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The Demarcation Problem

A (Belated) Response to Laudan

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The Premature Obituary of the Demarcation Problem

The "demarcation problem," the issue of how to separate science from pseudoscience, has been around since fall 1919—at least according to Karl Popper's (1957) recollection of when he first started thinking about it. In Popper's mind, the demarcation problem was intimately linked with one of the most vexing issues in philosophy of science, David Hume's problem of induction (Vickers 2010) and, in particular, Hume's contention that induction cannot be logically justified by appealing to the fact that "it works," as that in itself is an inductive argument, thereby potentially plunging the philosopher straight into the abyss of a viciously circular argument.

Popper famously thought he had solved both the demarcation and induction problems in one fell swoop, by invoking falsification as the criterion that separates science from pseudoscience. Not only, according to Popper, do scientific hypotheses have to be falsifiable (while pseudoscientific ones are not), but since falsification is an application of *modus tollens*, and hence a type of deductive thinking, we can get rid of induction altogether as the basis for scientific reasoning and set Hume's ghost to rest once and for all.

As it turns out, however, although Popper did indeed have several important things to say about both demarcation and induction, philosophers are still very much debating both issues as live ones (see, e.g., Okasha 2001 on induction, and Hansson 2009 on demarcation). The fact that we continue to discuss the issue of demarcation may seem peculiar, though, considering that Laudan (1983) allegedly laid to rest the problem once and for all. In a much referenced paper quite definitively entitled "The Demise of the Demarcation Problem," Laudan concluded that "the [demarcation] question is both uninteresting and, judging by its checkered past, intractable. If we would stand up and be counted on the side of reason, we ought to drop terms like 'pseudoscience' and 'unscientific' from our vocabulary" (Laudan 1983, 125).

At the risk of being counted on the side of unreason, in this chapter I argue that Laudan's requiem for the demarcation problem was much too premature. First, I quickly review Popper's original arguments concerning demarcation and falsification (but not those relating to induction, which is beyond the scope of this contribution); second, I comment on Laudan's brief history of the demarcation problem as presented in parts 2 and 4 of his paper; third, I argue against Laudan's "metaphilosophical interlude" (part 3 of his paper), where he sets out the demarcation problem as he understands it; and last, I propose to rethink the problem itself, building on an observation made by Kuhn (1974, 803) and a suggestion contributed by Dupré (1993, 242). (Also see in this volume, Boudry, chapter 5; Hansson, chapter 4; Koertge, chapter 9; and Nickles, chapter 6.)

Popper's Attack

Popper (1957) wanted to distinguish scientific theories or hypotheses from nonscientific and pseudoscientific ones, and was unhappy with what he took to be the standard answer to the question of demarcation: science, unlike pseudoscience (or "metaphysics"), works on the basis of the empirical method, which consists of an inductive progression from observation to theories. If that were the case, Popper reckoned, astrology would have to rank as a science, albeit as a spectacularly unsuccessful one (Carlson 1985). Popper then set out to compare what in his mind were clear examples of good science (e.g., Albert Einstein's general theory of relativity) and pseudoscience (e.g., Marxist theories of history, Freudian psychoanalysis, and Alfred Adler's "individual psychology") to figure out what exactly distinguishes the first from the second group. I use a much broadened version of the same comparative approach toward the end of this essay to arrive at my own proposal for the problem raised by Popper.

Popper was positively impressed by the then recent spectacular confirmation of Einstein's theory after the 1919 total solar eclipse. Photographs taken by Arthur Eddington during the eclipse confirmed a daring and precise prediction made by Einstein, concerning the slight degree by which light coming from behind the sun would be bent by the latter's gravitational field. By the same token, however, Popper was highly unimpressed by Marxism, Freudianism, and Adlerianism. For instance, here is how he recalls his personal encounter with Adler and his theories:

> Once, in 1919, I reported to [Adler] a case which to me did not seem particularly Adlerian, but which he found no difficulty in analysing in terms of his theory of inferiority feelings, although he had not even seen the child. Slightly shocked, I asked him how he could be so sure. "Because of my thousandfold experience," he replied; whereupon I could not help saying: "And with this new case, I suppose, your experience has become thousand-and-one-fold." (Popper 1957, sec. 1)

Regardless of whether one agrees with Popper's analysis of demarcation, there is something profoundly right about the contrasts he sets up between relativity theory and psychoanalysis or Marxist history: anyone who has had even a passing acquaintance with both science and pseudoscience cannot but be compelled to recognize the same clear difference that struck Popper as obvious. I maintain in this essay that, as long as we agree that there *is* indeed a recognizable difference between, say, evolutionary biology on the one hand and creationism on the other, *then* we must also agree that there are demarcation criteria—however elusive they may be at first glance.

