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THOUGHT AND OPINION

Velikovsky and Social Studies of Science

Henry Bauer

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The Velikovsky Affair (1, 2) has been cited by social scientists (3-10) as illustrating the resistance offered by science and scientists to new ideas; for example: the Velikovsky affair was "the most massive case of theoretical, methodological and 'social' non-conformity in the recent history of science," and of interest about Velikovsky is "the reception of his work by the scientific community" (8). Such citations are based explicitly or implicitly on the claim that the technical merits of Velikovsky's ideas could be left aside: "I am not interested here in the precise nature of Velikovsky's claims nor in their scientific validity, though the latter is relevant in some degree to the discussion which follows...." (8); "...the merits of the scientific issue do not alter the deplorable treatment that his ideas received from the profession" (11).

I shall argue that the nature and validity of Velikovsky's claims must be considered before one decides that the Affair can illuminate the reception of new ideas in science; that examination of the claims reveals that they have nothing useful to do with science; that therefore the Affair should not be used as an example in the social studies of science (unless it is used as an example of non-science or

of pseudo-science).

The Demarcation Issue

There exists, of course, no general agreement on the criteria one might employ to distinguish science from other intellectual activities. It does not follow, however, that any intellectual activity can legitimately be called "science" or be regarded as relevant to the manner in which science reacts to new ideas. That there is no formula by which science can be distinguished does not entail that we cannot validly say of something, "this is not science." I shall argue that there is no good reason for talking about Velikovsky and about science in the same context, because Velikovsky's activities had none of the attributes of scientific work.

Science is, among other things, an activity carried on by an identifiable group of people: scientists seek to describe the natural world; they communicate with one another about their work; they train new workers; they have associations which offer membership on the basis of professional qualifications. The community has recognized rewards and sanctions. The modes of communication are structured: meetings both informal and formal, exchange of written communications both informal and formal. And so on. Though no simple formula exists for determining who

is a scientist and who is not, and though there are certainly cases where no clear-cut decision would be generally agreeable, there does exist agreement over a wide area, based implicitly on the sort of grounds just enumerated, about who is a scientist and who is not: Reverend Jerry Falwell, for example, is not a scientist, even though he says things about science.

None of the social characteristics of scientists provide any warrant to call Velikovsky a scientist. It is well to recall that "scientist" is a generic term that encompasses practitioners of recognized specialities -- there is no scientist who is not also a biologist or a chemist or a geologist or some other sort of specialist. Now Velikovsky's purportedly scientific claims have to do primarily with astronomy, geology, and physics (with chemistry also entering the picture thereby). In none of those fields did Velikovsky possess the qualifications required of certified practitioners. He had no advanced degree in any science, and not even a specialized first degree in any science. He never held a position as teacher or researcher in a college or in a public or private research institute. He never had a paper on astronomy or physics or geology or chemistry published in any of the technical journals devoted to those matters. He was never a member of any of the "invisible colleges" in any of those fields. There seems to be no basis at all, in Velikovsky's way of working or in his associations, for calling him a scientist.

Admittedly, Velikovsky made written as well as oral statements about matters of astronomy and physics and so forth, statements for which some people have claimed a certain validity. But we do not apply the

term "scientist" to everyone who makes statements about astronomy, physics, and the rest, even when those statements have a certain validity. Much science fiction has had sound and prophetic things to say about several sciences, but those authors are not called "scientists," nor are studies made of how their statements were received by science, nor are such studies cited as instances of resistance by scientists to new ideas. Why should Velikovsky's work be thought to be relevant to science?

The literature and history of the controversy (2) reveal that, in point of fact, leading astronomers and geologists and physicists asserted from the beginning that Worlds in Collision was not a work of science and that scientists could not do anything useful with it. But some writers of popular science, and more journalists, and some humanists and social scientists thought that Velikovsky's substantive ideas had some plausibility; and they thought that scientists ought therefore to take those ideas seriously. When scientists then reacted strongly, overstated the implausibility of some of Velikovsky's assertions, and made ad hominem attacks, support for Velikovsky became more general, and issues of free speech and openminded examination of ideas and reception of new ideas by science were raised. But those issues were not validly raised. Even if Velikovsky pointed to some possible truths about the natural world, it does not thereby follow that scientists ought to have taken up his ideas for examination. It is not the case that scientists take up the ideas of others simply because they concern deep questions about nature: the ideas must seem to have some scientific plausibility, they must be scientifically examinable, they must seem important

to the relevant practitioners; and that is unlikely to be the case when the ideas are presented in the form of a popular book rather than in technical journals, by one who has no standing in any science and whose writings display notable ignorance of elementary points of the relevant sciences. In point of fact, the notion that Velikovsky's work has something to do with science became widespread only because sufficient publicity and credence were given to Velikovsky's own claims that his work was significant for matters usually associated with the names of such as Darwin and Newton. A number of people (but not scientists) accepted Velikovsky at face value.

The commonly held belief, that science should concern itself with any seemingly important question that anyone poses about the natural world, displays a mistaken view of the character of scientific activity. That such a belief is indeed commonly albeit implicitly held throughout our society, I have illustrated in detail in relation to the Velikovsky affair (2). I want here to point out that such a belief actually is a scientistic one, for it builds on the implicit assumption that only science can make valid pronouncements about the truth of matters relating to the natural world. The demand that scientists should have seriously examined Velikovsky's propositions is grounded in part on that scientistic belief.

One cannot, then, take the Velikovsky affair as illustrating anything about the reception of new ideas by science without assuming that Velikovsky's particular claims are properly the subject of scientific enquiry. Those who insisted that Velikovsky's propositions should have been examined by scientists were assuming that scientists had dismissed those

propositions on inadequate grounds.

The Nature of Velikovsky's Claims

Even a summary of Velikovsky's notions makes plain that scientists had ample and excellent grounds for not taking them seriously. Velikovsky presented a chain of interpretations of historical and mythic references as demonstrating that the accepted chronology of the Middle East was seriously in error; that global catastrophes not recognized in current established knowledge had occurred in recent historical times (around 1500 BC and 700 BC); that the accepted theory of gravitations is inadequate; that Venus had erupted as a comet from Jupiter, had caused the aforementioned global catastrophes, had also suffered close contact with Mars, and had subsequently settled into its present orbit as a planet.

