned a subservient role to genuine cognition in the process of scientific inquiry. But because scientific inquiry as and where observed does not fit neatly into a purely rational view of science does not necessarily mean there is no independent cognitive content in the scientific laboratory, nor that such content plays a minimal, subservient, or otherwise dependent role in scientific inquiry.

Notes

1. Focusing upon the conversations between scientists as they occurred in their natural setting, the laboratory, the present paper analyzes scientific inquiry on the basis of a participant-observer study conducted at McGill University, Montreal, Canada, from January to April 1981 and from October 1980 to May 1981. It also included hands-on engagement in scientific activities. During observations, a cassette recorder was permitted to record conversations. In addition, a two-track reel-to-reel system was installed in the laboratory and monitored conversations for each working day during the period March 25, 1981, to April 24, 1981. The taping of conversations was supplemented by frequent note taking, interviewing, and attendance at and recording of seminars and conferences. The laboratory I studied is involved in limnological investigations, generally defined as the scientific study of physical, chemical, and biological conditions of fresh waters (lakes, rivers, ponds, streams). The laboratory is principally concerned with looking at the behavior of chemicals which are found in lakes, chiefly oxygen, phosphorus, and lead. It is directed by one full-time professor with four graduate students, as well as a small cadre of undergraduates and part-time technicians. The graduate students were working on the feasibility of building quantitative multi-factor models which would allow them accurately to predict the chemical characteristics of lake water and sediments.
2. The following are just some of these studies: D. McKegney, "Inquiry into Inquiry: the Case of Wildlife Biology," paper presented at the conference "Science and Technology Studies--Toronto '80," University of Toronto; J. Law and R. Williams, "Putting Facts Together: A Study of Scientific Persuasion," *Social Studies of Science*, 12:#4 (November, 1982); M. Lynch, "Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory," 1981, unpublished PhD Dissertation, University of California, Irvine. Also, M. Lynch, "Technical Work and Critical Inquiry: Investigations in a Scientific Laboratory," *Social Studies of Science*, 12:#4 (November, 1982).
3. For an account of the significance of the conceptual transition in the study of science from a philosophical to an historical and then social perspective, see R. Krohn, "Scientific Ideology and Scientific Process: The Natural History of a Conceptual Shift," in E. Mendelsohn et al. (eds.), *The Social Production of Scientific Knowledge, Sociology of the Sciences Yearbook* (Vol. 1, D. Reidel, 1977). See also R. Krohn, "Toward the Empirical Study of Scientific Practice," in K. Knorr et al. (eds.), *The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook* (Vol. 4, D. Reidel, 1980).
4. The term "materialist" refers to a theory that physical matter is the only or fundamental reality and that all being and processes and phenomena can be explained as manifestations or results of matter. The relation to Marx's dialectical materialism is obvious. Just as to Marx economic and social change is materially caused, so to investigators of science adopting a materialist orientation are ideas and ideational change subservient to a material economic base. Hence the use of economic terminology by investigators of science adopting this theoretical approach.

5. K. Knorr, "Tinkering Toward Success: Prelude to a Theory of Scientific Practice," *Theory and Society* 8 (1979): page 348.
6. Knorr, *ibid.*, page 362.
7. Knorr, *ibid.*, page 354.
8. Knorr, *ibid.*, page 358.
9. Here the similarity to the Marxian "class struggle" is evident.
10. K. Knorr, "Producing and Reproducing Knowledge: Descriptive or Constructive? Toward a Model of Research Production," *Social Science Information*, 16 (1977): pages 669-696.
11. K. Knorr and D. Knorr, "From Scenes to Scripts: On the Relationship Between Laboratory Research and Published Paper in Science." Paper presented at the Annual Meetings of the American Sociological Association, San Francisco, California, 4-9 September 1978.
12. Knorr and Knorr, *ibid.*, page 34.
13. Knorr and Knorr, *ibid.*, page 37.
14. K. Knorr, "The Scientist as an Analogical Reasoner: A Critique of the Metaphor Theory of Innovation," in K. Knorr et al. (eds.), *The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook* (Vol. 4, D. Reidel, 1980).
15. Knorr, *ibid.*, page 14.
16. Knorr, *ibid.*, page 17.
17. Bruno Latour and Steven Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (SAGE, 1979).
18. Latour and Woolgar, *ibid.*, especially page 147 and pages 175-177.
19. Latour and Woolgar, *ibid.*, page 153.
20. Latour and Woolgar, *ibid.*, page 213.
21. It is absolutely crucial to understand that the focus of analysis is not on who made an unclear presentation, but rather the process through which clarification was attempted. Although it is possible the process through which scientists in a laboratory request clarification from each other varies according to their perceived status and experience, that was not the aim of the present study.
22. The participants were not quizzing Bill as to the literary origins of his "hypothesis," probably because they were quite familiar with the literature and therefore tacitly accepted its legitimacy. In any case, the issue here is not the origin, but rather, the use of "hypothesis."
23. Latour and Woolgar, *ibid.*, page 147.

GLOSSARY

Benthic: Of, relating to, or occuring at the bottom of a body of water.

Benthos: Small organisms that live in or on the sediments of lakes and rivers.

Core (or chamber): A tube which is designed to collect samples from lake sediments.

Hypolimnion: The water layer underlying the thermocline of a lake.

Limnology: The scientific study of physical, chemical, and biological conditions of fresh waters (lakes, rivers, ponds, streams).

Plankton: The passively floating or weakly swimming usually minute animal and plant life of a body of water.

Polonium: A radioactive metallic element that is similar to tellurium and bismuth, occurs especially in pitchblende and radium-lead residues, and emits a helium nucleus to form an isotope of lead.

Respiration: The exchange of oxygen and carbon dioxide between the organism and the environment. It is the chemical processes by which a body of water supplies itself with the oxygen needed for metabolism and relieves itself of the carbon dioxide formed in energy-producing reactions.

Sediment Oxygen Demand (S.O.D.): The uptake of oxygen by sediments.

Thermocline: A layer in a thermally stratified body of water that separates the upper warmer water from the deeper colder water.

Water Oxygen Demand (W.O.D.): The respiratory demand placed on the oxygen of the water by organisms.

BOOK REVIEWS

Betrayers of the Truth: A Fraudulent and Deceitful Title from the
Journalists of Science

Review of William Broad and Nicholas Wade. **Betrayers of the Truth: Fraud and Deceit in the Halls of Science.** New York: Simon and Schuster, 1982.

Fraud, assert Broad and Wade, is endemic to the scientific enterprise; it is not merely aberrant action by a few "bad apples." "Newton having set the standards, it is perhaps not so surprising to find other scientists using the truth to support their own theories in ways that make a mockery of the scientific method." "Scientists' cavalier attitude toward data in the nineteenth century" If plagiary, the grossest offense against intellectual property, merits just knuckle-rap treatment from the scientific community "The looseness of the system allows the John Longs of science to penetrate it time after time without challenge." "... [T]he prevalence and frequent success of fraud" "Science is not self-policing." And on and on they go.

Such a sweeping condemnation of Science and the Scientific Establishment is usually the hallmark of cranks whose pet theories have not been respected. When such criticism comes rather from journalists of science, one is perhaps more inclined to take note and to consider the proffered evidence. In the case at hand, alas, it turns out that Broad and Wade are as guilty of fudging their data as were the individuals about whom they write, and very guilty indeed of trying to pass off their assertions as objective or factual.

An appendix lists 33 "known or suspected [nota bene, suspected] cases of scientific fraud," from the 2nd century BC through 1981. The inference seems obvious, that proven fraud in science is quite extraordinarily rare. Some 30-odd times in 2 millennia is surely less frequent than the most recondite of other human failings, not to speak of such comparable situations as embezzlements by bankers or lawyers or bribe-taking by elected officials. Of those 33 instances, moreover, 13 date from the 1920s and back, and in most of those one must ask, are we not betraying a lack of historical sense in assuming that the practices and ideals of the scientific community were (or ought to have been) then as they are (supposed to be) now? In Newton's time, there did not even exist what we now call the scientific community; the very term, "scientist," was only introduced about 150 years ago; and "science" as a career or profession is, if anything, younger still (1). So one might legitimately look for proof of the Broad-Wade thesis in the more recent evidence they proffer, 20 cases since 1940. In doing that, however, one is still brought face-to-face with the question, what to include under "science" in this context? For 10 of the miscreants are medicos, 3 are biochemists, 1 a psychologist, another a parapsychologist, another was a graduate student, yet another an undergraduate, and still another had no other standing than forged degrees and letter of recommendation. I am reminded of my scientific mentor (Ph.D., Bonn; M.D., Padua; D.Sc., Sydney), who used to say, "The trouble with doctors is that they are not scientists"; and he would say that most frequently when he had just returned to his university office from his clinical research laboratory at the hospital.

Broad and Wade know that not everything loosely called "science" is the same thing. Thus, they assert that the incidence of fraud "appears to be somewhat less in the 'hard' sciences." In another place, they report findings of clear bias and preconception and lack of consistency in the refereeing of articles in psychology, whereas no bias was detected in the reviewing of articles in physics. Yet in spite of this, and without giving a rationale, Broad and Wade are able to "doubt that there are serious differences among the ways that physicists, biologists, or sociologists go about their work The study of fraud sheds light on how all scientists behave" But their own data lend support rather to my mentor's view and to the conclusion that fraud in science is very unusual indeed.

It is not only on this central point that Broad and Wade's conclusions are at variance with what their evidence says. "The rewards for even deceitfully gained success are considerable and the chances of apprehension negligible," the reader is told. But the miscreants described were apprehended, and lost rewards (even Ptolemy, Newton, and Mendel are losing some of the only reward that remains to them, their good name). "Science is not self-policing," the reader is told. But all of this cited fraud and deceit was uncovered by the members of the scientific community itself, even the community of medical researchers which I briefly derogated above. And yet further, the majority of the cited cases shows that uncovering and retribution follow very swiftly upon the transgression. Broad and Wade claim that "Alsabti's success shows how ineffectually the social mechanisms of science operate to check the excesses of ambition and careerism." To the contrary: in his first attempt to establish himself in the U.S., "Alsabti lasted a month"; his second attempt "succeeded" for 5 months; in Texas, he lasted 6 months; in Virginia, about a month. I would take that as evidence that the medical community was astonishingly alert and swift.

Broad and Wade relate that Alsabti amassed his fraudulent bibliography by "publishing in obscure journals on backwater subjects"; but they fail to emphasize the obvious corollary, that he knew that he could not get away with it otherwise; and the further corollary, that no one can achieve status or rewards in that manner. Peers do look at where and what one publishes, and can discount "obscure" publications on "backwater subjects" quite as knowledgeably as can Broad and Wade.

As I was reading this book, I found myself making notes about the authors' naivety in criticizing certain aspects of the workings of science, only to find, later in the book, that the authors have a better understanding than they earlier revealed. "A single attempt at replication would have stopped the fraud dead in its tracks," I read. Don't the authors know, I fumed to myself, how long it can take to replicate something precisely, to be sure that one is doing things exactly as in the other lab? Don't they know how long it can take to acquire technique -- and the possibly needed new equipment or materials? Don't they know that a failure to replicate certain results would lead to further attempts to replicate, quite a number of times before one would begin to be suspicious of the published work? But then, after I had read some fifteen pages of examples of such naivety, I came to a very good summary of the difficulties of replication, and an explanation of what replicability actually means in scientific practice. So the authors understand better than they at first reveal, but that understanding also makes

one wonder why they wrote in that other fashion in the first place. Is this perhaps some compromise between two co-authors? Does Broad understand but not Wade (or vice versa), so that one is responsible for the naive criticisms and the other for the informed counter-argument?