Popper's analysis led him to a set of seven conclusions that summarize his take on demarcation (Popper 1957, sec. 1):

1. Theory confirmation is too easy.
2. The only exception to statement 1 is when confirmation results from risky predictions made by a theory.
3. Better theories make more "prohibitions" (i.e., predict things that should *not* be observed).
4. Irrefutability of a theory is a vice, not a virtue.
5. Testability is the same as falsifiability, and it comes in degrees.
6. Confirming evidence counts only when it is the result of a serious attempt at falsification (this is, it should be noted, somewhat redundant with statement 2 above).

7. A falsified theory can be rescued by employing ad hoc hypotheses, but this comes at the cost of a reduced scientific status for the theory in question.

The problems with Popper's solution are well known, and we do not need to dwell too much on them. Briefly, as even Popper acknowledged, falsificationism is faced with (and, most would argue, undermined by) the daunting problem set out by Pierre Duhem (see Needham 2000). The history of science clearly shows that scientists do *not* throw a theory out as soon as it appears to be falsified by data, as long as they think the theory is promising or has been fruitful in the past and can be rescued by *reasonable* adjustments of ancillary conditions and hypotheses. It is what Johannes Kepler did to Nicolaus Copernicus's early insight, as well as the reason astronomers retained Newtonian mechanics in the face of its apparent inability to account for the orbit of Uranus (a move that quickly led to the discovery of Neptune), to mention but two examples.[1] Yet, as Kuhn (1974, 803) aptly noticed, even though his and Popper's criteria of demarcation differed profoundly (and he obviously thought Popper's to be mistaken), they did seem to agree on where the fault lines run between science and pseudoscience: which brings me to an examination and critique of Laudan's brief survey of the history of demarcation.

Laudan's Brief History of Demarcation

Two sections of Laudan's (1983, secs. 2, 4) critique of demarcation are devoted to a brief critical history of the subject, divided into "old demarcationist tradition" and "new demarcationist tradition" (and separated by the "metaphilosophical interlude" in section 3, to which I come next). Though much is right in Laudan's analysis, I disagree with his fundamental take on what the history of the demarcation problem tells us: for him, the rational conclusion is that philosophers have failed at the task, probably because the task itself is hopeless. For me, the same history is a nice example of how philosophy makes progress: by considering first the obvious moves or solutions, then criticizing them to arrive at more sophisticated moves, which are in turn criticized, and so on. The process is really not entirely disanalogous with that of science, except that philosophy proceeds in logical space rather than by empirical evidence.

For instance, Laudan is correct that Aristotle's goal of scientific analysis as proceeding by logical demonstrations and arriving at universals is simply not attainable. But Laudan is too quick, I think, in rejecting Parmenides' distinction between *episteme* (knowledge) and *doxa* (opinion), a rejection that he traces to the success of fallibilism in epistemology during the nineteenth century (more on this in a moment). But the dividing line between knowledge and opinion does not have (and in fact *cannot be*) sharp, just like the dividing line between science and pseudoscience cannot be sharp, so that fallibilism does not, in fact, undermine the possibility of separating knowledge from mere opinion. Fuzzy lines and gradual distinctions—as I argue later—still make for useful separations.

Laudan then proceeds with rejecting Aristotle's other criterion for demarcation, the difference between "know-how" (typical of craftsmen) and "know-why" (what the scientists are aiming at), on the ground that this would make pre-Copernican astronomy a matter of craftsmanship, not science, since pre-Copernicans simply knew how to calculate the positions of the planets and did not really have any scientific idea of what was actually causing planetary motions. Well, I will bite the bullet here and agree that protoscience, such as pre-Copernican astronomy, does indeed share some aspects with craftsmanship. Even Popper (1957, sec. 2) agreed that science develops from protoscientific myths: "I realized that such myths may be developed, and become testable; and that a myth may contain important anticipations of scientific theories."

Laudan makes much of Galileo Galilei's and Isaac Newton's contentions that they were not after causes, *hypothesis non fingo* to use Newton's famous remark about gravity, and yet they were surely doing science. Again, true enough, but both of those great thinkers stood at the brink of the historical period where physics was transitioning from protoscience to mature science, so that it was clearly way too early to search for causal explanations. But no physicist worth her salt today (or, indeed, shortly after Newton) would agree that one can be happy with a science that ignores the search for causal explanations. Indeed, historical transitions away from pseudoscience, when they occur (think of the difference between alchemy and chemistry), involve intermediate stages similar to those that characterized astronomy in the sixteenth and seventeenth centuries and physics in the seventeenth and eighteenth centuries. But had astronomers and physicists not eventually abandoned Galileo's and Newton's initial caution about hypotheses, we would have had two aborted sciences instead of the highly developed disciplines that we so admire today.