It takes little particular or deep examination of those claims to recognize that they contradict established knowledge in many disciplines: Biblical exegesis, history and archaeology, geology, astronomy, and physics, among others. And that recognition in itself is adequate grounds for scientists not to examine the claims any further. Science, as all other intellectual disciplines, seeks to make new discoveries by grounding itself on some portion of already reliably established knowledge: it has no way of dealing with propositions that discard not only accepted theory but also accepted facts and accepted methodology -- and Velikovsky did reject accepted facts and methods of the various sciences as well as theories.

If one delves a little into Velikovsky's particulars about matters of physics and chemistry and astronomy and so on, one finds that many of his statements are plain wrong, and that he displays a

serious lack of understanding of the fundamentals of those disciplines (2). There was not common ground on which scientists could even have engaged him in substantive debate.

In point of fact, in this light Velikovsky can be properly compared with people whom we do not hesitate to class as pseudo-scientists. The reluctance to so label him stems from his manifest intellectual powers, from the massive documentation he brought to his writings, from his remarkable erudition and memory: but the substance of his claims no more warrants scientific scrutiny than do the claims of von Däniken, or for that matter those of Ignatius Donnelly, who had published in the 1890s a book strikingly similar to Velikovsky's (2).

Velikovsky and Social Studies of Science

In the event, some social scientists failed to recognize the spuriousness of the claim that Velikovsky could have some significance for science. As a result, they have drawn from the case some conclusions about science that are misleading.

For example, the public reactions of scientists might have been harsh, it was suggested (8), because Velikovsky "broke the (Mertonian) rule of communality...by allowing popular interpretations of his work to be published before the main opus itself had been presented." But the "main opus" itself was only a popular book; and Velikovsky was never a member of a community to which these norms or rules apply.

The critics, it was said (8), "did not subject Velikovsky's claims to rigorous examination before assessing the validity of these claims." Since the merit of the claims has been left aside for the purpose of that commentary, however,

this statement implies that any claim at all needs to be rigorously examined; and thus this same criticism could be directed at science for its reaction to the rankest piece of pseudo-science. Surely, rigorous examination is called for only if cursory examination reveals something that is prima facie of scientific interest. But Velikovsky's notions do not possess that.

It was claimed that the laws of mechanics "operate as norms, departure from which cannot be tolerated" (8). In point of fact, the affair shows only that such unsound departures as Velikovsky's are ignored; such departures as relativity are tolerated very well.

"Particularly noticeable...is...the persistent tendency of scientists...to justify rejection of Velikovsky's claims simply by indicating the latter's departure from established beliefs" (8). But those established "beliefs" happen to be the most fundamental laws of physical science, massively supported by empirical evidence. Cavalier dismissal may not be appropriate for ideas that are a little unorthodox, but it is surely an appropriate response to ideas that are absurd. Science is not usually criticized for cavalier dismissal of astrology; the criticism in the Velikovsky case implies that Velikovsky's suggestions are somehow more scientific than that. In point of fact, however, whenever Velikovsky deals with physical science he is entirely out of his depth (2).

Another unfounded claim is that Velikovsky's work was rejected because it is interdisciplinary: "using historical records of natural events, [Velikovsky] put in question the essential methodology of modern science" (8). But Velikovsky's use

of historical records is itself most questionable (12). Further, the use of methodology from one discipline to obtain data for another is not a generally valid procedure, especially not when the disciplines differ as much as do folkloristics and astronomy. Velikovsky proposed unsound interdisciplinary activity, and was rejected for being unsound, not for being interdisciplinary.

In view of these weaknesses in the case (8), one must also reject the conclusions that "analysis of the Velikovsky case demonstrates...that the need for cognitive consensus which is reinforced by the educational process in science can impede consideration and generation of new ideas." Other cases than that of Velikovsky must be used if such a conclusion is to be regarded as empirically established, and preferably cases where the new ideas are good ones, useful ones, appropriate ones and not irrelevant to the technical content of science.

Another critic (9) took the Velikovsky controversy as illustrating the tendency of scientists to reject new ideas as a result of prejudice and of the threat posed by the possible overthrow of "an established way of looking at the world, a paradigm of science." Referring to the prejudice encountered by Copernicus, Galileo, Mendel, Newton, and Wegener, it was claimed that "one of the most sensational incidents of scientific prejudice" was that of Velikovsky. But any examination of Velikovsky's claims makes plain that they cannot be talked of in the same breath as those of Copernicus et al., which proposed a new view within the framework of accepting much of reliably established knowledge; Velikovsky rejected fundamental principles of every discipline with which he concerned himself. Proponents of

pseudo-science routinely point to the rejection of Copernicus et al. as analogies to their own reception by science, but that is not a valid argument in support of their own theses. Gruenberger (13) has called this "the Fulton non-sequitur" -- they laughed at Fulton; Fulton was right; they laugh at me; therefore I am right.

Once again, by not submitting Velikovsky's technical claims to prior scrutiny on intrinsic grounds, a critic of the affair has treated as science what is not science. So also Barber's seminal article (14) is quoted (9) to the effect that resistance to a scientific discovery is likely to be stronger, the less is the professional status of the discoverer; and "one might wonder what might have happened to Velikovsky's theories if he had been a professor at Harvard." I am indebted to Robert Schadowald for reminding me that Barry Fell of the Harvard faculty has found no receptive audience of academics for his notions about pre-Columbian settlement in America, notions propounded like those of Velikovsky in popularly written books (15) and not in technical journals.

Another critic (7) maintained that "Like all 'true believers,' the scientist...resists...with every resource at his disposal or runs the risk of being destroyed as a person...committed scientists can exercise no other alternative to assure their own intellectual survival than to discredit the discoverer rather than the discovery." That is quite wrong -- the bulk of the literature showing resistance to new ideas in science is replete with arguments addressed to the ideas themselves; it is in the case of pseudo-science that one sees more in the way of ad hominem dismissals than analytical criticism of the propositions.

The same critic (7) posed the question, "why should some new ideas be acceptable to scientific fraternities while others are rejected on nonscientific grounds?" But it has not been established that Velikovsky's ideas were rejected on non-scientific grounds, so the question is out of place in this context. So also is the claim that the Velikovsky affair "represents a challenge to the self-correcting ethic [of science]"; that would be the case only if scientifically interesting and potentially valid ideas had been dismissed. To the contrary, since Velikovsky's ideas are of no relevance to scientific enquiry, science has indeed been "self-correcting" by not wasting time on them.

