The Philosophy of Science

An Interdisciplinary Perspective

The Philosophy of Science in an Interdisciplinary Context

Although STS or science studies is becoming an increasingly interdisciplinary conversation, there is still a gulf of understanding among the different constituent disciplines, especially between the philosophical and social studies wings. I use the term "social studies" to include historians, cultural studies researchers, and social scientists. Following the American usage, I use "social scientist" to refer to sociologists, cultural anthropologists, political scientists and policy analysts, economists, management and administration scientists, and some other researchers such as geographers. (In other countries the term "social scientist" has a much more restricted usage, generally referring to anthropologists, sociologists, and political scientists.) In contrast, the term "humanities" refers to fields such as history, literature and cultural studies, rhetoric, philosophy, and perhaps some of the more humanistically minded cultural anthropologists and sociologists. Sometimes "human sciences" is used as an umbrella term for the social sciences and humanities. Although the humanities/social science divide has been the subject of controversy within some disciplines, in the STS field the most significant disciplinary division has been between those who have some allegiance to traditional Anglo-Saxon and German philosophy of science and those who have a more social or cultural orientation. The term. "philosophy" is generally adequate to cover the first group (even though there is a continental philosophical tradition that is more influential in technology studies), but some other term is necessary to cover the second group of socially oriented social scientists and humanists. In the Anglophone world, "social studies" is probably the best term to designate descriptive, empirical research that includes the work of social scientists as well as humanities scholars in history, cultural studies, and other humanities fields. In the humanities, "cultural studies" is sometimes used as an umbrella

term, but the term would exclude several of the social sciences. Thus, I will use "social studies" as a generic but imperfect term to cover social scientists and those humanists who are concerned with social aspects of the world they study.

This chapter begins the survey of key concepts in science studies by reviewing the philosophy of science through an interdisciplinary lens. Introductions by professionally trained philosophers can accomplish a much more detailed and inclusive level of discussion than will be achieved here.1 This chapter complements those introductions by focusing on two aspects of the philosophy of science in the general interdisciplinary setting: misunderstandings and possibilities in the sometimes acrimonious dialogue between philosophers and social studies researchers, and the application of the philosophy of science to the problem of designating good criteria for choice among major theories or research programs. Following Steve Fuller (1988), I will seek a middle ground in the dialogue between philosophers and social studies researchers by beginning with the distinction between prescriptive and descriptive approaches to science and technology. Although some philosophies of science (such as evolutionary epistemology) are descriptive, for the most part it is helpful to see the central problem of the philosophy of science as making clarifications that could help scientists decide how they should go about improving the ways they think about and do science. Fuller has probably developed the prescriptive role of the philosophy of science more clearly than any other philosopher, and he has introduced the term "social epistemology" for one type of prescriptive use of the philosophy of science. In his words, the fundamental question of social epistemology is,

> How should the pursuit of knowledge be organized, given that under normal circumstances knowledge is pursued by many human beings, each working on a more or less well-defined body of knowledge and each equipped with roughly the same imperfect cognitive capacities, albeit with varying degrees of access to one another's activities? (1988: 3)

Although social epistemology brings the philosophy of science into the realm of prescriptive work for science in society, I will also interpret the traditional philosophy of science as prescriptive in a more narrow sense: its contribution to understanding how to make better scientific theories and explanations. Fuller's work is a good starting point because it clearly locates the division of labor between philosophy and social studies in the distinction between prescription and description, or normative versus empirical

approaches (xi). Although philosophers certainly describe science and technology, and social studies researchers often engage in discussions of policy and activism that can be explicitly prescriptive. Fuller's distinction is useful as a way of moving toward a productive dialogue between philosophical and social studies outlooks on science and technology. In other words, philosophy may be helpful to social scientists and humanists when they are in the prescriptive mode, and likewise the research of social and cultural studies may be helpful to philosophers when they are making descriptive claims about science and technology.

This review of some concepts in the philosophy of science will focus on one type of prescriptive question: what grounds should scientists use to justify their choices among major theories or research programs? To answer this question, as in other philosophical problem areas, philosophers pursue a dialogue of arguments and counterarguments. Although the dialogue may never result in a final consensus, the back-and-forth procedure makes it possible to progress by finding the shortcomings in previous solutions and providing alternatives that answer those shortcomings. This review will cover the following major positions: positivism, conventionalism, falsificationism, historicism, naturalism/realism, constructivism/relativism, and feminism.

Positivism

In the philosophy of science "positivism" is shorthand for logical positivism or logical empiricism, terms that are not exactly identical but will be treated so here for the sake of simplicity In STS circles the term "positivism" is usually associated with the philosophical positions that emerged around the Vienna Circle. However, for social scientists the word "positivism" may also refer to the thought of Auguste Comte, a nineteenth-century French social theorist. Comte believed in the unity of sciences and supported an evolutionary theory of scientific progress that led to a positive stage that happened to match his nineteenth-century understandings of science. In this sense the term "positive" might be glossed as "I'm positive I'm right because my position is founded on science." In the humanities and cultural studies, another use of the word sometimes appears. "Positivist" can be a pejorative label for (i) someone perceived to have a simplistic and uncritical view of science, and/or (2) someone who wishes, in a very simplistic way,

to base social science or humanities methods on an ideal version of those in the natural sciences. In polemical debates, the label "positivist" is usually opposed to "postmodernist," although in debates where these labels get hurled back and forth there is usually little substance.

Returning now to philosophical positivism, the Vienna Circle was a group of philosophers whose work flourished during the interwar period. Many were trained in physics and influenced by British formal philosophy in the tradition of Bertrand Russell. Because some were Jews and some were leftists, their social position set the stage for the fragmentation of the circle when the Nazis came to power. Most of the circle's members moved to Britain or the United States, where they had an important impact on the Anglo-American philosophy of science. Members of the Vienna Circle included Moritz Schlick, Ernst Mach, Otto Neurath, and Rudolf Carnap; A. J. Ayer, Herbert Feigl, Kurt Godel, and Hans Reichenbach were among those associated with the circle. Karl Popper maintained close ties with some of the members of the circle, but he was not considered a true positivist.

Perhaps the key concept associated with the positivist philosophy of science, at least in its early versions, was the verifiability principle, which held that statements are meaningful if verifiable. (A weaker version of this principle held that statements are meaningful if confirmable to some degree.) Although some statements could be verified by logic or by definition, the more important means of verification was experience. For example, the sentence "Crows are fifteen" is meaningless because the sentence cannot be confirmed as either true or false in the sense that the sentence "Crows are black" can be.2 Although the verifiability principle lost importance with the passage of time, the empiricist interpretation of meaning continued to underlie the sharp distinction that positivists often drew between theoretical terms and observational terms. Theoretical terms such as energy in physics can therefore be interpreted as meaningless in the strict sense because they are not observable directly or even relatively directly through measuring devices.

The interpretation of meaning as reference contrasts sharply with the semiotic understanding of meaning that is common in the humanities and some social sciences. This is one of the first major opportunities for cross-disciplinary misunderstandings. For example, under Ferdinand de Saussure's definition of value, the meaning of a statement derives from its relative position in various codes of semantic difference.3 Thus, the meaning of the

sentence "The coyote is laughing" is understood through a series of contrasts. These include the contrast between the coyote md otb^ rnvwais <v. is not a raven, a swan, or a crow); the contrast between laughing and other activities (it is not remaining black, turning white, or flying into a wall); the grammatical juxtaposition of coyote and laughing in a sentence compared with other possibilities; and the semantic mapping of associations of "coyote" and "laughing" (for example, the coyote clan may harbor a tricksterish shaman whose attributes are like those of the coyote totem). This view of meaning has been enormously influential in contemporary linguistics, cultural anthropology, history, and literary/cultural studies through the various intellectual currents known as structuralism, poststructuralism, and deconstruction.

Thus, one view of meaning associates a word with the thing to which it refers, whereas another view opens up a world of interpretations. In contrast with the positivists' project to formulate a universal, formal language, senuotic approaches to meaning interpose languages and cultures as a necessary point of reference. Although the differences between the positivist and semiotic views of meaning are profound, the two can nonetheless be made compatible. In effect, they are two ways of looking at the same linguistic fact: reference and semiotic value. This is the first of the clarifications that I wish to make regarding the "duck-rabbit" (or crow-coyote) nature of philosophy and social/cultural studies. As is the case with the gestalt diagram that switches back and forth between a duck and a rabbit, it is not necessary to choose between the two views of meaning. Rather, it is better to understand how each is bound up in a different set of questions and issues.

One misleading way to interpret this difference is to compare positivist and semiotic views of meaning with what are sometimes called correspondence and coherence views of truth. As a rough first approximation, a correspondence view of truth holds that statements are true when they refer to things that exist, whereas a coherence view holds that statements are true if they can be situated logically within a coherent body of knowledge. It would be a great misunderstanding to argue that the positional theory of meaning associated with de Saussure implies that those social science and humanities fields that use Saussurean and post-Saussurean theories of meaning will assume a coherence theory of truth. Because social scientists such as linguists are making theories of linguistic observations, philosophically they may hold either a correspondence or a coherence view of truth. Thus, a semiotic view of meaning and a coherence view of truth

should not be confused, even if there may be some cases of specific theorists for whom they coincide.

Consistent with the view of meaning that was anchored in observation, positivists distinguished sharply between theory and observation. Carnap, for example, distinguished simple, local observations from general empirical laws that remained grounded in observational language, and in turn he distinguished empirical laws from theoretical laws or theories, which were not grounded in observational language (1995: 27). The gap between theoretical terms (e.g., thermal energy) and observational terms (e.g., a temperature measurement) posed a problem of translation. Positivists attempted to resolve the problem by proposing what were variously called correspondence rules, rules of operationalization, bridge laws, or a dictionary. These rules or definitions made it possible to translate across categories.

Consistent with the sharp distinction between theoretical and observational terms is the doctrine of instrumentalism, the view that theories are computational devices for predicting or explaining observable phenomena. Note that in the social sciences instrumentalism may refer to a type of analysis that interprets the motivations for action in terms of gain or turf protection, usually by using economic or military metaphors. In philosophy, instrumentalism contrasts with ontological realism, which holds that theoretical terms capture something of the deeper structures of reality. Some positivists were suspicious of the metaphysical (and therefore meaningless) nature of realist claims for theoretical terms. In this sense, one can argue that positivists who endorsed instrumentalism were not realists. In his later work even Carnap seemed to recognize the power of the realist argument that theoretical terms tended to become observational terms over time, but he preferred to reframe the debate with the question, "Shall we prefer a language of physics (and of science in general) that contains theoretical terms, or a language without such terms?" (1995: 256).

With the vocabulary now established, it is possible to turn to the problems of justification, induction, and theory choice. Hans Reichenbach (1938) made popular the central distinction between the context of discovery and that of justification.4 As Ian Hacking explains, philosophers are more concerned with the latter issue. In order to justify a theory or law, one asks,

> Is it reasonable, supported by the evidence, confirmed by experiment, corroborated by stringent testing? These are questions about justification or soundness. Philosophers care about justification, logic, reason, soundness, [and] methodology. (1983: 6)

In a similar vein, Karl Popper (1959: no) used the metaphor of a courtroom to describe the philosophical understanding of justification. Scientific knowledge is like the verdict of a jury, whereas philosophical justification is like the judgment of a judge. A verdict simply exists, whereas a justification or judgment can be correctly or erroneously related to a general set of principles.