Laudan then steps into what is arguably one of the most erroneous claims of his paper: the above mentioned contention that the onset of fallibilism in epistemology during the nineteenth century meant the end of any meaningful distinction between knowledge and opinion. If so, I wager that scientists themselves have not noticed. Laudan does point out that "several nineteenth century philosophers of science tried to take some of the sting out of this *volte-face* [i.e., the acknowledgment that absolute truth is not within the grasp of science] by suggesting that scientific opinions were more probable or more reliable than non-scientific ones" (Laudan 1983, 115), leaving his readers to wonder why exactly such a move did not succeed. Surely Laudan is not arguing that scientific "opinion" is *not* more probable than "mere" opinion. If he were, we should count him amongst postmodern epistemic relativists, a company that I am quite sure he would eschew.

Laudan proceeds to build his case against demarcation by claiming that, once fallibilism was accepted, philosophers reoriented their focus to investigate and epistemically justify science as a *method* rather than as a body of knowledge (of course, the two are deeply interconnected, but we will leave that aside for the present discussion). The history of that attempt naturally passes through John Stuart Mill's and William Whewell's discussions about the nature of inductive reasoning. Again, Laudan reads this history in an entirely negative fashion, while I—perhaps out of a naturally optimistic tendency—see it as yet another example of progress in philosophy. Mill's ([1843] 2002) five methods of induction and Whewell's (1840) concept of inference to the best explanation represent marked improvements on Francis Bacon's (1620) analysis, based as it was largely on enumerative induction. These are milestones in our understanding of inductive reasoning and the workings of science, and to dismiss them as "ambiguous" and "embarrassing" is both presumptuous and a disservice to philosophy as well as to science.

Laudan then moves on to twentieth-century attempts at demarcation, beginning with the logical positivists. It has become a fashionable sport among philosophers to dismiss logical positivism out of hand, and I am certainly not about to mount a defense of it here (or anywhere else, for that matter). But, again, it strikes me as bizarre to argue that the exploration of another corner of the logical space of possibilities for demarcation—the positivists' emphasis on theories of meaning—was a waste of time. It is *because* the positivists and their critics explored and eventually rejected that possibility that we have made further progress in understanding the problem. This is the general method of philosophical inquiry, and for a philosopher to use these "failures" as a reason to reject an entire project is akin to a scientist pointing out that because Newtonian mechanics turned out to be wrong, we have made no progress in our understanding of physics.

After dismissing the positivists, Laudan turns his guns on Popper, another preferred target amongst philosophers of science. Here, however, Laudan comes close to admitting what a more sensible answer to the issue of demarcation may turn out to be, one that was tentatively probed by Popper himself: "One might respond to such criticisms [of falsificationism] by saying that scientific status is a matter of degree rather than kind" (Laudan 1983, 121). One might indeed do so, but instead of pursuing that possibility, Laudan quickly declares it a dead end on the grounds that "acute technical difficulties confront this suggestion." That may be the case, but it is nonetheless true that within the sciences themselves there has been quite a bit of work done (admittedly, much of it since Laudan's paper) to make the notion of quantitative comparisons of alternative theories more rigorous. These days this is done by way of either Bayesian reasoning (Henderson et al. 2010) or some sort of model selection approach like the Akaike criterion (Sakamoto and Kitagawa 1987). It is beyond me why this sort of approach could not be one way to pursue Popper's eminently sensible intuition that scientificity is a matter of degrees. Indeed, I argue below that something along these lines is actually a much more promising way to recast the demarcation problem, following an early suggestion by Dupré (1993). For now, though, suffice it to say that even scientists would agree that some hypotheses are more testable than others, not just when comparing science with proto- or pseudoscience, but within established scientific disciplines themselves, even if this judgment is not exactly quantifiable. For instance, evolutionary psychology's claims are notoriously far more difficult to test than similarly structured hypotheses from mainstream evolutionary biology, for the simple reason that human behavioral traits happen to be awful subjects of historical investigation (Kaplan 2002; Pigliucci and Kaplan 2006, chap. 7). Or consider the ongoing discussion about the (lack of) testability of superstring and allied family of theories in fundamental physics (Voit 2006; Smolin 2007).

