After an initial period of excitement following the publication of *The Structure of Scientific Revolutions*, Thomas Kuhn's work in the philosophy of science was subjected to severe and widespread criticism, and seemed destined to be of largely historical interest. Frederick Suppe remarked in 1977 that

Despite his sustained efforts to reply to critics and clarify, modify, or improve his position when he feels his critics have a legitimate objection, since [1969] Kuhn's views have undergone a sharply declining influence on contemporary philosophy of science... Philosophy of science is ceasing to view the *Weltanschauung* analyses as offering promising avenues for the development of an adequate understanding of science.¹

Much of the criticism of Kuhn arose from a growing interest in realist approaches to the philosophy of science. Of the four principal factors in Kuhn's fall from grace cited by Suppe, three are clearly motivated by a commitment to realism. The fourth objection, that Kuhn "makes discovering how the world really is irrelevant to scientific knowledge,"² also encompasses the first, that Kuhn extends the positivists' antirealist treatment of theoretical terms to observation terms as well. The second objection is that Kuhn "shortchanges the role of rationality in the growth of scientific knowledge."³ This is another way of saying that Kuhn fails to provide science with an epistemology which would adequately justify the realist claims that scientific theories "converge" as approximations to the truth and that scientific knowledge grows by accumulation. Only Suppe's third objection, that Kuhn imposes upon the history of science a rigid and simplistic schema of oscillations between periods of normal and revolutionary science, is not directly connected with realism. We shall soon see that this

*¹I would like to express my gratitude to the National Endowment for the Humanities, whose generous award of a Summer Research Fellowship enabled me to complete this project. I would also like to thank Mark Okrent for his very helpful comments on an earlier version of this paper.

third objection involves a misreading of Kuhn’s position, leaving Kuhn’s antirealism as the principal point of attack.

Kuhn is admittedly antirealist: he explicitly denies any nontrivial application of a correspondence theory of truth to science, and rejects realist interpretations of scientific terms, whether observational or theoretical. His critics have not sufficiently understood his position, however, for he is commonly construed as an idealist or an ‘irrationalist,’ whatever that is. Kuhn’s position undermines the presuppositions implicit in opposing realism and idealism as philosophical alternatives, and is concerned to account for the rationality of scientific research without relying upon a realist metaphysics. I will not try to demonstrate this by directly responding yet again to the canonical criticisms of Kuhn proposed by Scheffer, Shapero, Suppe, and others,4 nor by attempting once more to untangle Kuhn’s own various formulations of his position. Rather I shall try to show how the fundamental theses of Kuhn’s analysis can be generated from the more general ontological investigations in Heidegger’s Sein und Zeit. Kuhn has explicitly lamented the absence of an adequately developed philosophical alternative to an epistemology based upon distinctions between an interpretive scheme and the uninterpreted content to which it is applied.4 Heidegger’s ontology requires the rejection of that distinction along with the related epistemological distinctions between analytic and synthetic statements, and between representations and states of affairs represented, and provides grounds for rejecting realist criticisms of Kuhn. Apart from many highly suggestive passages, however, Heidegger never works out an interpretation of science following from his ontology. Kuhn’s and Heidegger’s positions, with some modifications, thus turn out to complement each other in important respects.

A realist treatment of scientific statements as true according to a correspondence theory of truth has always faced the problem of how to gain access to the things to which those statements are to correspond. Traditionally, three approaches have been taken to this problem. The positivists abandoned realistic treatments of theoretical terms, but suggested that in scientific observation we have direct and unsullied access to the things themselves. This left them with the problem of how to interpret scientific claims in a vocabulary drawn from observation reports and an appropriate logical apparatus. Transcendental idealists have argued that we already have such access, because things are constituted as the things they are by our acts of synthesis, and these acts and their objective products are directly presented to transcendental reflection. Contemporary scientific realists look to a causal or naturalistic theory of reference to justify our claims to be talking about what is really there, even if many of our beliefs about these things are false.

Heidegger argues, and Kuhn takes for granted, that these are approaches
to the wrong problem. The problem is not in finding the right sort of access to reality such that our statements can be known to correspond to it; once a gap between ourselves and the world is postulated, as it is in this formulation of the problem, the gap can never be closed. Heidegger has proclaimed that "the scandal of philosophy" is not, as Kant would have had it, that scepticism about reality has not been definitively overcome, but that such scepticism is still thought of as something which needs to be overcome.4 For Heidegger, we are already being-in-the-world, prior to any dissolution into subject and object or mind and world. The epistemological relation between knower and known is, he thinks, demonstrably founded upon being-in-the-world, and the first division of Sein und Zeit is an articulation of this claim. I expect to show that Kuhn must be understood to begin from the same point. Heidegger calls such an attempt to characterize science from the standpoint of being-in-the-world an "existential conception of science" which is concerned to articulate "the conditions for the possibility of [our] existing in the way of scientific research" (p. 357; English translation, p. 408). Heidegger insists he is not concerned to "make a problem of the ontical history and development of science, of the factual occasions of it, [and] of its proximate goals" (p. 357; English translation, p. 408). Such an ontical account of science as one particular human activity is what Kuhn is aiming for, but it must be understood in the context of something like Heidegger's existential-ontological conception of science. It is in this respect that the two positions are complementary.