Again, in many places Broad and Wade express (or feign) surprise that certain results were not immediately suspect, that certain individuals did not at once suspect their associates of dishonesty. It might have occurred to them that "If we tried to run science so as to protect ourselves from fabrication, I think we would ruin the whole enterprise"; if for no other reason, it might have occurred to them since they quote that statement. Nowhere do Broad and Wade show awareness of the fundamental basis for the belief, supported by the evidence in this book, that science is self-policing. Since science properly so-called (namely natural science -- chemistry, physics, etc.) is an explication of nature, fraud cannot succeed because nature will not be mocked. In the short run, of course, as new claims are made, fraud can be present just as what we later learn to be plain error or even what many would (again with the benefit of hindsight) call pseudo-science. New ventures are tentative and they are risky. But Broad and Wade ignore entirely the important distinction between new claims on the one hand and the long-digested and long-accepted parts of science on the other. If fraud is indeed endemic in science, where are the laws and theories that were generally accepted into science, to be later found fraudulent? Again, the authors do not benefit from their own evidence. If Newton and Mendel fudged their data, they were able to get away with it for so long only because their ideas happened to be correct enough. Had they fudged data to illustrate a fraudulent effect or law, they would have been caught at it long before now. Indeed, no one would ever have heard of Mendel since his laws would not have been rediscovered. This sort of fudging stems from the utter conviction that the investigator has uncovered a truth. In that sense, the perpetrators are somewhat unbalanced, a diagnosis that Broad and Wade dismiss altogether too lightly. We readily dismiss as unbalanced only those who believe utterly in some notion that we reject; but the psychological corollaries of utter belief can hardly vary according to the truth of the believed proposition (since none of us knows of any proposition whose absolute and irrevocable truth has been unequivocally established). At any rate, one takes an enormous gamble in seeking to build a career or reputation in science on fraud, since the chance of being proven wrong is ever-present; and the more striking and apparently credit-worthy one's claims are, the greater is also the risk of early discovery, since many others will rush in to capitalize on the new finding and to join the new bandwagon. So science is self-policing because it is self-correcting. Not instantaneously, of course; and that is what gives Broad and Wade their occasion for adducing apparent support for their sweeping claims. To make their case, however, they would need to show that it takes longer to detect and correct fraud in science than in other human activities.

In their attempt to support a case that is unsupportable, and failing of evidence, Broad and Wade use such unwarranted inferences as illustrated above, and also argument by innuendo: "An operation more subtle might [nota bene, might] never have been detected"; "the proliferation of papers that at least in some instances [emphasis added] do little more than feather the nests of ambitious academicians"; "One wonders what would have happened"; "How much erroneous or fraudulent science might be turned up if ...?" "How common is

fraud in science? ... the true dimensions of the problem can only be estimated." True; and how Broad and Wade do estimate! "We would expect that for every case of major fraud that comes to light, a hundred or so go undetected. For each major fraud, perhaps a thousand minor fakes [E]very major case of fraud that becomes public is the representative of some 100,000 others, major and minor combined, that lie concealed in the marshy wastes of the scientific literature." That pulling out of the air of 100s and 1000s is the "evidence" Broad and Wade supply that fraud is not rare in science, that it is endemic; together with, of course, the 33 cases in the appendix -- which, as I have already indicated, actually reduce to less than half-a-dozen proven and in science over the course of 2 millennia.

I became angry as I read this book, and I remain quietly angry. In part, and certainly initially, my anger came from the perverse discrepancies between the evidence presented and the conclusions drawn, from the sweeping generalizations adduced by innuendo. But my anger remains because Broad and Wade show in many places that they actually have quite a good understanding of many facets of science -- even those about which they write with apparent lack of understanding in other places in the book. My anger remains because Broad and Wade have produced what many will see as a thoroughgoing indictment of science, when what they apparently wished to do was to demythologize science, so necessary a task. My anger remains because Broad and Wade could have written so much better and more useful a book. As it stands, many scientists will not be willing to read the offensive portions of the book in order to get to the good ones; particularly since the former predominate at the beginning, and it is Chapter 7 before the best begins. That is a pity indeed, because scientists need to learn that (and how and why) their naive positivist, reductionist, religion-like faith easily appears to others as willful arrogance and self-serving dogmatism. As it stands, I wager that Broad and Wade will not be pleased at the fullsome praise that will come their way from the cranks and pseudo-scientists and assorted chronic anti-scientists. As it stands, and my anger notwithstanding, I hope that the book will have many readers but that most readers will be able to forget the extreme statements and red herrings and rather comprehend and remember some of the book's excellently authentic descriptions of some important facets of science: about replication on pages 76 and 77, for example, and most of Chapters 7 through 9, and Chapter 12, especially pages 217 to 220 and 223 to 224.

Let me summarize what Broad and Wade apparently wanted to do, so that readers will not, incensed, throw the book aside after a few pages or chapters. The popular image of science, the conventional wisdom about science, is dangerously erroneous. Because science is relatively reliable and relatively logical; because it has been so recently and so astonishingly successful; because there is no competing religion of knowledge; and no doubt for other reasons as well. Science is popularly conceived as infallibly correct and logical and successful and true. And scientists, the priests of this awesome god, are supposed to be infallibly correct and logical and without sin. But scientists like priests are human, and the successes of the enterprise of science stem from the weeding out over time -- and sometimes a very long time -- of the many errors and the occasional frauds that are inevitable in any human enterprise. If scientists themselves had a sophisticated understanding of this, perhaps they could be more on their guard, perhaps they could improve some of the workings of their community, perhaps they would be more effective in their dealings with the wider society.

Broad and Wade, I suspect, decided to use "fraud" as a device to capture attention to their subject, which is the whole of science and not just fraud and deceit; the title of the book is misleading and unfortunate. Perhaps Broad and Wade believed that only stridency and sensation would have the impact they desired. But for whatever reason, their critique is often so extreme that it becomes as misleading, albeit to the other extreme, as the naive views that the authors rightly criticize. Broad and Wade became carried away with zeal for their mission, and pushed their conclusions not only beyond the evidence but beyond what is reasonable. They say, for example, that science is at least as prone to fraud, error, dogma, prejudice as any other human activity, maybe even more so. No evidence is presented for that view, nor does it stand to reason. Priests are human, and they are also sinners, that I will admit; what I will not admit -- particularly not in the absence of evidence -- is that priests sin as grievously and as often and as glibly and as callously as do non-believers. Scientists are human, and they are also capable of cheating, of invoking authority unfairly, of arguing rhetorically and dogmatically, of all the rest; but it would be strange indeed if they acted in contradiction to their own beliefs as frequently as do those who do not share those beliefs. Most scientists -- and especially the best, the elite -- respect their enterprise, even hold it sacred, even regard it as the quest for the truth; they think they have a calling for what they do. When they keep knowledge secret instead of sharing it, when they long for fame rather than for discovery, when they transgress any of the naive ideals of the popular image of science, they feel guilt (and are sometimes driven to make public confession, vide "Honest Jim" (2)). Scientists know that they should be Arrowsmith (3) and not Woods (4); and when, for example, loyalty to country and loyalty to science conflict, scientists experience tragic dilemmas (5, 6).

Broad and Wade are correct: the popular view of science is myth, and the historians and philosophers and sociologists of science have pretended, of course without justification, that their partial understandings are the whole story, or at least the most important part of it. But Broad and Wade are also quite wrong when they want to overturn and contradict totally those views and understandings. Truly needed is the qualification of those understandings, the elucidation of the needed modifiers and nuances, a fitting into the complex whole of the partial truths of the historians and philosophers and sociologists and of others too: psychologists, for example, not to mention scientists themselves. It is currently fashionable but nevertheless misguided to ignore the views of practicing scientists about what they do and how and why. So Broad and Wade dismiss as self-interested and self-protective rhetoric the testimony of Handler, president of the National Academy of Sciences, when he articulated the ideals and beliefs that motivate the best and the ablest scientists. But can it really be that a person's ideals have no influence on his actions? Are not medicos more responsible because they believe that they should hold dear the Hippocratic oath?

Anyone who has known scientists well has known at least some scientists whose work is also their life, who can get satisfaction only from doing it right, for whom "rewards" gotten by fraud would bring no pleasure at all; they crave the scientific rewards, the intoxications that come when one has seen more deeply or clearly into nature than any human before. Certainly the scientists who populate the "elite," the "scientific establishment," tend to be like that; or to believe that they are like that, or that they ought to be like that -- in any of which cases, they will tend to act as though they are like that. Broad

and Wade appear to deplore that "prestige comes not just on the merits of work but also because of position in the scientific hierarchy"; but how, might I ask, does one get position in that hierarchy in the first place? Certainly not by birth! The best students get the best recommendations, and chances at the best graduate schools. And those who do best there get the best postdoctoral opportunities, and those who make the most of those get the best positions at the best universities, and a better chance at grants to begin their work, and so on. Of course, by-and-large and on the average only, there is also some playing of games and swapping of favors and so on. But not too much, because nature will not be mocked. If one of the elite manages to get a weak student of his well placed, and the protege fails to make good, then later recommendations from that source will not be as influential as they once were. In every specialty and sub-specialty, the invisible college amasses a wealth of information, never shared with outsiders, about who deserves his status and who does not, about who can be trusted and who cannot, about who are the superficial grantsmen and who the genuine leaders of insight, about whose students are a pleasure to work with and whose not worth taking on. Over time and on the average, those in the elite are properly there. Broad and Wade point out that the elite find it easier than others to get published, to get grants and editorships and honors; of course, and I would hope so, because the elite have the best credentials. It is quite rational to give grants without much ado to one who has already gained a Nobel prize, but to demand the most impeccably presented case for support from an unknown, unpublished scientist at a mediocre institution. Broad and Wade worry that people may remain in the elite long after they deserve to. Perhaps so, though they cite no cases. On the other hand, it is not difficult to think of individuals who lost their places, and their access to funding, because they trod too far outside the mainstream -- Erwin Chargaff, Linus Pauling and Albert Szent-Gyorgyi, are examples.

Broad and Wade are right, of course, that careerist pressures make it more difficult to stay quite clean. But does it really make sense to expect perfection and deplore its absence. Ultimately, I think, the trouble is that Broad and Wade are not comfortable with the inevitable imperfections of a human enterprise that is open and free, and they become so obsessed with the weaknesses that they discount the strengths -- just as political liberals tend to do when democracy does not always produce the results they would like, and they forget that democracy is the worst of all systems except the available alternatives. So Broad and Wade give absurd prescriptions for "improvements" in the workings of science: absurd because there exists no authority that could implement such changes, absurd also in substance. "Every idea would be taken on its merits, and every person judged on the worth of his ideas," in their ideal community. But we already have as close to that as is humanly possible, in an international scientific community that honestly believes that and preaches it and practices it (albeit fallibly and humanly, with less than total perfection). There exists, however, no impersonal algorithm for measuring the merits or worth of an idea. At the moment, it is the consensus of the scientific community, particularly of its elite, that judges the merits and the worth; and, over the long haul, that consensus is kept honest by the tests of nature and of time. What alternative is there? A jury of science writers perhaps?

"Market forces of supply and demand should be introduced wherever possible into the game of academic publishing," suggest Broad and Wade. Yet it is precisely a market that is now in action. In any case, the proliferation of publishing does not fool the insiders, any more than Wall Street is fooled into equating the merits and worth of over-the-counter stocks with those of the blue chips. We know very well which journals are fly-by-night and which are not. Our tenure committees know what the rejection rates of the different journals are, and can distinguish efficiently between the book from Exposition Press and that from the Oxford or Princeton University Press. "Administrators should develop sophisticated means of reading and evaluating a research record, such as citation analysis ...," suggest Broad and Wade. Some of us administrators, they should know, have a relatively sophisticated understanding of the pitfalls of citation analysis; and some of us even prefer that such judgments be made by a consensus of an individual's peers rather than by an administrator, no matter how sophisticated are the latter's tools. Some of us believe in a republic of science, and would rather bear its imperfections than see judgments imposed from the outside, no matter on whose authority -- that of the government or of the mob or of the media.

Broad and Wade have gotten quite a deal wrong about science. But they have also got quite a lot right, and their book is even gripping in places, for some of the stories of fraud are fascinating vignettes of people caught in crisis. And Broad and Wade have attempted a task that is certainly important, and one that few others have attempted. I wish them many readers, but I fervently hope that those readers will be thoughtful and critical and discriminating as they read this book.

Notes

1. See, for example, Arnold Thackray's Presidential Address, which I conveniently read a few weeks before I read Broad and Wade; Arnold Thackray, "The Politics of Knowledge," *4S Newsletter*, Vol. 7, #4 (Winter 1982), 3-10.
2. James D. Watson, *The Double Helix*; of most interest now is the Norton Critical Edition (W. W. Norton, 1980), edited by Gunther Stent, which contains commentaries and reviews and the original papers from *Nature* as well as Watson's text.
3. Sinclair Lewis, *Martin Arrowsmith* (Harcourt, Brace, 1925).
4. William Cooper (pseudonym of H. S. Huff), *The Struggles of Albert Woods* (Jonathan Cape, 1952).
5. Nigel Balchin, *A Sort of Traitors* (English edition, 1949); published in the U. S. as *Who is My Neighbor?* (Houghton, Mifflin, 1950).
6. James Hilton, *Nothing So Strange* (Little, Brown, 1947).