Laudan eventually gets to what really seems to be bothering him: "Unwilling to link scientific status to any evidential warrant, twentieth century demarcationists have been forced into characterizing the ideologies they oppose (whether Marxism, psychoanalysis or creationism) as untestable in principle. Very occasionally, that label is appropriate" (Laudan 1983, 122). I am not sure why ideology needs to be brought in. I am certainly not naive enough to suggest that anyone—scientists, philosophers, or pseudoscientists—do not subscribe to ideological positions that influence their claims. But surely we can constructively do philosophy nonetheless, and do not have to confine ourselves to politics and psychology. Popper actually wrote that "the Marxist theory of history, in spite of the serious efforts of some of its founders and followers, ultimately adopted this soothsaying practice [making its predictions so vague that they become irrefutable]. In some of its earlier formulations (for example in Marx's analysis of the character of the 'coming social revolution') their predictions were testable, and in fact falsified" (Popper 1957, sec. 2). In other words, Popper saw Marxist theories of history as analogous to the modern case of cold fusion (Huizenga 1992), an initially legitimate scientific claim that was eventually falsified but that degenerated into a pseudoscience in the hands of a small cadre of people who simply refuse to give up the idea regardless of the evidence.

As far as Freudian and Adlerian theories are concerned, again they are no longer taken seriously as scientific ideas by the practicing cognitive science community, as much as they were important (particularly Freud's) in the historical development of the field (see Cioffi, this volume). When it comes to creationism, things are a bit more complicated: very few scientists, and possibly philosophers, would maintain that *specific* creationist claims are not testable. Just as in the case of claims from, say, astrology or parapsychology, one *can* easily test young creationists' contention that the earth is only a few thousand years old. But these tests do not make a science out of creationism for the simple reason that either one must accept that the contention has been conclusively falsified, or one must resort to the inscrutable and untestable actions, means, and motives of a creator god. When a young-earth creationist is faced with geological evidence of an old earth, he has several retorts that seem completely logical to him, even though they actually represent the very reasons why creationism is a pseudoscience: the methods used to date rocks are flawed (for reasons that remain unexplained); the laws of physics have changed over time (without any evidence to support the suggestion); or God simply created a world that *looks* like it is old so that He could test our faith (called "last Thursday" defense, which deserves no additional commentary). So, *pace* Laudan, there are perfectly good, principled, not ideological reasons to label Marxism, Freudianism, and creationism as pseudosciences—even though the details of these reasons vary from case to case.

The rest of Laudan's critique boils down to the argument that no demarcation criterion proposed so far can provide a set of necessary and sufficient conditions to define an activity as scientific, and that the "epistemic heterogeneity of the activities and beliefs customarily regarded as scientific" means that demarcation is a futile quest. I agree with the former point, but I argue below that it represents a problem only for a too narrowly constructed demarcation project; the second point has some truth to it, but its extent and consequences are grossly exaggerated by Laudan within the context of this discussion.

Laudan's "Metaphilosophy"

Laudan maintains that the debate about demarcation hinges on three considerations that he labels as "metaphilosophical" (though it is not clear to this reader, at least, why the "meta" prefix is necessary). Briefly, these are: "(1) What conditions of adequacy should a proposed demarcation criterion satisfy? (2) Is the criterion under consideration offering necessary or sufficient conditions, or both, for scientific status? (3) What actions or judgments are implied by the claim that a certain belief or activity is 'scientific' or 'unscientific'?" (Laudan 1983, 117). As we shall see, I agree with Laudan's answer to question 1, I think that question 2 is too simplistic as formulated, and I forcefully reject his answer to question 3.

Laudan correctly argues (question 1) that modern philosophers thinking about demarcation ought to take seriously what most people, particularly most scientists, actually agree to count as science and pseudoscience. That is, it would be futile to pursue the question in a Platonic way, attempting to arrive at a priori conclusions regardless of whether and to what extent they match scientists' (and most philosophers') intuitions about what science is and is not. Indeed, I think of the target of demarcation studies along the lines sketched in figure 1.1: some activities (and the theories that characterize them) represent established science (e.g., particle physics, climate science, evolutionary biology, molecular biology); others are often treated as "soft" sciences (e.g., economics, psychology, sociology; Pigliucci 2002), characterized by some of that "epistemic heterogeneity" referred to above; yet more efforts are best thought of as proto- or quasi-scientific (e.g., the Search for Extraterrestrial Intelligence, superstring physics, at least some evolutionary psychology, and scientific approaches to history); finally, a number of activities unquestionably represent what most scientists and philosophers would regard as pseudoscience (Intelligent Design "theory," astrology, HIV denialism, etc.). Figure 1.1 is obviously far from exhaustive, but it captures