Kuhn begins his account of science with the scientist's ordinary involvement in the practice of research, within which the philosophical problem of access to things has not arisen. For Kuhn, this unproblematic approach extends to all of the 'fundamentals' of one's field, which are used with an unquestioned assurance (this does not mean that one never questions them, but only that their applicability in research is taken for granted). Heidegger would call this beginning "science in its average everydayness," and it corresponds to where he begins his phenomenology of Dasein's being-in-the-world. It is important to recognize from the outset that neither Heidegger nor Kuhn think that the unquestioned confidence characteristic of normal science provides an adequate understanding of science. Normal science systematically misconstrues itself, but both find this misconstrual revealing, and consider it essential to the practice of science. To overlook it in favor of an epistemological starting point which demands that our natural sense of involvement with things be suspended and justified would lose the possibility of seeing the significance of this misconstrual.

Normal science, or science in its average everydayness, is not a particular cognitive attitude toward the objects of scientific research, but a particular way of manipulating and dealing with the world. This distinguishes normal science from Husserl's concept of the "natural attitude." Kuhn describes it
as "research firmly based upon past achievements" (p. 10, my emphasis),
while Heidegger insists that scientific research as a way in which human
beings exist or behave must be considered more fundamental than science
as a logical structure of statements or propositions (pp. 11, 357; English
translation, pp. 32, 408). Kuhn argues that scientific research normally
involves work on a particular, well-defined task or problem (puzzle-
-solving), utilizing socially constructed and approved techniques,
equipment, and facts (exemplars) whose application to the problem is
guided by their 'place' within an ordered structure of ontological commit-
ments, values, standard practices and vocabularies, etc. (a disciplinary
matrix). This can be seen as a specification of Heidegger's description of
everyday Dasein as absorbed in "the work to be produced, as the 'towards-
which' of such things as ... equipment" (p. 70; English translation, p. 99),
which are only encountered within the context of a "referential totality," an
"in-order-to-for-the-sake-of" structure which Heidegger calls "signifi-
cance" (i.e., that which allows any particular being to have significance).
Thus on both accounts, scientists in their everyday research activity are not
proximally concerned with things, with properties, or with relations to be
observed, but with equipment which is already understood as being usable
for some particular purpose, is involved in its very being in a means–end
structure, and is most appropriately understood in being so used. In
scientific research, we must include as equipment not just physical equip-
ment (instruments, laboratory paraphernalia, libraries, etc.), but also
methodological "equipment" (standardized techniques, methods, and
procedures) and intellectual "equipment" (conceptual and mathematical
tools, and standardized facts). There are obviously differences between
physical, methodological, and intellectual tools, but these are less essential
than their common character as equipment. Thus, when Kuhn talks about
scientists blaming themselves rather than their tools for their research
failures (p. 79), he is clearly using 'tools' indifferently to refer to all kinds
of equipment. The ontological characteristic of equipment is its holism. As
Heidegger remarks,

There is no such thing as an equipment. (p. 68; English translation, p. 97)

Equipment exists and is dealt with only in the context of other equipment,
and the practices in which and the ends toward which they are used
together.

What sort of understanding do scientists have of their equipment and the
referential context of significance within which it makes sense? Kuhn's dis-
cussion of this focuses upon two points. First, such understanding is
largely tacit, although it begins to be more extensively articulated when it
becomes problematic in times of crisis. Second, it is not expressed (or even
fully expressible) in the form of explicit statements or rules, but is 'embedded' in a direct grasp of concrete problem solutions (exemplars) which is open-ended and directed, allowing them to be applied analogously to new sorts of situations and problems. Heidegger's analysis of our understanding of significance is richer and more detailed. He begins by contrasting such practical understanding with the understanding of a disinterested observer:

If we look at things just 'theoretically,' we can get along without understanding readiness-to-hand. But when we deal with them by using them and manipulating them, this activity is not a blind one; it has its own kind of sight, by which our manipulation is guided. (p. 66; English translation, p. 98)

Such sight (Sicht) Heidegger calls "circumspection" (Umsicht); it is a form of interpretation of what is already understood. Understanding for Heidegger is the projection of possibilities, not in the sense of entertaining specific possibilities, but of providing the background against which specific possibilities manifest themselves. A scientist circumspectly considering what to do next in her or his research will explicitly evaluate a number of possible courses, but these possibilities are 'pre-selected' from an enormous range of 'logical possibilities' which are never raised as real possibilities against this background of understanding, which Heidegger calls "interpretation" (literally, "laying-out," Auslegung). Interpretation arises out of understanding in three respects, which Heidegger calls its "prepossession" (Vorhaben), "preview" (Vorsicht), and "preconception" (Vorgriﬀ). Prepossession is one's general familiarity with the sorts of things one is dealing with, how they interact and belong together, what can be done with them. The laborious formal education and apprenticeship of scientists is designed to equip them with a prepossession adequate to permit them to understand and engage in research. Against this prepossessed background, our understanding gives us a general sense of what is problematic and how we might proceed to deal with it. This general sense of how to go on is our preview (or "foresight").

This preview 'takes the first cut' out of what has been taken into our prepossession, and it does so with a view to a definite way in which this can be interpreted. Anything understood which is held in our prepossession and towards which we set our sights 'foresightfully' (Vorsichtig) becomes conceptualizable through the interpretation. (p. 156; English translation, p. 191)

But this sense of how to go on with a problem can only make sense in conjunction with a preconception of what would count as a solution to the problem, which allows us to know when to stop with it.