Henry H. Bauer
College of Arts and Sciences
Virginia Polytechnic
Institute and State
University

K. Hufbauer. **The Formation of the German Chemical Community (1720-1795)**. Berkeley: University of California Press, 1982. vii + 312 pp.

Robert E. Kohler. **From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline**. New York: Cambridge University Press, 1982. ix + 399 pp.

The history of science was long seen as something like that of nineteenth-century America. Following a kind of manifest destiny, scientists pushed back the frontier and occupied territory that had been quite unknown, or merely in the idle hands of a few Apaches. But it can be seen as more like that of Europe, where the concern has been with many frontiers shifting with time. In this case the frontiers are between the various sciences, which emerge as something like independent states or uneasy confederations, and may grow or be swallowed up by more dynamic neighbours. The frontiers between the sciences are not natural divisions, but have grown up; and they are often found to be slightly different places in different countries at different times.

The two books under review study the emergence of sciences. Chemistry has a very long history, and nowadays has a firm status as a basic science; while Biochemistry is a much newer discipline, of somewhat more doubtful status. The history of chemistry has been written from various points of view: there have been nationalistic claims that Boyle or Lavoisier was its founder, and there is Partington's enormous History which describes the progress of the science in some detail but is a work of reference rather than compulsive reading. The assumption made is that the history of chemistry is a series of discoveries to which the names, and possibly also the addresses, of discoverers could be attached. In many cases this turns out to be possible, but few professional historians of science are now satisfied with this kind of activity. Scientists often like, or think they like, an approach to the history of science which shows heroes exploring objective reality; but to anybody who has served on a committee of a Faculty of Science this will not do as the whole story.

Owen Hannaway, in The Chemists and the Word (Baltimore, 1975) looked for the origin of modern chemistry about 1600 when the textbook of Libavius with its ideal of clear and unambiguous language came to replace the occult tradition of Paracelsians such as Croll, who believed that language should be resonant and reflect between man the microcosm and the world, the macrocosm. Chemistry thus became a science which could be taught, rather than something which had to be learned the hard way; it was information rather than wisdom. But clearly the information which Libavius communicated was rather different from that taught in later generations.

Arnold Thackray, in Atoms and Powers (Cambridge, Mass., 1970) and A.L. Donovan in his Philosophical Chemistry in the Scottish Enlightenment (Edinburgh, 1975) sought to place eighteenth-century chemistry in the context of the intellectual history of the time, connecting it with both Newtonian theory and the philosophy of the Enlightenment, and thus showing the intellectual constraints within which anybody working in chemistry had to operate. Since the Scottish chemists Cullen and Black were also professors in medical schools the medical dimension cannot be omitted; and the picture of chemistry as an autonomous and natural body of knowledge is blurred as we see chemists trying to adapt an astronomical world-picture to a science institutionally based in medicine in the world of David Hume and Adam Smith.

For the nineteenth century, we have studies of learned societies such as M.P. Crosland's The Society of Arcueil (London, 1967), and of a professional society, C.A. Russell, N.G. Coley, and G.K. Roberts', Chemists by Profession (Milton Keynes, 1977). This latter book is concerned with the Royal Institute of Chemistry, a body concerned with fees, professional qualifications, legal restriction of fields such as water analysis to its members, and other activities associated with a kind of white-collar trade union. The study marked the centenary of the Institute, which had been formed because the Chemical Society of London had not shown much interest in how its members might earn their daily bread. The two societies have now merged, and this presumably reflects an acceptance that chemistry is a livelihood and a body of techniques exploitable by society as well as a fascinating research activity.

Such studies are necessarily connected with one time and place. More recently, however, a series of comparative studies edited by M.P. Crosland, The Emergence of Science in Western Europe (London, 1975), raised questions of why science developed in different directions and at different speeds in different countries at various times. It has long been appreciated that various sciences have flourished particularly in one country in some period, like chemistry in Germany in the later nineteenth century; that important discoverers have often become aware of the scientific traditions of more than one country; and that institutional models like the peripatetic annual meetings of the scientists of a country, or the research laboratory in a university, have been copied in other places.

One such transfer was the British Association for the Advancement of Science, and in Gentlemen of Science (Oxford, 1981). J. Morrell and A. Thackray used the early years of this society to identify the scientific community in Britain in the 1830s and '40s and the questions of status and patronage that went with it. Actual science of enduring importance was done under British Association auspices, but that was not the only interesting feature of it. If science is public knowledge it must involve a community - it is a social as well as a practical and intellectual activity, as I tried to set it out in my Nature of Science (London, 1976).

In his book Karl Hufbauer is looking at something rather different; at the formation of a chemical community - that is, a group of people aware of themselves as chemists, and in some kind of contact with each other even if this is limited to reading the same journal. His group was also conscious of itself as German; and since Germany at this time was not a state, the German experience was bound to be very different from that of the French or the British. The country was also confessionally divided, and the Catholic and Protestant universities were rather different kinds of institutions, especially in their attitudes to government and self-help.

Hufbauer's book abounds in tables, and has a prosopographical appendix of 'biographical profiles' as well as brief notes on the histories of the various institutions with which his chemists were associated. It is also well-written, and should have a general appeal way beyond the rather small circle of those deeply concerned with its place and people. He includes a lifespan table of the kind that made Joseph Priestley famous, and also provides maps which help the reader to see the growth of this chemical community. The trouble with tables is that the conclusions cannot be any firmer than the data, and lists of appoint-

ments in chemistry in the eighteenth century are bound to be a little fuzzy. Chemists were in the process of dissociating themselves from medicine, but the process was not completed until Liebig's time, and institutions did not clearly reflect the independence of chemistry. One has therefore to accept the author's judgement as to how chemical a particular post is. Nevertheless, there seems little doubt that there was an expansion.

Because of the excitements of pneumatic chemistry and of the antiphlogistic theory, we are all probably more familiar with the names of French and British chemists from this period. Wiegand, Crell, Hermbstaedt, Klaproth, Gren and Westrumb are not household names, although Stahl (to complete the list of those with portraits here) might count as such; and Hufbauer's story is not concerned with towering intellectual achievement or great originality. What we are presented with is evidence of growing moral support for chemistry as its public image improved and its promises of usefulness seemed more plausible; and then its growth into a discipline, with a fair measure of agreement concerning its aims, its theory and its practice, and some consensus about who was, and was not, a chemist. We are then given some account of the breakup of this community under the impact of Lavoisier's new theory.

The argument is that the agent which brought about the crystallisation of the community was a journal: Crell's Annalen. This gives a valuable perspective for examining the history of this new community. Hufbauer has assembled lists of subscribers, and traces the journal in its progress from a six-monthly to a three-monthly and then monthly appearance, and from selling through booksellers to direct subscription. He describes Crell's objectives: publishing original comprehensible papers, translations, reviews, and perhaps research-proposals; and shows how they worked out. These aims do not seem very extraordinary or original, and they are not very different from those of later editors of chemical publications, such as William Crookes. But to find the universal in the particular is one of the tasks of the historian.

Hufbauer believes, and produces evidence from publication and citation counts to support it, that the journal defined the German chemical community, but that under pressure as Lavoisier's theory began to gain ground, at first amongst those of cosmopolitan tendencies, the journal began to be the vehicle of the phlogistonists. Instead of representing a discipline, it became the mouthpiece of a sect - a German chemical community, rather than the German chemical community. Hufbauer applies a Kuhnian analysis to the chemical revolution in Germany. While there were some conversions, the pattern seems to have been that distinguished members of Crell's community were left high and dry. Later German chemical communities were hardly direct descendents of this first one, although Crell's success paved the way for others, and perhaps began the slow process by which chemistry moved from medicine through pharmacy to become a pure science, a branch of Wissenschaft.

This is the point at which Kohler begins his story. His problem is that while biochemistry had its beginnings in Europe in the nineteenth century, and its superstars there in the earlier twentieth century. it became institutionalised in the USA. He begins with a survey of developments in Germany and in Britain, which is interesting though one is alarmed to find the group Oxford, Belfast, Aberdeen, Cardiff, Sheffield and Leeds referred to as 'the smaller provincial universities of the midlands'! The thesis is that in Europe what we would call biochemistry had its origins in physiology. Especially in Britain, the divorce

of medicine from chemistry had become complete, and chemical physiology was seen, as appointments showed, as a part of physiology. There was a sharp break in medical courses between the academic sciences such as physiology and the practical clinical training; and so chemical physiology was kept distinct from pathology, pharmacology, and hygiene.

In the USA at the end of the nineteenth century the situation was quite different because medical schools were mostly low-grade proprietary institutions without university links, and accepting students without university training. Kohler then describes the revolution in medical training which took place in the early years of the present century, in which the practitioner-teacher was replaced by the researcher-teacher as medical schools became graduate institutions with close university connections. There had been little physiology in universities in the USA, and some medical chemistry had formed a part of the curriculum of the medical schools. Under the new system, with new enthusiasm for laboratory instruction, biochemistry found a place among the clinical sciences. Here, as the medical schools picked up again in student numbers and prestige, and as charitable foundations came to favour long-term research, biochemistry could find a reasonably secure place in which to develop as a discipline.

Reasonable security was about the best that could be hoped for, because this study brings out the 'touch and go' nature of institutional history. Promising departments have been closed in economy drives, famous institutions have teetered on the brink of closure, good teachers and researchers have been sacked, and excellent teams have broken up. The story told by Kohler covers a number of institutions, and the reader may feel that he has learned more than he wanted to know about faculty politics as the same kind of pattern is repeated. But no doubt this does bring out the 'underlying structural reasons' behind the 'chance events and personalities'.

Placing biochemistry in medical schools gave it an applied science ethos which meant that it was kept clear of chemistry, institutionally. Kohler drily points out the difference between 'ideals of intellectual affinity' and 'realities of departmental power'. Biochemists duly formed their society, ran their journal, and built up established schools. But by the 1950s they had come to seem plodders to the new 'molecular biologists' coming out of the pure sciences. Biochemistry failed to become one of the most intellectually exciting of sciences, and no doubt this was a feature of its institutional position.

These two books must indicate that history of disciplines has now arrived, and that it can deliver some kind of goods. A problem is that the science both authors describe is somewhat humdrum. No doubt most science is, and no doubt historians and philosophers of science have concentrated on the great intellects and the revolutions and neglected the administrators, the educators and the entrepreneurs. The study of disciplines is like the study of provincial scientific institutions in that it shows us the average and the normal, and reminds us that science is a very human activity: one, that is, which cannot be carried on without the support of a community and institutions, which will then help to determine how the science develops, and what its relations with other disciplines will be. The more different perspectives we can get of the history of science the better we shall see it; and disciplinary history is a good viewpoint.

David Knight
Department of Philosophy
University of Durham

G. C. Smith. **The Boole-DeMorgan Correspondence 1842-1864.** Oxford: Oxford University Press, 1982.

A great deal about nineteenth century scientific life can be discovered, and only discovered, by reading scientific correspondence. It is always a boon to have a coherent presentation of a well-defined piece of scientific correspondence like that G. C. Smith has provided in his little volume The Boole-DeMorgan Correspondence 1842-1864. Boole and DeMorgan were two of the most original logical thinkers of the mid-nineteenth century. Their correspondence ranges over issues in both logic and probability theory, and provides important glimpses into the development of their ideas.

In addition, the letters provide a personal picture of the two men. When their correspondence begins they hold very different positions in the world, the Cambridge-educated DeMorgan being well into his second decade of mathematics teaching at University College, whereas Boole is a virtually unknown, self-educated teacher in a small Irish town. Despite these significant disparities, and the fact they only rarely met each other, their correspondence flourished, bolstered by their common intellectual interests and, in time, by a real friendship. Their correspondence contains some 90 letters which not only cover important problems in logic and probability theory but which cohere to provide a sketchy picture of the lives of two nineteenth-century British scientists.

The continuity of the picture is greatly enhanced by the organization of the book. Rather than organizing the letters by topics, Smith has presented them chronologically letting them fall into reasonably coherent chapters determined by overall changes in their emphasis, lapses in the correspondence etc. Each chapter of anywhere from seven to eighteen letters is preceded by brief introductory remarks filling out some of the general biographical material about each man. Within each chapter, the letters are further divided into smaller groups each with a brief, sometimes technical introduction to the letters themselves. These introductions are sufficient to keep the endnotes to a minimum, a decent five or six per chapter, which contributes to the volume's readability.

