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THOUGHT AND OPINION

Disciplinary Boundaries: A Critical Synthesis

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I. A PRELIMINARY DIAGNOSIS AND PROGNOSIS

For our purposes, a discipline is "bounded" by its procedure for adjudicating knowledge claims. This procedure consists of an argumentation format which restricts word usage, borrowings permitted from other disciplines, and appropriate contexts of justification/discovery (some claims may be grounded on "reason alone," some on unaided perception, some on technically aided perception, etc.). There are at least three techniques for detecting disciplinary boundaries:

(1) Examine disciplines that adjudicate "ostensively similar" (Jones, 1983:132) knowledge claims. For example, philosophers (esp. epistemologists) appear to argue about claims that are also contested by linguists, psychologists, sociologists, and sometimes even physicists. Yet philosophy's argumentation format is quite different from the formats of the other disciplines. Failure to note this fact has led to premature reports of philosophy's obsolescence. In honor of a famous recent case (Rorty, 1979), we shall dub this oversight The Rorty Fallacy. It is remedied by using a discipline's argumentation format to infer the propositional attitude one is supposed to adopt to a given claim

(Fuller, 1982).

(2) Examine the metascience implicit in a discipline's argumentation format. When the claims of one discipline conflict with those of another, which discipline yields to the other's cognitive authority? The answer, which reveals the balance of cognitive power between the two disciplines, should be expected to change over time, as in the case of natural theology vis-a-vis natural philosophy. When the cognitive resources of one discipline are insufficient to solve one of its own problems, which other discipline "just outside" its boundary is invoked for help? When the validity of claims in one discipline is challenged, the validity of claims in which other disciplines is most threatened? Not only should the answers to these questions be expected to change over time, but they are also likely to be asymmetrical. For example, from the fact that political economy's model reasoner was originally drawn from the current (18th cen.) "rational man" psychology, it did not follow that once psychology surrendered the model (in the early 20th cen.) economics did so also immediately. However, the normative force of such historical observations should not be overestimated; indeed, Willard (1983:269) has argued that "borrowing incurs obligations," and so economics should have given up

the rational man model immediately after psychology did.

(3) Examine the strategies used to synthesize the research of two disciplines. Often a metalanguage is constructed for reducing the claims of the two disciplines to some "common ground" which mainly takes into account the synthesizer's intended audience. If the synthesizer relies on the lexicons of the original disciplines at all, it will be often by metaphorically extending word usage, perhaps so much so that the extensions would be deemed too ambiguous by intradisciplinary standards. And so, unless the synthesis itself spawns a new discipline, it is unlikely to affect the day-to-day workings of the original two. There is, however, one "intrinsically synthetic" discipline--pedagogy--whose recent British theorists have come to realize that while disciplinary boundaries may be the "grown-up" solution to the problem of knowledge, the idea of education presupposes that children can be taught the distinctive methods of all the disciplines (Degenhardt, 1982). Still, more typical of the fate of syntheses are the checkered attempts at fusing cognitive psychology and neurophysiology via artificial intelligence into "cognitive science," which is arguably a branch of philosophy (Haugeland, 1981).

The example of cognitive science also suggests that two disciplines separated by a boundary need not be limited to mutually exclusive domains of inquiry (Darden & Maull, 1977). Instead, they may cross-classify the same general subject matter. Let us call the phenomenon orthogonality (Fodor, 1981). For example, cognitive psychology and neurophysiology are orthogonal disciplines: reports of a particular type of mental state do

not always correspond to reports of the same type of brain state, yet each report of a mental state corresponds to a report of some type of brain state. And so, despite the fact that both disciplines study the thought processes of the human organism, laws interrelating types of mental states do not appear as laws when translated into the neurophysiologist's discourse. This is a key reason why the two disciplines cannot simply "build on" each other's work.

Interestingly, the same situation holds for such incommensurable domains as phlogiston chemistry and oxygen chemistry. In that case, though not all reports of phlogiston correspond to reports of the same substance in oxygen chemistry, each report of phlogiston corresponds to a report of some substance in oxygen chemistry (Kitcher, 1978). Indeed, orthogonality simply is incommensurability, but without the sense that only one of the two disciplines can survive in the long run. But even here the difference may boil down to one of historical perspective: two disciplines that appear merely orthogonal now may later be shown to have been incommensurable. It should come as no surprise, then, that a recent school of metapsychologists, eliminative materialists, have pre-empted history by arguing that only the familiarity of cognitive psychology's theoretical discourse (which refers to beliefs, desires, and other intentional entities) keeps it from being replaced by the scientifically more promising neurophysiology (Churchland, 1984:43-50). If such claims are generally correct, then disciplinary boundaries may be seen as fault lines concealing potential scientific revolutions.

II. ARE DISCIPLINARY BOUNDARIES NECESSARY FOR THE GROWTH OF KNOWLEDGE?

Here is a though experiment to test your intuitions on this issue. Suppose you were given our current corpus of knowledge and were asked to design the most efficient division of cognitive labor that would have produced the corpus. How different would your design be from the disciplinary boundaries drawn in the actual course of history? Start by treating this as a problem in bureaucratic management. You would then want to eliminate task redundancy by having each department of knowledge work on a discrete domain, with the research of several such departments coordinated at a higher level in the organization. Moreover, you would want perfect communication flow, with the work of the lower departments informed by, yet corrective of, the work of the higher ones. If you find this systematic strategy attractive, then you probably think that disciplinary boundaries are in principle dispensable, for, as we have seen, disciplines often cross-classify the same subject matter and impede any mutually useful synthesis.

However, history tells against the systematic approach. Its ideal hierarchy of domains--social groups (decomposed into) multicellular organisms, cells, molecules, atoms, elementary particles--overlooks that many key objects of knowledge have been products of orthogonality, including that emergent entity "man" (contra Oppenheim & Putnam, 1958). Indeed, anthropology is not merely a part of primatology, but the linguistic and technical reorganization of all bioevolutionary phenomena. This reorganizational may be seen in that while man's genetic similarity to apes seems to best explain our intelligence, man's ecological similarity to wolves must be invoked to account for our sociability (Graham, 1981:192). Thus, two features traditionally thought by

anthropologists to develop concomitantly--cognitive and social structures--appear unrelated from a bioevolutionary standpoint. This point is made more generally by the tendency of sociobiologists to draw on an assortment of species from various branches of the evolutionary tree--ants to apes--in order to model the full range of human phenomena.

Still, the systematist need not be deterred at this point. He can simply argue that these "products" of orthogonality are really problems that have arisen due to failures of communication and coordination; hence, anthropologists continue to practice their trade only from ignorance of sociobiology. An extreme version of this response--call it Boundary Berkeleyism--claims that boundaries arise solely because a discipline's activity is typically not monitored by other disciplines or the public, which allows a hermetic "insider's" discourse to develop. However, once "outsiders" enter this discourse--say, when a biology lab is invaded by sociologists--the boundaries dissolve as the insiders account for their work in terms quite familiar to the outsiders (Latour, 1981).

Admittedly, failures to meet the systematist's ideal may be by design, since disciplines actively set up boundaries to expand, protect, and monopolize their cognitive authority (Gieryn, 1983). And even if not by design, there may be more of such failures now than ever before, as the expertise of the average researcher decreases and his need to rely on the unquestioned authority of other experts increases (Friedson, 1984:14). Yet the systematist may generally agree with the eliminative materialist that these sociopolitical obstacles will be overcome in the long run.

Indeed, he may facilitate matters by pursuing a policy of reductionism (Neurath, 1983), which calls for the construction of, so to speak, an "interdisciplinary Esperanto." But on what shall this Esperanto be based? The logical positivists were torn between an authoritarian and a democratic solution: the former, physicalism, would have the lower disciplines recast their claims in terms of the cognitive authority delegated to them by the executive discipline, physics; the latter, phenomenism, would force all the disciplines to recast their claims in terms of a neutral medium of cognitive exchange (e.g., "sense data"), which the positivists would provide in the form of a "theory of evidence."

In either case, would the result look anything like the deep structure of knowledge growth? There is reason to think not (Feyerabend, 1981), which may mean that disciplinary boundaries are, after all, necessary for the growth of knowledge. A strategy that recognizes this possibility is encyclopedic, so named for the very embodiment of cross-classified knowledge, first proposed in 1751 by Diderot and D'Alembert with many of the above concerns in mind (Darnton, 1984). In particular, the Encyclopedists held that the orthogonality of disciplinary domains fostered the growth of knowledge by permitting one discipline to problematize the research of another discipline, thereby ensuring that the highest critical standards were maintained by everyone--a situation that did not obtain when theology was "queen of the sciences." In fact, Diderot was to averse to the systematist's ideal that he suspected Newton's mathematical physics of trying to replace theology in the role of cognitive despot (Prigogine & Stengers, 1984).

III. WHEN DISCIPLINES COLLIDE: THE BERNARD PRINCIPLE

Once a discipline's domain of inquiry has been "staked out" (Cambrosio & Keating, 1983), its practitioners must define and maintain the "normal" state of objects in the domain. This involves experimental and textual techniques for foregrounding the problematic claims under study against a background of claims that are stipulated to be unproblematic. The need for a vacuum against which to demonstrate the laws of motion is perhaps the most famous of these normal states. Philosophers have referred to this activity as "filling in the ceteris paribus clause," whose recognized function is to protect the claims problematized by a discipline from being too easily falsified by extradisciplinary considerations (Lakatos, 1970:110). However, philosophers have generally overlooked the fact that the means by which such a clause is filled in may require obscuring objects and processes that are potentially the concern of other disciplines. The term "ontological gerrymandering" (Woolgar & Pawluch, 1984) nicely captures this phenomenon. Consider Claude Bernard's demarcation of experimental medicine from the rest of biology. Bernard defined the normal state of an organism in terms of its milieu interieur, the equilibration of the organism's blood and lymph flow. For example, a rise in temperature in a mammal's milieu exterieur leads to vasodilation, which allows some heat to escape from its body, thereby restoring normal body temperature. Disease is simply the failure to make such homeostatic adjustments, as judged by a physician upon seeing the organism function in the new milieu exterieur (Canguilhem, 1978:29-45). It would seem that Bernard had drawn the boundary of

his discipline very clearly--perhaps too clearly. Nineteenth century France was slow to accept microbiology and evolutionary biology, and the autonomy of Bernard's discipline may be partly to blame (Mendelsohn, 1964). First, Bernard defined disease from the patient's standpoint--organic disequilibrium--rather than from the standpoint of a pathogenic agent; second, experimental medicine had no conceptual place for a disequilibrium that could not in principle be medically corrected, such as an organism's inability to adapt to a radically new milieu exterieur. The former disallowed microbes, the latter natural selection. It is thus fitting that we label the thesis that a disciplinary boundary can be drawn only at the risk of excluding other possible disciplines The Bernard Principle.