This three-part structure is parallel to the constraints definitive of a
puzzle for Kuhn. Unlike Kuhn, however, Heidegger has an argument as to why these constraints are tacitly understood. Philosophers of science have been primarily interested in the retrospective evaluation of the results of scientific research, and such evaluation presupposes that the research has been completed, and that what is to be evaluated has been made fully explicit. But if we are interested in the practice of research, we must consider it prospectively. Research demands that we project possible directions in outline, in advance of their actually having been carried out. To make such understanding fully explicit would be to have already carried out the research activities which the understanding is projecting. A fully explicit preconception would not tell us what would count as a solution to a problem; it would be the solution. A fully explicit preview of how to solve a problem would render its solution merely a technical exercise. It might be argued that we could be fully explicit about what we do know, even though it is not yet fully adequate to solve our problem, but according to Heidegger, this would be to misunderstand the projective character of understanding. Why would it be that just these explicit claims and no other have been projected as our preview and preconception? There must be a not yet articulated understanding on the basis of which any explicit consideration of alternative claims takes place.

The character of understanding as projection is such that the understanding does not grasp thematically that upon which it projects—that is to say, possibilities. Grasping it in such a manner would take away from what is projected its very character as a possibility, and would reduce it to the given contents which we have in mind. (p. 145; English translation, p. 185)

So long as work remains to be done on a problem, there must be an open-ended, non-thematic understanding of how to go on with it. When we have actualized the research possibilities of a problem it is either transformed into standardized equipment for dealing with new possibilities, or science passes it by.

Even in routine scientific work, a problem known to be solved is a dead problem; the task cannot be repeated even if one might want to, for the world is a bit different from what it was when the work began."

Who is it who understands, and engages in the interpretive activity of research based upon that mostly unthematized understanding? One is inclined to say that scientists, individuals who have been educated for and have taken up the role of researcher, do so. But Kuhn and Heidegger both claim this "obvious" answer is wrong. Kuhn remarks in retrospect that

Traditional discussions of scientific method have sought a set of rules that would permit any individual who followed them to produce sound knowledge. I have tried to insist, instead, that
though science is practiced by individuals, it is intrinsically a group product and that neither its peculiar efficacy, nor the manner in which it develops, will be understood without reference to the groups that produce it.”

It is not just that science develops by consensus, for consensus can be analyzed into the explicit belief of its members, and Kuhn denies this possibility:

No consensus [among members of a research community] is required. If scientists... accepted a sufficient set of standard examples, they could model their own subsequent research on them without needing to agree about which set of characteristics of these examples made them standard, justified their acceptance. [Scientists] agree in their identification of a paradigm without actually agreeing on, or even attempting to produce, a full interpretation or rationalization of it. (p. 44)

Scientists share equipment and the practices within which it is used; they share a form of life. Unlike a consensus of beliefs, a shared form of life cannot be analyzed into ‘individual forms of life’ of those who share it.

Yet Kuhn’s emphasis on the community or group is not quite adequate, because groups are composed of specifiable individuals. The possibilities for research opened up or closed off by such social practices cannot be attributed to any such specifiable group. Heidegger expresses this impersonal character of a social practice and the referential context of significance which it constitutes by saying that it is “das Man,” the anyone, who understands and projects.

The “who” [of everyday activity] is not this one, not that one, not oneself, not some people, and not the sum of all. The “who” is the most, the “anyone”. (p. 126; English translation, p. 164)

Equipment is made to be used, not by someone in particular, but by anyone equipped to use it. Jerome Ravetz has detailed how scientific tools and facts are standardized, shorn of their specialized refinements and protected from common pitfalls. Problem formulations and solutions are constrained by ‘what one does’ within a particular field. It is not, as Feyerabend would have it, that an authoritarian scientific community or its elder oligarchs arbitrarily compel others to accept their standards and procedures. Everyone conforms to common practice, in order to be understood. No one legislates community standards and practices in science, but everyone enforces them, in order to maximize the intelligibility of what is done. The ‘anyone’ is not a person, but a way of being a person, an anonymous way in which we all are most of the time. Kuhn’s account of the social character of normal science has been criticized by Popper as glorifying the most hidebound and unimaginative aspects of bad scientific work.
Heidegger grants that this way of being which is constitutive of normal science and its understanding is a “levelling down” which “glosses over everything that is original” (p. 127; English translation, p. 165). But he also insists that it is constitutive of the possibility of any intelligible social practice, including science. As we shall see, this tension between original discovery and its being averaged out and covered over is essential to Heidegger’s understanding of truth. For Heidegger, truth in its most genuine sense is this tension.

The picture of science which Kuhn and Heidegger have sketched out so far is one in which research tasks are projected and solved on the basis of a largely unthematized, anonymous social understanding, into which the solutions are appropriated to project new possibilities for further research. In fact, both insist that this is only part of the story; research rarely if ever proceeds so smoothly. Scientific research normally encounters numerous roadblocks and seemingly inexplicable failures before achieving solutions satisfying its preconception, and many times the problem must be abandoned without solution. These difficulties range from inadequate equipment (physical or conceptual) to unaccountable empirical anomaly. Kuhn emphasizes the latter: “all scientific theories confront counterinstances at all times” (p. 80), but the point can be extended to conceptual conflict with other work, and the various “pitfalls” encountered in research. He has carefully described the various possibilities of equipment breakdown, and how they affect our understanding of and absorption in the task at hand. Whether our equipment proves inadequate to the task, or the right equipment is not at our disposal, or something gets in our way, we become momentarily aware in a new way of the context of significance within which we work. The things we work with, which before we took for granted and were only aware of circumspectively, now stand out as objects of reflection. In this experience of dysfunction, the scientist comes closest to the traditional role of observer. But this is fleeting, for one becomes re-absorbed in a new task, overcoming the pitfall or accounting for the anomaly. One’s focus within the referential context of significance shifts, but it is once again presupposed as the guide to our interpretive activity.