This correspondence is obviously particularly important for those concerned with the development of logic, and probability. The editor is very helpful in interpreting technical details, including supplementary translations of archaic logical symbolisms, and an appendix with a proof of Boole's Theory on Definite Integration. Despite Smith's introductory remarks, the picture which emerges about 19th century scientific life remains sketchy. When Smith tries to take historical issues directly the discussion quickly degenerates into mush. So, for example, in his conclusion he writes "... making comparisons between well-educated persons of today and Boole and DeMorgan one cannot but observe that the latter had a better general education than comparable persons of the 1970s" (112). This historical weakness is perhaps inherent in the nature of this kind of work where editorial comments must be kept brief. In this form, the letters can serve as a valuable starting place for those integrated in developing a fuller picture of the scientific experience of Boole, DeMorgan and their compatriots.

J. L. Richards
Department of History
Brown University

Timothy Lenoir. **The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology.** Dordrecht, Boston, & London: D. Reidel, 1982. Vol. 13, Studies in the History of Modern Science.

Timothy Lenoir has written a book that has changed radically our perception of nineteenth century biology. Not since E. S. Russell's classic on Form and Function (1917) has an historian so comprehensively surveyed the German morphological and embryological literature of the first half of the last century. Russell's catalogue and exegesis of the factual material still remain unrivaled, and his thesis concerning a dialectic between an emphasis on form and one on function will continue to be valuable. But Lenoir has delved more deeply into the philosophical and methodological literature and has emerged with an account of a broad mechanistic and teleological tradition, which embraced some of the most influential German morphologists of the century. By tying together such familiar figures as Blumenbach, von Baer, Kiemeier, Johannes Müller, Schwann, Virchow and others into a single common movement, Lenoir has forced us to come to terms with the methodological core of German biology in the pre-Darwinian period.

Briefly put, and the most valuable historical theses are easy to put succinctly, Lenoir argues that Kant sanctioned teleological explanations for three generations of biologists. Not until some of the younger disciples of Müller, e.g., Du Bois Reymond, Helmholtz, and Ludwig, began attacking this particular Kantian core of German biology, and not until Darwin's mechanistic theory of evolution appeared on the scene, did this fruitful research tradition, spawned by the Critique of Judgment, come to a natural end. The thesis, so put, is not altogether novel. Bits and pieces appear in the secondary literature from time to time. What is unique in this new treatment is the systematic and massive way in which Lenoir demonstrates its outlines.

Also unique is Lenoir's artful dissection of the Kantian teleological tradition into a number of quite distinct components and generations. Readers of 4S REVIEW will find of particular interest Lenoir's inclination to tease apart and identify specific generational differences. This and other sub-themes are best indicated through a synopsis of the contents of the book.

In the first chapter Lenoir draws upon his earlier prize winning essay ("Kant, Blumenbach, and Vital Materialism in German Biology," ISIS, 1980, 71:77-108) to establish what he calls the tradition of vital materialism. Together, Kant and Blumenbach laid down the guidelines for a "regulative" use of any special biological force. Kiemeier, the influential anatomist from Stuttgart was not so restrained, but "... overstepped the valid limits of the concept of teleology as Kant had formulated it; he had begun to make a constitutive use of the Lebenskraft" (p. 53). In Chapter Two Lenoir describes what he calls a program of "developmental morphology." Consisting first of the student generation of the Empire, including Tiedemann, Meckel and Döllinger - stalwarts trained in Paris under Kiemeier's most influential student, Georges Cuvier - then consisting of the following generation, which included some of the most influential German biologists of the century, such as von Baer, Johannes Müller, Heinrich Rathke, and Rudolph Wagner, this program of developmental morphology promoted in various guises the constitutive use of a life force. Correcting a modern tendency to attribute all such teleological inclinations to Schelling and Oken, Lenoir repeatedly insists that none of these biologists with whom he deals should be identified with the parallel movement of romantic Naturphilosophie.

In the third chapter Lenoir follows the teleomechanistic tradition into the third generation, but his account is not simply a seriatim chronology. By examining in succession von Baer's discovery of the ovum, Schwann's cell theory, and Muller's deployment of the same theory, Lenoir focuses on an increasing trend toward locating the teleological potencies of the developing embryo or cell in "material structures capable of direct observation" (p. 136). Thus Schwann, Carl Vogt, and Rudolph Virchow took more seriously Kant's strictures against constitutive teleological agents.

Chapter Four, entitled "The Functional Morphologists" is in my view the most exciting chapter in the book. It pursues a parallel theme in physiology to the previous one in cytology. Lenoir describes how Justus Liebig, Hermann Lotze, Carl Bergmann, and Rudolph Leuckart, each in his own way recognized that the teleological dimension of life must be understood in terms of the organizational plan of the body rather than as a constitutive force. Rapid developments in organic chemistry and a greater appreciation for the integrated wholeness of the organism underscored their conclusions. In Chapter Five, entitled "Worlds in Collision," Lenoir presents a brilliant analysis of Helmholtz's physiological experiments that lead up to the conception of the conservation of energy. Some of Muller's most gifted disciples found in this generalization a guarantee that teleology, even in its regulative sense, must be banished from biology. Kant's Critique of Pure Reason became the guiding text for a reductionistic life science. Lenoir reviews some of the famous ensuing debates that took place as the two Kantian views collided.

In the final chapter, Lenoir examines the reaction of some of the foremost teleomechanists to the Darwinian theory of natural selection. This is a subject that could be extensively expanded upon in a separate work. Nevertheless, Lenoir masterfully reveals its outlines here by concentrating upon the considered reaction to natural selection by von Baer. As one of the most successful exploiters of the Kantian teleomechanistic tradition during the 1820s, it is enormously instructive to find the aged dean of German embryology keeping true to the Kantian teleological prescription nearly fifty years later - Evolution? yes! but not one founded upon the principle of chance.

The above thumbnail sketch does only scant justice to rich offerings of this important book. Lenoir has mastered and accounts for a staggering amount of primary material; he has found and deftly exploited some fine manuscripts, e.g., the von Baer-Leuckart correspondence; he has explicated and integrated into his story some of the most confusing methodological controversies of the century, and he has provided other historians with a securely constructed framework upon which to hang the history of nineteenth century biology. The reader will have to work for his rewards for the issues Lenoir deals with are complex. He might also be unhappy at the inordinate number of typographical errors in a scholarly book which costs nearly 20 cents per page. With perseverance, however, the reader will find this one of the most innovative and daring books to be published recently in the history of biology.

Frederick B. Churchill
Department of History and
Philosophy of Science
Indiana University

John Hubbel Weiss. **The Making of Technological Man: The Social Origins of French Engineering Education.** Cambridge, MA: MIT Press, 1982. xx + 377 pp., bib., index.

Leave your hotel in Paris, with its nineteenth-century façade and its prerevolutionary ambience. Take the "RER" train south on the Robinson line. Walk down the major motorway to Châtenay-Malabry. There, in Parisian suburbia, is the rather unimaginative, concrete architecture of what would pass for a minor-league, liberal-arts college in the United States. Young women run around the cinder track. Rock music blares from the dormitory blocks. Espresso coffee is served in the dining hall. It is the new campus of the elite finishing school in engineering and the sciences, the Ecole Centrale des Arts et Manufactures.

In The Making of Technological Man, John Hubbell Weiss, a professor in the History Department at Cornell University, tells us why a small training center for engineers should have such a peculiar title. It began early in Restoration France, when the bourgeoisie sought technical adepts for industry. The school (only the French would have christened an intended centerpiece of higher learning as a "school" tout court) emerged in Paris at a location distant from existing, sister institutions. The Ecole Centrale processed ingenieurs civils, civilian engineers. They were to be counterparts of the corps of military engineers produced by the Ecole Polytechnique and marked for government service. Professor Weiss takes us through the rhetoric behind the school's founding, its finances, the pedagogical expositions of its instructors, the daily routine of its students. His thesis is that in the school we see the emergence of an ideology for science industrielle, industrial learning, and with it the forging of a die for "technological man." The privately run school was to serve France as, saute-mouton fashion, the country leaped into the industrial age without experiencing the failures and the agony that accompanied Great Britain's technological rise. Technological Man is a book about makings, not about doings. It details ideology and motivations behind an evolving course of action.

Professor Weiss may be placed in a social-historical tradition that stretches back to the end of the nineteenth century, when German and French writers sought to characterize the social and economic preconditions behind scientific excellence. Among his more immediate predecessors are Karl-Heinz Manegold, Edward Layton, and Terry Shinn. Like them, the author is capable of brilliant metaphor. When discussing the emergence of secondary education in France, he expands on the accepted notion that, throughout Europe, universities preceded the schools that later came to prepare students for higher learning:

By the middle of the fifteenth century, the colleges had become autonomous centers of study. They thus took form as institutions not so much subsequent to elementary education as they were prior to higher education. They grew as stalactites, not stalagmites. Not until the middle of the twentieth century did they merge to any substantial degree with the primary education that had been founded to instruct the masses on the floor of the social cave (pp. 27-28).

The great strength of this book lies in its vigorous style. Especially in the opening, background chapters on French industrial needs and secondary-school teaching, the prose is limpid and compelling. Statistics and lists inobtrusively find their way into the narrative. The text easily survives a tendentious foreword and dust-jacket panegyric by solicitous patrons.

France, of course, did not land all at once in the new industrial world. Science and technology foundered as one government cascaded disastrously into another. By 1900, French technical ability brought to mind not the tower of Centrale graduate Alexandre Gustave Eiffel but rather the Panama disaster of Ferdinand de Lesseps. Into the middle of the twentieth century, French technology would evoke the recriminations of Philip Wylie and Edwin Balmer's maverick French scientist as he contemptuously dismissed his colleagues' plans for a new rocket: the fools! it would never fly! The Centrale shares some of the blame for a failure to produce innovative engineers who could transform French industry. At the end of his book, Professor Weiss addresses why the Centrale failed. Yet detail in the first half of the book does not really prepare a reader for the conclusion. I missed, at any rate, archival material in the form of private letters which might have revealed more about the broader ambitions of Centrale teachers: desire to rise to an academy membership, university chair, or industrial directorship; and especially expressions of pride or embarrassment at being affiliated with the school. I wonder if the Centrale gave rise to the incapacitating emotions--frustration, jealousy, depression, shame--endemic among talented faculty marooned at less than first-rate institutions.

The text, incisive and convincing on the level of prosopography, is less sure when treating scientific content. Examples dealing with the writings of two Centrale professors come to mind. When discussing a course proposed by Phillippe Benoît at the time that "initial plans for the school were being made," Professor Weiss identifies Benoît's survey of simple machines as "the familiar classifications of Reuleaux" (p. 98). The technological taxonomist in question is the great German professor of engineering Franz Reuleaux; he was born in the same year that Centrale opened its doors. The reference to Reuleaux seemed at first to come from an authorless book. Only after failing to find the title in the multi-subject bibliography did I read the notes sequentially to discover that an undated manuscript is under consideration. Several pages further along, Eugène Péclet's remarks of 1847 on scientific method are held to be "a fairly conventional statement of what ordinary mid-nineteenth-century scientists thought they were doing" (p. 106). It is more than noteworthy that, in his treatise on physics, Péclet advocates a Baconian-like philosophy of fact-gathering. Hypotheses played a minor role in his cosmology. No indication seems to be present of the bold, Laplacian ideal and its successor programs to interpret nature by short-range forces; no intimation of the hypothetico-deductive method, which would soon sweep over physics; no thought that the results from a number of experimental runs or observations could be synthesized by the method of least squares.

For a historian of science, these borderline calls, appearing as parenthetical and inessential digression, encourage dark thoughts about the viability of his field. Social historians often insist that historians of science demonstrate awareness of the latest word on literacy levels and social mobility. When a social historian alludes to the history of physics, however, he seldom seems to feel an analogous urging to familiarize himself with the available secondary material. The full range of writings by historians of science is far from apparent here. I wonder how much better Technological Man would have turned out if the manuscript had been reviewed by Terry Shinn, George Weisz, Gerd Schubring, Kenneth Caneva, John Hedley Brooke, Robert Fox, Mary Jo Nye, Geoffrey Cantor, Henry Guerlac, or Maurice Crosland.

Professor Weiss has written a fine book, but the reader's progress through it is slowed by decisions taken in the offices of MIT Press. The text is set in an unappealing font called VIP Trump. In what has the appearance of a gaffe, all pagination is located on the upper-left-hand corner of the page and the left-hand page margin is twice the width of the right-hand one--regardless of whether the page is left- or right-facing. Across fifty-four pages of endnotes, the note number stands alone on one line; its content follows on the next line. Professor Weiss's is a book where arguments depend for their effect on source documents. It deserved footnotes. Publishing houses must be persuaded that relatively little cost is incurred by hiring a free-lance paste-up assembler at the stage of page proofs; in a serious, scholarly book, footnotes are as important as acid-free paper. The publisher on the Charles River no doubt considered all these points. Chacun a son goût.

