CHAPTER SEVEN

What Is Science? The Problem of Demarcation

Science is very clearly a conscious artifact of mankind, with well-documented historical origins, with a definable scope and content, and with recognizable professional practitioners and exponents.

—John Ziman (1998)

The need to define "science" transcends mere, neutral classificatory goals. The term and designation *science* has developed cultural and epistemic meaning and authority. This authority should not be assigned lightly—though philosophically it should be questioned in itself. Right or wrong, we often use "science" and its cognates as marks or even totems of authority and power. For example, a new product might be advertised as "scientifically tested," whether it actually is or not, in order to attribute legitimacy to it. Debates as to whether such controversial theories as Marxism, Freudianism, Darwinism, and Intelligent Design are "scientific" also seem to hinge on questions of their legitimacy. By corollary, terms like *pseudo-science* carry a negative cultural meaning and suggest a denial of authority. Recently, the term *junk science* has also entered the public discourse on science with similar denotation and connotation. However, to a large extent, this term is not attributed academically to scientific methodologies or arguments, but rather politically to scientific conclusions one simply does not like. "Science" is not easily defined. It is not a natural kind but a human practice that has evolved over time. It is connected to our natural instinct to know, to investigate. Like philosophy, it too begins with wonder. But where it ends up, its limits and boundaries, are not so clear. Accordingly, attempts to provide a clear demarcation between science and nonscience have been highly contentious. We survey several of these in this chapter.

THE PURPOSE(S) OF SCIENCE

One avenue to begin to understand science is to take the Aristotelian advice to look to purpose. That is, once we identify the goal, aim, or function of science we may well be on our way to understanding what science is in its essence. We may also see that understanding the possible purposes of science gives us a window into distinct theoretical views about science. That is, different theoretical schools will emphasize different purposes to science and these emphasized purposes may be a sign of deeper philosophical commitments and beliefs held by these distinct theoretical schools.

The purposes of science are manifold and each may be taken as merely one among many, while there are some philosophers and philosophies of science that will focus on one of these as indicative or definitive of science itself. Each of these purposes may also be associated with a particular traditional branch of philosophy. One simple purpose of science is a *descriptive* purpose: the idea that the purpose of science is to merely describe the way the world is; what particular and general (classes, species, etc.) things there are (or perhaps were) in the world. Some specific sciences, such as natural history or anatomy, might be understood as wholly or primarily descriptive. But most sciences would not seem to be exhausted by a mere descriptive purpose. This purpose may be connected to the philosophical branch of metaphysics, especially in regard to the presumptions many sciences make about the world and the acceptance of methodology as realist or instrumentalist.

Most scientists and philosophers of science would claim that mere description does not go far enough in understanding science's purpose and hold that science has a further *predictive* purpose: to determine not just the way the world is but in what states and with what things the world will be. The predictive function of science allows us to manipulate nature and apply science as technology. If we can accurately predict the results of our actions on nature, we can use our knowledge of nature to our benefit. This predictive purpose is associated with the epistemology of science, as it is directly connected to how we know what will happen, but also secondarily to the metaphysics and ethics of science. To focus on prediction may sometimes imply an abrogation of metaphysical claims through an instrumentalist or pragmatic approach to scientific method. Moreover, if the predictive function of science allows us to manipulate nature to our benefit, an ethical understanding of what our benefit is would be necessary.

Merely understanding the way the world is or the way the world may be will not be enough for some. For them, science should go further in having an *explanatory* purpose: to explain why events occur or are the way they are, not just that they will occur or that they are the way they are. The explanatory purpose is clearly connected to the epistemology of science, as understanding the why of things and events is clearly a question of knowledge and how that knowledge is attained. Secondarily, the explanatory function may be related to the metaphysics of science, as an explanation may presume an underlying structure of the world.

A somewhat controversial purpose that goes beyond any of these but many argue today is an inescapable part of science is a prescriptive purpose: to aid us in determining what we should do, especially in terms of public policy. This purpose is somewhat controversial because traditionally science is seen as value free or value neutral. But many today argue that not only is this not true, it is not even possible. Merely choosing a research project or the government funding for a particular research goal reflects and expresses a value system. This function is clearly connected to the ethics of science. This prescriptive purpose appears particularly relevant (and in another way, particularly controversial) in the realm of the social sciences, as the subject of these sciences is humans or humanity.

A final possible purpose is a *justificatory* purpose, which can be seen not as an independent purpose in itself, but as secondary to any one or combination of those mentioned previously: science should provide a justification for any of its claims, be they descriptive, predictive, explanatory, or prescriptive. This purpose is clearly associated with the epistemology and logic of science. To justify a claim is to know what makes it knowledge and how knowledge claims are connected through logical connections.

Now, most philosophers and philosophies of science will not choose just one of these as *the* proper function or purpose of science, but will accept some combination of those mentioned previously, while perhaps denying specific ones for important philosophical reasons. Understanding which of these purposes is accepted and how, and which are denied and why, is one means of coming to understand any particular philosopher or philosophy of science.

LOGICAL POSITIVISM: SCIENCE IS VERIFIABILITY

As indicated in Chapter 6, one of the main tenets of logical positivism was the verifiability principle. This principle can also be identified as the essential feature of science itself according to logical positivists. Recall that the verifiability principle holds that the meaning of a statement is to be found in its verification process. On the surface, to verify a claim means merely to demonstrate its truth. But this principle is about verifiability, not verification. “The meaning of a statement,” writes positivist Rudolf Carnap, “lies in the fact that it expresses a (conceivable, not necessarily existing) state of affairs” (1928/1967, p. 325). So the criterion is a hypothetical one: it is not about whether a specific claim is or is not verified but whether it is logically or theoretically possible to verify a specific claim. Again, according to Carnap, “One can know that a statement is meaningful even before one knows whether it is true or false” (1928/1967, p. 325). Thus, even a false claim could be considered scientific, in at least a formal sense. Taken broadly, the verifiability principle addresses the question of meaning in general, not purely within the domain and practice of science. In their discussions of meaning, the logical positivists commonly directed their criticisms and charges of meaninglessness against speculative metaphysical philosophers. Generally, of course, metaphysics is that branch of philosophy that studies reality and various concepts of reality. More specifically, what logical positivists mean by “metaphysics” is philosophy, which presumes “knowledge of a reality transcending the world of science and common sense” (Ayer, 1952/2000, p. 28), or “the field of alleged knowledge of the essence of things which transcends the realm of empirically founded, inductive science” (Carnap, 1932/1959, p. 80). The works of Martin Heidegger, Gottfried Hegel, and F. H. Bradley are often cited (and even mocked) as examples. Hegelian-influenced philosophers were especially prominent in Britain immediately before the development of logical positivism. Philosophers such as Bertrand Russell and G. E. Moore argued for a less idealist, more concrete, and empirical philosophy than what the neo-Hegelians practiced. Neo-Hegelians spoke in terms of conceptually vague notions like the “absolute.” Religious terms such as *soul*, *spirit*, and *god* would also fall into this category for the logical positivists. Based on this “knowledge” beyond science and common sense, metaphysicians would reach what A. J. Ayer characterized as “startling conclusions”: “time and space are unreal, or that nothing really moves, or that there are not many things in the Universe but only one, or that nothing which we perceive through our senses is real or wholly real, or that there is no such thing as matter, or no such things as minds” (Ayer, 1970, p. 64). It did not help that Hegel and Hegelians often repudiated logic as it is traditionally understood. One of the central concepts of Hegel’s philosophy, the dialectic, affirms the meaningfulness of contradictions.

