

## CHAPTER 3

*Interpretation in Natural  
and Human Science*

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In recent years there has been considerable controversy over whether the notion of interpretation, and the hermeneutic circle that interpretation invokes, belong distinctively to the human sciences. An influential tradition in the philosophy of social science, going back at least to Wilhelm Dilthey and Max Weber, insists that what calls for interpretation in the strong sense is human action. Interpretation is necessary, according to this tradition, because actions (including speech acts) are meaningful, and their identity and significance as actions can be disclosed only by uncovering and clarifying the meaning expressed by or through those actions. The meaningfulness of action has been variously situated in actors' intentions, rules constitutive of their behavior, background practices that provide the context within which action is intelligible, and so forth. All such views, however, suggest important methodological and epistemological differences between human and natural science, along with ontological differences in kind between nature and the "social world."

This hermeneutic tradition in the philosophy of the social sciences acquired new momentum from the widespread criticism since the late 1950s of positivist demands for unity of method throughout the sciences; indeed, its defenders prominently contributed to the critique of positivism. Nevertheless, the critique of positivism also generated controversy about the epistemological locus of interpretation,

for prevailing accounts of the natural sciences were also radically revised. A number of prominent philosophers, most notably Thomas Kuhn, Richard Rorty, and Mary Hesse, noted deep similarities between the image of natural science emerging from the critique of positivism and the various accounts of interpretive social science. Postpositivist philosophy of science thus threatened to abolish any fundamental differences between natural and human science, or between nature and society as objects of knowledge. Defenders of hermeneutic social science responded quickly by proposing important differences between the interpretation of nature and the interpretation of human beings, differences that were thought to restore the alleged methodological and ontological distinctions between two kinds of science.

I have argued extensively elsewhere that no such interesting differences in kind between natural and human science can be sustained on the basis of a need to interpret meaningful action that is absent from our dealings with the natural world.<sup>1</sup> I am led to further discussion of the issue by reflecting on two features of these debates. First, the views of some critics of the natural science-human science distinction, such as Thomas Kuhn and myself, are substantially indebted to the work of its defenders, notably, Charles Taylor and Hubert Dreyfus. The very arguments that are supposed to establish the uniqueness of the human sciences have contributed to our accounts of natural science, thereby further undercutting the distinction. Second, Rorty and Kuhn, while giving up the attempt to distinguish the human sciences as hermeneutical, have nevertheless tried to retain the idea that hermeneutics, or "hermeneutic interpretation" in Kuhn's phrase, is distinct from other forms of inquiry. I think this distinction is also untenable; ironically, Charles Taylor's account of interpretation helps us see why this is so.

I would like to begin by outlining what I take to be the important areas of disagreement among Taylor, Kuhn, and myself. The most prominent issue, clearly, is whether there is a philosophically interesting difference in kind between the natural and the human sciences. Taylor believes there is such a difference, manifest in the inescapably hermeneutical character of the human sciences. The crucial turn in his version of the claim is that the objects of the human sciences are *essentially* self-interpreting agents for whom their actions

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are significant, whereas the natural sciences deal with objects that need not be, indeed should not be, understood as self-interpreting. Thus, even if one wanted to say that the natural sciences were interpreted in some sense, one would have to conclude that the human sciences are *doubly* interpretive, interpretations of interpretations. Taylor goes on to argue that this difference restores the important methodological and epistemological differences between natural and human science.

Thomas Kuhn's investigations of the historical development of the natural sciences have led him to question Taylor's way of drawing this distinction. Kuhn has figured most prominently among those philosophers and historians who have reminded us that we never encounter the natural world unmediated by previous understanding. Our knowledge of nature is a response to prior knowledge and the successes and failures it engendered. The result is that there are conflicts of interpretation over such things as voltaic cells, classical resonators or quantum oscillators, and chemical reactions that parallel the interpretive issues which Taylor thinks characteristic of human activity alone. The reason is straightforward: cells, oscillators, and reactions belong to a meaningful field of human activity, with a history, and a significance that cannot be disentangled from that history. Making sense of these objects and their interactions is inextricable from understanding scientific practices.

