

Science & Technology Studies

(formerly 4S Review)

ISSN 0886-3040

Volume 4 Number 1 Spring 1986

Journal of the Society for Social Studies of Science

Society for Social Studies of Science

President: David O. Edge

Secretary: J. Scott Long

Treasurer: Thomas F. Gieryn

Council: Ruth Schwartz Cowan

Susan E. Cozzens

Marc De Mey

Ronald Giere

David Hull

Marcel C. LaFollette

Rachel Laudan

Bruno Latour

Steve Shapin

Publications Committee: Marcel C. La Follette, *chairwoman*

Randall Albury

Marc De Mey

James L. McCartney

Sal Restivo

David O. Edge, *ex officio*

Thomas F. Gieryn, *ex officio*

Science & Technology Studies

Co-editors:

Daryl E. Chubin

School of Social Sciences

Georgia Institute of Technology

Atlanta, Georgia 30332 USA

Susan E. Cozzens

Room 1229

National Science Foundation

Washington, D.C. 20550 USA

Publisher: Jerry Gaston

Department of Sociology

Texas A&M

College Station, TX 77843 USA

Computer Consultant: Juan Jewell

Science & Technology Studies (ISSN 0886-3040), journal of the Society for Social Studies of Science, is published four times a year. Journal subscription includes membership in the Society. Rates are \$40 institutional, \$30 individual, and \$15 student.

For membership information, contact J. Scott Long, 4S Secretary, Department of Sociology, Washington State University, Pullman, Washington 99164 USA. Dues in European currencies may be paid through the European Association for the Study of Science and Technology. For specific rates, contact Arie Rip, Wetenschapsdynamica, University of Amsterdam, Nieuwe Achtergracht 166, 1018 WV Amsterdam, The Netherlands.

© 1986 by the Society for Social Studies of Science

SCIENCE & TECHNOLOGY STUDIES

(formerly 4S Review)

Journal of the Society for Social Studies of Science

Volume 4 Number 1 Spring 1986

TABLE OF CONTENTS

SYMPOSIUM

External and Internal Factors in the Development of Science Dudley Shapere	1
Incorrectness and Specific Doubts: Comment on Shapere P. Thomas Carroll	9
The Sociology of Science in Its Place: Comment on Shapere Stephen Turner	14
Replies to Carroll and Turner Dudley Shapere	18

DISCUSSION PAPER

Research Policies and Strategies of Five Industrial Nations, and Implications for the United States Leonard L. Lederman, Rolf Lehming, and Jennifer S. Bond	23
--	----

NEWS AND VIEWS

Editorial Statement-in-Progress	31
Meetings Calendar	31
Science Policy Snapshot Evan Berman	32
The President's Page David Edge	33
News Clips	34

ABOUT THE AUTHORS	Inside back cover
-------------------------	-------------------

INFORMATION FOR CONTRIBUTORS	Inside back cover
------------------------------------	-------------------

In this issue . . .

Welcome. We hope you will enjoy the first issue of *Science & Technology Studies*, journal of the Society for Social Studies of Science. The essence of the Society is interaction, and this issue is designed to stimulate it. We are pleased that Dudley Shapere, P. Thomas Carroll, and Stephen Turner accepted our invitation to feature their exchange of views, first aired at the Annual Meeting in Troy, in this inaugural issue. We also welcome your reaction to a discussion paper (Lederman, Lehming, and Bond), a review by our President of 4S problems and issues, and a snapshot of a science policy research unit. For the next issue, we hope to hear from *you*, with letters, announcements, commentary, and articles.

The Editors

External and Internal Factors in the Development of Science

Dudley Shapere
Wake Forest University

I

According to a familiar traditional view, science is a distinctive enterprise, demarcated sharply from all other human pursuits. The exact nature of that line of demarcation was conceived differently by different thinkers or schools. Depending on whom one read, the demarcation might be held to lie in its experimental method, or in its building upon irrefutable observational facts, or in the distinctive rules (deductive or inductive) by which it adopts a theoretical superstructure on the basis of those facts, or in its goals of attaining knowledge of nature, or in a combination of several or all of these factors. But despite all such important differences of detail, according to all versions of this traditional sort of view, it was the unalterability, the inviolability, the eternity, attributed to the distinguishing marks of science, that most strikes us today. For according to those approaches, the distinguishing characteristics of science must characterize it forever, unaffected by any results of scientific inquiry. If it was its method that demarcated science from other sorts of human activities, that method was conceived as having since its introduction been applied to gain further and further truth, without itself ever having been altered or alterable by any of the discoveries to which it led; and it would always continue to be unalterable in its future applications. The situation was supposed to be similar in the case of rules of scientific inference: they were "logical" (either deductive or inductive), which implied both that they were unalterable and that their status was independent of the world of facts to which they were applied. As to facts themselves, much of that tradition held them to be irrefutable also.

That tradition, as is well known, is now a shambles, rejected almost universally. Already by the 1950s it had become clear to many that all attempts to characterize such eternal and inviolable

demarcators had failed. The notion of "observational facts" as brute undeniable givens, wholly independent of our fragile and insecure interpretations of them, was all but surrendered, and the idea that there was anything "given" in experience was thrown in jeopardy. All attempts to give a precise characterization of scientific method, whether experimental or logical, whether in terms of deductive or inductive logic, were, if not wholly abandoned by then, at least viewed with extreme pessimism by a growing number of those concerned with the study of science. New philosophical analyses emphasized that theories are radically underdetermined by observation; that what are called observations, far from being interpretation-free, are heavily theory-laden; that if indeed, as appeared to be the case, it was impossible to specify any unladen "given" in experience, then perhaps theory-ladenness even predetermined the outcome of experimental tests; that for any given body of what are taken to be data, alternative explanations, perhaps even an infinity of them, are always possible, and that perhaps *any* alternative can, with sufficient ingenuity, be defended "come what may," in the face of *any* body of what are alleged to be observational data.

The rebellion against tradition was not confined to philosophers: an even more serious threat came from the newly-professionalized discipline of the history of science. A torrent of historical studies indicated more and more convincingly that changes over the development of science have gone far deeper than mere change of theory. The changes seemed to extend also to what was counted as evidence, as observational, as factual; to criteria of adequacy of explanations of that evidence, or those observations or facts, and even to what counted as an explanation; and to method, which seemed not to be a single thing after all, but a multiplicity varying from period to period and subject to subject. The studies exposed the presence of broader and deeper "interpretative frameworks" which guided the construction of evidence, observation, fact, explanation, and theory, and which even determined the methodological rules, the criteria of scientific adequacy and the meanings of scientific terms, and even

Author Address: Department of Philosophy, Wake Forest University, Drawer 7229 Reynolda Station, Winston-Salem, North Carolina 27109 USA

This paper was first presented at the Annual Meeting of the Society for Social Studies of Science, Troy, New York, October 26, 1985.

the goals of science itself. And it appeared that such interpretative frameworks differed in fundamental, perhaps incommensurable, ways from tradition to tradition or from group to group.

The implications seemed profound. If facts underdetermine theories—if indeed the sum total of scientific constraints, whether factual (observational), methodological, logical, criteriological; or whatever, underdetermine scientific belief—then some other, non-scientific, considerations must enter in to fill the gap between scientific constraints and scientific belief. Since science is a group endeavor, the most promising candidate would be social considerations. An even more extreme implication threatened: for if fact or observation, method, logic, and criteria all depend on presuppositions which vary from one tradition or group to another, then there are *no* independent scientific constraints standing above ultimate interpretative frameworks, and there would remain *no* intrinsically scientific constraints on belief; *everything* about science would be a matter of “external” considerations, perhaps socially conditioned. If for example observational facts are interpretation-laden through and through, then how could they be expected to enable us to decide for or against the interpretation to which they owe their very status, indeed their very meaning? Thus one degree or another of social relativism loomed: either social (or other non-scientific) considerations made up the difference between scientific constraints and scientific belief, or else there were no scientific constraints which were not themselves socially conditioned or determined. There was nothing intrinsically “internal” to the scientific enterprise which distinguished its procedures and conclusions from “external” ones.

There can by now be no reasonable doubt of the pervasive role of presupposition, of interpretation, in science and scientific change. There are no brute facts which confront us and force our theory choices in certain obligatory directions; there is no “given” which does not involve interpretation. Nor is there a single scientific method which is applied unambiguously across the board in all science, past, present or future. The extraction or testing of theories and hypotheses is far more complicated than can be captured by rules of any formal logic. What counts as an observation; conceptions of the objects or processes under study (and even whether what we study are appropriately characterized as “objects” and “processes”); the problems arising with respect to those objects and processes; the characteristics expected of answers to those problems, and the criteria of adequacy by which one answer is selected from the body of possible ones; the methods by which those answers are to be attained; the goals

of inquiry—all these are shaped in the light of background beliefs, background beliefs which are different at different periods of history and in different domains of inquiry. Science is built upon a background of presupposed beliefs, beliefs which form the basis of our interpretations of nature and our methods of studying it, and which are in principle always subject to alteration, abandonment, or replacement.

Nevertheless, it remains another question whether some of the conclusions which have been drawn really do follow from this pervasiveness of presupposition and interpretation. More specifically, we need to examine more closely whether either of the following theses follows from that pervasive role:

- (1) that there are no “internal” factors guiding scientific development independently of non-scientific factors (for reasons obvious to sociologists, I will call this “the strong thesis”); or
- (2) that, while there are such internal factors, they are insufficient by themselves to guide science, and must be supplemented by “external” factors. I will call this “the weak thesis.”¹

In what follows, I will argue that neither of these theses follows from the pervasive employment in science of background beliefs and the deep changes that have affected the beliefs so employed. In other words, I shall try to show, firstly, that there is an important distinction between internal and external factors in science—though that distinction has to be radically recast in the light of the way science relies on antecedent beliefs and undergoes pervasive changes in those beliefs; and secondly, that the internal factors are generally sufficient to guide science in its inquiries—even though the claims it builds on those background beliefs are always in principle subject to possible doubt—and that that sufficiency has, as a matter of contingent fact rather than a necessity of logic, tended to increase over the history of science.

II

According to the view of science which I shall present, the distinction between external and internal considerations guiding the development of science, far from being an *a priori* and essential characteristic present in science from its inception, has itself been a product of that development. In early periods of the history of efforts to understand nature, there was precious little guidance as to what, precisely, required investigation, what was relevant to the investigation, how to go about the investigation, and what a conclusion of the investigation would be

like. The Milesian philosophers of the sixth century B.C., who perhaps more than anyone deserve the reputation of founders of the knowledge-seeking enterprise, do not seem to have focussed on particular problems concerning particular sorts of substances or processes, but took all existence, all change, as their domain of inquiry, and everything—or rather nothing in particular—was relevant to their efforts. What methods they had were of the most simplistic and intuitive logical and analogical sort; and as to what they demanded in an explanation, that was little more than that an explanation should show nature not to be capricious.

Clarification with regard to these four aspects of inquiry—what to study, what was relevant to the study, the appropriate methods for that study, and the character of an explanatory conclusion to the study—required *learning how to learn* about nature. Later I will mention some other aspects of the scientific enterprise with respect to which we had to learn how to learn, but for the present let us focus on these and ask this question: How did we, in seeking knowledge about nature, come to learn how to go about determining what needs to be studied, what is relevant to that study, the methods of going about that study, and the character of explanations which would be the goals of those studies? How did we get from Miletus to today's science, not just in respect of the profound changes of beliefs that have come about, but in respect of how we go about studying nature to obtain those beliefs, and how did that profound transition produce a distinction between considerations "internal" and "external" to science? Of course in this short paper I can only give a sketch, an overview, of the process; but perhaps that will be enough to provide a basis for some conclusions about the debate concerning the nature and relative status of internal and external considerations in the development of science.²

We can begin by considering one important development which gradually assumed centrality in the scientific enterprise during the sixteenth through eighteenth centuries: the approach of examining specific subject-matters, such as moving bodies, salts, gases, in isolation from others—what we may call the *piecemeal approach to inquiry about nature*.³ Although it had been present far earlier, it was then only one of many, not coming to prominence in the search for knowledge until the early modern period. In particular, it replaced an older holistic approach, stemming from Milesian philosophy, of trying to explain everything at once—for example, of trying to explain the nature of change or of substance *in general*. The specific subject-matters for study in this new approach may be referred to as *domains of investigation*.

Early domains of investigation were necessarily those of common, everyday classifications, based on considerations such as sensory similarities, use, place of discovery of a substance, and the like. Thus certain metals were given the same name and considered one sort of substance on the basis of their appearance, and similarly for certain transparent crystalline substances. Views of what the problems concerning those classifications were, of the methods by which those problems were to be approached, and of what sorts of answers to try to provide, what it was to give an explanation of the items of a domain, were likewise based on prevailing views, of which there were many contradicting one another, with few having any clear advantage over their competitors; and all were ill-formulated and vague. What was and was not relevant to the classifications and the attempt to deal with them was obscure.

But with the beginnings of intensive study of those presumed types of substances, distinctions were found between members of the types which led to distinct classifications. Domains were thus reconstituted and restructured, old bases of classification coming to be seen as superficial, other bases formerly seen as superficial, or new ones not known before, coming to be seen as of fundamental importance. What had previously been classified indiscriminately as "salts" became separate domains for investigation; on the other hand, early differences found between electricity and magnetism, the former, for example, acting on light bodies of all sorts, the latter only on heavy iron ones, came to be seen as superficial, and the two types of phenomena ultimately became unified in Maxwell's theory. And as the domains of scientific investigation were thus shifted and altered, so too were the items making them up reconceived, reinterpreted, and often redescribed and renamed. The most far-reaching of such reconceptions occurred in the so-called Chemical Revolution of the late eighteenth century, in conjunction with which Lavoisier and his associates proposed the renaming of all chemical substances according to their elemental constituents. And of course new discoveries added new items to domains, and even new domains.

But how did these restructurings of scientific fields come about? I suggest that the very adoption of the piecemeal approach to inquiry—the laying-out of boundaries of specific areas of investigation—automatically produced a standard against which theories could be assessed. Whatever else might be required of an explanation of a particular body of presumed information (domain), that explanation or theory could be successful only to the extent that it took account of the characteristics of the items of that domain. This is of course rather vague. What

were to be counted as "characteristics" of the domain items: How were we to decide what it was about, say, salts or magnetism, that had to be "accounted for"? To what extent did we have to "account for" them: with complete exactness, or was nature just not so precise? And in what would such an "account" consist? Should it tell what the perfect state of the substance was, as in some alchemical views (I will call this the *perfectionist approach* to understanding material substances)? Or should it take what might be called a *compositionalist approach*, deriving the properties of the domain items from their constituent parts, the arrangement of those parts, and the forces holding those parts together—a view propounded in chemistry in (among others) Stahl's theory and coming to full expression in the theories of Lavoisier and his associates?

In all these respects, what are naturally called "hypotheses" played a role; and there was, in earlier phases of science, little to go on in selecting these hypotheses. Or more exactly, the motivating considerations in selecting explanatory approaches might come from just about anywhere. Antagonism to Aristotelian forms, natures, and final causes, rather than the dictates of nature, entered into adoption of the mechanistic and atomistic approaches of the middle and late seventeenth century; Newton developed his theories of motion (and thus of space and time) at least partly in the light of theological considerations, objecting to Cartesian physics on such grounds just as his own views were deemed atheistic by Leibniz and his followers. And in general, the large gap between scientific ambition and scientific conclusion had to be filled, under such circumstances, by considerations which we today would consider non-scientific, external, though at the time there was little or no ground to so distinguish them. Indeed, even the ambitions of science at such stages were dictated, at least partly and perhaps largely, by considerations which would today be called external. For the distinction between the external and the internal to science was at best only rudimentary and in many cases did not exist at all.

Under such circumstances, the only available option, however little it was apparent to the participants, was to wait and see which if any of the various hypotheses as to domain classification, presumed distinctions between superficial and really important domain-item characteristics, and the nature of "accounts" would hold up—which if any, that is, would *allow* successful account to be given of the domain in question. As it turned out, certain approaches—for example, the mechanistic approach to the study of the motions of planets, falling bodies, and projectiles by the late eighteenth century, and the compositionalist approach to the study

of material substances by the 1860s or so—did show themselves to account to a considerable degree for their respective domains. Let us examine some of the factors involved in this.

One particular achievement of the mechanistic approach was to show that accounts of domains could achieve *precision*, and that such precision was, at least often, expressible in mathematical terms. Things might not have been that way. Indeed, for Plato, although mathematics (geometry) was a necessary ingredient for understanding the world of experience, of change, it was not sufficient, for that world was intrinsically indeterminate; and Aristotle held that mathematics, the category of quantity, could not provide understanding of the essence of things. The idea that all details of experience could be explained precisely originated with Kepler. With him, it was based on a rationalistic theology which implied that there is (*i.e.*, that God must have had) a sufficient reason for everything in nature, a principle with which Kepler associated geometrical exactitude. Within the limits of available observational accuracy, it was shown to be an achievable ideal by the mechanical philosophy.