The Bernard Principle assumes many forms, which are particularly well illustrated in the various attempts to stake out the foundational discipline of the human sciences. Unfortunately, we can do little more than cite seven easily overlooked cases:

(1) Descartes foreclosed the possibility of a sociology of knowledge when he declared at the start of the Meditations that one must entirely withdraw from daily life in order to consider the nature of things. Virtually all previous philosophers had recommended a restricted institutional setting, such as a school.

(2) Classical political economy, in search of a unified theory of value, followed the "Newtonian" strategy of reducing the different kinds of value in objects to their lowest common denominator, commodity value. As a result, the discipline could not in principle distinguish between

the value of human labor and the value of a product of human labor, which implied that it had taken the "human" out of the human sciences. Moreover, as Marx saw, the alienation of labor under capitalism threatened to remove all reminders that such a distinction needed to be made (Althusser, 1972).

(3) By writing A System of Logic, J.S. Mill intended to lay the groundwork for the "moral sciences." That the book did, after it was translated into German in 1849 and read by Wilhelm Wundt, the founder of experimental psychology. Mill's Logic also unintentionally rendered the discipline of history, and later all the humanities, problematic. History, as practiced by Ranke and Niebuhr, claimed to study the past in its specificity. However, Mill's empiricist epistemology did not permit this possibility: either one knows the past in its generality (by induction, the psychologist's way) or one knows the present in its specificity (by direct acquaintance, the ordinary way). Verstehen was Diltbey's attempt at bridging this difficulty. But notice that the Geisteswissenschaften (the German translation of "moral sciences") would not have appeared problematic, had Mill's radical empiricism not been presumed unproblematic by late 19th cen. German methodologists (Fuller, 1983).

(4) In order to draw a clear boundary between psychology and physics, Wundt trained subjects to report only their "sensations" and not the physical objects "inferred" from them, which figure in ordinary accounts of experience; hence, in a vision experiment, one would report a "red-round-shiny-presence" instead of an "apple." This stricture also served to prevent psychology from studying higher mental processes. Wundt justified the move on the grounds that reliable results could

not be ensured for the higher processes. Nevertheless, the Wurzburg School performed the forbidden experiments and were promptly accused of committing "stimulus error" and practicing (mere) philosophical introspection. In a 1913 review of the curious difficulties encountered by Wundt in training subjects to issue proper reports, Wolfgang Koehler (1971:13-39) concluded that Wundt was so concerned with protecting the autonomy of psychology as a science that he missed the point of all his difficulties, which was that the distinction between raw sensations and cognitive meaning is an empiricist philosophical myth whose survival was due entirely to the "artificiality" of Wundt's own technique. Soon thereafter Koehler spearheaded the Gestalt movement.

(5) Although behaviorist accounts are notorious for avoiding any reference to mental processes not accessible to the experimenter, less noticed has been the fact that behaviorist experiments are typically designed to minimize the appearance of the traditional "outward signs" of mental process. For example, Thorndike operationalized animal intelligence as a function of learning rate, the aim being to lower the time taken by an animal to solve the "puzzle box," a prototype of the Skinner Box. He supposed that once placed in the box, the animal knew what was expected of it; consequently, any of its behaviors that did not contribute to a solution were counted as errors. However, mental process had been thought to involve a blockage of the motor nerve pathways, which reduced the kind of hesitancy associated with deliberation--in other words, a lengthening, not a shortening, of response time. Moreover, the postulation of an active mind stemmed from a view of the animal as

always being in a situation that may be rendered intelligible in many different ways. In fact, this view especially suited non-human animals, which were never expected to have understood what the experimenter had planned for them. Thus, an animal's initial fumbings in the puzzle box should be regarded as attempts at determining the problem rather than as failed attempts at a solution. Interestingly, Koehler (1971:197-223) was also behind this critique of behaviorism, from which he concluded that in order to retain its identity as a "science of mind," psychology must relinquish the efficiency aims of instrumental conditioning.

(6) Sensitive to the attempts by Sechenov, Pavlov, and Bechterev to "naturalize" the study of man, the school of critics known as "the Russian formalists" sought to ground "literary science" on a methodology designed to break the laws of classical conditioning, both by blocking the conditioning of linguistic reflexes and by extinguishing such reflexes once they had been conditioned. The exemplar of this process of "defamiliarization" was the difficult poetry of the Symbolists, which forced one to reconsider his normal reading habits. The formalists were philosophically influenced by Ernst Mach (and hence roundly condemned by Lenin in Materialism and Empirio-Criticism), which led them to regard the literary scientist as a technician much in the same way as the "reflexologist" regarded himself--the difference being, of course, that the literary scientist develops techniques for systematically undermining the effects of reflexology (Shklovsky, 1965). This, so to speak, "neopositivist aesthetic" legitimated several modern art trends, most notably Surrealism's subversion of

perspective. After the Bolshevik Revolution, the formalists emigrated to Prague and eventually to Paris, where one of their number, Roman Jakobson, started the recent Structuralist movement.

(7) Freud's avowed aim in declaring sanity to be the limiting case of neurosis was to make all psychological phenomena, not merely pathologies, fair game for the psychoanalyst. More interestingly, since all sane people are virtual neurotics, the self-reports made during Wundt's controlled introspection cannot be presumed unproblematic, but must first be decoded for repressed messages from the unconscious. This justified Freud's dismissal of experimental psychology as pseudo-scientific, a charge that would later be levied against psychoanalysis, but which at the turn of the century contributed to the fall of the introspected paradigm. However, Freud also refashioned certain introspection techniques for his own ends, largely through the influence of Carl Jung, a student of the Wundtian psychopathologist Emil Kraepelin. For example, Jung introduced free association, which Kraepelin had used as a diagnostic tool for identifying fixations by counting how often the same word appeared in a subject's set of responses. Psychoanalysis supplemented the method, against Wundtian strictures, by drawing inferences about the significance of the repeated words (Maher & Maher, 1979:566-573).

IV. DISCIPLINARY AMBIVALENCE; POPPERIAN AND FOUCAULDIAN VERSIONS

Robert Merton (1976) introduced the concept of sociological ambivalence to capture the fact that social roles often serve multiple functions whose performance cannot be jointly maximized; moreover, society does not usually offer any ready-made

rules for making the necessary trade-offs, which leaves the role occupant in a state of tension. For example, the scientist is supposed to both expedite the flow of knowledge and not rush into print. But how can he "expedite" without also "rushing"? While Merton tends to suppose that all scientists experience the same kinds of ambivalence, a finer grained analysis of the concept might reveal that each discipline has a characteristic way of resolving its ambivalences, which, in turn, becomes the basis on which its cognitive status is evaluated by other disciplines and the public at large. This thesis of Disciplinary Ambivalence may be illustrated by considering the multiple linguistic functions performed by disciplines. Our model, adapted from Popper (1972:119-121), specifies four such functions, each associated with a virtue of disciplinary discourse:

(1) The virtue of signalling is efficiency. A discipline aims to convey the most information per unit of discourse expended.

(2) The virtue of expressing is surveyability (Wright, 1980:117). A discipline aims to make each step of its reasoning evident in its discourse.

(3) The virtue of describing is accuracy. A discipline aims to maximize the total amount of truth conveyed in its discourse.

(4) The virtue of arguing is precision. A discipline aims to minimize the total amount of non-truth conveyed in its discourse.

We shall call the ambivalence that arises between (1) and (2) Foucauldian after the most famous recent student of disciplines as the means by which knowledge is used to control nature and culture (Smart, 1983:108-122). Foucauldian ambivalence has little interested analytic philosophers of science, with the exceptions of Toulmin

(1972:261-318) and Elkana (1981). In contrast, the ambivalence that arises between (3) and (4) has been virtually the sole concern of analytic philosophers. We shall call it Popperian after the philosopher who took a particular resolution of this ambivalence--namely, maximizing the precision of hypotheses at the risk of their accuracy--as the criterion for demarcating "science" from other disciplines (Popper, 1972:193). As we shall see, both the Foucauldian and Popperian species are needed for providing a framework capable of charting the history of Disciplinary Ambivalence. But first let us explore the considerations involved in each ambivalence.

Foucauldian Ambivalence. Grice's rules of conversational implicature, especially the quantity maxim (Leech, 1983:84-89), force a trade-off between efficiency and surveyability. For any given discipline, the most efficient discourse would convey only new information, with the necessary background information supplied by the intended audience. However, this move would also minimize surveyability by making the discipline's discourse accessible only to insiders. Still, the discipline might not at first regard this hermeticism as such a loss, for if its discourse also exhibits a measure of accuracy and precision, then the insiders will, in effect, have knowledge over some domain, the source of which would remain a mystery to outsiders unable to survey the discipline's reasoning. This is the cult of expertise, associated with the professionalization of knowledge (Collins, 1975:520-523). Furthermore, if the insiders are experts on something that affects the behavior of the outsiders, then the formal definition of institutional power has been

satisfied (Crozier, 1964:158). Yet the promise of power is not enough to dispel the ambivalence. Some degree of surveyability is necessary for pedagogical purposes. In fact, one of these purposes is also relevant to mature practitioners of the discipline, namely, if each step of one's reasoning is made explicit, then errors, misunderstandings, and disagreements can be localized and treated as they occur. But this depends on complete surveyability, which is impracticable, since members of the same discipline generally work in disparate communities with only the elusive medium of print connecting them (Collins, 1974:183). If unchecked for long, incomplete surveyability may lead to deep misunderstandings between such communities, engendering "schools" and perhaps even Kuhnian incommensurability, which is often followed by the formation of new disciplines (Mulkay, et al., 1975:198). Thus, while maximizing efficiency promises adumbrates outside the discipline, minimizing surveyability adumbrates instability within the discipline.