The problem with these metaphysical terms is that they refer to proposed entities that would be “super-empirical” and unable to be deduced from empirical knowledge (Ayer, 1952/2000, p. 28). These metaphysicians often referred to some form of innate knowledge or rational intuition on which they based their knowledge of such entities. The Russell/Moore/Logical Positivist turn toward strict empiricism illustrated that these claims from rational intuition were without foundation, because no two metaphysicians’ rational intuitions seemed completely consistent. What one metaphysician might claim is supported by his rational intuition, another might deny. And there is no independent, objective standard against which to judge the disputed intuitive claims. An empirical standard for knowledge provides, so the logical positivists believed, an independent, objective standard for knowledge claims. By neglecting this empirical standard the metaphysician “produces sentences which fail to conform to the conditions under which alone a sentence can be literally significant” (Ayer, 1952/2000, p. 29). In other words, metaphysicians do not so much produce false claims as they produce meaningless or nonsensical claims.

What are these conditions under which a sentence can be significant? According to logical positivists, to be meaningful, a statement must be analytically true, self-contradictory, or empirically verifiable. These criteria imply the principle of the analytic/synthetic distinction that was an integral element in the logical positivist philosophy. According to this distinction, all statements (assertive sentences) are analytic, self-contradictory, or synthetic. An analytic statement is one in which the predicate is included in the subject. The classic example is “All bachelors are unmarried.” What makes this an analytic statement is that the concept of “being unmarried” is included in the concept of “bachelor.” Another simple example would be “The red ball is red.” Here again, the predicate “red” is included in the subject “red ball.” Given that the predicate of an analytic statement is included in its subject, another quality of analytic statements that follows from this is that they are necessarily true. This quality might also be referred to as being analytically true or true by definition. That is, no matter what, a true analytic statement will be true. Its truth does not depend on any matter of fact or state of the world, but merely on the relation of concepts within the statement itself. Because an analytic statement is necessarily true, its negation would be self-contradictory, which is the second type of statement Hempel refers to in his first category. For example, the statement “The red ball is not red” contradicts itself and can under no circumstance be true. The same can be said of the statements “All bachelors are married” and “Some bachelors are married.” It is the relationship (one of identity or contradiction) among the concepts included in an analytic or self-contradictory statement that makes it cognitively meaningful.

Synthetic statements are empirically verifiable statements. A synthetic statement is one that brings together (synthesizes) two unrelated concepts. This, of course, is in contradistinction to analytic statements in which the predicate is included in the subject.

For example, as noted before, whereas "The red ball is red" is an analytic statement, the statement "The ball is red" would be a synthetic statement. There is no necessary or a priori relationship between the concept "ball" and the concept "red." The predicate is not included in the subject. Rather, the statement synthesizes two unrelated concepts and asserts a (contingent) relationship between them. This relationship is one that can be verified by empirical observation. In the case of this simple statement, one need only observe the ball in question to verify that it is indeed red. It is also worth noting that the logical positivists intended this standard to be interpreted as "verifiability in principle," and not merely "practical verifiability" (Ayer, 1952/2000, p. 29). A practically verifiable statement would be one that, given current knowledge and technology, is in fact possible to verify empirically. However, a statement could still be meaningful if it made a claim that was not practically verifiable but was still verifiable in principle. Consider the Planet X hypothesis. This hypothesis is that there is a planet in our solar system that astronomers have so far failed to detect. The reason for this failure is that Planet X shares an orbit with the planet Earth. However, its orbit is such that it is always on the opposite side of the sun. Thus, we can never see this planet from our own. The claim of Planet X's existence is not a practically verifiable claim. However, if we could build a powerful enough spaceship, we could travel around the sun to verify the existence of this planet, or verify its nonexistence. Thus, the claim of Planet X's existence is verifiable in principle, and thus is a meaningful statement given logical positivist criteria.

Statements of a metaphysical sort are not analytic and are not subject to this type of "experiential test"—even hypothetically (in principle). Carnap uses the example of statements referring to "god." As the term *god* refers to an entity "beyond experience" (Carnap, 1932/1959, p. 66), any claim regarding "god" would not be experientially testable. In other words, such a claim would not be verifiable. Any statement that predicates any quality of "god" could not be said to be true or not true. Such a claim would, hence, be not necessarily false but nonsense. Ayer uses, as an example, a statement from the metaphysical philosopher F. H. Bradley: "the Absolute enters into, but is itself incapable of, evolution and progress" (Ayer, 1952/2000, pp. 29–30). Like metaphysical statements about "god," this metaphysical statement is not verifiable practically or in principle, because, says Ayer, "one cannot conceive of an observation which would enable one to determine whether the Absolute did, or did not, enter into evolution and progress" (1952/2000, p. 30). Thus, like statements about "God," this statement, being neither analytic nor empirically verifiable, is nonsense. Hans Reichenbach used a statement from Hegel as an example: "Reason is substance, as well as infinite power, its own infinite material underlying all the natural and spiritual life; as also the infinite form, that which sets the material in motion" (Reichenbach, 1951, p. 3). Such a statement, Reichenbach claimed, could well inspire a reader to cast the offending book into the fire.[1] Logical positivists even refused to refer to statements such as these as "statements" (given that a "statement" should be meaningful, that is, determinable as true or false) and instead referred to them as "pseudo-statements" or "pseudo-propositions" (Ayer, 1952/2000, p. 29; Carnap, 1928/1967, p. 326; 1932/1959, pp. 61, 68, 69–78).

Before we can address the question of what this theory of meaning says about science, there is an issue of internal debate among logical positivists that is worth noting. One can interpret the verifiability principle in two ways, what Ayer (1952/2000) refers to as strong and weak verifiability. Strong verifiability requires the truth of a claim to be, in principle at least, *conclusively* verifiable, that is, "conclusively established in experience" (Ayer, 1952/2000, p. 30). Under strong verifiability then, only those statements whose truth could be established absolutely and beyond all doubt could be accepted as meaningful. This interpretation sets a rather high bar for meaningfulness. According to Ayer (1952/2000), Moritz Schlick affirmed this high bar. Ayer thought this criterion too restrictive though. Accepting this interpretation would entail rejecting as nonsense many commonly accepted generalities or "general propositions of law" in Ayer's (1952/2000, p. 30) terms. For example, the claim "All men are mortal" could not be accepted as meaningful under this interpretation of verifiability. The problem is that general claims such as these are not only commonly and intuitively accepted but quite useful also. One cannot *prove* (in the mathematical or logical sense of the word) that all men are mortal. One can at best prove that all men observed to date have turned out to be mortal. Yet not accepting such a plainly true proposition would seem foolish. Under the strong verifiability principle, general claims would be nonsense much the same as claims about "God" or "the Absolute." Moritz Schlick negotiated his way around the problem of recognizing the usefulness of general claims while maintaining their status as nonsense under the strong verifiability principle by referring to such claims as "an essentially important type of nonsense" (Ayer, 1952/2000, p. 30).