The Newtonian version of the mechanistic approach was able to do what it promised to do better than its rivals (Scholastic Aristotelianism, Cartesianism) were able to do what they promised to do; and because of the role played by mathematical precision, it was able to do what it did better than they did what they did. Some of this power was evident from the work of Newton; but its full force came out only with Laplace, who brought together and extended the work of a century and molded mechanics into an impressive tool for the analysis of problems of celestial and terrestrial motions and forces. But from the perspective of our present problems, Laplace's work was important for another reason: with him, theological hypotheses began to become external to science. For Newton had not merely postulated attributes of the Divine Will in constructing his theories; he also found in the laws of nature on which he based his system reasons which he believed required God to intervene in the events of His creation. On various occasions he appealed to three implications of his laws. Thus, he pointed out, bodies lose momentum in impacts: the universe would run down unless there were intervention beyond the laws of nature to maintain that momentum—a job left by the laws of nature to God. Again, if the solar system were left to the governance of the law of gravitation, the mutual gravitational perturbations of the planets would pull them out of their neat Keplerian orbits; God would have to intervene every so often to maintain the order. And finally, there were the disturbing contradictions, pointed out so tentatively

(and incompletely) by Richard Bentley, concerning the possibility of universal gravitation in either a finite or an infinite universe; only God, Newton sometimes proposed in response to these difficulties, could maintain a universe in which universal gravitation operated.

Though a fully satisfactory treatment of the argument from loss of momentum would have to await the nineteenth and twentieth centuries, it had already been rejected by most people by Laplace's day: momentum is conserved, at least in collisions of the elementary atomic constituents of things which were conceived as "perfectly elastic" (even if absolutely rigid); and if momentum appears not to be conserved in collisions of ordinary bodies, that must simply be because it has been transferred to those inner, invisible constituents. As to the stability of the solar system, though he did not complete the job, Laplace laid down the basic arguments to demonstrate that gravitational perturbations are in the long run self-correcting: the law is, after all, sufficient to account for the orderliness of the solar system.

The Bentley paradoxes of gravitation were overlooked, indeed forgotten until the end of the nineteenth century; they were resolved only with the theory of general relativity. (Einstein, in pardonable ignorance of the history of the problem, christened it "Seeliger's Paradox.") Whether the story of his encounter with Napoleon is apocryphal or true, Laplace at least had solid arguments (as opposed to philosophical generalities) to offer for showing that science had no need of such hypotheses by counter-arguing specific arguments purporting to show that it did. Though the matters involved were not yet completely settled, and would not be for a long while, arguments that theological considerations were relevant to science had been deflated; such considerations were external to science precisely because the laws of science had been shown (even if as yet imperfectly and incompletely) to be sufficient to account for certain phenomena which had previously seemed to require divine intervention.

A similar portrayal could be offered for the development of the compositionalist approach to chemical explanation: as the nineteenth century wore on and questions concerning the nature of acids, the chlorine problem, and a multitude of other issues fell into place, the compositionalist approach came to be seen as clearly able to do a great deal toward the understanding of material substances—far more than its by-then-dead rival, the "perfectionist" approach.

The nineteenth century saw the vindication of many ideas for many domains: electricity, magnetism, heat, light, many areas of chemical investigation, and so on, these vindicated beliefs being of all levels of generality, from very specific beliefs about,

e.g., particular substances or particular behavior of compass needles in particular circumstances to very general theories such as Maxwell's. Theoretical accounts of domains were shown to be highly successful with regard to their domains: the theories had a responsibility for accounting for a body of putative information which was, in general, if not always, well-delineated, and did so effectively within the range and limits of experiment and observation. Though there were problems with regard to many of these ideas, in many cases they had to do with specific reasons for supposing the theories to be *incomplete* rather than *incorrect*: they were not contradicted by any domain information, they just failed to take account of some item of their domain, and so, there was reason to believe, merely had to be extended rather than rejected or changed in fundamental ways. (Electromagnetic theory still had to be extended to cover the electrodynamics of moving bodies, for example.)

By now, several such theories had another appeal besides their success in accounting for their own circumscribed domains. For since Newton had fused terrestrial and celestial mechanics into a single theory, there had been increasing reason to believe that theories of different domains might well be the same. Electricity and magnetism, despite the differences noted by Gilbert and his successors, were gradually fused in a historical process beginning in the 1820s and culminating in Maxwell's theory; and that theory also incorporated light. That unification itself required the prior unification (mainly by Faraday) of various alleged kinds of electricity. Beginning with Davy and Berzelius, chemical bonding also began to be seen in terms of electrical attraction and repulsion. The process continued in the twentieth century, with the resolution of conflicts between electromagnetism and mechanics by special relativity, the unified accounts of spectra, atomic structure, chemical valency, and so on by quantum theory and ultimately by quantum mechanics; the quantum field-theoretic treatment of the domains of weak, electromagnetic, and strong interactions, resulting in the unified electroweak theory and movement toward grand and still grander unification. The trend was already evident in the nineteenth century, even though it was not always clear which type of theory should provide the basis for unification: should we continue to move toward a more complete mechanical world-view, absorbing electromagnetism and other areas into it, or vice-versa? (We see similar problems in the 1950s and 1960s: should field theory be abandoned as the guiding program for unification, and replaced by something else, like S-matrix theory? But notice that the questions were, in the twentieth century case as in the late nineteenth, resolved.)

From the viewpoint of our topic, though, the important facet of such unification was this: that *in addition to its success in accounting for its domain, a theory could be judged in terms of its compatibility with theories of other domains.* (There were, however, developing constraints on such judgments: not all domains could be reasonably expected to be explained in the same terms. I shall ignore such qualifications here, though.) In addition to doubts based on its failures to account for its domain of responsibility, a theory can also be doubted on the ground that it fails to conform to a type of theory with which we believe it ought to conform—for example, because that type of theory has been successful in several other domains.

Over the course of the centuries (the last few especially), a great many beliefs about nature have been found to have been both *successful* with regard to their domains of explanatory responsibility, and *coherent* with theories of other domains. This is not to say that there were not problems with regard to some of these theoretical beliefs. But in assessing the significance of such problems, we must be careful in three important respects. First, we must remember that problems of incompleteness are not reasons for rejecting a theory, and do not become reasons even for doubting a theory until a great deal of effort has been put into attempting to extend it to cover the recalcitrant domain items. Second, the fact that a theory has been tested and found successful and coherent with other theories only within certain experimental limits (of accuracy, velocity, mass, and other relevant parameters) is not by itself a reason for doubting the theory—though of course it may be a reason for extending the tests of the theory beyond those limits.

The third point is perhaps the most important. However much success and coherence a theory may achieve—however much it may be free of the *specific* doubts imposed by its specific failures to account for domain-items and to cohere with other theories with which we believe it should cohere—such doubts *might* arise in the future: there is always the *general possibility* of doubt. But the general possibility of doubt arising, insofar as it is a *mere possibility*, is not itself a ground for doubting any particular theory. The same may be said of all general possibilities as “I may always be dreaming,” or “A demon might be deceiving me in all my beliefs,” or “Perhaps I am only a brain in a vat.” Applying as they do equally and indiscriminately to *any* claim whatever (including the negation of the claim itself), such *universal doubts* cannot count against any one claim in preference to another, and *therefore in the actual knowledge-seeking enterprise they do not count as legitimate grounds for doubt.* Many philosophers and sociologists of science have failed to recognize this point in their accounts of

the dubitability of scientific claims; but it is ignored only at the peril of having failed to move beyond the approach to knowledge represented by Descartes’ dream and demon arguments in the seventeenth century—arguments which play no role in science, except as reminders that specific doubts *may* always arise in the future.

Keeping these three points in mind, we can see that it makes sense to speak of an accumulation of beliefs which, by virtue of their success and coherence, could be trusted by scientists. Many of these beliefs were free of any specific doubts in the senses I have just discussed, or at least close to it. The growth of such a body of beliefs was not a simplistic march of accumulation: many of the beliefs did fall subject to doubts, and had to be rejected. Other beliefs had their ups and downs: I have mentioned in passing the fortunes of quantum field theory in the 1950s and 1960s; more generally, that approach may be seen as promising in the early 1930s, with doubts increasing, satisfaction with regard to many of them in the late 1940s, doubts sufficient to lead many physicists to reject the approach in the 1950s and 1960s, and triumphant successes in 1971 and thereafter from which it will not easily be displaced. But even though such doubts can arise with regard to any theory, even those regarding which there are at a given time no specific doubts, *that body of beliefs which are, at a given time, free of such doubts (or even reasonably close to being free) constitute a basis on which science can alter its domains and build further hypotheses, methods, rules of reasoning, and goals.*

What has been developed through this process is a distinction between beliefs which can serve, from a scientific standpoint, as background in terms of which scientific change can proceed—in terms of which new scientific ideas, methods, goals, and so forth can be built—and those which it cannot employ in such building, at least in the sense that they have not been *scientifically* legitimated. It is, in short, a working distinction—one which has a function in the knowledge-seeking enterprise—between internal and external considerations in science. But it is a distinction which has been forged in the very process of investigation of nature, not laid down in some edict from heaven or philosophy which determines what counts as scientific and what does not. The process is one of a gradual discovery, sharpening, and organization of relevance-relations, and hence of a gradual separation of the objects of its investigations and what is directly relevant from what is irrelevant thereto: a gradual demarcation, that is, of the scientific from the non-scientific. Those considerations become internal, scientific, which *have been found*, as a matter of contingent fact, to be doubt-free (successful and coherent) and relevant to the

domain under investigation. All other considerations become external, non-scientific.

In the light of this process of internalization, it is easy to see why and how scientific change is so pervasive. In the course of inquiry, domains come more and more to be formulated in the light of background beliefs which have proved doubt-free and relevant to the domain being investigated, rather than in terms of such criteria as sensory similarities. But the background beliefs which lead to such changes in domain structure and conception also lead to alterations, sometimes profound, in other parts of the fabric of science. Sometimes it becomes necessary, in the light of problems that arise concerning a restructured domain or a new theory of the domain so restructured, to reject or modify some of the very background beliefs which led to the restructuring. The problems associated with particular domains become altered, as do the lines between recognized "scientific" problems and questions that are classed as "non-scientific." What counts as "observation" of a subject-matter, too, may be altered; both the entities or processes to be studied, and the methods of observing them, can develop with the acquisition of new successful and doubt-free beliefs. New background information, or old information formerly considered irrelevant, is found to be relevant to a particular domain. Old methods are rejected or reinterpreted, new ones introduced; new standards of possibility and acceptability arise. Even the goals of science may alter, as in the transition from perfectionist to compositionalist matter-theory.

Still, at any given stage, there is a body of background beliefs on which science relies. The hypotheses of current particle physics are not deduced or induced from a brute given; still less are they constructed in an intellectual vacuum. Their mode of formulation and their plausibility alike, despite their problems, are obtained from a prior background. Grand Unified Theories (GUTs) and their application to cosmology apply the idea of symmetry breaking using the Higgs mechanism previously employed in the highly-successful electroweak unification to join the latter with the newly functional theory of the strong interaction, quantum chromodynamics; the electroweak theory and QCD in turn were based on the gauge-theoretic (more specifically, Yang-Mills) approach whose importance became clear with the establishment of the renormalizability of such theories in the early 1970s. That approach itself was developed in the light of a long background including the enormously successful theory of quantum electrodynamics and the even more central place which group theory, especially Lie groups, had come to play in physical theory. All these pieces of background traced their pedigrees ultimately to quan-

tum mechanics and its necessary logical extension through the application thereto of special relativity, the latter two theories, at least, particularly when so joined, having proved free of specific and compelling reasons for doubt, and thus having the status of scientific background beliefs suitable as bases on which to build.

To make this process of scientific change clearer, it is necessary to call attention to one way in which reaction to traditional views of science has gone too far: namely, the rejection of the "given." For there is an important sense in which there is a "given" after all. True, it is not the given as imagined in classical and positivist myth; it is not, that is, a given in the sense that it is found as a result of pure perception, pure, that is, of any prior belief. Much less is it a given in the sense that, once recognized, it can tell us automatically the character of the world by which it is given. Nor is it even necessarily the product of a single effort, a look, but is often the product of many efforts, complete with the open possibility of error. Rather it is a given in the following three respects: that, (1) having been marked out as significant by our best available background ideas, (2) having been appropriately described in terms of those background ideas, and (3) having been made accessible by application of background ideas (a "theory of the instrument"), the specific character or value we find it to have is independent of—not determined by—those background ideas. Even with regard to it, doubt might always arise; but in its absence, the character or value of the "given" can lead to modifications in other beliefs.

III

Through what I have called a process of internalization, science has found it possible more and more to achieve autonomy from external influences in building its future beliefs, methods, problems, rules of reasoning, explanatory patterns, standards, and goals. It has done so by relying on a body of background beliefs selected for their success in accounting for their domains of responsibility and for their coherence with theories of other relevant domains. It has learned how to learn. Things need not have turned out that way. Connections between things in nature might have been so tight that a piecemeal approach to inquiry would have failed; theories of different domains might not have been coherent with one another; and so on. The achievement of internalization is a contingent matter, not one of logical necessity or of the nature of science.

But because the degree of such autonomy is a function of the available background beliefs, it is evident that the information will, in at least some cases, to

some extent, be insufficient to guide the construction of new beliefs and research programs. And where our legitimized "internal" background beliefs are inadequate, we must look elsewhere for guidance. Sometimes we will have to look to less well-founded beliefs, or to beliefs which, however well-founded, have not been shown to be unambiguously relevant to the domain under consideration. And although it is highly unlikely, for example, that new approaches in contemporary particle physics, if they do not come from within physics itself, will come from anywhere else except mathematics, there is nothing to rule out occasional success of purely external appeals.

But the situation in modern science is radically different from what it was in early periods. Then, as we have seen, choice of what background considerations to appeal to or rely on was very much open: the distinction between internal and external considerations, between the scientific and the non-scientific, had not yet evolved. To say that the hypotheses proposed, the problems conceived, the standards applied, and so forth, were shaped or even determined by external (philosophical, political, economic, social, psychological) considerations, even in the majority or perhaps all cases, can be quite convincing. But to make similar all-embracing claims about modern physics, for example, is to ignore the substance of the subject. Sociology of science does itself a disservice when (and if) it makes the blanket claim, as some adherents of the "Strong Program" frequently appear to do, that all alleged internal considerations in science are externally determined, or even the "weak" blanket claim that, though there are internal considerations, they must always be supplemented by external ones to permit scientific development. One of the reasons why such extreme views have been advocated has been the difficulty of seeing how the internal-external distinction could be formulated given the rejection of the traditional view of science which I surveyed at the beginning of this paper. But the view I have outlined here provides a formulation of that distinction which is completely coherent with the newer view of the pervasiveness and depth of scientific change.

With this new formulation of the distinction in hand, it is possible to conceive more clearly the tasks of the sociology of science. To legitimize itself as a subject it does not need to deny that there are internal considerations at work in science, or to claim that such internal considerations as there may be are inadequate to do the job of science. But the present view, while showing how the internal-external distinction is viable, and how much of science—increasingly through the process of internalization—is directed by internal considerations, leaves open the possibility that in *some* cases,

even in the most modern circumstances, the internalized background of science is *in fact* inadequate. What sociology of science needs are sharpened tools for deciding, *on a case-by-case basis rather than in the light of a general thesis*, if and when this happens. That is, rather than presupposing, on the basis of very general philosophical arguments, that *all* science is (or should be viewed from the sociological perspective as) shaped by external considerations, it needs to be able to decide *when* "negotiation" is concluded in the light of scientific considerations and *when and to what extent* such conclusions are arrived at on the basis of factors other than scientific. (It might of course conclude that whether the factors involved in a particular case are external or internal is ambiguous—a possibility clearly permitted by my formulation of the distinction.) In other cases, the immensely important traditional task of the sociology of science remains: to show how extrascientific considerations often combine with internal ones to direct science, sometimes aiding, sometimes conflicting with, the direction that would be taken if internal considerations alone were operative. (Whether and how, for example, social and political conditions in England in the late seventeenth and eighteenth centuries⁴, or in Germany in the 1920s⁵, combined with internal factors to influence the course of science, rather than determining that course by themselves.) But for this purpose the sociology of science *requires* the distinction between internal and external factors, and that is why it does itself a disservice by denying it—in addition to the fact that denying it appears so perverse.

For the sociology as for the philosophy of science, the day is past for sweeping universal theses about the nature of scientific thought based on a few general and questionable philosophical or methodological contentions, or on case studies which arguably presuppose rather than support those contentions. We need a closer look at the subject purportedly under investigation, one which recognizes the patent differences between the considerations relied upon in sophisticated science as scientific, and those which, while often influencing science (as science influences them), are *de facto* non-scientific, external to science. And we need a look which recognizes that the extent and character of those interactions may be quite different from case to case, and in some cases may even be non-existent. In short, it is time at last for a piecemeal approach.