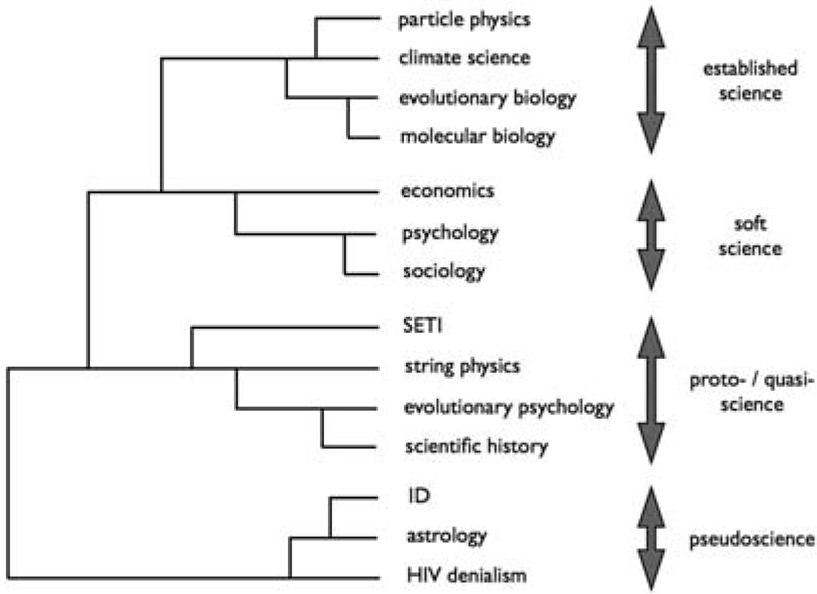

Figure 1.1

Laudan's idea that—no matter how we philosophize about it—demarcation analyses should come up with something that looks like the cluster diagram I sketched, or we would have reasonable doubts that the analysis was not on the right track. To some this might seem like an undue concession to empirical evidence based on common practice and intuition, and one could argue that philosophical analysis is most interesting when it does *not* support common sense. That may be, but our task here is to understand what differentiates a number of actual human practices, so empirical constraints are justified, within limits.

I also agree with Laudan (1983, 118) that "minimally, we expect a demarcation criterion to identify the *epistemic* or *methodological* features which mark off scientific beliefs from unscientific ones," though these criteria (necessarily plural, I think) would have to include much more than Laudan was likely thinking about, for instance, considerations of science as a social activity of a particular kind, with a number of structures in place (e.g., peer review) and desiderata (e.g., cultural diversity) that contribute indirectly to its epistemic and methodological features (Longino 1990).

My first major departure from Laudan's "metaphilosophy" is with respect to his answer to question 2 above: "Ideally, [a demarcation criterion] would specify a set of individually necessary and jointly sufficient conditions for deciding whether an activity or set of statements is scientific or unscientific" (Laudan 1983, 118). He goes on to clarify that a set of necessary but not sufficient conditions would permit us to point to activities that are not scientific (those lacking the necessary conditions) but could not specify which activities are indeed scientific. Conversely, a set of sufficient (but not necessary) conditions would tell us what counts as science, but not what is pseudoscientific. Hence the need for necessary and sufficient conditions (though no single set of criteria needs to be both at the same time).

This strikes me as somewhat old-fashioned, particularly for someone who has been telling his readers that many of philosophy's classic pursuits—such as a priori truths and the search for logical demonstrations—went out the window with the advent of more nuanced philosophical analyses in the modern era. It seems like the search for sets of necessary and sufficient conditions to sharply circumscribe concepts that are clearly not sharp in themselves ought to give pause at least since Ludwig Wittgenstein's talk of family resemblance concepts—which inspired the above mentioned suggestion by Dupré (1993).

As is well known, Wittgenstein (1958) discussed the nature of complex concepts that do not admit of sharp boundaries—or of sets of necessary and sufficient conditions—such as the concept of game. He suggested that the way we learn about these concepts is by example, not through logical definitions: "How should we explain to someone what a game is? I imagine that we should describe games to him, and we might add: 'This and similar things are called games.' and do we know any more about it ourselves? Is it only other people whom we cannot tell exactly what a game is? . . . But this is not ignorance. We do not know the boundaries because none have been drawn. . . . We can draw a boundary for a special purpose. Does it take that to make the concept usable? Not at all!" (Ibid., 69).

Figure 1.2 is my graphic rendition of Wittgenstein's basic insight: games make up a family resemblance concept (also known as a "cluster," in analogy to the type of diagram in figure 1.1) that cannot be captured by a set of necessary and sufficient conditions. Any such set will necessarily leave out some activities that ought to be considered as legitimate games while letting in activities that equally clearly do not belong there. But Wittgenstein correctly argued that this is neither the result of our epistemic limitations nor of some intrinsic incoherence in the concept itself. It is the way in which "language games" work, and philosophy of science is no exception to the general idea