This initial disagreement points to a second issue lurking behind the dispute over the natural and the human sciences, namely, the question of realism. Take realism to be the view that beliefs within some domain are true or false depending upon the real properties of objects in that domain, that is, properties they possess regardless of anyone's beliefs, practices, or standards. Taylor wants to be a realist in this sense about the objects of natural science, even while insisting that a realist construal of "social reality" is unintelligible. Kuhn, of course, has mounted a powerful attack upon realism in the natural sciences, with his insistence upon the incommensurability of competing scientific practices and standards, and his denial that we can make any sense of what the world is "really" like independently of any reference to a history of scientific interpretation.

On these first two conflicts, Kuhn and I line up on one side, and Charles Taylor on the other. There is, however, a third issue among us on which the lines do not show up quite so neatly. This issue concerns whether there is an epistemologically distinct activity appropriately called "interpretation," which is characteristic of some activi-

ties or disciplines rather than others. Taylor insists that despite the sense in which the natural sciences involve interpretation because data are always theory laden, there is a stronger sense in which interpretation is specific to the human sciences. He has memorably objected to the currency of more expansive accounts of interpretation, ironically noting how "old-time Diltheyans, their shoulders hunched from years-long resistance against the encroaching pressure of positivist natural science, suddenly pitch forward on their faces as all opposition ceases to the reign of universal hermeneutics."<sup>2</sup> But Kuhn also believes that there is a distinctive activity of interpretation, which concerns what we do when confronted with texts or practices that are unfamiliar or puzzling. Thus, on Kuhn's view, historians and ethnographers must frequently engage in interpretation, whereas physicists or economists typically do not.<sup>3</sup>

It should not be hard to see that Kuhn and Taylor do not draw the line between interpretive and noninterpretive activities in quite the same place, and they certainly do not do so in the same way. Indeed, it is critical to Taylor's account that interpretation in the human sciences is continuous with interpretation in everyday life, in which meanings need not be explicitly articulated. The human sciences must be interpretive, on Taylor's view, because human beings are already "self-interpreting animals," in just the *same* sense of 'interpretation'. The practices in which we try to get clear about unfamiliar meanings are just a subset of our quite varied interpretive activities. In this dispute I am going to come down on Taylor's side rather than

<sup>2</sup>Charles Taylor, "Understanding in Human Science," *Review of Metaphysics* 34 (1980): 26.

<sup>3</sup>In some ways, Kuhn's distinction resembles Richard Rorty's distinction between hermeneutics and normal discourse (in *Philosophy and the Mirror of Nature* [Princeton: Princeton University Press, 1979], in which normal discourse proceeds within a shared vocabulary and taken for granted epistemic standards, whereas hermeneutics is the attempt to converse across differing standards and vocabularies without reducing them to a common framework. This resemblance should not be surprising, since Rorty's distinction is an extension of Kuhn's earlier notions of normal and revolutionary science. Kuhn's and Rorty's positions are not quite the same, however, because Kuhn thinks that the space of "hermeneutic interpretation" has relatively stable disciplinary boundaries, whereas Rorty takes the boundary between normal and hermeneutical conversations to be contingent upon the evolving history of inquiry. I nevertheless think that Rorty's distinction is vulnerable to the same sorts of arguments I raise against Kuhn. Working this out in detail seems inappropriate here, since Rorty has abandoned this version of the distinction. Criticizing its analogue in his most recent work (*Contingency, Irony, and Solidarity* [Cambridge: Cambridge University Press, 1989]), which is the difference between private irony and liberal hope and solidarity, would take us too far afield of the other concerns of this essay.

Kuhn's, but when Taylor's position is combined with Kuhn's views about realism and the natural sciences, it leads to a denial of there being any epistemically distinctive domain or activity of interpretation. This is a conclusion that I happily endorse, but neither Kuhn nor Taylor is likely to join me in so doing.

A good place to begin discussion of these three issues is with what I have learned from Charles Taylor about the natural sciences, and about the work of Thomas Kuhn. This account will provide an important background to my arguments against Taylor's attempts to preserve some version of Dilthey's difference in kind between the *Geistes-* and the *Naturwissenschaften*. As will become clear, my objections have much more to do with his understanding of the natural sciences than it does with the consequences he thinks this distinction has for the human sciences themselves.