Philosophers of this persuasion therefore viewed the context of discovery-how scientists arrive at a theory or law-as the realm of history, psychology, or sociology. They were quite willing to admit a role for social factors in science, as long as these factors were relegated to the context of discovery. From this perspective, philosophers see their task as the study of the rational aspects of science, whereas social studies are relegated to the arational aspects of science (Laudan 1977). However, this view is very difficult to maintain because social action is also rational, and rationality is socially conditioned. It is possible to develop historical and sociological descriptions of rational processes such as methodology or logic, and this characterization of the division of labor between social studies and philosophy is bound to fail. The misunderstandings are eliminated when the key disciplinary division is seen as descriptive versus prescriptive work.

Ronald Giere understands this fundamental point. He notes that for Carnap, and presumably many other philosophers in the positivist tradition, philosophy is "the study of how, a priori, an ideally logical scientist should think" (1988: 24). This is an important point, because some philosophers seem to think they are describing historical cases when in fact they are reconstructing idealized scientists to derive prescriptive accounts. Giere adds, "For logical empiricism, then, the gap between the psychology or sociology of science and the philosophy of science is like the gap between 'is' and 'ought.' It is logically unbridgeable" (24). The fallacy of positivism was not to make a distinction between is and ought, but to view philosophical prescription as founded in rational processes that were somehow outside society and culture.

The choice of the word "justification" points to one complex of values that underlay the positivist program. The concept of "justification" in Western science and philosophy might be compared with its sibling in Western religion. The Calvinist wing of the Protestant Reformation argued for justification by faith alone in contrast with justification by good works. Justification in this context meant the basis upon which one would be deemed a good Christian and therefore able to pass muster when confronted with the gatekeepers of heaven. Prior to the Reformation, the

pope sometimes remitted penalties for sins, including those that would be paid for in purgatory, and he sometimes granted these favors in exchange for monetary contributions to the church. The doctrine of justification by good works was therefore extended to financial contributions to the church, and in effect rich people were buying a stairway to heaven. In arguing for justification by faith alone (which in turn sanctioned good works), Protestants such as Martin Luther were attempting to rationalize the justification process.

In a similar way, positivists wanted to rationalize the justification process in science. To do so, they attempted to anchor justification in logical rules that were derived from a process of reasoning akin to mathematics: justification by reason or logic alone. Like theologians, they viewed their logical systems as located outside the influence of human history or culture. Although this position may strike readers today as philosophically theological and anthropologically naive, let us for the moment follow out their arguments before considering the counterarguments and alternative positions.

One central problem in the philosophical justification of scientific knowledge was the problem of induction, that is, how to derive general empirical laws from observations. As Carnap asked, "What justifies us in going from the direct observation of facts to a law that expresses certain regularities of nature?" (1995: 19). In deductive logic, conclusions follow with certainty from premises as long as the proper logical rules are followed. For example, if all swans are white and if this bird is a swan, then this bird is white. (Another example of deductive logic is the style of mathematical proof that many people had to learn in high school.) However, the certainty of deductive conclusions did not apply to inductive logic, that is, deductively invalid inferences from experimental data to empirical laws. Carnap defended the idea that it was possible to work out an inductive logic for the confirmation or justification of empirical laws. Although he rejected the view that this logic could be simplified to the point of an inductive machine, he believed that it was "in many cases possible to determine, by mechanical procedures, the logical probability, or degree of confirmation," of a hypothesis based on a set of observations (1995: 34). Thus, he could determine the logical probability, or degree of confirmation, of a prediction or even a set of laws on the basis of observations.

A much larger justification problem emerges when two scientists induce two different theories. What criteria should one use to choose between two well-formulated theories? This is perhaps the most interesting question

in the philosophy of science. If philosophers can help sort out this question, they can contribute to making theory choice work better in both the natural and social sciences. Clearly, a first answer to the question is that one should prefer the theory that corresponds better to the empirical laws or observations. Find out which theory fits all the observations better, either by examining the set of observations already available or by performing a crucial experiment that will allow a choice between the two theories. In practice, this approach often works in science, and little else is needed from philosophy.

However, the situation is not always so simple. In some cases the two theories are evidentially indistinguishable; in other words, they can explain the same set of facts. In this case the theory that can predict and explain new laws or observations is clearly preferable. However, there is also a stronger form of evidential indistinguishability, which occurs when "two theories lead in all cases to exactly the same predictions" (Carnap 1995: i5i). Some realists argue that positivists bite the bullet when they face this kind of evidential indistinguishability. The choice between two theories that meet the stronger form of evidential indistinguishability is metaphysical and therefore not part of science.

However, in this circumstance Carnap admitted simplicity as a criterion of choice. In his case study of the choice between Euclidean and non-Euclidean geometry, he even distinguished between two types of simplicity. Non-Euclidean geometry was more complicated but it greatly simplified the system of physical laws, and Einstein and his followers opted for the systemic form of simplicity rather than the computational form (Carnap 1995: 162-64). The simplicity criterion has a long history dating back at least to William ofOckham and is known as Occam's (Ockam's, Ockham's) Razor or the rule of parsimony This principle states that entities should not be multiplied beyond necessity Carnap therefore updates a very old criterion for theory choice and clarifies which type of simplicity is better. The problem appears to be solved.

Interlude: The Unity of Science Thesis

The unity of science thesis provides one line of continuity between nineteenth-century positivism and twentieth-century logical positivism. Nineteenth-century positivists John Stuart Mill and Auguste Comte built grand

schemes of the relations among the sciences in which the hierarchy of scientific disciplines also corresponded to a hierarchy of phenomena. As then, unifying schemes for the sciences today usually involve a defense of reductionism. For example, the biological level can be reduced ultimately to biochemistry and physics, and mental states can be reduced to neural states.

Fuller notes that there were significant variants in the unity of science thesis, even during the 1920S and 1930s when the thesis was strongest.5 In Europe, the unity of science thesis emphasized the mathematical unity of the sciences, and mathematics or the mathematical side of physics was seen as the model science. Fuller argues that in Germany, where chemistry and physics were associated with the losing World War I effort, positivists tended to distance themselves from those fields. In contrast, in the United States, where technology was associated with the war victory, theorists such as John Dewey linked science and technology.

As a philosophical proposition, the unity of science thesis holds that theories across different scientific disciplines should not be contradictory, even when the different levels of science and observations are not reducible or not yet reducible. Thus, it should be possible to use different theories conjointly to predict new observations. At the level of methods and justification criteria, the unity of science thesis would hold that a philosophy of science as it is understood in one discipline could be transported to other disciplines. In other words, philosophies of science based on physics could be generalized to other scientific disciplines.

Probably the greatest weakness in this position comes when the philosophy of science is generalized from the natural sciences to the human sciences. The tendency for philosophers of science to use physics as their model has been another source of cross-disciplinary misunderstandings with social scientists and humanists. Some humanities scholars have defended a radical difference between the natural and human sciences in terms of a methodological difference of explanation versus interpretive understanding. Thus, in the more humanistic social sciences and in many of the humanities, the preferred method is some type of hermeneutics or interpretation rather than explanation. However, others have held (and I would agree) that interpretive understanding is merely a type of explanation. Thus, it is possible to maintain a unitarian view in terms of this methodological issue, even if one does not accept other types of unity such as a reductionism of the social/symbolic to the neurological/biological (a project about which I

am very skeptical). Still, the emphasis on predictive explanation in physics seems impossible for much of the phenomena studied by the social sciences. Many social phenomena are far too complicated to be predictable, at least at the present time. Consequently, as social scientists read philosophies of science that draw their arguments from physics or even other natural sciences, they immediately see the failure of some of the arguments to transport to their own field and they become skeptical of the entire enterprise. The counterargument of some philosophers that the social sciences are merely undeveloped or inferior sciences only makes matters worse by underscoring the social scientists' and humanists' perception that philosophers of science do not understand their endeavors.

Another dimension of the natural/human science divide is the cultural meaning attributed to the difference. The difference between the human sciences and the natural sciences is a reproduction, internal to the field of science, of the division between science and cultural domains that are understood as not science, such as the arts and to some extent (via the tradition of biblical hermeneutics) religion. This difference of viewpoints is often so dramatic that C. P. Snows old formulation of "two cultures" continues to be relevant (1959). Feminist science studies analysts have shown how the classic divide between the natural sciences and the humanities, and the associated but not identical divide between hard and soft sciences, are laden with highly gendered imagery (Keller 1985: 33, 77; Bleier 1986: 6). These divisions are recursive; in other words, gendered divisions often occur not only in formulations of the difference between science and nonscience or between the sciences and the humanities (with the social sciences mediating the divide), but across disciplinary divisions in the sciences (e.g., physics versus, for example, biology) or even across divisions within disciplines (e.g., molecular biology versus ecology, or experimental versus clinical psychology). Thus, feminist theory points not only to one type of disunity of the sciences but also to the masculinist biases that may be built into some formulas of the grounds for the unity of science, particularly the claim ofreductionism.

A related division is the distinction between idiographic and nomothetic science. Idiographic science is the study of historical particulars, as in the natural history of a geological or ecological region, or a historical, textual, or ethnographic study in the humanities/social sciences. Nomothetic science is characterized by the search for general laws. This division occurs both in the natural sciences (biology versus natural history) and in the social sciences and humanities (sociology versus history). Even within some

disciplines different phases are considered idiographic and nomothetic. Thus, within anthropology ethnography (the description of a people) is idiographic, whereas ethnology and cultural/social anthropology seek general patterns. The different goals, methods, and histories of the various idiographic and nomothetic disciplines suggest another basis for questioning the unity of science thesis.

Another way in which the social sciences complicate the question of the unity of science is through the use of the disunity of science as a useful research tool in social studies of science. For example, as Sheila Jasanoff (1990) has shown, the standards for judging acceptability in regulatory science and research science vary greatly. In the former, legal versus statistical tests of sufficiency play a relatively greater role. In the next chapter I will cover some other examples of the disunity of the sciences in empirical social science research. Robert Merton (1973), for example, distinguished between codified sciences, or those vertically built on previous findings, and noncodified sciences, in which new research horizontally adds new empirical material but does not necessarily build in a linear way on previous research. This distinction is similar to the nomothetic/idiographic distinction but not quite identical to it. He found the distinction useful in formulating some empirical patterns on age effects and cumulative advantage processes. Furthermore, a number of measures show stable differences across the range of sciences. Those measures include some of the findings of the institutional sociology of science to be discussed in the next chapter, such as differences across disciplines in acceptance rates for journal submissions and the proportion of publications in articles and books.

Thus, social science research suggests ways in which the unity of science thesis is limited, and in this respect it has philosophical implications. Many philosophers today are equally skeptical about such abilities to generalize across fields. As Paul Durbin has noted, "Now philosophy of science is itself a multiply diversified field . . . and each of the sciences, with endlessly multiplying subspecialties of all sorts, goes its own way in total defiance of any unity of science model" (1988: 334). It would still be possible to defend the thesis on one or several dimensions while recognizing a disunity of science on other dimensions. Likewise, it would still be possible to formulate a prescriptive position that views the unity of science as a goal to be achieved. However, the empirical research on the ways in which the sciences are not unified can contribute to assessing how realistic and desirable it would be to invest time in formulating such projects.