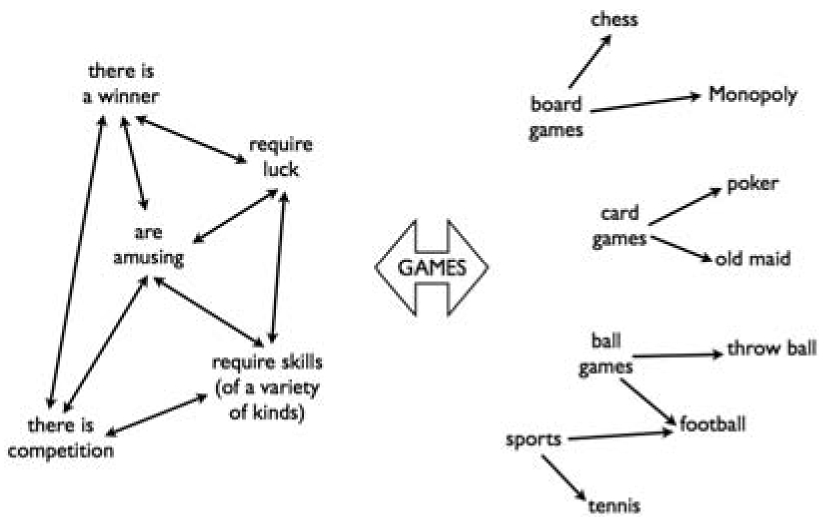

Figure 1.2

of a language game. I return to the possibility of understanding science as a Wittgenstein-type cluster concept below to make it a bit more precise.

I also markedly disagree with Laudan in answer to his question 3 above, where he says:

> Precisely because a demarcation criterion will typically assert the epistemic superiority of science over non-science, the formulation of such a criterion will result in the sorting of beliefs into such categories as "sound" and "unsound," "respectable" and "cranky," or "reasonable" and "unreasonable." Philosophers should not shirk from the formulation of a demarcation criterion merely because it has these judgmental implications associated with it. Quite the reverse, philosophy at its best should tell us what is reasonable to believe and what is not. But the value-loaded character of the term "science" (and its cognates) in our culture should make us realize that the labeling of a certain activity as "scientific" or "unscientific" has social and political ramifications which go well beyond the taxonomic task of sorting beliefs into two piles. (Laudan 1983, 119–20)

Seems to me that Laudan here wants to have his cake and eat it too. To begin with, the "value-loaded" character of science is not exactly an unqualified social positive for all things labeled as "scientific." We regularly see large sections of the public, especially in the United States, who flatly reject all sorts of scientific findings when said public finds them ideologically inconvenient or simply contrary to pet notions of one sort or another. Just think about the number of Americans who deny the very notion of human-caused climate change or who believe that vaccines cause autism—both positions held despite an overwhelming consensus to the contrary on the part of the relevant scientific communities. Obviously, labeling something "scientific" does not guarantee acceptance in society at large.

More important, Laudan simply cannot coherently argue that "philosophy at its best should tell us what is reasonable to believe and what is not" and then admonish us that "[the] social and political ramifications . . . go well beyond the taxonomic task of sorting beliefs into two piles." Of course there are political and social implications. Indeed, I would argue that if the distinction between science and pseudoscience did *not* have political and social implications, then it would merely be an academic matter of little import outside of a small cadre of philosophers of science. There simply is no way, nor *should* there be, for the philosopher to make arguments to the rest of the world concerning what is or is not reasonable to believe without not just having, but *wanting* political and social consequences. This is a serious game, which ought to be played seriously.

Rethinking Demarcation

As Bacon (1620) rightly admonished us, it is not good enough to engage in criticism (*pars destruens*); one also ought to come up with positive suggestions on how to move ahead (*pars construens*). So far I have built an argument against Laudan's premature death certificate for the demarcation problem, but I have also hinted at the directions in which progress can reasonably be expected. I now briefly expand on those directions.

The starting point is provided by Dupré's (1993) suggestion to treat science (and therefore pseudoscience) as a Wittgensteinian family resemblance, or cluster concept, along the lines sketched in figure 1.1. As is well known—and as illustrated for the concept of game in figure 1.2—family resemblance concepts are characterized by a number of threads connecting instantiations of the concept, with some threads more relevant than others to specific instantiations, and indeed sometimes with individual threads entirely absent from individual instantiations. For example, while a common thread for the concept of games is that there is a winner, this is not required in all instantiations of the concept (think of solitaire).

Several useful concepts within science itself are best thought of as Wittgensteinian in nature, for instance, the idea of biological species (Pigliucci 2003). The debate on how exactly to define species has been going on for a long time in biology, beginning with Aristotle's essentialism and continuing through Ernst Mayr's (1996) "biological" species concept (based on reproductive isolation) to a number of phylogenetic concepts (i.e., based on ancestry-descendant relations, see De Queiroz 1992). The problem can also be seen as one generated by the same sort of "metaphilosophy" adopted by Laudan: the search for a small set of jointly necessary and sufficient conditions adequate to determine whether a given individual belongs to a particular species or not. My suggestion in that case—following up on an original remark by Hull (1965) and in agreement with Templeton's (1992) "cohesion" species concept—was that species should be treated as cluster concepts, with only a few threads connecting very different instantiations like those represented by, say, bacterial and mammalian species, and a larger number of threads connecting more similarly circumscribed types of species, like vertebrates and invertebrates, for instance.