McAulay (10) looked for a possible influence on the reception of new views in science of "the metaphors and background assumptions which underpin scientific theories." "If in the past traditional religious sentiments have spurred resistance to Copernican and Darwinian theories, the Velikovsky 'affair,' conversely, marks the resistance of establishment science to a cosmology charged with fundamentalist religious significance. (Note: this should not be interpreted to suggest that Velikovsky's theories are rejected solely on extra-scientific grounds nor that this decision is necessarily wrong). In this light the response to Velikovsky's ideas...bears reexamination in terms of the tacit scientific world view at odds with catastrophism." But since rejection of Velikovsky's ideas is warranted solely on intrinsic scientific grounds, the case cannot be used in the search for extra-scientific grounds. One would at the least have to demonstrate that the scientists who rejected the ideas by stating their unacceptability in light of accepted laws actually had other reasons; and

that has not been demonstrated. An examination of the unusually vehement reactions of Velikovsky's detractors might reveal other motives; but even then, one would have to view that as unusually vehement denunciation of pseudo-science, not of science.

Conclusion

The Velikovsky affair has been cited by some social scientists as exemplifying how science reacts to new ideas, and conclusions have been drawn therefrom about the nature of scientific activity. But there is no basis, other than Velikovsky's own wishes, for considering his ideas to have any relevance for science. What the controversy illustrates is how vehemently scientists can react against attempts by outsiders to insist that non-scientific notions should be taken seriously by scientists. Commentaries on the affair also reveal that some social scientists accepted Velikovsky's claims and not the claims of accredited scientists about the merits of Velikovsky's ideas; and thereby came to discuss as science what is actually in substance a rather typical piece of pseudo-science: thus pseudo-scientists and their supporters typically claim that their ideas have been rejected without rigorous examination, and point to such people as Copernicus and Galileo to prove that science has, in other instances too, been wrong about new propositions.

Such strictly externalist discussions as those here criticized (6-10), in which the merits of the intrinsic technical issues are left aside, may be appropriate when the issues considered are unquestionably matters of science; but in cases where there is a division of opinion over the issue, is this at all science or is this pseudo-science, when prior consideration of the

merits of the technical claims themselves is in order. In point of fact, this is unlikely to be a serious restriction on the range of topics available for study by social scientists: scientists do not vehemently and essentially univocally decry as pseudo-science something in science that is new, even startlingly new; they may denounce it as unfounded, or as wrong, or as unduly speculative, they may not give it credence, but they do not call it pseudo-science and they do not call one another pseudo-scientists.

Notes

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2. Henry H. Bauer, **Beyond Velikovsky -- The History of a Public Controversy**, Urbana & Chicago: University of Illinois Press, 1984.
3. Philip D. Converse, Letter to the Editor, **American Behavioral Scientist**, November 1963, p. 22.
4. George A. Lundberg, *ibid.*
5. Charles Perrow, *ibid.*, January 1964, p. 29.
6. Alfred de Grazia, "The Scientific Reception System," pp. 171-231 in note 1.
7. Andre M. Bennett, "Science: The Antithesis of Creativity," **Persp. Biol. Med.**, 11 (1968) 233-246.
8. Michael Mulkay, "Some Aspects of Cultural Growth in the Natural Sciences," **Social Research**, 36 (1969) 22-52.
9. Harold Graff, "Scientific Prejudice: The Velikovsky Incident," **Bull. Philadelphia Assoc. Psychoanalysis**, 23 (1973) 288-306.
10. Robert McAulay, "Velikovsky and the Infrastructure of Science; The Metaphysics of a Close Encounter," **Theory and Society**, 6 (1978) 313-342.
11. Jacques Barzun, **Science: The Glorious Entertainment**, New York: Harper & Row, 1964; footnote on p. 78.
12. Bob Forrest, **Velikovsky's Sources**; 6 parts published 1981 to 1983 by Bob Forrest, 53 Bannerman Avenue, Prestwich, Manchester M25 5DR, England.
13. Fred J. Gruenberger, "A Measure for Crackpots," **Science**, 145 (1964) 1413-1415.
14. Bernard Barber, "Resistance by Scientists to Scientific Discovery," **Science**, 134 (1961) 596-602.
15. Barry Fell, **America B.C.**, New York: Quadrangle, 1976; **Saga America**, New York: Times Books, 1980.

**International Workshop on New Developments in
the Social Studies of Technology**

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Enschede, The Netherlands**

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There may be social students of technology, but there is no social studies of technology. As a field, commensurate with, say, the social study of science, it simply has not yet come into being. I say "has not yet" because the Twente workshop suggests that it is coming. Prodded into action by Wiebe Bijker (Twente University of Technology) and Trevor Pinch (University of York), about forty scholars with an interest of one kind or another in the social dimensions of technology gathered at Twente for a three day working meeting. The organisers had cast their net wide. There were historians of technology, sociologists of science, people concerned with science and technology policy and/or risk assessment, people who have worked fairly extensively on the sociology of technology, plus a range of others (historians of science, social psychologists, philosophers) with an interest of one kind or another in the social study of technology. The result could have been disastrous, a dialogue of the deaf. But the remarkable feature of this meeting was the way in which, almost without exception, participants appeared to have come not only to talk but also to listen. Much of the workshop was thus concerned with exploration, with establishing common ground or simply finding out about differences.

Let me talk about some common ground, since this may represent the core of the future field of the social studies of technology. The first point is that most, if not all, the participants took the view that there is, indeed, a job to be done in the sociology of technology. The argument here parallels that in the sociology of science. Just because an artefact works (knowledge is taken to be true) this does not mean that it is beyond sociological explanation. This is because what counts as "working" is not self evident but rather has to be negotiated. This point has been made strongly in the literature by Edward Constant (Carnegie-Mellon University, Pittsburgh) with his notion of a tradition of technological testability: what counted as an efficiently working water turbine was defined in the United States in the late nineteenth century by performance as measured at the Holyoke testing flume. There was nothing natural about this flume or the tradition of testing that surrounded it. It was rather the outcome of contingent negotiations and developments in aspects of physics, craftsmanship and social and economic concerns. In a paper to the workshop on developments in missile accuracy (of which more below) Mackenzie (University of Edinburgh) made essentially the same point. Accuracy is not something that is given. Rather it is

negotiated and is a partial function of a variety of social interests.