Ayer favored the weak interpretation of the verifiability principle, describing the strong interpretation as "self-stultifying" (Ayer, 1952/2000, p. 30). He found Schlick's qualification of general laws as *important* nonsense as a hedge: a recognition of a paradox created by this restrictive criterion without actually removing the paradox (Ayer, 1952/2000). According to the weak interpretation, a claim is meaningful only if "it is possible for experience to render it probable" (Ayer, 1952/2000, p. 30). Unlike the strong interpretation, a claim does not have to be provable in an absolute or beyond all doubt sense. It only has to be provable to a degree of probability. Thus, general claims and general laws could be accepted as meaningful under this criterion. One may not be able to prove the truth of a claim like "All men are mortal" to an absolute degree of certainty, but one can prove it to a degree of probability, making it meaningful under the weak verifiability principle. The problem with statements like these is that they do refer to something beyond experience just as metaphysical claims do. The claim "All men are mortal" refers not only to all the men who have died (and thus empirically establishing their mortality), but it also refers to all currently living men whose mortality is yet to be established and to all future men, one of whom, it is possible, could be born immortal. Schlick, by maintaining these types of statements as nonsense, retained a sense of consistency but at the cost of commonsense and an asserted paradox: "important nonsense," whereas Ayer saved common sense and avoided a paradox, and hence he risked charges of inconsistency.

Although the verifiability principle can be taken as a general principle of meaning, it also establishes an important general criterion for scientific methodology and even the meaning of "science" itself. Because the verifiability principle can be interpreted as a general theory of meaning, it cannot act as a sufficient condition for "science." It, however, does seem a necessary condition. That is, it seems a clear thesis of logical positivism that one cannot have "science" without the verifiability principle as a standard. Otherwise, science would devolve into a morass of pseudostatements and lose meaning, efficaciousness, and authority. "All empirical sciences (natural sciences, psychology, cultural science)," writes Carnap, "acknowledge and carry out in practice the requirement that every statement must have factual content … each statement which is to be considered meaningful in any one of these fields … either goes directly back to experience … or it is at least indirectly connected with experience in such a way that it can be indicated which possible experience would confirm or refute it" (1932/1967, p. 328).

The application of the verifiability principle seems at times normative and at times merely descriptive. The aforementioned quote from Carnap appears to apply the principle descriptively. He seems to be merely stating the way that science does in fact work, what scientists do in fact do. However, it can also be applied normatively, to ridicule the nonsense of studies outside the realm of science, to establish a proper philosophical method or to establish the proper meaning and practice of science so as to exclude mere pseudosciences. Regarding the second of these, the logical positivists applied this criterion to philosophy, not only cutting away the metaphysical philosophy of the likes of Hegel, Bradley, and Heidegger but establishing a stricter, more empirical approach to philosophy sometimes called by them "scientific philosophy" (Carnap, 1932/1959, p. 77; Reichenbach, 1951), which would focus on the logical analysis of language. Regarding the third of these, if a field of study cannot demonstrate a consistent employment and deployment of meaningful terms, it would then not be science—even if it patently made claims to be so.

The strong versus weak interpretations of the verifiability principle become particularly important in the context of science. General claims and laws are central to science. Reichenbach (1951) goes so far as to say that "Generalization … is the origin of science" (p. 5). Newton's laws of motion are generalizations. The claim that lead is heavier than gold is a generalization. The claim that a particular therapy is helpful in relieving a particular malady is a generalization. We cannot have science without such generalizations. This is likely the prime reason Schlick qualified these nonsense statements (under his interpretation) as "important" nonsense. Not only are they central to daily life and common sense, but they are also central to science. This is likely also why Ayer risked charges of inconsistency by advocating the weak verifiability principle.

This centrality of generalization to science also points to the centrality of inductive logic, as it is inductive logic that generates generalizations. And, of course, an inductive argument can establish its conclusion only to a degree of probability. Knowledge, then, is generated by a mixture of empirical observation and logic. Empirical observation itself only provides observations of particular phenomena. In order to generate genuine scientific claims, we have to apply inductive logic to these observations. Thus, we get empirical observation plus logical inference, which is the essence of science for the logical empiricists: empiricism plus logic.

POPPER: SCIENCE IS FALSIFIABILITY

Karl Popper (2002) was the first to formally define this issue and coin the term *problem of demarcation* (p. 11). The logical positivists may not have explicitly identified verifiability as a criterion for science, but in their vigorous attacks on metaphysics they clearly implied such a criterion. And Popper, in his writings on the question, clearly accepted verifiability as a logical positivist criterion for science. But in accepting verifiability as the logical positivist criterion, he strongly rejected it as the correct criterion. In doing so, he also denied the centrality of inductive logic to science in favor of deductive logic and distinguished the question of meaning from the question of demarcation.

Popper's stated inspiration for embarking on this particular study was the prevalence of a number of theories in the early 20th century. These theories were Einstein's theory of relativity, Marx's theory of history, Freud's psychoanalysis, and Alfred Adler's individual psychology (Popper, 1963/2000, p. 9). Whereas all four of these theories at the time were receiving a lot of attention from some very smart people, to Popper, Einstein's theory seemed different from the other three. To him, those three appeared more mythical than scientific, more like astrology than astronomy. He then set out to determine what quality or qualities precisely defined this distinction. The first step in this process was to evaluate the logical positivist answer. He found verifiability inadequate as a criterion for demarcation. It is theoretically illiberal, according to Popper, because the "positivists, in their anxiety to annihilate metaphysics, annihilate natural science along with it" (Popper, 2002, p. 13). He makes explicit reference to Schlick and his use of the strong verifiability principle. Recall the problems that the principle had with general laws. Under the strong verifiability principle it becomes impossible to verify as seemingly simple a scientific claim as "arsenic is poisonous." Such a claim depends on inductive logic, as does the verifiability principle itself. Inductively, it is impossible to *conclusively* prove that arsenic is poisonous. To do so, one would have to test every sample of arsenic. One might even have to test every sample on every living being, or at least on every human being if we could delimit the arsenic claim in such a way. Not only did Popper (2002) reject the verifiability principle as a criterion of demarcation, he further rejected induction as a form of logic. In this way he might be said to more consistently follow the ideas of Hume than the logical positivists.