Clearly, a concept like science is at least as complex as one like "biological species," which means that the number of threads underlying the concept, as well as their relative importance for any given instantiation of the concept, are matters for in-depth discussions that are beyond the scope of this chapter. However, I am going to provide two complementary sketches of how I see the demarcation problem, which I hope will move the discussion forward.

At a very minimum, two "threads" run throughout any meaningful treatment of the differences between science and pseudoscience, as well as of further distinctions within science itself: what I label "theoretical understanding" and "empirical knowledge" in figure 1.3. Presumably if there is anything we can all agree on about science, it is that science attempts to give an empirically based theoretical understanding of the world, so that a scientific theory has to have both empirical support (vertical axis in figure 1.3) and internal coherence and logic (horizontal axis in figure 1.3). I am certainly not suggesting that these are the *only* criteria by which to evaluate the soundness of a science (or pseudoscience), but we need to start somewhere. And of course, both these variables in turn are likely decomposable into several factors related in complex, possibly nonlinear ways. But again, one needs to start somewhere.

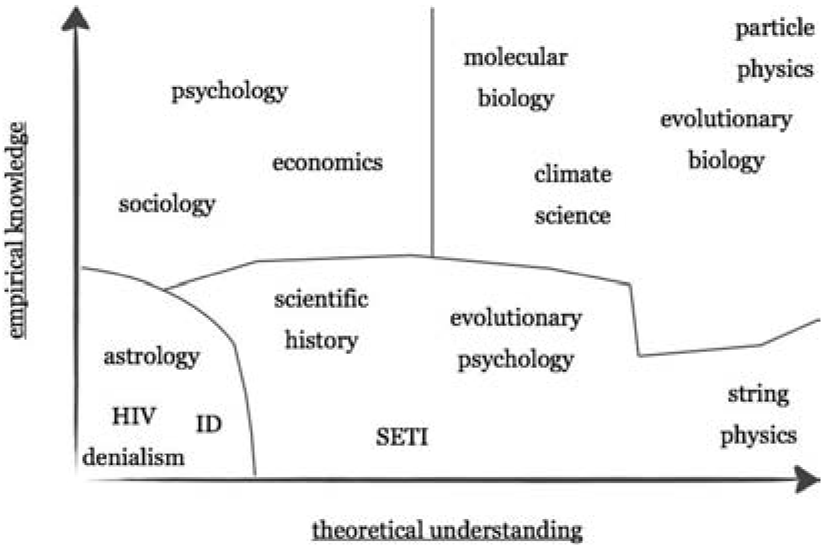

Figure 1.3

Figure 1.3, then, represents my reconstruction of how theoretical and empirical strengths begin to divide the space identified by the cluster diagram in figure 1.1: at the upper right corner of the empirical/theoretical plane we find well-established sciences (and the scientific notions that characterize them), like particle physics, evolutionary biology, and so forth. We can then move down vertically, encountering disciplines (and notions) that are theoretically sound but have decreasing empirical content, all the way down to superstring physics, based on a very sophisticated mathematical theory that—so far at least—makes no contact at all with (new) empirical evidence. Moving from the upper left to the upper right of the diagram brings us to fields and notions that are rich in evidence, but for which the theory is incomplete or entirely lacking, as in many of the social (sometimes referred to as "soft") sciences.

So far I doubt I have said anything particularly controversial about the empirical/theoretical plane so identified. More interesting things happen when one moves diagonally from the upper right to the lower left corner. For instance, the "proto-/quasi-science" cluster in figure 1.1 is found in the middle and middle-lower part of figure 1.3, where theoretical sophistication is intermediate and empirical content is low. Here belong controversial disciplines like evolutionary psychology, the Search for Extraterrestrial Intelligence (SETI), and "scientific" approaches to the study of history. Evolutionary psychology is theoretically sound in the sense that it is grounded on the general theory of evolution. But as I mention above, there are serious doubts about the testability of a number of specific claims made by evolutionary psychologists (e.g., that a certain waist-to-hip ratio in human females is universally attractive), simply because of the peculiar difficulties represented by the human species when it comes to testing historical hypotheses about traits that do not leave a fossil record (Kaplan 2002). In the case of SETI, despite the occasional ingenious defense of that research program (Cirkovic and Bradbury 2006), the fact remains that not only has it (so far) absolutely no empirical content, but its theoretical foundations are sketchy at best and have not advanced much since the onset of the effort in the 1960s (Kukla 2001). As for scientific approaches to the study of history (e.g., Diamond 1999, 2011; Turchin 2003, 2007), their general applicability remains to be established, and their degree of theoretical soundness is far from being a settled matter.