Popperian Ambivalence. There is something fundamentally paradoxical about Popper's falsifiability thesis that instills the ambivalence bearing his name. The thesis implies that a discipline becomes scientific once its members realize that eliminating errors is, in the long run, the most effective means of accumulating truths--indeed, more effective than simply trying to accumulate truths in the short run. Furthermore, the short and long run pursuits of truth are incompatible--the former being guided by accuracy and the latter by precision. Accuracy demands that a discipline issue highly probable claims, while precision demands that it issue highly improbable but (ultimately) true claims. Consider a claim that has just been

shown false. How does the discipline correct the error? The easiest way of increasing the claim's probability is by specifically excluding the falsifying case--say, by appending an ad hoc hypothesis or by nimbly rewording the original claim. In both cases, the discipline has adjusted its discourse, more to describe past encounters with the world than to anticipate future ones. While some (Skorupski, 1976:205-223) have taken this dogged pursuit of accuracy as emblematic of the subrational mind, others (Bloor, 1978) have pointed out that not even mathematicians are immune to its charms. In contrast, Popper would have the discipline would be advised to make it even more vulnerable by placing further constraints on the possible situations that will subsequently corroborate it. And so, whereas the pursuit of accuracy encourages consensus (and perhaps even stagnation) in a discipline, the pursuit of precision promotes divisiveness as its members undermine each other's claims in the course of circumscribing their truth content. However, contra Popper, the dogged pursuit of precision is more a gamble than a guarantee for ultimate truth. What precision does guarantee is a quick turnover in claims, which may lead to the discipline's demise, if some fairly uncontested truths are not collected along the way (Mulkay et al., 1975:195).

Can a historical trend be discerned in the resolution of Disciplinary Ambivalence. If we look at the full panoramic sweep of organized knowledge in the West (say, from Thales to the present), the trend has been to accord high cognitive status to disciplines whose discourses maximize efficiency and precision at the expense of surveyability and accuracy. Moreover, this cognitive status has

been "accorded" not only through the plaudits dispensed by philosophical kibitzers but also through the allocation of economic and political resources. The facts surrounding this trend are by now familiar to sociologists: the increased division of cognitive labor, the increased frequency with which disciplines come and go, the increased technical control over well-defined domains, the increased store of undigestible information. Of course, even to the most Whiggish eyes, the sweep presents several slowdowns and setbacks along the way. One of these seems to have a permanent place in the social structure of knowledge--namely, "folk wisdom"--which, regardless of content, resolves Disciplinary Ambivalence in a manner diametrically opposed to the trend, by maximizing surveyability and accuracy. A discipline (or its wayward practitioners) whose discourse heads in this direction is thus engaged in "popularization."

A more interesting deviant resolution is to maximize surveyability and precision in one's disciplinary discourse. This is the image of science as "conjectures and refutations" on which Popper and most analytic philosophers of science have fixated. Whatever its normative status, sociologically it has been limited to the Athenian polity and scholastic disputation--and therein lies a tale. Although it is well known that the Athenians were entertained by public debate, less known is the fact that so were the scholastics by disputation. In fact, there is reason to believe that, at least for Aquinas, the primary concern in composing a disputation as a series of objections followed by responses was to heighten the suspense of the lecturer's dialectical fate. Moreover, in order to make the lecture's task seem as formidable as

possible, trivial and important arguments were thrown together indiscriminately so as to pad the number of opening objections (McInerny, 1983:263). The net result was high drama, indeed, but also a rather ineffective way of gauging cognitive progress, as was observed by those otherwise opposed ideologues of the Scientific Revolution, Bacon and Descartes. No doubt such unmitigated enthusiasm for dialectics has also contributed to scholasticism's confusing legacy. But most importantly, since scholasticism has more closely approximated, on a large scale, the "up-against-all-odds" attitude to inquiry than any other discipline, its fate suggests that a proviso needs to be added to the Popperian imperative: namely, that a discipline's practitioners should not get so caught up in the activity of falsification that they lose sight of falsification's long term goal of truth, or, in an anti-Kantian spirit, falsification should always be a means and never an end-in-itself.

The last possible deviant resolution of Disciplinary Ambivalence remains the subject of much controversy (Adorno, 1976). Maximize efficiency and accuracy in one's discourse: so goes the imperative of the hermeneutical disciplines--theology, jurisprudence, and literature, as well as several continental schools of history and philosophy, and perhaps even sociology (Baldamus, 1976:18-29). Since our terms for the linguistic virtues seem to lead to counterintuitive results in this case, let us consider a piece of hermeneutical discourse, the brocard (Tourtoulon, 1922:310). A brocard is an apophorism that often introduces a judge's decision in civil law countries. The decision itself takes the form of a commentary on the brocard, during which the case under consideration is shown to

exemplify the judge's interpretation. Brocards are designed to be used repeatedly for various cases by various judges, the overall effect being to lend continuity to the legal tradition. They are thus short and memorable, but also somewhat oracular, since their intended range of application is wide. Indeed, brocards are generally worded so that contradictory propositions can be read off them. And so, since brocards can be used on all occasions--under different interpretations--they are quite literally never wrong (hence, they maximize accuracy); furthermore, brocards also convey the most information possible, a proposition and its negation (hence, they maximize efficiency). Take, for instance, "The end never justifies the means." There is, of course, the standard Kantian reading of this brocard, but a clever judge can also divine an Ultra-Machiavellian reading from it: the end alone never justifies the means, for the means should also have some desirable byproducts. However, the cost of hermeneutical discourse is high: hardly surveyable at all, nevertheless, unlike natural scientific discourse, its studied aversion to admitting error renders hermeneutics incapable of registering cognitive change--a fact having reactionary methodological and ideological consequences, as Popper (1972:183) and Habermas (McCarthy, 1978:169-187), respectively, have noted.

The cognitive degradation of hermeneutics has been long in the making. Perhaps because many hermeneuticians today define their own activity as "emotive" or "pragmatic" instead of "cognitive," it is often forgotten that the revival of classical learning known as the Renaissance was largely a "Hermeneutical Revolution."

Cognitive "progress" then consisted of clarifying one's understanding of the wisdom contained in the ancient texts. Such texts were thought to be wise precisely because of their authors' temporal proximity to the Creation. Indeed, this sense of cognitive progress also legitimated the Ciceronian theory of translation, which we still accept today, albeit rather uncritically: translate so as to preserve the content of the original (Why not aim, instead, to preserve in translation the response that the text elicited from its original audience?). Even during the Scientific Revolution, no less than Isaac Newton can be found to justify his harmonious world-system as an explication of the prisca sapientia of the early Greeks and Hebrews (McGuire & Rattansi, 1966). However, the precedent for regarding hermeneutical knowledge as both old and obsolete had already been set. In an apparent attempt to integrate the recently recovered Aristotelian corpus into the scholastic curriculum, Aquinas argued that Aristotle's natural scientific method fully articulated reliable routes to knowledge that had only been inchoately expressed by the more hermeneutical liberal arts (McInerny, 1983:258). When and why did the balance of cognitive power finally weigh decisively against hermeneutics? The answers to these little examined questions are far from clear. But an important key lies in an interdisciplinary history of the theory and practice of translation, which has so far been subject to purely "internalist" treatments (Bassnett-McGuire, 1980). In particular, one would want to see under what conditions translators started to challenge the intuition that maximal understanding is necessary for making the most use of another's text; for what probably separates practitioners of the "sciences" most from the those of

the "arts" is the scientist's sense that the time and effort spent in interpreting his precursors is time and effort taken away from contributing to the growth of knowledge (Fuller, 1983 a).

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REVIEW ESSAY

Discovery in Modern Astronomy

John Lankford
University of Missouri-Columbia

[Reviews of Richard F. Hirsh, Glimpsing an Invisible Universe: The Emergence of X-Ray Astronomy. Cambridge: Cambridge University Press, 1983. Pp. viii + 186; and Martin Harwit, Cosmic Discovery: The Search, Scope & Heritage of Astronomy. New York: Basic Books, 1981. Pp. xi + 334.]

The books reviewed here unfortunately might be overlooked by students of science studies. Neither appears to reflect methodological or theoretical perspectives in mainstream science studies. Both, however, are provocative. They are concerned with discovery. In addition, Harwit deals with major policy issues.

The Hirsh volume is organized as an historical narrative. It contains a wealth of information that needs discussion and interpretation. Much of this material deals with instrumentation and the process of discovery as well as relationships between data and theory. As a social historian, I am not entirely comfortable with current thinking (Mulkey, 1979, 29-49) in the science studies community on theory dependency of observations. The notion of theory most often encountered is far too broad. It encompasses a wide variety of social and cognitive acts from labeling to axiomatization. By paying close attention to the sequencing of historical events in the early history of X-ray astronomy it may be possible to gain a better understanding of the steps involved in making observations dependent on theory. This is, I believe, a process that occurs over time and in a series of ever-widening social contexts.

The process of discovery begins in a social historical context. In the case of X-ray astronomy the context includes traditional patterns of resistance displayed by established astronomers toward new research technologies (Lankford, 1981, 1984). In the post-War period optical astronomers were slow to respond to the possibilities of both radio (Edge and Mulkey, 1976) and X-ray astronomy. Indeed, in the new field of space science NASA experienced great difficulty attracting astronomers (Hirsh, 31-33; Tatarewicz, 1984). At first those who sought to observe astronomical objects using X-ray and radio portions of the spectrum tended to be marginal: migrants from physics and engineering or, as was the case with leaders in X-ray astronomy like Bruno Rossi and Riccardo Giacconi, transplanted Europeans. Frequently, their base of operation was marginal to mainstream astronomical research. This was true for teams working at the Naval Research Laboratory (NRL) and American Science and Engineering (AS&E), a private Boston-based company advised by Rossi that employed the young Giacconi.

These outsiders were able to take risks. In the 1950s astrophysical theory predicted weak X-ray fluxes from stellar sources. Expected levels were far too low to be observed with available instrumentation. There was no

reason to suppose theorists in error since solar X-ray fluxes confirmed predictions (Hirsh, 32-33). However, physicists Herbert Friedman at NRL and Rossi were both drawn to the problem. As Rossi recalled, "Whenever a scientist ventures into a previously unexplored field, he's likely to be confronted with entirely new and unexpected phenomena....I didn't have any specific reason to anticipate the emission of strong fluxes of X-rays from stellar objects. The only thing I knew was that no one had looked for them. Or at least no one had looked with sufficiently sensitive detectors" (Hirsh, 35). In this case scientists were guided by hunches rather than astrophysical theory in planning an observing campaign. They were led by deep-seated exploratory instincts. At this initial stage, the data owed their existence to the imagination, will and technical skill of a handful of observers. Put another way, these data had but one role: as products (perhaps artifactual) of observations obtained with very simple counters flown on rockets above the atmosphere. As yet these observations made no public cognitive claims. There were no reference groups, no users of the data.