NOTES

1. A good survey of current approaches in the sociology of science is Michael Mulkay, *Science and the Sociology of Knowledge*, (London: Allen and Unwin, 1979). The "Strong Program" is

advocated in the writings of David Bloor, Harry Collins, Trevor Pinch, and many others.

2. Further details of the view outlined here may be found in the essays collected in my *Reason and the Search for Knowledge*, (Dordrecht: Reidel, 1983); "The Concept of Observation in Science and Philosophy," *Philosophy of Science*, March, 1982, 485-526; and "Objectivity, Rationality, and Scientific Change," in *PSA 1984*, P. Kitcher and P. Asquith, eds. (East Lansing: Philosophy of Science Association, 1985); and "Method in the Philosophy of Science and Epistemology: How to Inquire about Inquiry and Knowledge," in *The Processes of Science*, ed. N. Nersessian, (Dordrecht: Nijhoff, forthcoming). In those and other works I argue, beyond the theses of the present paper, that the "internal" considerations, upon which science increasingly is able to rely, count as "rational" considerations, and that they can, and sometimes do, provide "knowledge." Those further contentions, however, are not required here, where the sole purpose has been to outline the basis of the internal-external distinction and its relevance to the sociology of science as I see it.

3. It would be impossible to list all the primary and secondary references relevant to the cases discussed in the following pages. Many such references are given in other writings of mine, especially *Reason and the Search for Knowledge* (see footnote 2). A few major books on the cases discussed in this

paper are the following: W.K.C. Guthrie, *A History of Greek Philosophy*, 6 volumes (Cambridge: Cambridge University Press, 1962-1981); G. Vlastos, *Plato's Universe* (Seattle: University of Washington Press, 1975); A. Koyre, *The Astronomical Revolution* (Ithaca: Cornell University Press, 1973); R.P. Multhauf, *The Origins of Chemistry* (London: Oldbourne, 1966); M.P. Crosland, *The Language of Chemistry* (Cambridge: Harvard University Press, 1962); R.S. Westfall, *Never at Rest: A Biography of Isaac Newton* (Cambridge: Cambridge University Press, 1980); S.G. Brush, *The Kind of Motion We Call Heat: A History of the Kinetic Theory of Gases in the 19th Century* (Amsterdam: North-Holland, 1976); E.T. Whittaker, *History of the Theories of Aether and Electricity*, 2 volumes, (London: Thomas Nelson & Sons, 1951-1953). Needless to say, the interpretations of cases and the conclusions which I have drawn from them, which are expressed in this paper, have derived not only from these, but also from many other books and articles. That is not to say, of course, that those authors would necessarily subscribe to my interpretations and conclusions.

4. Margaret Jacob, *The Newtonians and the English Revolution* (Ithaca: Cornell University Press, 1976).

5. Paul Forman, "Weimar Culture, Causality and Quantum Theory, 1918-1927," in *Historical Studies in the Physical Sciences*, ed. R. McCormach, Vol. 3 (Philadelphia: University of Pennsylvania Press, 1971), 1-115.

Comments and Reply follow on pages 10-23.

Incorrectness and Specific Doubts: Comment on Shapere

P. Thomas Carroll
Rensselaer Polytechnic Institute

Like many historians, I first encountered Professor Shapere through his insightful critiques of *The Structure of Scientific Revolutions*. Though they did not exactly convert me to his position, Shapere's writings on the subject clarified many thoughts in my young graduate student mind, and they left a lasting impression. It is thus easy for me to consider Shapere's present work in historical perspective, an exercise that provides insight into the evolution of his views.¹

Shapere's review of the enlarged edition of *Structure*, and of *Criticism and the Growth of Knowledge*, remains a model of careful and close reasoning, direct prose, and attention to central contentions, all of which are exemplified in his final sentence, a direct statement of his main positions:

The point I have tried to make is not merely that Kuhn's is a view which denies the objectivity and rationality of the scientific enterprise; I have tried to show that the arguments by which Kuhn arrives at his conclusion are unclear and unsatisfactory.²

As Kuhn and many others who have worked in this area have no doubt discovered, satisfying Shapere is not easy, but I suspect most will admit—albeit perhaps begrudgingly—that the attempt greatly improves one's scholarship.

We can all be thankful, therefore, that the same kind of careful and close reasoning, direct prose, and attention to central contentions, the same tonic admonitions about the danger of vague generaliza-

tions, and that same focus upon the objectivity and rationality of the scientific enterprise, characterize "External and Internal Factors." As was the case in 1971, Shapere impresses with the range of his knowledge and the precision of his distinctions. Also true to character, and more important, is that he presents before this readership something we may not encounter often enough, namely, a reminder of what a stunning edifice the current Western scientific enterprise is. In our zeal to free ourselves from positivist, linear, and naively cumulative notions of scientific change, we sometimes fail to stress sufficiently—or even to acknowledge—the size, degree of articulation, subtlety, systematic integration, and palpable explanatory power of the modern Western sciences. As he rightly claims, practitioners in the social study of science—even unrepentant relativists—ignore these features of the modern scientific enterprise at their peril.³

In other ways, though, Shapere's paper differs significantly from his earlier work. I must list first, of course, his ready declaration that the traditional idealist view of scientific change "is now a shambles." As a historian, I am gratified to see him credit a "torrent of historical studies" with indicating "more and more convincingly that changes over the development of science have gone far deeper than mere change of theory."⁴ Anyone who convinces Shapere on such a major point deserves a medal. Indeed, all of us have modified our views about these matters at least a little in the last fifteen years or so, and Shapere's pluralistic characterization of what I prefer to call the sciences is one indicator of this. On the other hand, Shapere characterizes the demise with words like *shambles*, having a negative connotation and implying movement toward disorder. Others might celebrate the change as, say, the exorcism of demons, preferring what they consider to be a more naturalistic order in the paradigms that are replacing idealism.

Regarding Shapere's main thesis, this new essay retains the author's traditional sharp distinction between internal and external factors in the development of the sciences, but it emphasizes a significantly revised formulation of the distinction. Rather than

Author Address: Department of Science and Technology Studies, Rensselaer Polytechnic Institute, Troy, New York 12180-3590 USA

An earlier version of this paper was read at the tenth annual meeting of the Society for Social Studies of Science, Rensselaer Polytechnic Institute, October 26, 1985, as commentary on Dudley Shapere's "Internal and External Factors in the Development of Science," this issue, pp. 1-9. My thanks to the following for help: Daryl Chubin, Susan Cozzens, Nancy Slack, and the members of the informal study group on sociology of knowledge at the Program in Science, Technology, and Society, Massachusetts Institute of Technology. This work was supported in part by the Paul Beer Trust, School of Humanities and Social Sciences, Rensselaer Polytechnic Institute, and by an Exxon Research Fellowship, sponsored by the Exxon Education Foundation, in the Program in Science, Technology, and Society, Massachusetts Institute of Technology.

merely criticize another's theory, such as Kuhn's, Shapere here develops his own, based on contingent, evolving criteria of correctness. Like Einstein, he seeks the remaining universal reference point in a relativistic world, and he claims that we have found it through the piecemeal approach and a studious application of Ockham's Razor. To support this thesis, Shapere acts more like a historian than was his older style, attempting something of a thumbnail history of Western science that boldly spans the period from the "Milesian philosophers of the sixth century B.C." to Grand Unified Theories, the electroweak theory, and quantum chromodynamics. A final contrast is that the target of his criticism on this go-around is not Kuhn and paradigms, but the strong and weak programs in the sociology of knowledge. It is to these similarities and differences that I wish to address the remainder of my brief remarks.

A few clarifications are in order first. Although I took my doctorate in the history and sociology of science, I consider myself primarily an American historian with a specialty in the history of American science and technology during the nineteenth and twentieth centuries. I profess no great knowledge of the Milesian philosophers of the sixth century B.C., or even of Newton for that matter. What is more, I do not purport to represent the sociologists of the strong or weak programs who, though they may accept me as a character witness, would probably reject me as their defense counsel. Rather, I was asked to hint briefly at what historians of science have been up to in recent years and to consider what light their work and Shapere's shed on each other. My remarks are best understood as a report home from the research front, rather than as a carefully-constructed logical argument; I feel no particular urge to prescribe a right course for my fellow historians, nor to pick their monographs with a fine tooth comb. Finally, what follows is obviously not a definitive recent historiography of science, nor is this the place for one.⁵

The most pronounced trend in focus I see in the history of science over the last decade or so has been the shift to the period since the Industrial Revolution. Scholarship on earlier eras continues, of course—Westfall's scrutiny of Galileo being a superb case in point⁶—but most of my contemporaries have stuck to the last century and a half, and many other historians, such as Karl Hufbauer and Michael S. Mahoney, have begun considering much more recent topics than had been their practice a decade or so ago.⁷ A concomitant trend has been the move away from the history of physics and toward the history of the biological, earth, and social sciences, personified in the scholarship of Daniel J. Kevles.⁸ The first issue of the revived *Osiris*, on American science, symbolizes the attention paid in recent years by a great many younger historians to the peculiarities of the sciences

in particular national cultures.⁹ Though the annals of the Henry Schuman Prize and the Pfizer Award of the History of Science Society do not reveal it very clearly, there does appear to be a slight trend toward the study of groups of less-prominent scientists, such as Rossiter's study of American women, or my own analysis of immigrants in American chemistry.¹⁰ Much more clearly discernible is the attention paid to institutional contexts for science, most particularly (*vis-à-vis* Shapere's concerns) to research schools, as noted by Gerald L. Geison in *History of Science* a few years back.¹¹

These shifts bring much more of the current work in history of science squarely into the era in which, Shapere claims, science became "internalized." One might expect, then, that as they explore this special era of scientific autonomy, the historical community would be moving closer to his formulation, growing increasingly unimpressed with the explanatory power of such sociological shenanigans as the strong and weak programs, and commenting more on the influence of internalized tradition. I will concede to him that my colleagues do seem to accept the idea that the sciences have somehow entrenched in the last century or two. There is no question that, with professionalization (and the specialized education that accompanies it), the sciences have achieved a remarkable degree of autonomy and intellectual productivity. I will concede as well that, if my experience is indicative, historians of science now spend far less time than they used to do discussing the tensions between internal and external factors. If I may descend briefly to the consideration of unpublished discussions, I will even admit to witnessing a half-dozen spirited attacks among so-called contextualist historians upon the speciousness of "interests" as discernible entities, and there have even been suppressed yawns now and then about the descent of certain constructivist sociologists of knowledge into "mere epistemology."

Are historians of science thus coming to agree in large measure with Shapere's contention that discernibly internal factors increasingly determine scientific change? That is less clear. In those same conversations where "interests" were challenged and constructivism was dismissed, there was also a great deal of evolutionary and ecological rhetoric, with terms like "resonate," "inosculate," and "mutually constitute," but no mention of the progressive elimination of doubts, specific or otherwise. Perhaps many historians agree that we have indeed eliminated some doubts for the foreseeable future, but many also believe, more than Shapere, that we have not significantly narrowed what he calls "the large gap between scientific ambition and scientific conclusion." The difference of opinion is less over the volume of fluid in the partly-full glass,

and more over the size of the glass. The trend toward study of disciplines other than physics probably represents, more than anything else, the differentiation of scholarship one might expect—regardless of intellectual trends—in a growing field, but the stronger shifts toward national cultures, demographic foci, and research schools imply concentration upon those sites where cultural patterns take shape in institutions and in groups of practitioners, and become manifest in conceptual styles. This type of study does not strike me as indicative of a shift away from external determinants and toward internal determinants of scientific change. Either my overview is off the mark, or a sizable segment of the history of science community is going astray, or Shapere's thesis is flawed.

If historians of science show signs of studying the best evidence for Shapere's case (i.e., the recent and "internalized" period), then why are they not reaching the same conclusions as he? Let me offer two ideas. First, to an unprecedented degree, historians of science are being trained, first and foremost, as historians. That implies much less self-trust about the meaning in the historical evidence, much more reliance upon varieties of evidence, including substantial archival and manuscript sources, much more exposure to the larger cultural context of particular scientific developments, and a healthy aversion to whiggism and other logical maladies afflicting the trade. From such a stance, there are some grounds for considering Shapere's account to be questionable history. (It is also hardly an exemplar of the piecemeal approach to historical truth!) We find, for example, that "with the beginnings of intensive study" of "common, everyday classifications" of crystalline substances, "*distinctions were found between members of the types which led to distinct classifications*" [my emphasis]. We learn later, to mention just one more example among many, that "*certain approaches. . . did show themselves able to account to a considerable degree for their respective domains*" [my emphasis]. Who did this finding and showing? Under what circumstances? On what evidence do we base this? Can we be sure that, when those people used a term, they meant by it what we mean? Did all the translators since then get the meaning? Was there any comprehensive search for alternative distinctions and approaches possessing equal or better efficacy? And so on.

Shapere could no doubt reply here that his paper merely summarizes the exacting scholarship of a great many giants among historians, on whose shoulders he stands. All well and good, but herein lies the crux of a problem with claims for the cumulative growth of knowledge. Note what is lost in Shapere's synthesis: there are almost no people. Things just happen without the exercise of human

choice, without the messy step of discrete bits of theory getting jumbled up in the cluttered heads of real flesh-and-blood humans, and as in all true Hollywood films of the interwar era, somehow we know what the outcome will be before we even settle into our seats (i.e., modern scientific knowledge will emerge, at just about the same time that the guy in the white hat gets the girl). The problem from this line of reasoning is not, then, that Shapere has no case to make, but that the case is not quite made to the satisfaction of exacting historians. Many such people are probably willing to accord a measure of authority, efficacy, and autonomy to modern scientific knowledge, but Shapere advocates more than that.¹²

The cluttered heads of the real flesh-and-blood humans suggest a second and probably more serious reason for the dearth of neo-internalist formulations among historians of science. Shapere may espouse a revised approach to internalism—one with admittedly contingent standards of acceptability, and with a refreshing statement of the irrelevance of "universal doubts"—but he remains wedded to the same old pursuit, namely, the identification of outright determinants of scientific change. In this he and the members of at least the strong program are agreed. While Shapere takes the latter to task for attributing all scientific content to interests, he himself talks of "*the direction that would be taken if internal considerations alone were operative*" [my emphasis], as if the invisible hand of nature would shove us down a single fixed path if human meddlers would just stop getting in the way, and although he admits that nature has not always had its way in Western history, he contends that it increasingly does so.

All such formulations share a sort of binary logic of correctness or incorrectness, of mechanistic causation of B by A. Thus the title of my commentary. Because beliefs in the cluttered heads of real flesh-and-blood humans, including scientists, do not necessarily work in an A-causes-B manner, many historians—myself among them—are groping instead toward some sort of different process of mutual interdependence, one that can help us appreciate the very tangible accomplishments and authority of modern science, and that preserves some sense of definitive refutations in scientific thought, without conferring incontrovertibility upon that authority. In short, a great many historians have become ecological in their perspective. I can agree with Shapere that, in any social study of science, one's research focus should usually be piecemeal rather than programmatic. One's perspective is another matter. From an ecological point of view, the search for exact "causal determinants"—whether internal or external—seems an unnecessary burden, a reductionism no longer required to preserve the integrity

of scientific knowledge. Cumulative tradition, no matter how large, interconnected, useful, or even dependable, will never make scientific knowledge into certainty, nor guarantee its superiority over competing alternatives, and since it cannot do so, scientists risk error by being complacently internalist.¹³

Mention of unnecessary burdens calls to mind a common parable, told to me by John Koller, a philosopher colleague at RPI. Two Buddhist monks, an old master and his impressionable pupil, are taking a contemplative walk. They encounter a beautiful woman having trouble wading across a wide stream. Mindful of the vow of celibacy that all Buddhist monks take, the younger man avoids the woman, but the older monk takes her in his arms and carries her to the other shore. Astonished, the younger monk thinks of nothing else but this incident for the rest of the afternoon. As they reach home, he can contain himself no longer, so he asks his teacher to explain the apparently forbidden behavior. "Are you still bearing the weight of that woman?" asks the elder monk in reply. "I put her down back there on the shore."¹⁴

NOTES

1. Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962; 2nd ed., enlarged, 1970). For some engaging thoughts about the task of studying the evolution of thought in a historical context, see Dudley Shapere, "What Can the Theory of Knowledge Learn from the History of Knowledge?" *Monist* 60 (1977): 488-508.

2. "The Paradigm Concept," *Science* 172 (April-June 1971): 706-709, at 709. Shapere's most thorough and influential critique of Kuhn's thesis is his "The Structure of Scientific Revolutions," *Philosophical Review* 73 (1964): 383-394; reprinted in Gary Gutting, ed., *Paradigms and Revolutions: Appraisals and Applications of Thomas Kuhn's Philosophy of Science* (Notre Dame and London: University of Notre Dame Press, 1980), 27-38. I chose to open with his review of the second edition, however, not only because it was the first writing of his which I read, but also because it epitomizes succinctly those characteristics of his position I have chosen to emphasize.