Conventionalism

Conventionalism is historically the first major alternative to the positivist philosophy of science. Although conventionalism is usually identified with Pierre Duhem, it may be more accurate to view Jules Henri Poincare as the originator of this position (Gillies 1993: 6y, 90). Conventionalism holds that scientific laws (such as those of Newtonian mechanics) and mathematical axioms (such as those of Euclidean geometry) are neither experimental inferences nor a priori knowledge but instead are disguised definitions or conventions. Two specific theses associated with conventionalism are underdetermination and theory-ladenness.

The underdetermination thesis of Duhem and W. V. Quine holds that a theory can be maintained in the face of contradictory observations provided that an adjustment is made to the auxiliary hypotheses derived from the theory.6 In this sense theories are underdetermined by evidence. Thus, a core theory can be protected from refutation if after-the-fact changes are made in auxiliary hypotheses. An auxiliary or ad hoc hypothesis is a modification in a theory made in the face of a refuting instance in order to cover that refuting instance but no further problem.7 An example is epicycles, little circles within the planetary orbits, which were used in early models of the solar system to account for observations of planets that violated the ideal of perfect circular motion.

Related to the underdetermination thesis is the thesis of the theory-ladenness of observations, which was defended by Duhem and later by Paul Feyerabend, Thomas Kuhn, and Norwood Hansen. Although it is now generally accepted that theories shape, constrain, or color observations, in most cases the conditioned nature of observations is not considered to be strong enough to prevent theory choice based on observations obtained in research protocols designed to evaluate or test competing theories.

Regarding conventionalism, two limitations should be kept in mind. First, underdetermination and theory-ladenness are not necessarily as damaging as they first appear. In practice it may be possible to design experiments in which auxiliary hypotheses are more highly confirmed by existing evidence than the theory to be tested (Laudan 1990: 4.2 &.). Second, acceptance of the conventionalist account does not necessarily imply that it is impossible to maintain general prescriptive criteria for theory choice. One could follow Duhem and accept conventionalism as argued, but make theory choice subject to correspondence at a general level and coherence for specific theories. Thus, Duhem argued that a confirmation criterion

could be maintained for a body of theories as a whole that must approximate the totality of empirical laws or generalizations. For specific theories, theory choice criteria should include internal consistency and consistency with other theories (Duhem 1982: 220). Furthermore, Duhem did allow for a degree of disconfirmation to settle a dispute between empirically equivalent theories. In a description of theory change that sounds similar to Kuhnian paradigm shifts, he wrote, "We may find it childish and unreasonable for the [scientist] to maintain obstinately at any cost, at the price of continual repairs and many tangled-up stays, the worm-eaten columns of a building tottering in every part, when by razing these columns it would be possible to construct a simple, elegant, and solid system" (1982: 217). Of course, the classic example is the heliocentric view of the solar system that assumes simple elliptical orbits instead of the whistles and bells of epicycles. In this comment, Duhem also seems to be adding simplicity and elegance as prescriptive theory choice criteria to his more general criterion of consistency. Note that the simplicity criterion was also endorsed by Carnap, so one sees that on this issue the two positions of positivism and conventionalism are not as contradictory as they may first appear.

Falsificaf-ionism and the Demarcation. Problem

Popper is known for having developed a critique of positivism that avoided conventionalism. Often considered a positivist, he was not a member of the Vienna Circle and held views at odds with leading figures such as Carnap. lan Hacking provides a succinct formulation of their differences:

> Carnap thought that meanings and a theory of language matter to the philosophy of science. Popper despised them as scholastic. Carnap favored verification to distinguish science from nonscience. Popper urged falsification. Carnap tried to explicate good reason in terms of a theory of confirmation; Popper held that rationality consists in method. Carnap thought that knowledge has foundations; Popper urged that there are no foundations and that all our knowledge is fallible. Carnap believed in induction; Popper held that there is no logic except deduction. (1983: 4-5)

On the last point Popper (1963) resurrected David Hume's argument against inductive inference. In other words, just because all swans observed up until time (are white, there are no logical grounds for concluding that the next observation will not be a black swan. Hume argued that inductive

logic led to an infinite regress: I do not know whether one additional observation will confirm or disconfirm the law, so I make an additional observation. However, I am still in the same position, so I make an additional observation, and so on. I am caught in an infinite regress. (As will be discussed, this argument reappears in the sociological literature as an analysis of the experimenter's regress.)

The view that scientists induce theories or laws from observations was, according to Popper, a myth. Instead, they jump to conclusions, conjecture a hypothesis, and then try to refute it through observations. Thus, they do not proceed by confirming theories or laws, but only by failing to falsify them and therefore becoming increasingly convinced of their correctness. In principle any theory or law is defeasible, and Popper therefore also defended fallibilism, the view that no beliefs are immune from error.

Note that Popper criticized the positivist view of induction by using what appears to be a descriptive argument: scientists do not in fact extract theories or laws from observations. However, he does not have any credible research from the historical record or from social science surveys to back up the argument. Thus, one way of defending positivism against Popper would be to draw on empirical research to argue that scientists do in fact reason in a probabilistic manner similar to the inductive logic proposed by Carnap. In turn, a Popperian might counterargue that recourse to the historical record or empirical social science studies would be largely irrelevant. Even if scientists do in fact reason this way, they should reason as a Popperian scientist does by starting first with a theory or a law and then attempting to refute it. This example is one case of the slippages between description and prescription that sometimes occur in philosophical arguments.

Although Popper rejected the positivist view of induction, in other ways he remained very similar to the positivists. For example, his account of science was stiQ a rational one and therefore he wss stiQ engaged in a project of justification in a general sense. He had merely switched the problem from how to induce good hypotheses to how to refute bad ones. Yet the problem of evaluating theories or laws still required justification, and this justification was anchored in empirical practices akin to those of the positivists. Popper still believed that testing was the key to accepting or rejecting a hypothesis (1963). In this sense he was squarely aligned with the positivists, and consequently his falsificationism position has sometimes been called an extreme form of verificationism. One way of expressing this continuity is in terms of the "hypothetico-deductive method." A positivist interpretation

was that if a hypothesis is true and if a confirming observation is true, then the hypothesis is confirmed to some degree. Popper tweaks this method by arguing that if a hypothesis is true and if a confirming observation is false, then the hypothesis is not confirmed.

Popper originally developed his falsificationist argument as a solution to the demarcation problem, or how to distinguish science from nonscience. From the viewpoint of positivists (or at least some positivists), a claim that was in principle verifiable (or at least subject to testing and some confirmation) could be considered meaningful and scientific. A pseudoscientific statement was therefore in principle not capable of confirmation. Concerned with what he saw as the pseudoscientific successes of Marxism and psychoanalysis. Popper formulated falsifiability as a better demarcation criterion.

Popper's formulation of falsificationism as a valid demarcation criterion is not the only possibility. Thomas Gieryn (1994) argues that Robert Merton's formula of a set of universalistic norms represents another attempt to resolve the demarcation problem, as does Thomas Kuhn's criterion that sciences have a paradigm. Mario Bunge supplied eight negative criteria that served as indicators of nonscience (1982). Larry Laudan (1983) argues that the demarcation criteria of philosophers such as Aristotle, Carnap, and Popper were developed to rule out specific cases (such as psychoanalysis and Marxism for Popper), but the criteria all would allow other cases of nonscience to pass as science. He argues instead that a more honest approach would be to develop specific arguments against specific inadequacies of apparent pseudosciences rather than to develop universal criteria for which exceptions are likely to be found (cf. Fuller 1988: ch. 7). Subsequently, Charles Taylor (1996) surveyed a wide range ofSTS approaches to the demarcation problem and argued that they could be incorporated into a broader rhetorical framework.

The demarcation problem may seem to be an obscure philosophical issue, but it has direct policy implications in a number of areas. One example is the legal problem of determining criteria for acceptable expert testimony. In the United States a long-standing criterion was based on the 1923 Frye ruling, which defined the scientific by a criterion of general acceptance in the field in which the claim is made. In philosophical terms, this amounts to a grounds for theory choice based on a consistency criterion. In the context of legal disputes, there is a high danger of conservative bias in research fields that are not very autonomous and instead are more clearly structured by corporate, professional, or other interests (as in, say,

research on environmental carcinogens or pharmaceuticals). Rule 702 of the Federal Rules of Evidence, issued during the 19705, allowed greater leeway in expert testimony by basing acceptable expertise on knowledge, skill, training, or experience. However, this rule resulted in what some considered to be the problem of junk science. A 1993 Supreme Court ruling (Daubert v. Merrell Dow Pharmaceuticals, Inc.) developed four guideposts, which included the Frye criterion but also added testability, peer review and publication, and a declared (and presumably) low error rate.8 One can see how the courts have struggled with the very difficult problem of demarcation. As attorney Richard Jaffe (1996) has pointed out, the implications are enormous, given the fact that the Frye and Daubert criteria may work against plaintiffs who rely on the expertise of fields such as environmental medicine.

Popper also provided prescriptive criteria for theory choice. He agreed with a criterion of simplicity, and he used the theory of gravity as the example: "The new theory should proceed from some simple, new, and powerful, unifying idea about some connection or relation (such as gravitational attraction) between hitherto unconnected things (such as planets and apples) or facts (such as inertial and gravitational mass) or new 'theoretical entities' (such as field and particles)" (1963: 241). Note that this criterion also includes accuracy; in other words, it takes for granted the argument that the theory will explain a set of facts or the same set of facts as a rival theory. Second, the new theory must be independently testable, that is, in addition to explaining accepted evidence in a better way, it must have some new and testable consequences. Third, in addition to passing attempted refutations, it must make successful, new predictions of new effects. Thus, in developing his criteria for theory choice, Popper pushed the falsificationist position to an extreme by narrowing falsification to successful, new predictions. The criteria may have worked for the examples from physics that he was thinking about, but they are problematic for other sciences, especially the social sciences and other sciences that work with phenomena too complicated to be predicted.

Historidsm

One major criticism ofPopperian falsification, which philosophers such as Imre Lakatos recognized, came from Thomas Kuhn. In The Structure of Scientific Revolutions, Kuhn argued that scientists often continue to work

under a theory or set of theories even when faced with anomalies or refuting instances. However, Kuhn's work had a much more important place in the philosophy of science than merely providing an argument against falsificationism. His work contributed gready to what philosophers of science call historicism, the period and style that responded to positivism/Popperism and antedated naturalism, roughly from the l96os to the 1980S. This tendency in philosophy-epitomized in the work of Kuhn, Paul Feyerabend, Stephen Toulmin, and to some extent Imre Lakatos-rejected the lack of concern among positivists, Popperians, and conventionalists with the historical record and instead advocated a greater role for historical facts in philosophical argumentation. The emergence of historicism also brought about what anthropologists call the "peace in the feud" between Popper and the positivists. Hacking argues that in contrast to Kuhn-at least in The Structure of Scientific Revolutions-Carnap and Popper held many similar views: observation can be sharply distinguished from theory, knowledge growth is cumulative, science has a deductive structure, terminology is or ought to be precise, there is a unity to science, and a distinction can be maintained between the context of discovery and that of justification (Hacking 1983: 4-5).