Regarding the theories of Marx, Freud, and Adler, the problem is that they do seem verifiable. These theorists and their followers can point to many confirming instances. Here we get the counterintuitive quality of Popper's position. Confirming or verifying a theory does not really show it to be true. This is due in part to the limits of induction, but there is another related problem as well. Of psychoanalysis and individual psychology specifically, Popper wrote:

> I could not think of any human behavior which could not be interpreted in terms of either theory. It was precisely this fact—that they always fitted, that they were always confirmed—which in the eyes of their admirers constituted the strongest argument in favor of these theories. It began to dawn on me that this apparent strength was in fact their weakness. (1963/2000, p. 10)

The point was that it seemed to Popper that no matter what apparently disconfirming data might be presented, an adherent of either theory could interpret that data in such a way as to make it a confirming instance of the theory, thus "verifying" the theory. This slipperiness of interpretation negates any rigorous standards the verifiability principle might seem to have, making it too "easy" to "verify" a theory. Thus, the apparent strength of these theories—that they seemed to be verified at every turn—turns out to be a weakness. Of Marxism, he said that it makes predictive claims so vague that it would be difficult for them to fail. That is, once again, confirming instances become too promiscuous. But further, of Marxism, he claimed that it has faced disconfirming instances in the past. In response, Marxists have merely adjusted the theory to accommodate this apparently inconsistent data and "save" the theory. And once again, it becomes so easy to "confirm" or "verify" this theory that it seems nothing of any consequence is being verified.

Popper (2002) proposed that the principle of verifiability be replaced by the principle of falsifiability, which holds that "it must be possible for an empirical scientific system to be refuted by experience" (p. 18). This too is a hypothetical or subjunctive principle. It does not hold that a scientific claim, theory, or system *be* refuted or falsified. That would be ridiculous. Rather, the principle holds that any proposed scientific claim, theory, or system must have the possibility of being refuted or falsified by empirical evidence. That is, if the theory were false, there should be a way to empirically and logically demonstrate its falseness. Whereas the verifiability principle relies on a foundation of inductive logic (which is inherently uncertain, or not even really logic according to Popper [2002, p. 18]), the falsifiability principle relies on a foundation of deductive logic. As deductive logic results in conclusions of certainty (not merely probability), science as oriented around the principle of falsifiability would generate knowledge that is certain. Logically, this defines an asymmetry between these two principles. One results in, at best, probable knowledge; the other results in certain knowledge. Logically, the verifiability principle looks like the following in practice:

1. Observation 1 confirms Theory 1.
2. O_2 confirms T_1.
3. O_3 confirms T_1.

   …
   T_1 is (probably) true.

The ellipsis represents an indefinite number of possible observation premises. Given the nature of inductive logic, there is no formal rule regarding the adequate or requisite number of premises to justify a conclusion. The falsifiability principle in practice would employ the deductive *modus tollens* argument form:

1. If A then B.
2. Not B.
3. Therefore, not A.

This is a valid, deductive argument form, meaning that for any argument in this form, if the premises are true, then the conclusion will also be so. This particular form will be explained more fully in the following chapter. In the *modus tollens* form falsifiability would look like the following:

1. If T_1 is true, then Consequence 1 should follow.
2. C_1 does not follow.

   T_1 is not true.

In this case, there is no uncertainty regarding the necessary numbers of premises or observations. A single observation can refute or falsify the theory. For example, in order to *verify* the claim that water boils at 100°C at sea level, one would have to attempt to boil water at sea level and note that it boils at 100°C. Then that experiment would have to be replicated at another sea level locale—then again and again … . But no matter how many observations confirm this claim, it can never be conclusively verified. There is always the possibility that a sea level locale exists where water would not boil at 100°C. However, to falsify this claim requires only one instance in which water did not boil at 100°C at sea level. The same point could be made at the level of scientific theories and systems.

Here, then, is where we find the difference between Einstein's theory and the other theories that Popper seemed to initially intuit as different and problematic. As it is so difficult (if not impossible) to find disconfirming (refuting) observations for those three theories, they can be judged nonfalsifiable and hence nonscientific. Einstein's theory, however, can generate clear and specific predictive consequences, which, if they did not materialize, would refute the theory. An interesting consequence here, though, is that no scientific theory can be conclusively verified. Of course, for Popper, conclusive verification is not a criterion for science. So on that level that "shortcoming" is not a problem. But it does mean that no theory is "true" or a "fact" in the customary use of those words. Every scientific theory is forever open to new testing and forever vulnerable to a potential falsification. This aspect of science imbues the practice for Popper with a level of risk. In fact, he predicated risk itself as a quality of good science. The better, the stronger a theory is, the riskier it is. The problem with Marxism, psychoanalysis, and individual psychology then is that they involve no risk. There is little, if any, chance of falsification, thus no risk. This criterion also suggests that a strong theory be specific. General, vague theories would be difficult to falsify. More specific theories take greater risks (of being falsified) but are bolder and move science and human knowledge forward more.

Popper also distinguished himself on this question from the logical positivists by a certain narrowing of the question. Unlike verifiability for the positivists, falsifiability for Popper does not operate as a general criterion for meaning, only as a demarcation criterion for science and nonscience or pseudoscience. Thus, for Popper, nonscientific claims can be meaningful. Indeed, he noted that many times science has arisen from nonscientific practices, such as chemistry from alchemy. Also, myths, metaphysics, or other cultural forms of knowledge may contain elements or the beginnings of science or a scientific theory without literally being science—according to the falsifiability principle (Popper, 1963/2000).

One possible weakness of the theory that Popper (2002) foresaw is that it might be possible for a theory to evade falsification "by introducing *ad hoc* an auxiliary hypothesis, or by changing *ad hoc* a definition" (p. 20). The problem is that a theory could then become like psychoanalysis or Marxism in becoming nonfalsifiable through changes or interpretations engineered merely to "save" the theory. Thus, if all theories were susceptible to this form of reinterpretation through ad hoc changes, no theory would be falsifiable and there would be no science. This would be an absurd conclusion. To avoid this absurdity, he argued that the point of the empirical scientific method is not to evade falsification (note how he praised "risky" theories) but to expose a theory to falsification "in every conceivable way … not to save the lives of untenable systems but … to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival" (Popper, 2002, p. 20). This "fierce struggle for survival" brings a sense not only of risk but of rigor to science, establishing it as a practice with high epistemic standards and highly justified claims of knowledge.

KUHN: SCIENCE IS PUZZLE SOLVING

Much like the positivists, the problem of demarcation was not of primary importance to Thomas Kuhn, particularly in the context of his most famous work, *The Structure of Scientific Revolutions* (hereafter, *Structure*). However, many found an implied statement regarding that problem in this work; Kuhn (1970) himself later commented on and expanded on the implications of his theory on the problem of demarcation. Early in *Structure* Kuhn writes:

> Historians of science have begun to ask new sorts of questions and to trace different and often less than cumulative, developmental lines for the science… . Seen through the works that result … science does not seem altogether the same enterprise as the one discussed by writers in the older historiographic tradition … these historical studies suggest the possibility of a new image of science. (Kuhn, 1962, p. 3)

These few introductory statements suggest challenges of long-held presumptions about science and new, "revolutionary" ways of looking at science. Merely appealing to historians of science represented a break from earlier philosophy of science, which largely ignored and discounted the lessons of history. This quote also implies that science may not be a simple unified phenomenon or practice, but one that undergoes fundamental changes. A further, deeper implication here is that, in contrast to Popper and the positivists, the essence of science cannot be understood purely by an appeal to methodology, whether that methodology be verifiability or falsifiability. As revolutionary as Kuhn may seem from these statements and as revolutionary as he has come to be considered by reputation, he in fact discounted his disconnection with past philosophy of science and asserted a fundamental connection to past philosophy of science, particularly that of Popper. He believed that his agreements with Popper outnumbered his disagreements. Specifically, he asserted that he disagrees with Popper on the relation of the scientist to tradition and the sufficiency of the falsifiability principle as a criterion of demarcation (Kuhn, 1970, p. 2).