There are three important points that I at least think I learned from the work of Charles Taylor, and that I take to be very important for understanding scientific inquiry. Taylor was not the only possible source for these points, which could undoubtedly be extracted from Ludwig Wittgenstein or Martin Heidegger, but he articulated them more clearly and powerfully than I had seen before. The first point explicitly concerns how to read Kuhn's *Structure of Scientific Revolutions*. Many critics of that book understood Kuhn to be saying that research communities in the natural sciences are, and perhaps ought to be, united by an enforced consensus about fundamental issues of ontology, method, and value. Charles Taylor taught me to see the difference between consensus, an agreement in belief between individuals who each hold that belief separately, and what he once called the sharing of "intersubjective meanings." He had in mind the way in which understanding a common language and participating in the social practices, institutions, and norms associated with that language shape the intelligible possibilities for individual belief and thereby make *either* consensus or dissensus possible. Ever since reading Taylor, I have understood Kuhnian paradigms to be "ways of experiencing action in society which are expressed in the language and descriptions constitutive of institutions and practices."<sup>4</sup> Paradigms, that is, are shared fields of activity rather than shared beliefs.

The second point I took from Taylor is that Kuhn's views do not entail the claim that language is somehow constitutive of the world;

it would be better to say that language and the real are mutually dependent. Taylor pointed me in this direction with his emphasis upon the inseparability of language and social practice. Linguistic distinctions and ways of employing them are embedded in larger contexts of social practice, which in turn could not exist without the appropriate linguistic resources. Taylor took this to mean that one could not characterize "social reality" apart from the language and practices that constitute it, and vice versa. But in the case of scientific inquiry, the relevant practices include ways of encountering, responding to, and being resisted by the things scientists are dealing with. The intertwining of language, social practice, and reality cannot be neatly bounded at the points where we run up against the natural world, for our encounters with the world, and indeed the very boundaries between self and world, belong to our interpretive social practices. I will have more to say about this later.

The third point I learned from Taylor took me in a different direction from anything I found explicitly in Kuhn's work, but it has been very important for my own thinking about the natural sciences. In the extended example that was the focus of "Interpretation and the Sciences of Man," Taylor was concerned to show that an empiricist political science will seriously misunderstand some politically important phenomena when it limits itself to considering only political action identifiable *within* a given configuration of "intersubjective meanings" and "common meanings." Political conflict can occur over the shaping of that field of meaningful action itself. Taylor found this phenomenon exemplified in the 1960s counterculture challenge to "the vision of society as a large-scale enterprise of production," and to the politics of negotiation, which is "bound up with the distinct identity and autonomy of the parties and with the willed nature of their relations."<sup>5</sup> Such conflicts are especially tricky, because the very language in which to characterize what is at issue in the conflict is part of what is at issue. The result is that the conflict is easily misrepresented or trivialized by those who occupy the position of the dominant field of action.

Taylor thought that this was a serious problem with mainstream political science. Such is also the situation, I believe, in the case of feminist, postmodernist, and neo-Romantic criticisms of the dominant culture of "technoscience," to use Bruno Latour's term for that mobilization of social resources that continually breaks down any

<sup>4</sup>Charles Taylor, "Interpretation and the Sciences of Man," in *Philosophy and the Human Sciences: Philosophical Papers, 2* (Cambridge: Cambridge University Press, 1985), p. 38.

<sup>5</sup>*Ibid.*, pp. 46, 32.

boundaries between science and society.<sup>6</sup> There is an understanding of natural science, and of nature as the object of scientific knowledge, that represents not just the consensus of many people within the industrialized West; it shapes what they—we—understand as intelligible possibilities for action. Indeed, the ideals of objectivity and rationality that inform that understanding of science are part of the common meaning through which the dominant community identifies itself to itself. Conflict over such ideals was precisely the sort of conflict that Taylor thought would be obscured unless it was understood hermeneutically.