3. Even if the strong program people are right that scientific knowledge does not deserve the respect we used to accord to it, the fact remains that the Western sciences are no garden-variety manifestations of interests—they're humdingers.

4. I worry, however, that Shapere denies the sociologists and anthropologists their due in this transformation. In recent decades, the social sciences have altered the way historians interpret the past in many ways; this role in the eradication of idealism is no less significant for its indirectness. Also, the phrase "more and more convincingly" implies a kind of linear progress in historical scholarship that makes me uncomfortable.

5. Readers interested in more than my reflections should consult such sources as Arnold Thackray, "History of Science," in Paul T. Durbin, ed., *Guide to the Culture of Science, Technology, and Medicine* (New York: Free Press, 1980), 3-69; and Roy MacLeod, "Changing Perspectives in the Social History of Science," in Ina Spiegel-Rösing and Derek de Solla Price, eds., *Science, Technology, and Society: A Cross-Disciplinary Perspective* (London and Beverly Hills: SAGE Publications, 1977), 149-195. The historiographic reviews in *History of Science* constitute another useful source.

6. Richard S. Westfall, "Science and Patronage: Galileo and the

Telescope," *Isis* 76 (1985): 11-30.

7. A comparison of the two most recent membership guides of the History of Science Society indicates the trend. In the 1980 *Isis Guide to the History of Science*, only 19 per cent of the 1468 listed members chose to be indexed as interested in the nineteenth to early twentieth centuries. In the 1983 *Guide*, the number had risen to 26 per cent of the 1654 listed members. There is a similar trend for interest in "contemporary science," although I have not tabulated it. (My thanks to *Guide* co-editor Linda Flagler Stevens for help with these computations.)

Hufbauer's early work on Enlightenment German chemistry, begun in graduate school and culminating in *The Formation of the German Chemical Community (1720-1795)* (Berkeley: University of California Press, 1982), has given way to an interest in modern interdisciplinary work and the tackling of the stellar energy problem (e.g., "Astronomers Take Up the Stellar-Energy Problem, 1917-1920," *Historical Studies in the Physical Sciences* 11 [1981]: 277-303). Mahoney's interest in algebra in the Scientific Revolution (e.g., *The Mathematical Career of Pierre de Fermat [1601-1665]* [Princeton: Princeton University Press, 1973]) has been augmented by study of the contemporary computer revolution (personal conversation). These are only salient instances in my immediate acquaintance, but they are typical.

8. Cf. *The Physicists: The History of a Scientific Community in Modern America* (New York: Alfred A. Knopf, 1978), begun as a doctoral dissertation in the early 1960s, and *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (New York: Alfred A. Knopf, 1985). I suspect, however, that like me, Kevles will deny being either a historian of physics or a historian of biology, preferring to delineate his interests by time periods and cultural contexts instead of scientific disciplines. That in itself testifies to the plausibility of my claims.

9. Sally Gregory Kohlstedt and Margaret W. Rossiter, eds., *Historical Writing on American Science*, constituting *Osiris*, 2d ser., 1 (1985). For a glowing review, see Hunter S. Dupree, "A Landmark in the Cumulative Literature of the History of American Science," *History of Science in America: News and Views* 3, 4 (November-December 1985): 1-3.

10. Margaret W. Rossiter, *Women Scientists in America: Struggles and Strategies to 1940* (Baltimore and London: Johns Hopkins University Press, 1982); and P. Thomas Carroll, "Immigrants in American Chemistry," in Jarrell C. Jackman and Carla M. Borden, eds., *The Muses Flee Hitler: Cultural Transfer and Adaptation, 1930-1945* (Washington, D.C.: Smithsonian Institution Press, 1983), 189-203. Scholarship such as this begins to provide what Lewis Pyenson advocated in "Who the Guys Were: Prosopography in the History of Science," *History of Science* 15 (1977): 155-188, although his main title (a quotation from Namier) is ironic in light of Rossiter's subject matter. The shift of attention from great men to little people is, of course, a characteristic feature of the move away from idealism that Shapere notes, not only in the history of science, but across many disciplinary boundaries.

11. "Scientific Change, Emerging Specialties, and Research Schools," *History of Science* 19 (1981): 20-40. The study of institutions is far larger than this, of course, but here is no place to review the relevant literature.

12. The usual response here, of course, is that the particular actors and the circumstances of their work become irrelevant over time. After all, one will always find " $f=ma$ " in the end, because that is the way nature is. Perhaps. I happen to share Shapere's faith that nature is neither capricious nor wholly inscrutable. However, how many other equally fundamental and simple insights into the workings of nature have we yet to discover, and could we have reached a state of "internalized" science without ever being concerned with the relations between force, mass, and acceleration? There is also the usual nominalist problem with these concepts: mass, after all, is no longer "really" mass, but is now a certain something else—neither matter nor

energy—measured in electron volts. Therefore “ $f = ma$ ” is not “really” the way nature is, but only a model of something we do not fully understand. Shall it always be thus, and if so, are we any more “internalized” in any substantive epistemological sense than we ever were? (For a careful study of the theory behind names and classification in one aspect of chemistry, see John E. Lesch, “Conceptual Change in an Empirical Science: The Discovery of the First Alkaloids,” *Historical Studies in the Physical Sciences* 11 [1981]: 305-328.)

13. Regarding ecological thinking in science and technology studies, the theories of Donald T. Campbell are suggestive, if not entirely satisfactory (see Marilyn B. Brewer and Barry E. Collins, eds., *Scientific Inquiry and the Social Sciences: A Volume in Honor of Donald T. Campbell* [San Francisco: Jossey-Bass Publishers, 1981]). Both the historical scholarship and the historiography of Charles E. Rosenberg are noteworthy; see, for example, *No Other Gods: On Science and American Social Thought* (Baltimore and

London: Johns Hopkins University Press, 1976); and “Toward an Ecology of Knowledge: On Discipline, Context, and History,” in Alexandra Oleson and John Voss, eds., *The Organization of Knowledge in Modern America, 1860-1920* (Baltimore and London: Johns Hopkins University Press, 1979), 440-455.

Regarding Shapere’s enthusiasm for the piecemeal approach, there are clear signs that the sociologists of knowledge agree with him. Their more recent studies have involved delimited, *in situ* epistemological explorations of specific laboratories, as well as specific historical cases. In this sense, it is a bit misguided to attack the strong program *per se* when sociologists themselves have moved on to laboratory life and similar projects.

14. Both Koller and his written source offer a more sophisticated version than this, although the main point is the same. See “Muddy Road,” selection 14 in Paul Reys, comp., *Zen Flesh, Zen Bones: A Collection of Zen and Pre-Zen Writings* (Garden City, N.Y.: Doubleday and Company, Anchor Books, 1961), 18.

The Sociology of Science in Its Place: Comment on Shapere

Stephen Turner
University of South Florida

Explanations explain differences, or contrasts. To ask a why question is to frame, implicitly, a contrast-space, a "why this rather than that" (or some set of "thats").¹ The dominant strategy of the sociology of science at the eve of the emergence of the "Strong Programme" was to construct contrast spaces by applying to science explanatory categories which had proven their value in the study of other aspects of social life, such as politics and "stratification." The extension was made possible by virtue of the manifest fact that scientists, *qua* members of the "scientific community," are often participants in institutions, and hold institutional positions where they must make decisions and take actions which have consequences for other scientists: they act as editors, as members of committees which review grant proposals, hire and tenure faculty, select recipients for scientific awards, elect officers of scientific organizations, participate in decisions about investments in research technology, and, of course, make decisions about what research to do—often on behalf of other scientists working in the same lab or unit.

Community and institutional practices were the easy cases, for actual contrasts fell readily to hand. There were various differences—national, historical, disciplinary, between institutional settings, and in decision-making practices, and even more conveniently, a great many differences in aggregate patterns, e.g., of citation, co-citation, and noncitation, which could be conveniently counted and correlated with familiar demographic variables, as well as with newly invented ones, such as "professional age."

There was, however, no natural extension of this strategy to the subject matter of scientific belief and practice proper. It is a *de facto* characteristic of both fully developed scientific theories and advanced scientific practices that they are—unlike religious or

political doctrines and practices—without a usable wide range of genuine competitors. At the present time, for example, there is no set of full-blown alternative theories of elementary particles. In science, the best theory becomes pretty unambiguously the best, so much so that, as Shapere says, it can be treated by later physicists as part of the given. Nor are there comparisons to be found outside of science. The person on the street has no elaborate opinions on the subject. Nothing that makes for a serious comparison may be found in the cosmologies of either Melanesians or Presbyterians. The problems which most scientific theories address are themselves "problems" only within the framework of traditions of a very limited community.

Creating Contrast-spaces

The evident implication of this is that, brave sloganeering about "the extension of sociological analysis to all forms of social activity" notwithstanding, the sociologist interested in scientific belief and practice is faced with a serious problem of constructing contrast spaces within which to apply a "sociological" explanation. Responses to this practical problem have taken several forms. Latour and Woolgar (1979, pp. 37,43) invited us to enter the laboratory in the frame of mind of an anthropologist entering a remote community: this was a distancing device, which gave them a newly seen set of facts, such as inscription practices (pp. 45-53), to describe. Pinch (1985) did in a very explicit way what other sociologists of science have done implicitly: took descriptions of a particular scientific episode as they would be given by philosophers of science, then reconstructed the episode using a different vocabulary which made no explanatory appeals to the philosopher's concepts—truth, rationality, success, and progress. Scientists' own accounts of their work, which also often make such appeals, or use such terms as "discovery," can be employed to produce similar contrasts. As Pickering puts it, we can "attempt to understand the process of scientific development, and the judgments entailed in it, in contemporary rather than retrospective terms" (1984, p. 8). In scientists' own accounts, the judgments

Author Address: Center for Interdisciplinary Studies in Culture and Society, University of South Florida, 140 Seventh Avenue South, St. Petersburg, Florida 33701 USA

An earlier version of these comments was read at the tenth annual meeting of the Society for Social Studies of Science, Troy, New York, October 26, 1985, as commentary on Dudley Shapere's "External and Internal Factors in the Development of Science," this issue, pp. 1-9.

of the scientist are "retrospectively legitimated by reference to the reality of theoretical entities and phenomena" (1984, p. 13).

The contrasts here are no longer actual contrasts, but rather contrasting descriptions. One might simply treat them as incarnations of the problem of many descriptions, and decline to suppose that there is any point to a "decision" between them.² However, much of the writing which employs these idioms has proceeded by making the naive essentialist claim that "this is the way science is, really." In general, as Shapere observes, these efforts do not seem persuasive. Shapere thus draws the moral that sociologists ought to get out of the business of offering overarching interpretations of science, and attend to cases. Where one can construct an interpretation of a given scientific episode in, e.g., "negotiation" terms, the episode can invariably be construed, with suitable adjustments, in the idiom of the other traditions. *Pace* Shapere, this indicates not that sociology of science should attend to cases more carefully but that these conflicts of interpretation cannot be decided on the basis of the historical record: the factual characterizations will simply reproduce the "overarching" disputes. If the claim of the sociology of science to a place in the explanation of the cognitive parts of science rested solely on these quasi-philosophical grounds, one might proceed against it by meta-philosophical arguments designed to show that the overarching claims behind various sociologies of science are incoherent or absurd. But it is questionable whether the claims of the sociology of science to a place other than that assigned to it by Shapere rest solely on these grounds.

Beliefs and Sub-beliefs

Pickering's use of the term "judgment" is suggestive. To the extent that sociologists take as a topic what Pickering calls judgments and research strategies (i.e., prospective bets on what in the fallible world of new techniques and new experimental results—and of hunches and general methodological prejudices—is to be expected to become the subject of scientific consensus), they are freed of the constraint of the *de facto* noncomparability which holds for the best scientific beliefs, and therefore freed from the necessity of merely comparing divergent, hotly contested, descriptions. Indeed, a whole world of actual contrasts opens up. Particular cognitive and practical skills, for example, *may* become constitutive of scientific competence in a given area and therefore become "universal." But before they do, they will be characteristic of particular local settings, particular kinds of personal biographies, or particular kinds of natural endowments. Judgments and expectations,

what I will call "sub-beliefs," will often vary between persons whose backgrounds vary, as will their decisions on how to invest their efforts.³

Scientists' expectations and strategies vary biographically, and the variations may result from familiar "external" facts, such as differences in organizational constraints, resources, and opportunities. The rational strategy for a scientist making a decision to pursue an idea, in the face of uncertainty, differ from that of scientists with other circumstances, endowments, costs, prior expectations and commitments, and so on. Thus these sub-beliefs escape the difficulty of noncomparability. Moreover, their role in the process of belief-formation in science threatens the traditional internal-external distinction itself, for they cannot be readily collapsed into either category. Even with Shapere's historicized version of the distinction the difficulty remains, and this has implications for both his historical conclusions and his methodological morals.

Shapere suggests that there is a "process of internalization" by which hypotheses, methods, and the like come to be taken as given. The existence of such a process, he further suggests, has had the historical effect of freeing science from external influences (p. 8). Thus he is ready to concede that in the old days, external influences—meaning "philosophical, political, economic, social, psychological" considerations—"shaped or even determined" "the hypotheses proposed, the problems conceived, the standards applied, and so forth" (p. 8).⁴ But he claims that this does not happen in modern physics, because these are no longer relevant "considerations" or "appeals" within physics. To deny this, as he puts it, is to "ignore the substance of the subject." If "internal" considerations are taken to be those which science has "internalized" in the sense of making them into givens, this thesis comes down to the claim that the *only* things that (nowadays) influence hypothesis selection, problem conceptualization, and methodological standard-setting are "obtained from" (p. 7) what has previously been established as "given."

One need not defend any ambitious overarching interpretation of science to find this claim to be implausible. Taking it in a naive, literal sense, it is simply false. A scientist (editor of a sub-series in one of the top ten physics journals) recently remarked to me that "today science is technology-driven." By this he meant that problems in his area arise from opportunities created by new platforms and new instrumentation—which in his area happen usually to be designed by engineers, not physicists, and are often constructed for nonscientific purposes, as for example the LORAN navigation system was. Does Shapere really mean to deny the independent effects of technology?⁵ Does he mean to deny that scientists'

expectations about the most promising lines of approach are influenced by the prospect of employing particular technologies? Does he mean to claim that there is some radical difference between the effects of this kind of "external" influence on scientists' expectations and the effects of those influences which arise biographically, such as possession of particular cognitive skills?

Sociological claims on these topics are distinctly less controversial than those which arise directly from the attempt to construct an overarching sociological interpretation of science that competes with scientists' self-descriptions or with the conventional philosophy of science. This raises a possibility. Even if one concedes that theories which are *de facto* taken to be the best theories, or have become part of the given, are the best, there nonetheless remains a large range of variation on purely internal grounds in *prospective* judgments and expectations. If these variations have any causal relevance to the cognitive development of science, then conceding the part of Shapere's argument that bears on overarching interpretations has little effect on the causal purport of the sociology of science. Making the weaker claim preserves most of the specific explanatory claims of the cognitive sociology of science.

A picture might help explain this. Prospectively, science proceeds in a way analogous to crossing a stream by going from rock to rock. We choose to step on the rocks that can give us a firm footing and from which we can move on to other rocks that give us a firm footing as well and which move us in the general direction of the other side. We might make wrong choices—get on a rock and discover that it cannot be made firm; or get on a rock and discover that, firm or not, we have no place to go forward. Our judgments determine the rocks we get on, the path we take, but that path could have been different. *Retrospectively*, when we have gone from most promising to most promising, we have gone from best to best. But our judgments about what was most promising at any given point could have been wrong. The rocks we disdained could have made our footing firmer and led to better subsequent rocks, or perhaps simply to a different series.

It is difficult for us to see that different decisions might have been made. Whewell formulated the general historiographic reasons for this difficulty in *Novum Organon Renovatum*.

The very essence of [scientific triumphs is that they lead us to regard the views we reject as not only false, but inconceivable. And hence we are led rather to look back upon the vanquished with contempt than upon the victors with gratitude. . . . We have a latent persuasion that we in their place should have been wiser and more clear-sighted;—that we should have

taken the right side, and given our assent at once to the truth.

Yet in reality, such a persuasion is a mere delusion. . . . How many ingenious men in the last century rejected the Newtonian Attraction as an impossible chimera! How many more, equally intelligent, have, in the same manner, in our own time, rejected, I do not now mean as false, but as inconceivable, the doctrine of Luminiferous Undulations! To err in this way is the lot, not only of men in general, but of men of great endowments and very sincere love of truth (1858, pp. 32-4).

