

T H E R O A D S I N C E
S T R U C T U R E

*Philosophical
Essays, 1970–1993,
with an
Autobiographical
Interview*

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The Natural and the Human Sciences

"The Natural and the Human Sciences" was a prepared contribution to a panel discussion at LaSalle University, sponsored by the Greater Philadelphia Philosophy Consortium, on February 11, 1989. (Charles Taylor was to have been on the panel as well, but had to withdraw at the last minute.) It was published in The Interpretive Turn: Philosophy, Science, Culture, edited by David R. Hiley, James F. Bohman, and Richard Shusterman (Ithaca: Cornell University Press, 1991). Used by permission of Cornell University Press.

LET ME BEGIN WITH a fragment of autobiography. Forty years ago, when I first began to develop heterodox ideas about the nature of natural science, especially physical science, I came upon a few pieces of the Continental literature on the methodology of social science. In particular, if memory serves, I read a couple of Max Weber's methodological essays, then recently translated by Talcott Parsons and Edward Shils, as well as some relevant chapters from Ernst Cassirer's *Essay on Man*. What I found in them thrilled and encouraged me. These eminent authors were describing the social sciences in ways that closely paralleled the sort of description I hoped to provide for the physical sciences. Perhaps I really was onto something worthwhile.

My euphoria was, however, regularly damped by the closing paragraphs of these discussions, which reminded readers that their analyses applied only to the *Geisteswissenschaften*, the social sciences. "*Die Naturwissenschaften*," their authors loudly proclaimed, "*sind ganz anders*" ("The natural sciences are entirely different"). What then followed was

a relatively standard, quasi-positivist, empiricist account of natural science, just the image that I hoped to set aside.

Under those circumstances, I promptly returned to my own knitting, the materials for which were the physical sciences in which I had taken my Ph.D. Then and now, my acquaintance with the social sciences was extremely limited. My present topic—the relation of the natural and human sciences—is not one I have thought a great deal about, nor do I have the background to do so. Nevertheless, though maintaining my distance from the social sciences, I've from time to time encountered other papers to which I reacted as I had to Weber's and Cassirer's. Brilliant, penetrating essays on the social or human sciences, they seemed to me, but papers that apparently needed to define their position by using as foil an image of the natural sciences to which I remain deeply opposed. One such essay supplies the reason for my presence here.

That paper is Charles Taylor's "Interpretation and the Sciences of Man."¹ For me it's a special favorite: I've read it often, learned a great deal from it, and used it regularly in my teaching. As a result, I took particular pleasure in the opportunity to participate with its author in an NEH Summer Institute on Interpretation held during the summer of 1988. The two of us had not had the opportunity to talk together before, but we quickly started a spirited dialogue, and we undertook to continue it before this panel. As I planned my introductory contribution, I was confident of a lively and fruitful exchange to follow. Professor Taylor's forced withdrawal has been correspondingly disappointing, but by the time it occurred, it was too late for a radical change of plans. Though I'm reluctant to talk about Professor Taylor behind his back, I've had no alternative but to play a role close to the one for which I was originally cast.



To avoid confusion, I shall start by locating what Taylor and I, in our discussions at the 1988 institute, primarily differed about. It was not the question whether the human and natural sciences were of the same kind. He insisted they were not, and I, though a bit of an agnostic, was inclined to agree. But we did differ, often sharply, about how the line

1. C. Taylor, "Interpretation and the Sciences of Man," in *Philosophy and the Human Sciences* (Cambridge: Cambridge University Press, 1985).

between the two enterprises might be drawn. I did not think his way would do at all. But my notions of how to replace it—about which I shall later have just a bit to say—remained extremely vague and uncertain.

To make our difference more concrete, let me start from a too simple version of what most of you know. For Taylor, human actions constitute a text written in behavioral characters. To understand the actions, recover the meaning of the behavior, requires hermeneutic interpretation, and the interpretation appropriate to a particular piece of behavior will, Taylor emphasizes, differ systematically from culture to culture, sometimes even from individual to individual. It is this characteristic—the intentionality of behavior—that, in Taylor's view, distinguishes the study of human actions from that of natural phenomena. Early in the classic paper to which I previously referred, he says, for example, that even objects like rock patterns and snow crystals, though they have a coherent pattern, have no meaning, nothing that they express. And later in the same essay he insists that the heavens are the same for all cultures, say, for the Japanese and for us. Nothing like hermeneutic interpretation, Taylor insists, is required to study objects like these. If they can properly be said to have meaning, those meanings are the same for all. They are, as he has more recently put it, absolute, independent of interpretation by human subjects.