Kuhn's Structure of Scientific Revolutions had a substantial impact on both the philosophy and social studies of science, but after the 1970s the impact was increasingly that of a foil against which other positions and theories were articulated. In the sociology of scientific knowledge, the various positions developed during and after the 1970s rapidly replaced Kuhnian analysis, as did naturalistic and realistic approaches in philosophy. Furthermore, as the alternatives were formulated, the novelty of Kuhn's work also came into question. Many of Kuhn's sociological ideas had antecedents in the 1930s studies ofLudwik Fleck (1979) on thought styles and collectives (a social unit in science such as a discipline, network, or community), and Kuhn's philosophical arguments also had predecessors in the conventionalist tradition within the philosophy of science. The final verdict is that as a sociologist Kuhn has been shown to be more or less Mertonian, and as a philosopher he has been read increasingly "pace [Dudley] Shapere, less [as] a revolt against positivism than a continuation of it."9

By the 1990s the importance of The Structure of Scientific Revolutions within STS was generally seen as historical rather than contemporary, and many regarded the historical influence as a conservative one in the sense that it continued rather than challenged fundamental theories in social studies and philosophy. However, outside STS circles Kuhn's work contin-

because opponents may be able to sidestep translation and move to direct rehearsal of procedures and protocols. This argument has become more important as social scientists have studied science as practice rather than merely as theory making (Pickering 1992). Furthermore, opponents often are quite adept at understanding the terms of the opposing side (MacKenzie andBarnes 1979: 200).

One implication of incommensurability among grand theories or paradigms is that change may be more like a conversion process than a change of minds led by argument and evidence. As a result, Kuhn followed Max Planck (1949) when he argued that paradigm change is often associated with a shift of generations, in which an older paradigm becomes extinct as an older generation dies off. However, Kuhn also denied criticisms that he had reduced the problem of theory choice to "mob psychology." Instead, he defended a loose set of more or less universalistic criteria for theory choice, thus heading off criticisms that his theory of paradigm change reduced theory choice to epistemological relativism. Kuhn listed the major criteria for theory choice as the following:

> First, a theory should be accurate: within its domain, that is, consequences deducible from a theory should be in demonstrated agreement with the results of existing experiments and observations. Second, a theory should be consistent, not only internally or with itself, but also with other currently accepted theories applicable to related aspects of nature. Third, it should have broad scope: in particular, a theory's consequences should extend far beyond the particular observations, laws, or subtheories it was initially designed to explain. Fourth, and closely related, it should be simple, bringing order to phenomena that in its absence would be individually isolated and, as a set, confused. Fifth-a somewhat less standard item, but one of special importance to actual scientific decisions-a theory should be fruitful of new research findings: it should, that is, disclose new phenomena or previously unnoted relationships among those already known. (Kuhn 1977: 321-22)

Because Kuhn referred to his theory choice criteria as values, he evidently believed that he was describing a set of values that indeed guided scientists in action. His criteria therefore apparently doubled as a description of, on the one hand, what he thought (without doing any empirical research) scientists actually held as values that guide theory choice and, on the other hand, a prescription of what he thought should guide theory choice. Subsequent empirical research by social scientists has questioned the Kuhnian argument that scientists' action is in fact guided by these or other lists of universalistic values, such as the complementary list of institutional values

known as Mertonian norms. However, when interpreted as prescriptive criteria, Kuhn's "values" no longer suffer from the problem of lack of empirical evidence.

Thus, I suggest viewing Kuhn's values as prescriptive theory choice criteria. From this perspective, Kuhn synthesizes theory choice criteria as articulated by Carnap, Duhem, Popper, and other philosophers. Note that the accuracy criterion could be reconciled with some type of verificationism or falsificationism. Scope and fruitfulness are similar, and they could be interpreted as corollaries of the accuracy criterion when applied to future predictions. The consistency criterion is similar to the theory choice criterion favored by conventionalists like Duhem, and simplicity was also advocated by Carnap, Duhem, and Popper. In fact, because simplicity must be judged against a background of other theories, it is closely related to consistency In short, Kuhn's main contribution to the theory choice issue is to articulate and synthesize previous discussions. I would add that scope, fruitfulness, and accuracy are closely related as one group, and simplicity and consistency are closely related as a second group. Do other philosophical traditions help expand this list in new directions?

Post-Kuhnian. Theories of Progress

Imre Lakatos accepted Kuhn's argument that theories coexist within an ocean of anomalies and therefore are not easily rejected even in the face of potentially refuting instances. However, unlike Kuhn he proposed a methodology of scientific research programs that developed what he called a sophisticated falsificationism. Lakatos argued that the "basic unit of appraisal must be not an isolated theory or conjunction of theories but rather a 'research program'" (1978: no). An example is Newtonian science, which at its core consisted of a conjunction of theories or conjectures: the three laws of mechanics and the law of gravitation (4). These constitute what Lakatos calls the hard core or negative heuristic of a research program; they are the sacred space that must be protected at all cost from anomalies or refuting instances. The hard core of the research program is defended by a "protective belt of auxiliary hypotheses" that digest anomalies (48). The positive heuristic "consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research program, how to modify, sophisticate, the 'refutable' protective belt" (50). As descriptions of the theoretical structure of some sciences, the concepts

of a hard core with its negative heuristic and a protective belt with its positive heuristic seem to correspond well to some cases.

However, as for Lakatos's prescriptive criteria for choosing among research programs, when the smoke clears there is not much new. Lakatos wrote, "AH the research programs I admire have one characteristic in common. They all predict novel facts, facts which had been either undreamt of, or have indeed been contradicted by previous or rival programs" (5). Lakatos designated as "progressive" those research programs that are generating successful, surprising predictions, in contrast with "degenerative" programs. In progressive programs there is a series of theories, each of which is better than the previous one. "Better" is defined as meeting three criteria: the new theory has excess empirical content over the previous one, that is, it predicts novel facts; the new theory explains the success of the previous theory that is, it subsumes the previous theory; and at least some of the new theory's excess content is corroborated (32).

One problem with Lakatos's proposal is that the requirement of subsumption means that his criteria would not apply to cases where there is Kuhn loss, and consequently the real-world applicability of his second criterion is limited. Furthermore, his use of successful, surprising predictions as the criterion for choice among theories (or research programs) is not very new and restricts the applicability of the criteria to sciences that provide predictive explanations. One might recall that Carnap and Popper (as well as Kuhn) used new predictions as a basic criterion for theory choice, so at best the Lakatosian criterion is a clarification of what the other philosophers had suggested. Moreover, Lakatos does not provide for the strong form of evidential indistinguishability, as described by Carnap, in which two theories (or research programs) made similar predictions. Here, one would have to return to other sorts of criteria, such as simplicity or consistency.

Lakatos also contributed to the muddle of the interdisciplinary dialogue when he used Marxism as his exemplar of a degenerative research program (1978: 6-7). He compared the failed predictions and post hoc explanations of Marxism with the successful, surprising predictions of Newtonian mechanics. The comparison was motivated more by his historical location in Cold War politics, but it assumes a version of the unity of science thesis that is highly questionable. It is unlikely that he could find any social theory or social science research program that provided successful, surprising predictions similar to those of Newtonian mechanics, simply because social phenomena are considerably more complex. He suggests as much when at

another point he refers to the social sciences as "underdeveloped" (9). One of the reasons social scientists lose patience with philosophers of science is that we are constantly told that we are in some sense deficient scientists- we lack a paradigm, predictive ability, quantitative exactness, and so on- instead of being seen as divergent or different scientists.

Larry Laudan developed a slightly different approach to Lakatos's analysis of research programs and progress in science. He proposed the concept of a "research tradition" to replace the Kuhnian paradigm or Lakatosian research program:

> 1. Every research tradition has a number of specific theories which exemplify and partially constitute it; some of these theories will be contemporaneous, others will be temporal successors of earlier ones.
>
> 2. Every research tradition exhibits certain metaphysical and methodological commitments which, as an ensemble, individuate the research tradition and distinguish it from others.
>
> 3. Each research tradition (unlike a specific theory) goes through a number of different, detailed (and often mutually contradictory) formulations and generally has a long history extending through a significant period of time. (By contrast, theories are frequently short-lived.) (Laudan 1977: 78-79)12

The concept of a research tradition, as defined here, loses some of the conventionalist wisdom that Lakatos retained in his definition of a research program as having a negative and positive heuristic. However, like the Kuhnian paradigm Laudan's definition of the research tradition has the benefit of pointing to extratheoretical commitments, that is, something closer to what I call a research culture. This change represents an improvement on the formal emphasis of Lakatos's research program. He also is more willing to allow for internal contradictions and a sense of historical evolution.

What does Laudan contribute to the problem of theory choice? He defined progress as increasing problem-solving ability. Problems are understood as either empirical, such as resolving an anomaly, or conceptual, such as when a theory is either internally inconsistent or inconsistent with another accepted theory (1977: 49). Thus, the first criterion amounts to a type of empirical subsumption, and the second returns us to a version of consistency and simplicity criteria. A supporter of Kuhn would say that to the extent that Lakatos and Laudan have added anything new to his list of accuracy, consistency, scope, simplicity, and fruitfulness, they have clarified and extended the list rather than overturned it.13

The term "progress" or "progressive" as used by Lakatos and Laudan provides another possibility for interdisciplinary misunderstandings. For example, consider one social scientist's approach to the problem, Derek de Solla Price's "scientific doomsday" thesis (1965). Price argued that world science had been expanding since its inception at an exponential growth rate, so much so that it was doubling every ten to fifteen years. Because that growth rate could not be sustained, Price predicted a scientific doomsday, and his prediction seems to have become a reality in the cost-cutting years of the 1990s. (Sometimes social scientists do make successful, even somewhat surprising, predictions.) Although it is still possible to define some kind of "progress" in an era of cost cutting and accountability to pragmatic interests, it is useful to inject this social science finding into the philosophical debate to destabilize the concept of progress. Why should one be content with such a narrow definition of progress as appears in Lakatos's and Laudan's formulas? Can one also set up criteria for the progress of science at the institutional level, such as the minimal maintenance of wages and resources at cost-of-living levels, or the increased amount of diversity and equity in the institutional organization of scientific and technical production? These definitions might even be linked to progress in the sense of content, such as the argument that an increasingly diverse institutional organization of science tends to lead to the weeding out of some of the most egregious instances of gender, race, and other biases (Haraway 1989). I therefore interpret Lakatos's and Laudan's descriptions of scientific progress-replacing one theory with a better one-as very narrow. Social studies therefore can contribute to a more general philosophical analysis of the idea of "progress" in science.