Kuhn's humility and admiration of Popper aside, his disagreements with Popper turn out to be quite profound. Kuhn identified two types of science: normal science and revolutionary or extraordinary science. Normal science, as Kuhn explains, "means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice" (Kuhn, 1962, p. 10). In other words, the practice of normal science presumes and employs a host of beliefs, practices, expectations, and theories that are all part of the scientist's worldview, web of belief, or mind-set. Together these beliefs, practices, expectations, and theories compose a "paradigm." Kuhn has been criticized for employing this term in various ways throughout *Structure*, but a simple definition he provides early on is "an accepted model or pattern" (Kuhn, 1962, p. 23). Within the practice of normal science a paradigm is accepted or presumed and not typically challenged or criticized. Before Kuhn, a scientist would not likely be conscious of working within a paradigm but merely employing the beliefs, techniques, and so forth that are simply proper for the field in question. Thus, the concept of paradigm begins to place into question the assumed ultimate rationality and objectivity of science, as one paradigm is simply one of many and it is questionable whether a scientist could observe her own paradigm from an external, objective viewpoint.

The simplest, most common example of two distinct paradigms covering the same area of science is the distinction between the geocentric theory and the heliocentric theory. The geocentric theory of course posited that the Earth was at the center of the solar system (or even the universe) while the heliocentric theory posited that the sun was at the center. The geocentric theory from a Kuhnian perspective cannot simply be dismissed as prescientific naïveté or religious-based belief. This theory was not only accepted as defining the structure of the universe but was developed, expanded on, given support, and utilized by astronomers for centuries. The movements of planets were observed and measured. New concepts were introduced (epicycles and equants) in order to give the theory better explanatory and predictive power. It, for the centuries for which it was accepted, made sense. The adoption of the heliocentric theory about 500 years ago was a different, more complicated phenomenon, according to Kuhn, than has traditionally been believed. First, the idea of heliocentrism far predated Copernicus, to whom, of course, is attributed the modern heliocentric theory. In the third century BCE, the Greek astronomer Aristarchus proposed a heliocentric theory. The theory would appear again, from time to time, in both Western and Eastern civilizations. Yet, it did not begin to "stick" until the age of Copernicus, Kepler, and Galileo. Kuhn calls this change a paradigm shift. A paradigm shift occurs when a number of observations are noted or results arise ("anomalies") that are not explainable within the presiding paradigm. Now the difficult part here is that there are always anomalies that occur within a paradigm. Most of the time, these anomalies are explained away, often either as human or instrument error. However, at some point a number of anomalies may arise, a "crisis," which makes the paradigm unsustainable. At this time, another paradigm will be adopted. How many anomalies comprise a crisis is not a question that Kuhn addressed, which makes the change of paradigm seem not entirely rational. It also challenges the common view of scientific knowledge as growing cumulatively. The science that occurs at these paradigm shifts is called revolutionary or extraordinary science.

This extraordinary science, as the name suggests, is not the norm of science, though. Most science is normal science: research that occurs within an accepted, unquestioned paradigm. Within normal science, the paradigm provides a framework from within which to work and to provide standards and criteria for what can be accepted as justified knowledge, as answer to research questions. The paradigm is a "promise of success" and normal science is "the actualization of that promise" (Kuhn, 1962, pp. 23, 24). In actualizing that promise, the scientist's function is one that Kuhn describes as puzzle solving. A research question represents a type of puzzle that the scientist sets out to solve within the parameters of the paradigm in which she works. Just as ordinary puzzles—be they Sudoku, crossword puzzles, or chess puzzles—test the ingenuity and skill of the puzzle solver, so do scientific puzzles test the ingenuity and skill of the scientist. Here arises an important distinction from Popper. For Popper, scientific research tests the presuppositions (theories, principles, etc.) of science. But for Kuhn, in normal science, it is the scientist (and her ingenuity and skills) that is tested. Kuhn describes this distinction as the difference between the scientist as "problem-solver" (Popper) and the scientist as "puzzle-solver" (Kuhn, 1970, p. 5). In this formulation then problems test science and puzzles test the scientist. Moreover, like ordinary puzzles, rules are requisite. In order to solve a Sudoku, the puzzle solver must understand the rules restricting each digit 1 to 9 appearing only once in each horizontal line, vertical line, and nine-square box. In order to solve a chess puzzle, the puzzle solver must understand the rules defining and restricting the movements of the various pieces on the board, as well as the rules of castling, and so forth. The scientist too has rules to follow. These rules are provided by the paradigm and define what types of research questions can be asked, what is involved in experimentation, what standards justify a scientific conclusion, and so forth. Just like the Sudoku player, the scientist must stick to these rules. They are enforced by the community through peer review, replication, and other social oversight. Thus, the normal scientist does not question these rules or the paradigm in general, but instead maintains a deep commitment to the rules and the paradigm. This aspect points to a further distinction from Popper. For Popper, science was a constantly critical practice. The scientist must always question her beliefs, presuppositions, even the rules guiding her research. This deep commitment noted by Kuhn once again points to a less than fully rational concept of the practice of science, as rationality is traditionally understood.

John Watkins cleverly describes Kuhn (1970) as up-valuing normal science and down-valuing extraordinary science; in contrast, Popper could be seen as up-valuing revolutionary science and down-valuing normal science. Popper's motto would be "*Revolutions in permanence!*"—and Kuhn's motto would be "*Not nostrums but normalcy!*" (Watkins, 1970, p. 28). For Kuhn, science is primarily the mundane, everyday practice of the scientist in the trenches. Extraordinary science exists, but it is not the norm. Normal science is also the "practice of science [for which] professionals are trained" (Kuhn, 1970, p. 6). Kuhn's focus then when he thinks about the nature of science is the nonsensational, mundane practice of science occurring in labs around the world every day. One can understand this focus. Comparing the occurrence of what he calls extraordinary science to what he calls normal science, the latter seems to be common; the former, rare. Thus, if we were to define "science" as extraordinary science, much of what we refer to as science today would in fact not qualify. "Science" would be restricted to the rare instances, its "occasional revolutionary parts," represented by Popper's examples of Lavoisier's experiment on calcination or the eclipse expedition of 1919 (Kuhn, 1970, p. 6). What happens in between, though, would be a state of limbo following Popper's view. Normal science is more accurately descriptive of "science" than extraordinary science, according to Kuhn, also because the critical attitude Popper identifies with science is not limited to what is typically accepted as science. One can find it in the study of philosophy as well. Thus, this critical attitude, according to Kuhn, is neither a necessary nor a sufficient property of science.