That viewpoint seems to me mistaken. To suggest why, I shall also use the example of the heavens, which, as it happens, I had used also in the set of manuscript lectures that provided my primary text at the 1988 institute. It is not, perhaps, the most conclusive example, but it is surely the least complex and thus the most suitable for brief presentation. I did not and cannot compare our heavens with those of the Japanese, but I did and will here insist that ours are different from the ancient Greeks'. More particularly, I want to emphasize that we and the Greeks divided the population of the heavens into different kinds, different categories of things. Our celestial taxonomies are systematically distinct. For the Greeks, heavenly objects divided into three categories: stars, planets, and meteors. We have categories with those names, but what the Greeks put into theirs was very different from what we put into ours. The sun and moon went into the same category as Jupiter, Mars, Mercury, Saturn, and Venus. For them these bodies were like each other, and unlike members of the categories 'star' and 'meteor'. On the other hand, they placed the Milky Way, which for us is populated by

stars, in the same category as the rainbow, rings round the moon, shooting stars and other meteors. There are other similar classificatory differences. Things like each other in one system were unlike in the other. Since Greek antiquity, the taxonomy of the heavens, the patterns of celestial similarity and difference, have systematically changed.

Many of you will, I know, wish to join Charles Taylor in telling me that these are merely differences in beliefs about objects that themselves remained the same for the Greeks as for us—something that could be shown, for example, by getting observers to point at them or to describe their relative positions. This is not the place for me to try very seriously to talk you out of that plausible position. But given more time, I would certainly make the attempt, and I want here to indicate what the structure of my argument would be.

It would begin with some points about which Charles Taylor and I agree. Concepts—whether of the natural or social world—are the possession of communities (cultures or subcultures). At any given time they are largely shared by members of the community, and their transmission from generation to generation (sometimes with changes) plays a key role in the process by which the community accredits new members. What I take “sharing a concept” to be must here remain mysterious, but I am at one with Taylor in vehemently rejecting a long-standard view. To have grasped a concept—of planets or stars, on the one hand, of equity or negotiation, on the other—is not to have internalized a set of features that provide necessary and sufficient conditions for the concept’s application. Though anyone who understands a concept must know *some* salient features of the objects or situations that fall under it, those features may vary from individual to individual, and no one of them need be shared to permit the concept’s proper application. Two people could, that is, share a concept without sharing a single belief about the feature or features of the objects or situations to which it applied. I don’t suppose that often occurs, but in principle it could.

This much is largely common ground for Taylor and me. We part company, however, when he insists that, though social concepts shape the world to which they are applied, concepts of the natural world do not. For him but not for me, the heavens are culture-independent. To make that point, he would, I believe, emphasize that an American or European can, for example, point out planets or stars to a Japanese but cannot do the same for equity or negotiation. I would counter that one can point only to individual exemplifications of a concept—to this star

or that planet, this episode of negotiation or that of equity—and that the difficulties involved in doing so are of the same nature in the natural and social worlds.

For the social world Taylor has himself supplied the arguments. For the natural world the basic arguments are supplied by David Wiggins in, among other places, *Sameness and Substance*.² To point usefully, informatively, to a particular planet or star, one must be able to point to it more than once, to pick out the same individual object again. And this one cannot do unless one has already grasped the sortal concept under which the individual falls. Hesperus and Phosphorus are the same *planet*, but it is only under that description, only as planets, that they can be recognized as one and the same. Until identity can be made out, there is nothing to be learned (or taught) by pointing. As in the case of equity or negotiation, neither the presentation nor the study of examples can begin until the concept of the object to be exemplified or studied is available. And what makes it available, whether in the natural or the social sciences, is a culture, within which it is transmitted by exemplification, sometimes in altered form, from one generation to the next.

I do, in short, really believe some—though by no means all—of the nonsense attributed to me. The heavens of the Greeks were irreducibly different from ours. The nature of the difference is the same as that Taylor so brilliantly describes between the social practices of different cultures. In both cases the difference is rooted in conceptual vocabulary. In neither can it be bridged by description in a brute data, behavioral vocabulary. And in the absence of a brute data vocabulary, any attempt to describe one set of practices in the conceptual vocabulary, the meaning system, used to express the other, can only do violence. That does not mean that one cannot, with sufficient patience and effort, discover the categories of another culture or of an earlier stage of one's own. But it does indicate that discovery is required and that hermeneutic interpretation—whether by the anthropologist or the historian—is how such discovering is done. No more in the natural than in the human sciences is there some neutral, culture-independent, set of categories within which the population—whether of objects or of actions—can be described.