Kuhn argues, however, that some difficulties are so acute that they do not permit such unproblematic re-involvement in one’s previous context of research. Such acute problems generate a crisis among the practitioners of a research discipline, such that they no longer can take for granted their traditional understanding of the field. The loss of (or at least loss of confidence in) that which gives significance to all one’s professional activities must be extremely disorienting, and according to Kuhn, research in times of crisis is markedly different from normal science. One can no longer take for granted that one’s own work, or anyone’s work, really makes sense. If
such doubts were to become complete, research activity in the field would cease, as one would have no idea how to proceed at all. This never quite happens; loss of confidence is always partial, and the difference between crisis and normal science is always one of degree. It should, in any case, be clear why the accusation that Kuhn simplistically schematizes the history of science is erroneous. Normal science and crisis are not historical periods, but are ways of practicing science. One or the other may predominate at any one time, but this is not essential. Some scientists experience crisis over problems that do not disturb the majority of their colleagues. Others go on blithely with their normal research even though their colleagues are no longer certain what to make of that research. Normal science and crisis almost always co-exist in this way, and whether or not to interpret a particular historical episode as a crisis must be determined in retrospect.

What distinguishes crisis-provoking difficulties from the pitfalls and anomalies omnipresent in normal science? This has been a particularly nettlesome question for Kuhn, since in an important sense, there is no difference at all.

Excepting those that are exclusively instrumental, every problem that normal science sees as a puzzle, can be seen, from another viewpoint, as a counterinstance and thus as a source of crisis. (p. 79)

Unfortunately, Kuhn’s efforts to account for why such problems actually are seen one way rather than the other have failed. He has suggested that an increasing number of difficulties is relevant, but how are anomalies and the like to be enumerated? And if one such problem can be sufficient to provoke crises, as Kuhn admits, what is the function of more? The pervasiveness of an anomaly, its recalcitrance to solution by the field’s most eminent men and women, is another possibility he proposes. But how long is long enough? When ought one to be convinced that the light at the end of the tunnel will never be just around the corner? This question perplexes us even more after Kuhn cites anomalies whose well-known persistence for sixty years or more did not provoke a crisis (pp. 81-82). Kuhn notes that

Sometimes an anomaly will clearly call into question explicit and fundamental generalizations of the paradigm...” (p. 82)

But this begs the question: if a problem clearly calls into question fundamental generalizations, it has already provoked a crisis. To call it a fait accompli does not explain how it was accomplished.

The problem is that Kuhn is erroneously looking to the discipline’s presuppositions (its understanding) to call themselves into question. Heidegger would argue that the difference between normal and crisis-pro-
voking difficulties is due to features of the problem, but to the disposi-
tion (Befindlichkeit) or mood of the practitioners. To grasp the relation
between understanding and mood, it is helpful to recall that understanding
is constituted as the projection of possibilities for further interpretation.
Possibilities are always projected out of a situation we find ourselves
already in, and toward which we are already disposed in some more or less
definite way.

Possibility... does not signify a free-floating ability-to-be (Sichkönnen) in the sense of the
‘liberty of indifference’. In every case, Dasein, as essentially a disposition has already got
itself into definite possibilities. (p. 144; English translation, p. 183)

It should be clear how our disposition can affect which possibilities are
projected as real possibilities to be considered. Someone confident of his or
her capabilities will entertain possibilities not open to one whose bearing is
timid and insecure. In the limiting cases, boredom or anxiety tend toward
obliterating all possibilities, while panic fear opens up too many possibili-
ties (everything must be done, at once, and therefore all ranking of
possibilities according to significance collapses). Scientists’ confidence in
their research practices will transform how they are affected by difficulties
(for Heidegger, disposition is the ontological condition for being affected
by things at all), as will their interest in the research possibilities opened up
by those practices (I suspect boredom, or a sense of the triviality of work in
one’s field, has had more to do with scientific revolutions than is usually
suspected). Kuhn’s mistake according to Heidegger would presumably be
in trying to find objective features of science’s social understanding which
determines its mood, when mood and understanding are co-determining. I
have emphasized the effect of mood upon understanding to redress an in-
balance, but obviously one’s understanding of possibilities will also affect
one’s mood. It should be realized that Heidegger does not regard mood as
a subjective, psychological state, but as a public phenomenon, akin to the
style of one’s behavior. Not only can the “anyone” have moods as well as
individuals, but in a social practice like science, individuals will be con-
cerned to conform to the anonymous mood of their community as much as
to its understanding.

Crisis can be resolved in one of three ways. The crisis may subside,
either because the difficulties around which it focused are resolved, or
because the mood of the community shifts such that they are no longer seen
as troublesome and can be put on the back shelf. Or the crisis may persist
and the community of researchers dissolve. We tend to identify communi-
ties as large, stable groups (solid-state physics, nucleic acid biochemistry,
etc.), but many research groupings are more ephemeral, and their comings
and goings are a regular feature of scientific research. Finally, the commu-
nity, or part of it, may coalesce around a new way of practicing its research, in what Kuhn has called a scientific revolution. Revolutions are a way of reinstating normal science, and their occurrence has been a source of much of the controversy over Kuhn's work. The center of controversy has been the claim that scientific practices before and after the revolution are incommensurable. Most of Kuhn's critics have interpreted this to mean that the statements accepted as true by scientists before and after the revolution are logically incommensurable, perhaps even untranslatable. We are now in a position to see why this is a gross misinterpretation of Kuhn. First of all, Kuhn is not particularly concerned with statements and their logical relations at all. Nor would he have reason to deny that scientists' utterances are intertranslatable. They presumably understand one another's sentences perfectly well. The sense in which practitioners using different exemplars "talk through one another" is in failing to comprehend the force of one another's arguments. Their arguments for their way of practicing research depend upon different senses of what it is significant to accomplish, what facts and theories it is important to take account of, what tools and methods are legitimate to use. Even here, there will always be some common ground, since many of these features will be preserved across scientific revolutions, albeit often occupying somewhat different places in the in-order-to-for-the-sake-of-structure of significance. If this were not the case, it is hard to see how proponents of different ways of practicing science could even hold a debate, as Proust and Berthollet did, let alone conclusively resolve it. Finally, it should be clear that the claim that competing theories can be logically compared and evaluated according to empirical evidence is utterly irrelevant to Kuhn's position. For in order to compare two theories logically, they must be essentially completed. But what Kuhn is considering is the comparison of competing open-ended possibilities for further research. For this, the translation and logical comparison of results to date may well be relevant, but can never be conclusive. After all, those results may later be challenged, superseded, or reinterpreted within the research program which produced them. Unlike Imre Lakatos, Kuhn is not committed to there being a "hard core" of statements definitive of a program of research. Kuhn would deny even such a relativized version of the analytic/synthetic distinction. 14