Whewell's point applies *a fortiori* to judgments and other sub-beliefs. *Only* by failing to grant elementary interpretive charity to those whose ideas were not followed up, corrected, and improved can we persuade ourselves that the only reasonable judgments in science were those which *were* followed up, corrected, and improved.⁶

As Whewell understands, our retrospective standpoint typically places a shroud of unintelligibility over defeated ideas. On occasion, however, science develops in a way that enables us to see how things might have gone if other judgments and choices had been made. Current ideas in cosmology depend on determinations of the properties of fossil radiation, a topic which could have been studied more aggressively. As Jeremy Bernstein has recently suggested, this was a matter of expectations:

the most important reason was that the whole field of cosmology was not taken very seriously by the scientific community. It was one of those circular things. It was not taken seriously because of the lack of crucial data, and there was a lack of crucial data because it was not taken seriously (1985/86, p. 14).

This implies that what has been believed about the universe was different from what would have been believed had different choices been made—and again, this is the consideration relevant to Shapere's historicized notion of internalization.

Judgments of what is promising are often socially distributed. The Pearson-Bateson dispute, at least in the earliest stages, i.e., before experimental evidence of segregation had accumulated to the point that the other biometers began defecting, makes sense as a consequence of the distribution of statistical skills. What *was* successful in statistical genetics, and gave Pearson good reasons for thinking that this was the most promising direction for genetics as a whole, could only be understood by those with statistical expertise similar to his. In 1903, these were local skills, the product of a local tradition. Most biologists of the time, who did not share that tradition, also did not share his optimism, and invested their efforts differently. If Pearson's expect-

tations, rather than those of his opponents, had become the beneficiary of the investments of contemporary genetics, genetics would have continued to develop. But what would have been taken as *given*, or "internalized" in Shapere's sense, would have been very different from what is in the textbooks we all learned our genetics from.⁷

The "battles" of contemporary science are conducted in a highly organized community where decisions are made on the basis of scientists' judgments, which are organized in various ways. Judgments, expectations, and opinions about what ought to be done are sometimes socially variable. The potential causal relevance of this variation derives from the banal truth that theories won't become "best theories," in many areas of science, unless scientists make decisions which enable the theory to develop—decisions to create technological opportunities, decisions to invest in the people who have the skills, and so on. Shapere's historical thesis implies the diminishing relevance of "external" considerations, and therefore of the causal relevance of sociological facts. Only by persuading ourselves that science always makes optimal choices can we comfortably accept the claim that socially variable prospective judgments have little or no effect on what becomes part of the given at any given historical moment. There is precious little reason to believe anything of the sort.⁸

NOTES

1. This notion has been elaborated for the social sciences by Alan Garfinkel (1981, pp. 21-48. Cf. Putnam, 1978, pp. 41-5).
2. There is a long sociological tradition, founded on long philosophical traditions, which supports the idea that some of these descriptions are true in some special, essentialist, sense, and that the others can be either dismissed or sublated. Sometimes this has been taken—e.g., by Marxists battling "false consciousness"—as the goal of inquiry itself. The inspiration for this particular notion is to be found in Marx's philosophical tutor: similarly for the concepts of the sociological study of belief. Pragmatism, by way of Mead, is the source for the "symbolic interactionists" who invented the notion of negotiated order. Husserl was the source of Alfred Schutz's phenomenological sociology, the direct inspiration of the idea of "the social construction of reality," and of Garfinkel's ethnomethodology. It should surprise no one that redescription originating in these traditions reproduce the conflicts between essentialism and positivism.
3. By introducing this term I do not mean to suggest that any precise line may be drawn between a sub-belief (any of the many things that might lead to slightly stronger preference for one expectation over another) and a confirmed belief—something "internalized" in Shapere's sense. Indeed, the final point in this paper draws its forces from the notion that the choices that shape the contents of the cognitive side of scientific research depend on a mix of hunches, expectations, things taken as given, and so forth. The less determinate the line between these categories, the more difficult it is to escape the implications of the point.
4. Shapere advises sociologists to concern themselves with cases where extrascientific considerations . . . combine with internal ones to direct science, sometimes aiding, sometimes

conflicting with, the direction that would be taken if internal considerations alone were operative (p. 8).

This phrasing suggests that a unilinear image of the development of science, an image which would fit comfortably into the thought of a Buckle or Spencer, lies behind Shapere's advice: sociology is given the place of dealing with that which interferes with some supposed natural teleological development of science explicated by the philosophy of science proper.

5. In the case of many technological decisions, one needs little imagination to see that there are consequences stemming from what gets "internalized" in Shapere's historicized sense. The content of the textbook geophysics of oceans and atmospheres at this particular moment is different, in the most banal and unproblematic sense, from what it would have been had different political decisions about the space program been made in the sixties, and what it would have been without the LORAN system.

6. Perhaps unsurprisingly, one of the "sociological" analyses Shapere apparently accepts is one I have questioned on the grounds of interpretive uncharity (1981): Forman's analysis of German physicists' rejection of a causality in the Weimar-era (1971).

7. For the purposes of this argument, all that needs to be shown by this example is that both Pearson and his opponents rationally believed that their views held the promise of further development, or had reasonable grounds for doubting the promise of their opponents' approach. Only a drastic failure of interpretive charity would prevent this concession. Provine (1971) established the relevant point, that the biometric view was open to development. Whether the debate between Mendelians and biometers was a genuine case of incommensurability is a disputed issue (cf. Mackenzie and Barnes, 1979, Roll-Hansen, 1983), but this dispute is not strictly relevant to my point here.

8. Indeed, because of the omnipresence of highly organized decisional processes in contemporary science, these elements of the realm of decision, which cannot be reduced to Shapere's category of "internal" considerations, may be as significant today as they have ever been.

REFERENCES

- Bernstein, Jeremy. "The Birth of Modern Cosmology." *The American Scholar* 55(1985/86): 7-18.
- Forman, Paul. "Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment." *Historical Studies in the Physical Sciences* 3(1971):1ff.
- Garfinkel, Alan. *Forms of Explanation: Rethinking the Questions in Social Theory*. New Haven, Conn.: Yale University Press, 1981.
- Latour, Bruno and Steve Woolgar. *Laboratory Life: The Social Construction of Scientific Facts*. Beverly Hills: Sage, 1979.
- MacKenzie, D. and B. Barnes. "Scientific Judgement: The Biometry-Mendelism Controversy." In B. Barnes and S. Shapin, eds., *Natural Order*. Beverly Hills: Sage, 1979, 191-210.
- Pickering, Andrew. *Constructing Quarks*. Chicago: University of Chicago Press, 1984.
- Pinch, Trevor. "Theory Testing in Science." *Philosophy of the Social Sciences* 15(1985): 167-187.
- Provine, W. B. *The Origins of Theoretical Population Genetics*. Chicago: University of Chicago Press, 1971.
- Putnam, Hilary. *Meaning and the Moral Sciences*. London: Routledge & Kegan Paul, 1978.
- Roll-Hansen, Nils. "The Death of Spontaneous Generation and the Birth of the Gene: Two Case Studies of Relativism." *Social Studies of Science* 13(1983): 481-519.
- Turner, Stephen P. "Interpretive Charity, Durkheim, and the 'Strong Programme' in the Sociology of Science." *Philosophy of the Social Sciences* 11(1981): 231-243.
- Whewell, William. *Novum Organon Renovatum*. London: Parker and Son, 1958.

Replies to Carroll and Turner

Dudley Shapere
Wake Forest University

Positivism claimed that there are ultimate defining characteristics of science, and sought to discover them. Since its view was that those defining characteristics are purely "formal," devoid of any substantive claims about the way things are, their discussion could supposedly be carried on wholly independently of any consideration of the substantive content of science. From the beginning of my work in philosophy of science, I saw the body of positivistic views as suffering from several major faults. First, all attempts within that tradition to detail the defining characteristics of science, the so-called "metascientific concepts" like explanation, theory, and confirmation, had been failures. Further, the distinction between "theory" and "observation," which that tradition had made central to its interpretation of science, was untenable, at least as that distinction had been conceived by positivistic and traditional empiricist writers. There was a more fundamental objection still: for even if there were absolute defining characteristics of the scientific enterprise, how could their adoption be justified? The promised derivation from modern formal logic had not been forthcoming: a large variety of positivistic attempts failed to lay out how, in the most general sense, a scientific theory is an "interpreted" axiomatic system, the "interpretation-statements" themselves being characterizable in purely logical terms. Similar failures were to be found in attempts to characterize scientific explanation in terms of deduction from lawlike statements, especially in the light of the concomitant failure to show how scientific lawlike statements could be characterized in purely logical terms, *e.g.*, as universally quantified statements. My studies of the history of science, and especially of the new interpretations of that history by professional historians of science, convinced me that there was a still more profound error in the positivist-empiricist tradition, that of supposing the character of science to be laid down once and for all, independently of the content of any scientific belief.

The approaches of critics of positivism, like Kuhn and Feyerabend, seemed to me suggestive. Their historical approach was especially attractive insofar as it emphasized what I had already suspected might lie at the base of positivism's failure: that even the most general features of science were subject to change and indeed had changed over history. Also attractive was their insistence that presuppositions shaped not only the substantive views of nature which science held at a given epoch, but also the problems, the methods of dealing with those problems, the general characteristics expected of answers to those problems (the sorts of things that could count as theories and explanations), the nature and force of evidence, and even the goals of science at such epochs. Nevertheless, I found their views to be so filled with vagaries, confusions, and contradictions as to obscure rather than clarify the scientific enterprise.

Moreover, on any plausible interpretations of those views, the ultimate presuppositions on which the science of a particular community or tradition of science were alleged to rest, both in its substantive views and in its goals, methods, and standards, were themselves ultimately arbitrary. There was no provision for the possibility, even in principle, of determining, except by ultimately arbitrary fiat, that one set of such presuppositions was in any way superior to any other. In his commentary, Tom Carroll has recognized both the influence and at least part of the problem with such views: "In our zeal to free ourselves from positivist, linear, and naively cumulative notions of scientific change, we sometimes fail to stress sufficiently—or even to acknowledge—the size, degree of articulation, subtlety, systematic integration, and palpable explanatory power of the modern Western sciences." (Carroll, p. 10) The problem, however, is more than one of insufficient stress; for given the failure of positivist, linear, and naively cumulative notions of scientific change, with what notions can we possibly replace them which will do justice to these notable and undeniable features of modern science? The alternative which seemed to me worth investigating was one which, while abandoning the positivistic claim that there are unalterable

Author Address: Department of Philosophy, Wake Forest University, Drawer 7229, Reynolda Station, Winston-Salem, North Carolina 27109

defining characteristics of science, scientific absolutes independent of the content of scientific belief, would nevertheless show how science could, through its historical development, forge its conceptions of its goals, methods, and reasoning as products of its investigations themselves.

The views offered in the present paper have grown directly out of my efforts to resolve these issues. They thus do not represent, as Carroll suggests, significant differences from my earlier work. In offering a catalogue of such differences, he says, "I must list first, of course, his ready declaration that the traditional idealist [i.e., positivist-empiricist] view of scientific change 'is now a shambles.' As a historian, I am gratified to see him credit a 'torrent of historical studies' with indicating 'more and more convincingly that changes over the development of science have gone far deeper than mere change of theory.'" (Carroll, p. 10) That makes it sound as if I had begun as an adherent of the general positivistic viewpoint and was now finally admitting its failure, having progressively moved from that viewpoint to a more agreeable historicist one. But as my brief chronicle of the development of my thoughts on these matters should indicate, my statement that traditional positivistic empiricism is now a shambles is not a penitent confession of conversion, but a statement of what I believed was the case, and the problem, from the outset of my studies. In questioning the positivist assumption of universal and unalterable defining characteristics of science, I had from the beginning felt the necessity of accounting for the scientific enterprise in terms of a historical development without absolutes. We have had to learn not only about nature, but also how to learn about it and how to think about it: to learn what is involved in investigating, in raising problems, in obtaining evidence; to understand what it is to understand or explain. And we even had to learn that we had to learn all these things. That process of learning has been a product of history, not of logic, of pure reason.

Nor, in offering the views of the present paper, am I acting "more like a historian than was his older style." (Carroll, p. 11) On the contrary, the problems of the empiricist-positivist tradition as I saw them stemmed not only from the philosophical failure of that tradition, but also from what I found in the recently professionalized writings of historians of science. My earliest writings in the philosophy of science, depending heavily on analysis of historical cases, already bore the impact of such studies. It is important to note this, for Professor Carroll also finds "grounds for considering Shapere's account to be questionable history." (p. 12) In this connection I must emphasize that the historical cases sketched in this paper are summaries of long and detailed in-

vestigations. I have written in detail about some of them elsewhere, giving appropriate references to at least some of the extensive primary and secondary literature on which they are based.¹ And if there are questions about the cases like, "Can we be sure that, when those people used a term, they meant by it what we mean? Did all the translators since then get the meaning?" and so forth (Carroll, p. 12), these are questions that can be raised concerning *any* historical interpretation; and we *do* have standards and methods for dealing with such difficulties in concrete cases. At the very least, my interpretations rest on a thorough and critical examination of the best historical work now available.

But there is more behind Carroll's questioning of my historical interpretation than worries about the adequacy of the evidence for them. "Note what is lost in Shapere's synthesis: there are almost no people. Things just happen without the exercise of human choice, without the messy step of discrete bits of theory getting jumbled up in the cluttered heads of real flesh-and-blood humans." (Carroll, p. 12) But this is precisely the point. Let me explain.

Many positivists were right on at least one point, which they used to express by saying that confirmation and disconfirmation are relations between propositions, relations which are independent of the people who believe or state the propositions. If evidence E confirms hypothesis H, it confirms it independently of who says so, or even if no one says so. Their error, as I see it, lay in believing that there must be a "logic" of confirmation, of inductive reasoning, a system of formal rules of confirmation, which is independent of the substance of scientific belief. My paper keeps the core of their point, while avoiding their error, by maintaining the following: that *as a major product of its history, science has developed a body (I should really say bodies) of beliefs, methods, standards, and so forth that lay out interrelations between claims that are independent of the people who make them.* Those interrelations are not confined to those between evidence and hypothesis; they also include relations between accepted beliefs and hypotheses which are reasonable to propose in the light of those accepted beliefs. And we accept those background beliefs because they have shown themselves to be successful and free of specific and compelling doubt in dealing with their domains, domains which themselves have, in the light of other successful and doubt-free background beliefs, shown themselves to be appropriate ways of dividing nature up into areas for investigation.

The Big-Bang hypothesis was suggested by Hubble's observations of the recession of galaxies coupled with the Friedmann solution of the field equations of General Relativity, the latter having already by the 1920s and 1930s shown itself to be more

than just a promising theory, even if not yet fully successful and doubt-free. The relation of "suggestion" here either held or did not hold independently of the people making the suggestion. If the Big-Bang hypothesis seemed doubtful in the 1950s, that was because of contradictions in age and distance determinations which seemed to indicate that a Big-Bang universe would be younger than some of the objects in it; and such contradictions—themselves suggesting the possibility of an alternative cosmological theory—either held or did not hold independently of who noted their existence. And if the Big-Bang hypothesis was confirmed by the observation of the 3° microwave background radiation in the 1960s, that too was a relation which either held or did not hold independently of the people who made the discovery or asserted the relationship to hold.

Similarly, quantum mechanics is a theory which already by the 1930s had shown itself highly successful with regard to a number of domains (e.g., spectroscopy, the periodic table, chemical bonding) and relatively free of specific and compelling doubts as to its correctness (though some, like Einstein, advanced reasons for considering it incomplete). It was thus eminently suitable as a background theory in terms of which to construct further theories, for example of electromagnetism (Dirac, and later Feynman, Schwinger, and Tomonaga), radioactivity (Fermi), and the atomic nucleus (Yukawa). And the relation of "being a background theory in terms of which new theories, of new domains, could be constituted and proposed" was independent of the people who so used quantum mechanics.

There is thus an important sense in which speaking of the ideas, methods, beliefs, standards, and so forth in sophisticated science independently of the people who used them, is not a loss, as Carroll supposes. My use of the passive voice in certain contexts (e.g., "distinctions were found. . .") which so concerns Carroll is not an evasion of proper attribution, but rather a recognition that scientific ideas have a life of their own, with their own interrelations and importance. To ignore this status is to ignore the very stunning character of science, the explanatory power, the systematic organization, and the other features which Carroll himself has admonished us not to forget.