So far so good. But how do the technological, the scientific, the social and the economic interrelate? In his Networks of Power Hughes (University of Pennsylvania), offers us one approach. As is well known, he is concerned with the growth of large technological systems. Artefacts are seen as part of a much larger structure that includes elements drawn from the social, the economic and the scientific. Such entrepreneurs as Edison, Inshull and S. Z. Mitchell who were responsible for the development and growth of networks of electricity generation and distribution could not afford to think in anything other than system terms. To introduce a phrase that I used in my own paper they were "heterogeneous engineers." All manner of bits and pieces were fitted together in order to build relatively stable systems. The conventional barriers erected between the social, the economic, the political, the scientific and the technological -- these were of no concern to them. This then represents common grounds to which most of the participants could subscribe: technology is integrally related with the rest and the social study of technology must take this integration seriously.

This conclusions has a number of implications. Perhaps the most immediate of these is that it entails rejection of technological determinism. If the social/economic influences the technological, then technology cannot develop simply as a function of its own (or for that matter scientific) momentum. Neither can it be technology that directs social change though it may, of course, influence this. But having agreed that the relationship between artefacts and their environment is complex the question

then arises: how, exactly, are they to be related? Or is this a wrong question since it presupposes a natural distinction between technology and the rest? Much of the workshops was concerned, in one way or another, with precisely these issues. Hughes, looking at the electricity networks from the standpoint of the entrepreneur, has his own answers to these questions: using a military metaphor, he talks of reverse salients. The reverse salient is a section of the system where development has lagged behind the rest to the point where it is becoming recognised that it represents a problem for the system as a whole. Thus Edison struggling with the consequences of Ohm's law and the high price of cooper, realised that an economically feasible distribution network could be built if the resistance of the lamp filament were to be increased.

However, Hughes was not the only participant at the workshop to make use of the notion of reverse salient. Donald MacKenize, in the fascinating albeit alarming study mentioned above, argued that the notion of reverse salient only makes sense if the system is directed by goals, goals which themselves need explanation. In the case of missiles, the goal is that of increasing their accuracy, and this is in turn explained with reference to the social interests of certain organisations, notably (on the American side) the military and Congress. Other participants, while not talking of reverse salients, used concepts which appeared to do work of a similar kind. Thus Michel Callon (Ecole Nationale Supérieure des Mines, Paris) talked to "problematization." In a study of the French controversy surrounding proposals to develop an electric vehicle to replace the petrol driven car, Callon described the way in which Electricite de France sought

to relate the supposed problems of others (pollution, traffic congestion) to its own solution, the development of the electric vehicle. He argued that the creation of such problem-solution pairings is a difficult, uncertain and potentially reversible process, the product of struggle between different organisations and actors. Indeed, in the case of the electric vehicle the problematisation advanced by Electricite de France was subsequently overturned by Renault, the car manufacturer, which consigned the electric vehicle to a peripheral role in French transport policy.

There is obviously a close relationship between the reverse salient/goal pairing on the one hand and problematisation on the other. Perhaps the difference between them lies partially in the following: MacKenzie (and Hughes?) are willing to make an assumption that there exists a background social structure organised in terms of relatively stable social interests which defines relatively stable goals. This obviously reflects a fairly standard sociological stance, and in particular that developed by the "Edinburgh School" of the sociology of science. In contrast, Callon is unwilling to make this assumption. For him (and he shares this position with such other members of the "Paris School" as Bruno Latour) there is no reason to presuppose such a provisionally stable background. The backcloth is rather one of continuous struggle between actors and organisations, while social interests and goals represent two of the consequences of that struggle. My own comment on this would be that at the level of sociological theory the difference is obviously fundamental but at the level of empirical study it at present appears to be less pressing.

Another and linked debate concerned the relationship between the concept of system used by Hughes and that of network adopted (albeit in different ways) by Ruth Schwartz Cowan (State University of New York, Stonybrook), Callon, and myself. Schwartz Cowan's contribution was of particular interest. She started by rejecting linear models of technological development on the ground that these rapidly become too complicated to be a useful research tool. She then described the way in which she had turned, instead, to network models based on actors' interests. Thus given her well known concern with household technology, she posed the question: how do consumers confront artefactual systems between which they make choices? Her model took the form of a network with the consumer at the centre, organisations such as government and the utilities at the periphery, and a system of distribution linking the periphery with the centre. This system of distribution (i.e., the choices actually available to the consumer) was structured by struggles and other links between the organisations at the periphery. Charles Weiner (M.I.T.) aptly described the model as a kind of inverse spider's web, with the consumer caught in the middle and the predators round the edge. The relationship between this model and that of system is intriguing because the system approach tends to look at technology from the standpoint of the managers, whereas Schwartz Cowan was concerned with it from the standpoint of the consumer, while simultaneously trying to take account of the struggles between formal organisations. Accordingly, though she modestly presented participants with a set of problems rather than solutions, her paper helped to underline the fecundity of a system/network metaphor by showing that it might be applied to any kind

of actor. Consumers, like electricity generating companies, construct systems or networks which include artefacts, and there is no reason to limit our study of technology to those who are officially responsible for its production. On the other hand, her model also raised a fundamental problem: what should be treated as lying within the boundaries of the system/network, and what should be treated as lying outside?

Hughes' answer to this question was clear enough: one of the virtues of a system metaphor is that it makes it possible to talk about how the system reacts to its environment. Accordingly, some sort of distinction must be maintained between inside and outside. Such an assumption also lay behind MacKenzie's work on missiles. While I also made use of a network metaphor in my own paper on the technology of the fifteenth century Portuguese voyages to India, the notion of inside and outside that I used was somewhat different. Specifically I sought to argue that the growth of social/technological networks is a function of the juxtaposition of heterogeneous bits and pieces such that they keep each other in place: particular types of boats, specially trained sailors, navigational instruments, all were necessary if the Portuguese were to round Cape Bojador without shipwreck. I also argued the (Paris School) view that networks continuously sought to dissociate their neighbours and incorporate elements previously belonging to these into themselves. Thus, for instance, once it was tamed Cape Bojador formed a part of the Portuguese network along with the vessels and the sailors and no longer dissociated these from each other. Much more lay within my networks than Hughes' systems.