Lewis Pyenson
 Institut d'histoire et
 de sociopolitique des
 sciences
 Université de Montréal

FOR COMPLETE PROGRAM INFORMATION ON THE
 MEETINGS IN BLACKSBURG, PLEASE LOOK AT SPECIAL PROGRAM INSERT

*****Synoptic Schedule*****

7:30 pm **4S Council Meeting (dinner)**

Session VIII: Are Theories of Scientific Change Relevant to Technology
Presider: Susan Cozzens, National Science Foundation

Saturday, 5 November

- 7:00 am - 9:00 am 4S REVIEW Editorial Board Breakfast Meeting
- 8:00 am - 12:00 am **Registration & Limousine Reservations**
- 9:00 am - 11:00 am **Session IX: Studies of Technology --A [Poster Session]**
President: Ron Westrum, Eastern Michigan University
- Session X: Nuclear Weapons and Public Policy**
President: Linda Lubrano, American University
- 11:00 am - 12:00 am **Film: "How Much is Enough?" [also shown at 5:30 pm]**
- 12:00 am - 1:00 pm -----Lunch-----
- 1:00 pm - 3:00 pm **Session XI: Studies of Technology --B [Poster Session]**
President: Ron Westrum, Eastern Michigan University
- Session XII: Funding and the Growth of Scientific Specialties**
President: John C. James, National Institutes of Health
- 3:00 pm - 3:30 pm -----Coffee Break-----
(Sponsored by Department of History, VPI & SU)
- 3:30 pm - 5:30 pm **Session XIII: Issues in the Comparative Analysis of Science-
Related Controversies**
President: James Petersen, Western Michigan University
- Session XIV: Behavioral Technology and Social Control**
President: James Capshaw, University of Pennsylvania
- 5:30 pm - 6:30 pm **Film: "How Much is Enough?" [Repeat showing of 11:00 am]**
- 6:30 pm - 7:30 pm -----Cocktail Hour-----
(Sponsored by Graduate School, VPI & SU)
- 7:30 pm -10:30 pm ---4S Awards Banquet and Business Meeting---

Sunday, 6 November

- 8:00 am - 9:00 am SOCIAL PSYCHOLOGY OF SCIENCE Subgroup Breakfast Meeting
- 9:00 am - 10:30 am **Session XV: The Evolution of a Scientific Field as Shown
in Three Decades of Systematic Zoology**
President: Thomas Nickles, University of Nevada-Reno
- 10:30 am - 10:45 am -----Coffee Break-----
(Sponsored by Sociology Department, VPI & SU)
- 10:45 am - 12:30 pm **Retrospective on the Development of Science Studies**
President: Arnold Thackray, University of Pennsylvania
- Panel:** Thomas Kuhn, Massachusetts Institute of Technology
Bernard Barber, Columbia University
John Ziman, Imperial College of Science and Technology
- 12:30 pm -----Meetings Concluded-----

[The Complete Program with Session Papers Listed Begins on Next Page]

SOCIETY FOR SOCIAL STUDIES OF SCIENCE

8th Annual Meeting, 3-6 November 1983

Blacksburg, Virginia

Thursday, 3 November

7:30 pm 4S Council Meeting (dinner)-----*Smithfield Room*

Friday, 4 November

8:00 am - 3:00 pm **Registration & Limousine Reservations----***Marriott Lobby*

9:00 am - 9:30 am **Welcome-----***Olin-Preston Room*

Dean Henry Bauer
College of Arts and Sciences
Virginia Polytechnic Institute &
State University

Announcements

4S President Arnold Thackray;
Program Committee;
Local Arrangements Committee

9:30 - 11:30 am

Session I-----*Montgomery Room*

TAKING SCIENTIFIC PRACTICE SERIOUSLY

Presider: Karin Knorr-Cetina, Department of Sociology, Wesleyan University

**Sharon Traweek, Program in Anthropology and Archeology and Program in
Science, Technology and Society, Massachusetts Institute of Technology
"Laboratory Practice in High Energy Physics"**

**Joe Rouse, Department of Philosophy, Wesleyan University
"Science as Social Practice: A Philosophical Interpretation"**

**Karin Knorr-Cetina, Department of Sociology, Wesleyan University
"Studying the Laboratory"**

Discussants:

**Deborah C. Mayo, Department of Philosophy, VPI & SU
Nicholas Mullins, Department of Sociology, Indiana University**

9:30 - 11:30 am

Session II-----Smithfield Room

THE INSTITUTIONAL SUPPORT OF SCIENCE

Presider: Thomas F. Gieryn, Department of Sociology, Indiana University

Stephen Turner, Department of Sociology, University of South Florida
"Patronage and Trust in Science"

Harry Collins, Science Studies Centre, University of Bath
"Scientific Knowledge and Science Policy: Some Forseeable Implications"

Donald Fisher, Department of Social and Educational Studies, University of
British Columbia
"The Development of the Social Science Research Council: Integration as Science"

Thomas F. Gieryn, Department of Sociology, Indiana University
"Political Ecology of the National Science Foundation: Do the Social Sciences Belong?"

11:30 am - 1:00 pm -----Lunch-----

1:00 - 3:00 pm

Session III-----Montgomery Room

CURRENT RESEARCH IN THE PSYCHOLOGY OF SCIENCE

Presider: Ryan Tweney, Department of Psychology, Bowling Green State University

Joshua Klayman, Graduate School of Business, University of Chicago
"Processes of Discovery and Learning in a Probabilistic Environment"

Michael Doherty, Department of Psychology, Bowling Green State University
"Toward a Psychological Demarcation Rule"

Hillel J. Einhorn and Robin M. Hogarth, Graduate School of Business,
University of Chicago
"A Theory of Diagnostic Inference: Judging Causality"

Leslie Kern, Department of Psychiatry, The Ohio State University
"The Role of Data Error in Inducing Confirmatory Inference Strategies in
Scientific Hypothesis Testing"

James F. Voss, Learning Research and Development Center, University of Pittsburgh
"Social Science Problem Solving: Similarities and Differences to Problem
Solving in Other Disciplines"

1:00 - 3:00 pm

Session IV-----Smithfield Room

KEPT SCIENCE: INSTITUTIONAL INFLUENCES ON SCIENTIFIC RESEARCH

Presider: Daryl Chubin, Department of Social Sciences, Georgia Institute of Technology

Gary Bowden, University of Calgary

"The Structural Basis of a Scientific Controversy: The Case of U. S. Crude Oil Estimates"

Ron Johnston, University of Wollongong

"Kept Science: Credit Ratings in the Knowledge Business"

Dhirendra Sharma, Jawaharlal Nehru University

"India's Nuclear Estate: Nuclear Weapons and Public Policy"

Discussant:

Ellsworth Fuhrman, Department of Sociology, VPI & SU

3:00 pm - 3:30 pm

-----Coffee Break-----

Sponsored by Department of Philosophy, VPI & SU

3:30 - 5:30 pm

Session V-----Mongtomery Room

CAREER CONTINGENCIES AND PROBLEM CHOICE IN SCIENCE

Presider: John Ziman, Department of Social and Economic Studies, Imperial College of Science and Technology

Susan E. Cozzens, National Science Foundation

"The Internal Structure of Problem Sets: A Closer Look"

Stewart Gillmor, Department of History, Wesleyan University

"Geographic, Intellectual, and Social Mobility in the History of a Scientific Community"

Ian Lubek, Psychology Department, University of Guelph

"Social Psychology Applied to the History of Science: Publish or Perish in 'fin de siècle' France--the Case of Augustin Hamon and George Bernard Shaw"

John Ziman, Department of Social and Economic Studies, Imperial College of Science and Technology

"Problem Choice in Quasi-Academic Science"

3:30 - 5:30 pm

Session VI-----Smithfield Room

TOWARD A NATURALISTIC PHILOSOPHY OF SCIENCE

Presider: Thomas Nickles, Department of Philosophy, University of Nevada-Reno

Andrew Lugg, Department of Philosophy, University of Ottawa

"Well-Reasoned Belief and Its Social Explanation"

Ronald Giere, Department of History and Philosophy of Science, Indiana University

"Toward a Unified Theory of Science"

Sal Restivo, Science and Technology Studies Division, Rensselaer Polytechnic Institute

"The End of Epistemology"

Howard Smokler, University of Colorado

"The Complex Norms of Science"

5:30 - 6:30 pm

-----Cocktail Hour-----Olin-Preston Room

**Sponsored by Center for the Study of Science
in Society, VPI & SU**

6:30 - 8:00 pm

-----Break for Dinner (Individuals' Free Choice)-----

8:00 - 10:00 pm

Session VII-----Montgomery Room

THE GENERATION AND USE OF SCIENTIFIC LITERATURE

Presider: William Snizek, Department of Sociology, VPI & SU

Charles Bazerman, English Department, Baruch College, CUNY

**"Physicists Reading Physics: Purposes and Schema in Selection, Understanding, and
Evaluation"**

Katherine W. McCain, School of Library and Information Science, Drexel University

"Mapping the Dynamic Structure of Macroeconomics"

Greg Myers, English Department, University of Texas-Austin

**"The Uses and Limits of Rhetoric: Two Case Studies of the Composing Processes
of Biologists"**

F. C. H. D. Van Den Beemt, Foundation for the Technical Sciences, Utrecht

"Peer Review and Evaluation"

8:00 - 10:00 pm

Session VIII-----**Smithfield Room**

ARE THEORIES OF SCIENTIFIC CHANGE RELEVANT TO TECHNOLOGY

Presider: Susan Cozzens, National Science Foundation

Rachel Laudan, Center for the Study of Science in Society, VPI & SU
"Cognitive Change and Social Structure in Science and Technology"

Peter Weingart, Department of Sociology, University of Bielefeld
"The Structure of Technological Change: Reflections on a Sociological Analysis of Technology"

Edward Constant, Department of History, Carnegie-Mellon University
"Kuhnian Communities and External Social Structure and Dissemination of Petroleum Engineering Knowledge"

***** ***** *****

Saturday, 5 November

7:00 am - 9:00 am 4S REVIEW Editorial Board -----**Stone Room**
(Breakfast Meeting)

8:00 am - 12:00 am **Registration & Limousine Reservations**----**Marriott Lobby**

9:00 - 11:00 am

Session IX-----**Montgomery Room**

STUDIES OF TECHNOLOGY--A [POSTER SESSION]

Presider: Ron Westrum, Department of Sociology, Eastern Michigan University

Trevor Pinch, Department of Sociology, University of York (UK)
"A Unified Approach Towards the Sociology of Science and Technology--The Sociology of Facts"

Wiebe Bijker, Technische Hogeschool Twente
"A Unified Approach to the Sociology of Science and Technology--The Sociology of Artefacts"

Donald MacKenzie, University of Edinburgh
"Technology, the State and the Strategic Missile"

Peter Koefoed, Netherlands Universities' Joint Social Research Center
"Interdisciplinary Research: Problems and Solutions"

Ron Westrum, Department of Sociology, Eastern Michigan University
"What Happened to the Old Sociology of Technology?"

9:00 - 11:00 am

Session X-----Smithfield Room

NUCLEAR WEAPONS AND PUBLIC POLICY

Presider: Linda Lubrano, School of International Service, The American University

Frances McCrea and Jerry Markle, Department of Sociology, Western Michigan University
"Minutes to Midnight: Four Decades of Scientific Opposition to the Bomb"

Michael Altimore, Division of Social Sciences, Mount Mercy College
"Policy Perspectives in Nuclear Weapons Decisionmaking: Justifications From Within"

John Dowling, Department of Physics, Mansfield University of Pennsylvania
"Influence or Impotence? Scientists and Nuclear Weapons Policy"

Discussants:

Judy Reppy, Center of International Studies Peace Studies Program, Cornell University
Spencer Weart, Center for History of Physics, American Institute of Physics

11:00 - 12:00 am

**FILM: "HOW MUCH IS ENOUGH?" [A Film on nuclear weapons and
foreign policy; also shown at 5:30 pm]-----Board Room**

12:00 am - 1:00 pm -----Lunch-----

1:00 - 3:00 pm

Session XI-----Montgomery Room

STUDIES OF TECHNOLOGY--B [POSTER SESSION]

Presider: Ron Westrum, Department of Sociology, Eastern Michigan University

William L. Ziglar, Eastern College
"The Moon Race: Why the U. S. Won"

Wesley Shrum, Department of Sociology, Louisiana State University
"Participation, Elite Status, and the Evaluation of Technical Subfields"

Joop Schopman, Department of Philosophy of Science, Utrecht
Semiconductor Development in the Netherlands--1955: A Study of Its Determinants"

Edward J. Woodhouse, Rensselaer Polytechnic Institute
"The Speed of Technological Change: A Policy Analysis"

[List of papers for Session XI cont'd--]

[Session XI, continued]

John Wilkes, Social Science and Policy Studies, Worcester Polytechnic Institute
"NASA's Great Computer Debate or Technology and the Politics of Space Science"

Britta Fischer, Emmanuel College
"Engineers at the Center or Periphery of Technological Progress?"