Kuhn was also critical of the focus of Popper's demarcation thesis, the principle of falsifiability. Kuhn took on this element of Popper's philosophy by addressing one of Popper's own examples of what turns out to be a pseudoscience according to his criterion of demarcation: astrology. According to Popper (1963/2000), astrology is not a science but is a pseudoscience because it makes no falsifiable claims. Any seemingly falsifiable or seemingly false claims could be explained away by the adherents uncritically *committed* to the belief system. Kuhn disputed this straightforward claim: "The history of astrology during the centuries when it was intellectually reputable record many predictions that categorically failed" (Kuhn, 1970, p. 7). The dispute here is less philosophical and more a claim of fact that history may answer. Although, arguably, there is room for some degree of interpretability in determining whether a claim is falsifiable or indeed has even been falsified (i.e., "categorically failed"). If Kuhn is correct in this factual dispute, then it would seem that astrology would qualify as a science according to Popper's criterion. He points this out not to argue that astrology is indeed a science but to identify a weakness of Popper's demarcation criterion. Essentially, the criticism here is that Popper's definition is too broad in allowing in practices not commonly or intuitively considered science. According to Kuhn's definition of normal science, the defining criterion is the practice of puzzle solving. It is true, he admitted, that astrology has rules, much like normal science. These rules, though, are not a function of its status as a science but as a "craft" (Kuhn, 1970, p. 8). A craft has pure utilitarian function, as well as means–end engineering. Medicine before the introduction of biomedicine in the 19th century was also, according to Kuhn, a craft. Although crafts like astrology or premodern medicine have rules, their practitioners do not, according to Kuhn, solve puzzles. In astrology, failed predictions do not "give rise to research puzzles, for no man, however skilled, could make use of them in a constructive attempt to revise the astrological tradition" (Kuhn, 1970, p. 9). So, according to Kuhn, astrology is not a science *not* because its claims are not falsifiable—for indeed, according to him, there have been instances of falsification. Rather, it is not a science because it presents its practitioners with no puzzles to solve.

Popper criticized Kuhn's theory as it applies to demarcation on at least two fronts. First, Popper noted, "few, if any, scientists who are recorded by the history of science were 'normal' scientists in Kuhn's sense" (Popper, 1970, pp. 53–54). This statement highlights how fundamental the distinction is between Popper's focus on what Kuhn would call extraordinary science and Kuhn's focus on normal science. Just as Kuhn did not deny the existence of extraordinary science—just that it exemplified the nature or essence of science—Popper did not deny the existence of normal science, or at least the existence of normal scientists. But, according to Popper's estimation, they are not real scientists. The normal scientist is someone who "has been taught badly … taught in a dogmatic spirit … a victim of indoctrination" (Popper, 1970, pp. 52–53). So the normal scientist does exist, but only as one who does not live up to the expectations of real science, according to Popper. The normal scientist has not been properly taught the critical attitude of a scientist, that one should only provisionally accept any scientific claim or theory, that one should not maintain an irrational, dogmatic commitment to any position, theory, or claims. The normal scientist, in Popper's view, has not been taught science but been indoctrinated with a belief system. Kuhn's view is not only mistaken in taking the focus from the "real" scientists; it is also dangerous, claimed Popper. By normalizing the so-called normal scientist, Kuhn discounted or "devalued" the critical aspect of science, leading to a normalization of dogmatism, which would be "a danger to science and, indeed, to our civilization" (Popper, 1970, p. 53).

Popper also referred to the normal scientist as an "applied scientist" as opposed to a "real scientist" whom he refers to as a "pure scientist" (Popper, 1970, p. 53). These designations may well point to a philosophical bias on Popper's part that discounts the workaday scientist who toils in her lab with no recognition. However, this person seems to be the focus for Kuhn. This person also seems to be more common and numerous than the scientific innovators "recorded by the history of science." Of course, no normal scientist has been recorded by history. That is part of what makes them normal scientists. Their existence, their work, and their accomplishments, however, should not be discounted or dismissed. Their accomplishments may not be as flashy or sensational as those of the extraordinary scientists, but they are often important in their own right. Many who study science or the philosophy of science may agree with John Watkins (1970) when he says, "Normal science seems to me to be rather boring and unheroic compared with Extraordinary Science" (p. 31). The extraordinary, revolutionary parts of science may be what attract many to it. The normal scientist does seem dull in comparison to the Newtons, Galileos, and Einsteins of the world. Yet these geniuses, though geniuses they may be, do not operate in a vacuum. Normal scientists may not make revolutionary breakthroughs, but they often make small important steps forward that benefit humanity and may even make small steps toward revolutionary breakthroughs that revolutionary scientists build on.

Popper's second criticism of Kuhn's theory as it applies to demarcation was sounder. He questioned Kuhn's claim that at any one time in any one scientific domain there is but one dominant paradigm that rules. Part of the difficulty of assessing this claim is attempting to demarcate paradigms themselves. But at least in regard to theories of matter, Popper maintained that there has not typically been one dominant paradigm but "constant and fruitful discussion between the competing dominant theories" (Popper, 1970, p. 55). And, depending on how one does demarcate paradigms from one another, it seems possible to raise other examples of not one but competing paradigms ruling within a scientific domain. This criticism reinforces Popper's view of science as essentially critical. It is this critical nature of science, for Popper, that guarantees the highest standards for knowledge, guarantees the rationality of science, and guarantees progress in science that cannot be guaranteed in other fields of endeavor: "In science (and only in science) can we say that we have made genuine progress: that we know more than we did before" (Popper, 1970, p. 57). Of course, part of Kuhn's project seems to be to put this kind of belief (faith?) in the rational progress of science into question. Yet, even without this sense of progress, puzzles are still solved, progress made within paradigms, and revolutions do occur from time to time—often for the good of humanity.

IMRE LAKATOS: SCIENCE IS A RESEARCH PROGRAM(S)

At the risk of oversimplification, it might be said that Imre Lakatos carved a middle path between Popper and Kuhn, attempting to retain a Popperian sense of falsifiability and critical rationality, while integrating a Kuhnian sense of the traditionalism or conventionalism of normal science. The methodology he pursued in carving out this middle path was to criticize Kuhn while apologizing and (possibly) emending the work of Popper. According to Lakatos, Kuhn "fall[s] back on irrationalism" (Lakatos, 1970, p. 93), because the Kuhnian concepts of crisis, paradigm, and paradigm shift, which figure into his theory of scientific change (revolution), are not rationally determinable concepts. Leading science down a path of irrationality was beyond acceptable to Lakatos.

Kuhn arrives at this point of irrationality due to a misinterpretation of Popper on falsificationism, according to Lakatos. Two types of falsificationism can be gleaned from the work of Popper: naive falsificationism and a more sophisticated version. Kuhn's error was to only see the naive version in Popper's work, for only the naive version will fall to Kuhn's criticism of the falsifiability principle. Under naive falsificationism, "any theory which can be interpreted as experimentally falsifiable, is 'acceptable' or 'scientific'" (Lakatos, 1970, p. 116). This is the form of falsificationism discussed earlier in the sections on Popper and Kuhn, for this is the form that most patently comes out of his work. Lakatos, however, gleaned from Popper's work a more sophisticated version in which "a theory is 'acceptable' or 'scientific' only if it has corroborated excess empirical content over its predecessor (or rival), that is, only if it leads to the discovery of novel facts" (Lakatos, 1970, p. 116). Under the naive version, a theory or claim is discarded once it is falsified or refuted. Yet, as Kuhn has pointed out, the history of science and the needs of scientific investigation dispute this version as a descriptive claim and likely also as a normative claim. Under sophisticated falsificationism, a claim or theory is not disposed of due simply to a single disconfirming instance (refutation). Due to the complexity of theory design and the vagaries and variables of research in practice, the easy dismissal of naive falsificationism would be imprudent and counterproductive. Therefore, under sophisticated falsificationism, even though a theory may encounter disconfirming instances, it should be retained if it is corroborated, useful, and productive (of "novel facts")—particularly, more so than its rivals or predecessors. In more colloquial terms, do not throw the baby out with the bath water.