The best reading of Kuhn's use of 'incommensurable' that I know of has been given recently by Richard Rorty.

By "incommensurable" I mean able to be brought under a set of rules which tell us how rational agreement can be reached on what would settle the issue on every point where statements seem to conflict. Note that this sense of "incommensurable" is not the same as "assigning the same meaning to terms," ... [which] seems to me an unenlightening way of describing the fact that [the parties] cannot find a way of agreeing on what would settle the issue. 15
Heidegger puts this issue in an ontological setting. Communities who do research within different referential contexts of significance do indeed "live in different worlds," but only in what Heidegger calls "a pre-ontological existential [as opposed to existential] signification of 'world'" (p. 65; English translation, p. 93). We all always live in such 'worlds' (Heidegger always uses quotation marks for this existential sense), not just particular scientific 'worlds,' but the 'academic world,' the 'business world,' and myriad others. Such 'worlds,' Heidegger says, are modes of the world (p. 65; English translation, p. 93; he calls world in this sense the "worldhood of the world"). If they were untranslatable alternative conceptual frameworks, they would be instances of the universal concept 'world.' They are not, because they acquire their intelligibility and wholeness as a 'world' only within the horizon of the one and only world. There can be for Heidegger no other possible worlds, but only other possible arrangements within this one. The world is not an entity such that we might conceivably encounter other distinct entities of the same sort; it is the horizon of intelligibility within which entities are encountered at all. There is no more comprehensive horizon within which it might be situated in order to consider it as an object, and Heidegger calls it "worldhood" rather than "world" to emphasize this. More comprehensively than Davidson, for whom the translatability of someone's language into ours is constitutive of its being a language at all, for Heidegger, the comprehensibility of another group's or culture's practices is constitutive of their being Dasein (i.e., the same way of being [Sein] we are).

Of course, to understand another's practices in this way is not yet to project them as real possibilities for ourselves. We grasp them only in an average way, which allows us some understanding of what one does when living in such a way (e.g., in a particular culture, social class, scientific community), such that we know how to respond to them. Such understanding is clearly compatible with incommensurability in Kuhn's sense: one can perfectly well understand what others do, and why they do any one particular thing in the context of their whole way of doing things, without in any way grasping why they would want to do things that way. This is not to say that the practices of other cultures or communities must be inexplicable in this way. We may very well see the advantages of their way of life quite clearly. We might even see it as preferable (in the abstract) to our own. It is a different question whether we can take up their way as a real possibility. We may be able to live in another's existential 'world' only as a foreigner (our ways might be too much constitutive of who we are). Others may find radical shifts of 'world' possible, with or without retaining one's sense of belonging to one's original traditions. All this has to do with Heidegger's discussion of Dasein as a thrown project, and the various possibilities depend largely upon one's disposition. Within this complex of problems
surrounding throwness, disposition, and projection is the ontological basis for Kuhn's discussion of the existentiell situations of scientists making (or not making) the transition to a new way of practicing science after a revolution (pp. 150–52). Such situations involve considerable continuity, of course, and rarely if ever involve difficulties comparable to those of changing cultures.

Many of Kuhn's critics would still object to his claim that scientific practices across revolutions are incommensurable, however, even when that claim has been distinguished from the unintelligible notion of 'radical meaning variance.' Criticism of this more limited sense of the incommensurability of different ways of practicing science usually develop their arguments against Kuhn's treatment of the concept of "truth" in science, but the arguments can be constructed more generally (as Abner Shimony does) as arguments against Kuhn's denial of the possibility of "ultimate justifications" of scientific practice or its results. Kuhn suggests that the traditional concept of truth is both unintelligible and superfluous in its application to the philosophy of science.

We may have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth. ... Does it really help to imagine that there is some one, full, objective true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal? (pp. 170–71)