H. Gill Peach, Hood River Project
"The Hood River Conservation Project: A Model for Consensus-Building in Applied and Energy Research"

1:00 - 3:00 pm

Session XII-----Smithfield Room

FUNDING AND THE GROWTH OF SCIENTIFIC SPECIALTIES

Presider: John C. James, National Institutes of Health

Steven F. Cohn, Department of Sociology, University of Maine
"The Effects of Funding Changes upon the Rate of Knowledge Growth in Algebraic and Differential Topology"

Thomas F. Gieryn, Department of Sociology, Indiana University
Joseph N. Tatarewicz, National Air and Space Museum
"Federal Funding and Planetary Astronomy, 1950-1975"

C. Stewart Gillmor, Department of History, Wesleyan University
"Federal Funding and Knowledge Growth in Ionospheric Physics, 1945-1981"

Carl D. Douglass, John C. James, Donald T. Disque, and Janet J. Bartch, Division of Research Grants, National Institutes of Health
"NIH Support of Research Investigations by the 'Phage Group,' Forerunner of Molecular Biology"

Charles Upton Lowe, National Institutes of Health
"Measuring the Growth of Knowledge in the Biomedical Sciences"

Henry Small and Edwin Greenlee, Institute for Scientific Information
"Collagen Research in the 1970's"

3:00 - 3:30 pm

-----Coffee Break-----

Sponsored by Department of History, VPI & SU

3:30 - 5:30 pm

Session XIII-----Montgomery Room

**ISSUES IN THE COMPARATIVE ANALYSIS OF SCIENCE-RELATED CONTROVERSIES:
PANEL DISCUSSION**

Presider: James Petersen, Department of Sociology, Western Michigan University

Panelists:

Diana Dutton, Health Services Research, Stanford University

Gerald Markle, Department of Sociology, Western Michigan University

Allan Mazur, Department of Sociology, Syracuse University

Arie Rip, Chemistry and Society Program, State University of Leiden (The Netherlands)

3:30 - 5:30 pm

Session XIV-----Smithfield Room

BEHAVIORAL TECHNOLOGY AND SOCIAL CONTROL

Presider: James Capshew, Department of the History and Sociology of Science,
University of Pennsylvania

Stephen J. Cross, Johns Hopkins University

**"Discipline and Disciplines: Social Interests and Professional Strategies in
American Social Science, 1918-1940"**

Richard Gillespie, University of Pennsylvania

**"The Psychopathology of Industrial Life: Elton Mayo and the Prehistory of
the Hawthorne Experiments"**

James H. Capshew, University of Pennsylvania

"Muzak for the Masses: Psychology in Industrial Culture"

Jack D. Pressman, University of Pennsylvania

**"The Salvaged Soul: The Use of Psychosurgery to Return the Nation's Mentally
Ill to active Employment, 1942-1955"**

5:30 -6:30 pm

**FILM: "HOW MUCH IS ENOUGH?" [A Film on nuclear weapons and
foreign policy; also shown at 11:00 am]-----Board Room**

6:30 - 7:30 pm

-----Cocktail Hour-----Marriott Patio
Sponsored by Graduate School, VPI & SU

7:30 -10:30 pm

**4S AWARDS BANQUET -----Jacob's Lantern
AND
BUSINESS MEETING**

Sunday, 6 November

8:00 am - 9:00 am Social Psychology of Science Subgroup-----**Stone Room**
Breakfast Meeting

9:00 - 10:30 am
Session XV-----Smithfield room

**THE EVOLUTION OF A SCIENTIFIC FIELD AS SHOWN IN
THREE DECADES OF SYSTEMATIC ZOOLOGY**

Presider: Thomas Nickles, University of Nevada-Reno

David Hull, Department of Philosophy, University of Wisconsin-Milwaukee
"Thirty-One Years of Systematic Zoology"

Discussants:

Richard E. Blackwelder, former editor of Systematic Zoology

William B. Lacey, Department of Sociology, University of Kentucky

10:30 - 10:45 am -----Coffee Break-----
Sponsored by **Department of Sociology, VPI & SU**

10:45 am - 12:30 pm
General Session-----Smithfield Room

RETROSPECTIVE ON THE DEVELOPMENT OF SCIENCE STUDIES

Presider: Arnold Thackray, University of Pennsylvania

Panelists:

Thomas Kuhn, Massachusetts Institute of Technology

Bernard Barber, Columbia University

John Ziman, Imperial College of Science and Technology

12:30 pm -----Meetings Concluded-----

* * * * *

ATTENTION AUTHORS

* * * * *

DO YOU HAVE A BOOK TO DISPLAY ???

The 1983 4S meetings will include an exhibit of recent books relative to social studies of science. If you would like to bring any of your books to the attention of colleagues, please contact (or ask your publisher to contact) the organizer of the 4S book exhibit:

Harve C. Horowitz
10369 Currycomb Court
Columbia, MD 21044

Telephone: 301-997-0763

Inform him of the author's name, title of the book, and publisher.

IT IS STILL NOT TOO LATE TO PLAN TO ATTEND 4S MEETINGS IN
BLACKSBURG

FOR INFORMATION ON THE FOLLOWING--

Hotel Reservations

Pre-Registration

PLEASE SEE PAGES 42-46 OF 4S REVIEW, SUMMER ISSUE, #2

ANNOUNCEMENTS: FEDERAL AGENCIES

NATIONAL SCIENCE FOUNDATION

Ethics and Values in Science and Technology

November 1, 1983 is the next closing date for submitting preliminary proposals to the EVIST program.

This program supports research and related activities to improve public and professional understanding of the ethical and value aspects of contemporary issues that involve science and technology. Projects often focus on the roles of scientific and engineering research in their social applications. They are meant to clarify the ethical implications or value assumptions of those roles and to contribute to the formulation of sound policy about them. The program makes awards for collaborative research projects, individual professional development activities, and dissertation support.

For further information, contact

Rachelle Hollander
EVIST, NSF
Washington, D. C. 20550
202/357-7552

ANNOUNCEMENTS: RECENT CONFERENCES/SYMPOSIA

THE SCIENCE STUDIES COMMITTEE

Reported by
Steve Woolgar, Brunel University, UK

A recent initiative in support of Science Studies was a letter published in The Times on 13 January 1982. This letter pointed to the risk that small interdisciplinary subjects might suffer disproportionately in the overall budget cuts then being planned. This point was noted by the University Grants Council (UGC), and may have had some effect in moderating the cuts in individual universities.

On 31 March 1982, the situation was discussed at a meeting in London of individuals representing many departments in this area of studies. A small steering group was set up, representative of the various interests, institutions and regions involved, to carry these policies further. The Council for Science and Society agreed to support the work of this committee, with secretarial facilities and out-of-pocket expenses.

The Committee has established a network of "correspondents" in all the institutions of Higher Education where there are Science Studies curricula, and is monitoring the staff, student, and recourses situation throughout the country. A general assessment of the situation at the end of the 1981-82 session showed that there were not so many definite cuts as was first feared, but future prospects for staff employment, undergraduate admissions, research funds, material resources, etc., were still very worrying. The Committee will continue to give what help it can to groups in difficulty, and will also monitor national developments in the policies of the UGC, National Advisory Body (NAB), Research Councils, and other relevant bodies. It will also keep in close touch with the various Learned Societies in this field, both national and international.

In October 1982, the Committee prepared a short statement on the significance of Science Studies, emphasizing its broad scope and educational, vocational and scholarly value. A longer document, drawing attention to the contributions of the various disciplinary viewpoints to the subject as a whole, is in preparation and should be available early in 1983.

Meeting on 25 June 1983

One of the main deficiencies of Science Studies in Britain is the lack of a general meeting place for the community of scholars and teachers. The Committee therefore organized a one-day meeting in London on 25 June 1983 as a step in this direction. The attendance of more than 80 indicates there has been a favourable response to these initiatives, from individuals, from teaching departments, from national educational and research organizations, and from scholarly societies. The diversity of disciplinary interests represented at the meeting also suggests there is now wide support for more co-ordinated policies and actions in this field. The six speakers represented the History, Philosophy, Sociology, Politics and Economics of Science, and Technology.

Program of Speakers

Dr. John Irvine
Science Policy Research Unit, Sussex University
"A Framework for Research Evaluation in Basic Science"

Dr. Harry Collins
Department of Sociology, University of Bath
"Scientific Knowledge and Science Policy"

Professor Mary Hesse
Department of History and Philosophy of Sciences, Cambridge
"Epistemology Socialized"

Professor Colin Russell
Department of History of Sciences and Technology, Open University
"Chemical Elites in Victorian Britain"

Sir Bruce Williams
Technical Change Centre
"Are We in the Trough of a Kondratieff Cycle?"

Dr. Hillel Steiner

Department of Government, Manchester University

"Some Doubts about the Justifiability of Conservation Policies"

John Irvine presented some further applications of the recent and controversial Science Policy Research Unit (SPRU) model for evaluating science research. This time the focus was on the relation between the expenditure and research output of certain high energy physics institutes. Irvine's argument was that his method of partial indicators did no more than make available additional objective indicators of research performance to bodies outside the scientific research community. Harry Collins voiced the disquiet of some participants in suggesting that the SPRU evaluation technique presupposed a particular kind of competitiveness in scientific research. Rather than base evaluation and subsequent policy implications on a reward system model, an improved approach should perhaps take more account of the interdependence of researchers within a community of scientific knowledge. Mary Hesse offered an account of the modification of traditional epistemology at the hands of social studies of science. She suggested that we require further discussion of the way in which constructivist epistemology is used to demystify scientific knowledge. Colin Russell's discussion of the role of the scientific elite in Victorian science led to a general debate about the contribution of the history of science to science studies. Participants heard tentative evidence, based on the growth of science studies courses in schools and junior colleges, that both history and philosophy of science were having beneficial effects on the education of scientists.

The very eclectic program was interspersed with discussions as to how best to represent the various interdisciplinary interests in the event of a future round of cuts, most likely to be directed at polytechnics and colleges rather than universities. Various strategies were considered for making clear the undoubted importance of science studies to the National Advisory Body.

CANADIAN SOCIOLOGY AND ANTHROPOLOGY ASSOCIATION

Reported by

Trevor Pinch, University of York, UK

The 18th Annual Meeting of the Canadian Sociology and Anthropology Association was held at the University of British Columbia, Vancouver, June 1-4, 1983. The three sessions concerned with sociology of science and sociology of knowledge indicates that the field is at least maintaining a presence in Canada. As usual it is the story of a few isolated individuals with the necessary energy and enthusiasm. In this case special thanks are due to Gus Brannigan and Gary Bowden of the Sociology of Department of Calgary University who were responsible for much of the organization of the sessions. The papers were generally of high quality and reflected the diverse themes to be found in current sociology of science. One of the most encouraging aspects of the sessions was the attendance of several sociologists working in other fields. This was in marked contrast to, say, the annual meeting of the British Sociological Association where the study of British sociology of science continues to be one of "separate development." The warm homely atmosphere of the meeting was encouraged by the ambience of the UBC campus ("Lotus Land" - as a participant from Calgary remarked). One

disappointment was that more foreign participants could not get the travel funds to attend the meeting and present their papers. If the CSAA really wants to open up Canadian sociology to foreign participation it must be prepared to offer some financial incentives.