We finally get to the lower left corner of figure 1.3, where actual pseudoscience resides, represented in the diagram by astrology, Intelligent Design (ID) creationism, and HIV denialism. While we could zoom further into this corner and begin to make interesting distinctions among pseudosciences themselves (e.g., among those that pretend to be based on scientific principles versus those that invoke completely mysterious phenomena versus those that resort to supernatural notions), they all occupy an area of the diagram that is extremely low both in terms of empirical content and when it comes to theoretical sophistication. This most certainly does *not* mean that no empirical data bears on pseudosciences or that—at least in some cases—no theoretical foundation supports them. Take the case of astrology as paradigmatic: plenty of empirical tests of astrological claims have been carried out, and the properly controlled ones have all failed (e.g., Carlson 1985). Moreover, astrologers certainly can produce "theoretical" foundations for their claims, but these quickly turn out to be both internally incoherent and, more damning, entirely detached from or in contradiction with very established notions from a variety of other sciences (particularly physics and astronomy, but also biology). Following a Quinean conception of the web of knowledge (Quine 1951), one would then be forced to either throw out astrology (and, for similar reasons, creationism) or reject close to the entirety of the established sciences occupying the upper right corner of figure 1.3. The choice is obvious.

Could the notions captured in figures 1.1 and 1.3 be made a bit more precise than simply invoking the Wittgensteinian notion of family resemblance? I believe this can be done in a variety of ways, one of which is to dip into the resources offered by symbolic nonclassical logics like fuzzy logic (Hajek 2010). Fuzzy logic, as is well known, was developed out of fuzzy set theory to deal with situations that contain degrees of membership or degrees of truth, as in the standard problems posed by notions like being "old" versus "young," and generally related to Sorites paradox.

Fuzzy logic as a type of many-valued logic using *modus ponens* as its deductive rule is well equipped, then, to deal with the degree of "scientificity" of a notion or field, itself broken down in degrees of empirical support and theoretical sophistication as outlined above. For this to actually work, one would have to develop quantitative metrics of the relevant variables. While such development is certainly possible, the details would hardly be uncontroversial. But this does not undermine the general suggestion that one can make sense of science/pseudoscience as cluster concepts, which in turn can be treated—at least potentially—in rigorous logical fashion through the aid of fuzzy logic.

Here, then, is what I think are reasonable answers to Laudan's three "metaphilosophical" questions concerning demarcation:

(1) What conditions of adequacy should a proposed demarcation criterion satisfy?

A viable demarcation criterion should recover much (though not necessarily all) of the intuitive classification of sciences and pseudosciences generally accepted by practicing scientists and many philosophers of science, as illustrated in figure 1.1.

(2) Is the criterion under consideration offering necessary or sufficient conditions, or both, for scientific status?

Demarcation should *not* be attempted on the basis of a small set of individually necessary and jointly sufficient conditions because "science" and "pseudoscience" are inherently Wittgensteinian family resemblance concepts (fig. 1.2). A better approach is to understand them via a multidimensional continuous classification based on degrees of theoretical soundness and empirical support (fig. 1.3), an approach that, in principle, can be made rigorous by the use of fuzzy logic and similar instruments.

(3) What actions or judgments are implied by the claim that a certain belief or activity is "scientific" or "unscientific"?

Philosophers *ought* to get into the political and social fray raised by discussions about the value (or lack thereof) of *both* science and pseudoscience. This is what renders philosophy of science not just an (interesting) intellectual exercise, but a vital contribution to critical thinking and evaluative judgment in the broader society.

Laudan (1983, 125) concluded his essay by stating that "pseudo-science" and "unscientific" are "just hollow phrases which do only emotive work for us. As such, they are more suited to the rhetoric of politicians and Scottish sociologists of knowledge than to that of empirical researchers." On the contrary, those phrases are rich with meaning and consequences precisely because both science and pseudoscience play important roles in the dealings of modern society. And it is high time that philosophers get their hands dirty and join the fray to make their own distinctive contributions to the all-important—sometimes even vital—distinction between sense and nonsense.

NOTE

1. As several authors have pointed out (e.g., Needham 2000), Duhem's thesis needs to be distinguished from Quine's (1951), even though often the two are jointly known as the Duhem-Quine thesis. While Duhem's "adjustments" to rescue a theory are local (i.e., within the circumscribed domain of the theory itself), Quine's are global, referring to changes that can be made to the entire web of knowledge—up to and including the laws of logic themselves. Accordingly, Duhem's thesis properly belongs to discussions of falsification and demarcation, while Quine's is better understood as a general critique of empiricism (in accordance to its appearance as an aside in his famous paper "Two Dogmas of Empiricism").

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