This in turn highlights a further point of debate -- the question of the extent to which natural and social objects should be treated symmetrically. In my study (as in that of Callon and, perhaps, Hughes) I talked indifferently of people and of artefacts. This kind of talk made some, notably those from the British school of the sociology of science who are strongly influenced by interpretive sociology, very uneasy. Thus such participants as Trevor Pinch and Harry Collins (University of Bath) argued that we were reserving parts of the technological from sociological explanation and were hence being insufficiently radical. Even worse, we might be explaining parts of the social in terms of technological or natural factors. In either case we were backing away from ground that had been won only with difficulty in the sociology of science. They did not want the social study of technology to withdraw from its equivalent of the "strong programme." Obviously this question remains on the agenda. Let me just add that I agree with the diagnosis: Callon, Latour and I are indeed avoiding the position of explaining everything in terms of sociological factors. I differ only about the desirability of this. I can see no reason for privileging the social in this way and would prefer to avoid sociological reductionism. The task, instead, is to understand how networks/systems that include artefacts grow, stabilise themselves, and decline, a radical task that will inevitably take us from social to other kinds of bits and pieces. This is a debate that will continue!

When I was asked to write this report I was told that I might be selective. As will be obvious from the above, I have used this license to the full, preferring to concentrate on a particular set of

of issues to do with networks, systems and appropriate strategies of explanation in a certain amount of detail rather than write three lines on every contribution. However I want to conclude by noting that there were, of course, other major topics on the agenda, and the working papers presented represented a wide range of approaches and interests. Several participants were concerned with policy related and/or risk assessment debates: Jaap Jelsma and Wim Smit (Twente University of Technology) discussed the recombinant DNA debate, Henk Bodewitz, Henk Buurma and Gerard de Vries (University of Groningen) considered drug assessment and regulation and Henk van den Belt (Nijmegen) and Arie Rip (University of Leiden) presented a paper which used material from the nineteenth century dyestuffs industry to discuss the Nelson-Winter/Dosi model of technological development. Edward Yoxen (University of Manchester) offered an attractive preliminary analysis of the development of ultrasound in obstetrics, Wiebe Bijker presented a strategy for theory by drawing upon his own empirical work (on bicycles amongst much else) while Harry Collins (University of Bath) talked about expert systems and their implications for the enculturation model of learning and Bruno Latour (Ecole Nationale Supérieure des Mines, Paris) argued that there are parallels between successful scientific statements and technological devices since both may be displaced without distortion. Finally, Edward Constant confronted head-on the difficult problem of the relationship between expert knowledge and organisational structure, while Arie Rip, Wiebe Bijker and Trevor Pinch all, at different times and in different ways, attempted the awkward task of trying to draw themes out of the workshop without being unfair to any

of its participants!

This summary, as I've noted, is unfair on many. Yet, though other participants may read the meeting in quite other ways, I believe that most will have regarded it as highly rewarding. Perhaps, in retrospect, we may look back to it as the place where the social study of technology first became a recognisable field rather than a set of individuals.

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of Science
P. O. Box 487
Canton, MA 02021
U.S.A.**

REVIEW ESSAY**Finalization: Cool Radicalism Versus the
Republic of Science**

Sal Restivo
Rensselaer Polytechnic Institute

[Review of Wolf Schäfer (ed). Finalization in Science. Boston
 Studies in the Philosophy of Science, Vol. 77. Boston: D.
 Reidel, 1983.]

In 1970, Carl Friedrich von Weizsäcker established the Max Planck Institute for the Study of the Conditions of Life in the Scientific-Technical World at Starnberg in West Germany. The Institute was jointly directed by von Weizsäcker and Jürgen Habermas. Soon afterward, Gernot Böhme, Wolfgang van den Daele, and Wolfgang Krohn formed a research group at the Institute known as "Alternatives in Science." They were later joined by Wolf Schäfer, Rainer Hohlfeld, and Tilman Spengler. The perspective guiding the work of this group was sketched in a 1973 paper by Böhme, van den Daele, and Krohn titled "Die Finalisierung der Wissenschaft." Finalization in Science is the first document in English by the "finalists" on their collective efforts.

The finalists view science as a "collective utility." Their program is rooted in the development of ecology as a "normalized natural science" in which objectivity (scientific theory-formation) and values (the pursuit of social and political goals) are consciously and intentionally integrated. Nature (in broadest terms, the ecosphere) is viewed as a subject rather than a productive, juridical, or economic object. The finalists' commitment to grounding the pursuit of societal goals in scientific theories is designed to avoid "the

instrumentalization of science"; and their effort to give "task communities" the responsibility for pursuing science and technology in the public interest is designed to avoid "the academicization of oriented research." Finalization thus seeks to integrate a theory of science (based on critical as opposed to positivist science studies) and a vision of a "reconstructed science." The reconstruction of science is aimed at turning it away from Faustian quests that threaten to destroy the natural environment in the process of pursuing knowledge of nature "for its own sake." Three alternative approaches to the ecological and environmental crisis are identified and found to be inadequate. "Going Natural" is criticized for romanticizing the pastoral past and raising the spectre of a "Blood and Soil" agenda nourished by sociobiology. "Going Luddite" is described as working for the total abolition of science and technology as we know it, and criticized for going too far, that is, for being "hot" radicalism. "Going Technocrat" (basically the "technological fix" approach) is viewed as a perverse attempt to solve problems caused by a technologistic orientation by intensifying our commitment to technologism.

The main ingredients of the

CALL FOR APPLICATIONS FOR THE EDITOR IN CHIEF AND MANAGING EDITOR OF THE 4S REVIEW

The Society for Social Studies of Science is seeking an Editor in Chief and a Managing Editor to assume responsibility for the 4S Review beginning in 1986. A person may apply to hold both positions simultaneously or apply for only one of the positions. The Editor in Chief and Managing Editor would take responsibility for the March, 1986 issue, and have a term of three to five years. The Publication Committee of the Society will review applications and make a recommendation to the Board, which will make the final decision. Applications are due by July 15, and the Board will make its decision by October 1, 1985.

The 4S Review is the official publication of the Society for Social Studies of Science. The Review is distributed internationally to 475 subscribers and institutions. The Review publishes book reviews, critical essays, news, and reports of meetings. Associate Editors are responsible for collecting much of the material published in the Review.

THE EDITOR IN CHIEF will be responsible for coordinating the activities of the Associate Editors in charge of Book Reviews, News, and Bibliography and Literature (Critical Essays). Additionally the Editor may seek contributions on topics of vital interest to readers of the Review. Specific responsibilities of the Editor include: reading newly submitted manuscripts, selecting reviewers and monitoring the review process; corresponding with authors, Associate Editors and reviewers; editing and preparing materials accepted by the Editor and submitted by the Associate Editors; sending edited materials to the printer; preparing an annual report for the Publications Committee, attending Board meetings and annual Society meetings; and maintaining a budget for the editorial office. The Editor should have access to secretarial services for correspondence and typing of the final camera ready copy of the Review. The Society provides some reimbursement to the Editor for office expense, but the editor normally might expect to make use of some resources from her or his supporting institution.