We can now turn to my more detailed response to Taylor's attempt to preserve an epistemologically significant difference in kind between the natural and the human sciences. There are three lines of argument that I want to develop against Taylor's view. The first is a point I simply take from Thomas Kuhn. Charles Taylor's descriptions of the way in which social reality cannot be identified "in abstraction from the language we use to describe, invoke, or carry out" various social practices<sup>7</sup> also apply characterizes physical reality as the object of natural scientific research. Kuhn reminded us that scientists do not and could not achieve knowledge of things independently of particular historically contingent categories and practices. They always encounter nature as a configuration of significant possibilities for research activities. This is why Kuhn first identified "normal science" as a kind of social practice, and why he said that "after discovering oxygen Lavoisier *worked* in a different world."<sup>8</sup> Taylor thought that the human sciences had to be interpretive because what they were trying to understand was a field of meaningful human action, which was constituted by a shared understanding of what would count as intelligible and significant action within that context. Kuhn insisted that learning how to practice research within a scientific field is very much like being socialized into a culture, while trying as an outsider to understand that research activity. *and the world if research is very much like the work of an ethnographer.* If it still seems odd to you to think of electrons and tectonic plates as belonging inescapably to social reality in Taylor's sense, remember that the social world on Taylor's account certainly includes the various sorts of equipment that people use in their activities. One cannot make

sense of those activities without understanding what that equipment is, what it is for, and how it is used. Kuhn's argument is that a field of natural scientific research is a work world in which the objects of research are understood as equipment, and they make sense only through the ways we deal with them in research and development.<sup>9</sup> Hence the appropriateness of his famous analogy between scientists who too readily abandon a paradigm in the face of anomaly and carpenters who blame their tools.

Taylor recognizes the affinity between Kuhn's account of the way scientific knowledge is always mediated by previous understanding and his own account of interpretation in the human sciences, but he still insists there is an important difference. As he claimed in response to Richard Rorty, "The understanding which is relevant to the sciences of man is something more than this implicit grasp on things. . . . We are talking here of what you could call human understanding, understanding what makes someone tick, or how he feels or acts as a human being."<sup>10</sup> I will not discuss here the particular way Taylor tries to explicate the relevant difference in understanding in terms of "desirability-characterizations," a point that has been extensively criticized elsewhere. My concern, which makes up my second line of argument, is that however Taylor tries to work out the notion of "understanding a human being," it will not support his point about the natural and human sciences unless there were an extensional correspondence between two distinctions that in fact do not match neatly at all. The boundary between natural and social reality, which also supposedly demarcates the boundary between the objects of natural and human science, needs to match up with the distinction between those areas of inquiry in which our self-understanding as human agents is at issue in the inquiry, and those areas in which it is not. Any such match is at best historically contingent, but I want to argue that it does not occur at all.

We need to remember that drawing the relevant line between the human sciences and the natural sciences is not just a simple matter of ascertaining the objects of the various sciences. Taylor certainly wants to exclude human biology from the interpretive human sciences, and he may well want to include some aspects of primatology or other studies of animal behavior among them. Hence the line depends upon there being a fairly sharp split between the categories

<sup>6</sup>Bruno Latour, *Science in Action* (Cambridge: Harvard University Press, 1987).

<sup>7</sup>Taylor, "Interpretation and the Sciences of Man," p. 33.

<sup>8</sup>Thomas Kuhn, *The Structure of Scientific Revolutions*, 2d ed. (Chicago: University of Chicago Press, 1970), p. 118.

<sup>9</sup>This reading of Kuhn is developed in my *Knowledge and Power*, chap. 2, and extended beyond Kuhn's own treatment in chap. 4.

<sup>10</sup>Taylor, "Understanding in Human Science," p. 30.

in which human action is meaningful, and the categories through which we appear merely as physical objects. The difficulty is that this distinction is itself a contestable point of human self-understanding. Whether there is such a distinction, and where it lies, is inescapably part of our sense of who we are and what it means for us to act. It is no accident that anthropologists study cosmology as an inescapable part of culture. Human beings understand themselves as much through their understanding of nature as anything else. We should also not forget how much the disenchantment of the natural world, the Darwinian account of natural selection, or the turn from qualitative and perfectionist to compositional accounts of substances is integrated into our own self-understanding as agents.