This is far from denying that science is done by people, or that scientists are sometimes led to advocate or to ignore certain scientific ideas and relationships by their own psychological or social circumstances. Nor does it deny that there are "national styles" and the like, or that "The rational strategy for a scientist making a decision to pursue an idea . . . is going to differ from that of scientists with other circumstances . . . and so on" (Turner, p. 16), though I think that the extent to which such

decisions differ is all too easy to exaggerate in many cases. Such factors can affect the direction of science deeply, and their study by historians and sociologists of science is absolutely necessary. Thus I agree with Stephen Turner's observation that it is "simply false" that "the *only* things that (nowadays) influence hypothesis selection, problem conceptualization, and methodological standard-setting are 'obtained from' . . . what has previously been established as 'given.'" (Turner, p. 16) But contrary to his impression, that is not my view. I have claimed that, while it is a measure of the degree of sophistication of an area of science that its body of background beliefs autonomously suggest or imply certain hypotheses, problems, methods, and so forth, those suggestions or implications are often affected by external considerations.

(I might add that I do not consider technology always to be "external" to science, as one of Turner's queries [Turner, p. 16] suggests. Indeed, while technological innovation was necessarily a matter of tinkering, of trial and error, in earlier days, the more sophisticated and powerful scientific theories have become, the more technological innovation comes to depend on it; in particular, the instruments that particle physicists or astronomers or molecular biologists employ, while their detailed design and construction require an engineering expertise, are now almost invariably proposed and constructed in the light of some scientific theory in the area in which they are to be used. To that very great extent, technology has itself been "internalized" into the disciplines in which it is employed, affecting the problems and strategies of that discipline as the latter affects it. And to that same extent, I must reject Turner's claim, phrased *via* a rhetorical question [Turner, p. 16], that there is no difference between the profound effect of technology on science and that of other "external" factors.)

But the importance of the external considerations noted by my commentators does not obliterate or lessen the importance of the impersonal interrelations that, through the process of internalization, have been evolved over the history of science and become its "internal" as opposed to any "external" considerations. Indeed, a central part of my argument in this paper has been that sociologists of science—and, I may add, historians of science also—can do their job only with a prior understanding of such internal considerations; for only if there are such considerations can there be something that a scientist or group of scientists can be said to be confused *about*, which can be said to have been distorted or envisioned clearly, or regarding which certain social, economic, or political factors can be said to have aided or hindered. (I certainly do not limit sociology only to "dealing with that which interferes" with science. [Turner, p. 18] Individual and social factors are certainly im-

portant; but the course of development in science is shaped by those factors *combined with* what the scientific considerations themselves indicate. That is, the "internal" considerations also can and often do serve as constraints on what happens.

There is no doubt that, as Carroll points out, historians of science have not been arriving at the interpretation of the history of science that I am advocating. Why not? Part of the reason, I would argue, is that they have simply not realized (or perhaps, in their enthusiasm for the "whole" picture of history, forgotten) that their own words require a recognition that there are internal factors. But there are other reasons also. One of the most important is the deep influence of views to the effect that science is after all inadequate to achieve the aims it claims to be achieving. Thus scientific evidence is today widely held to be inadequate to establish the contentions of science or to refute rival contentions, or, worse still, to be wholly impotent even to give any degree whatever of support to those contentions or to count in any way at all against their rivals. Or it is said that science has always made errors, or may have gone off on a wrong track somewhere in its history, and so cannot be trusted today; or, again, that arguments between scientists of very different scientific persuasions are "incommensurable," not rationally based or decidable at all. These and related theses, widely accepted since the 1960s, have been claimed to be both supportive of and supported by studies of the influence of "external" factors on science, and have lent credence to relativist and subjectivist views of science. (It is not at all irrelevant to point out that such views have frequently been appropriated by anti-scientific groups such as religious fundamentalists.) One source of their wide acceptance has been an understandable reaction against the excessive claims on behalf of science by positivism; and that reaction has perhaps prevented people from recognizing that (as I have often argued) the relativist-subjectivist reaction has been equally excessive, and that a view such as I have outlined is necessary if we are to do full justice to both the achievements and the fallibility of science.²

Some of Turner's points reflect the kind of relativism which I have been arguing. Thus he says that "When one can construct an interpretation of a given scientific episode in, e.g., 'negotiation' terms, the episode can invariably be construed, with suitable adjustments, in the idiom of the other traditions. *Pace* Shapere, this indicates not that sociology of science should attend to cases more carefully but that these conflicts of interpretation cannot be decided on the basis of the historical record." (Turner, p. 16) It may well be that there are several interpretations of any given historical case (though I cannot trust the philosophical argument that this

is invariably so). Nevertheless, some interpretations may be unacceptable in the light of the historical evidence, and of those that remain, some may be more illuminating than the others for some purposes. In any event, my point remains untouched by this suspect claim of Turner's: a good interpretation requires close attention to the case being interpreted; and a good case study should avoid procrustean distortion by interpreting the case in terms of an unquestioned antecedent "tradition." At the very least, the assumptions of the interpretation should be exposed so that they themselves can be examined critically. One denies the possibility of such criticism (which historians engage in all the time) only by adopting, on general philosophical grounds, an extreme and untenable relativism.

A further central point of my paper is that, as a matter of contingent historical fact rather than a requirement of logic, the internal considerations on which science relies increase in extent, depth, and power over the history of science, and have to a considerable extent narrowed the gap between scientific ambition and conclusion. Carroll finds reason to doubt whether that narrowing has in fact come about. To counter such doubts, we should remind ourselves again of the immense achievements of modern science. Today's physicists are closer than ever before to a unified picture of nature, a unified understanding of the fundamental forces and particles of nature in the context of an understanding of the origin of the universe.³ The broad picture of heredity and evolution, too, despite many open questions of detail (questions of incompleteness rather than of incorrectness), shows how much of scientific ambition has been achieved. In any case, with the increasing scope and power of internal considerations, scientific conclusions have come to be obtainable, more and more, independently of external ones. And this implies that what needs to be done by sociologists is to see how external forces affect, for better or for worse, what is indicated by the internal ones—what effects those external forces have on the conclusions (or, in other cases, the alternatives) indicated, suggested, or implied by the internal background beliefs of the science of a given epoch or field. One must not forget, either, to examine the effects of internal science on the external factors themselves.

None of this implies that those internal background considerations are not open to the possibility of criticism: they too might become subject to doubt and rejection. Nor does it imply that I am engaged in "the same old pursuit, namely, the identification of outright determinants of scientific change." (Carroll, p. 12) Sometimes the indications of the internal background considerations are clear and unambiguous, at least on some level of specificity; but often the

background suggests alternative possibilities which must be investigated and tested; sometimes there is ambiguity, and sometimes there is no clear indication at all. And again, even where the indications are clear and unambiguous, the indicated direction may turn out to have been a wrong one. Thus, in no way can it be said that, by attempting to identify "outright determinants of scientific change," I am arguing for "mechanistic causation of B by A" in scientific change.

Nor do I deny, as Turner seems to imply that I do, that "there remains a large range of variation in prospective judgments and expectations" (Turner, p. 17); there is often such a range even in our most sophisticated sciences, though sometimes the range is not all that broad. My view is far from the "unilinear image of the development of science" (Turner, p. 18) which Turner suspects it is; and since I also hold that the goals of science themselves are learned and altered in the course of the scientific enterprise, it is an error to say that I believe in "some supposed natural teleological development of science explicated by the philosophy of science proper." (Turner, p. 18) On my view, it is a matter of contingent fact that we have been able to get as far as we have in science. And though the question of "a sort of binary logic of correctness or incorrectness" (Carroll, p. 12) in science is a far more complex one than has been recognized (as I have discussed at length in forthcoming work ⁴), I hope my paper has shown that and how, in at least some circumstances in science, there can be stronger or weaker considerations, of an internal sort, in favor of or against some hypothesis as opposed to some others, and that the extent and power of such internal considerations has increased over the history of many areas of science.

NOTES

1. A few major secondary sources are given in Footnote 3 of my symposium paper (this issue, pp. 1-9)

2. In this connection, I must say that the anti-whiggism so praised by Carroll (p. 12) as a virtue of modern historians of science—their opposition to interpreting historical cases from a modern perspective rather than from that of the scientists in the historical situation concerned—is not an unmixed blessing. Too often it has been conceived as a purely relativistic doctrine: that when we understand the perspective of the scientist concerned, we see that that perspective was *wholly* defensible when viewed "in its own terms."

3. It is no rebuttal to say that physicists have thought that before: a close examination of the present situation shows specific ways in which it is very different from those past ones; there are manifestly solid reasons for the expectation now. Of course we might once again be wrong; but if that possibility is a *mere* possibility—what I call in the paper a universal doubt—then again I must say that the mere possibility of doubt arising is not itself a reason for doubt. This applies directly to Turner's concern that "it could be that our judgments about what was most promising at any given point were wrong, in the sense that on the rocks we disdained our footing could have been made firmer and led to better future rocks, or simply to different series of future rocks." (Turner, p. 17) Right: but that is a "universal doubt," applicable to any belief whatsoever. There are no guarantees on the view I present, though we can have good reasons.

There are some further points to consider. I think that, although there is something important in Turner's point, there are also significant ways in which the extent to which alternative possibilities are ignored has been exaggerated, at least for some important cases. In the case of the microwave background radiation which Turner mentions, there were plenty of reasons, even though not conclusive ones, for not looking into the matter earlier—there were, for example, quite good reasons for not taking cosmology seriously. And in any case, reasons did arise for reviving the background radiation hypothesis later.)

Having been a frequent critic of Kuhn's incommensurability thesis, I cannot see any justification for Turner's statement, as if it were an evident and undeniable fact, that there is a "*de facto* noncomparability which holds for the best scientific beliefs." (Turner, p. 16)

4. Especially in the as-yet unpublished "Reason and the Search for Truth."

Research Policies and Strategies of Five Industrial Nations, and Implications for the United States

Leonard L. Lederman, Rolf Lehming, and Jennifer S. Bond
National Science Foundation

Editor's note: From time to time, we intend to publish papers drawn directly from the practice of science policy analysis. We were pleased to invite the present contribution as a first instance. Discussion of the issues raised here is welcome in the form of letters to the editors or longer comments.

This article is drawn from a background paper prepared for the Government-University-Industry Research Roundtable. This body was convened under the auspices of the National Academy of Sciences, with membership from the scientific community, industry, and government, to examine the organization and conduct of research in the United States in light of current and future needs of, and demands on, science. To help in this exercise, the Roundtable has considered what aspects of the research policies and strategies of other advanced industrial nations might be relevant for United States science.

The article examines similarities and differences among the research systems, strategies, and policies of France, Japan, Sweden, the United Kingdom, and West Germany, and the United States; their government research funding systems and coordination mechanisms; efforts to assess the effectiveness and results of research; some research outputs; and dissatisfactions currently being expressed in most or all of these countries. The paper concludes by posing some questions for consideration in science policy discussions in the United States. A comparative table shows selected data on the distribution of funding, performance, type of research and development (R&D) personnel.

General Similarities and Differences

The research systems of the six nations have some broadly similar features. Governments provide the

bulk of academic research support and distribute it through a mix of agencies whose central function is the support of science, and through mission agencies which support science as part of their responsibilities in health, education, agriculture, and the like. All governments have some mechanisms for negotiating a balance between governmental and scientific interests in setting research directions and priorities. In all nations, academia conducts most fundamental research and trains future scientists and engineers; industry performs most applied research. All governments provide big science facilities for use by academic scientists and engineers, and also maintain government laboratories. (2,3,4,9)

The European nations and Japan share some features that distinguish their research systems from that of the United States. The governments of these nations have traditionally supported higher education and academic research as a means of transmitting and enhancing the national culture. Therefore, national, government supported university systems are the norm, and private universities and colleges are rare. In all nations but the United States and Japan, all qualified students have a right to higher education, pay no or low tuition costs, and often can receive stipends or subsidized loans. Nonetheless, a smaller proportion of the European college-aged population participates in higher education than is the case in the United States and Japan. In both Europe and Japan, higher education is less tightly coupled to research training than in the United States. (1,2,4)

European and Japanese governments support academic research through general, formula based operating funds that recipient universities distribute among their various functions, including research. This general support pays for academic salaries, research facilities, major equipment, and some support services. It is supplemented by competitive awards for investigator initiated work, and by funding for strategically targeted research. Under this dual support mechanism, questions regarding the allocation of research time and the proper accounting of indirect research costs are less prominent than in the United States. (1,2,4)

Author Addresses: National Science Foundation, Washington, DC 20550 USA.

The views presented are the authors' and do not necessarily reflect the position of the Foundation. A number of persons have contributed to this paper by guiding and reviewing drafts. The authors gratefully acknowledge these inputs and take full responsibility for any remaining deficiencies. A longer version of this paper appears in the April, 1986, issue of *Science and Public Policy*.

The faculty members of European and Japanese institutions of higher education are lifetime civil servants subject to a uniform salary structure. Their salaries are paid from general operating funds and are not affected by, or paid from, project or program support. Compared to the United States, the mobility of academic researchers—and those in other sectors—is low. The governments regard the level of mobility as an impediment to the transfer of research results and techniques, and several have started providing incentives to encourage greater movement between sectors and into strategically targeted R&D areas. (1,2,4)

In practice, the various dual support systems for academic research are being modified by several developments. Demands for costly instrumentation are forcing a shift of some research costs from general academic operating funds to research program budgets. Moreover, overall growth of academic research budgets has been curtailed as the European nations have placed greater reliance on targeted research and strategic research initiatives. (1,2,4)

In contrast to the United States reliance on shortterm project support, the European and Japanese governments tend to support research groups, programs, or laboratories for periods from 3-5 or even 10 years. They also make greater use of block grants, senior investigator awards, and special collaborative efforts. (2,3,4)

In some nations, notably France and West Germany, much of what in the United States would be academic science is conducted in systems of institutions that are allied with, but administratively independent of, the universities (e.g., CNRS Laboratories, Max Planck Institutes). (2,4)

The importance of mission agencies in funding research is especially pronounced in the United States. A major difference between the United States and the other nations, except the United Kingdom, is the large proportion of United States government R&D resources allocated to defense, now over 75 percent. However, when development expenses are subtracted, defense related research represents only about 22 percent of total federal research support and is exceeded by government research support for health. (2,4)

In Europe and Japan, a broad consensus exists for the principle of government support for research targeted on general economic development objectives. In contrast, the United States government funds industrial research primarily in support of mission agency purposes. The European nations and Japan are targeting national research efforts on specific commercial areas that governments consider important for future economic development, such as electronics, biotechnology, materials research, and informatics. In these endeavors, governments regard

the existence of strong industry-university relationships as a key to success. While these ties have traditionally been weaker than in the United States, all governments, including that of the United States, have formulated policies intended to strengthen them. The proportion of direct government funding of industrial R&D is quite variable, ranging from 2 percent in Japan to about 30 percent for the United Kingdom and the United States. (1,2,4)

The European countries and Japan expend a greater portion of central government R&D funds on international cooperative research than does the United States. The size of the United States science establishment permits economies of scale that have made possible the operation of national big science facilities for United States scientists, and a basic research effort that is of significant magnitude across all S&E fields. (2,9,10)

The appendix provides selected comparative data on the distribution of funding, performance, type of R&D, and science and engineering personnel for all six nations. Although the focus of this paper is on research, comparable data separating research from development in these countries are very limited. Thus, total R&D is shown to round out the picture. It should be pointed out that the research portion (basic and applied) is lowest in the United States (37 percent) and highest in France (54 percent).

Coordinating Mechanisms

We now turn to brief discussions of the government research funding systems of specific nations, their coordinating mechanisms, efforts to assess the effectiveness and results of research, and some research outputs.