Most of you will long since have recognized these theses as redevelopments of themes to be found in my *Structure of Scientific Revolutions* and related writings. Letting a single example serve for all, the gap that

2. D. Wiggins, *Sameness and Substance* (Cambridge, MA: Harvard University Press, 1980).

I have here described as separating the Greek heavens from our own is the sort that could only have resulted from what I earlier called a scientific revolution. The violence and misrepresentation consequent on describing their heavens in the conceptual vocabulary required to describe our own is an example of what I then called incommensurability. And the shock generated by substituting their conceptual spectacles for our own is the one I ascribed, however inadequately, to their living in a different world. Where the social world of another culture is at issue, we have learned, against our own deep-seated ethnocentric resistance, to take shock for granted. We can, and in my view must, learn to do the same for their natural worlds.



What does all of this, supposing it cogent, have to tell us about the natural and human sciences? Does it indicate that they are alike except perhaps in their degree of maturity? Certainly it reopens that possibility, but it need not force that conclusion. My disagreement with Taylor was not, I remind you, about the existence of a line between natural and human sciences, but rather about the way in which that line may be drawn. Though the classic way to draw it is unavailable to those who take the viewpoint developed here, another way to draw the line emerges clearly. What I'm uncertain about is not whether differences exist, but whether they are principled or merely a consequence of the relative states of development of the two sets of fields.

Let me therefore conclude these reflections with a few tentative remarks about this alternate way of line-drawing. My argument has so far been that the natural sciences of any period are grounded in a set of concepts that the current generation of practitioners inherit from their immediate predecessors. That set of concepts is a historical product, embedded in the culture to which current practitioners are initiated by training, and it is accessible to nonmembers only through the hermeneutic techniques by which historians and anthropologists come to understand other modes of thought. Sometimes I have spoken of it as the hermeneutic basis for the science of a particular period, and you may note that it bears a considerable resemblance to one of the senses of what I once called a paradigm. Though I seldom use that term these days, having totally lost control of it, I shall for brevity sometimes use it here.

If one adopts the viewpoint I've been describing toward the natural

sciences, it is striking that what their practitioners mostly do, given a paradigm or hermeneutic basis, is not ordinarily hermeneutic. Rather, they put to use the paradigm received from their teachers in an endeavor I've spoken of as normal science, an enterprise that attempts to solve puzzles like those of improving and extending the match between theory and experiment at the advancing forefront of the field. The social sciences, on the other hand—at least for scholars like Taylor, for whose view I have the deepest respect—appear to be hermeneutic, interpretive, through and through. Very little of what goes on in them at all resembles the normal puzzle-solving research of the natural sciences. Their aim is, or should be in Taylor's view, to understand behavior, not to discover the laws, if any, that govern it. That difference has a converse that seems to me equally striking. In the natural sciences the practice of research does occasionally produce new paradigms, new ways of understanding nature, of reading its texts. But the people responsible for those changes were not looking for them. The reinterpretation that resulted from their work was involuntary, often the work of the next generation. The people responsible typically failed to recognize the nature of what they had done. Contrast that pattern with the one normal to Taylor's social sciences. In the latter, new and deeper interpretations are the recognized object of the game.

The natural sciences, therefore, though they may require what I have called a hermeneutic base, are not themselves hermeneutic enterprises. The human sciences, on the other hand, often are, and they may have no alternative. Even if that's right, however, one may still reasonably ask whether they are restricted to the hermeneutic, to interpretation. Isn't it possible that here and there, over time, an increasing number of specialties will find paradigms that can support normal, puzzle-solving research?

About the answer to that question, I am totally uncertain. But I shall venture two remarks, pointing in opposite directions. First, I'm aware of no principle that bars the possibility that one or another part of some human science might find a paradigm capable of supporting normal, puzzle-solving research. And the likelihood of that transition's occurring is for me increased by a strong sense of *déjà vu*. Much of what is ordinarily said to argue the impossibility of puzzle-solving research in the human sciences was said two centuries ago to bar the possibility of a science of chemistry and was repeated a century later to show the impossibility of a science of living things. Very probably the transition I'm suggesting is already under way in some current specialties within

the human sciences. My impression is that in parts of economics and psychology, the case might already be made.

On the other hand, in some major parts of the human sciences there is a strong and well-known argument against the possibility of anything quite like normal, puzzle-solving research. I earlier insisted that the Greek heavens were different from ours. I should now also insist that the transition between them was relatively sudden, that it resulted from research done on the prior version of the heavens, and that the heavens remained the same while that research was under way. Without that stability, the research responsible for the change could not have occurred. But stability of that sort cannot be expected when the unit under study is a social or political system. No lasting base for normal, puzzle-solving science need be available to those who investigate them; hermeneutic reinterpretation may constantly be required. Where that is the case, the line that Charles Taylor seeks between the human and the natural sciences may be firmly in place. I expect that in some areas it may forever remain there.