Nor can one try to sustain Taylor's distinction by claiming that the understanding of nature influences our self-understanding, but that nature itself must be understood to be independent of the ways we understand it.<sup>11</sup> Taylor himself attempts such a strategy by insisting upon the importance of what he calls an "absolute" conception of the world, freed from "the meanings it might have for human subjects, or of how it figures in their experience."<sup>12</sup> But the project of such an "absolute" conception is quite remote from what actually goes on in the natural sciences. As Mary Hesse once noted (responding not to Taylor, but to Jürgen Habermas): "It is impossible in studying theories of evolution, ecology, or genetics, to separate a mode of knowledge relating to technical control from a mode relating to the self-understanding of man . . . [since] the very categories of these theories, such as functionality, selection, survival, are infected by man's view of himself."<sup>13</sup> Even the most basic ontological distinctions, for example, between an organism and its environment in evolutionary theory are problematically engaged with our conceptions of individuation and agency.<sup>14</sup> These difficulties are not confined to the biological sciences. Evelyn Fox Keller has suggestively described how the notion that chemical processes are "regulated" or "controlled" by "master molecules" implicates a gendered conception of

<sup>11</sup>I am grateful to William Blattner for showing that I must deal directly with this objection.

<sup>12</sup>Taylor, "Understanding in Human Science," p. 31.

<sup>13</sup>Mary Hesse, *Revolutions and Reconstructions in the Philosophy of Science* (Bloomington: Indiana University Press, 1980), p. 186.

<sup>14</sup>Richard Levins and Richard Lewontin, *The Dialectical Biologist* (Cambridge: Harvard University Press, 1985), part I.

human agency.<sup>15</sup> The attempt to characterize the "state" of a physical system in quantum mechanics and to interpret what it means for a system to have a definite (possibly indeterminate or probabilistic or both) state is likewise difficult to disentangle from our self-conception.<sup>16</sup> The point is not just to suggest that scientific theory is debilitated by ideological, although that is sometimes the case. Commonly, the opposite point is apropos: natural science proceeds quite well without needing to try to eliminate aspects of our self-understanding from scientific interpretations of the natural world.

An interesting connection between Taylor's view that only the human sciences interpret the self-understanding of human agents and his communitarian political arguments against liberal individual conceptions of the self may help show what is wrong with the former view. Taylor argues that it is a serious mistake to draw the boundaries of the self at a point short of our social relations with others. There are irreducibly social dimensions to our self-understanding, and there are irreducibly social goods that cannot be reduced to individual preferences. Taylor objects, rightly, to those approaches to the human sciences that rule out on methodological grounds the possibility of an irreducibly social dimension to human life, and that regard all of our social relations instrumentally: "the ontology of mainstream social science," he argues, "lacks the notion of meaning as not simply for an individual subject; of a subject who can be a 'we' as well as an 'I.'"<sup>17</sup> Yet Taylor is similarly ruling out on methodological grounds the idea that the boundaries of self-understanding extend beyond our social relations in the narrowest sense to encompass our dealings with the natural world.

The result is a remarkably Whiggish interpretation of the history of science since the seventeenth century. On Taylor's view, it was unequivocally progressive to interpret nature as dead and noncommunicative, such that our dealings with it can be only instrumental (of value only with respect to societal "preferences," to maintain the analogy to his critique of liberal individualism). Taylor believes that the further extension of atomistic and mechanistic understanding to a liberal utilitarianism was a mistake, of course, but that has now

<sup>15</sup>Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven: Yale University Press, 1985), chap. 8.

<sup>16</sup>Keller (ibid., chap. 7) also offers a useful account of how the issues surrounding the interpretation of the Schrödinger equation display the entanglement of self-understanding and the interpretation of nature.

<sup>17</sup>Taylor, "Interpretation and the Sciences of Man," p. 40.

presumably been exposed. We have now learned to replace the dualism between the mental as the realm of meaning and physical nature as what is unaffected by how it is meant with a more adequate dualism between social practice as the realm of meaningful interpretation and physical nature. Well, maybe. Perhaps we have also learned from the unfortunate political history of the distinction between nature and society, which has been used to justify social hierarchies of race, gender, culture, and class, so that we will no longer make those mistakes. But I remain suspicious of the sort of history of science that Taylor's distinction may compel us to accept.