As the NRL and AS&E teams produced more observational material, X-ray astronomy entered a new phase. Consumers appeared when theorists external to the emerging specialty put its findings to use. In the 1960s cosmologists disputed the merits of the steady state and the big bang theories of the origin of the universe. Big bang theorists impressively argued that observations indicating the existence of weak isotropic X-ray radiation favored their position. The successful use of data enhances its status. Data take on new roles as a wider public finds them of

value and theory dependency enters a new phase (Lankford, manuscript).

At this point X-ray astronomy was identified as a coming field. Between 1967 and 1970 new research technologies were developed with support from NASA and the military. The Aerobee rocket replaced earlier launch vehicles and instrument packages become more sophisticated. AS&E scientists, led by Giacconi, set to work designing X-ray "telescopes," but would need satellites rather than rockets in order to deploy this technological innovation. When X-ray observers were able to pinpoint the location of these new objects, optical astronomers joined in their study.

In 1964 Friedman and his NRL team performed one of the most elegant experiments in contemporary astrophysics. They observed the occultation of the Crab nebula by the Moon at X-ray wavelengths from an Aerobee rocket. Friedman expected these observations would prove the X-ray emitter in the Crab nebula was a point source, presumably a neutron star. The results indicated the opposite: the X-ray source was diffuse. It could not be stellar in nature. Theorists used this information in developing a model of the physical processes that produced X-ray radiation in a non-stellar environment.

A few tentative conclusions concerning the relation between observations and theory may be drawn from this case study. The process by which observations become theory dependent is neither linear nor additive. Observational data oscillate, sometimes violently, as rival theories compete for their use. Only when the scientific community completes the difficult process of consensus building does the supporting observational infrastructure attain the status of

full-fledged theory dependency.

Two teams dominated X-ray astronomy in the U.S. throughout the 1960s. Eventually they reached informal agreement. NRL concentrated on developing programs that would survey the sky for X-ray sources while AS&E focused on intensive studies of individual objects (Hirsh, 92). The NRL group exhibited a conservative approach to instrumentation. AS&E, on the other hand, was more innovative. With help from scientists at MIT, the AS&E team created a new generation of X-ray detectors utilizing Giacconi's ideas. The group then sold NASA on the idea of an X-ray astronomy satellite which was launched in 1970.

Prior to 1970 available research technologies provided a total of only two or three hours of observing time. Here we find the most significant linkage between instrumentation and observation revealed in this study. Launch vehicles of the 1960s permitted only brief periods of data collection for a given object. Further, detectors had very low resolution. The new generation of detectors carried on satellites was able to observe objects for long periods of time with greater sensitivity, producing data that would lead to the binary star accretion disk theory of many X-ray sources.

Hirsh believes it an article of faith among scientists "that as their tools gained sophistication, so did their conceptual understanding" (Hirsh, 122). The narrative implies one other article of faith as well: hunches and intuitions may be more useful guides than physical theory in planning new projects. If anything, at this primal stage in the discovery process, observations are heavily laden with personality

rather than theory.

Harwit's volume is too complex and richly textured to be easily summarized. With Hirsh, he shares a belief in the centrality of instrumentation in expanding astronomical knowledge. Harwit attempts to prove this through a series of historical sketches ranging in length from a paragraph to several pages (Harwit, 55-153). These vignettes do not succeed as either history or sociology. Based on limited documentation, they assume discovery is straightforward, involving complete and virtually instantaneous acceptance of new findings by the whole scientific community.

Perhaps it is most fruitful to read Harwit as an essay in the new science policy for post-affluent America. It fits with current thinking in the Reagan administration as well as among many social analysts. Discussions of individualism and deregulation are interspersed with admonitions that federal funding for research has reached its upper limits. Further, there is nostalgia for the good old days; in this case, not a time of frontier individualism and traditional family values, but rather a golden age of scientific research, ca. 1960-1966.

At the heart of the discussion is Harwit's desire to make science policy more scientific. If policy making can be developed as an empirical science, then it can be separated from politics, preferences and values. To achieve this end, Harwit embarks on a perilous voyage. He attempts to predict the number of cosmic phenomena left for astronomers to discover and the rate at which discoveries occur (Harwit, 13-51; 291-301). On the basis of quantitative analysis, Harwit concludes that there are about 130

multimodal phenomena left to discover and that by the year 2200 ninety percent of them should be known. The quantitative discussion is supplemented by a list of forty-three discoveries (Harwit, 16) plus the lengthy historical discussion referred to earlier. It is not clear just how the list was compiled. However, it obviously reflects the views of an astrophysicist rather than a student of astrometry or celestial mechanics.

Harwit's theory of discovery is simply stated. It is based in part on the assumption that scientists have an "intuitive grasp of what is meant by a new phenomenon." Because of this, scientists are "quick to recognize" a new phenomenon "when it is first discovered" (Harwit, 35). Instrumentation is central in the discovery process: "Repeatedly, discoveries of novel cosmic phenomena have been made by men and women who looked at the universe with new instruments and came to see it from a fresh perspective" (Harwit, 153). The theory is neat and has no problematic aspects, resting as it does on intuition and instrumentation.

Harwit does not believe that planning since World War II has aided the discovery process in astronomy. In his view, the 1960s were a golden age of discovery, but also a time in which there were minimal efforts at planning astronomical research. When the National Research Council and the National Academy of Sciences, encouraged by the Committee on Science and Public Policy, produced a series of reports establishing priorities for research and funding in astronomy, the discovery rate fell. To Harwit this is proof that "the cornerstone for astronomical progress still is the individual researcher, not the large committee"

(Harwit, 50).

Much of Harwit's analysis rests on a model of the scientist as entrepreneur. Indeed, there is an element of romantic individualism in his thinking. Science (that is, in Harwit's view, the process of making discoveries as opposed to analysis) is seen as an "exciting, highly personalized adventure" (Harwit, 229) in which gifted individuals make important discoveries with modest equipment (Harwit, 280-281). However, as a field matures (e.g., moves farther away from the epoch of its first dramatic discoveries) it becomes caught up in the big telescope syndrome (Harwit, 281). Concern shifts from discovery to the process of creating and funding larger and larger instruments which are seen as the only way the field can make progress. Emphasis has shifted from the individual to the instrument.

The book closes with a series of policy recommendations. Some of these recommendations are non-controversial. For example, Harwit suggests more intensive training in physics at the graduate level and urges the development of mechanisms to permit retooling in mid-career. He also asks that ways be developed to attract workers who have created novel instrumentation in other fields (Harwit, 252-253).

Much more controversial are recommendations covering funding and long-range planning. NASA and NSF should develop programs that will support the creation of new instrumentation. Grants must run for long periods and peer review should be made less rigid. Only with these changes will innovative astronomers be supported (Harwit, 254-259). Harwit recommends that activities of national observatories be reviewed. Since none of the discoveries on his list was made at

national astronomy centers, Harwit doubts the value of these institutions. By implication, at least, the money might better be spent on individuals (Harwit, 259-273). Panels charged with planning do not represent cutting edge work. They are caught up in recommending larger instruments rather than seeking innovative fields to support. Their priorities are too inflexible and need frequent review (Harwit, 279-287). Harwit concludes that "the benefits and harm accruing from astronomical plans should be studied with care to see the extent to which regulation or deregulation of astronomy might lead to greater progress" (Harwit, 287). Many would see the recommendations for new areas worthy of funding as equally controversial. Harwit urges support for neutrino astronomy and the search for gravitational waves (Harwit, 274-278).

Some of Harwit's recommendations will find a sympathetic response. Peer review has often been questioned and a more cost effective approach to research support would be welcomed by politicians and science policy administrators. But Harwit is caught up in a web of intolerable contradictions. In order to make science policy objective he has created a master list of discoveries. This will serve as a yardstick to evaluate (at least retrospectively) the work of many observatories including federally funded national astronomy centers. But to accept the criteria of discovery and list proposed by Harwit is to be tried to a procrustean conception of what the scientific enterprise is all about. Discovery, as Harwit uses the concept, deals only with the identification of new cosmic phenomena, not their explanation. A full historical and sociological analysis must include not only

initial identification, but the process by which these data become facts and then discoveries. This is, as Hirsh demonstrates for X-ray astronomy, not a linear progression.

There is a second, more glaring contradiction. Harwit's yardstick and projected pattern of discovery to the year 2200 provide objective criteria by which future research could be judged. Far from encouraging the individual, committees, panels and an expanded bureaucracy would monitor the progress of science. Quotas, targets and five or ten year plans aimed at adding to the list of discoveries would become commonplace. Thus, science policy would not become more empirical, only more bureaucratic.

However well intentioned, this attempt at developing an empirical foundation for rational planning of astronomical research is misguided. To be sure, most of us would agree with Harwit's concluding recommendation: "To facilitate future astronomical planning, a continuing effort must be made to improve our understanding of the factors that lead both to conceptual and observational advances in astronomy. That improvement can only come about through historical and sociological studies conducted at the highest levels of professionalism" (Harwit, 288). Indeed, the statement should be generalized to all the sciences.

Many research projects demonstrate little more than how not to attack a problem. Yet even in failure they can be useful. Martin Harwit has correctly identified the problem, but failed to provide solutions. Clearly the problem is fundamental to science policy planning and evaluation. How are we to use the tools of history and sociology of science to improve our understanding

of those factors that lead to conceptual and observational advances? With Harwit, I believe it can and should be done. The next step is to decide how (Cozzens, forthcoming). This is a major challenge facing the science studies community, perhaps the major challenge.

Notes

1. Cozzens, S., et. al. Forthcoming. Federal Funding and Knowledge Growth. Scheduled for publication as a special issue of Social Studies of Science.
2. Edge, D.O. and M.J. Mulkey. 1976. Astronomy Transformed: The Emergence of Radio Astronomy in Britain. New York: Wiley.
3. Lankford, J. 1981. Amateurs and Astrophysics: A Neglected Aspect in the Development of a Scientific Specialty. Social Studies of Science 11:275-303.
4. _____. 1984. The Impact of Photography on Astronomy. In Astronomy and Twentieth-Century Astronomy to 1950, Part A, ed. O. Gingerich, pp. 16-39. Cambridge, Eng.: Cambridge University.
5. _____. Manuscript. Priorities, Funding and Ripeness in the Age of Little Science: A Case Study and a Model.
6. Mulkey, M. 1979. Science and the Sociology of Knowledge. London: Allen & Unwin.
7. Tatarewicz, J.N. 1984. Where Are The People Who Know What They Are Doing? Spaced Technology and Planetary Astronomy, 1958-1975. Doctoral Dissertation: Indiana University.