Like Kuhn, however, Lakatos did not restrict his analysis to the level of "theory." Similar to Kuhn's "paradigms" he coined a broad concept referring to a large scientific view or grounding: the research program. Like a paradigm, a research program (or "programme" to use Lakatos's spelling) consists of the basic unquestioned beliefs and rules underlying the work of the scientist in any specific field. He placed the rules into two categories: negative heuristics, which preclude certain paths of research (e.g., in modern biomedicine as a research program, hypotheses positing a demon etiology of disease), and positive heuristics, which suggest or indicate proper research paths to follow (in modern biomedicine, the doctrine of specific etiology [Blaxter, 2004]). Science itself can be seen as one huge research program that is methodologically defined by "Popper's supreme heuristic rule: 'devise conjectures which have more empirical content than their predecessors'" (Lakatos, 1970, p. 132). Yet Lakatos's view, somewhat similar again to Kuhn's, is that science is composed of various research programs, such as Cartesian metaphysics (Lakatos, 1970, p. 132), Newton's gravitational theory (Lakatos, 1970, p. 133), or modern biomedicine. In addition to the two types of rules to be found within research programs, they are also structurally composed of two parts. First, there is a "hard core," from which the negative heuristics steer away criticism. Second, there is a "protective belt," composed of "auxiliary hypotheses," toward which criticism may be directed. The hard core remains intact while the protective belt is open to change. The negative heuristic of the programme forbids us to direct the *modus tollens* at this hard core. Instead, we must use our ingenuity to articulate or even invent "auxiliary hypotheses," which form a *protective belt* around this core, and we must redirect the *modus tollens* to *these* (Lakatos, 1970, p. 133). In this way, a research program maintains a sense of consistency without falling into dogmatism and irrationalism. Note also that the "ingenuity" he refers to is reminiscent of Kuhn's analogy of puzzle solving, while the protective belt invites the kind of critical rationality of Popper's ideal. Regarding Newton's theory of gravitation, the hard core would be composed of Newton's three laws of motion and law of gravitation. The protective belt would be composed of "auxiliary, 'observational' hypothesis [sic] and initial conditions" (Lakatos, 1970, p. 133). This theory, though eventually widely accepted, was initially beset by anomalies. The adherents (as this designation suggests) of the theory stuck to it, or at least stuck to the hard core while adjusting the protective belt to preserve the theory as a whole.

Therefore, according to Lakatos, contrary to Popper, not all scientific beliefs should always be questioned and criticized. Some hard core needs to be preserved in order to provide general, fundamental guidance for research. Yet, contrary to Kuhn, change in science is not simply a matter of "mob psychology." The protective belt changes for rational reasons. In this way, Lakatos claimed to rationalize conventionalism: "We may rationally decide not to allow 'refutations' to transmit falsity to the hard core as long as the corroborated empirical content of the protecting belt of auxiliary hypotheses increases" (Lakatos, 1970, p. 134). In other words, the commitment Kuhn proposed that scientists hold to the rules and beliefs of the paradigm in which they work is, in the context of a research program, not an irrational attachment. Within a research program, this commitment or conventionalism is rational, not religious, emotional, or irrational. Furthermore, Lakatos allows for change not only within the protective belt but to the research program as a whole as well. Just as paradigms shift, so do research programs. But again, the shift will follow a rational process. However, the shift will not occur as simply or quickly as might be suggested by naive falsificationism. As long as the program is still generating new data and a better program is not present, it will be retained. Not only does this standard allow for general, fundamental aims and purposes to scientific investigation, it allows for new theories (as in the earlier example of Newton's gravitational theory) to be given a chance to develop and take hold.

PAUL FEYERABEND: SCIENCE IS ANARCHISM

Austrian-born philosopher Paul Feyerabend carved out a theoretical position on this question different from the positivists, Popper, Kuhn, and Lakatos, and one considered far more "radical" than any of theirs. Feyerabend studied with Popper early in his career and was influenced by him. He later developed theoretical views vastly different from and critical of those of Popper. Popper's view of science, according to Feyerabend's later work, is too reductionist. The logical positivists would be faulted in this regard as well. Feyerabend exhibited an influence of Kuhn in recognizing the historical and other "extra-scientific" influences on science itself. On this point, Feyerabend wrote that, "the actual development of institutions, ideas, practices, and so on, often *does not start from a problem* but rather from some extraneous activity, such as playing, which, as a side effect, leads to developments which later on can be interpreted as solutions to unrealized problems" (Feyerabend, 1975, p. 154). The institution of science, then, cannot be reduced to its supposed logical and empirical methodology. There will be many other influences outside of what is considered science (in a pre-Kuhnian mind-set) that affect the development and essence of science itself.

Although showing influence of Kuhn, Feyerabend was highly critical of Kuhn's theory of science as well. Popper's form of critical rationality would exclude too much from scientific investigation due to the seeming ease with which any claim might seem to be falsified. Kuhn's view of normal science might be too inclusive, but also too exclusive of new ideas in its conventionalism. Feyerabend argued that Kuhn's puzzle-solving essence of normal science might allow more than we would want into the category of science, possibly even allowing organized crime to be seen as a science (Feyerabend, 1970, p. 200). As conventionalist, normal science encourages too much loyalty to the status quo, inhibiting the new ideas necessary for science. However, strictly speaking, according to Feyerabend, there is no such thing as normal science as Kuhn described it. Normal science requires the existence and authority of one paradigm at a time within a discipline, yet there are commonly, claims Feyerabend, "mutually incompatible paradigms" existing in a discipline at once (Feyerabend, 1970, p. 207).

Feyerabend referred to his view of science as epistemological anarchism. Science cannot be reduced to one logical/empirical methodology, nor can it be reduced to one methodology:

> The whole history of thought is absorbed into science and is used for improving every single theory. Nor is political interference rejected. It may be needed to overcome the chauvinism of science that resists alternative to the status quo. (Feyerabend, 1975, p. 33)

Reduction to a methodology or to methodology itself would suffocate needed innovation. As "the world which we want to explore is a largely unknown entity" (Feyerabend, 1975, p. 12), restricting our investigation to one methodology would needlessly, and possibly harmfully, narrow the scope of our perspective and limit our investigations. Even limiting science to what is considered "rational" (in terms of consistency, coherence, etc.) would be too limiting. To demand that new theories be consistent with old, that theories be wholly consistent with the facts, would again unduly restrict investigation and hypothesis creation. Not only that, such consistency cannot really be found in the history of science, especially regarding scientific progress (Feyerabend, 1975, p. 24). Feyerabend's primary principle of science is simply "anything goes." Any rules are damagingly dogmatic. What scientists need more than methodological rules is creativity. Rules inhibit creativity. We see here also a political reflection of the freedom and democracy that characterize modern society, although "anarchism," politically, implies more than the freedom promised by democracy. "Anarchism" implies a complete lack of structure and possibly chaos. But the practice of epistemological anarchism in science will not, according to Feyerabend (1975), lead to chaos because "the human nervous system is too well organized for that" (p. 13).

