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COMMENTARY

Frontier Science and Textbook Science

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Shapere, Carroll, and Turner[1] agree on some important aspects of science: that the reliability and coherence of modern science are striking, and that external as well as internal factors influence science. But the degree of agreement could have been more evident had the distinction been recognized explicitly between well-established science and science-in-themaking.

At the frontiers of science, (almost) anything goes. Working scientists display degrees of competence that range over the human spectrum and therefore---if for no other reason---what they do is influenced by many things that are not norms or ideals of science. Individuals, groups, and institutions seek to have science serve their ideological ends, and they lobby among their peers toward acceptance of some part or corollary of their particular sets of beliefs. Even were there the ideal scientist, he would still make mistakes when trying to do or to understand something quite new, and his mistakes would tend to be in the direction of his wishes---his choice of hypotheses would be influenced by what he regards as desirable, and he would tend to see the data that support his ideas and to miss those that conflict. Science-in-the-making, or frontier science, is heavily subject to external factors.

But science-in-the-making is not all there is to science. As Polanyi[2] and Ziman[3] in particular have cogently argued, there is a "republic of science" in which theories and data and paradigms are subjected to mutual criticism; a thing becomes an actual part of science only when consensually accepted by the appropriate scientific community or sub-community.

The requirement of consensuality inevitably filters out some of the external influences---those stemming from the idiosyncracies of the people who have carried out the work, since those idiosyncracies are not likely to be shared by all the referees, editors, and other critics to whose judgment the work is subject. Only consensually accepted work is eventually incorporated into textbooks; and it is the coherence and reliability not of frontier, but of *textbook* science that we find striking. Textbook science generates few arguments, whereas matters on the frontiers are almost invariably matters for argumentation.

Shapere, of course, was talking chiefly about textbook science. For example, when he says, "the situation in modern science is radically different from what it was in early periods" because of the progress made in "learning how to learn about Nature," we can only agree---provided we are concerned with those parts of modern science that pertain to well-developed disciplines or parts of disciplines. In not-so-well-established specialties, we are still very much in the process of learning how to learn; even in those parts of physics that have to do with gravity waves, say, or with magnetic monopoles.

Again, on the matter of "real flesh-and-blood humans," Shapere is clearly concerned with those parts of science that have already run and survived a considerable gauntlet of competition and criticism; this means that ideology and wishfulness have been largely filtered out to leave thingss about which wide agreement is possible.

By contrast, Carroll is talking chiefly about frontier science, about science-in-the-making, when he draws attention to the fact that scientists are flesh-and-blood humans; and so is Turner when he speaks of *"prospective* judgments and expectations"---research *strategies* (emphasis added) influenced by individual sub-beliefs. In talking about cosmology, Turner chooses a subject that will always remain frontier science; and in talking about stream-crossing, Turner focuses only on the path across that happens to be taken, which is again science-in-the-making (even when viewed in retrospect, be it noted). It is the solidity of the other bank, when reached, that constitutes textbook science. There may be many ways across the stream, but there are only two banks, and neither is influenced by the different ways across that different people take.

Thus much of the apparent disagreement among Shapere, Carroll, and Turner results from their implicit concentration on different aspects of science: Shapere focuses more on textbook science, Carroll and Turner on frontier science. This is not, of course, a sharp distinction of matters of kind: frontier and textbook science are the extremes of a continuum. What Shapere says is merely more true toward one end than toward the other, just as what Carroll and Turner say is more true toward the other end than toward the first; sharp distinctions can rarely, if ever, be made in science studies (in contrast to within science, see below). Nevertheless, I suggest that the distinction between frontier science, science-in-the-making, and

textbook science, well-established and widely accepted, is a significant one. That distinction is not the same as (but overlaps with) the distinction between the contexts of discovery and of justification. It is not the same as that between normal and revolutionary science---much science-in-the-making is perfectly normal science. Nor is all contemporaneous science necessarily frontier science; many scientists practice textbook science, seeking to apply or to refine or to amplify, not to generate new or wider understanding (again, of course, a matter of degrees and not either-or). And "textbook" science is not necessarily correct or true, of course, though it is much more likely than frontier science not to be untrue.[4]