Applicants should submit a vita and a brief statement indicating how they propose to meet the editorial responsibilities. Applicants should include the following in their statements; their view of the field of social studies of science, its problems and its future; ideas about the editorial focus of the journal; ideas and plans for future innovations in the Review (if any); and plans to increase the number of quality manuscripts.

THE MANAGING EDITOR is responsible for the business management of the Review. The Managing Editor is responsible for soliciting a printing contract, arranging for the production and mailing of the Review. (The subscriber mailing list is prepared by Academic Services in Canton, MA.) Applicants should submit a vita and a brief statement indicating how they would meet the responsibilities of Managing Editor.

Application for these positions or the combined position should be sent, before June 15, to:

James L. McCartney
Department of Sociology
University of Missouri
Columbia, Missouri 65211
U.S.A.

ANNOUNCEMENTS: FUTURE CONFERENCES/MEETINGS**AAAS Symposium on "Social Ethics, Agricultural Change,
and Agricultural Research"**

The Annual Meeting of the American Association for the Advancement of Science will feature a symposium on "Social Ethics, Agricultural Change, and Agricultural Research." The symposium is scheduled for Monday, May 27, 1985 in the Los Angeles Hilton Assembly Center. Symposium speakers will examine some of the controversies, the actors and the stakes in choices for agriculture and agricultural research, particularly in California. Participants include Molly Coye, Medical Officer for the National Institute of Occupational Safety and Health in San Francisco; William Friedland, Professor of Sociology, University of California Santa Cruz; Theodore Hullar, Executive Vice Chancellor, University of California Riverside; William Stiles, Agriculture Committee Staff Consultant, U.S. House of Representatives; Bill Harriott, Past President of the American Society of Agricultural Engineers; John Cobb, Professor, School of Theology at Claremont; William Aiken, Associate Professor of Philosophy, Chatham College; and William Lacy, Associate Professor of Sociology and Committee on Agricultural Research Policy, University of Kentucky. The symposium is being organized by Kenneth Dahlberg, Professor of Political Science, Western Michigan University, and Rachelle Hollander, Director, Ethics and Values in Science and Technology Program, National Science Foundation. For further information, write:

EVIST Program
National Science Foundation
Washington, D.C. 20550
Telephone: 202/357-7552

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P. O. Box 487
Canton, MA 02021
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**1985 Annual Meeting
SOCIETY FOR THE HISTORY OF TECHNOLOGY
Call for Papers**

The SHOT Program Committee calls for session and paper proposals for the 1985 Annual Meeting, which will take place in Dearborn, Michigan on 17-19 October.

We seek a diverse and well-balanced program and would like to encourage participants to address topics which have received relatively little attention at recent meetings. We will particularly welcome contributions in the following areas: non-western technology; technology transfer and its effects on recipient cultures; pre-industrial technology; material culture; technology and capitalism; computers and the new communications technologies. We are also especially interested in interdisciplinary proposals concerning the role of workers, women, blacks, and other minorities in technical development or which analyze the organizational context in which technical change has occurred. In addition, we would like to see at least one session or panel discussion focus on recent theoretical and methodological approaches in the history of technology, such as innovation theory, technology as knowledge, or the systems concept. In keeping with tradition, we plan to schedule both works-in-progress sessions and sessions sponsored by SHOT's special interest groups (Jovians, Pelicans, TEMSIG, TS&E, and WITH). We encourage these groups to submit proposals.

The deadline for proposals is May 1, 1985. Individual proposals must include a 150-word abstract of the paper and a one page curriculum vita. Session proposals should include a statement of the general theme as well as an abstract and curriculum vita for each participant. Please send four copies of each proposal and c.v. to:

Susan J. Douglas
School of Communications
Hampshire College
Amherst, MA 01002
Telephone: (413) 549-4600 X559

Because the committee receives more proposals than can possibly be accommodated in the limited space available, we will only consider those proposals which are complete by the May 1st deadline. To broaden participation in SHOT meetings we will follow the usual practice of giving preference, other things being equal, to individuals who have not made presentations in recent years.

1985 SHOT Program Committee:
Susan J. Douglas (Hampshire College)
Jeffrey L. Sturchio (The Center for History of Chemistry)
W. Bernard Carlson (Michigan Technological University)

1985 Annual Meeting
CANADIAN SCIENCE AND TECHNOLOGY HISTORICAL ASSOCIATION
Call for Papers

The Canadian Science and Technology Historical Association will hold its annual meeting in Kingston, Ontario, 25-27 October 1985. Papers are invited in all areas bearing on the history of Canadian science, technology, and medicine. Contributions on the American influence on Canada's science, technology, and medicine, and interdisciplinary proposals relating those topics to other specialties (such as women's history, labor history, and urban history), are especially welcomed. The deadline for proposals is 1 April 1985. Proposals should include the title, abstracts of papers not to exceed 150 words, and curriculum vitae no longer than one page. Address inquiries and proposals to:

Dr. A. W. Tickner
 Senior Archival Officer
 National Research Council
 Ottawa (Ontario) KIA OR6
 Canada

**Summer Conference on Historical, Philosophical, and
 Social Studies of Biology**
 St. Mary's College, Notre Dame, Indiana (South Bend)

A Summer Conference on Historical, Philosophical, and Social Studies of Biology will be held on the campus of St. Mary's College, Notre Dame, Indiana (South Bend), 24-29 June 1985. The conference will consist of plenary sessions, workshops, and works-in-progress sessions. Suggestions for topics and personnel for plenary sessions and workshops are welcome. Submissions of abstracts for works-in-progress sessions should be postmarked no later than 1 April 1985. Send correspondence to Summer 1985, 314 Decio Hall, Program in History and Philosophy of Science, University of Notre Dame, Notre Dame, IN 46556.