Naturalism and Realism

Laudan's work was influenced by American pragmatism, as is evident in his definition of progress through the concept of problem solving. In philosophy pragmatism has sometimes been referred to as the Chicago school of philosophy, and it is useful to pause with this term because it is another example of variation across disciplinary cultures. In philosophy the term "Chicago school" usually refers to the period when John Dewey was at the University of Chicago. The term probably was born in 1903 when William James hailed Dewey s Studies in Logical Theory as the birth of the Chicago

school of pragmatist philosophy (although later Carnap came to Chicago, thus bringing positivism to pragmatism). The Chicago school in philosophy can be distinguished from Chicago schools in the social sciences, which influenced North American STS in a different way and will be described later. Leading early pragmatist philosophers were Dewey, James, George Herbert Mead, and Charles Sanders Peirce. For pragmatists, knowledge is derived from experience and work (including scientific experimentation), and truth is determined through practical results such as prediction and control or desired psychological/social consequences. Like positivists, pragmatists were critical of metaphysical speculation, but like contemporary naturalists pragmatists tended to rely on scientifically generated natural facts as a ground or touchstone for philosophical argumentation. Pragmatism is therefore a contributing current to naturalism in the contemporary philosophy of science.14

Naturalism is, according to one prominent American naturalist philosopher, the view "that all human activities can be understood as entirely natural phenomena, as are the activities of chemicals or animals" (Giere 1988: 8). Often naturalism is described as the third phase in the philosophy of science after the early debates among positivism, Popperism, and conventionalism and the second wave of historicism. Some naturalist philosophers consider the American pragmatists (such as Dewey, Peirce, and James) to be closer intellectual ancestors than the leaders of the major traditions within the philosophy of science. However, there are also clear lines of influence from the earlier schools of the philosophy of science. Like historicism, naturalism tends to move away from argumentation grounded on formal, a priori reasoning that was characteristic of the positivists, and instead it interjects accepted empirical facts into philosophical argumentation. However, unlike historicism naturalism tends to rely more on the natural facts of cognitive and/or evolutionary processes as a reliable model for distinguishing truth from belief. Naturalists also are usually more deeply involved in the discipline-specific philosophies of science such as the philosophy of biology or cognitive science, in contrast with the emphasis on physics in earlier generations of philosophy. Their deep involvement in specific disciplines tends to make the philosophy of science more continuous with theoretical debates within scientific disciplines. Consequently, justification is often formulated not against an imaginary skeptic but instead against a goal of producing approximately true knowledge about the world.

Perhaps the most influential type of naturalistic approach in the contem-

poi-ary philosophy of science is evolutionary epistemology, that is, any theory that explains human and animal cognition with evolutionary theory Werner Callebut (1993: 286) argues that it is important to distinguish two different programs: (i) biological evolutionary epistemology (also known as bioepistemology or EEM, for evolutionary epistemology mechanisms), which is the natural science of the evolutionary basis of animal cognitive and perceptual systems; and (2) an evolutionary account of science or EET, evolutionary epistemology of theories (following Bradie 1986). Daryl Chubin and Sal Restivo (1983) called the latter the "mild program" in STS. Leaders in the development of an evolutionary approach to science have been Donald CampbeU, David Hull, and Popper. Hull's contribution to evolutionary theory includes the delineation and/or elucidation of the following: a replicator, "an entity that passes on its structure largely intact in successive replications"; an interactor, "an entity that interacts as a cohesive whole with its environment in such a way that this interaction causes replication to be differential"; selection, "a process in which the differential extinction and proliferation of interactors cause the differential perpetuation of the relevant replicators"; and lineage, "an entity that persists indefinitely through time either in the same or an altered state as a result of replication" (1988: 408-9). Hull argues that his distinction between interactors and replicators is an advance on Campbell's term "vehicle," which does not entail the distinction and is therefore likely to create conceptual confusion (Hull 1988: 414; CampbeU 1979).

These terms apply to biological as well as conceptual, scientific, and cultural evolution, and thus they have relevance for social sciences such as cultural anthropology. However, in general cultural anthropologists and sociologists today tend not to be interested in evolutionary approaches to social phenomena. These approaches have a checkered history in the nineteenth century, when they were often used to order societies in a way that legitimated the "white man's burden" of colonialism. In the twentieth century, evolutionary approaches were linked to functionalism, as in Talcott Parsons's social theory, which is now discredited for reasons that will be explained. The different meanings and histories of evolutionary theory in philosophy and the social sciences therefore provide another opportunity for cross-disciplinary misunderstandings.

Usually naturalists such as evolutionary epistemologists are also realists, a term that also provides enormous opportunities for misunderstanding. Several realists are also Marxists, and in Britain the term "critical realist" usually is a code word for a Marxist. Hacking distinguishes three types of realism:

i. ontological, in which scientific theories are either true or false "in virtue of how the world is" (opposed to instrumentalism);
a. causal, in which "the theoretical terms of the theory denote theoretical entities which are causaUy responsible for the observable phenomena";
3. epistemological, in which "we can have a warranted belief in theories or in entities (at least in principle)" (Hacking 1983: 28).

The first type of realism is usuaUy connected with some theory of scientific progress, such that theories and/or methods describe the real world beyond observations increasingly better over time, or theoretical terms that seem to be merely instrumental acquire ontological status over time.15

The second definition of realism involves the philosophy of explanation. Generally, two major approaches are distinguished: inferential and causal. The inferential approach treats explanation as either an inductive or deductive argument in a tradition developed by Carl Hempel (1965; Hempel and Oppenheim 1948). To summarize, if the set of premises (explanans) is true, and the conclusions (explanandum) are logically related to the explanans, then the conclusions are true or probably true. Under the covering law model, the conclusions are subsumed under a general law or laws and some antecedent conditions. The first type of explanation is caUed nomological-deductive, and explanations are understood as predictions of events made after the events occur (retrodiction) and based on universal laws (nomological). When at least one of the laws is of a statistical nature, the type of explanation is deductive-statistical rather than deductive-nomological. However, some would argue that the conclusions (explanandum) may also be derived inductively; hence, the third type of inferential explanation: inductive-statistical. In aU cases explanation is seen as a kind of description. Hence, this understanding of explanation is consistent with the view that one sometimes hears from scientists, namely, that science provides only highly general descriptions of the world but not explanations.

Because the covering law model is subject to countercases, some philosophers have defended the causal approach, a version of which appears as Hacking's second type of realism. From this perspective, explanations consist not of an argument that shows that the phenomenon was to be expected but of a statement of causes that shows how the phenomenon was brought about. A classical starting point in discussions of causality is Humean causation, which as reconstructed by twentieth-century philosophers is as foUows: given two events El and £2, El causes £2 if (i) El happened

before £2, (2) it is possible to deduce E2 by knowing Ei occurred, a set of laws of nature, and statements of mutual conditions; and (3) it is not possible to deduce E2 from El by knowing only the set of laws of nature and statements of mutual conditions. This description more or less describes the understanding of causality implicit in medical research that follows Koch's postulates.16 Hacking's description of a causal form of realism, which applies to theoretical terms, is an example of another type of causal explanation.

The third type of realism involves an opposition to epistemological relativism. Philosophers have occasionally charged some social scientists who work in the constructivist tradition with epistemological relativism. These debates often involve a great deal of cross-talk and therefore wan-ant some clarification. Before proceeding to that issue, one might ask whether realism contributes to the basic problem discussed in this chapter: providing better grounds for theory choice. Probably the unique contribution of the realist would be a criterion that, assuming evidential indistinguishability, preference sho^A be given to the theory that uses terms that are more realistic. In other words, preference would be given to a theory with more observational terms or theoretical terms that could be transformed someday into observational terms. Likewise, preference would be given to a research program for which theoretical terms showed some tendency toward conversion into observational terms over time, and to a program that gave increasingly better technological benefits. For example, viruses were theoretical terms that have, over time and with better technology, become observational terms. Other than this criterion, realism probably would not add much to the discussions of theory choice criteria.

Constructivism and Relativism

Although philosophers are usually very precise at using terms, there seems to be a great deal of confusion regarding Constructivism and relativism. Both labels are sometimes applied to the work of historicists such as Kuhn as well as some social scientists. It is helpful to distinguish the various meanings of both Constructivism and relativism.

In the social studies of science and technology, the term "social constructivism" is often used as a general label for studies that examine how social variables shape the pattern of choices about what research gets done, how it is done, how choices among theories are made in controversies, and the extent to which observations, laws, theories, and other knowledge

claims become accepted in wider scientific communities. Philosophers tend to use the term "Constructivism" somewhat differently to refer to the idea that scientists do not discover the world but impose a structure on it or in some sense "make" the world. In its extreme version, Constructivism amounts to more than an instrumentalist account of theories; it refers to a social idealism in which there is no material reality that constrains or structures sensory observations. Furthermore, regarding the problem of theory choice, this extreme version would hold that the world does not in any serious way constrain theory choice; in this sense the world is made or constructed rather than discovered. Some philosophers argue that the Constructivism of social studies of science necessarily implies social idealism and epistemological relativism, but I suggest that there is no necessary connection between the two. There may be some social scientists who would accept the philosophical position of social idealism and epistemological relativism, and if they exist I would suggest calling their philosophical position "radical Constructivism." Certainly some sociologists of scientific knowledge have made statements that suggest they accept radical constructivism or they did at one point.

An alternative to radical Constructivism is the position that scientific theories are realistic maps or explanations of a real world and at the same time vehicles that encode culture-bound linguistic categories and cultural values (what I call cultural Constructivism), and/or are shaped by social interests and other social variables (what I call social Constructivism). I think of this hybrid philosophical position as moderate Constructivism, although similar positions have been articulated under the banners of constructive realism or realistic Constructivism. Many researchers in the social studies of science would probably accept some version of moderate Constructivism. They believe that scientific theories and observations are constrained by a real, material world, but not completely so. Social variables and cultural values also play a shaping role. The mix of the material and social/cultural varies greatly across discipline and phase of research.

I would suggest adding yet a third philosophical position: conservative Constructivism, which would hold that social interests and cultural values shape scientific theories only by instilling bias. Eventually that bias can be removed, thereby producing a Utopian state in which science is objective or freed from infusion with cultural values and categories. A moderate constructivist would not see the referential and sociocultural aspects of scientific representations in such a zero-sum manner. Cultural values are not weeds to be picked from the garden of science to make room for

flowers; rather, they are the soil upon which the flowers grow. In other words, scientific theories participate in their general epistemic, national, temporal, gender, and other cultures, thereby encoding values even as they simultaneously represent nature.

Let us take, for example, Marx's famous pronouncement about Darwin's evolutionary theory, that it encodes quite nicely the nineteenth-century bourgeois order of competitive capitalism as projected onto nature. A radical constructivist might argue that evolutionary theory, as well as the observations Darwin made, were a projection of nineteenth-century capitalism onto a natural world that was more or less a tabula rasa. We continue to believe in evolutionary theory today because we continue to live in a capitalist society that is permeated by the same metaphors and cultural structures as in Darwin's day From this viewpoint, there would be no way to choose between evolutionary theory and creation science, except through power and persuasion or consistency with a surrounding cultural ethos and worldview.

A conservative constructivist would maintain that any cultural biases in evolutionary theory that entered into Darwin's model have been successively eliminated by subsequent versions of the theory. Even if Darwin did project some of his society onto nature and did produce a distorted map of nature, subsequent developments of evolutionary theory (and observations) have progressively weeded out those distortions. Like a progressive research program, evolutionary theory has improved over the years. Therefore, evolutionary theory is asymptotically approaching a pure, transparent representation of nature.