There is general agreement that the United States R&D system and organization are at the pluralistic and less centralized end of the spectrum, the French at the more planned, centralized, and strategically targeted end of the spectrum, and the other nations somewhere in between. The United States system is seen as more market oriented, flexible, and is thought to adjust more easily to changing demands than those of the other nations, which are more stable overall and tend to provide longer term research support. (1,5,6)

In the United Kingdom, the bulk of research funding is provided by the central government. Major performers are the universities, government laboratories, and private industry. Academic research is supported largely by the central government through block grants from the University Grants Council (funded by the Department of

Education and Science), and competitively awarded research grants from five autonomous Research Councils. The Research Councils also operate government supported laboratories that are accessible to university scientists. The Department of Trade and Industry, with cost sharing by industry, supports research activities with potential benefits for industry. The government is also stimulating joint efforts of industry, academe, and government in manufacturing and information technology, optoelectronics, and other specially targeted areas. (2,3,4)

No strong central coordinating body exists in the United Kingdom's science system. A decade ago, offices of Chief Scientists were created in major government agencies. The Cabinet Chief Science Advisor's office produces an annual review of government-funded R&D and coordinates major government initiatives. An Advisory Board for the Research Councils (ABRC) advises the Secretary of Education and Science on allocation of research funds; efforts are underway to increase its role vis-a-vis the Councils in resource allocation decisions. Major advisory groups include the Advisory Council on Applied Research and Development (ACARD), which directs attention to areas of emerging commercial importance; the Department of Industry Requirements Board, and similar bodies in the mission agencies, which identify and recommend support of R&D linked to industrial needs; and the National Research and Development Corporation, a quasi-governmental body, which identifies and promotes inventions with industrial applications. (2,4)

In Sweden, the government performs little in-house research, only a few mission agencies maintain their own laboratories, and there are no national laboratories. The national universities are the main performers of research and house large laboratories. Government resources for academic research are provided in the form of general operating funds from the education ministry; supplemental competitive project and program support flows through three Research Councils. The National Swedish Board for Technical Development provides funding for applied academic research and applied research and development in industry. (3)

The Swedish government's Council for Planning and Coordination of Research coordinates the research of three Councils that support fundamental research. The government periodically formulates comprehensive science policy plans which are enacted by the Parliament (e.g., 1984 Research Policy Bill). It has undertaken two major initiatives: encouraging more industrially relevant research by universities; and directly providing funds and other support to industry through the National Swedish Board for Technical Development (STU), which also

supports applied academic research. (3)

In Japan, most government academic research support is provided by the Ministry of Education, Science, and Culture (Monbusho), principally to national universities and their affiliated laboratories. Other major funders are the Science and Technology Agency (STA), the Ministry for International Trade and Industry (MITI), and several other mission agencies. A key STA effort supports interdisciplinary teams of scientists from industry, academe, and government in a program of breakthrough science. MITI carries out industrially relevant R&D in its own laboratories; promotes privately supported research institutes that carry out nonproprietary R&D in certain product areas (e.g., semi-conductors, new synthetics); undertakes focused R&D initiatives; and provides low interest loans. Industry in Japan is more important than elsewhere as both a supporter and performer of research. (1,4)

Japan's fundamental coordinating mechanism is cultural; it rests on the setting of national policy through an emerging consensus judgment. The most important formal coordinating body is the Council for Science and Technology in the Prime Minister's Office, composed of educators, industrial managers, scientists, and engineers. Special councils are formed periodically to assess progress in different fields and recommend priorities. Agencies use advisory councils and industry associations to ensure consonance of government research with industry interests, and they plan and conduct research in cooperation with industry. MITI's National Project System targets national priority areas, with R&D mostly funded and conducted by industry. Efforts are under way to strengthen the weak university-industry linkage. (2,4)

The French national government provides a larger share of national R&D funds than the other governments. About a third of central government funds are disbursed by the National Center for Scientific Research (CNRS) under the Ministry of Research and Technology. CNRS laboratories are the main locations for the conduct of academic research, and most of these laboratories are located on higher education campuses but administratively separate organizations. Research funds are also provided by the mission agencies, for example, CNES (space), CEA (atomic energy), ANVAR (industrial), INSERM (health and medicine), IFREMER (oceans and fisheries), INRA (agriculture), and AFME (renewable energy). (2,3,4)

French government research priorities are stated in 5-year R&D plans. The Ministry for Research and Technology oversees resource allocations for civilian R&D agencies and advises on research programs of nationalized firms. Coordination may be facilitated by longer term block support, joint operation of

big science laboratories by mission agencies, and government involvement in industrial R&D through subsidiaries or direct participation. CNRS (as well as other research agencies) has a Committee for Industrial Relations that strives for closer articulation of CNRS work and the R&D needs of French industry.(2,4)

In West Germany, there is extensive joint funding by federal and Laender (state) governments, with federal funds predominating for major national laboratories, and Laender funds for regional applied research institutes. The Research and Technology Ministry (BMFT) provides the bulk of federal funds, with major additional resources coming from the ministries of defense, economics, and education and science. Several legally autonomous societies, jointly funded by federal and Laender governments, allocate government science funds: the German Research Society (DFG) to academia, the Max Planck Society (MPG) for in-house research and to its associated basic research institutes, and the Fraunhofer Society, half of whose funds come from industry, to its applied research institutes. (2,3,4)

West Germany achieves a degree of coordination of basic research without direct government control by supporting the DFG and the MPG. For applied research and work in the national laboratories, the Association of Big Science Establishments (AGF) participates in setting research directions. A Science Council advises government, DFG, and MPG. No formal government-industry coordinating bodies exist, but the Ministry for Research and Technology (BMFT) and the Science Council plays a coordinating role. Federal, Laender, and industry priorities are reflected in the applied research of the jointly funded Fraunhofer institutes. Industry and government collaborate in selected national laboratory research activities.(2,4)

Research Evaluation

All nations make some attempts to assess the effectiveness and quality of their research systems, and many are trying to increase their evaluation efforts. In principle, research assessments can be carried out at all levels. In practice, assessments of the performance of national science systems are rare. More frequently, attention is given to determining the quality of science in a given discipline, or sometimes in groups of institutions or broad fields of science or application.(2,3,4)

Assessments rely largely on peer review, but all countries are seeking a broader range of inputs and are examining the usefulness of bibliometric techniques. All nations have found that peer review assessments work better for disciplinary research

than for cross-disciplinary work, programs with fundamental and applied components, or comparisons across areas of science. Some evidence suggests that the United States devotes more effort to determining the prospective quality of research, based on proposal peer review, while other nations tend to focus on assessing progress and outputs. This may reflect other nations' greater reliance on longer term support to entire institutions or research programs. (2,3,4)

Perhaps the most ambitious assessment efforts exist in Sweden. Peer evaluations span basic and applied research and cover programs, research teams, institutions, and fields of science. Their focus is on specific fields of science and application. Results are available to all and feed into decisions about R&D strategies, research directions, and support levels. Comparison is made with other countries, and foreign scientists and engineers play a key role as members of the peer review teams. (3)

Research Outputs

Only rough comparisons can be made of the nations' research outputs. The number of publications by a nation's scientists in various fields can serve as a measure of the quantity of output. For all fields combined, the United States accounts for about 35 percent of the publications included in the *Science Citation Index* set of S&T journals, although the share varies by field. The United States share exceeds the other nations' combined total, with Japan, France, France, and the United Kingdom contributing between 5 and 9 percent each, Sweden less than that, reflecting its smaller science establishment. (7,10)

A rough impression of output quality can be derived by examining whether the share of references to that nation's published output falls short of, approximates, or exceeds its share of publications. In each major discipline, the United States share of references received exceeds the United States share of publications, with substantial variation among disciplines. In a number of specialties (e.g., physical chemistry, solid state catalysis, organic synthetic chemistry) the United Kingdom, West Germany, or Japan appear to hold leadership positions. (10)

The United States awards more first university degrees in the natural sciences than the five other nations combined (over 100,000 in 1983). Data on advanced degrees are broadly consistent with data on undergraduate degrees. In the United States, West Germany, and the United Kingdom, first degree graduates in the natural sciences exceed those in engineering. In contrast, engineering graduates exceed those in the natural sciences by a considerable margin in Japan and Sweden. Engineering degrees are

a smaller portion of total first degrees in the United States than is the case for the other countries. Japan graduates a larger number of first-degree holders in engineering than does the United States, but the United States graduates more doctorate-level engineers than does Japan. Better than half of the recipients of United States engineering doctorates are foreigners. (7)

Dissatisfactions

All six nations are attempting to improve their international competitive position. The following briefly lists some dissatisfactions currently being expressed in most or all of the other countries. Expressed inadequacies include:

- too little cooperation between academia and industry in research, education, and exchange of information; (3,6)
- insufficient flexibility and responsiveness of academic and government research institutions; (3)
- insufficient transfer of research skills and information due to low mobility of researchers; (3,6)
- aging faculty and few positions available for young researchers; (2,3)
- inadequate coupling of academic research with teaching, resulting in graduates ill equipped to operate near state-of-the-art; (2,3) and
- variable quality and relevance of government laboratories' work. (3,6)

To address these perceived shortcomings, the European and Japanese governments have taken a number of steps. All are targeting certain research areas based on anticipated future economic returns and international competitive potential, including biotechnology, electronics and computers, informatics, materials, production technology, and environmental research. Most are emphasizing a user orientation in government supported (especially applied) research. (3,5,6)

In the United Kingdom, West Germany, and Sweden, real growth in academic science budgets has been reduced or stopped in favor of strategically targeted programs. In contrast, the Japanese central government intends to increase basic research support. It is also providing incentives for increased university-industry-government laboratory cooperation, and greater mobility of research personnel from one sector to another. In France and Germany, modest programs are under way to provide academic positions for younger researchers. All governments are seeking increased funding for industrial R&D, especially in smaller enterprises, through direct and indirect mechanisms. Governments also are giving more attention to assessing the quality of their nation's research in specific science and application

areas, relative to that of other countries.(2,3,4,6)

Lessons for the United States

Given United States needs and problems, no straightforward lessons can be drawn for United States science from the other nations' experiences. While a number of countries appear to have brought aspects of their policies and strategies a little closer to those of the United States, the elements of individual national science systems form a whole, fit into a particular national context, and are not necessarily transferable. Since little objective information exists that could reveal what works better or worse under given circumstances, the advantages and disadvantages of different policies and strategies are not clear. Positive and negative consequences of a particular option frequently depend on how it is implemented.

The following list of questions should be considered with these cautions in mind.

- Should the United States move toward longer term, programmatic science support? This is one of the most striking differences between the United States and the other nations. (2,3,4)
- Should the United States more directly support graduate students in fields with strongly growing demand which exceeds supply? Project grants are the main federal support vehicle for United States S&E graduate students; the other governments use stipends and subsidized loans. (3,5,6)
- Should alternative mechanisms be considered for supporting academic research facilities and equipment? Governments elsewhere treat research facilities and equipment as a capital investment; in the United States, they are paid for through project support, special programs, or indirect research costs. This may be an especially difficult issue for the United States, because of the prominence of federal mission agencies in the research support system. (2,4)
- Should the United States provide greater government support for nonproprietary research addressing industrial needs, especially in areas of increasing international technological competitiveness? European and Japanese governments undertake such actions based on a national consensus; in the United States, no agreement exists on the issue. (5,6)
- Should the United States expand international cooperative research, especially where facilities and equipment costs are high? This would require careful consideration of advantages and disadvantages to the United States of various forms of cooperation (bilateral, multilateral) in particular science fields. (2,4)
- Should the United States move toward greater centralization or coordination of government re-

search? Recent proposals have included establishment of a Cabinet-level Department of Science and Technology, a Department of Science, a National Technology Foundation, a National Institute for Research and Advanced Studies, and a National Applied Science Administration. Centralization is not synonymous with coordination or quality, and no good evidence can be drawn from the experiences of the countries examined to support the greater efficacy of more centralized vs. more pluralistic systems. (1,2,4)

REFERENCES

1. Jarlath Ronayne, *Science in Government* (London: Edward Arnold, 1984). Covers: U.S., Canada, U.K., FRG, Belgium, Japan, Australia.
2. Bruce L.R. Smith and Charles Kidd, "Research in Europe and the United States," in *The 5-Year Outlook on Science and Technology 1981*, Volume 1, Source Materials (Washington, DC: National Science Foundation, 1981), 255-284. Covers U.K., France, FRG.
3. Leonard L. Lederman, "Recent Changes in Science and Technology Policy, Priorities, Practices on Programs in France, Federal Republic of Germany, The Netherlands, Sweden and United Kingdom," unpublished paper based on interviews and documentary sources, National Science Foundation, January 31, 1985.
4. C. Ailes et al., "Performer Organizations and Support Strategies for Fundamental Research in Six Countries," SRI International, April 1985. Prepared for the National Science Foundation, Division of Policy Research and Analysis. Covers basic research for U.S., FRG, U.K., France, Japan, USSR.
5. John Irvine and Ben R. Martin, *Foresight in Science, Picking the Winners* (London: Francis Pinter, 1984) Covers basic and strategic research; France, FRG, U.S., Japan.
6. *Science and Technology Policy Outlook, 1985* (Paris: Organization for Economic Cooperation and Development, 1985).
7. *International Science and Technology Data Update, January 1985* (Washington, DC: National Science Foundation, Directorate for Scientific, Technological and International Affairs, Division of Science Resources Studies).
8. *Recent Results-Selected S&T Indicators 1979-1984*, OECD Science and Technology Indicators-Basic Statistical Series (Paris: Organization for Economic Cooperation and Development, 1984).
9. *Science and Technology Indicators—Resources Devoted to R&D*, (Paris: Organization for Economic Cooperation and Development, 1984).
10. *Science Indicators 1982*, (Washington, DC: National Science Board, 1984), 1-37.

SELECTED COMPARATIVE DATA

Data is for a year in the 1980-1984 period depending on country and item.

	U.S.	Japan	FRG	France	U.K.	Sweden ¹
4.1. Total R&D by Source of Funds	100%	100%	100%	100%	100%	100%
Industry	50	64	55	42	42	57
Government ²	47	26	41 ³	57	49	40
Other	3	10	3	1	8	3
4.2. Total R&D by Performer	100%	100%	100%	100%	100%	100%
Industry	72	62	70	57	61	67
Government ²	13	10	14 ³	25	22	7
Higher Education	12	24	16	16 ^{3a}	14	27
Private Nonprofit	3	4	—	1	3	—
4.3. Total R&D by Character	100%	100% ¹	100%	100%	100% ¹	100%
Basic Research	13	14	22	21	12	23
Applied Research	24	25	78	33	25	17
Development	64	61	—	46	63	60
4.4. Research Expenditures by Performer	100%	100%	100%	100%	100% ¹	100%
Industry	51	47	n.a.	35	39	24
Higher Education	29	37	n.a.	29 ³	28	64
Government ²	14	12	n.a.	34	30	12
Nonprofit	6	4	n.a.	2	2	—
4.5. Basic Research Expenditures by Performer	100%	100%	100%	100%	100% ¹	100%
Higher Education	57	61	60	67 ²	55	88
Industry	19	26	18	9	13	8
Government ²	16	11	22 ³	22	30	4
Private Nonprofit	8	2	1	3	2	—

	U.S.	Japan	FRG	France	U.K.	Sweden ¹
4.6. <i>Government² R&D Funding by Objective</i>	100%	100%	100%	100%	100%	100%
Defense	64 ⁴	2 (5)	10 (15)	33 (40)	49 (59)	22
Advancement of Knowledge ⁵	4	46 (4)	44 (14)	27 (15)	21 (13)	36
Space	6	6 (12)	4 (9)	5 (9)	2 (5)	8
Energy	7	17 (26)	15 (21)	8 (8)	5 (7)	8
Health	11	6 (6)	3 (9)	4 (5)	4 (2)	6
Industrial Growth	0.3	7 (12)	12 (12)	12 (8)	7 (4)	4
Agriculture	2	11 (25)	2 (3)	4 (4)	5 (5)	2
All Other	6	5 (10)	9 (17)	8 (11)	7 (5)	13
4.7. <i>Industrial R&D Expenditures¹ by Industry</i>	100%	100%	100%	100%	100%	100%
Electrical Equipment	22	25	24	24	31	23
Machinery & Computers	13	10	14	8	11	13
Chemicals/Allied Products	12	17	22	15	16	10
Motor Vehicles	9	14	14	11	5	21
Aerospace	22	—	6	18	20	2
Instruments	7	3	2	1	2	31
All Other	16	31	18	22	15	
4.8. <i>Government² Percent of Industrial R&D Expenditures</i>						
Electrical Equipment	37%	1%	13%	25%	46%	11%
Machinery & Computers	14	1	10	5	10	8
Chemicals/Allied Products	6	1	4	4	1	3
Motor Vehicles	11	0	3	1	4	19
Aerospace	75	1	76	68	68	7
Instruments	15	0	11	14	4	
4.9. <i>Scientists & Engineers Engaged in R&D by Sector</i>	100%	100% ¹	100%	100%		100%
Industry	74	59	60	41	n.a.	57
Higher Education	13	32	24	38	n.a.	34
Government	9	7	15	18	n.a.	9
Private Nonprofit	4	2	1	2	n.a.	—
4.10. <i>First University Degree by Field</i>	100%	100%	100%		100%	100%
Natural Sciences ⁶	10	3	16	n.a.	25	4
Engineering	7	19	14	n.a.	16	11
Agriculture	2	4	4	n.a.	2	1
Social Sciences	11	41	29	n.a.	29	10
All Other	71	33	35	n.a.	29	74
4.11. <i>Doctoral Degrees by Field</i>	100%	100%	100%	100%	100%	100% ⁷
Natural Sciences ⁶	35	12	20	42	42	24
Engineering	9	19	8	12	18	13
Agriculture	3	8	3	8	3	2
Social Science	19	1	10	16		23
All Other (includes MD)	35	60	59	30	37	38

¹Natural sciences and engineering only; all other figures include social science and humanities; the U.S. excludes humanities.

²In the U.S., the government sector is federal only; in other countries, government includes all levels. Data in parentheses exclude general university funds and are thus more comparable to distribution of U.S. separately budgeted Government supported R&D.

³In France, the CNRS R&D is classified as higher education in performance data but as government source of funds data; in FRG the Max Planck Institutes are classified as government.

⁴1983; 66% in 1984, estimates are 68% in 1985 and 73% in 1986.