**SOCIETY FOR SOCIAL STUDIES OF SCIENCE
10TH ANNUAL MEETING**

RENSSELAER POLYTECHNIC INSTITUTE, TROY, NY

OCTOBER 24-27, 1985

PRELIMINARY PROGRAM

THURSDAY, OCTOBER 24

Thursday, 5pm Publication Committee

Thursday, 7pm Council Meeting

FRIDAY, OCTOBER 25

Friday, 7am 1986 Program Committee

Friday, 9-11:45am

The cultural coding of scientific activity

Chair, Edward Manier, University of Notre Dame

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

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

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

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

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

Econometric, bibliometric, and sociometric analyses of science and technology

Chair: Wesley Shrum, Louisiana State University

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

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

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

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

"Publication and referencing patterns in a Mexican research institute"

Michelle Lamont, Stanford University, "La question de l'autonomie relative du champ des sciences humaines au quebec: essai de positionnement des disciplines"

The social production of science

Chair, John Wilkes, Worcester Polytechnic Institute

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

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

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

John M. Wilkes, WPI, David Vachon, St. Joseph's College, and Brian Starr, WPI, "Cognitive orientation, background and sex as determinants of career choice in the sciences"

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

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

Friday, 1:30-5pm

Tour of nineteenth century industrial sites of Troy, NY

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

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

Friday, 1:30-3:25pm

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

Chair, Pinna G. Abir-Am, Harvard

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

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

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

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

Everett Mendelsohn, Harvard, Commentator

Friday, 3:30-5:30pm

Naturalistic explanations of scientific knowledge

Chair, Ronald N. Giere, Indiana University

Steve Fuller, University of Pittsburgh, "Critique of Bloor on objectivity"

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

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

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

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

Friday, 7pm 4S Banquet Bernal Award Bernal Address

SATURDAY, OCTOBER 26

Saturday, 7:30am

Breakfast Discussion Session--"Education in Science, Engineering, and Public Policy"

Presentation and Discussion led by Albert H. Teich, AAAS

Saturday, 9am

China's policy for scientific research and industrialization

Chair, Dennis Simon, MIT

Anthropological studies of scientific and technological communities

Chair, Sharon Traweek, MIT

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

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

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

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

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

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

Saturday, 1:30-3:25pm

The relevance of history and philosophy of science for science studies

Chair, Edward Manier, University of Notre Dame

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

Sociology of technology

Chair, Ron Westrum, Eastern Michigan University

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

"Science/industry: monoclonal antibodies"

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

Deborah Mayo, VPI "Policy values and epistemic values"

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

Policy priorities and research goals

Chair: S. Zeldenrust, University of Amsterdam

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

S. Zeldenrust, University of Amsterdam, "Choice and implementation of goals in university and industrial research groups"

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

Wesley Shrum, Louisiana State University, commentator

Saturday, 3:30pm

Three mile island

Chair, John Wilkes, Worcester Polytechnic Institute

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

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

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

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

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

Mary Amanda Dew and Evelyn Bromet, University of Pittsburgh, "A matter of perspective: community residents' and nuclear plant workers' long term reaction to the TMI accident"

Bibliometric evaluation of research

Chair, Nicholas Mullins, Virginia Polytechnic Institute and State University

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

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

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

Susan Cozzens (NSF) discussant

Saturday, 8pm

INVITED ADDRESS

SUNDAY, OCTOBER 27

Sunday, 9am

The role of pictorial conventions in science

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

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

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

Terry Shin, "Graphics and visual imagery in cognition"

tentative: Bruno Latour, Ecoles des Mines, Paris; John Murdoch, Harvard, Marc de Moy

Frontiers in bibliometrics: today and tomorrow

Chair, Susan Cozzens, National Science Foundation

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

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

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

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

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

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

Sunday, 1:30pm

Science Policy Studies

Chair, Nicholas Mullins, Virginia Polytechnic Institute and State University

C. Kruytbosch, NSF, "Science indicators"

J. Holmfeld, Congressional Committee on Science and Technology, "Policy review"

D. Gerstein, National Academy of Sciences, "Disciplinary review"

Social construction of science and technology

Chair, Ruth Cowan, SUNY at Stony Brook

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

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

Stuart Leslie, Johns Hopkins University, "Scientists roles in industrial research laboratories"

Computers and creativity

Chair, John Reed, Oak Ridge National Laboratory

William Snizek, VPI, Impact of terminals and PCs on work routines of university scientists and engineers

Lois Peters, New York University, and Henry Eskowitz, SUNY
"Computer Science: Convergence and specialization in the growth of scientific disciplines"

Program committee, Tenth Annual Meeting, 1985

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

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

Ruth Cowan, History, SUNY-Stonybrook

Susan Cozzens, Policy and Research Analysis, National Science Foundation

Karin Knorr-Cetina, Sociology, University of Bielefeld, West Germany

Wesley Shrum, Sociology, Louisiana State University, Baton Rouge, La

John Wilkes, Social Psychology, Worcester Polytechnic Institute, Worcester Ma

TRAVEL ARRANGEMENTS TO THE 1985 MEETING

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

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

Amtrak train service via Albany-Rensselaer station is fairly good from New York (Grand Central, not Penn Station), Montreal, and Buffalo-Rochester. Albany and Troy are also served by Greyhound, Adirondack Trailways, and Vermont Transit.

Taxi service from the airport and the Turf Inn (terminus of the limousine line) is \$9.50 for one person; the charge for each additional person is \$1.50. From the train or bus stations in Albany the fee varies around \$12 to \$15 for 1-2 people, with a small additional charge for luggage.

Driving -- from the north, take I-87 south toward Albany to I-90, take I-90 east toward Boston to I-787, take I-787 north toward Troy to exit marked "Route 7 East, Bennington, Troy," cross Collar City Bridge over Hudson River, exit to right before end of bridge onto 6th Avenue leading to Downtown Troy; continue through three signals to Holiday Inn on left.

-- From the west, take New York Thruway (I-90) east to Exit 24, take I-90 east toward Boston to I-787, take I-787 north toward Troy, and proceed as in "from the north" above.

-- From the south, take New York Thruway (I-87) north to Exit 23, take I-787 north toward Troy, and proceed as in "from the north" above.

-- From the east, take Massachusetts Turnpike to New York Thruway, pick up I-90 west at exit B-3, take I-90 west to I-787, take I-787 north toward Troy, and proceed as in "from the north" above.

For further information, write:

4S Local Arrangements Committee
Science and Technology Studies
Rensselaer Polytechnic Institute
Troy, New York 12180-3590

Or phone any member of the committee: Linnda Caporael, Tom Carroll, Ed Hackett, Sal Restivo, Ned Woodhouse, or Rick Worthington at (518) 266-6574; or Maurice Richter at (518) 869-6720 (evenings and weekends).

REGISTRATION AND RESERVATIONS

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

- - - - - cut and send bottom portion with check - - - - -

Conference registration (Professional \$25, Student \$15).	\$ _____
Lunch @ \$6 each day	
Friday	_____
Saturday	_____
Sunday	_____
Breakfast session on Saturday @ \$5 (see program)	_____
Banquet @ \$20	_____
TOTAL	\$ _____

Please reserve a room at the Holiday Inn for these nights:

Oct. 24 _____ Oct. 25 _____ Oct. 26 _____ Oct. 27 _____

for

one person/one double bed _____ two people/one double bed _____

or two people/two double beds _____

____ Yes, I'd like to take the tour of historic sites.

Name _____

Send to:

Address _____

Thomas F. Gieryn
Ballantine Hall, Room 744
Indiana University
Bloomington, IN 47405

Telephone _____

SEE REVERSE OF THIS PAGE FOR
RESERVATION FORM AND REGISTRATION INFORMATION FOR
1985
4S MEETINGS

SOCIETY NEWS

NOMINATING COMMITTEE REPORT

CANDIDATE PROFILES

The following candidate profiles were solicited from candidates for President and for Council of 4S, and are listed alphabetically. The election will be held by mail ballot soon.

Candidates for President

David Edge

BIOGRAPHY: David Edge graduated in Physics at Cambridge in 1955, and took his Ph.D. there, in Radio Astronomy (under Martin Ryle). From 1959-66, he was a talks producer in the BBC Science Unit (Sound Radio), responsible for a wide range of broadcasts about science, including several third programme series. On 1 March 1966, he moved to Edinburgh University, where he founded the Science Studies Unit, of which he is Director. In 1979, he was promoted to Reader in Science Studies. He is a member of the University Court and of the Educational Policy Committee. He has been associated with both 4S and STSA since their inception, and has just completed a term on the 4S Council. He was also a member of the SISCON Council, and has been actively involved since 1972 in the work of the UK Council for National Academic Awards (CNAA), which validates degrees in the public sector in Britain. He is currently a member of the CNAA Reviews Co-ordinating Subcommittee and the CNAA Interfaculty Studies Board, and has recently been nominated as Chairman of the CNAA History of Ideas Panel. In 1970, together with Roy MacLeod, he founded the journal Social Studies of Science, of which he is now Editor. He is co-author (with Michael Mulkay) of Astronomy Transformed (1976), and co-editor (with Barry Barnes) of Science in Context (1982).

GOALS FOR 4S: I see the following as important aims for 4S over the next few years:

1. To maintain and develop the present wide spread of interest and research methods among the 4S membership. Variety may be our greatest strength.
2. To ensure that the highest disciplinary standards are maintained in all 4S research. Whoever "uses" our results (whether politicians or philosophers) must have good reason to believe that they can rely on them.
3. To encourage interdisciplinary dialogue - especially between History and Sociology, and between the so-called "critical" areas of scholarship (History/Philosophy/Sociology) and those areas of political, economic and administrative sciences that are more "policy-related." The latter dialogue may be especially crucial in the years immediately ahead.
4. In addition to the present representation in 4S of North America and Western Europe, to ensure that work currently under way in Australasia,

Latin America, Eastern Europe, India and China is properly represented in the 4S forum, and that 4S communities are encouraged elsewhere.

5. To consider the role of 4S in higher education - in the education of both scientists and non-scientists.

6. To attract funds to assist in this work (especially aim 4).

These aims may not, of course, always be compatible (for example, 2 may have to be relaxed, from time to time, in pursuit of 3 and 4); but they define criteria by which the health and success of 4S may be judged.