Kuhn's critics answer that something like the "Hypothesis of Verisimilitude" (Shimony, Popper) or the "convergence of scientific knowledge" (Boyd, Putman) is both intelligible and necessary for an adequate philosophical account of science, and that this cannot be understood without reference to truth. It is on the grounds of his rejection of such a hypothesis that they label him an unacceptable idealist, subjectivist, and/or irrationalist. What is the issue? If all that is meant by the "convergence" of scientific knowledge is that we can construct a historical narrative of how we got to our current practices and beliefs, which makes both our past successes and failures intelligible in the light of current practice, then Kuhn would agree that science converges. Such convergence is the noncontroversial result of the fact that all understanding arises out of a tradition, which we have described as science's prepossession, prevalue, and preconception. All discoveries gain their significance from their historical context. But this context is constantly being reorganized and renewed, even through normal science. Such converging narratives cannot undermine and justify our current practices, because they depend upon them. But Kuhn's critics mean more by 'convergence' than a narrative convergence toward current practices. They also mean more than that our current practices indefinitely project future possibilities toward which they, too, will
eventually be seen to converge. They mean that the history of scientific practice converges toward some practice-independent truth, which will provide an ultimate context of intelligibility not situatable in some further context, and not projecting any nontrivial possibilities for further research. Kuhn has denied that theories can be appropriately construed in this way as representations approximating "what is really out there." The concern of his critics is that by denying that truth is such an ultimate context, Kuhn has somehow divorced science from any contact with the world, thereby leaving its historical success inexplicable. Shimony, for example, explicitly discusses Kuhn's position in conjunction with traditional arguments against the reality of the external world, while Putnam and Boyd both argue that some form of realism is required lest the predictive success of science be consigned to the realm of the miraculous.

Kuhn has repeatedly expressed bewilderment at such criticism. He has, after all, been concerned to account for how science has succeeded in advancing and consolidating its capabilities, and sees his account as reasonably successful. He has never doubted, let alone denied, that there are real things in the world that scientists refer to and deal with in their work. Whereas Kuhn's critics feel compelled to defeat the sceptical claim that most of our terms might fail to refer and most of our beliefs be false, for Kuhn the question of scepticism never arises. Such a question conceives of an articulable world of fully determinate entities present-at-hand, and asks whether we are referring to the entities in this world and describing them correctly. But on our reading of Kuhn, it is only within the context of a social practice with its in-order-to-for-the-sake-of structure that any such questioning is intelligible. In Heidegger's terms, it is only within the horizon of the world opened up by our total behavior that any beings are disclosed to us at all. The sceptical question thus presupposes the intelligibility of the behavior in the context of which it is asked, and cannot put that intelligibility into doubt.

The realist might counter that it is not the intelligibility of our practices which is in question, but their reliability. Our practices might make sense, but nevertheless fail to accomplish their goals because they fail to correspond with the way the world is. Such an argument is developed by Hilary Putnam in "Reference and Understanding." Putnam begins by arguing that we can understand and use a language without reference to the truth conditions of what we say, and concludes that a theory of understanding does not require realism.

Nothing in this account of [language] "use" says anything about correspondence between words and things, or sentences and states of affairs. But Putnam insists that we cannot explain why those practices succeed (as
well as why some fail) without referring to a realist account of truth and reference.

The success of the ‘language-using program’ may well depend upon the existence of a suitable correspondence between the words of a language and things, and between the sentences of a language and states of affairs. For example, the instructions for turning on electric light on and off – ‘just flip the switch’ – do not mention electricity. But the explanation of the success of switch-flipping as a method for getting lights to go on and off certainly does mention electricity.

Putnam’s point seems to be this. All human practices are sufficiently intelligible to be understood internally, so that someone could learn those practices. But some of these practices are consistently successful in attaining their own goals, and some are not. We need to be able to understand and account for the difference between those which are successful and those which are not, for without such an account our successes are unacceptably mysterious. Putnam then argues that the only adequate basis for such an explanation is that successful practices have “a suitable correspondence between the worlds of [its] language and things.” Kuhn’s refusal to countenance a correspondence theory of truth thus leaves him bereft of an account of why science is successful.

Heidegger provides Kuhn with two responses to Putnam’s criticism. The first response is that an account of correspondence provides only a trivial explanation of the success of our behavior. It cannot be just that our successful practices are by definition based upon an adequate correspondence, for that would not provide such an independent explanation. If the success of the practice of flipping switches as a means of getting the lights on needs explaining, then so does our reference to ‘electricity’ in our scientific practices. And the same is true of our philosophical practice of talking about ‘reference,’ ‘truth,’ and ‘correspondence.’ Why does the practice of talking about correspondences between words and things succeed in explaining the success of our other practices? Putnam himself later came to acknowledge this.

Notice that a ‘causal’ theory of reference is not of any help here; for how ‘cause’ can uniquely refer is as much a puzzle as how ‘cat’ can.

Richard Rorty concluded a similar argument against Putnam by pointing out that since the theory of reference is part of our ‘total theory’ (the assertions derivable from our total behavior),

We discover how language works only within the present theory of the world, and one cannot use a part of one’s present theory to underwrite the rest of it.
But this formulation would not be sufficient for Kahn or Heidegger unless the notion of theory were broadened. It is not within the context of our current theoretical representation of the world that we discover how language works, but in the context of our total behavior (the interlocking in-order-to-for-the-sake-of structures of human practices, including the practice of scientific research). The world is that wherein our total behavior is situated. We are ‘in’ the world not as an object is in a box, but as one is ‘in’ science or business. We are involved in the world, and the world is the ‘wherein’ of our involvement. Where Putnam tried to explain the success of our practical involvement in the world by reference to the correspondence between our assertions and things, Heidegger insists that it is assertion which is derivative.