A moderate constructivist would argue that Darwinian evolutionary theory both projected Darwinian society onto nature and provided a relatively accurate map of a selected aspect of the natural world. Subsequent developments in evolutionary theory represented more accurate maps, pace the conservative constructivist, but those developments did not entail escaping from culture. Instead, subsequent changes correspond in part to general cultural shifts in science and society. For example, as evolutionary theory developed in the twentieth century toward equilibrium models and later nonlinear models, these changes drew on general metaphors and structures that were appearing as part of the general cultural shifts from the nineteenth century through modernism to postmodernism. It is thus possible to think about scientific theories as both realistically and socially constructed, much as in duck-rabbit Gestalt drawings in which two interpretations are simultaneously consistent with the material. In a similar but not quite identical

formulation, Fuller argues that " 'the social' and 'the cognitive' are not separate parts of the scientific enterprise; rather, they are two relatively autonomous discourses that are available for analyzing any part of science" (i993a: 57). Thus, I would prefer to think of the sociocultural and referential aspects of scientific theories as two dimensions of the same phenomenon rather than two alternative approaches to it. Some version of this view is supported by philosophers such as Giere under the terms "perspectival realism" or "constructive realism" and Fuller under the term "realistic constructivism," as well as by some social scientists.17

To argue that scientific theories or models are representations may require some unpacking because the term "representations" shifts across disciplines.18 For realist philosophers such as Giere, representations imply the idea that scientific theories represent or map reality. In history and cultural studies, Durkheim's "collective representations" are the shared ideas and beliefs of a community or other social unit, as in religious cosmologies. Given the multivocality of the word "representation," it may be uniquely situated for the hybrid world of moderate (realistic) constructivism. In other words, scientific representations are both referential-maps or models of the world-and sociocultural-encodings of values, general cultural categories, interests, and so forth.

Now that I have outlined the moderate constructivist position, let us play devils advocate and accuse it of question-begging. What causes scientists to choose among theories: the fact that they accurately represent the world or that they represent social interests or cultural values and categories? From a prescriptive viewpoint, there may not be much disagreement: the former should inform good theory choice. At a descriptive level, however, the situation is more complicated. One might answer the question by saying both, that theory choice is overdetermined. Or, as I would advocate, one might refuse to answer the question in the universalizing terms in which it is posed and instead answer it on a case-by-case basis. By adopting these two strategies-separating the prescriptive from the descriptive question and reformulating the universal grounds of the question-the problem of relativism can be avoided.

Relativism is for philosophers a word similar to positivism for social scientists and humanists: it is better to accuse someone else of having it. To make the word useful, I like to distinguish four types. In social sciences such as anthropology, cultural relativism refers to a research stance and method that begins with the understandings of a community, actor, or some other social unit. In other words, social action is interpreted relative

to the cultural meanings attributed to it by the actors involved. However, cultural interpretation or social science analysis does not end with local meanings; in turn those meanings are explained by general theories available in the social sciences. The recourse to theories and explanation usually puts cultural relativism in opposition to epistemological relativism.

Epistemological relativism is the position that (i) evidence and other universalistic criteria (such as consistency) do not play a crucial role in theory choice, which instead is largely conditioned by contingent or particularistic social factors; and (2) attempts to articulate prescriptive theory choice criteria are useless because scientists will not follow them. Although some social constructivists may support this position, many social scientists believe that although theory choice is heterogeneous, evidence still plays a crucial role in many if not most controversies. Thus, the first assumption is violated, and therefore attempts to articulate prescriptive theory choice criteria still are worthwhile.

Metaphysical (ontological) relativism is the position that theories and theoretical language do not necessarily capture anything of the deep structure of reality behind observations. Both positivists (as instrumentalists) and radical constructivists would therefore tend to be relativists in this sense, as opposed to realists and realistic/moderate constructivists. Because the realist position can add something to the theory choice debate that the relativist position cannot, on this issue realism seems warranted.

Finally, moral relativism is the position that there should be no universally upheld values, such as United Nations-sanctioned lists of human. rights. Generally, anthropologists and others who employ cultural relativism as a method today support a political and moral stance of tolerance of and aid to marginalized groups; thus, in their minds cultural relativism is linked to opposition to moral relativism. People who attempt to argue that cultural relativism and moral relativism are identical, or that cultural relativism leads to moral relativism, simply are confusing the issues. Usually, the opposite is the case.

Note that accepting one type of relativism does not necessarily imply accepting another, although cultural and moral relativism are often opposed, and epistemological and ontological relativism are often linked. Furthermore, my four types of relativism are not the only ways ofthinkinoabout the topic. For example, Karin Knorr-Cetina and Michael Mulkay distinguish epistemic relativism, which asserts "that knowledge is rooted in a particular time and culture," from judgmental relativism, which holds "that all forms of knowledge are 'equally valid; and that we cannot compare

different forms of knowledge and discriminate among them" (1983: 5^ In my terms, this is a distinction between moderate constmctmsm and epistemological relativism. Harry CoUms also endorses relativism (probably my second and third types) in his empirical program of relativism (1983^ 1985) Fuller (i993a: 66-69) distinguishes between realism/antirealism and relativism/objectivism. The former involves the question, "Are there legitimate -rounds for criticism in science aside from those having to do with judgments of empirical adequacy?" and the latter revolves around the question, "Are there legitimate grounds for criticism m science aside from those having to do with judgments of expert authority?" Laudan (1990) provides a humorous parody of relativism, in which the relativist serves as a philosophically uninformed Simplicio/skeptic character in dialogue with a more intelligent positivist and realist, and an even more intelligent pragmatist Unfortunately, the parody leaves the impression that social scientists are stupid and incapable of finding a philosophically coherent position for themselves, and as a result it only fans the flames of the cross-talk rather than attempting to find a way of moving beyond it. To be clear, I support only cultural relativism in the narrow sense defined here of methodology in cultural anthropology.

Social Studies and the Problem of Theory Choice

It is now possible to return to the problem of justifying theory choice and to consider how social studies of science can contribute to this problem in a helpful way that avoids the trap of epistemological relativism. Social scientists have often invoked underdetermmation and theory-ladenness as philosophical license for a descriptive account that shows how particularistic values or social interests shape theory choice (e.g., Knorr-Cetina and Mulkay 1983- 3) However, the conventionalist arguments are largely irrelevant, not because conventionalists such as Duhem provide other prescriptive grounds for theory choice, but because prescriptive arguments are distinct from descriptive accounts. How scientists should choose theories is largely irrelevant to the empirical problem of describing how they in fact do choose theories. However, is the reverse also the case?

Descriptive accounts of scientists in practice suggest that they often do not follow the universalistic, prescriptive criteria suggested by the philosophy of science, even a loose family of criteria such as those articulated by Kuhn To understand these accounts, it is useful to distinguish between

private or covert criteria for theory choice (those that individuals keep to themselves or share only among networks of allies) and public criteria, which often emerge to legitimate positions in controversies. Often public criteria (as they appear in publications, memoirs, or public disputes) correspond to one of the philosophical ideals for theory choice, whereas private or covert criteria do not. Thus, it is important to follow scientists in action-in the laboratory and behind the scenes. Empirical studies of this sort have shown that in addition to universalistic values-such as accuracy, consistency, and simplicity-scientists evaluate theories and observations by reference to particularistic or personalistic criteria. In the social sciences a universalistic value orientation refers to the situation whereby actors use the same value system consistently across social situations. In the case of theory choice, this would mean applying the values of accuracy, consistency, scope, and so on. In contrast, particularism means that social actors shift their value system depending on the social situation (such as one set of values for friends and family, or for one's own social group, and other sets for other groups).

Philosophers who have thought carefully about the social studies of science recognize the limitations of universalistic values for descriptive accounts of scientists' action, because scientists turn out to be much more particularistic than they may admit in public. Helen Longino is one example of a philosopher who recognizes the limitations of universalistic values in descriptive accounts when she discusses the problematic nature of the distinction between constitutive and contextual values. She defines the former as internal to the sciences and "the source of rules determining what constitutes acceptable scientific practices," whereas contextual values "belong to the social and cultural environment in which science is done" (1990: 4). For our purposes here her distinction between constitutive and contextual values can be translated (more or less) as examples of the social scientist's distinction between universalistic and particularistic values. Longino argues that the traditional claim of value-freedom in science amounts to an argument that the two types of values are distinct and independent from one another. In general, the older view that good science is value-free or governed wholly by constitutive/universalistic values, whereas bad science is infiltrated or corrupted by contextual values, now seems naive to many researchers. Longino argues that the traditional distinction between the two types of values cannot be maintained. Instead, there has been a shift toward thinking about the two types of values as coexisting in the processes of science production.

What exactly is meant by particularistic values? As they apply to the problem of evaluation of research and theory choice, they would include the following somewhat overlapping categories:

1. Favoritism: The person who is proposing a scientific claim or theory is a friend (or an enemy), or allied with networks of friends or enemies, or I owe them a favor (or vengeance).
2. Social prejudice: The person is a member of x social category external to science, which I either like or dislike. In other words, in the ideal world of science, the person has a "functionally irrelevant status" such as gender, race, ethnicity, religion, nationality, sexuality, and so forth. (Cole 1992: 162).
3. Cognitive cronyism, or cognitive particularism: The person is writing outside my area of expertise or outside my research network, so I am inclined to give them less than a fair hearing because I am unfamiliar with their research framework or the network in which they move.19
4. Personal gain: I should support or block the person in order to help (or prevent damage to) my career, funding, or reputation, or that of my friends and allies.
5. Reputation: The person occupies what I perceive as x position in the social hierarchy of science, according to criteria such as educational credentials, institutional location, fame, awards, and journal prestige.

These and other particularistic criteria for theory choice are part of science. Reputation is complicated because if the reward and gatekeeping systems in science are universalistic and fair, then indices of reputation are not necessarily particularistic. However, most of the empirical research suggests that the reward and gatekeeping systems are not completely universalistic and fair. In a perfect world one would evaluate a scientist with minimum credentials by ignoring indices of reputation such as their institutional location. Because this does not occur in practice, reputation can be classified as at least somewhat particularistic.

These particularistic values can be hard to document because when scientists make public or retrospective statements about their theory choices, they tend to pave over the particularistic criteria with universalistic criteria that correspond more closely to philosophers' "rational" criteria such as Kuhnian values. In other words, they may justify or rationalize their decisions. However, particularistic criteria are more than dirty laundry that tends to remain hidden; they also play an important functional role in science as preliminary screening devices. For example, even though criteria

such as reputation may be dismissed as at least potentially ad hominem and particularistic, they are widely used and relatively effective preliminary screening guides. Can we therefore say that particularistic criteria should be included in a list of prescriptive criteria for theory choice? Not necessarily: to make that leap would not only confuse description and prescription but also commit the naturalistic fallacy (things are this way; therefore, they should be this way). Statements about what is the case can be relevant to an attempt to formulate prescriptive rules, but only because they allow one to argue that certain prescriptive options seem unrealistic given our present descriptive accounts of the way things are.

Instead, consider two productive philosophical implications of social science research that suggests that particularistic values, especially cognitive particularism, do play a nontrivial role in the evaluation of research programs, theories, and empirical claims. One argument is that the mixing of particularistic and universalistic criteria in actual science seems not to have harmed science. Stephen Cole takes this position even further by suggesting that a particularistic system may have benefits such as allowing researchers to spend less time writing proposals and providing conditions for more creative work (1992: 203). He argues that cognitive particularism may be ubiquitous and a functional requirement for high productivity in science. Therefore, one of the implications of social science research for a prescriptive philosophy of science is that it might be worthwhile to sort through particularistic criteria to determine whether any of them should be included in a prescriptive list of theory choice criteria, and if so under what circumstances.

A related argument is that even apparently universalistic criteria for theory choice may vary across disciplines and over time. For example, an empirically derived list of universalistic criteria used for evaluating a good theory in late twentieth-century anthropology would not overlap completely (or necessarily at all) with a similar list used for physicists' theory choices in the late nineteenth century. One implication for philosophical projects is that prescriptive criteria might be more useful if pegged to the temporal and disciplinary context of knowledge making and perhaps also to the stage of knowledge production and consumption (laboratory work, paper writing, assessing written fact claims, taking sides in a controversy, and so on). Of course, this argument assumes a widespread disunity of science.