SUMMARY

In 1981, the state of Arkansas passed a law that required public schools to provide "balanced treatment" in teaching both creation science and evolutionary theory. The state was quickly sued by a group including not only parents, biology teachers, and the National Association of Biology Teachers but various religious groups as well, representing Catholic, Protestant, and Jewish faiths. Put simply, the plaintiffs' claim was that creation science, despite the name, was indeed not science but religion. Thus, for the state to require the teaching of creation science in public schools would be a violation of the First Amendment right to freedom of religion (the Establishment Clause) by imposing a specific religious view on students. We have here a clearly important cultural issue regarding the problem of demarcation. In January of 1982, the U.S. District Court ruled in favor of the plaintiffs and the view that creation science is religion rather than science. The ruling of Judge Overton recognized the difficulty of the demarcation problem and provided reasoning that cannot be reduced to any one theoretical view. Elements of Popper, the logical positivists, and other theorists can be seen informing his argument. This decision did not of course put this issue to rest. It arose again in Dover, Pennsylvania, when the Dover school board voted to include a statement on the limitations of evolutionary theory and the theory of intelligent design as an alternative as part of biology classes at Dover High School. This statement further guided students toward the intelligent design manual *Of Pandas and People*. A group of parents, with the help of the American Civil Liberties Union (ACLU), sued the Dover school board. The plaintiffs contended that intelligent design was merely a thinly veiled version of creation science, and hence a violation of the Establishment Clause once again. Each side presented their arguments, bringing in their own scientists and philosophers to support their respective views. In December 2005, Judge John E. Jones III agreed with the plaintiffs in his decision, presenting much similar reasoning as employed by Judge Overton in 1982.

What is science? We may not have, as yet, answered that question. A simple response, given the multiplicity and uncertainty of theories presented here, is that there is no definition. Yet, given that we seem to be weighing these theories against what seems to be our intuitive or common sense notions of what is and is not science suggests that there is some definitional coherence to what we have in mind when we use this word. Each of these theories may contain some part of the truth. So what we have here is a continuing debate but not a merely academic debate. This question can have important practical implications. Most simply, the determination of this question will affect what we fund as scientific research. And as indicated previously, there are also deep cultural issues affected by this debate.

QUESTIONS FOR REFLECTION

1. Is science primarily descriptive, predictive, or explanatory? Or can none of these be identified as primary?
2. Are certain specific sciences more primarily descriptive, predictive, or explanatory?
3. Is science prescriptive? Does it have its own values? Does it exist to tell us how to live? Or is that a function independent of science?
4. Which provides a more stable, justified criterion for science: verifiability or falsifiability?
5. Is it possible to have a meaningful statement that is not verifiable?
6. Based on the various definitions of science presented in this chapter, would creationism and intelligent design theory be considered sciences? Would biological evolution?
7. Based on the various definitions of science presented in this chapter, would nursing be considered a science?
8. Do nurses engage in puzzle solving in the manner that Kuhn describes?
9. Has there been a paradigm shift, a revolution as Kuhn describes, in nursing?
10. Can science be anarchic, as Feyerabend advocates?

NOTE

1. Reichenbach is clearly alluding to one of the positivists' empiricist, anti-metaphysical forbears, David Hume, who famously ended his book, *An Enquiry Concerning Human Understanding*, with these dramatic words: "If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask; *Does it contain any abstract reasoning concerning quantity or number?* No. *Does it contain any experimental reasoning concerning matter of fact and existence?* No. Commit it then to the flames: for it can contain nothing but sophistry and illusion" (Hume, 1748/1974, p. 430).

REFERENCES

Ayer, A. J. (1970). *Metaphysics and common sense*. San Francisco, CA: Freeman, Cooper and Company.
Ayer, A. J. (2000). The elimination of metaphysics. In J. McErlean (Ed.), *Philosophies of science: From foundations to contemporary issues* (pp. 28–34). Belmont, CA: Wadsworth Publishing. (Original work published 1952)
Blaxter, M. (2010). *Health*. Malden, MA: Polity Press.
Carnap, R. (1959). The elimination of metaphysics through logical analysis of language. In A. J. Ayer (Ed.), *Logical positivism* (pp. 60–81). Westport, CT: Greenwood Press. (Original work published 1932)
Carnap, R. (1967). *The logical structure of the world: Pseudoproblems in philosophy* (R. A. George, Trans.). Berkeley and Los Angeles, CA: University of California Press. (Original work published 1928)
Feyerabend, P. K. (1970). Consolations for the specialist. In I. Lakatos & A. Musgrave (Eds.), *Criticism and the growth of knowledge* (pp. 197–231). Cambridge, UK: Cambridge University Press.
Feyerabend, P. K. (1975). *Against method*. London, UK, and New York, NY: Verso.
Hume, D. (1974). An enquiry concerning human understanding. In J. Locke, G. Berkeley, & D. Hume (Authors), *The empiricists: Locke, Berkeley, Hume* (pp. 307–430). Garden City, NY: Anchor Books. (Original work published 1748)
Kuhn, T. S. (1962). *The structure of scientific revolutions*. Chicago, IL, and London, UK: University of Chicago Press.
Kuhn, T. S. (1970). Logic of discovery or psychology of research? In I. Lakatos & A. Musgrave (Eds.), *Criticism and the growth of knowledge* (pp. 1–23). Cambridge, UK: Cambridge University Press.
Lakatos, I. (1970). Falsification and the methodology of scientific research programmes. In I. Lakatos & A. Musgrave (Eds.), *Criticism and the growth of knowledge* (pp. 91–196). Cambridge, UK: Cambridge University Press.
Popper, K. R. (1970). Normal science and its dangers. In I. Lakatos & A. Musgrave (Eds.), *Criticism and the growth of knowledge* (pp. 51–58). Cambridge, UK: Cambridge University Press.
Popper, K. R. (2000). Science: Conjectures and refutation. In T. Schick, Jr. (Ed.), *Readings in the philosophy of science: From positivism to postmodernism* (pp. 9–13). Mountain View, CA: Mayfield Publishing. (Original work published 1963)
Popper, K. R. (2002). *The logic of scientific discovery*. London, UK, and New York, NY: Routledge.
Reichenbach, H. (1951). *The rise of scientific philosophy*. Berkeley and Los Angeles, CA: University of California Press.
Watkins, J. (1970). Against "normal science." In I. Lakatos & A. Musgrave (Eds.), *Criticism and the growth of knowledge* (pp. 25–37). Cambridge, UK: Cambridge University Press.
Ziman, J. (1998). What is science? In E. D. Klemke, R. Hollinger, & D. W. Rudge (Eds.), *Introductory readings in the philosophy of science* (pp. 48–53). Amherst, NY: Prometheus Books.