Shapere's description of the piecemeal approach and its success, and corollaries of that, I found very useful; so, too, is Turner's concise illustration of the work of sociologists and their need when looking at science to *create* contrast-spaces. It may be that the piecemeal approach, successful within science, cannot be so successful in science studies. The striking successes of science have come in fields where distinct categories could be discovered and used; science studies deals with matters of degree and not of kind: the continuum of influencing factors, internal to external; a continuum of normal to revolutionary bits of science; disciplines and sub-disciplines that span the range of young to mature; the variability I have discussed above, frontier to textbook science; and so forth. Chemists (say) have the luxury of dealing with a finite number of discrete elements, and a very small number of forces that can rationalize all the interactions of atoms and of molecules; moreover, it turns out that the magnitudes, the values, of most properties can be calculated by simply additive means. But students of the activity of science cannot do anything analogous. For example, we cannot aim to evolve a formula by which the degree of external as opposed to internal influence on a bit of science can be estimated from knowledge of where into the structure of scientific knowledge that bit fits, and when it was discovered, in what country, by man or woman, in a large or a small laboratory, a well-known or an obscure one, by atheist or believer . . . and so on. Yet we have to admit that those and many other factors probably do influence the degree to which external factors played a role in the particular discovery.

In one sense, then, Shapere's call for a piecemeal approach in science studies is very well taken. Surely we know enough about science to recognize that sweeping statements about the whole of science are unlikely to be widely accepted, let alone to be true. Indeed, the burden of this comment has been to suggest that a piecemeal approach, differentiating between frontier science and textbook science, would have made the exchange among Shapere, Carroll, and Turner more immediately productive.

At the same time, in applying such piecemeal distinctions, we need to remember that the distinctions are not inherently sharp ones. It can hardly be productive, then, to argue on the one hand for the decisiveness (say) of external factors in science, and on the other hand for the decisiveness of internal ones, when the degree of influence varies for different bits of different sorts of science. Rather, the task is to elucidate increasingly the mix of factors that might tend (and only *tend)* to strengthen the effects of external in contrast to internal factors, though that mix of factors cannot be expressed in a meaningfully additive way. It must be a process of continually adding and refining nuances, and defining more and more clearly under what other conditions any given factor is most likely to express itself strongly.

Much discussion has consisted not of attempts to refine or to add, but flatly to contradict sweeping statements with other equally sweeping ones. For example, Kuhn's distinction between normal and revolutionary science immediately rang true for many practicing scientists, as did his notion of paradigm. Naturally both the distinction and the concept needed refinement, the adding of nuances and qualifications; yet much of the criticism, especially at first, seemed to be attempts to argue in sweeping terms against the very distinction itself and against the very possibility of defining rigorously and usefully the concepts underlying Kuhn's uses of "paradigm." In other words, the critics seized on what might be wrong rather than on what might be right---or the difference between destructive and constructive criticism. Science studies needs to build understanding through the cumulation of nuances and qualifications to distinctions that can never be true in more than qualified ways; and it needs to build by a piecemeal approach---at least until someone has shown how human beings can come to understand a complex matter through some other approach. In such a process, arguments over sweeping generalities are unlikely to take us much further---as indeed Shapere, Carroll, and Turner have all agreed.

NOTES

1. *Science & Technology Studies* 4 (1986): 1-23.

2. Michael Polanyi, "The Republic of Science," *Minerva* 1 (1962): 54-73.

3. John Ziman, *Reliable Knowledge* (London, New York, & Melbourne: Cambridge University Press, 1978).

4. Errors in textbook science are discovered periodically; and textbook science typically ignores anomalies or lacunae for which no immediate possibility of solution is seen. On the ignoring of certain advances, see Gunther S. Stent, "Prematurity and Uniqueness in Scientific Discovery," *Scientific American,* 1972 (December): 84-93; for an example of the prolonged use of a "constant" that is not constant, see Henry H. Bauer, "The Electrochemical Transfer-Coefficient," *Journal of Electroanalytical Chemistry* 16 (1968): 419-432.