Clearly, the insight of good descriptive accounts from history and the social sciences does not necessarily imply a radical relativism in which

anything goes. Rather, these empirical findings can only complicate the prospects of prescription. Conversely, philosophers are right to challenge some of the descriptive accounts in the social studies of knowledge that may have underestimated scientists' use of universalistic criteria, even in controversies.20 Yet, it seems precipitous to reassert blindly the overwhelming importance of universalistic criteria based on a few counterexamples. If philosophers want to challenge descriptive accounts, they must go and do the research (as in Giere 1988) and suffer evaluation according to the standards of the social studies discipline in which they are working empirically. In general, I would not advocate making a broad statement about whether universalistic or particularistic criteria operate more powerfully throughout the sciences. Rather, I suspect that particularistic criteria operate more powerfully the closer the case is to applied science, economic and political interests, gender- and race-related issues, the research front, and controversies. It seems better to leave the relative role of universalistic and particularistic criteria open to empirical analysis that allows for considerable variation.

In sorting out the problem of the relative role of the two types of values, I prefer the distinction of particularistic versus universalistic over that of cognitive/rational versus social/cultural. These other distinctions are imprecise ways of referring to the extent to which personalistic or particularistic criteria enter into decision-making processes that are supposed to be governed by universalistic criteria. To think of this problem in terms of the cognitive versus social or the rational versus cultural invites confusion. To begin, the opposition between the cognitive or rational on one side and the social or cultural on the other is tenuous. Styles of reasoning cannot be wholly reduced to psychobiological processes of cognition; therefore the pattern of rationality is subject to variation across time and culture. For example, Fuller problematizes the concept of rationality by delineating three major types in Western history: Greek telos, in which reason is inherent in the world; Enlightenment raison or Vernunft, in which reason is inherent in the world but first has to be released; and modern rationalization, in which reason is not inherent in the world but must be imposed from the outside.21 Certainly, these three definitions have clear historical and cultural addresses that make a universal claim on the part of one version highly suspect.

Alternatively, if one defines rationality as cognition, it can be considered asocial or acultural only if it refers to universal psychobiological processes that do not vary across cultures. Cognitive processes of this sort exist (such

as the ability to acquire language), but there is as yet no widely accepted evidence that these psychobiological cognitive processes are relevant to the problem, of the extent to which scientists use particularistic or universalistic criteria in evaluating theories and knowledge claims. Scientists, like everyone else, have access to the same cognitive processes whether they are orienting their action according to particularistic or universalistic values. In other words, it takes reason, cognition, or rationality to make decisions that follow particularistic orientations, just as it does for universalistic orientations. Does this mean that there is no specific type of rationality associated with modern science? Many social scientists would argue that the type of rationality in science is not qualitatively different from that of other specialist occupational groups in advanced capitalist societies. This view seems to be gaining ground among philosophers as well, particularly naturalists who work along cognitive or evolutionary lines. For example, Giere argues for limiting the definition of rationality to the "effective use of appropriate means to achieve desired goals" (1995: 15). My only complaint is that he tends to reduce the adaptation of means in science to the appropriate selection of methods. This may be a good prescription for rationality in science, but descriptively the structure of social action in science involves a wider field of means and ends. Thus, scientists may be acting quite rationally when they employ as means rhetorical strategies, including universalistic ones, for particularistic ends (e.g., using consistency arguments against other theories in order to advance their own careers). Likewise, they could adapt particularistic means and methods to universalistic ends (e.g., diverting funds to do sound research). The assessment of what scientists see as their field of means and ends is an empirical issue, and one that is not easily resolved. I would suggest three guidelines for making this kind of assessment: (i) do not assume that one case study can be generalized to all of science; (2) do not confuse legitimating accounts made in public or in publications with hidden values; and (3) do not accept uncritically claims for universalism in retrospective accounts constructed in memoirs for posterity or in interviews with science studies researchers.

To summarize, the'distinction between universalistic versus particularistic value orientations is more clear than categories such as rational (cognitive) versus social. The extent to which universalistic and particularistic values shape scientific decisions is a descriptive problem, and it cannot be resolved by blanket statements designed to cover all cases, disciplines, and time periods. We are still left with the question of whether particularistic criteria should be used as part of prescriptive theory choice criteria. Cer-

tainly. Cole has raised an argument in their favor. To answer this problem, we turn to the feminist philosophy of science.

Feminist Epistemologies

Feminists have led the way on the issue of including particularistic criteria in prescriptions for theory choice. One starting point is standpoint epistemologies, which can be viewed as an extension of what I am calling moderate constructivism: they hold that in order to be able "to detect the values and interests that structure scientific institutions, practices, and conceptual schemes," and therefore to move on to better but nevertheless ultimately fallible and culture-bound accounts, one good strategy is to begin research with the perspectives of marginalized groups (Harding 1992: 581; 1986). Of course, in practical terms there are not enough resources to examine the perspectives of all marginalized groups. Even if there were bountiful resources, one would not want to examine the standpoint of every group that has a marginal status. One therefore needs to weigh this particularistic criterion against other ones, such as reputational markers of scientific credentials that would suggest that the arguments of a marginalized group might be of some scientific value. For example, in my research on alternative cancer therapies, I found that there are literally dozens of alternative therapies to consider. One way of using standpoint theory in a limited but coherent way is (i) to select from the field a series of researchers who have relatively good scientific credentials and a published record of empirical research; and (2) within this first category to consider the work of a marginalized network of women researchers (Hess i99?a).

Standpoint epistemologies therefore represent only a starting point in this discussion. "Strong objectivity" represents one way of developing the argument into a more useful program. In Sandra Harding's words, this program would

> specify strategies to detect social assumptions that a) enter research in. the identification and conceptualization of scientific problems and the formation of hypotheses about them (the "context of discovery"), b) tend to be shared by observers designated as legitimate ones, and thus are significantly collective, not individual, values and interests, and c) tend to structure the institutions and conceptual schemes of disciplines. These systematic procedures would also be capable of d) distinguishing between those values and interests that block the production of less partial and distorted accounts of nature

> and social relations (less "false" ones) and those-such as fairness honesty detachment, and, we should add, advancing democracy-that provide re^ sources for it. (1992: 580)

The term "objectivity" here appears to mean the development of representations (theories, models, laws) that capture more of the real or at least the observable, and that project less of the social and cultural, particularly as it appears through biases introduced by unquestioned political or social assumptions (also Porter 1992). In this sense, there is more of the object in the representations, and they can therefore be seen to be more "strongly objective." A related concept is Donna Haraway's situated knowledges which analyzes theories, theorists, and sciences by giving them a social address or location. "Unmarked knowledges" are those characterized by a presumption of objectivity that usually obfuscates their social embeddedness in white, male, or other dominant cultural perspectives (1991: in, 188).

The concepts of standpoint epistemologies, strong objectivity, and situated knowledges are valuable starting points for a new prescriptive approach to the problem of theory choice. They are still in the process of being articulated, and they would benefit from more explicit theorization of their hnuts and of the philosophical claims that they are and are not making One possible limitation is that standpoint epistemologies may work well only for certain types of sciences. For example, they have already been applied to the social sciences and some areas of the biomedical sciences where social biases have frequently distorted theorizing and empirical in^ quiry, but their utility remains to be demonstrated in the exact physical sciences. They would probably work well in scientific and technical disputes over environmental justice issues. There is little work on the implications of these theories for relatively autonomous fields such as theoretical physics or physical chemistry.

Moreover, standpoint epistemologies and related frameworks need to be embedded in an overall theory of justification if they are to avoid the problem of epistemological relativism. Standpoint epistemologies can be valuable contributions only if they are interpreted to mean that research does not end with the perspectives of marginalized groups. In this sense a standpoint epistemology amounts to a methodological prescription that'is similar to anthropology's cultural relativism: to start with local points of view. Anthropological and standpoint epistemologies are also similar in this respect to Marxist critiques of bourgeois science from a proletarian perspective (Hesse 1994). All three begin their critiques with local or excluded

viewpoints, but in the best case their analyses do not stop with those viewpoints. In cultural anthropology this second move is very explicit: analyses still have to be translated into social scientific theory and contested according to the (also contestable) methodological standards of the community of anthropology researchers. Otherwise, the method falls victim to epistemological relativism and becomes incoherent. Stated in these terms, standpoint epistemologies-or better, the more encompassing framework of anthropology's comparative principle of cultural relativism-are one means for achieving new and better scientific theories (something hke a stronger form of objectivity, to use Harding's terms). Another means might be simply to work on making scientific communities more diverse socially and wait for the new theories and methods to flow from that diversity. Haraway's studies of primatology (1989) suggest that when women and Asian primatologists entered the field, they led significant reforms that substantially improved the quality of theories, methods, and observations. However, neither cultural comparison nor improved social diversity provides an overall guide to the problem of justification of choices among empirically equivalent theories or research programs.

One coherent formulation of a justification strategy that is friendly to standpoint epistemologies, strong objectivity, and situated knowledges is Helen Longino's six criteria for theory (or research program) choice that she has distilled as implicit in a number of feminist science studies analyses, including those of Harding and Haraway. Longino's criteria are empirical adequacy, novelty, ontological heterogeneity, complexity of relationship, applicability to current human needs, and diffusion of power (1994). Those six criteria are not restricted to gender-based standpoint epistemologies; they could be extended to other dimensions of culture/power exclusion such as class, race, nationality, and sexuality. Thus, they represent a fairly generalized extension and synthesis of feminist contributions to this problem.

First consider the definitions of the criteria. By novelty Longino is thinking of Harding's and Haraway's call for knowledges situated in the standpoints of women and other categories historically excluded from the conversation of science. By ontological heterogeneity Longino refers to (i) a concern with diversity in the object of study, as in Barbara McClintock's attention to the diversity of the kernels of a corn cob (Keller 1985) or women primatologists' attention to diversity within and among primate groups (Haraway 1989); and (2) the rejection of theories of inferiority, that is, theories that see difference as substandard, a deviation, or a failure. By

complexity of relationship Longino refers to the rejection of "single-factor causal models for models that incorporate dynamic interaction" (479). Applicability to current human needs implies "improving the material conditions of human life or alleviating some of its misery," and diffusion of power refers to research choices that favor programs that are less limiting in terms of access and participation (479).

Longino's justification criteria can be compared to Kuhn's values of accuracy, simplicity, fi-uitfulness, consistency, and scope. As in the case of Kuhn, the six criteria are subject to interpretation; Longino also sees them as fallible and therefore subject to revision. I interpret the Longino criteria as complementing rather than displacing a list such as Kuhn's. There is some overlap, as in the call for accuracy and empirical adequacy, which after all remains the key to resolving many differences of opinion in science. Without it, science would simply be politics by the same means. However, in other cases the two lists appear to be in conflict, such as novelty versus consistency and simplicity versus complexity. Comparison of the strengths and weaknesses of the two lists warrants further work.