The pointing-out which assertion does is performed on the basis of what has already been disclosed in understanding or discovered circumspectly. Assertion is not … capable of disclosing beings in general in a primary way: on the contrary it always maintains itself on the basis of being-in-the-world. (p. 156; English translation, p. 199)

Heidegger’s second response to Putnam’s criticism would be to show that one can make the failure of our practices intelligible, without needing or having a realist explanation of our success. The initial problem is to understand how our practices can fail at all. If the world is only that wherein we engage in practical activity, and is established as the world by our involvement in such activity, how can that involvement be disrupted by things within the world? Our practices sometimes fail, such that our efforts to adjust them to correct their failures make them incoherent (we then say that our beliefs are false, and perhaps that our terms fail to refer). Such disruption often occurs to our scientific practices: research encounters pitfalls, problems, anomalies, and crises. But even crises disrupt the intelligibility of our behavior only partially. A total disruption would leave us worldless, with no possibilities at all, and as such would be totally unintelligible. Even disruption presupposes a horizon of intelligibility, a meaningful context of behavior which it ruptures. An idealist for whom our behavior constitutes the world might have difficulty understanding such disruption. But for Heidegger, our behavior ‘opens up’ the world and allows it to be, without making it what it is. The world is not fully subject to our control; indeed, the uncompleted project of science is to subject it to total control. And such control is always subject to disruption.

It is here that the self-interpretation of normal science turns out to be inadequate, for normal science conceals this vulnerability to disruption from us. It projects its methods and equipment as capable of being extended to solve any problem which it encounters, and its failure to do so at any point is unintelligible to it. In performing an experiment, for example, a scientist
assumes that all the factors relevant to its outcome are either already understood, or can be revealed and understood within the context of his or her current research practices. Without such an assumption, one could not regard the experiment as significant. The point is not that scientists are deduced that they can never fail, but that they are unequipped to understand where, when, or why the framework of their research practices will fail (the failure of specific practices can be understood, but only by taking for granted the success of most others, which allows us to make sense of the failure). To attain such an understanding of failure is to have succeeded, but the success transforms the context of their research practice. Anomalies, pitfalls, and crises are our encounters with the unintelligibility of failure (Heidegger would call them encounters with 'das Nichts,' the 'nothing'). We respond to this unintelligibility by adjusting our behavior so that in the new context they become intelligible; we can then give a coherent account of why, in light of our current practices, our prior ones failed. Such intelligibility is often achieved at the cost of reconstructing what past scientific practice was up to, and Kuhn has inveighed at length against the dangers of such anachronistic 'textbook history.' We can understand why phlogiston chemistry failed only by referring to phenomena which would be incomprehensible to it, and by abandoning some of its characteristic goals as pointless. This reflects what Kuhn has called the "essential tension" between tradition and innovation in scientific research. Only within a tradition is research possible; yet traditions break down and must be renewed through tradition-breaking innovation.

Heidegger sees this as an instance of a more fundamental tension between inauthenticity and authenticity in human existence. Inauthentic human existence projects possibilities for itself such that everything is under control, and one can then 'lose oneself' in one's concern with things around one. But inauthenticity leaves us unprepared for the breakdown of our projects, which is disturbing because it reveals the groundlessness of our absorption in those projects. Of course, we can never altogether cease being inauthentic, since to do so would leave us bereft of specific possibilities to take up. This is why Kuhn insists that in normal science, the fundamental presuppositions of one's practices do not and cannot usually come into question, and even when questioned, are not abandoned without an alternative to replace them. Authentic existence thus projects possibilities inauthentically, but holds itself open for their disruption. Such holding oneself open is always indefinite, because one's understanding cannot reveal when and where the disruption will occur. In authentic existence we engage in the social practices around us without being absorbed in them. Heidegger calls this openness to the unintelligible "resoluteness."
The phenomenon of resoluteness has brought us before the primordial *truth* of existence. As resolute, *Dasein* is revealed to itself in its current *factual* ability to be [Seinhalten] and in such a way that *Dasein* itself is this revealing and being revealed. The primordial truth of existence demands an equi-primal being, in which one maintains oneself in what resoluteness discloses. It [resoluteness] gives itself the current factual situation and brings itself into that situation. The situation cannot be calculated in advance or presented like something present-at-hand which is waiting for someone to grasp it. It merely gets disclosed in a free resolving which has not been determined beforehand but is open to the possibility of such determinations. What, then, does the certainty which belongs to such resoluteness signify? Such certainty must maintain itself in what is disclosed by the resolution. But this means that it simply cannot become rigid as regards the situation, but must understand that the resolution, in accordance with its own meaning as a disclosure, must be held open and free for the current factual possibility. The certainty of the resolution signifies that one holds oneself free for the possibility of taking it back—a possibility which is factically necessary. (pp. 307–8; English translation, p. 355)

Why does Heidegger speak of resoluteness, the ability to maintain oneself in the essential tension between tradition and the possibility of its disruption, as the primordial truth of existence? What sense does ‘truth’ have here? Aristotle remarked in the Metaphysics that

That which causes derivative truths to be true is most true.19

Since Quine’s “Two Dogmas of Empiricism,” most philosophers have agreed that sentences cannot be tested for truth one by one, but only as a whole.

The unit of empirical significance is the whole of science. The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which implies on experience only along the edges.20

Hence, it is the truth of our total theory which would establish the truth of its parts; this is to say, in Aristotle’s terms, that what is “most true” is our total theory. Heidegger would describe this as an account of the difference between what is true (sentences) and truth (that which allows our sentences to be true, i.e., our total theory). But Heidegger would argue that this view of the truth of what is true is mistaken. Our total theory is itself something which is true (or false), and the question can be asked, within what context is its truth determinable? For Heidegger, it is within the context of our total significant behavior, within *Dasein* and its possibility of authentic resoluteness, that our total theory is revealed and allowed to be true or false. As *Dasein*, the way of being we are in opening up a context of significant behavior, which is the truth of what is true.

Why is the locus of truth (not what is true or false) our total behavior rather than our total theory? Our total theory is itself something which is
coherence of others' practices is to discover that these were not significant practices (and that they are not Dasein).