The program was as follows:

Social Studies of Scientific Practice

Chair: Gary Bowden (University of Calgary)

Papers:

Michael Lynch (UCLA)

"Aspects of Visual Documentation in Scientific Texts"

G. Ross Baker (University of Alberta)

"The Social Organization of Biomedical Research Specialties, Laboratories and Research Teams"

Linda J. Muzzin (McMaster University)

"A Blockmodel Analysis of Another Research Specialty: Clinical Reasoning"

Discussant: Richard W. Hadden (McMaster University)

Sociology of Knowledge

Chair: Augustine Brannigan (University of Calgary)

Papers:

Volker Meja (Memorial University) and Nico Stehr (University of Alberta)

"Towards a Critique of the Sociology of Knowledge"

Hilary Callan (Trent University)

"The Imagery of Sociobiology: A Case Study in the Anthropology of Knowledge"

Steve Woolgar (Brunel University)

"Irony in the Social Study of Science"

Discussant: Pauline Vaillancourt (Universite du Quebec a Montreal)

The Sociology of Science

Chair: Augustine Brannigan and Gary Bowden (University of Calgary)

Papers:

Trevor Pinch (University of Bath)

"The Social Construction of Experimental Knowledge in Physics"

Bryan Campbell (University of Lancaster)

"Uncertainty Arguments in Disputes Among Experts"

Aant Elzinga (University of Goteborg)

"Research Bureaucracy and the Drift of Epistemic Criteria"

Discussant: Ronald Schwartz (Memorial University)

Following the meeting two sessions on the sociology of science were held by the Discourse Analysis Research Group of the University of Calgary. Mike Lynch and Trevor Pinch both gave presentations.

E A S S T

Joint EASST/STSA Conference: CHOICE IN SCIENCE AND TECHNOLOGY

(Imperial College, University of London, 16 - 18 September 1983)

Programme

Friday 16 September 1983

10.00 Registration and coffee

11.30 Plenary Session 1: "Choice in Laboratory Life", Dr. Sydney Brenner,
FRS, MRC Laboratory of Molecular Biology,
Cambridge

12.30 Lunch

14.00 Plenary Session 2: "Choice in the Curriculum", Dr. Richard West,
Director, Secondary Science Curriculum Review

15.00 Tea

15.30 Plenary Session 3: "Choice in Science and Technology", Sir Herman
Bondi, Chairman, Natural Environment Research
Council

16.30 Plenary Session 4: "Choices in Energy Policy", Sir Francis Tombs,
Director, N M Rothschild & Sons Ltd. and form-
erly Chairman, Electricity Council, and South
of Scotland Electricity Board

17.30 STSA General Meeting

Saturday 17 September 1983

9.15 Plenary Session 5: "Choice in Industrial Life", Mr. Barrie Sherman,
formerly Research Director, Association of
Scientific, Technical, Managerial Staffs, and
author of 'Freedom from Work'

10.15 Coffee

10.30 Parallel Group Discussions 1

11.45 Parallel Group Discussions 2

13.00 Lunch

14.00 Parallel Group Discussions 3

15.15 Parallel Group Discussions 4

- 16.30 Plenary Session 6: "Choice of Scientific Theory", Professor Martin Rees, FRS, Kings College, University of Cambridge
- 17.30 EASST General Meeting

Sunday 18-September 1983

- 10.00 Plenary Session 7: "Choice for Science and Technology in the Media", Dr. Bob Young, Consultant, Crucible Series
- 11.00 Coffee
- 11.15 Critical Perspective Respondants
- 12.30 Conference Closing Session
- 12.45 Disperse

Approximately 50 contributed papers will be available for discussion. These will be split so as to produce about 16 sessions of group discussion during the Saturday. Details of the sessions and copies of the papers will be available at registration.

Authors and titles of contributed papers, arranged according to the provisional programme for the parallel group discussions, are given below.

Saturday, 10.30 - 11.45

Views of Choice I

- Ken Penney (Dept. of Economics, University of Exeter), The Economists Theory of Choice: A Critical Review
- Colin Divall (Dept. of Science and Technology Policy, University of Manchester), Choice Through Science? Scientific Planning and Middle-Opinion in the Thirties
- F.J. Carrillo (London School of Economics), Psychology of Science: A Matter of Choice
- Lesley Burr (Technology Policy Unit, University of Aston), Product Innovation and Consumer Choice

Education I

- Charles Boyle (Trent Polytechnic), Trends in STS Education
- John Dickson, Technician Education - Some Observations and Alternative Curriculum
- Geoff Beuret (Leicester Polytechnic), Choices in Engineering Education

Political Problems of Control

- David Collingridge (Technology Policy Unit, University of Aston), The Political Control of Technology

Gordon J. Lake and Russell A. Green (Newcastle-upon-Tyne Polytechnic),
A Holy Ground? Nuclear Waste Disposal, Test Drilling and the Cheviots
José van Eijndhoven (State University of Utrecht), Social Steering of
Potentially Risky Technological Developments

Establishment of Theory

- David Gooding (Humanities and Social Sciences, University of Bath),
Experience and Technique: Invention, Exemplar and the Representation
of Novel Electromagnetic Phenomena circa 1820
- Werner Callebaut (Logika en Kennisleer, University of Gent), Costs and
Benefits (Real and Presumed) of Reductionist Strategies in 19th and
20th Century Biology: A Cross-Disciplinary Approach

Saturday, 11.45 - 13.00

Views of Choice II

- D.J. Bennett, P.E. Glasner and G.D.L. Travis, Risks and Benefits in Gen-
etic Manipulation: A Novel Case of Choice by Self-Regulation in
Science
- Christien Enzing (State University of Utrecht), Choice in Biotechnology
- Carl Peters (Technology Policy Unit, University of Aston), Choice in
Drug Innovation Strategies
- Pieter Weeder (Technical University of Eindhoven), Variation and Select-
ion: Strategic Choices in Industrial Laboratories

Education II

- Koos Kortland (PLON, State University of Utrecht), How to Teach Science
- Leonardo Cannavò (Dept. of Sociology, Università La Sapienza, Rome), The
Social Conditioning of Science in Curriculum Choice and Laboratory
Tasks
- Shigeru Nakayama (Tokyo), PSSC, STS and Scientific Literacy: A Historian's
View

Hazards and Risks

- Alan Irwin (Dept. of Science and Technology Policy, University of Man-
chester), Risk Analysis: Whose Choice in Science and Technology
- Len Moore and Fred Steward (Technology Policy Unit, University of Aston),
Scientific and Social Theory as Inputs to the Construction of Expert
Advice on Risk
- Peter Groenewegen (University of Amsterdam), Uncertainty and Choices in
the Development of Toxicology
- Melanie Miller (Technology Policy Unit, University of Aston), Choice in
Mechanisms for Controlling Technological Hazards

Research Programmes

- Howard Smokler (Committee on Science Policy, University of Colorado),
The Complex Norms of Science
- Henk Zandvoort (Filosofisch Instituut, University of Groningen), Intrinsic
Success and Extrinsic Success of Research Programmes

Sjerp Zeldenrust (Science Dynamics Unit, University of Amsterdam),
Choice of Goals in Research Groups

Saturday, 14.00 - 15.15

Government Policy

Glynn Jones (Dept. of Science and Technology Policy, University of Manchester), Government Support for Marine Technology: Japan and a Brief Comparison to the UK, US

Stewart Russell (Technology Policy Unit, University of Aston), Choosing a Framework for Explaining an Absence: Whatever Happened to Combined Heat and Power?

Colin Reeve (Technology Policy Unit, University of Aston), Interaction of Theory Choice and Policy Choice - The Case of Smoking and Health

D.S. Horner (Technology Policy Unit, University of Aston), Freedom and Planning: The Politics of Science 1939-1949

Health Care and Medicine

M.B.H. Visser (Agricultural University of Wageningen), Technocratization of Choices in Decision Making on Biomedical Issues

Edward Yoxen (Dept. of Science and Technology Policy, University of Manchester), What Choice Do We Have with In Vitro Fertilization?

Tom Kitwood (School of Science and Society, University of Bradford), Science - Out of Touch? Reflections on the Use of Hands in "Alternative Medicine"

David Cantor, Choice in Cancer Research: Cytogenetics and History in the Early 1940's

Law, Science and Participation

David C. Purchase (Dept. of Science and Technology Policy, University of Manchester), The Choice of Forum for Scientific/Technological Disputes

Anna M. Garry (Dept. of Science and Technology Policy, University of Manchester), The Windscale Inquiry - A Choice to Participate?

Frank Steenkamp (University of Leiden), Processes of Problem Definition in Conservation Ecology

The Process of Choice

Geert Lameris (Delft University of Technology), Choice and Rationality (to be presented by Saul M. Lemkowitz)

Wiebe Bijker and Trevor Pinch (Twente University of Technology), Choice in the Constitution of Facts and Artefacts: Choice Mechanisms and Relevant Social Groups in Science and Technology

Vittorio Ancarani (University of Torino), Choices and Scientific Delay: A Center-Periphery Model

Saturday, 15.15 - 16.30

The Third World

Wolfgang Rudig (Dept. of Science and Technology Policy, University of Manchester), Nuclear Energy, Technological Dependence and Development: A Case Study of Technological Choice in the Third World
 Anja Wieberdink (Science Dynamics Unit, University of Amsterdam), The "Salvation of Lake Managua" in Nicaragua and the Role of Science

Morality and Defense Research

S.D. Veazey (Dept. of Science, Luton College of Further Education), An Ethical/Moral Dimension in Science and Technology
 Philip Gummatt (Dept. of Science and Technology Policy, University of Manchester), Defense Research Policy
 Graham Lewis (Technology Policy Unit, University of Aston), Control of Military Technology: The Development of the U.S. Ballistic and Cruise Missiles

Alternatives: Radical and Possible

D.A. Elliott (Technology Policy Group, Open University), Energy Options for the U.K.: The Alternative Paradigm
 Kenneth Green (Dept. of Science and Technology Policy, University of Manchester), Choices in Direction of Science and Technology in Europe: Reconstructing a Radical Programme
 Stewart Russell (Technology Policy Unit, University of Aston), Denial of Choice: Technological Determinism and Its Ideological Role

The Organisation of Work

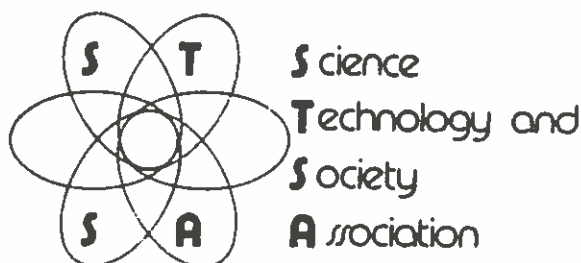
John Dickson (University of Bradford and Bradford and Ilkley Community College), Contextualizing Taylorism: A Contribution to the Labour Process Debates
 Paul Slaa (Planning and Policy Unit, State University of Utrecht), Flexible Production Automation: Taylorism Revisited
 Ian Jones (Technology Policy Unit, University of Aston), The Choice for Trade Unions

= = =

For information about the conference:

During the conference: Mrs. Elspeth Robinson, Department of Social and Economic Studies, 53 Prince's Gate, Exhibition Road, London SW7 2PG, Phone 01-589 5111 Ext. 2848.

Otherwise: Mrs. Maureen Cook, Faculty of Science, Napier College, Colinton Road, Edinburgh EH10 5DT, Phone 031-447 7070 Ext. 525



ANNOUNCEMENTS: FUTURE CONFERENCES/SYMPOSIA

**PRESIDENT FARKAS ANNOUNCES ISA-RC
SOCIOLOGY OF SCIENCE CONFERENCE, VARNA, SEPTEMBER 1984**

The Research Committee on Sociology of Science (RCSS) with the International Sociological Association and the Bulgarian Sociological Association (BSA) will organize the next RCSS conference in Bulgaria in 1984.

The Conference will take place in the Bulgarian city of Varna on the Black Sea coast from September 3 through 5, 1984.

The main theme of the Conference will be "New Trends in the Sociology of Sciences and Knowledge". "New problems of the relationship between science and society" is proposed as one of the possible subthemes.

BSA will bear the costs of the Conference - halls and living expenses (including full board and lodging) for members of the Board, as well as for all active members of the Research Committee. There is a possibility of inviting some representatives of other scientific bodies, e.g. the Society for Social Studies of Science, the International Council for Science Policy Studies, the European Association of Science, Society and Technology, the Board of the International Sociological Association, offering them the same support. Travelling expenses are to be covered by the participants.

The working language of the Conference will be English.

If you are interested in participation at the Conference, kindly inform us about your intention and the preliminary title of your paper. The deadline for application for participation is November 15, 1983. Circular letter No 2 containing the scientific programme of the Conference will be circulated by January 30, 1984.