Fermilab Conference
"Particle Physics in the 1950s: Pions to Quarks"

An international history symposium on "Particle Physics in the 1950s: Pions to Quarks," will be held at Fermilab in Batavia, Illinois, on 1-4 May 1985. The meeting will cover the period of particle physics from the discoveries of the pion and strange particles, and the building of the first large accelerators, to the introduction of symmetry concepts and proposal of the quark. Speakers will include physicists Murray Gell-Mann and Emilio Segre and historians Peter Galison, Silvan Schweber, Andy Pickering, Hywel White, Allan Franklin, Armin Hermann, and John Heilbron. For information as it becomes available, write to:

L. Hoddeson
 Fermilab
 P. O. BOX 500
 Batavia, IL 60510

THE USES OF EXPERIMENT

A CONFERENCE ON EXPERIMENTATION IN THE NATURAL SCIENCES

NEWTON PARK COLLEGE, BATH, ENGLAND

Friday 30 August - Monday 2 September 1985

PROVISIONAL PROGRAMME

The character and uses of observation and experiment will be approached from historical, sociological and philosophical perspectives. Sessions will be arranged under several thematic headings, including:

1. **Experimental Instruments: Testing and Persuasion in Science and Technology**
Contributions expected from: Ian Hacking (Toronto), Willem Hackmann (Oxford), Frank James (Royal Institution), Donald MacKenzie (Edinburgh), Simon Schaffer (Cambridge), Mari Williams (LSE), others TBA.
2. **Experiment and Consensus-Formation**
Contributions expected from: Harry Collins (Bath), Allan Franklin (Colorado), Peter Galison (Stanford), Gerry Geison (Princeton), Andrew Pickering (MIT), Steve Shapin (Edinburgh);
Science in Practice and Science in the Classroom
Paul Atkinson, Sarah Delamont (Cardiff), David Gooding (Bath), others TBA.
3. **Observation and the Construction of Experience; Experiment and Argument**
Contributions expected from: H. Brown (Illinois), James Brown (Toronto), Patrick Heelan (SUNY at Stony Brook), Michael Lynch (Walla Walla), Ron Maylor (Thames), Thomas Nickles (Reno), Trevor Pinch (York), Martin Rudwick (Cambridge), Dudley Shapere (Wake Forest), Ryan Tweney (Bowling Green), John Worrall (LSE), others TBA.

Full details of the programme and accommodation will be available in May 1985. Until that date inquiries should be addressed to Dr D C Gooding, Science Studies Centre, School of Humanities & Social Sciences, University of Bath, Bath BA2 7AY, England. The conference is organized by H M Collins, D C Gooding, (both Science Studies, Bath), T J Pinch (Sociology, York), S Schaffer (History of Science, Cambridge) and S Shapin (Science Studies, Edinburgh). Sponsors include the British Society for the History of Science and the BSA Science Studies Group.

Those wishing to receive the programme and registration form should return the reply slip below to: "The Uses of Experiment", c/o Mrs Nicola King, School of Humanities and Social Sciences, University of Bath, Bath BA2 7AY, England, as soon as possible.

PLEASE SEND DETAILS OF THE CONFERENCE ON EXPERIMENTATION:

Please print or type:

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.....POSTCODE

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The Second

OXFORD SCIENCE STUDIES SUMMER SEMINAR

13-27 July 1985 at The Queen's College, Oxford

The seminar is an international forum for discussions between scientists, scholars, policy-makers, teachers and others with professional or lay interest in science studies. It will explore topical issues by means of lectures, seminar sessions and contributed papers.

13 - 20 July: SCIENTIFIC CONTROVERSIES

The seminar will consider the causes and consequences of controversy in science and technology. There will be discussion of historical case-studies, science and ideology, "parascience", and the role of expertise in legal and political decision-making.

Guest lecturers will include: Harry Collins, David Elliott, John Hasted, Mary Hesse, Bernard Norton, and Roger Smith.

20 - 27 July: MEDICINE, ETHICS AND SOCIETY

Sponsored jointly by the University of Oxford Department for External Studies and the Institute for Society, Ethics and the Life Sciences at Hastings-on-Hudson, New York, the seminar will cover a wide range of issues including the rights and responsibilities of doctors and patients, the ethics of clinical experimentation, and the implications of new reproductive technologies such as *in vitro* fertilization.

Guest lecturers will include: Daniel Callahan, Arthur Caplan, Raanon Gillon, Carol Levine, Michael Lockwood, and Dame Mary Warnock.

The two weeks may be attended either separately or in combination.

The cost per week (inclusive of seminar fee and accommodation in The Queen's College) is £175 (U.K. and other E.E.C. residents) / £250 (residents of all other countries). Some bursaries are available for U.K. residents.

Write for further information to: Dr. J.R. Durant, Room 5.27,
Department for External Studies, University of Oxford,
3-7 Wellington Square, Oxford OX1 2JA
Tel. Oxford (0865) 52901

1985 Conference
AUSTRALASIAN ASSOCIATION FOR THE HISTORY, PHILOSOPHY AND
SOCIAL STUDIES OF SCIENCE
Call for Abstracts

The Australasian Association for the History, Philosophy and Social Studies of Science will hold its 1985 conference in conjunction with the Australasian Association of Philosophy at the University of New South Wales during the week of 25-31 August 1985. Plans are under way for three major symposia on the following topics: historical and philosophical aspects of the cognitive sciences (Peter Slezak, organizer); industrial research in Australia (George Bindon, organizer); and the history of anthropology and the study of culture (Guy Freeland, organizer). Potential contributors are encouraged to contact the relevant symposium organizer as soon as possible for further information. Abstracts of papers offered for possible inclusion in these symposia must be received by the program committee no later than May 1985. In addition to these three symposia, a number of sessions of a general nature are planned, for which offers of papers in any area of the history, philosophy and social studies of science are welcomed. Abstracts of those papers must be received by the program committee no later than 15 June 1985. Address submissions and inquiries to W. R. Albury (Convenor), George Bindon, Guy Freeland, or Peter Slezak:

School of History and Philosophy of Science
 University of New South Wales
 Post Office Box 1
 Kensington (Sydney), New South Wales
 Australia, 2033

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ANNOUNCEMENTS: POSITIONS AVAILABLE**Sarah Lawrence College**

Science and Society/History of Science. Possible position to teach at Sarah Lawrence College. Candidate should have expertise in the moral, political, economic and social issues arising out of the developments in modern science and technology, and an ability and interest in developing a teaching program to deal with these issues in an undergraduate environment. Requirements: successful teaching experience, a strong commitment to undergraduate teaching, and Ph.D. or an equivalent commitment to scholarship. At Sarah Lawrence, a small liberal arts college near New York City, faculty teach in seminar classes and individual tutorials. One year, full or half-time position, possibly a full-time or half-time three year appointment to follow. Send curriculum vitae, descriptions of research interests and outlines of two proposed courses with reading lists to: Charlotte A. Price, Chair of Division III, Sarah Lawrence College, Bronxville, New York 10708; deadline April 15, but an earlier response is encouraged. Affirmative Action/Equal Opportunity Employer; minorities and women encouraged to apply.