Truth for Heidegger is thus not the being true or false of particular utterances or those of the ideal community of inquirers. Truth is the way of being open to the world which is constitutive of the possibility of inquiry at all. It is the openness (dis-closedness) of the whole in-order-to-for-the-sake-of-which structure within which particular beings (including ourselves) are freed to be encountered for the first time. Within this openness, both truth and falsity, success and disruption, are encountered. Since truth opens the possibility of disruption, it must be understood as authentic resoluteness, and not as the inauthenticity of normal science, or more broadly, normal discourse and intercourse with the world. There is no practice-independent truth which would allow one (total) theory to truly correspond with the world; the world is always more than what can be revealed of it, and all theories are underdetermined by it. Thus, we can never find (or even intelligibly conceive) of a truth which establishes the atemporal validity of our scientific practices. On the other hand, we have no need of such an account of the true version of the world. We can make sense of the success and failure of our scientific practices without reference to an ideal limit of total success, of the completion of science. The attempt to project such an ideal limit as the measure of our activities is inauthentic: an authentic understanding of our successes and failures always holds itself open for the possibility of its own failure. Kuhn's philosophy of science is an effort to understand the ongoing activity of science as open to its own disruption, through crises and revolutions. This explains how Kuhn can dismiss the realist demand for a context-free truth as the ultimate justification for the validity or invalidity of current scientific practice. Scientific research, in its success and in the indefinitely open possibility of its partial failure, is already in the truth.

NOTES

2. Ibid., p. 648.
true or false; in traditional terms, it can either correspond or fail to correspond with the way things are. This correspondence itself is established within the context of our total behavior. Only within this context do either things, assertions, or correspondences between them, become significant. ‘True’ is a predicate which only functions within the total context of our practices. This is what Heidegger meant by saying that assertion maintains itself on the basis of being-in-the-world.

It is important to realize that Heidegger is not espousing a relativistic concept of truth here. Truth is not internal to the particular practices of some particular natural kind of beings (Homo sapiens). A view of truth as relative to the practices or beliefs of particular beings would fall prey to the same criticism as did the view that the truth of what is true is our total theory (or the ideal total theory). For we could ask wherein this particular group of beings was revealed as what they are; and the truth of this revelation would be dependent upon the context wherein they were revealed. Truth is the being of Dasein as authentically resolute being-in-the-world, whereas our particular complex of practices is, as was argued earlier, only a mode of being-in-the-world. Our practices are not constitutive of truth; our practices being in the truth is rather constitutive of their being meaningful, and of our being Dasein.

We can see Heidegger’s point by comparing Heidegger’s argument with the argument by Donald Davidson and Richard Rorty, that the notion of “alternative conceptual frameworks” is incoherent. They have argued that in order to provide translations of the utterances of others, we must construe those utterances so that as many of their beliefs as possible are true according to us. This is not because we share most of our beliefs with other language communities; we cannot intelligibly argue that their beliefs are the same as ours or radically different without begging the question. The “principle of charity” is constitutive of our translation being a translation of a language. The alternative to such charitable translation is to deny that the utterances in question are in a language at all.

Heidegger would extend Davidson’s argument to the claim that most of a community’s practices, of which their multifarious language games are a part, successfully cope with the world, and that such overwhelming success is a prerequisite for understanding the possibility of failure. We can only make sense of these practices and of their success within the world as we find it in the course of our own behavior. This is not to say that our practices and beliefs are the basis for judging other practices, but that the world as revealed by our current practices is the basis for interpreting them. The coherence of their total behavior and the significance of things as disclosed within that behavior are presupposed in taking them to be Dasein.

‘Dasein is not a particular kind of being (Seiend), but a way of being (Sein) which particular beings may or may not be. To discover the total in-
5. Thomas Kuhn, *The Structure of Scientific Revolutions*, second edition (Chicago: University of Chicago Press, 1970), p. 126; all parenthetical page references to Kuhn will be to this work.

6. Martin Heidegger, *Sein und Zeit*, Urvoründerakte 4. Auflage (Halle: Max Niemeyer Verlag, 1935), pp. 203–5; English translation by John Macquarrie and Edward Robinson, *Being and Time* (New York: Harper and Row, 1962), pp. 247–49; all parenthetical page references to Heidegger will be to this work; I have very slightly altered some of the translations, although I have tried to keep as close to the standard version as is consistent with accuracy and readability.


8. Kuhn is thus suggesting that the exemplar is the most fundamental kind of scientific tool, and that an analysis into physical, methodological and intellectual tools such as I made above is derivative. I see no substantive disagreement between Kuhn and Heidegger on this point.

9. The quotation marks around "theoretical" suggest that Heidegger does not consider theorizing to be "theoretical" in this sense.


12. Ibid., p. xix.


16. Kuhn explicitly mentions the influence of Quine’s "Two Dogmas of Empiricism" on his work (p. viii). Donald Davidson implies that his attack on conceptual relativism in "On the Very Idea of a Conceptual Scheme," *Proceedings of the American Philosophical Association 17* (1973–1974):5–20, is a critique of Kuhn among others. In fact, Kuhn would accept Davidson’s argument as complementing his own work (if he does not, he ought to). In his reflections upon the work in the history of science which led him to his philosophical position, Kuhn even formulates something akin to a Davidsonian "principle of charity." See Kuhn, *The Essential Tension*, pp. x–xli.


23. Ibid., p. 100.


25. Ibid., p. 126.


28. The translators coined "factual" to distinguish "faktisch," that which is "in fact" the case within one's already understood situation, from "tatsächlich" ("actual"), the notion of a brute fact independent of context.