To facilitate the organization of the Conference a National Organizing Committee is founded under the sponsorship of the President of the State Committee for Science and Technological Progress, Minister of the People's Republic of Bulgaria. This Committee intends to print in a separate brochure the abstracts of the papers included in the programme before the Conference, while the papers themselves will be published in a separate volume after the Conference.

For application please contact the President of the Research Committee at the following address:

Farkas János, Institute of Sociology
Hungarian Academy of Sciences
49, Uri Utca, H-1014, Budapest
Hungary

ANNOUNCEMENTS: AAAS PROJECT**AAAS OPENNESS IN SCIENTIFIC COMMUNICATION PROJECT**

Secrecy and openness in scientific and technical communication -- what are the effects on the conduct of scientific work?

The issue will be examined in a major research project of the American Association for the Advancement of Science's (AAAS) Committee on Scientific Freedom and Responsibility.

The AAAS project will address assumptions which form the basis for the concept of the right to open communication in science and the evidence available to describe the effects of restricted communication on scientific productivity.

The project will consist of ten background papers which will examine secrecy and openness from a variety of circumstances, including national security interests, industrial and commercial concerns, and individual efforts to restrict communication because of competitive interests, privacy, or quality control objectives. Authors of the ten background papers each will receive an honorarium of \$1000.

Papers will be presented and discussed in a series of seminars to be held during 1984 in Boston, Chicago, San Diego, Nashville, and Washington, D.C. After completion of the seminars, a selected group of papers will be published by SCIENCE, TECHNOLOGY AND HUMAN VALUES in a special issue. In addition, project papers will be presented in a symposium at the 1984 AAAS Annual Meeting in New York.

The project will seek to identify individuals from various disciplinary backgrounds who will be knowledgeable about the ethical issues involved in science and secrecy debates. These individuals can then be linked with professional society groups as well as other public and private organizations to develop criteria and procedures to resolve conflicts concerning openness and secrecy in scientific and technical communication.

Support for the project is being provided by the National Science Foundation and the National Endowment for the Humanities. Rosemary Chalk, program head of the AAAS Committee on Scientific Freedom and Responsibility, will serve as project director.

A call for papers for the project has been published in SCIENCE (2 September 1983). Persons who wish to submit papers for consideration should send a five-page (double-spaced) abstract to the AAAS Committee on Scientific Freedom and Responsibility, 1515 Massachusetts Avenue, N.W., Washington, D.C. 20005 no later than 15 October 1983.

Co-sponsors for the Seminars

Program on Science, Technology and Society, Massachusetts Institute of Technology (MIT)

Regional Host: Marcel LaFollette
Editor, Science, Technology and Human Values

Science, Technology and Public Affairs Program
University of California,
San Diego

Regional Host: Sanford Lakoff
Professor of Political Science

Program for the Management of
Technology, Vanderbilt University
Regional Host: Robert House
Program Director

Center for the Study of Ethics
in the Professions (CSEP)
Illinois Institute of Technology
Regional Host: Vivien Weil
Research Associate, CSEP

Advisory committee members guiding the development of the AAAS project are: Sissela Bok, lecturer on moral philosophy in the core curriculum at Harvard University and author of Secrets: On the Ethics of Concealment and Revelation; Loren Graham, professor of the history of science, MIT; Harold P. Green, professor of law, George Washington University; Lee Grodzins, professor of physics, MIT; Louis Menand, senior lecturer in political science and special assistant to the provost, MIT; and Eugene Skolnikoff, director, MIT Center for International Studies.

Further information about the project can be obtained from Rosemary Chalk at the above address, or call (202) 467-5238.

ANNOUNCEMENTS: POSITIONS AVAILABLE

ISIS: EDITORIAL ASSISTANTSHIP FOR 1984-85

DUTIES: The Editorial Assistant takes responsibility for the day by day administration of the Book Review section of ISIS. The Editorial Assistant also helps the Editor and his associates keep abreast of developments in the history of science and participates in every aspect of the work of the journal.

QUALIFICATIONS: The assistant must be an accepted candidate in the Ph.D. program in the History and Sociology of Science at the University of Pennsylvania. Previous experience in the history of science or in editorial work is preferred though not required.

STIPEND: Currently \$6,600 plus tuition. The assistantship is a one year, half-time position that begins on 1 July 1984.

FUTURE ROLE: It is hoped that in the second year the Editorial Assistant will succeed to the position of Editorial Coordinator (also half-time), for which the stipend is currently \$7,200. The Editorial Coordinator plays a major role in the production of ISIS (proofreading, layout, maintaining timetables) and in promotion, distribution and back issue sales.

APPLICATIONS: Candidates should first submit a letter outlining their academic experience and indicating the relevance of ISIS and the assistantship to their graduate work and future plans. Candidates will be sent the usual University of Pennsylvania application forms by which to seek admission to the Department of History and Sociology of Science. The candidate's initial letter (which should be addressed to the Editor) weighs heavily in the selection process. Applicants from all countries are welcome. The closing date is 1 February 1984.

RENSSELAER POLYTECHNIC INSTITUTE ASSISTANT PROFESSORSHIP

Tenure track Assistant Professor position for Ph.D. scholar with interests in science and technology policy issues. Strong preference for candidate with competence in quantitative methods. This position is funded through a grant from the Andrew W. Mellon Foundation for strengthening tenure track academic positions for scholars in applied interdisciplinary research. Candidates must show outstanding promise for funded research and be willing to work with others on interdisciplinary projects.

This position offers a stimulating environment, a minimum teaching load, ample travel funds, superb computer facilities. To apply, send a curriculum vitae and the names of three references to: Professor Shirley Gorenstein, Chair, Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, NY 12181. Deadline for applications: December 1, 1983. Rensselaer Polytechnic Institute is an Equal Opportunity employer with an Affirmative Action Program.

TEXAS A&M UNIVERSITY ASSISTANT PROFESSORSHIP

The Department of Sociology anticipates filling several tenure track position for Fall 1984 (or beginning as early as 16 January 1984). One position may be filled with a candidate having research interests in the sociology of science and technology. The Ph.D. should be completed before the date of appointment for Fall 1984. The successful candidate will be one who shows excellent potential for a productive career in research and teaching. Evaluation of candidates will be a continuous process but applications received before 1 November 1983 will be assured of full consideration. Applicants should send letter of application, vita, and should arrange for three letters of reference to be sent to: Jerry Gaston, Department of Sociology, Texas A&M University, College Station, TX 77843. Affirmative Action/Equal Opportunity Employer.

VIRGINIA POLYTECHNIC INSTITUTE AND STATE UNIVERSITY TENURED OR TENURE-TRACK POSITION

The Virginia Tech Center for the Study of Science in Society seeks to make a tenured or tenure-track appointment of a scholar with demonstrated research ability in social studies of science. The Center, an interdisciplinary department-level unit in the College of Arts and Sciences, supports research and graduate degree programs focusing on cognitive, social and political factors in the development of science and technology. Primary consideration in making this appointment will be given to the following criteria: (1) ability to complement the Center's current strength in history and philosophy by bringing the theoretical frameworks of the social sciences to bear on the study of science, and (2) readiness to engage with other members of the Center in the development of research and teaching programs in science and technology studies. Rank open, Ph.D. required. Send application and three letters of reference by December 1, 1983 to Rachel Laudan, Search Committee Chair,

Center for the Study of Science in Society, Virginia Polytechnic Institute and State University, Blacksburg, VA 24061. Equal Opportunity/Affirmative Action Employer.

STS RESEARCH FELLOWSHIPS AT MIT

The MIT Program in Science, Technology, and Society, with the support of the Exxon Education Foundation, invites applications for several one-year research fellowships on the relationships of science, technology, and society.

Selection criteria include:

- (a) a record of outstanding performance in a particular field of science, engineering, social science, or the humanities;
- (b) evidence of a commitment to research involving the interaction of science, medicine, or engineering with society;
- (c) a proposal of study and research for the fellowship year related to the Program's areas of research and teaching, which include:
 - Social and Historical Studies of Science and Technology
 - Technological Change and the Political Economy of Industrial Societies
 - Cultural Dimensions (e.g., ideological, aesthetic, ethical) of Science and Technology
 - Policy Studies involving Science and Technology

Preference this year will be given to the history of science.

Application should be made in a letter of about five double-spaced typed pages and a curriculum vitae. Additional material will be requested if necessary. Ph.D. degree or equivalent desirable. Ph.D.'s at all levels of professional career and foreign nations are eligible. Partial or full stipend available, normally not exceeding \$25,000. Stipend based on current salary. Senior candidates are encouraged to supplement stipends with other funds. Fellows are expected to reside in the Boston area, and appointments will begin in September 1984. Address application to:

Shawn Finnegan, Secretary
Exxon Fellowship Committee
STS Program, E51-128
M.I.T.
Cambridge, MA 02139

Deadline for receipt of applications: January 15, 1984.

MIT is an Equal Opportunity/Affirmative Action Employer.

ANNOUNCEMENTS: NEW APPOINTMENTS**MICHIGAN TECHNOLOGICAL UNIVERSITY**

Four historians of technology now have appointments in Science, Technology and Society at Michigan Technological University. Joining Mark Rose and Larry Lankton are W. Bernard Carlson and Terry S. Reynolds. Lankton now serves as Head of the Department of Social Sciences at MTU, and Reynolds has been appointed the new Director of STS, a program housed in the social science department.

Reynolds joins MTU's STS Program as a tenured Associate Professor. Prior to going to Tech he taught the history of engineering/technology at the University of Wisconsin-Madison. He is the author of Stronger than a Hundred Men: A History of the Vertical Water Wheel, and of the forthcoming 75 Years of Progress: A History of the American Institute of Chemical Engineers.

Carlson is just now completing his Ph.D. degree from the University of Pennsylvania's Department of the History and Sociology of Science. He comes to Tech directly from the Thomas A. Edison Papers project at Rutgers, having also taught the history of technology at Penn and at Virginia Polytechnic Institute and State University.

ANNOUNCEMENTS: ERRATUM

In Volume 1, #2 of 4S REVIEW, part of a sentence was omitted from John Wilkes' Essay Review, "Case Studies: A Promising Way to Assess Technological Impacts?" The sentence (on p. 17) was printed as follows: "One of the traditional resource concept of technology to describing the official reality, yet it is not clear that she saw this as a technological issue." The sentence should have read as follows: "One of the great contributions of the case is how she moves back and forth from the traditional resource concept of technology to describing the official reality, yet it is not clear that she saw this as a technological issue." The Editor apologizes for this mistake.

ANNOUNCEMENTS: DEATH NOTICE RECEIVED

As the 4S REVIEW was going to press, we learned with deep regret about the death of Professor Derek J. deSolla Price who died in England on 3 September 1983. A Memorial Meeting was held at Yale University on 11 September. The next issue of 4S REVIEW will contain an appropriate acknowledgement of this loss to the Society for Social Studies of Science.

4S REVIEW

ISSN: 0738-0526

Journal of the Society for Social Studies of Science

The 4S REVIEW is published four times each year, beginning in the spring of 1983 with Volume 1, Number 1. The 4S REVIEW succeeds the 4S Newsletter which concluded with Volume 7, Number 4.

4S REVIEW is sent to all members of the Society for Social Studies of Science; membership is on a calendar year basis. There are three categories of membership: Professional, \$15; Students, \$5; Institutional (including libraries), \$25.

Correspondence concerning membership and subscriptions should be sent to:

Lowell Hargens
4S
Department of Sociology
Indiana University
Bloomington, Indiana 47405

Correspondence concerning manuscripts for publication, reviews, opinions, and news should be sent to the appropriate editor:

Jerry Gaston, Department of Sociology, Texas A&M University,
College Station, Texas 77843

Lawrence Stern, (Book Reviews), Department of Sociology, College
of the Holy Cross, Worcester, Massachusetts 01610

Steve Woolgar, (News), Department of Sociology, Brunel University,
Uxbridge Middlesex, UB8 3PH, United Kingdom

Terry Shinn, (News), Groupe d'Étude des Méthodes de L'Analyse,
Maison des Sciences de L'Homme, 54 Boulevard Raspail,
75270 Paris, France

David Miller, (News), School of History and Philosophy of Science, University of
New South Wales, Kensington, N.S.W., 2033 Australia

Thomas Gieryn, (Bibliography and Literature), Department of Sociology,
Indiana University, Bloomington, Indiana 47405

4S REVIEW

Department of Sociology
Texas A&M University
College Station, Texas 77843

NONPROFIT ORG.
U. S. POSTAGE
PAID
COLLEGE STATION,
TEXAS 77843
PERMIT NO. 215