John Ziman

BIOGRAPHY: John Ziman is Visiting Professor at the Department of Social & Economic Studies at Imperial College, London. He was born in 1925, and was brought up in New Zealand, where he took his first university degree. He then studied at Oxford, where he completed a D.Phil. in theoretical physics in 1952. In 1954 he became a lecturer at the Cavendish Laboratory, Cambridge, and later a Fellow of King's College. In 1964 he was appointed to a chair at Bristol where he was the Director of the H.H. Wills Physics Laboratory from 1976 to 1981. His researches in the theory of the electrical and magnetic properties of solid and liquid metals have been published in a number of scientific papers and books, and earned his election to the Royal Society in 1967. In recent years, his interests and activities have been mainly concerned with various aspects of the social relations of science - philosophically through his books, Public Knowledge (1968) and Reliable Knowledge (1978); educationally through lectures on 'Science and Society' [published as The Force of Knowledge (1976)] and Teaching and Learning about Science and Society (1980); 'politically' as Chairman of the Council for Science and Society; internationally as a member of the Scientific Council of the International Centre for Theoretical Physics in Trieste and Chairman of the European Association for the Study of Science and Technology; journalistically by articles, book reviews and radio broadcasts. He was a member of the Council of 4S from 1982 to 1984, and chaired the Bernal Prize Committee.

GOALS FOR 4S: The purpose of 4S is to provide a forum for its members to meet and exchange their scholarly wares. I would interpret that purpose generously. The first priority is obviously unfussy competence in organizing meetings and publications, which is the only way to win and keep members, at home (wherever that is?) or abroad. But variety of scale and venue is an essential spice. Philadelphia and Ghent, for example, brought in the benefits of exogamous relationships with cognate organizations; Blacksburg made us appreciate the homely delights of doing our own thing together as a specialty cluster in the network. The most serious threat to the 3 S's is undue persistence in stale research programs and in sterile controversies between invincibly entrenched opponents. Science is such a multifarious activity (process, institution, etc.) that there is something to learn about it from every aspect - internal and/or external, evolutionary and/or constructivist, functional and/or phenomenological, folk and/or ethnomethodological, etc. Eclecticism may not be a virtue in the individual scholar, but diversity of choice is the prime requisite of a market, and I would push strongly for that in 4S.

Candidates for 4S

Council

Susan E. Cozzens

BIOGRAPHY: Susan E. Cozzens is a policy analyst at the National Science Foundation, where she runs a small program of funding for science policy studies. She has been active on the 4S program committee for four years, and last year served as 4S program coordinator for the meetings in Ghent. She is also a member of EASST, the History of Science Society, and AAAS. Her recent research is on the role of third parties in disputes over credit for multiple discoveries, and she is starting a study of the interaction between funding programs and intellectual developments in the neurosciences since 1955. Her work has been published recently in Social Studies of Science and Scientometrics, and she is editing a special issue of 3S, scheduled to appear in 1986.

GOALS FOR 4S: I am proud to be asked to run for 4S Council, because I am proud of science studies. Over the past decade, 4S has helped our field develop an exciting intellectual agenda, and its members have shed new light on science as an intellectual, social, political, and moral endeavor. To insure that the agenda continues to develop and expand, the field and the Society need to keep recruiting. Too few historians and philosophers find our meetings worthwhile. Anthropologists and political scientists are only beginning to come, and economists have yet to be enticed. To make recruitment possible, I would place highest priority for the next three years on raising the average quality of work presented at the annual meeting and establishing our journal as a first-class scholarly publication. In the longer term, the most important issue we need to consider is how 4S can encourage the training of new researchers with a taste for interdisciplinary problems in science studies.

Ronald N. Giere

BIOGRAPHY: Ronald N. Giere is Professor of History and Philosophy of Science at Indiana University. He earned his B.A. (Oberlin College, 1960) and M.S. (Cornell, 1963) in physics, and his Ph.D. (Cornell, 1968) in philosophy. During the academic year 1971-72 he was an Associate Research Scientist at the Courant Institute of Mathematical Sciences, New York University. In 1974 he was a Visiting Associate Professor of Philosophy at the University of Pittsburgh, and in 1982-83 he was a Senior Research Fellow at the Center for the Philosophy of Science, University of Pittsburgh. Professor Giere is the author of Understanding Scientific Reasoning, and co-editor of Foundations of Scientific Method: The Nineteenth Century, and of PSA 1980. He has published widely on induction, probability, causality, and statistics, and on relations between history and philosophy of science. He is currently working on a general study of the nature of science which combines methods from the history, philosophy, psychology, and sociology of science. He is a member of the American Philosophical Association, the Philosophy of Science Association,

the Society for Social Studies of Science, and a Fellow of the American Association for the Advancement of Science.

GOALS FOR 4S: Of all professional groups, the Society for Social Studies of Science now best reflects the diversity of backgrounds among people studying science and technology as human enterprises. I would encourage the continued broadening of membership in the society across disciplines and across national boundaries. I would also encourage cooperation with related organizations both in the U.S. and Europe. We all have much to learn from each other. And we all stand to gain from the vigorous pursuit of every area of science and technology studies.

C. Stewart Gillmor

BIOGRAPHY: C. Stewart Gillmor is Professor of History and Science, Wesleyan University, Middletown, CT 06457. BSEE Stanford; M.A., Ph.D. Princeton (1968). Formerly: National Bureau of Standards, Boulder, Colorado and Soviet Base, Mirnyy, Antarctica. At Wesleyan since 1967. Editor of Eos. Editor of series History of Geophysics. Author of Coulomb and the Evolution of Physics and Engineering in Eighteenth-Century France (1971). Former pianist and valve-trombone player: Gilded Garter Saloon, Central City, Colorado. Current research: History of ionospheric physics.

GOALS FOR 4S: I am a founding member of 4S. I think of myself primarily as an historian of physics and engineering sciences, yet I have some considerable interest in quantitative and qualitative measures useful to social studies of science and in possible applications of these. At the first 4S meeting at Cornell in 1976, about two-thirds of the attendees seemed to be historians of science and sociologists, with about one-third being political and policy scientists, and philosophers. It seems to me that historians, at least, have taken a less central part in the Society in recent years. I believe a multi-disciplinary 4S is good, and does not necessarily indicate weakness. In general, I would like to see increased opportunities for non-North Americans both to become members of 4S and to attend the annual meetings. As an example, I think the Ghent meeting was a good idea. I support the strengthening of the 4S Review. I would be pleased to serve on the 4S Council.

Belver C. Griffith

BIOGRAPHY: Following four years of research on speech, learning and concept formation, Griffith served as Associate Director, and then, Director of the Project on Scientific Information Exchange in Psychology, American Psychological Association from 1961 to 1969. Since 1969, Griffith has been a professor in the College of Information Studies, Drexel University. He has published approximately 100 reports, articles and chapters, dealing mainly with scientific communication, information use, scientific achievement, and the citation and aging of scientific and scholarly literatures.

Griffith's professional activities include membership on National Academy of

Science-National Research Council Committees and on the National Institutes of Health Biomedical Communication Study Section. He has served as a consultant to the National Science Foundation, The United States Office of Education, NATO, the British Library and Aslib. In 1975-1976, he was Honorary Research Fellow at University College London and Visiting Fellow at the University of Sussex. In 1980 he received the Drexel University Research Achievement Award and in 1982, the Outstanding Teaching Award of the American Society for Information Science. In 1984, he was Visiting Professor, Royal Institute of Technology, Stockholm and consultant on quantitative studies of science to the Technological Institute (STW), Utrecht, The Netherlands. He has served on several 4S Program Committees.

GOALS FOR 4S: The 4S should have three goals: (1) improving communication among scholars; (2) linking scholars and policy makers dealing with science, technology and social impacts; and (3) integrating diverse national and ideological views of science and technology. Accordingly the Governance of the Society must strike a necessarily difficult balance between a concern with quality and with insuring that no point of view is inappropriately advanced or neglected.

John Lankford

BIOGRAPHY: John Lankford, educated at Oberlin and the University of Wisconsin-Madison, is professor of the social history of modern science at the University of Missouri-Columbia. A 4S member since the Bloomington meeting, he has organized sessions and/or presented papers at Toronto, Philadelphia and Ghent. Lankford's research field is the history of astronomy and astrophysics and he is at work on a collective biography of the American astronomical community, 1859-1940. Lankford's papers have appeared in Soc. Stud. Sci., 4S Review, Isis, Journ. for the Hist. of Asty., and Sky & Tel. and he contributed the chapter on astronomical photography to volume 4A of the Gen. Hist. of Asty. (1984). At Missouri he teaches courses on women in science, science policy and post-industrial society.

GOALS FOR 4S: 4S is by far the most vital and dynamic group with which I have ever been associated. It is truly multi-disciplinary and international. My ideas involve fine tuning: a triennial membership directory, European meetings on a regular basis, nurturing the 4S Review and generally facilitating the dialogue across disciplines and oceans.

Rachel Laudon

BIOGRAPHY: Rachel Laudan, Associate Professor, Science Studies Center, Virginia Tech. Ph.D. 1974 University of London in history and philosophy of science. Editor of The Nature of Technological Knowledge (1984), "The method of multiple working hypothesis and the discovery of plate tectonic theory in geology in T. Nichles, ed., Scientific Discovery (1980)", "Redefinitions of a discipline" histories of geology and geological history," in L. Graham et al (eds.) Functions and Uses of Disciplinary History Sociology of the Sciences Yearbook, 1983 and other articles on the history of the earth sciences. Council, History of Science Society. Editorial Board, Isis. Current interests: history and philosophy of the earth sciences, structure of

technological change, historiography.

GOALS FOR 4S: The health of 4S and of the Review depends on the continued involvement of the best and most active scholars in science studies. In such a new interdisciplinary field many have commitments to other societies, meetings and journals. My aim as a member of Council would be to try to encourage the sustained participation of as many of these scholars as possible through whatever institutional means are possible such as appropriate timing and location of meetings, consideration of joint meetings, an active approach to invitations.

FINAL LIST: OFFICERS AND COMMITTEES--1985

President

Nicholas C. Mullins
 Department of Sociology
 Virginia Polytechnical Institute and State University
 McBryde Hall 648
 Blacksburg, VA 24061
 (703) 961-7205, 961-6878 (messages)
 (703) 951-7711 (home)

The President is ex officio a member of all committees

Secretary

J. Scott Long
 Department of Sociology
 Washington State University
 Pullman, WA 99164
 (509) 335-3249
 (509) 335-5151 (messages)

The Secretary is a member of Council.

