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The Sociology of Science in Its Place: Comment on Shapere

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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 useably 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 reconstrued 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

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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 expec-

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.

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