My suspicions are heightened when I cast about for examples where it would make a difference to follow Taylor in denying that our understanding of nature involves interpretation in his strong sense. I cannot resist beginning with the example Taylor himself offers in defense of his view. Taylor says of traditional Japanese society that "they live under the same heavens as we do, only understand it differently, but it is not true that they have the same kind of bargaining as we do."<sup>18</sup> Perhaps it is just a remediable slip that he takes "living under the heavens" to capture a meaning-independent reality identifiable apart from how we understand our own lives, but I do not think so. I am worried that Taylor's lack of self-consciousness in so regarding that phrase reflects a complacent identification with the ascendancy of a contentious interpretation of science and nature comparable to the 1950s understanding of the ascendancy of liberal pluralism as the "end of ideology."

We can see this in the way Taylor's distinction between the natural and human sciences bears upon some recent intellectual and political controversies. Feminist interpretations of the gendering of scientific knowledge and of nature as the object of such knowledge offer one important group of examples. Taylor's view can accommodate those feminist philosophers and scientists for whom the discovery of gender in science is the discovery of a bias to be eliminated in accord with an unchanged notion of scientific objectivity. But his view rules out on methodological grounds any stronger feminist account of natural scientific knowledge as gendered. Thus, I believe Taylor must reject out of hand the idea, suggested by Nancy Chodorow and Evelyn Fox Keller, that the separation between self and nature, which he thinks essentially characterizes natural science, is the product of male psychodynamics in societies where women nurture infants. He

<sup>18</sup>Ibid., p. 33.

must also reject the idea, put forward in rather different ways by G. H. von Wright and Keller, and illustrated in Keller's example of "master molecules," that causality is not conceptually separable from how we understand ourselves as agents. For it is important to Taylor that efficient causality be a real relation in nature, and not a part of the intersubjective meanings of a particular historical culture. If scientists like Barbara McClintock or the pseudonymous Anna Brito find it appropriate to interpret their understanding of maize or lymphocytes in terms of friendship or love,<sup>19</sup> this must at best be metaphorical or merely psychological. These are not the sorts of interactions that can really be reciprocal relations, so they must be reduced to something else in much the same way the individualist reduces social relations to individual subjective values or preferences. On Taylor's view, such accounts can only concern a kind of attunement to the world that we must regard as separable from theoretical understanding, even if the scientists themselves somehow fail to see this.

A related sort of example is suggested by the various attempts to create an ecological politics that would take noninstrumental relations to the natural world as irreducible goods. Social ecologists, deep ecologists, ecological feminists, Marcusean critical theorists, and perhaps some defenders of animals are engaged in precisely the sort of challenge to the common and intersubjective meanings of our culture that Taylor recognized in the counterculture of the 1960s. To insist that they and their political concerns must be understood within the parameters of the dominant understanding of scientific rationality seems to me comparable to the attempt, which Taylor deplores, to impose the vision of a society of work and the social practices of negotiation as the horizon within which we must make sense of the protests of the 1960s. My point in raising these examples is not to endorse any or all of these views, any more than Taylor wanted unequivocally to endorse the protests of the 1960s. Indeed, the parallel is a strong one. Taylor thought that a social science and social reality constituted by political and methodological individualism was incapable of understanding what was at issue in those debates. I similarly want to argue that a conception of natural science and the natural world constituted by a sharp separation between the social and the natural is incapable of grasping much of our current political

<sup>19</sup>Evelyn Fox Keller, *A Feeling for the Organism* (San Francisco: W. H. Freeman, 1983); June Goodfield, *An Imagined World* (New York: Harper & Row, 1981).

situation. My first line of argument suggested that Taylor's distinction between the human and natural sciences could not accommodate what we have learned from Thomas Kuhn and others about natural science. Now, this second line of argument suggests that the distinction also cannot accommodate the kind of political insight that is rightly associated with the work of Charles Taylor.

There is a third line of argument against Taylor's distinction which I cannot really develop here, but which I want to mention because I think it explains much of the intuitive appeal which that distinction continues to possess. Taylor sees a deep connection between his view of the human sciences and his views about what it is to be a human agent. The human sciences must be interpretive in a stronger sense than is true of the natural sciences, because they have as their objects human agents who are in an important sense constituted by their interpretive activities. A human science that left out of account the self-interpretive dimension of human beings would be radically incomplete or misguided about its own object, whereas this would not be true of the natural sciences. Unfortunately, this argument involves a mistaken inference, which was first brought to my attention by Mark Okrent.<sup>20</sup> From the fact that all human understanding takes place within a field of intersubjective meanings, as Taylor rightly insists, it does not follow that this is a fact about human beings as objects which must always be taken into account in the human sciences. The point parallels Kant's Paralogisms of the Soul: conditions on the possibility of knowledge are not facts about knowers now taken as objects of knowledge. The argument from human beings as self-interpreting subjects to the human sciences as interpretive in Taylor's strong sense is a non sequitur.