To begin a discussion of a synthesis, consider the case of Brazilian Spiritism, a religious-philosophical movement whose members insist that it is scientifically grounded (Hess l99ib). Spiritists claim empirical adequacy or accuracy for their observations of phenomena such as poltergeists, hauntings, materializations, apparitions, and correspondences in messages from the same spirit across different mediums. As with any theoretical claim, one could certainly contest the empirical adequacy or accuracy of Spiritists' observational base, but for the sake of the argument let us go on. Two alternative and (let us assume for the sake of argument) evidentially indistinguishable theories could be a skeptical psychology of deception or dissociation and a parapsychology of extrasensory perception and psychokinesis.

Let us consider Kuhn's criteria first. The Spiritist theory is simple; we do not require a maze of psychological processes or a complex unconscious with extrasensory powers. Instead, things are exactly as they seem. Sometimes a cigano (gypsy spirit) is just a dgano. The theory is fruitful, at least to Spiritists, for they have developed new and different forms of mediumship, from the old Victorian types of psychography (automatic writing) and materialization to the new, Brazilian forms of mediumistic painting and spirit surgery. It seems to be progressive. The theory has a wide scope, and in Brazil I found that the Spiritists applied it to many problems of everyday psychology. For example, spirit intervention can explain good luck and bad

luck, disease, mental illness, the ups and downs of love, and any number of issues of great importance in everyday life, including the Weberian problems of ultimate meaning. Finally, the theory is internally consistent; it is encoded in six volumes that lay down the fundamental principles, the negative heuristic. It only fails on the external consistency criterion.

Now let us consider Longino's criteria. The Spiritist theory is novel, or at least it could claim to be novel with respect to the current assumptions of materialism and mechanism that a mainstream psychological theory of deception and dissociation would rely on. To borrow a phrase Harding used to describe an argument of Haraways, one might claim that the Spiritist theory shows "enthusiastic violation of the founding taboos of Western humanism," at least of secular humanism (1986: 193). The theory is also elaborated in a Third World culture, and women often provide leadership roles in Spiritist centers, so from the standpoint of standpoints it looks like a good candidate to provide a point of comparison for seeking out unrecognized biases in the accepted scientific wisdom. In fact, Spiritists bemoan the materialistic bias of orthodox science, even the "orthodox parapsychology" of the First World. The spirit world is ontologically heterogeneous; there are many types of spirits. There is even some interesting gender-bending in the world of mediums and spirit guides; for example, some of the well-known male mediums are gay. One might argue that the Spiritist theory is weak on the second aspect of oncological heterogeneity criterion, because it describes a hierarchical world in which difference is measured against a standard. However, the form of hierarchy is modern and meritocratic; all spirits are presumed to be equal in terms of their human value and their opportunity to achieve high levels of spiritual development. Thus, the theory would seem to pass the ontological heterogeneity requirement.

On the complexity criterion, the Spiritist theory could be given a pass because it cannot be reduced to single-factor causal models. Spiritists could also argue that their theory does well on the criterion of applicability to current human needs. Millions of Brazilians go to Spiritist centers to receive Spiritist healing treatments. Furthermore, Spiritists see their work and that of the spirits as one of educating, evangelizing, and uplifting their fellow Brazilians, and therefore transforming their country into a successful, developed land. Finally, in terms of diffusion of power. Spiritist mediumship/science does not require access to expensive resources.

Nevertheless, I suspect that most feminist scientists and science studies researchers would probably reject Spiritism out of hand as an alternative

research program, theory, or successor science to modern psychology. The "theory" could even be demarcated off the playing field by claiming that it was not a science at all. On one criterion, however, Spiritist science would not pass muster: consistency in the sense of consistency with the rest of science. Spiritist "science" (and I am using the label here only for the sake of argument) is not a peninsula attached to the mainland of science but an island unto itself. Yet, consistency is the very problem that standpoint epistemologies seek to redress. Recall the revisions of the Frye ruling and the conservative bias implicit in the consistency criterion. Feyerabend (l978: 35) went even further and argued that a consistency criterion is unreasonable because it preserves the older theory Certainly from a feminist perspective one is likely to be suspicious of the consistency criterion as a license for androcentrism and other undesirable values.

Kuhn and Longino both make clear that their lists are not complete and are not intended to be so. They merely provide a loose set of guideposts that can aid in the problem of evaluation of the grand theories of science. I have used this admittedly hol(e)y example merely to make the larger point that judgment is necessary even when clearly articulated criteria are available, and even when the criteria involve purportedly better or more equitable values than Kuhn's synthesis. I suspect that no list of lists will be complete, even if the spirits ofCampbell, Carnap, Duhem, Kuhn, Popper, Lakatos, and others have continued their dialogues in the metaphysical world and would be willing to communicate their results to us via a Brazilian medium.

Elsewhere I develop and apply a synthesis that brings together the various proposals for prescriptive theory choice criteria that have been considered in this chapter (Hess i997a). This evaluation draws on four groups of criteria: what I call the positivist, conventionalist, pragmatist, and feminist/antiracist groups. I argue that the best evaluation of a scientific research program (or a major theory)-which is altogether a different matter from the evaluation of a specific scientific observation or generalization-needs to include considerations of accuracy, consistency, social utility, and lower social bias than the alternative. My synthesis of the Kuhn, Longino, and other criteria for theory choice would solve the spirit world problem but still allow some place for the considerations raised by Longino and other feminists.

My synthesis of theory choice criteria implies that each of the philosophical traditions discussed here has its contribution to make, and that no simple formal algorithm can ever be developed for the evaluation of a

theory. Like Longino and Kuhn, I would still put accuracy, evidence, or empirical adequacy at the top of the list. There seems to be little disagreement among anyone-natural scientists, social scientists, or philosophers-on this point. Still, the other criteria are useful, particularly in cases of evidential indistinguishability. One can successfully use general guideposts that begin with Kuhn's list but amend it by consideration of subsequent philosophical traditions, particularly pragmatism and feminism, as discussed in this chapter. It is also possible that the list of evaluation criteria may need to be revised as it is applied to different disciplines. My list was developed for a particularly politicized field of medical research, where considerations of social utility and social bias were important. In some of the more autonomous and formalized fields, these criteria may-be less important, less evident, or less easy to operationalize as part of an evaluation of a theory. Like all philosophical discussions, this one is by no means closed, and it is part of an ongoing dialogue in which the strengths and weaknesses of alternatives are clarified.

NOTES TO CHAPTBR 2

1. Fuller (19930), Gillies (1993), Hacking (1983), Kourany (1987), Losee (1993), and Rouse (1996).

2. I am following Popper's interpretation of Russell and Wittgenstein here (Popper 1963: 69).

3. See Saussure (1966) or Culler (1986) on semiotics, and Rouse (1996) on contemporary philosophical debates on meaning.

4. Fuller notes that the distinction has a history that dates back to the nineteenth century, especially to William Whewell, who coined the term "scientist" and was a founder of the history and philosophy of science. The distinction had a social basis in Whewell's attempt to define a place for science and to ensure a privileged role for universities (Fuller, personal correspondence, January 2, 1996; citing Yeo 1993; see also Hoyningen-Huene 1987).

5. Fuller (1994: 255; and personal correspondence, January 2, 1996).

6. The term "Duhem-Quine thesis" may be a bit of a misnomer because Duhem's and Quine's versions are different (Gillies 1993: 98). See Duhem (1982), Quine (1980), and Hesse (i98oa).

7. Laudan (1977: 114). It is helpful to keep in mind the difference between ad hoc theorizing and post hoc analyses. The latter are statistical analyses made after data come in and reported in the discussion section of a paper. Often researchers perform post hoc analyses to find out why negative results were obtained. As a clearly labeled exploratory exercise for future hypothesis generation and research, they can be useful. However, when used with post hoc explanations and when multiple post hoc analyses are made on the same data, post hoc analyses are considered methodologically unsound. When post hoc analyses are not labeled and

are reported as "pre hoc" hypotheses in the results section, they are fraudulent.

8. See Solomon and Hackett (1996), Yearley (i995), and, for an applied discussion in the context of alternative medicine, Jaffe (1996).

9. Fuller (1992: 245); on Kuhn as a sociologist, see Hesse (i98ob: 32) and Restivo (1983: 294).

10. Although the term "anomaly" is widely associated with Kuhn, there are other uses in the literature. For example, historians have frequently pointed out that l6i

researchers who pay attention to anomalies rather than ignore them have occasionally made major theoretical or empirical breakthroughs. Another example is anomalistics, the term for the scientific study of anomalies defined as claims of phenomena not generally accepted by the bulk of the scientific community In the United States, the Society for Scientific Exploration studies anomalistics. Probably the Kuhnian theory of anomalies influenced the researchers' choice to position and name their field as anomalistics.

n. In addition to the Lakatos and Musgrave volume (1970), see Hacking (1983: ch. 5), who distinguished among topic-incommensurability, dissociation, and meaning-incommensurability, and Fuller (1988: ch. 5), who distinguished between textual and ecological incommensurability.

12. Laudan also proposed a solution to the metamethodological problem of justifying justification strategies. In other words, justification strategies often have a hierarchical structure in which theories are justified by methodological rules, which in turn are justified, b-y mo^e general aims, W the justification o£ general, aims opens the door to social and cultural contamination. To escape this problem, Laudan proposed a reticulated model of an interacting triad in which the three categories of theories, methods, and aims are set in motion to justify each other (1984: ch. 3). However, the reticulated model itself then faces the problem of how to justify the complex triangle of relationships.

13. As Rouse (1996) clarifies, this is the first of three programs developed by Laudan, for which the reticulated model served as the second phase. Because Laudan's second and third approaches did not contribute to the theory-choice criteria debate with the specificity that his first program did, I focus only on the first program.

14. See Hacking (1983: ch. 4) for a discussion of the relationship between pragmatism and realism.

15. Constructive empiricism, Van Fraasen's reply (1980) to ontological realism, holds that instead of thinking of theories as either true or false, they should be deemed only empirically adequate or not.

16. The principle of common cause holds that when two events are correlated, either one causes the other or the two are caused by a third event. Salmon (1989: no) defended the principle of common cause as well as an account of causality as a mechanism for the transmission of marks or structures in the world. Kitchner added the idea of the unification of causes to a comprehensive theory (Losee 1993: ch. 15). Van Fraasen has criticized the common cause principle and defended a more

pragmatic, "erotetic" approach to explanation, in which explanation is understood as an answer to a why question (1980). For an introduction that includes essays by Hempel, Salmon, and van Fraasen, see Kourany (1987).

17. Giere (1993, 1995), Fuller (i993a: 5), and probably Haraway (1989). On how evolutionary theory could be refrained according to temporal cultures, see my discussion in Hess (1995: ch. 4).

18. Some of the work in the social studies of science and the philosophy of science in the 1990s has shifted attention from science as representation to science as practice (e.g., Pickering 1992; Rouse 1995). The general philosophical implications of this shift have yet to be determined. For the purposes of the discussion here, moderate constructivism could also be extended to cover practices, much as culture includes both myth and ritual. Furthermore, regarding the general issue of theory-choice criteria to which this chapter is dedicated, the turn to practices has not contributed to the debate by providing grounds for an alternative set of theory-choice criteria.

19. The term "cognitive cronyism" is from Travis and Collins (1991), and "cognitive particularism" is from Cole (1992: 184).

20. For example, see the constructive realism of Giere (1988) or the "hard program" ofSchmaus et al. (1992).

21. Fuller (i993a: 59-61), following Redner (1986).