Treasurer

Thomas F. Gieryn
 Department of Sociology
 BH744 Indiana University
 Bloomington, IN 47405
 (812) 335-2950
 (812) 335-5125 (messages)

The Treasurer is a member of Council

Council

Harry Collins (1985)
 School of Humanities and Social Sciences
 University of Bath
 Claverton Down
 Bath BA2 7AY ENGLAND

Ruth Cowan (1986)
 Department of History
 State University of New York Stony Brook
 Stony Brook, NY 11794
 (516) 246-6505

Marcel LaFollette (1986)
Editor, Science, Technology, and Human Values
 RM E51-008
 Massachusetts Institute of Technology
 Cambridge, MA 02139

until June, 1985
 OTA RM 308
 Washington, D.C.
 (202) 226-2025

home:
 343 South Ave
 Weston, MA 02193

(617) 617 253-4010

Bruno LaTour (1986)
Maitre de recherche
Centre de Sociologie
Ecole des Mines de Paris
62 Bld St. Michel
75.272 Paris FRANCE
(1) 329.21.05 poste 595

home:
15 Rue Damremont
75018 Paris FRANCE

Sal Restivo (1985)
STS Department
Rensselaer Polytechnic Institute
Troy, NY 12181
(518) 266-8504

Stephen Shapin (1986)
Science Studies Unit
University of Edinburgh
34 Buccleuch Place
Edinburgh EH8 9JT SCOTLAND
(031) 667-1011

Arnold Thackray (1985)
Department of History and Sociology of Science
University of Pennsylvania
215 South 34th Street/D6
Philadelphia, PA 19104
(215) 898-5621

Ron Westrum (1985)
Department of Sociology
Eastern Michigan University
Ypsilanti, MI 48197
(313) 487-1073

Charge: Council is in overall policy charge of the society. At least one member of Council is appointed to each committee, but not as Chair. The Chair of each committee is responsible for the activity of the committees. All committees report to Council for action and approval.

Ex Presidents

Robert K. Merton
415 Fayerweather Hall
Columbia University
New York, N.Y. 10017
(212) 280-3696

home:
Riverside Drive
New York, NY 10017
(212) 662-4040

Warren O. Hagstrom
Department of Sociology
University of Wisconsin
Madison, WI 53706
(608) 262-6585

Dorothy Nelkin
STS Program
632 Clark Hall
Ithaca, NY 14853
(607) 256-3810

Bernard Barber
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Barnard College, Columbia University
608 West 120 Street
New York, NY 10027
(212) 280-4359

Arnold Thackray
Department of History and Sociology of Science
University of Pennsylvania
215 South 34th Street/D6
Philadelphia, PA 19104
(215) 898-5621

Charge: The most immediate past President is a voting member of the Council under the Charter. The previous Presidents are all nonvoting members of Council,

Program committee (1985)

Ed Manier (Chair)	500 Riverside Drive
Department of Philosophy	New York, NY
University of Notre Dame	(212) 678-4974 (messages)
South Bend, IN 46556	

Christopher Caswill
Social Science Research Council
1 Temple Avenue
London EC4Y 0BD
England UK
01-353-5252

Ruth Cowan (Council)

Karin Knorr-Cetina
Department of Sociology
University of Bielefeld
P.O. Box 8640
Bielefeld Federal Republic of Germany
D-4800

Wesley Shrum
Department of Sociology
Louisiana State University
Baton Rouge LA
(504) 388-1645

John Wilkes	4 Longfellow Rd
Social Science	Worcester, MA 01602
Worcester Polytechnical Institute	

Worcester, MA 01609
(617) 793-5578

(617) 842-4416

Charge: To develop the program for the October, 1985 program

Timetable: Call for papers in current issue of 4S Review. Preliminary program due by 30 May 1985 for printing in 4S Review. Final program including abstracts of all papers to be available by 1 August 1985.

Publications Committee

James McCartney (Chair)
Department of Sociology
University of Missouri
Columbia, MO 65211
(314) 882-3542

Harry Collins (Council)

Jerry Gaston (ex officio, Editor 4S Review)
Department of Sociology
Texas A M University
College Station TX 77843
(713) 845-5133

Tom Gieryn (Treasurer)

Marcel C. LaFollette (Council)

Stephen Shapin (Council)

Sal Restivo (Council)

Harriet Zuckerman
Department of Sociology
Columbia University
New York, N.Y. 10017
(212) 280-2149
(212) 865-8440

Charge: To set general policy for publications of the Society. To review and oversee the publication of the 4S Review. To supervise other possible publications such as a membership directory. To select a neww editor and managing editor for the 4S Review. To consider changing the name of the review for librarians and bibliometric purposes.

Bernal Prize

Arnold Thackray, Chair

Thomas Kuhn (1983 Prize winner)
STS Program
Massachusetts Institute of Technology
Cambridge, MA 02139

Robert Merton (1982 Prize winner, ex President)

Joseph Needham (1984 winner)
The Needham Research Institute
East Asian History of Science Library
16 Brooklands Avenue
Cambridge CB2 2BB ENGLAND

Kenneth Prewitt
Social Science Research Council
605 Third Ave.
N.Y., N.Y. 10158
(202) 662-4040

Charge: To nominate the 1985 winner of the John Desmond Bernal Award of \$750 for distinguished contributions to the social studies of science. To provide Council with a document suggesting procedures for selecting future Bernal Prize committees.

Timetable: Preliminary list of candidates to be circulated by 30 April 1985; awaree to be agreed on by 30 June 1985.

Nominations Committee

Henry Small (chair)
Institute for Scientific Information
3501 Market St.
Philadelphia. PA 19106
(215) 386-0100

Bruno Latour (Council)

Roberta Miller
Director, Division of Social and Economic Sciences
National Science Foundation
1800 G. Street, N.W.
Washington, D.C. 20550
(202) 357-7966

Dorothy Nelkin (ex President)

Tom Nickles
Philosophy Department
University of Nevada
Reno, NV 89557
(702)

Stephen Straker
Department of History
University of British Columbia
Vancouver, B.C
V6T 1W5 CANADA
(604) 228-5173

Charge: To nominate 6 individuals for 3 Council vacancies with appropriate

disciplinary and geographic spread, and candidates for President.

Timetable: Preliminary list of candidates to be circulated by 30 April 1985.
full list of candidates with c.v. and statement of goals to be available by 30
June 1985, for publication with the ballot in the 4S Review

Liason Committee

Daryl Chubin (Chair)
Science, Technology Studies Program
Georgia Institute of Technology
Atlanta, GA
(404) 894-3195

Scott Long (Secretary)

Albert Teich
Office of Public Sector Programs
American Association for the Advancement of Science
1776 Massachusetts Ave., N.W.
Washington, D.C. 20036
(202) 467-4310

Sharon Trawek
Massachusetts Institute of Technology
Cambridge, MA 02139

Ned Woodhouse
Science, Technology and Society Program
Rensselaer Polytechnic Institute
Troy, NY 12181
(518) 266-8506

Charge: To coordinate representation from the 4S to the AAAS sections and other
associations. To propose a means of designating 4S representatives to other
societies. To set up a procedure for developing 4S sponsored sessions at the
meetings of these associations.

Timetable: A document suggesting representation strategy to be prepared for
Council by 1 September 1985. A procedure for 4S sessions to be set up by 1
May 1984.

Local Arrangements - Troy

all: Science Technology Studies
Rensselaer Polytechnic Institute
Troy, NY 12181

Sal Restivo (Council) (cochair)
(518) 266-8504 on leave after July, 1985

Rick Worthington (cochair)
(518) 266-8515
(518) 489-8222 (home)

Linda Caporael
(518) 266-8519

Tom Carroll
(518) 266-8508

Ed Hackett
(518) 266-8399

Ned Woodhouse
(518) 266-8506

Maurice N. Richter Jr.
Department of Sociology
SUNY-Albany
(518) 457-8468

Charge: To coordinate local arrangements for Troy, New York meeting

Timetable: To present local plans for publication in 4S Review by 1 May 1985.

CALL FOR AAAS SYMPOSIA PROPOSALS

4S is again planning to co-sponsor sessions at the 1986 Annual Meeting of the American Association for the Advancement of Science. The meeting is scheduled for 25-30 May in Philadelphia.

The AAAS Liason Committee, chaired by Daryl Chubin, invites the submission of ideas for half-day symposia. Proposals should include the official AAAS form (see page 1188 of the 7 June 1985 issue) and a "Synopsis of Objectives." Sponsorship should be listed as "Submitted to 4S for Consideration." The due date is 1 August 1985 in the AAAS Meetings Office. Simultaneously, the AAAS Liason Committee will review the proposals and endorse a subset to various AAAS section secretaries. Selection will reflect the diversity of 4S interests as well as the relative strength of the proposals. The Committee may also suggest the names of additional participants.

Because of the pressing deadline, it is important to submit all forms both to AAAS and to the 4S Committee:

AAAS:

Meetings Office
AAAS
1101 Vermont Ave., N.W.
Washington, D. C. 20005

4S Liason Committee:

Daryl Chubin
TASP, School of Social Sciences
Georgia Tech
Atlanta, GA 30332

ANNOUNCEMENTS: FEDERAL AGENCY

NATIONAL SCIENCE FOUNDATION

Division of Science Resources Studies
announces its 1986
Program for the Analysis of Science Resources

Research grants will be awarded for studies directed toward quantitative indicators for science and technology policy within such areas as:

- New institutional arrangements for promoting R&D
- Technological innovation and diffusion
- Bibliometric and other measures of scientific or technological advance
- International flows of science and technology, including personnel
- Public attitudes toward science and technology
- The relationships of science and technology with society

Proposals should be submitted by January 15, 1986. Awards will usually cover one to two person-years of effort. Specific information is given in the Announcement of the Program for the Analysis of Science Resources, which can be obtained from:

Division of Science Resources Studies
National Science Foundation
1800 G Street, N.W.
Washington, DC 20550

ANNOUNCEMENTS: THE SOCIETY FOR THE HUMANITIES

The Society for the Humanities--Cornell University

The Humanities and Science: A Reassessment

The Society for the Humanities at Cornell University offers six post-doctoral fellowships for 1986-87 to scholars in humanistic disciplines working on such topics as:

- (1) the relations between humanistic and scientific inquiry.
- (2) the study of science as a culturally-located activity, including the application of historical, philosophical, and literary interpretive methods to scientific writings.
- (3) the effect of scientific and technological advances on central humanistic concepts such as mind, freedom, responsibility, imagination, genius.