We can now turn to the last issue I promised to address. There might still be an interesting group of activities that are distinctively "interpretive," even if its boundaries do not map onto those usually drawn between the natural and the human sciences. Kuhn, for example, still holds onto the idea that there is a characteristic activity of interpretation typically engaged in by historians, ethnographers, literary critics, and the like, in their encounters with the unfamiliar. We engage in interpretation in this sense when we try to get inside an alien culture or text, learn our way about, and eventually perhaps explicate what is going on there. This is fundamentally different,

Kuhn wants to argue, from the ways in which we live more or less unproblematically within a culture that is very much our own, including our own scientific cultures.

Taylor helps us to understand what is wrong with this view. He insisted that "already to be a living agent is to experience one's situation in terms of certain meanings; and this in a sense can be thought of as a sort of proto-'interpretation'."<sup>21</sup> The reason for this claim is twofold: first, making sense of one's own culture and even clarifying one's own sense of the situation one is in are more reflective and interpretive than a sharp distinction between understanding familiar and alien meanings would suggest; and second, there is less homogeneity and more room for construal and misconstrual even within a familiar and shared field of activity than is often assumed. After all, it was Thomas Kuhn who taught us that even the shared practices of normal science incorporate multiple interpretations of that practice. Scientists usually "read" one another's work in ways that largely manage to smooth over those differences, and it is not clear why these readings are different in kind from making sense of practices one explicitly recognizes to be puzzling or alien. I am happy to accept the insight that I believe Kuhn is trying to capture, namely, that there are times when it is important to be attentive to the possibility that others are using words or things in ways that do not neatly fit our own assumptions, and other times when we can safely make those assumptions for the time being. But I insist that this difference is a pragmatic one, which does not demarcate real and intrinsic differences between the alien and the familiar; that even this pragmatic difference does not neatly divide disciplines; and that the need to be alert for a possible breakdown of commensurability can sometimes arise in the midst of what we took to be familiar surroundings. There is undoubtedly much more to say on this topic. But I believe the outcome would not fundamentally change: interpretation, characterized by the hermeneutic circle, and the entanglement of understanding with self-understanding, does not demarcate a particular domain of activity or inquiry (or its objects), but is a general characteristic of how things become manifest.

Let me conclude by attempting to head off one serious misunderstanding of what I have been trying to say about the relation between natural and human science. Taylor's defense of a strongly interpretive account of the human sciences was directed against the logical

<sup>20</sup>Mark Okrent, "Hermeneutics, Transcendental Philosophy, and Social Science," *Inquiry* 27 (1984): 23-49.

<sup>21</sup>Taylor, "Interpretation and the Sciences of Man," p. 27.

positivists' insistence upon the methodological unity of science, and against the construction of mainstream American social science to accord with positivist demands. So when I reject his view that human and natural science are different in kind, I might be read as resurrecting the view that there is only one kind of science, now perhaps modeled upon the human sciences rather than physics. This would be a mistake. What I am arguing instead is that neither natural nor human science forms a natural kind. There are many interesting and important differences among the various scientific disciplines. Paleontology or meteorology may be as interestingly different from one another and from high-energy physics as they are from macroeconomics or political science. All have been shaped by a history of internal development and interaction with other scientific fields and other social practices. I follow Friedrich Nietzsche in insisting that "only that which has no history is definable"<sup>22</sup>, successful practice in the various sciences always has and will continue to escape the constraints and typologies placed upon them by methodologists armed with the latest account of what a science must be like in order to be successful.

<sup>22</sup>Friedrich Nietzsche, *On the Genealogy of Morals*, trans. Walter Kaufmann and R. J. Hollingdale (New York: Random House, 1967), p. 80.