⁵General purpose research; including an estimated portion of general university funds (except U.S.).

⁶Includes Mathematics and Computer Scientists/Specialists

⁷Includes Master Degrees

⁸Included in Natural Sciences

—Sources for all data are OECD and NSF and country data.

—Figures may not add to totals due to rounding.

Editorial Statement-In-Preparation

Some editorial statements declare intentions and establish the rules of the game; others grope toward comprehensiveness. In the process, they leave the door open to features, contributors, and journal policy. We are opting for the latter. In this and other issues of *S&TS* this year, we move toward a publication that is part scholarly journal, part newsletter, part newspaper-like features, and part to-be-announced. In this way, we declare our interest in what the membership (a) has to offer and (b) would like to see.

In this, our premier issue, we inaugurate a column by the 4S President and a series of descriptions of science policy shops. We hope these short communications will generate letters from readers. There is much that such dialogue can accomplish which long essays and research reports cannot.

In the coming issues, we will present a preliminary policy for reviewing unsolicited manuscripts. The names of contributing editors and editorial advisors will also appear. Their diversity is the Society's

strength. *S&TS* will provide a forum for asserting that strength.

As editors we invite you to inform us about our successes and our deficiencies. But beware that a comment will probably lead to an invitation that you write something. We will press into action all who care enough to make a contribution, in their own ways and (for the most part) at their own pace, to the communities that 4S represents. We have much to reveal to one another—not just as 4S officers, members of Council, or workers on standing Society committees, but also in *Science & Technology Studies*. Society offices are indispensable organizational roles, but secondary to our activities as researchers, teachers, authors, administrators, etc. Those are the activities—the talents—we must bring to bear in these pages. Until we learn about what you have to contribute, our editorial statement will remain “in preparation.” In the meantime, talk to us.

DEC/SEC

MEETINGS CALENDAR

29-30 May 1986. Philadelphia, PA. International Association for Impact Assessment. 1986 Annual Meeting. “Enhancing the Impact of Impact Assessment.”

12-26 July 1986. Oxford. Oxford Science Studies Seminar. “Futures for Science”/“Science and Belief.”

14-16 July 1986. Oxford. British Society for the History of Science, Conference on “The Patronage of Science Since the Seventeenth Century.”

18-23 August 1986. New Delhi. International Sociological Association. XIth World Congress of Sociology.

24-28 August 1986. Veszprem, Hungary. INTERSTUDY and Hungarina Academy of Sciences. Fourth International Conference on Interdisciplinary Research.

28-31 August 1986. Edinburgh. Institute for Advanced Studies in the Humanities, Project Scottish Enlightenment. Conference on “Science and the Enlightenment in Europe.”

30 August - 3 September. New York City. American Sociological Association Annual Meeting.

17-19 September 1986. London. The Royal Institution. A RICHST Symposium, “Laboratories: The Place of Experiment.”

29 September - 4 October 1986. Florence. IUHPS Scientific Instrument Commission. VIth International Symposium on Scientific Instruments.

23-26 October 1986. Pittsburgh, PA. Society for Social Studies of Science (in conjunction with the History of Science Society, the Philosophy of Science Association, and the Society for the History of Technology). Annual Meeting.

1 November 1986. London. British Psychological Society and British Society for the History of Science. Conference on “The Psychology of Science.”

Science & Technology Studies welcomes information about forthcoming events of interest to its readership.

Editors' Introduction: An abiding research and teaching interest among many 4S members is science policy: who makes it, who contributes, and how scholarship affects the process. In an effort to bridge the gap between research community and policymakers, this column will offer "snapshots" of various policy shops, in North America and elsewhere. Included in each profile will be a description of the shop's purpose, its location in the government it serves, and the names of persons who may be contacted directly for further information. This month's correspondent is Evan Berman, a doctoral candidate in the Science, Technology, and Public Policy Program, George Washington University, Washington, DC, 20052.

Science Policy Research Division, Congressional Research Service

SPRD is one of seven research divisions within the Congressional Research Service (CRS). With a staff of 40, SPRD is one of the world's largest research centers for science policy. It serves the members of the U.S. Congress and Committee staff on subjects where science and technology are of foremost concern, including organizational issues relevant to science itself. Two other units within CRS also deal with science issues: the Environment and Natural Resources Division (encompassing agriculture, food, energy supply, forestry, etc.), and the Congressional Reference Division, which is responsible for responding to short-term questions (e.g., budget, health problems).

To serve the needs of Congress, SPRD projects assume many forms. They range from ten-minute phone calls, to 10 page memos and services related to organizing hearings, to full analytic reports and "issue briefs." Issue briefs are maintained on topics of current interest and are updated on a monthly basis. At present, SPRD maintains some 30 issue briefs on such topics as Computer Services and Crime, the Strategic Defense Initiative, Genetic Engineering, Technology Transfer, and Science Policy in the Reagan Administration. Analytic reports are often published as committee prints, acknowledging CRS authorship.

CRS does not provide a public information service (unlike, for example, the Office of Technology Assessment); it also has no mailing list for the routine dissemination of its work. The public can secure SPRD products through the relevant congressional committees or from their congressional representatives.

However, Christopher Hill, senior specialist in science & technology policy (Library of Congress, Washington, DC 20540 USA; phone 202/287-7042), and Richard Rowberg, chief of SPRD (287-7040), are happy to help anyone find a specialist in the fields of interest covered at SPRD.

SPRD is divided into four sections: life sciences; geosciences, materials, and industrial technology; energy, aerospace, and transportation; and policy, information, and behavioral sciences. Some current topical areas are Biotechnology, Health Research, Food Research, Oceanography, Weather, Materials Research, Strategic Defense Initiative, Alternative Energy, Space & Aeronautics, Peer Review, Science & National Security, and Integrity of Science. The staff working in these areas have diverse backgrounds: half hold a graduate degree in a natural or engineering science; a quarter have a bachelor's degree plus work experience in science issues; and the rest, according to Hill, contribute because they are "smart, do good research, and know how to reach out." In addition, SPRD has two senior specialists who have "flexible, professor-like" job activities.

The mode of operation at SPRD includes "networking and outreach" to the relevant communities. Outside consultants are seldom hired (only 2-3 percent of the budget is spent this way). Similarly, SPRD has no funds for student internships, though a small number of students can be hosted, with desk space and supervision provided, if course credit is arranged and alternative funding guaranteed in advance. Professors, too, are welcome to spend a sabbatical at SPRD: "almost anything of interest to a 4S member is probably of interest to somebody here."

The agenda for work through 1986 at SPRD, as at all Federal agencies, is somewhat unpredictable. First, SPRD expects to be called upon by Congress to analyze the impact of Gramm-Rudman-Hollings on specific programs, especially those that are heavily cut. Second, with the Science Policy Task Force completing its work this spring, a Technology Policy Review exercise might be initiated. Third, a number of hot issues, e.g., SDI, the Superconducting Super Collider, and the Space Station are likely to demand further research from SPRD. Finally, SPRD is mindful that "there are the usual, unforeseeable surprises."

Evan Berman
Correspondent

Israel in 1988?

No-one has yet bothered to write a job description for 4S President. (Don't all rush!) The file just descends on to your lap, and you have to make it up as you go along. At least, by this time next year, we will have a "President Elect" in place, and s/he can engage in a little anticipatory socialization. Meanwhile, my manifesto commits me to attempting to engage the Society's members more directly in its affairs. The Editors have given me this page. I want to use it to invite your comments on some of the issues which cross my in-tray. Your responses will help guide our decisions. And I hope that this modest experiment will help us all to arrive at our Annual Meeting in Pittsburgh already briefed on the issues before us—and so help the Society to meet its challenges and realize its potential. Nice rhetoric.

Well, then, what's new(s)? Here are some items to mull over:—

1. The referendum on the Council's proposal to increase the membership dues is not (by our Charter) *formally* binding, but its results will carry great weight. Without *some* increase, the Society cannot even tread water comfortably—quite apart from expanding. So there are difficult decisions ahead.
2. The 1987 Annual Meeting will be at Worcester, Massachusetts. We will be meeting later than usual—probably 13-15 November. Comments on this change will be welcome. The nightly accommodation charge at the Meeting HQ Hotel is likely to be around \$70. If this seems excessive, squeal now!
3. The 1988 Annual Meeting will be another "away fixture," four years after the successful Ghent Meeting. Israel has been suggested as a possible venue. As to dates, nothing is yet fixed, but we would like to maximize the chances of members being able to attend. Again, comments welcome—especially from Australasia and other far-flung Dominions of the 4S Empire.
4. Any takers for the 1989 Meeting?
5. The Bernal Prize was originally intended, by Gene Garfield's generosity, as an award to an outstanding "4S" scholar who is, essentially, coming to the end of her/his career. But, as many members have pointed out, the number of people who are "*Bernalabile*" is now rather limited. The "younger generation" still have many active years ahead of them! So it has been proposed that we might change the terms of the award—to, for instance, an outstanding book or paper. Comments, please. We must preserve the distinction of this Prize, since it is the Society's major accolade.
6. We need someone to Chair a Fundraising Committee. Who would like to start the queue? Or make a nomination?
7. Reflecting on the programs of recent Meetings (and the content of the *4S Review*), I wonder if anyone can spot "gaps" in our coverage of (a) "hot" topics, or (b) members' interests? Suggestions welcome. And what further activities might the Society initiate, to better realize its aims, and satisfy members' needs? Sponsorship of smaller, specialized and/or regional conferences or workshops? New publications (e.g., monographs or bibliographies)? Ditto.
8. At Troy, Al Teich agreed to maintain a watch, for 4S, of 4S/SSS/STS/HPS groups in US colleges who are currently under pressure from financial cutbacks. This will help 4S take appropriate action to help those under threat. If you have relevant information, write Al at: AAAS, 1333 H Street NW, Washington, DC 20005.

That's enough for one issue. Sorry not to give you Inspiring Intellectual Leadership: you elected the wrong person! But I am happy to serve the Society for two years, as best I can. Help me to help you by writing to me at: 25 Gilmour Road, Edinburgh EH16 5NS, Scotland, UK (Tel. 031-667.3497). I'll warn the Post Office.

David Edge

4S Update

The charter amendments and dues increase both passed. There were 133 votes in favor of an increase and 38 votes opposed.

To use the new dues most effectively, the editors of *S&TS* are exploring economical ways of giving the Society a first-class journal. For instance, our new typesetting process is almost paying for itself through savings in printing and postage costs.

Books

Randall Albury. *The Politics of Objectivity*. Albury proposes an alternative solution to the problem of scientific objectivity and its relation to the pursuit of social objectives. An appendix by Ditta Bartels addresses the possibility of public participation in the control and direction of scientific development. 72 pages, paper. Available from Deakin University Press, Victoria 3217, Australia.

Daryl E. Chubin, Alan L. Porter, Frederick A. Rossini, and Terry Connolly, eds. *Interdisciplinary Analysis and Research*. The editors have scoured the literature for ideas and writing from science, engineering, social science, and the humanities to answer these questions: What is interdisciplinary analysis and research? Why is it important? Who are the participants? Where is the institutional setting? What is the outcome? How is it performed and managed? 439 pages plus annotated bibliography, cloth. Available from Lomond Publications, P.O. Box 88, Mt. Airy, MD 21771 USA. 800-443-6299.

Wesley Shrum. *Organized Technology: Networks and Innovation in Technical Systems*. Examines the interaction of government agencies, national laboratories, private firms, universities, regulatory agencies, Congress, and public-interest groups in the technology development process. Published in Science and Society: A Purdue University Press Series in Science, Technology, and Human Values. 300 pages, paper, \$4.50. Available from Purdue University Press, South Campus Courts-D, West Lafayette, Indiana 47907 USA.

Jeffrey L. Sturchio, ed. *Corporate History and the Chemical Industries: A Resource Guide*. Historiography of the chemical process industries, with bibliographies and

practical essays on business archives and oral history in the corporate setting. 53 pages, \$3.00 post paid. Available from Publications, Center for the History of Chemistry, 215 South 34th Street, Philadelphia, PA 19104-6310 USA.

Stephen P. Turner. *The Search for a Methodology of Social Science: Durkheim, Weber, and the Nineteenth Century Problem of Cause, Probability, and Action*. Boston Studies in the Philosophy of Science 92. Traces the conflict between nomic ideals of explanatory form and the statistical practices of the social sciences from its origins in the methodological disputes and statistical literature of the early nineteenth century. 248 pages, cloth, Dfl. 105/US\$ 39.50. Available from D. Reidel Publishing Company, P.O. Box 17, 3300 AA Dordrecht, The Netherlands or Kluwer Academic Publishers, 190 Old Derby Street, Hingham, MA 02043 USA.

New Additions

The Society for Literature and Science (SLS) was formally launched last summer. Major interests include studying the relationships between science and literature and countering the notion that scientists and artists operate in separate and unrelated worlds. Direct membership inquiries to David Porush, SLS Secretary-Treasurer, Language, Literature and Communication, Rensselaer Polytechnic Institute, Troy, NY 12181 USA.

Project Appraisal: Ways, means and experiences. A quarterly journal on how to decide, in advance, on the merits of proposed projects, be they large in money terms, or small but of great possible impact. The journal is interdisciplinary, and aimed at natural scientists, social scientists, administrators, and others with an interest in appraising projects. Editors: Vary Coates (J.F. Coates, Inc., Washington DC USA) and John Weiss (Project Planning Centre for Developing Countries, University of Bradford, England). Annual subscription: British 55 or US\$96. Contact William Page, Beech Tree Publishing, 10 Watford Close, Guildford, Surrey GU1 2EP, England.

Science & Technology Studies welcomes news items of interest to its members. Deadline for the next issue is July 1, 1986.

About the Authors

JENNIFER BOND is the Senior Staff Associate for Science and Technology Resources Studies, Division of Science Resources Studies, National Science Foundation. Her previous experience with international science policy issues includes the preparation of the International Chapter of *Science Indicators* reports, and work at the Massachusetts Institute of Technology, the World Bank, the National Academy of Sciences, and the Brazilian Research Institute.

P. THOMAS CARROLL is assistant professor of history in the Department of Science and Technology Studies, Rensselaer Polytechnic Institute. While on leave in 1985-1986, he is an Exxon Research Fellow at the Program in Science, Technology, and Society, Massachusetts Institute of Technology. He is author (with Thackray, Sturchio, and Bud) of *Chemistry in America, 1876-1976: Historical Indicators* (1985). His current research concerns the building of graduate chemistry and chemical engineering programs in the United States before World War II and the careers of doctorates trained in those disciplines.

LEONARD LEDERMAN is currently Senior Staff Associate for Strategic Planning and Assessment of the National Science Foundation's Directorate for Scientific, Technological, and International Affairs. His previous work at Batelle resulted in a number of publications and the book entitled *Federal Funding and*

National Priorities, An Analysis of Programs, Expenditures, and Research and Development (1971).

ROLF LEHMING is a policy analyst in the Division of Policy Research and Analysis of the National Science Foundation. In that capacity, he examines questions concerning the status, health, and international position of U.S. academic science and engineering. Recent projects include geographic distribution of research funds and scientific instrumentation.

DUDLEY SHAPER is Z. Smith Reynolds Professor of Philosophy and History of Science at Wake Forest University. His views on the rationale of scientific development are presented in *Reason and the Search for Knowledge* (1984), and are carried further in a forthcoming book, *Observation and Knowledge*.

STEPHEN TURNER is Professor of Sociology, courtesy Professor of Philosophy, and Director of the Center for Interdisciplinary Studies in Culture and Society at the University of South Florida. His works include *Sociological Explanation as Translation* (1980), *Max Weber and the Dispute over Reason and Value* (1984), and *The Search for a Methodology of Social Science* (1986). He is currently studying the bureaucratic politics of patronage in nineteenth century survey geology in the United States.

Information for Contributors

Science & Technology Studies is a multidisciplinary journal which publishes research, commentary, and reviews. As the official journal of the Society for Social Studies of Science, it seeks to foster exchange and communication among a variety of individuals and groups concerned with the development and dynamics of science and technology, including their relationships with politics and society.

Contributors should submit five copies of their manuscripts. If a blind review is desired, all of the author(s) should be removed on three of the copies. Contributions will be reviewed for interest to a multidisciplinary audience concerned with science and technology, for communication with that audience, and

for quality of data and dependability of information. In reference format, the journal follows the *Chicago Manual of Style*; recent issues should be consulted for examples.

Manuscripts may be submitted to either co-editor:

Daryl E. Chubin
School of Social Sciences
Georgia Institute of Technology
Atlanta, Georgia 30332 USA
(404) 894-6846

Susan E. Cozzens
Room 1229
National Science Foundation
Washington, DC 20550 USA
(202) 357-7826

These guidelines will be elaborated in a future issue.

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

NON-PROFIT ORG.
US POSTAGE
PAID
COLLEGE STATION
TEXAS 77843
PERMIT NO. 215