For information write, Jonathan Culler, Director, Society for the Humanities, 27 East Avenue, Ithaca, N.Y. 14853-1101. Deadline for applications November 1st, 1985

NEWS FROM DOWN UNDER

David P. Miller
University of New South Wales

In the Australian '4S community' 1985 is a year of new initiatives and special events, while progress continues on other fronts (see Conference Report below). Among new initiatives might be mentioned the new annual review Metascience under the editorship of Associate Professor W.R. Albury of UNSW (see Announcement Vol. 3 No. 1) and the launching of an Australian Scientific Archives Project, based at the Department of HPS, University of Melbourne under the direction of Professor Rod Home. The Project will serve as a half-way house - processing documents and preparing listings before finding them an appropriate long-term home. Inquiries should be directed to the archivist, Mr Gavan McCarthy.

In May, the University of Sydney celebrated its 'Science Centenary' with a panoply of lectures, exhibitions and displays. This included a Science and Society Conference jointly organized by the Australasian Association for the History, Philosophy and Social Studies of Science (AAHPSSS) and the University of Sydney's new Centre for Human Aspects of Science and Technology. Dr David Edge (Science Studies Unit, University of Edinburgh) delivered the inaugural lecture of the Centre, asking "Is Science Inhuman?", and also offered his reflections on "Twenty Years of Science Studies."

The annual AAHPSSS Conference will be held in August at the University of New South Wales and will include sections on 'Historical and Philosophical Aspects of the Cognitive Sciences', 'Industrial Research in Australia' and 'The History of Anthropology and the Study of Culture.' The latter is held in memory of Dr Ian Langham whose death in 1984 was a sad loss to us all.

**THE SECOND BICENTENNIAL HISTORY OF AUSTRALIAN SCIENCE
WORKSHOP**

**The Australian Academy of Science, Canberra,
20-22 February, 1985**

by

George H. Bindon

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The year 1988 will mark the passage of two hundred years since the rebelliousness of the American colonies necessitated the establishment of an alternative destination for convicts transported from the British Isles. As is common on such anniversaries, the occasion has been seized as an opportunity to establish a veritable industry of historical publishing. For the nascent band of enthusiastic researchers with an interest in the history of science in Australia, the initiative taken by the Australian Academy of Science to sponsor a substantial volume devoted to that topic has been a boon. And in particular, the enlightened decision to avoid the temptation to produce a collection of reminiscences, but rather to "punt" on a more analytical approach, has had the added benefit of translating what might have been a one-off exercise in creating a text, into the sponsorship of an exciting, on-going scholarly enterprise. Professor Rod Home of the Department of History and Philosophy of Science, University of Melbourne, has been the "product champion" as the editor of the up-coming volume, as well as of the History of Australian Science Newsletter and the "journal of record": Historical Records of Australian Science.

This Workshop was flawlessly organized by a committee of two -- David Miller, History and Philosophy of Science, and Ian Inkster, Economic History, both from the University of New South Wales. Just the right mixture of discipline and freedom was designed into the proceedings to maximise the benefit to, and enjoyment of, the participants. As a "Sydney insider", I always find the bucolic gentility of Canberra a relief from the cacophony of the "Big Smoke", and the ideal environment for collegial discourse and quiet thought.

The bicentennial volume will include five commissioned chapters, with the remaining page-space being filled competitively on the basis of refereed assessment of the merit of submissions. Since the first Workshop, the commissioned authors have been chosen, and this meeting offered them the opportunity to report on their progress. L.R. Hiatt of Sydney University discussed the chapter he and Rhys Jones from the Australian National University, are

This led to some lively exchanges about the value implications and intellectual difficulties of dividing "science" from "other" forms of knowledge about the natural world. Certainly the authors will have some food for thought after this encounter. Ian Inkster presented a framework for his chapter titled "The Support Structure of the Australian Scientific Community ca. 1851-1914", which included a dynamic model of the peripheral scientific community and its political/economic context that suggests the possibility of a successor to Basalla's benchmark design. The paper was given in two parts, the first being the "theoretical scheme" by Inkster followed by an excellent case-study by Jan Todd. Professor Roy MacLeod, Sydney University, arrived jet-lagged from an overseas trip, but landed on his feet with a challenging talk on "Australian Science and the 'Imperial Idea', 1880 - 1918", that offered a wealth of ambitious suggestions for a more coherent framework that might inform our whole historical enterprise. Rod Home described his work on "Australian Science at War, 1939 - 1945", with the almost unknown, and quite unappreciated story of the employment of physicists in a major Australian company -- AWA (Amalgamated Wireless Australia). This case-study will form a part of his chapter. Professor Ron Johnston, H.P.S., University of Wollongong, offered what was essentially a taxonomy of ways of looking at "Post-war Science in Australia", and refined and added to the list in the subsequent discussion.

As a venue designed to present "work in progress", the gathering was undoubtedly as useful to the "core" authors as it was to the rest of the aspiring contributors to the Bicentennial volume. The balance of the approximately forty-five participants came from every state save Tasmania. A dozen papers on a variety of topics were presented in addition to the five core chapters. Since the First Workshop in 1982, there is evidence of something of a "shakedown" having taken place, with those attending, while not all "professional" scholars in the history and social studies of science, nonetheless definitely skewed toward that end of the spectrum. The volume toward which all this effort is directed will, if the hopes of the participants are realized, mark not an end but a beginning. Consideration of the history of Australian science is important both in terms of its contribution to the general discourse as a "special case", but also as a vehicle for a consideration of the centre/periphery problem.

SOCIETY FOR PHILOSOPHY AND TECHNOLOGY
Third Biennial Meeting
Technical University of Twente
Enschede, The Netherlands
August 14,15,16, 1985

Wednesday, August 14, 1985

AM 9:00 - 12:00

Session 1. Autonomy of Technology:

Chair: Donald Hockney

Joseph C. Pitt; The Autonomy of Technology

Daniel Cerezuelle; Autonomy of Technology

George M. Guilmet; The Effects of Traditional Eskimo Patterns of Cognition
on the Acceptance or Rejection of Technological Innovation

Session 2. Religion and Technology:

Chair: Carl Mitcham

Albert Borgmann; Liberty, Festivity and Poverty: Harvey Cox on Christianity
and Technology

Anthony J. Wesson and Ron Brown; Religion, Technology and Human Anatomy

Mark Swetlitz; Technological Creativity: An Imitation of God

Session 3. Appropriate Technology:

Chair: Paul T. Durbin

Stanley R. Carpenter; Conversations on Appropriate Technology: How Can
Philosophers Contribute?

Thomas W. Simon; Democratic Technology: A Critique of Appropriate Technology

—
Lunch

PM 2:00 - 5:00

Session 4. Technology and Work:

Chair: Deborah C. Poff

Edmund F. Byrne; The Labour-Saving Device: Nemesis of the Working Class

Kristin Shrader-Frechette; Occupational Risk and the Double Standard: Technoligical

Risk and the Method of Expressed Preferences
Friedrich Rapp; Changing Attitudes Toward Work

Session 5. Methodological Issues:

Chair: Joseph Agassi

Sanford W. Krolick; The Mother of Invention? Reflections on Method in Science,
Technology and Engineering

Ronald Laymon; Applying Idealized Scientific Theories to Engineering

José Félix Tobar Arbulu; On the Ontological Status of Artifacts

Session 6. Religion and Technology:

Chair: Carl Mitcham

Edward Daub; Lynn White's Analysis of the Religion-Technology Relationship

Charles Mabee; The Theology of Technology

Egbert Schuurman; The Modern Babylon Culture

PM 7:00 - 9:00

Session 7. Workshop in Technology and Philosophy:

Chair: C.L. Sheng

Florence M. Hetzler; Persons and Technology

William Maker; Science, Values and Power

John E. Jalbert; The Autonomy of Technology: A Phenomenological Critique

SPT Board Meeting

Thursday, August 15, 1985

AM 9:00 - 12:00

Session 8. Decision-Making:

Chair: Joseph Margolis

Douglas N. Walton; Practical Reasoning

Joseph Agassi; Technology and Society

Ted W. Lockhart; Engineers as Social Activists: A Defense

Session 9. Lebenstechnik:

Chair: Günter Ropohl

Wolfgang Schirmacher; Homo Generator: The Challenge of Gene Technology

Paul Levinson; Guns, Knives and Pillows: On the Intrinsic Value and Evolutionary
Morality of Technology

Daniel O. Dahlstrom; Eating as a Technology of Life

Session 10. Reproductive Technology:

Chair: Stanley R. Carpenter

Anne Ronchin; Power and Procreation: Reproductive Technology and the Future
Of Motherhood

Deborah C. Poff; Big Brother May Not Be Watching But Brave New World Is Here

Valerie Mike; Medical Technology and the Clinical Trial: Statistical, Ethical
and Psychological Issues

Lunch

PM 2:00 - 5:00

Session 11. Social Science and Technology:

Chair: Alex C. Michalos

John Crane; The Evaluation of Social Policies to Control Hazardous Technologies:
Developing an Adequate Research Design

Louis H. Bluhm; Trust, Terrorism and Technology

Joseph Margolis; History, Nature and Technology

Session 12. Moral Issues in Technology:

Chair: Charles Mabee

Corbin Fowler; Nuclearism and Myths about Fallout

C.L. Sheng; Applications of Systems Theory to Moral Philosophy

John R. Carnes; Stopping the World: An Essay Concerning the Algorithmic Society
with Special Attention to the Professions and to Professional
Education

Session 13. Responsibility and Technology:

Chair: Friedrich Rapp

Werner Strombach; Information-Networking and Responsibilities

Günter Ropohl; New Ways of Performing Sociotechnical Responsibility

Sybille Krämer-Friedrich; What Does "the social character of technics" Mean?

6:00 Reception

7:00 - 8:30 Dinner:

8:30 Biennial Business Meeting

Friday, August 16, 1985

AM 9:00 - 12:00

Session 14. Ethics for Engineers:

Chair: John Crane

Paul T. Durbin; Beyond Engineering Ethics: Toward a Comprehensive Philosophy
Of Engineering

C. Thomas Rogers; The Ethical End-Use Problem in Engineering for War or Peace

Heinz Luegenbiehl; What is Engineering Ethics: A Prolegomena

Session 15. Computers and Technology:

Chair: Ted W. Lockhart

David H. Helman; Algorithms for Interpretation and Algorithms for Moral Deliberation

Barton D. Thurber; Computers, Language, Narrative

S. Muthuchidambaram; Information Technology, Citizen's Rights and Personnel
Administration: An Exploratory Study on Privacy Policy
Developments in Canada and the USA

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