**Worcester Polytechnic Institute
Possible One-Year Visiting Appointment**

The Department of Social Science and Policy Studies at Worcester Polytechnic Institute, Worcester, Massachusetts 01609, expects that one of its members will be on sabbatical next year. WPI is therefore initiating a search for a sociologist interested in science and technology to teach an introductory sociology course keyed to preparing students for independent project work in Science, Technology and Society issues; Social Problems, a course on Science and the Creative Process and an advanced seminar on issues in technology assessment and social impacts of science and technology. Familiarity with the science indicators effort is expected to be useful, but not essential. This course serves a small group of Society-Technology Majors and other interested upperclassmen at the college. Coordination of activities for majors in this program, consulting on their project ideas and progress is a minor but stimulating part of this position. Contact John M. Wilkes at (617) 793-5578 for details.

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**Worcester Polytechnic Institute
Director, Division of Interdisciplinary Affairs**

Applications and nominations are invited for the position of Director of the Division of Interdisciplinary Affairs. The position is available on July 1, 1985.

Responsibilities: The Director of the DIA is responsible for the guidance and continuing academic development of WPI's undergraduate science/society degree requirement, the Interactive Qualifying Project (IQP). The Director, together with designated faculty members, will assist the faculty in forming interdisciplinary IQP interest centers, establishing long-term research agendas, attracting research funding, encouraging publication, and in maintaining high quality levels for student IQP performance. The DIA Director is also academic and administrative head for all non-traditional interdisciplinary majors.

Qualifications: Candidates must have a demonstrated ability to lead cross-disciplinary activities. We seek candidates with a broad academic background that includes elements of science or engineering and also those areas of the social sciences or humanities appropriate to a science/technology/society program. Candidates should have an earned doctorate, substantial teaching experience, distinguished scholarly achievement as evidenced by publications in STS areas, and a strong commitment to academic excellence. Candidates should have experience in obtaining extramural funds for STS research, and must be qualified for faculty appointment.

WPI: WPI, the nation's third oldest private engineering college, is located in Worcester, Massachusetts. WPI offers degrees through the doctorate with an undergraduate enrollment of 2,500, a graduate enrollment of 250 full-time and 1,000 part-time students, and a full-time faculty of 200.

Selection review will begin on March 1, 1985. Nominations, inquiries, and applications with resume may be directed to:

Chairman
DIA Director Search Committee
c/o Personnel Office
Worcester Polytechnic Institute
100 Institute Road
Worcester, Massachusetts 01609

**Center for History of Physics
American Institute of Physics**

The Center for History of Physics at the American Institute of Physics seeks an Associate Historian for a one-to three-year appointment. Applicants should hold a recent Ph.D. or a virtually complete dissertation in the history of modern physical science. The historian will help conduct and administer the Center's programs in oral-history interviewing of physicists and astronomers, the preservation and cataloging of documents, and the use of historical materials for educational purposes. The historian will have opportunities to pursue personal research interests in related areas. Salary is competitive. Work may begin at any mutually convenient time during 1985. Send letter, vita, and names of three references to:

Spencer Weart
Center for History of Physics
American Institute of Physics
335 East 45th Street
New York, New York 10017

**Brandeis University
Bern and Barbara Dibner Chair in
History of Science**

Brandeis University announces the establishment of the Bern and Barbara Dibner Chair in the History of Science whose holder will be expected to teach courses in the history of science in the department of history and to take part in the interdisciplinary program in the history of Western thought. Applicants must be formally trained in the history of science and be capable of handling materials from the medieval period to the twentieth century. Preference will be given to candidates whose field of specialization is the biological or physical sciences in the nineteenth and twentieth centuries. Applicants must possess the qualifications necessary for a tenured appointment at the associate or full professor rank. Inquiries should be sent to:

Professor S. Schweber, Chairperson
Dibner Chair Search Committee
c/o Department of Physics
Brandeis University
Waltham, MA 02254

ANNOUNCEMENTS: NEW NEWSLETTER**Society for Literature and Science**

The Society for Literature and Science (SLS), currently in the process of formation, announces the establishment of the SLS NEWSLETTER. Under the editorship of Professor Stuart Peterfreund/Department of English/Northeastern University, the SLS Newsletter will appear four times a year, with one issue devoted to the annual bibliography of literature and science, edited by Professor Walter Schatzberg of Clark University. The other three issues will focus on announcements, notes, and grant and fellowship information relevant to the interests of those engaged in interdisciplinary work in the area broadly designated by Literature and Science. In time, the SLS NEWSLETTER may also print brief articles and provide a forum for issues of interest to its readership, for example, how to prepare a successful grant application in the fields covered by literature and science.

The SLS NEWSLETTER will serve as the informal organ of the Society for Literature and Science, which will be inaugurated at the International Congress for the History and Philosophy of Science, held from 31 July to 6 August, 1985, in Berkeley, CA. Publication schedules will not be finalized until that time, but it is expected that the first issue to appear on a regular quarterly basis will be that of September-October 1985. Address all queries regarding subscription, submission of information, and other matters to the Editor:

Professor Stuart Peterfreund
 Department of English
 Northeastern University
 360 Huntington Avenue
 Boston, MA 02115
 (617) 437-3967; 437-2512 (messages)

ANNOUNCEMENTS: NEW AUSTRALIAN PUBLICATION**Metascience**

The Australasian Association for the History, Philosophy and Social Studies of Science has recently published the inaugural volume of its annual review, Metascience. This volume is a special double issue on the theme, The Value of the Sociology of Knowledge for the Study of Science, with contributions by Keith Hutchinson, John Schuster, Ditta Bartels, Ron Johnston, John Clendinnen and David Turnbull. Individual copies of this inaugural Metascience volume may be obtained by sending \$15 (U.S. currency) to:

School of History and Philosophy of Science
 University of New South Wales
 P. O. BOX 1
 Kensington, N.S.W.
 Australia 2033

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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:

J. Scott Long
4S Secretary
Department of Sociology
Washington State University
Pullman, Washington 99164

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, Texas A&M University,
College Station, Texas 77843

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

Richard Gillespie, (News), Department of History and Sociology of Science,
University of Pennsylvania, Philadelphia, Pennsylvania 19104

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

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